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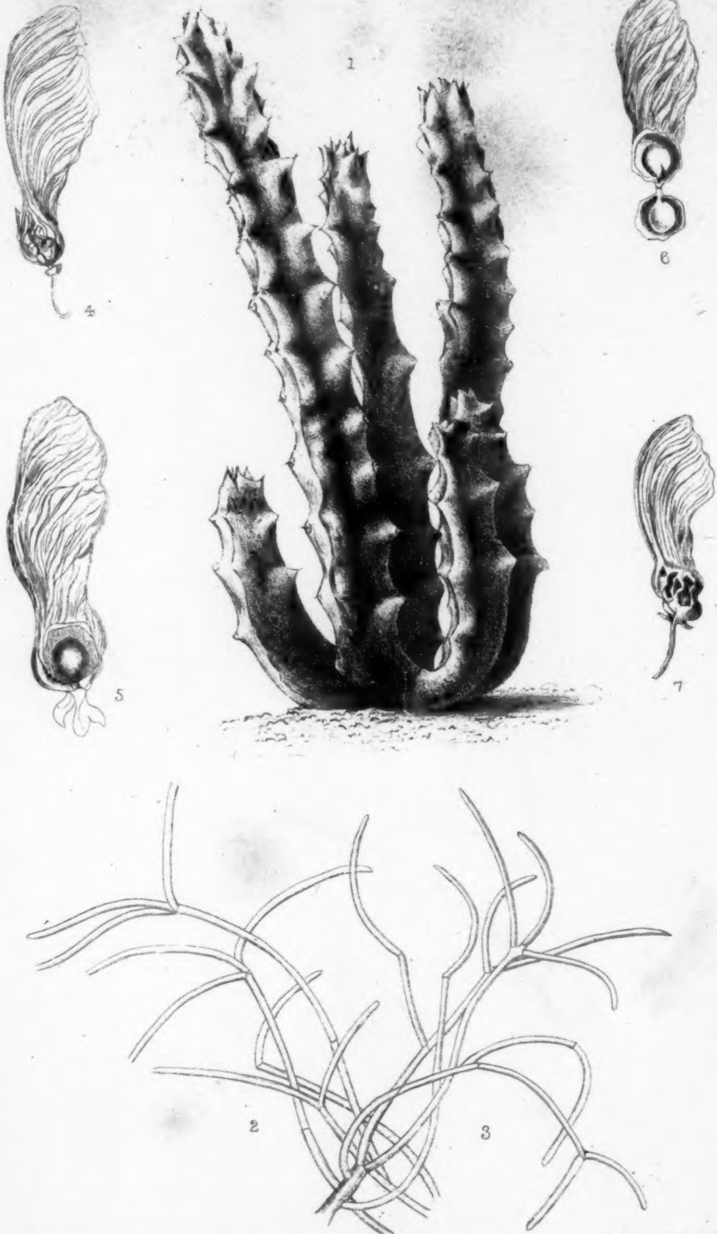

Dr. S.W. Bennett.

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Mimicry in plants.

POPULAR SCIENCE REVIEW.

MIMICRY IN PLANTS.

By ALFRED W. BENNETT, M.A., B.Sc., F.L.S.

[PLATE LXXIX.]

THE subject of so-called "Mimicry" in the animal kingdom has recently attracted no small share of attention both from naturalists and from amateurs. The phenomena included in the term are indeed such as, from their singularity and their apparent marvellousness, cannot but captivate even the most *dilettante* student of Nature. Mr. Bates, in his "Naturalist on the Amazons," may be said to have first introduced the subject to the notice of the general public. Mr. Trimen has recorded, in the "Transactions of the Linnean Society," some remarkable and beautiful instances among South African Lepidoptera; and Mr Wallace, in his delightful "Malay Archipelago," has done still more to arouse the interest of even the most unobservant reader. Some of the imitations depicted in the illustrations of the latter book are, indeed, simply wonderful. The object of this singular mimicry is considered, by those most conversant with the subject, to be a certain amount of protection gained by the "mimicking" species, through its superficial resemblance, thus acquired, to another species, which enjoys, for some reason, special immunity from the attacks of enemies, or to some inanimate object. Whether this explanation is supported by a careful examination of the facts it is not now my purpose to inquire, the subject having been ably debated elsewhere. This resemblance occurs sometimes between species belonging to one family or order, as between one butterfly and another; sometimes between forms much more distantly related, as between a fly and a bee, or an ant and a spider; sometimes between animals and inorganic objects, as between a caterpillar and a twig, or an insect in the perfect condition and a decayed leaf. The superficial resemblance is occasionally so close, and carried into such marvellously minute details of structure, that even the eyes of practised entomologists are deceived, as it is supposed those of the natural enemies of the animal are.

Two explanations, and two only, have been offered of the origin of this "mimetism," or "protective resemblance":—natural selection and hybridisation. Mr. Darwin, Mr. Wallace, and Mr. Bates advocate the former view, maintaining that the resemblance is brought about by exceedingly slow gradations, each small variation in the direction of the species ultimately mimicked being perpetuated to the prejudice of the offspring which do not thus vary, by the operation of the law of "The Survival of the Fittest." This theory commends itself, on its first enunciation, from its beauty and simplicity, and has been eagerly adopted and zealously defended by the ultra-Darwinians who form the bulk of our rising naturalists. That this explanation is, however, not so free from difficulty as its advocates have imagined, has been shown by several recent writers, and especially by Mr. Mivart in his very able "Genesis of Species," although he has not offered any definite counter-hypothesis. The theory of hybridisation has found an advocate in one able and experienced naturalist, Mr. Andrew Murray, but has not met with general acceptance, and, in addition to other objections, is obviously inapplicable, at all events, to the cases of the imitation by animals of inorganic forms.

That similar curious resemblances have not hitherto been described in the vegetable kingdom, is mainly because they have not been looked for with the same zeal; and no doubt also arises partly from the much greater difficulty of preserving the outward appearance of plants than of animals. The exterior covering of most animals, and in the case of insects the whole of the body, is comparatively easily preserved, without loss of colour or form, in museums or cabinets. We have no such method of preserving the tenderer parts of plants; and, with respect to the colour and form of the natives of tropical or unexplored regions, have to trust greatly to the very unreliable fidelity of artists, very few of whom have any accurate scientific knowledge. Since, therefore, the most remarkable developments of both animal and plant life occur in the wild luxuriance of tropical countries, it is only the few who have had the good fortune to travel in those regions who have much practical opportunity of studying the phenomena we are discussing, except in the case of the few species that have been cultivated in Europe. The only work that has come under my notice in which the subject is discussed, is a little book published in 1869, by Mr. L. H. Grindon, entitled "Echoes in Plant and Flower Life," and he has avowedly not treated it in a scientific manner, but has collected together a large number of curious and interesting facts for others to draw their conclusions from. At the last meeting of the British Association at Edinburgh, Professor Thistleton-Dyer read a short paper with this title, but it is very far from exhausting the subject. The visitors to the *soirées* of the Linnean Society for the last two years have also been attracted by the collections exhibited by that munificent patron of horticulture, Mr. W. Wilson Saunders, of so-called "mimetic plants," consisting of pairs of species resembling one another in their foliage or habit to so extraordinary a degree—and yet belonging to entirely different natural orders—that even a good botanist might well be excused for passing them over as identical.*

Before alluding to the theories which have been broached on the subject, let us examine the facts which may be collected, and attempt to classify them. The resemblances among plants sufficiently close to deserve the appellation of mimicry may be classed under two heads:—those which relate to the whole habit and mode of growth, and those which refer to the development of some particular organ or part.

Taking first the former of these classes: there are a number of facts which are familiar to every student of botany, and even to casual observers. Every one knows that to a certain extent that assemblage of characters which we call the habit of a plant becomes changed by the circumstances in which it grows. A tree in a warm genial climate becomes a dwarf shrub when exposed to the bitter cutting winds of northern latitudes; an annual in a temperate changeable climate becomes a perennial when transplanted to a tropical country where there is no alternation of summer and winter. Hence the general features which characterise what have been termed the phyto-geographical regions of the earth; the absence of trees, and the prostrate shrubs with a peculiar tortuous and compact habit of growth of the Arctic zone; the green pastures, showy flowering annual herbs, and deciduous forests of temperate latitudes; the shiny-leaved evergreen forests and profusion of splendid climbers of the tropics; and the scanty thorny or succulent vegetation of the deserts. Under peculiar conditions all plants, no matter to what class they belong, or how remote their relationship, have a tendency to assume a certain resemblance in external features. Plants growing in running water, whether flowering or flowerless, *Ranunculus* or *Myriophyllum*, *Chara* or *Potamogeton*, have the submerged leaves long and filiform,

* To the courtesy of Mr. Saunders and of his very intelligent gardener Mr. Green, who has paid special attention to this subject, we are indebted for the facility for making several of the drawings with which this paper is illustrated.

or cut into slender divisions. Maritime plants growing within reach of the salt spray are apt to become dwarf and fleshy in their habit; and the same remark applies to those which grow on exposed mountain summits, where they are liable to severe though short droughts during the brief but intense summer. In arid desert situations this feature of the vegetation is still more remarkable. Our yellow and white stone-crops, with their round juicy leaves, lovers of rocks and dry walls, are replaced, as we go farther south, by larger species of the same order, or by the similarly disposed Ficoideæ, as the pretty little *Mesembryanthemum crystallinum*, the ice-plant of our greenhouses, which refreshes with its cool foliage the borders of the desert in Egypt, and elsewhere in North Africa. Many orders of plants, indeed, occurring with us only as ordinary herbs or slender shrubs, are represented in those countries by genera of succulent plants, great favourites in our greenhouses, whose affinity it is hard to recognise.

One of the most remarkable features of the hotter and drier parts of America is the abundance of different forms of Cactus, so much cultivated in this country for the beauty of their flowers and the singular weird form of their trunks, which perform the functions of both stem and leaves. Having its head-quarters in Mexico, the order extends as far as the temperate latitudes of Chile and Canada, and includes, on a moderate computation, at least one thousand species. In Africa the order is entirely absent, or rather its absence is made more conspicuous by the occurrence of a single species of *Rhipsalis* at the Cape; but its place is supplied by another class of plants, the *Euphorbias*, a genus represented in this country by several inconspicuous but familiar weeds known as Spurges. In tropical and subtropical Africa the genus assumes the habit and general appearance of the absent *Cacti*, though in their botanical affinities they are nearly as remote as two orders of plants can well be. Except when they are in flower, it is, indeed, difficult to believe that these African *Euphorbias* are not in reality *Cacti*; and the resemblance is not merely a general one; particular groups, and even species, of African *Euphorbia* imitate particular groups or species of American *Cactus* in the form and habit of the stem and the arrangement of the spines, so that it is almost impossible to distinguish between them. This singular imitation is not, moreover, confined to these two families. The accompanying illustration (Fig. 1), reminding one irresistibly of a familiar *Cactus*, is drawn from a species of *Stapelia*, allied to *S. hirsuta*, belonging to the order Asclepiadaceæ, a near ally of the brilliant and fragrant *Stephanotis* and *Hoya* of our stoves, and equally remote, in any system of classification, alike from the Cactaceæ and the

Euphorbiaceæ. Additional instances of close general resemblance in habits of plants destitute of the slightest structural affinity are afforded by *Haworthia*, a genus of Liliaceous, and *Echeveria*, a genus of Crassulaceous plants, the former allied to the lilies and aloes, the latter to the stone-crops; and by Figs. 2 and 3 in our illustration, representing a Cactaceous (*Rhipsalis funalis*) and a Euphorbiaceous plant (*Euphorbia Tirucalli*), the one from tropical America, the other from South Africa. Multitudes of others might have been adduced equally striking.

If we now pass from general to special resemblances, we find ourselves entering on a still more extensive field. Granting the Darwinian or Lamarckian theory of the descent of allied forms from a common ancestor, and their gradual differentiation from one another, a wider margin of separation, as far as mere external and less important characters are concerned, appears to be allowed to near relatives in the case of plants than of animals. The same genus of plants includes frequently species much more widely divergent in habit and in all superficial features than ever occurs among animals. Hence far more play is given to a species to simulate the appearance of another species of some very remote genus, as is often indicated in the specific names of plants: *Polygonum Convolvulus*, *Solanum jasminioides*, *Osmanthus ilicifolius*, &c. To such a height in even minute details is this resemblance often carried, that the most experienced botanist has sometimes referred a plant, on a too cursory examination, to a genus or even natural order with which it has no affinity whatever. Thus Sir William Hooker is said to have actually figured a *Veronica* as a Conifer; Kunze, a great authority on ferns, considered the curious *Stangeria paradoxa*, a Cycad, allied to the Conifers, as a true fern; and Dr. Berthold Seemann speaks of having, in the Sandwich Islands, met with a variety of *Solanum Nelsoni*, "which looked for all the world like *Thomasia solanacea* of New Holland, a well-known Buettnereaceous plant of our gardens, the resemblance between these two widely-separated plants being quite as striking as that pointed out in Bates's 'Naturalist on the Amazons' between a certain moth and a humming-bird." *

Less striking instances than this are familiar to all who have made plants their study. The pseudo-papilionaceous flowers of the Cape species of *Polygala* have deceived many a young botanist. The flowers of *Mesembryanthemum* remind one irresistibly of the compound capitula of Compositæ. The remarkably fern-like foliage, extending even to the dichotomously-forked venation, of the hardy Conifer *Salisburia adiantifolia*, is well known to all arboriculturists. The so-called *Fungus melitensis* of Malta is in reality a flowering plant belonging to

* "Gardener's Chronicle," June 27, 1868.

the order Balanophoreæ. The resemblance between the true leaves of the *Eucalypti*, or gum-trees, and the dilated petioles or phyllodia of the *Mimosæ*, both presenting their edges instead of their surfaces to the sky and earth, and both abundant forms of trees in Australia, is very remarkable. The development of ascidia or pitchers from the leaf-stalk or leaf itself occurs not only in the American *Sarracenia* and *Darlingtonia* and the Asiatic *Nepenthes*, belonging to orders at almost the opposite poles of flowering plants, but in Rosaceæ, Asclepiadaceæ, and several other natural orders. The singular irritability of the leaves of the *Mimosa pudica*, or sensitive plant, and other species of Leguminosæ, occurs again in another order of very little structural affinity, but presenting curious analogies in its foliage, the Oxalideæ, or wood-sorrel order. Dr. Hooker describes and draws, in his "Flora Antarctica," a most singular species of *Caltha* (allied structurally to our marsh-marigold), whose leaves are almost an exact reproduction of those of the *Dionæa muscipula*, or "Venus's fly-trap." In the collection of Mr. Saunders is a species of olive, *Olea ilicifolia*, and a variety of the common holly, *Ilex aquifolium, var. macrocarpum*, in which the resemblance is extraordinarily close, not only in the shape of the leaf and of the spiny teeth, but in the very arrangement of the principal veins, and even in the texture and colour. Pairs of leaves exhibiting as close resemblance may be composed of an *Anemone* (Ranunculaceæ) and a *Pelargonium* (Geraniaceæ), a *Gnaphalium* (Compositæ) and a *Lavandula* (Labiatæ), an *Oxalis* (Oxalideæ) and a *Crotalaria* (Leguminosæ), a *Gentiana* (Gentianaceæ) and a *Veratrum* (Melanthaceæ), a *Grevillea* (Proteaceæ) and an *Acacia* (Leguminosæ), a carrot (Umbelliferæ) and a *Pelargonium* (Geraniaceæ), and of a *Thujopsis* (Coniferæ) and a *Selaginella* (Lycopodiaceæ); the last pair comprising a flowering and a cryptogamic plant.*

Nor are we confined to the leaf for the recurrence of the same type in widely separated families. The peculiar mode of dehiscence of the anther to allow of the escape of the pollen known as "opening by recurved valves" occurs in the Berberidaceæ, in the Lauraceæ, and in a single tribe of Combretaceæ. The pollen grains covered with spiny prominences are found in Malvaceæ and in some Compositæ. But far more curious and striking than these is a remarkable recurrence in several orders of an almost identical external appearance of the fruit. Any indehiscent fruit with a broad membranous wing is called by botanists a "samara," of which we have instances, among our own forest-trees, in the elm, the sycamore, the maple, and the "keys" of the ash. Figs. 4—7 represent the form assumed

* See complete lists in "Nature," May 26, 1870, and May 4, 1871.

by the samara in four genera, belonging to three distinct natural orders, all large shrubs or trees, natives of Brazil. A single genus of Polygalaceæ, the *Securidaca*, chiefly inhabitants of Tropical South America, but extending also into Africa and India, is distinguished by its remarkable winged fruit, varying somewhat in different species, one of the commonest of which is represented by Fig. 4. In Figs. 5 and 6 are delineated the similar samaroid fruits of two species belonging to different genera of the order Phytolaccaceæ, and having therefore no genetic affinity whatever with the first. Fig. 7 again is an example of the fruit of a *Heteropterys*, a genus of Malpighiaceæ, comprising a large number of species, also mostly Tropical American, with a few representatives in Africa. This order is again equally dissociated from both the preceding ones. It will be remarked that not only the form of the wing, but its very texture and the arrangement of the veins, are reproduced most accurately in all the species, a dissection of the fruit alone showing their essential difference in structure. So close indeed and deceptive is this resemblance when the plant is not in flower, that the very specimen of the *Seguiera* from which our drawing is taken, in the Berlin Herbarium, is labelled by so experienced a botanist as Klotzsch as *Securidaca*; and Walpers, in his "Repertorium," has erroneously described five species of *Seguiera* as *Securidacas*. Everyone, indeed, familiar with herbaria, will know of similar instances. It should be noted also that the samaroid fruit is not characteristic of any one of these three natural orders, but only of certain tribes or of single genera. When attention is directed to the subject, a careful search would doubtless be rewarded by the detection of a large number of instances of similar resemblance or mimetic analogy in the vegetable kingdom, as remarkable, or even more so, than those we have here instanced.

Having now chronicled a few of the facts of this curious and interesting subject, I shall be expected at least to attempt some explanation, or to start some theory respecting them. And here our real difficulty commences. Even to arrive at the recognition of any one law running through these phenomena seems, in the present state of our knowledge, impossible. In the first place I shall be found fault with for using the term "Mimicry" in reference to the subject at all. But I must confess to being unable to see the force of the objection, and must continue to consider the series of facts as observed in the animal and vegetable kingdoms as essentially parallel. Strictly speaking, on etymological grounds, the term is open to some objection; μίμησις, "an imitation; a representation by art," implies doubtless a conscious intentional mimicry, which we can no more believe in, in the case of butterflies, than of flowers; or at all events this hypothesis is entirely inconsistent with the theory of development by Natural Selection only. There is doubtless an apparent object in the one case which we are unable to detect in the other; but this does not seem to me sufficient reason for giving a different name to the phenomenon itself.

Professor Thistleton-Dyer objects to the application of the term "Mimicry" to the case of closely resembling plants, on the ground that we do not here find the imitative species occupying the same area as occurs in the animal kingdom. The instances I have given above will show, however, that his statement that "the resembling plants are hardly ever found with those they resemble" is a far too general one. Professor Dyer has made a useful suggestion in proposing the terms "homoplastic" and "Homoplasy" (first applied by Mr. E. R. Lankester to external resemblances in the organs of animals) to the class of phenomena under discussion. The term is a good one, as simply expressing a fact and not a theory, and is free from the objection I have mentioned above to the use of "Mimicry."

One explanation of Mimicry or Homoplasy in plants that has been suggested is that it is due to consanguinity or heredity; and a writer in "Nature" has even been bold enough to offer this suggestion to account for the resemblance between a *Thujopsis* and a *Selaginella* already referred to. But the value of the theory of hereditary reversion is entirely destroyed if it is strained in this manner. It is true that some botanists have traced a genealogical affinity between Conifers and the higher Cryptogams; but the relationship is at the best a very remote one; and to attribute the external facies of a Conifer to its alliance with a Lycopodium is as wild as to attempt to account for the varied colours of birds by their affinity to insects, or of snakes from their alliance with fishes. To be consistent, this theory ought to be applied to the animal kingdom also, and is a hundred times more to the purpose as an explanation of mimetism among Lepidoptera. We may compare with this unnatural straining of a theory the truly scientific manner in which Mr. Darwin applies the principle of heredity to account for the occasional occurrence of stripes on the hindquarters of the horse from its affinity with the zebra. If, however, hereditary reversion acts as remotely as has been suggested, this no more proves the horse to be related to the zebra than to the hyæna.

A certain class of general superficial resemblances may undoubtedly be attributed to the action of natural external causes, to a similarity of conditions of growth; and to these I have already sufficiently alluded. This explanation is, however, entirely inadequate in the case of the minute resemblances of species to species, either in the general habit, or in the development of some particular organ, the leaf or the fruit, such as I have attempted to describe and to represent in the illustrations. No conjunction of external circumstances will avail to account for these, whether acting through Natural Selection or any other known process.

The theory of Protective Resemblance, so seductive an explanation of similar phenomena in the animal kingdom, is also entirely inapplicable here; it is, in fact, more completely inadequate than either of the others. The only manner in which it seems possible to conceive that a species of the vegetable kingdom can benefit by resembling another species, is by presenting so close an imitation of its flowers, in appearance or odour, that it may thereby deceive insects that would otherwise pass it by into visiting it, and thus bringing about the necessary distribution of the pollen. But if such mimicry, where there is no genetic affinity, ever occurs in the flower, it is extremely rare. The only instance of such apparent imitation that occurs to me is in the case of the Bee Orchis, and perhaps one or two of its allies; and here the mimicry is not of another flower, but of the insect itself. It might well be assumed that the extraordinary resemblance of the flower of this singular plant to the body of a bee was designed to attract these insects to the flower; but, unhappily for this theory, the Bee Orchis appears to be one of the comparatively small number of plants that are independent of insect agency for the maturing of their seeds. Mr. Darwin, who has closely watched the plant, has never seen a bee or other insect alight upon its labellum; and both he and other observers state that the construction of the pollinia seems especially contrived to secure self-fertilization, in contrast to the provisions of the larger number of species belonging to the order. The special specific resemblances, on the other hand, which I have described, are chiefly in the foliage, the fruit, and the general habit, from which it is difficult to conceive any profit to arise to the species. In many cases also the resemblance occurs between plants which are natives of countries belonging to entirely different phytogeographical regions, which can never have come into contact with one another. It is just possible that we have a curious instance of protective, or rather of beneficial resemblance in scent, in the case of the carrion-like odour of the flowers of *Stapelia*, which attracts blue-bottle and other flies that may assist in the distribution of the pollen.

We seem then, in attempting to discover some explanation of these phenomena, to be forced back to a view of the operations of Nature which has been too much lost sight of by modern naturalists. Darwin and Wallace's theory of Natural

Selection undoubtedly expresses a great truth, that a struggle for existence is always going on among the far too numerous offspring of the same parents; and that, where no other causes come into operation, those of the offspring which possess any advantageous differences from the remainder will survive to the prejudice of the rest, and will have a tendency to perpetuate this divergence. When, however, Natural Selection is brought forward as adequate to account for the whole history of biological evolution, it presupposes the principle that no change can take place in the way of the evolution of one species from another that is not directly and immediately to the benefit of that individual species;—in other words, that each form of life exists for its own advantage only. But do we not see around us many facts which appear to negative this hypothesis? Biological forms have been evolved presenting peculiarities of structure, special developments of particular organs, not possessed by their parents, but which, as far as we have any means of judging, are and can be of no special advantage to them in the struggle for life. We seem, indeed, more and more compelled to the conclusion that we know next to nothing of the laws which govern the evolution of species, and the development of the marvellously diverse forms of animal and vegetable life that surround us. I cannot myself get away from the conclusion that we must attribute the tendency to variation which is admitted to be the material on which Natural Selection works, to some inherent force belonging of necessity to the functions of life, whether animal or vegetable, which is independent of, and in some sense superior to, the forces that govern the inorganic world. Above all, we are compelled to recur to the pre-Darwinian doctrine of Design; and to believe that Nature has some general purpose in the different modes in which life is manifested, a purpose not in all cases for the immediate advantage of the individual species, but in furtherance of some design of general harmony which it may take centuries of unwearied observation and laborious toil before we discover the key by which we may be able to unlock it.

EXPLANATION OF PLATE LXXIX.

Fig. 1. Stapelia sp. (Asclepiadaceæ).
„ 2. Rhipsalis funalis (Cactaceæ).
„ 3. Euphorbia Tirucalli (Euphorbiaceæ).
„ 4. Fruit of Securidaca lanceolata (Polygalaceæ).
„ 5. „ „ Seguiera floribunda (Phytolaccaceæ).
„ 6. „ „ Gallesia gorazema (Phytolaccaceæ).
„ 7. „ „ Heteropterys argyrophæa (Malpighiaceæ).

RECENT MICROSCOPY.

By HENRY J. SLACK, F.G.S., Sec. R.M.S.

THE object of the following Paper is to select, from records of the microscopical work of 1871, some matters of the most general interest, giving preference to topics which either suggest inquiries many readers can easily follow, or throw light upon well-known questions that have not hitherto been settled.

First, let us notice various attempts to elucidate the structure of the scales of butterflies and of other insects, which, although not belonging to the Lepidoptera, are furnished with scales of a similar or identical character.

Scales appear on various parts of insects besides their wings, and in many cases it is not difficult to show that the hairs of insects are similar structures to scales, both being modifications of the skin. Insects' wings must not be confounded with those of birds, though both are instruments of flight, acting upon the air in a similar manner. In the case of a bird, it is obvious that when the feathers are removed we come to a structure of bone and muscle, and it is the skin that gives rise to various kinds of feathers, that have a most important part to play in enabling a bird to sustain and guide itself in the air. The wing of the insect is an extension of the skin or integument covering the body, sufficiently hardened to bear atmospheric resistance, and strengthened by nervures that, although possessing none of the structure of bones, act in a mechanical way to give firmness and support. With insects, the scales and hairs are evidently related to similar appendages of higher creatures, but the wing feathers or scales of the insect do not assist flight, like those of the bird. They are an ornamental covering, probably of some use in protecting the membrane from which they spring, and on which we find them arranged like tiles on a roof; but the flying power of an insect does not seem impaired when numbers of them are rubbed off. By their beautiful aspects they make, according to Mr. Wallace's observations, males and females mutually attractive, and they are frequently the means of disguises that enable their possessors to escape notice of their enemies, as when a leaf-like butterfly perches on a twig, and looks as if it had grown there—one of many surrounding leaves.

These remarks may assist the student in looking for right analogies and avoiding wrong ones in investigating lepidopterous or other scales.

The minute markings of such scales have always been favourite objects for the microscopist to display, and some of them are still regarded as good practical tests for various powers. Among the most difficult to show clearly are certain markings on the scales of insects to which the name of Podura is still popularly given, though entomologists now call them by other appellations. The famous test scales of microscopists come from an insect now named *Lepidocyrtus curvicollis*; and since Dr. Pigott affirmed that with sufficiently corrected glasses a distinctly beaded structure was to be seen in them, fresh discussion as to their real nature has gone on without ceasing, and strong feelings, as well as reasonings, have been shown by many who had perfectly satisfied themselves with the appearance of the well-known note-of-exclamation marks, so well shown and so beautifully figured by the late Richard Beck.

It seems probable that insect scales are essentially composed of two membranes more or less corrugated, forming a sort of quill at the end where their insertion into the membrane takes place, and expanding upwards into a sort of bag, in the so-called "battledore" variety, and into a flattened plume in the ordinary sort. An intermediate membrane has been described by some observers, but this appears only the result of a deposit which in most scales takes a more or less beaded form, and may combine into a distinct layer in some kinds.

Dr. Pigott's "beads" are by no means inconsistent with the existence of corrugations, and the exclamation marks are probably *true aspects* with a particular focussing and illumination, though few observers, who have taken much care in the investigation, have for many years supposed them to afford an accurate and complete idea of structure.

The extreme delicacy of the Podura or Lepidocyrtus scale gives rise to so much difficulty, both of observation and interpretation, that it is advisable to be guided by analogy drawn from easier scales in its interpretation. This plan was pursued by the writer, who traced what seemed to be real beads in ordinary and easy butterfly scales, through more difficult ones, up to those of *Lepidocyrtus curvicollis*.

Mr. R. J. McIntire took up the question with great skill and with an absence of prejudice somewhat remarkable, in a discussion that has excited an unusual amount of strong feeling; and whatever ultimate conclusion may be reached, his observations and beautiful sketches * will have a permanent value. Taking some scales from the jumping spider (*Scenicus salticus*), and carefully viewing them with high powers, he found their edges "crenated," the "outer membrane" smooth, and the "inner membrane," or that next the creature's body, "puckered up into somewhat irregular rows of hackles." This seemed like a hint that the interjection markings of the Padura might be due to corrugation. In scales of *Polyxenus lagurus* he found, what was "very uncommon," according to his observations, "a deposit between the membrane," and the scale was a very solid structure. Most of his endeavours to detect beaded deposits led him to think such appearances were only "ghosts;" and it is well known that false appearances of beading are easily produced under certain conditions. Mr. McIntire's account of his observations and experiments scarcely warrants his conclusions, for he admits "pigment granules" in scales such as *Amalthusia Horsfieldii*, figured long since by Mr. De la Rue, and in some others.

Lieut.-Colonel Dr. J. J. Woodward, of the U.S. army, employed his well-known skill in photographing Podura and other scales. In the April number for 1871 of the "Monthly Microscopical Journal" will be found a paper by him, read before the Royal Microscopical Society, in which he says: "On the coarser *Degeeria scale* (*D. domestica*), I had no difficulty in making out appearances which, so far as I can gather from Dr. Pigott's own descriptions and the published discussions of his views, are substantially the same as those seen and shown by him . . . and even on the more minutely marked and difficult Lepidocyrtus scale I have been able to develop appearances which seem to be substantially similar." Dr. Woodward did not, however, pronounce any decided opinion as to real structure; but since the date of this paper he has kindly forwarded to the Royal Microscopical Society, and to the writer, photographs of *Degeeria domestica* beautifully exhibiting a beaded appearance. In a communication to the Royal Mocroscopical Society, read in May 1871, Dr. Woodward speaks of Mr. Joseph Beck having shown and left with him a fine Podura slide, showing the note-of-exclamation marks with remarkable clearness; "but immediately afterwards, with the same optical combination and magnifying power, without any change in the cover correction, by simply rendering the illuminating pencil oblique, and slightly withdrawing the objective from its first focal position, he obtained a negative which displayed the bead-like or varicose appearance of the ribbing more satisfactorily than he had previously been able to do." A photograph of this appearance may be seen at the Royal Microscopical Society's room.

* See "Monthly Microscopical Journal," January 1, 1871, &c.

Dr. Maddox took up this much controverted scale question, and sent a paper to the last-named society in May 1871, accompanied with a series of careful drawings given in the 'Monthly Journal' for June. This paper should be referred to, and the drawings carefully examined, in order to appreciate his work. He took a great deal of pains with chemical solvents to remove oily matters, and finally made out a ribbed structure, to which he thought the beaded aspects were due, as fine ribs crossing each other would give that effect. It does not, however, seem that the existence of such structures as Dr. Maddox figures negatives the existence of deposits in a more or less beaded form, nor do the investigations of Mr. Wenham, which prove the reality of surface irregularities more or less corresponding with the exclamation marks.

The reader may by this time have had enough of butterfly scales, and we turn to another subject, also entomological—a demonstration by Mr. Lowne that the so-called "suckers" on the feet of the water-beetle (*Dytiscus marginalis*) are not suckers at all, but an apparatus resembling the cushions attached to the feet of blow-flies for exuding a sticky fluid by which the creatures can be sustained in opposition to the force of gravity. The cushions, or *pulvilli*, of the Dytiscus and other insects seem to have been taken for suckers without any sufficient ground for such an opinion, and when it seemed a probable guess that they and other insects capable of strongly attaching themselves by their feet did so by some sort of air-pump mechanism.

Mr. Lowne found the pulvillus of the Dytiscus a mere modification of the structure of that organ as seen in common flies. He traced in it a secreting sac, supplying a viscous fluid, which percolated through "disk-bearing hairs.' He showed that a dytiscus, made insensible by chloroform, mechanically adhered to the inner glass of the receiver of an air-pump when the air was exhausted, and when, if atmospheric pressure had caused the adhesion, it must have fallen. Common flies are frequently found dead and adhering to window panes, the sticky fluid having hardened while they were alive and rendered them prisoners after their decease. Mr. Lowne has noticed in some dytisci a loss of tarsal disks, apparently from their having been allowed to adhere too tight, so that the insects had to pull their legs away without them.

During the past year numerous papers have appeared relating to the various forms of minute life, somewhat jumbled together under the now popular name *Infusoria*, and, as usual, the spontaneous generation controversy has continued without exhibiting any symptoms of final settlement. Indeed, it seems more likely to be decided at last by reasoning from a large group of facts than by actual observation. Mr. Crace Calvert, in a paper read in May before the Royal Society, alludes to the great difficulties of such investigations, and he specially signalises those arising from the rapid development of minute life under certain conditions. White of egg, for example, mixed with water free from life, and exposed for a quarter of an hour to the air in August or September, exhibited life in abundance. Even a momentary exposure to the atmosphere seems sufficient, as long since pointed out by Pasteur; but no one has hitherto been able to detect in the atmosphere that abundance of *divers* germs which the followers of Pouchet consider must exist therein if the panspermist theory is true. Few would now deny that living forms and their germs may exist in a condition so small, or so transparent, as to elude our best instruments; and it becomes practically impossible to start with establishing the negative proposition, that no life or germ exists in the materials or the vessels experimented with. One of the most interesting of Mr. Crace Calvert's researches related to the amount of heat minute germs of life will bear. He allowed life to be developed in sugar solutions contained in small stout tubes, and then gradually brought them in an oil bath to various temperatures. At 212° most of the living objects had disappeared, but some small black vibrions and three common ones still moved energetically. At 300°, sustained for half an hour, two ordinary and one or two black vibrions still moved; but at 400° and 500° no life was visible. The tubes were examined twenty-four days after the heating.

Much mental confusion exists on these matters from a vague use of the terms "life" and "living." If we consider the complicated phenomena exhibited by creatures enjoying the higher forms of life, and if calling them "alive" is a short way of summing up their properties and actions, and if the same word "alive" is used to sum up the smaller range of properties and actions of the most rudimentary objects of which life of any sort can be predicated, it is obvious that no precise meaning is attached to the term. Notwithstanding Dr. Lionel Beale's experiments, we have no reason for assuming that there is an abrupt and sharp transition from non-living to living matter, or that life consists in the action of a "principle" upon inorganic substances. We do not know what life is, and the less pretension of knowledge that does not exist the better the prospects of science will become.

Various questions of development are intimately connected with that of the origin of life, and many of them are much easier to study; and the more they are worked out the nearer we may approximate to some logical generalization that may conduct us beyond the regions open to direct research.

Mr. Metcalfe Johnson has communicated several papers on these subjects to the "Monthly Microscopical Journal." Referring to that publication for details, and strongly advising that other microscopists should endeavour to repeat and test Mr. Johnson's observations, it may now be sufficient to state that he gives a series of drawings, showing what he believes to be intermediate forms between organisms hitherto supposed to be quite distinct. Thus he pictures certain amœboid forms changing gradually into a paramecium, and he traces the development of the philodine rotifers from elementary forms. "Actinophrys sol," he says, "is only a phase in the life history of Amœba." The philodines he regards "as stages of development of one common form of animalcular existence." In another paper the same writer speaks of various forms of monads as being transitions to such creatures as *Kolpoda Cucullus*. It is of great importance that such statements should be experimentally investigated by other microscopists.

Among the recorded discoveries of new species during the past year is that of a very curious rotifer described by Dr. Hudson to the Royal Microscopical Society in September, and figured in the Journal. He names it *Pedalion myra*. It is remarkable for a prominent limb acting as an oar—whence its name—and in general appearance it suggests the idea of resemblance to the water-fleas. Dr. Hudson will, it is hoped, be able to furnish more details another season concerning its internal structure. At present he is satisfied that it is a rotifer with "six legs," and its crustacean appearance suggests that if sufficient specimens are found for complete examination, it may strengthen the views of those who have thought rotifers nearer the crustacean than the annelidan group.

Professor James Clarke, of Pennsylvania, described in 1861 some very interesting infusoria *roughly* resembling in form vorticellids, surmounted by a delicate transparent glassy funnel, and furnished with a flagellum or whip filament; and Mr. W. Saville Kent has been so fortunate as to find them in a pond at Stoke Newington. The new forms are named Cadosiga, Salpingæca, Ricosœca, and Anthophysa. They were succinctly described by Mr. Kent to the Royal Microscopical Society in November, and will be found in the reports of their Transactions.* Careful illumination and good objectives seem necessary to see the "hyaline collar" or funnel and the slender whip distinctly. When warmer weather arrives, ponds in various localities should be diligently searched for these interesting and beautiful objects.

Some papers concerning fungi which have been written

* "Monthly Microscopical Journal," Dec. 1871.

during the year require notice. Mr. Jabez Hogg took up the question of the diseases said to be caused by a fungoid growth in India, and well known as the "Fungus Foot." The conclusion he arrived at was that the fungus was not the cause of the disease, but a growth taking place in dead matter suitable for its development.

In France considerable annoyance and alarm, as well as interest, was excited in the summer by the appearance of a yellow fungus, which has been named *Oidium aurantiacum*, in the "munition bread" furnished to the French soldiers in Paris. This fungus produces orange-coloured spots on bread, and was first recognised by M. Payen on its appearance in bread supplied to French soldiers some thirty years ago (1843). A small portion of bread afflicted with it is sufficient to inoculate any quantity. The precise cause of its appearance at uncertain intervals is unknown, and forms one of the subjects to be investigated by a commission to whom the whole question has been referred. M. Decaisne stated to the French Academy that he met with bread affected by this *oidium* (if such it is to be called) in Italy in 1862, at a place named Radicofani, and the landlord of the inn told him that it was the second time it had appeared in ten years. The first time, he said, it had not disagreed with anybody who eat it, but on this occasion he considered it had made one of his servants ill, and M. Decaisne found him suffering from vertigo, which an emetic removed.

M. Gauthier de Claubry stated that he had found bread similarly affected in 1831, at Chartres, with a fungus he considered *uredo rubigo*; and in 1842 he saw the same vegetation on the munition bread of Paris. He also discovered spores of this fungus in the wheat employed. It is not stated that this fungus —whatever its name ought to be—produced ill effects on the Paris troops last summer, but M. Decaisne says its action is uncertain, and that all loaves that may be attacked should be treated as unfit for food.

This bread fungus is the more interesting from having been one of the first, if not the first, of its tribe that drew the attention of observers to the curious and unexpected fact that its spores were not deprived of their germinating power by a heat equal to that of boiling water, to which the loaves must have been exposed in baking. Should any reader meet with this fungus, he should carefully watch its growth, notice its fructification, and sow spores on rice paste and other substances. It is probably not a true species, but a variety of some commoner kind.

Amongst miscellaneous matters which cannot now be more than alluded to, it may be remarked that Mr. H. J. Carter has adduced reasons for considering cocoliths as plants allied to *Melobesia calcarea.* The same observer has likewise contributed to the knowledge of sponges by an interesting experiment. He fed a marine calcareous sponge with indigo, examined it at the moment, and then preserved it in spirit, where he says "it now shows all the cells (monociliated) with the cilium attached, and the indigo still in the cells."

From Professor Norris, of Queen's College, Birmingham, we learn that not only do blood corpuscles pass through the walls of blood-vessels, as previous observers had seen, but that some explanation may be offered of the puzzling fact of their doing so without the previous existence of apertures, or any trace thereof after they have passed through. He arrives at the following generalization: That a rigid or plastic body can pass through a colloid film, if there is, first, an intimate power of cohesion; secondly, a certain amount of pressure from within; thirdly, power of the substance of the film to cohere to the surface of the body, or to some intermediate matter which already coheres to the surface during its passage; and fourthly, cohesive plasticity of the particles of the material of which the film itself is composed, so that the breach in it may again become united as it descends upon the opposite surface of the body which is being extended. This is a view of the matter that requires to be supported by experiments with colloid films.

The mode in which silica is deposited in various plants, and especially in diatoms and others, which exhibit that mineral in regular patterns, has engaged the writer's attention, and he regards it as probable that the deposit usually takes place, not from the decomposition of an alkaline silicate, as usually supposed, but from a solution of pure silica in the colloid state, dissolved by rain-water. Such a deposit taking place slowly, through a plant membrane or on its surface, might be expected to assume the form of minute spheres, separated or coalescing according to the rapidity of the process, the quantity of silica in solution, and the exact nature of the surroundings. Quick deposits from rich solutions would probably lead to coalescence in amorphous forms, as seen to a remarkable extent in the bark of the curious pottery-tree, where it is found in lumps. If these views are correct, it is probable that all diatoms have their siliceous skeletons composed of spherules; and a careful examination not only of the so-called "costæ," but also of some of the apparently plane and homogeneous surfaces of pinnulariæ, lead to the conclusion that such is their structure. The so-called "costæ" are not solid ribs at all, but curious and complicated beaded structures. These views were brought before the Royal Microscopical Society in a paper published in the August number of the Journal.

The writer also called attention to the modifications of crystalline forms produced by dissolving the substance to be crystallized in an aqueous solution of colloid silica, instead of in plain water. Figures of the patterns obtained will be found in the Journal for March 1871. Objects so prepared are of unusual beauty when seen with the polariscope.

The attention of microscopists should be called to a series of papers by Dr. Braithwaite on the structure of bog mosses, which belong to the Transactions of the Royal Microscopical Society. They will be found to suggest very interesting observations concerning the minute structure of these highly curious and exquisitely formed objects, as well as indications of their true place in classification, which is higher than was formerly supposed.

Many subjects belonging to minute anatomy and physiology that have been investigated during the year, especially in Germany, are too technical for popular interest, but in addition to what has been already mentioned, Dr. Lionel Beale's discovery and beautiful exhibition of the nerves accompanying capillary vessels well merits notice. Whatever may be thought of portions of Dr. Beale's reasoning, and of his antagonism to views of evolution or development, he stands in the very highest rank, if not at the very summit of observers. His skill in preparation is quite remarkable, and his employment of the highest and finest powers places him in advance of all competitors less painstaking or provided with inferior optical means. Most physiologists will, it is apprehended, infer from the existence of capillary nerves, that they play an important part in controlling the circulation through minute vessels.

M. Georges Pouchet has arrived at the conclusion that the sudden change of colour observable in certain fishes arises from the action of nerves upon their chromoblasts, or colour cells.

In approaching the conclusion of these remarks, which have no pretensions to be exhaustive, the use made of the microscope by Mr. W. Kitchen Parker in his extremely valuable and remarkable morphological researches ought not to be omitted. Morphology is quite a modern science, and throws strong light upon such questions as the origin of species, Darwinian theories, &c. &c. Its object is to trace the earliest appearance and formation of special parts and organs in various animals, and to compare parts which are similar in origin and position in all the creatures in which they can be discerned, and in all stages of growth. Mr. Parker's attention has been recently given to the development of the skull in tadpoles and frogs; and although an intimate acquaintance with comparative anatomy is necessary to follow various details, the broad results are as simple as they are instructive. He finds, for example, in the tadpole skull indications of higher types, and even of bones belonging to the human ear. He tells us, with the authority of a profound and careful observer, "that the highest type—the human—passes through every stage of morphological structure seen in the series beneath; it does not stop at those stages; it does not utilise, so to say, the incipient structures ready to be so used, but runs rapidly along its own line, choosing, as it were, and refusing, until at length the perfect brain is obtained. Yet this perfection of parts, this production of a creature, who in his lowest attributes is the 'paragon of animals,' is not brought about irrelatively to the rest of creation; it is merely an *elected consummation* of all that is highest and best in morphological structure. Does this exclude 'Teleology,' or the fitness of every part to other parts and of the rest of the world? I think not."

In the mechanical department of microscopic science, Mr. J. F. Stanistreet, of Liverpool, has suddenly taken a high rank. He constructed a micro-ruling machine to while away certain hours of tedious illness, and with this he has made exquisite stars and other patterns both on glass and steel. As objects of beauty, these micro-rulings possess great interest, and they are likewise valuable as illustrating some curious optical facts and illusions. As Mr. Stanistreet's machine is figured in the "Monthly Microscopical Journal," and as a paper with illustrations will be found therein, describing his exquisite work, our readers may be referred to that source for further information. Mr. Stanistreet was quite unaware of the remarkable mechanical skill he possesses while other pursuits occupied his time. He still thinks his performances easy, though few could imitate them. Perhaps some other folks may be so fortunate as to discover that they possess special aptitudes which have hitherto been allowed to sleep. Specimens of Mr. Stanistreet's work have been presented by him to the Royal Microscopical Society, where they may be seen, on the introduction of a Fellow.

EXPERIMENTAL RESEARCHES ON THE CONTORTION OF ROCKS.

By L. C. MIALL,

Curator to the Leeds Philosophical Society.

[PLATE LXXX.]

THE geologist is continually engaged in attempting to reproduce a past world. From footprints he tries to reconstruct extinct animals; from accumulated sediments he infers the course of ancient rivers. He seeks to interpret fissures and veins, cliffs and valleys, by referring them to natural agents whose mode of operation is comprehensible. In general, from effects he reasons to causes.

No doubt this process is liable to continual error. The candid geologist will perhaps admit that, biologists excepted, no scientific inquirers have blundered so often and have so often been forced to relay the foundations of their labours as the students of physical geology. The extreme complexity of the phenomena concerned explains the natural tendency of geologists to guess instead of demonstrating.

The only remedy for this source of error is continual verification. When we have assigned a cause to any group of effects we must endeavour to show by tangible evidence that the cause invoked is actually existent, not wholly conjectural, and that it is adequate to the work hypothetically attributed to it.

It needs hardly be said that this process of verification is usually tiresome, and sometimes impossible. We cannot always imitate, even upon the smallest scale, operations which have played a great part in the history of the earth. Even where verification is easy we are too prone to neglect it, and trust to assumption or the application of formulæ. Our present subject of Contortion of Rocks is but one out of many branches of physical geology which has been overlaid with speculation, while the recorded experiments are few.

In this paper I propose to give results of experiments undertaken in the hope of verifying some generally received principles of geology. It will appear, I think, that we can imitate successfully—though on the humblest scale—those great bends and folds of strata which the geologist studies with admiration at Holyhead or Torquay or Fast Castle.

A thick solid bed of limestone bent to a right angle without fracture—how can that phenomenon have been produced? Such was the question which forced itself upon my notice some years ago when collecting facts respecting a series of anticlinal elevations in Craven. In a limestone quarry at Draughton, between Skipton and Bolton Abbey, I came upon beds of rock a foot or two in thickness bent into the figure of an inverted W. The angles were sharp and unbroken. You might pass your finger over the apex of one to make sure that there was neither crack nor vein. To what force must we assign this disturbance without fracture of strata once hard and horizontally laid?

Forty years ago it would have seemed natural to invoke a volcanic eruption acting upon plastic matter—to regard such folds and contortions as due to the formation or elevation of a kind of blister upon the earth's surface. But thanks to a few experimental inquirers, such as Sorby, Hodgkinson, and Tyndall, a number of plausible suppositions had been swept away as fallacies. In 1867 the geologist looked rather to lateral pressure (due possibly to contraction of the figure of the earth) as most probably the force concerned, and he did not find it necessary to suppose that the distorted rocks had ever been plastic. It is true that Sir James Hall assumed that the rocks of Berwickshire were ductile when contorted, and Dr. Edward Hitchcock, a well-known American geologist, had recently maintained that some contorted pebbles in a conglomerate at Newport, Rhode Island, must have been as plastic as moist clay when they were bent and twisted. But this gratuitous assumption was soon disposed of. There were a few fossils in the Draughton limestone, and these were distorted like the rest of the rock.

This seemed to prove that plasticity was not a necessary condition of contortion. The shells and corals had surely not been plastic. Indeed the matrix itself may well have been compact rock from the time of its deposition, growing by the addition of hard lumps and shells and films of stony calcareous matter.

A rigid body compressed without fracture into the figure of W—was this possible? There is, as I afterwards discovered, a source of error in the word "rigid"—a latent hypothesis which turns out to be erroneous. "Rigid" is purely a relative term. Stone is rigid in comparison with clay, but plastic in comparison with cast steel. Absolute rigidity is an unknown property of matter. Let us select a few examples of what are commonly regarded as rigid bodies. Rock crystal, glass, calc spar, steel and limestone, are surely fair specimens. Yet, Tyndall gives instances of quartz crystals altered in shape by pressure, some of them having yielded along transverse planes as if one-half had slidden over the other, but subsequently strongly cemented together by mere apposition and pressure. He regards the action of strongly compressed glass upon polarized light as proof of an alteration in its molecular arrangement. Mr. Sorby has cited examples of distorted crystals of calc spar in cleaved limestones. M. Tresca, in his paper on the "Flow of Solids," read before the Institution of Mechanical Engineers at Paris in 1867, gives the result of experiments made upon lead, iron, and even steel, and shows that these metals behave like liquids when subjected to adequate pressures. As to limestones and other rocks, I can say from my own experiments that they are both elastic and plastic, yielding more or less to forces of short duration, but recovering their original figure, while when subjected to long-continued pressures or strains of low intensity they are capable of setting permanently in a new shape.

It is curious to observe how speculation has been misled by the notion of absolutely rigid bodies, by the assumption that hard rock can exhibit neither an appreciable elasticity nor any ductile properties. Sir James Hall, of Dunglass, whom Professor Geikie has lately styled "the founder of Experimental Geology, since it was he who first brought geological speculation to the test of actual physical experiment," investigated the subject of contortion with much care. He had previously carried out laborious inquiries into the influence of pressure in modifiying the action of heat. The curved strata of the Berwickshire coast had engaged his attention since the year 1788. In 1814 appeared his remarks on "The Convolutions of Strata and their meeting with Granite."* In this interesting paper he describes the local phenomena with some minuteness, and then gives the "rude experiment" contrived to imitate the conditions which he supposed to have obtained in nature :—

"Several pieces of cloth, some linen, some woollen, were spread upon a table, one above the other, each piece representing a single stratum; a door (which happened to be off the hinges) was then laid above the mass, and being loaded with weights, confined it under a considerable pressure. Two boards being next applied vertically to the two ends of the stratified mass, were forced towards each other by repeated blows of a

* "Transactions Royal Society, Edinburgh," vol. vii. pt. 1.

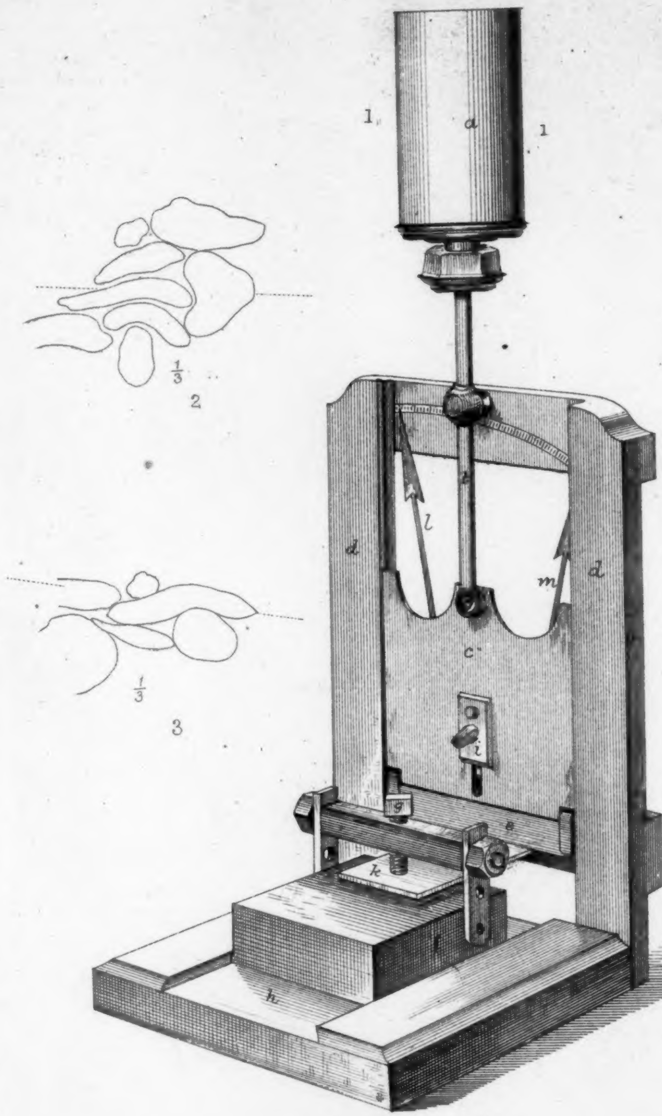
mallet applied horizontally. The consequence was, that the extremities were brought nearer to each other, the heavy door was gradually raised, and the strata were constrained to assume folds, bent up and down, which very much resembled the convoluted beds of killas, as exhibited in the crags of Fast Castle, and illustrated the theory of their formation.

"I now exhibit to the Society a machine by which a set of pliable beds of clay are pressed together, so as to produce the same general effect; and I trust that the forms thus obtained will be found, by gentlemen accustomed to see such rocks, to bear a tolerable resemblance to those of nature."

The positions which we may now consider to have been established by Sir James Hall's experiments and reflections are these: That strata originally horizontal have been curved and folded; and that the disturbing force has acted in a horizontal direction. His further decision that the force concerned is necessarily volcanic may be questioned. The absence of superficial traces of volcanic agency over large areas of contorted strata—the limestone district of Craven, for example—is not easily reconciled with the views derived by Hall from his instructor, Hutton. We must also emphatically dissent from his tacit assumption that the contorted rocks must have been "in a soft but tough and ductile state." Distorted fossils, crystals, and pebbles cannot well have been soft when they were pinched and bent out of shape. Nor need we assume such a condition during the formation of ordinary curved strata. The mechanical properties of limestones and other rocks, dry and at ordinary temperatures, are such as in themselves satisfy the conditions of the problem.

It is natural that early experimenters should fail to perceive many important aspects of the questions which they propose to themselves. A highly interesting addition to Sir James Hall's researches on the influence of pressure as modifying the action of heat was made by Faraday, who showed that the pressure of fifty atmospheres, believed by Hall to be requisite to prevent the escape of carbonic acid from limestone during the process of crystallization by heat, is not indispensable. The composition of the surrounding gas affects the facility of dissipation, and in an atmosphere of carbonic acid, fragments of limestone may be crystallized by heat at standard pressure. The student of geology will not need to be reminded of the importance of this qualifying consideration in connection with calcareous deposits in recent volcanic rocks. Similarly, in his contortion experiments, Hall stopped short at an early stage. It does not appear to have occurred to him that pieces of the very rocks whose curvature he was investigating might be made to bend permanently by means of the same apparatus employed in

Plate LXXX.



Apparatus for shewing Rock contortion.

W. West & Co. lith.

bending layers of cloth and clay. The omission may well seem strange to us, but an explanation is to be found in Hutton's teaching. Uniformitarian as he was, Hutton did not believe that great internal changes had taken place in strata after their solidification; nor did his pupil, when seeking to establish the Huttonian theory by direct experiment, think it needful to investigate the properties of consolidated rock.

In 1866 I began some simple experiments, taking up the points that had been disregarded by Hall. First of all, I took a thin slab of marble and placed it on the edge of a mantelpiece, so that the end projected. A few books kept the slab in its place and then I placed a letter weight of one ounce on the free end and left it for some weeks. On testing it by a straight edge it was found to be deflected to a trifling extent. Other plates of different materials, two or three inches long and as thin as possible, were next procured and subjected to the same treatment. But no accurate results were obtained, and the form of the experiment was inconvenient.

Subsequently I tried an improved plan. Two wooden slabs, ten inches by four, were fitted together by a hinge so that they could be set at any angle from 0° to 180°, just as you might open a book, keeping the letterpress always downwards. One slab was screwed to the table, the other could be adjusted at pleasure. The angle made by the two surfaces was indicated by a graduated semicircle. Upon the ridge various thin plates of stone were placed and attached at one end to the fixed slab by heavy weights. The other end of the plate of rock, projecting over, but not at first in contact with the movable slab of wood, was lightly weighted and allowed time for bending. The angle of sudden fracture could be obtained by setting the machine at a low angle and forcibly bending down the lamina of rock until it touched both surfaces. If it yielded thus far, the angle was slightly increased and the experiment was repeated.

This apparatus had some advantages but many defects. The most serious was that the pressure exerted at any time was difficult to estimate. A weight placed upon a surface of gradually increasing inclination exerts a diminishing pressure which changes appreciably even at angles of 3° or 4°. It was difficult to read the small deflections obtained with any accuracy, and the apparatus was liable to disturbance and accident.

At length I tried the machine represented in Plate LXXX.* Here a thin plate of limestone or other rock is screwed down to a block *f* travelling in a horizontal groove *h*. Upon this

* For the construction of this machine, and for many useful suggestions, I am indebted to Mr. Thomas Prince, of Bradford.

descends a vertical plate *c*, terminating in a hinged knife-edge *e*. We get in this way pressure applied always to the same line upon the lamina of rock, for when deflection begins the knife-edge inclines forwards out of the perpendicular at its lower edge without sliding over the rock specimen. By pushing the block *f* along the groove *h* perpendicularity is restored. An index *m* connected with *e* makes any deviation more apparent. When a piece of rock is to be tested, shot is poured into the cylinder *a*, which is in direct communication with the vertical plate *c*, and the pressure is taken by a steelyard or balance. The index *l* is set at zero by the screw *i*, and its motion along the graduated scale enables the observer to record with precision a deflection of less than ·01 in. If, in adjusting the knife-edge, the index is displaced, it can be restored by this screw independently of other parts of the machine.

With this apparatus I began a long series of observations on limestone. Thin plates of various thickness from ·1 to ·05 in. were subjected to low but protracted pressures. Experience taught the best form of plate and the time required to produce a given result. I succeeded in one case in bending a plate ·07 in. in thickness to an angle (reckoned as rectilinear) of 12°. This took three months to accomplish. On removal from the machine the plate cracked near the apex of the angle of deflection in three days, or I should have operated upon it again. The pressure was applied so gently and uniformly that sudden fracture seldom occurred except when intentionally produced. The bent slabs were, however, very fragile, and could seldom be kept many days after released from strain, cracks slowly extending themselves transversely across the part where the deflection was greatest. From this circumstance, which caused much annoyance at the time, some useful lessons were learned. Details of the experiments with limestone have already appeared.* It will now suffice to say that thin plates of mountain limestone (especially a certain bituminous kind, occurring in thin beds with partings of shale) proved indefinitely plastic. The elasticity of the rock was greater than I had expected, but the set or permanent deflection produced by long-continued pressures of inconsiderable amount far exceeded what I had hoped to find. It may be doubted whether there is any limit to the bending which a careful and patient observer can produce. I found that magnesian limestones, while usually much more elastic than specimens of pure carbonate of lime, were slightly more plastic. The two properties are not connected in any direct or inverse ratio that I can discover. Some of the

* "British Association Report," 1869; "Geological Magazine," November, 1869.

"flexible limestones" are very difficult to bend permanently. One specimen exhibited for many years in a public museum, with the two ends supported and the centre slightly depressed to show its flexibility, does not present any visible deflection when placed on its edge.

Thin natural laminæ of flagstone from the coal measures were also tried. Various specimens were selected according to their texture and mineralogical character, but none yielded important results. It will be seen further on, from other evidence, that considerable deflection has been unintentionally produced in these flagstones, but as yet I have never succeeded in bending thin plates more than 8′. Slates of various kinds have also proved very intractable, an interesting and not unexpected result. No material has yet done so well in my hands as carefully cut slabs of mountain limestone 4 in. × 3 in. and ·07 in thickness.

The frequent destruction by spontaneous fracture of bent plates when removed from the machine seems to imply that an indefinitely protracted and uniformly contorting force is needed to produce unbroken curvature, such as that on the coast of Berwickshire. The experiments next to be related tend to show that resistance on all sides diminishes the risk of fracture. While designed to answer other purposes, the precautions described and the result attained serve to strengthen the opinion that unbroken anticlinals and synclinals are only formed under a considerable weight of superjacent strata.

Anxious to imitate the natural condition of lateral pressure more closely, and at the same time to preserve well-contorted specimens for reference, I tried another method, which ultimately yielded interesting results. My object was to apply pressure to the edges of a slab of stone and overcome the tendency to fracture by embedding it in a matrix of some tenacious substance. The precaution was especially necessary in this second series of experiments. It is easy to see that when deflection begins the bending force is increased in a high ratio. We have the pressure acting upon the slab, not as a force transmitted through its plane, but concentrated upon the middle point, the two halves acting as levers. As the contortion proceeds the strain increases rapidly, and in practice it is found that no graduated pressure can be contrived sufficiently delicate to avoid sudden fracture.

To overcome such difficulties as these I imbedded thin slabs of limestone in pitch and fitted them into a cast-iron box, the two sides of which were removed. One end was cut to allow a screw to travel through it, and within was a plate of iron which could be moved along by the pressure of the screw so as to tighten the slab within the box. By this means pressure was applied to the edges of the plates of rock, and after a few failures the pitch did its work sufficiently well. I regard the matrix of pitch as almost indispensable to success when the experiment of lateral pressure is performed.

By the process just described slabs 9 in. long have been bent until they rose $\frac{3}{8}$ in. in the centre. More conspicuous results may be expected hereafter. The operations required for the production of apparently inconsiderable deflection are tedious and slow, but patience rather than fresh methods seems to be needed. The contortions which we would imitate were not made in a day. Nature is as superior to us in resources of time as of power. Completer and more varied experiments than these are to be desired. Larger specimens of rock should be tested, and the exposure should be longer than thin plates require. With appropriate apparatus a series of observations as detailed and exact as those instituted by Fairbairn in the case of iron and steel might be carried out, greatly to the advantage of geologists, physicists and engineers.

While occupied with this subject of the mechanical properties of rocks a number of examples of unintentional or natural contortion have come under my notice.

Not long ago I saw some small casts of the Elgin marbles prepared in the form of long strips of plaster of Paris $2\frac{1}{4}$ in. broad. These casts had been laid aside for some years and had warped visibly. In one case the deflection (estimated as a rectilinear angle) amounted to 6°. This led to some experiments on plaster of Paris. On submitting dry plates $\frac{1}{4}$ of an inch in thickness to the knife-edge machine a deflection of 8° was obtained in six weeks, and I soon found that this material is indefinitely plastic if the strain be gradually applied. It would probably be easier to bend a flat plate of plaster of Paris into a cylinder than a plank of deal.

Walking one rainy day past the burying-ground attached to a country chapel, I found some gravestones supported horizontally upon corner pedestals. The flagstone of which the monuments were constructed had yielded towards the unsupported centres, and there were pools of water standing in the hollows. The sculpturing of the inscriptions was too sharp to admit the supposition of extensive weathering. The stones were quite smooth, and the method of rubbing down the surface must have rendered them quite level before erection.

Shortly afterwards, I saw a flagstaff in a public park resting upon a broad flagstone $2\frac{1}{2}$ inches in thickness and supported in an upright position by iron ties fixed in the ground at a short distance. The weight of the mast I guessed to be about two tons. The flagstone at its base was visibly curved, as if it had bent beneath the weight of the pole. That this was actually the case appeared from the raising of the free edges of the slab above the surrounding pavement, which was elsewhere fairly level. But all doubt on this point was removed by subsequent measurement. A year and five months later the centre of the slab had sunk $\frac{1}{16}$ of an inch more, relatively to the ends.

Again, a friend communicated to me the experience of slaters, who find that when by yielding of the timbers the surface of an old roof has become bowed, the tiles are distorted to such an extent that they will not lie flat upon a new roof. Old stone tiles (I do not know whether the same is true of slates and brick tiles) are often rendered perfectly useless in this way, however sound they may be. It is perhaps unnecessary to cite other similar cases. Every observant architect and engineer can give from his own experience facts of interest in reference to the influence of long-continued pressure upon an unsupported edge of stone.

The magnificent instances of contortion sometimes displayed in coast sections are certainly more impressive, but perhaps less wonderful in reality, than the cases on record of distorted pebbles. The unlimited effects of long pressure are nowhere so clearly demonstrated as in the bending of round or oval masses of small size. Instances of pebbles elongated in the direction of planes of cleavage occur at Llyn Padarn, near Llanberis, in the Lake District and elsewhere. But the most remarkable cases of alteration of figure effected by pressure are those described by Dr. Hitchcock, Mr. G. L. Vose, and others, as occurring in New England. In Vermont, Maine, Massachusetts and Rhode Island, are found conglomerates where sometimes for hundreds of square feet every pebble, whether of granite, sandstone, schist or quartz, has been flattened. Occasionally one pebble has been driven into another, so as to indent it or squeeze it into a semicircular form, yet without fracture. Some of the examples figured resemble soft cakes jammed together into one mass with unyielding stones, so freely do they curve round in layers and adapt their shape to the various lines of force. Yet plasticity in any ordinary sense of the word is out of the question. These very pebbles are water-worn and some of them cleaved. Not a few are rolled fragments of plutonic rock. (See Plate LXXX. Figs. 2, 3.)

The connection between contorting and cleaving force is not quite clear. It would seem that when a certain freedom of extension is allowed even the most intractable substances yield and change their form. But if the compressed mass be wedged up so tightly that change of figure is impossible, the individual particles seem to revolve upon their axes, and arrange themselves, as coins would do, with their principal planes transverse to the line of pressure. If the conditions exclude even such change as this, the indestructible force may develope itself in other ways still less intelligible to us, and reappear as heat, chemical action, or segregation. Into these inquiries we need not now enter. Our W of limestone is explained—that is, brought into an intelligible relation with other observed phenomena.

It would be interesting, though hardly profitable, to pursue this subject yet further into the field of molecular philosophy. Many attempts have been made to resolve various physical states into combinations of certain hypothetical atomic forces. These speculations are sure to recur, and molecular or atomic theory will some day be the basis of all physical science. Newton, as he says, strongly suspected that all the phenomena of cohesion and aggregation, all the phenomena of chemistry and physiology, resulted from the agency of forces varying with the distance of the particles. Boscovitch endeavoured to establish a general theory of cohesion upon the properties of unextended atoms endowed with powers of attraction and repulsion varying not only in degree but kind with the distance, and to such elementary forces he expected ultimately to reduce the peculiar manifestations of chemical and vital change. In our day Sir William Thomson, expanding a suggestion made by Helmholz, has sought to show that all material phenomena may be due to motions created in an incompressible, frictionless, universal fluid—that the ultimate analysis of matter will hereafter give not particles, but vortices. As yet these doctrines remain mere unverified conjectures; the atomic history of the universe is yet to be constructed. We do not accurately know what takes place when a piece of india-rubber is bent or a piece of moist clay squeezed into a new shape. Still we are in the way of progress when we collect and sift facts, arrange them into classes under general propositions, and test those propositions by applying them to fresh cases. Hereafter it will be possible to apply one principle to explain at once the fluidity of water and the contortion of rocks. Even now we can group together the past and the present, the great and the small. We can show that the forces which curved round the Silurian rocks of Wales are still operative, and that the same forces can be displayed and recognised in the laboratory of the student.

EXPLANATION OF PLATE LXXX.

Fig. 1. Contortion apparatus.

a. Cylinder containing shot.
b. Rod connecting *a* with the vertical plate *c*.
d, *d*. Frame in which the plate *c* slides.
e. Knife-edge, hinged on *c*.
f. Iron block, sliding in groove *h*, to which the lamina *k* is fixed by the screw *g*.
i. Apparatus for setting the index *l* at zero in any position of the vertical plate.
l. Index moving over a graduated arc.
m. Index moving backwards and forwards to show the deviation of *e* from the perpendicular.

Scale nearly $\frac{1}{7}$ of the actual machine.

All the parts are iron or steel, except the cylinder *a*, the rod *b*, the indices and the scale, which are brass.

Figs. 2, 3. Examples of distorted pebbles in conglomerate at Newport, Rhode Island, reduced from Memoirs of the Boston Society of Natural History, vol. i. pl. 17, 18.

PSYCHIC FORCE AND PSYCHIC MEDIA.

By J. P. EARWAKER, Merton College, Oxford.

PART II.

IN our former article published in the October number of this Review, we examined the first experiments of Mr. Crookes on Psychic Force as described in the "Quarterly Journal of Science" for July 1871, and we essayed to show that those experiments, whilst ostentatiously called scientific, were in reality very far from being so. We drew attention to the errors of observation with which the accounts abounded, the gross inaccuracy of the details, the ambiguous way in which they were described, and in conclusion, showed how so many obvious causes of error had neither been allowed for nor even considered. Whilst that article was in the press further investigations on Psychic Force were in progress, and the October number of the same journal contained a second instalment of experiments on this subject. This last article of Mr. Crookes' is a somewhat remarkable one in many respects. With genuine modesty he begins by comparing himself to Galvani, and also draws a parallel between his own investigations and those of the great discoverer of Galvanism, and the first reception accorded to each. But no one, we think, whose mind was free from the bias of foregone conclusions, would see much resemblance between the two series of experiments—Galvani, who experimented on the physiological effects produced by means of the contact of two dissimilar metals, and those of Mr. Crookes, who investigates the phenomena which are produced by a well-known spiritualistic medium. Are the two inanimate and dissimilar metals of Galvani to be compared to the ever-versatile and changeable Mr. Home? Are the results, which, under *all* circumstances, can be obtained by those who repeat his experiments, to be compared for one moment to those which depend on Mr. Home's state of being at any particular time? Can we, in fact, compare certainties with uncertainties, facts with hypothetical assumptions, or the rigid laws of science with the vacillating hypotheses of pseudo-science? And if we cannot, why did Mr. Crookes, who wishes to be thought scientific, if he is anything, so compare them?

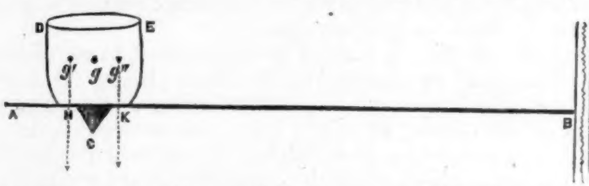
The digression which then immediately precedes the accounts of these new experiments only shows too clearly that their author was almost entirely ignorant of the history of most of the previous work which had been done in the same direction. Besides this, his expressions contradict themselves: in one page he tells his opponents, "Remember, I hazard no hypothesis or theory whatever;" and in another he says, "Professor Thury's ectenic force and *my* psychic force are evidently equivalent terms; . . . . . . the suggestion of a *similar* hypothetical nervous fluid has now reached us from another source," &c. Surely there must be some mistake here.

After this we come to quite a new phase in the history of the Psychic Force experiments. Most of our readers who have ever been to law and have had a verdict given against them cannot but be aware of the spirit in which they have generally received it. The stupidity of the jury, the wrong summing-up of the judge, the neglecting of the most important piece of evidence, &c.—all these details are narrated with more or less exaggeration to the sympathising ears of friends. So it is with Mr. Crookes. The two highest scientific tribunals in England have neglected his papers—the Royal Society has declined to receive them, and the British Association has declined to hear them. Naturally their author is indignant, and the pages of the "Quarterly Journal of Science" are filled with the editor's querulous complaints against his judges. The very letters which passed between them are printed, and all is done to expose the injustice he believes he has had to submit to; but still the unanswerable fact remains—his papers were rejected, the verdict of Science was given against him. And this means something more than is at first sight apparent. The names of Professor Stokes and Dr. Sharpey, the secretaries of the Royal Society, are not names to be lightly pushed on one side. They are the names of men whose lives have been devoted on the one hand to pure physical research, and on the other to that of pure physiology. Who then so competent to decide as to the merits of a case which may almost be called one of "physical physiology?" and their deliberate verdict and that of the Committee went against it. To the British Association Mr. Crookes did not volunteer to show any public experiments, all he offered was a paper detailing those he had already privately made, and this the Committee of Section A rejected, and rightly too; we have had enough of talk on this subject, we now want public experiments; and without these, papers in every successive number of the "Quarterly Journal of Science" will be useless.

We will now deal with the new experiments themselves, and we hope that the above digression will be pardoned us, but it was forced upon us by reading the vast amount of preliminary matter with which the account of them was prefaced. The changed condition of these experiments place us rather, we are afraid, behind the scenes. We hear no more of Dr. Huggins, no more of the "well-known serjeant-at-law," Serjeant Cox. Mr. Crookes is apparently left to fight his battle alone, a solitary hero opposing the now fast-uprising world of science. No more, alas! do we hear of accordions floating in mid-air, no more do dulcet sounds and plaintive airs proceed from them, alas! the very wirework cage seems to be thrown on one side. But all is not gone, and we may rejoice to find that the congenial employment of holding Mr. Home's hands and boots still seems to be deemed a necessary part of a scientific experiment! These changed conditions raise curious questions in our mind. Has it at last been suggested to their author that these very accordion experiments, of musical airs proceeding from an instrument held in one hand, is a very common trick shown at country fairs and performed by most itinerant jugglers? We have little doubt but that these conjurors could easily explain by what "occult" power other than Psychic Force they perform such scientific (?) experiments. Here is a new field for investigation at once opened, which we commend to any man of science ambitious of notoriety—"The Psychics of Conjuring, or a scientific explanation of the delights of our boyhood, by an F.R.S." Unfortunately for us, our education in the mysteries of conjuring was strangely neglected, or else we should have been proud to have answered Mr. Crookes, and, to quote his own words, "prove it to be a trick by showing *how* the trick is performed." But we congratulate ourselves that we have suggested a source whence this experience may be obtained, so that now in the future we shall hope to hear no more of the scientific wobblings of an accordion held in the hand of a Psychic Medium.

The experiments with the balance and the mahogany board, however, are still continued. Their details, whilst they are far too long for direct quotation, cannot adequately be understood without a figure, but they may be summed up as follows. There is the old mahogany board, one end of which is connected with the self-registering spring balance, but the other end now rests, not on the table, as before, but on a knife edge, placed some few inches from the extremity of the board, and this knife edge rests on a firm and heavy wooden table. On the board, exactly over the knife edge, is placed a large glass vessel filled with water, into which a hemispherical copper bowl, perforated with several holes in the bottom, is placed, and this hemispherical bowl rests in the water, supported at the end of an arm of an iron stand, placed some two inches from the board and unconnected with it. Into this bowl, inside the vessel, Mr. Home placed the tips of his fingers, and after a little time the balance showed slight displacements. These, however—and here is a very important point—were displacements in both directions, upwards and downwards, but it is only to one of these that we are more particularly referred. Now, let us examine this experiment very carefully. It is at once obvious that any displacement of the centre of gravity of the whole system *must* produce motion in the board. Anything, therefore, which tends to change the position of the centre of gravity from this, its original position of equilibrium, causes motion in the board which will obviously be registered on the balance. But just as the centre of gravity may be thrown either on one side or on the other, so we may get the end of the board tilted up or down; or, in other words, we may have what is ascribed to "a diminution of the force of gravitation as well as an increase!"

We would wish most forcibly to impress the significance of this source of error in this so much vaunted experiment on our readers, as it is typical of the whole set we are criticising. For its better elucidation the following diagram is necessary:—



A B is the board resting on the fulcrum or knife edge at C, and the end B fixed to a self-registering spring balance. The bowl D E we will suppose placed, as it really was, immediately over C (although the position is indifferent), and the spring balance adjusted to equilibrium. It is obvious that any displacement of the centre of gravity of the bowl will amount to a displacement of the centre of gravity of the whole system, and it may be displaced in two directions. Firstly, let us suppose it displaced to *g*. The board A B becomes then immediately a lever of the first kind, with one force acting at H, and the other at B, and the fulcrum at C. Now, although the distance HC will always be very small, the weight of the bowl and its contents is very large, so that the moment will be quite appreciable and the movement will be duly magnified at B in the ratio of CB to HC. By this motion the end B will rise. Similarly, if the centre of gravity be displaced to *g″* there will be the force acting downwards at K, and which, in like manner, will be obviously magnified at B, but, in this case, the end B must fall. But, since the whole system is originally in equilibrium, that is to say, has its weight compensated by supports, it is virtually a lever without weight; so that, theoretically, an infinitely small displacement, and practically, a very small one, would be sufficient to cause a perceptible motion at the end B. The value, therefore, of the scientific reasoning which would ascribe these so easily producible effects to an unknown Psychic Force may be easily appreciated. It is not the mere fact of placing the fingers or even the whole hand in the bowl which necessarily displaces its centre of gravity, and so it was found that *at first* no effect was noticed, but it was only after some little time that motion was observed. There is nothing stated as to the surface of the water in the bowl being absolutely level and at rest during the experiment, and yet it is evident that this is a point of vital importance, since the slightest lateral vibratory movement would be equivalent to a powerful discharge o Psychic Force. It must also be borne in mind that in such a delicately-balanced instrument the mere touching of the side of the vessel, whether accidental or otherwise, would produce great effects. This then is experiment No. 1, the conclusion of which is thus naively stated, that "Contact through water proves to be as effectual as actual mechanical contact"—a conclusion to which we cordially agree.

Experiments Nos. 2, 3 and 4 were then tried, to see whether any effect would be produced by Mr. Home placing his fingers on the stand of the apparatus, a short distance away from the end A of the board. As might have been anticipated, it was found that effects were produced, but as any tremor communicated to the stand on which the knife edge rested would affect the delicately-poised board, these experiments are not worth much. The results obtained with this apparatus—which is certainly much more delicate than that formerly employed (although it is open to the objections we have raised)—are much less marked than those obtained with his first rough experiments, for instead of a downward fall of 6lbs. we only read of one of 5,000 grains, about 11ozs. Thus it appears that, in proportion as the extraneous disturbing forces are eliminated, so does the disturbance ascribed to Psychic Force similarly diminish. Should we then be considered unreasonable if we assumed that with the complete disappearance of extraneous forces Psychic Force would vanish also?

Mr. Home now disappears from the scene for a time, and another person, a lady possessing similar "psychic" powers, is found, and for her a new set of apparatus is designed. The same difficulty which formerly occurred in fully describing the new apparatus without the necessary diagrams also arises in this case. "A piece of thin parchment is stretched tightly across a circular hoop of wood. At the end B of a freely-moving lever is a vertical point touching this membrane, and at the other end is another needle point projecting horizontally and touching a smoked glass plate, which is moved along by clockwork. The end of the lever is weighted so that it shall quickly follow the movements of the centre of the disk, and these movements are transmitted and recorded on the smoked glass plate by means of the lever and the other needle point. Holes are cut in the side of the hoop to allow a free passage of air to the under side of the membrane." It was then ascertained that "no shaking or jar on the table or support would interfere with the results—the line traced by the point on the smoked glass was perfectly straight in spite of all our attempts to influence the lever by shaking the stand and stamping on the floor." As we read this statement we were very much astonished and could scarcely credit it, but thought that it was only fair to Mr. Crookes to test it by an appeal to a very simple experiment. A banjo was placed on an ordinary dining-room table, with the strings first stretched tight and subsequently relaxed. It is obvious that the membrane in this instrument roughly represents the membrane in Mr. Crookes' apparatus; but, as we have only the ear to detect any vibrations, the means of observation are probably much less delicate in our experiment than in his. Yet, with all these disadvantages, it was found in all cases that the very slightest tremor communicated to the table sufficed to agitate the membrane, whilst walking across or stamping on the floor produced a distinct *resonance*. We shall return to the discussion of this point later on, but proceed now with the account of the experiment. The lady placed her fingers on the wooden stand at some little distance (the exact distance is not stated) from the membrane, and Mr. Crookes placed his hands over hers in order to detect any conscious or unconscious movement. In a short time came the result. Presently "percussive noises were heard on the parchment, resembling the dropping of grains of sand on its surface, and at each percussion the further end of the lever moved up and down. Sometimes these sounds were rapid, and at other times more than a second apart." Five or six tracings were taken on the smoked glass, some of which are given in the paper, and the disturbances it will be seen are in all cases very small indeed, showing that as the delicacy of the apparatus is increased the results decrease. Mr. Home was then tried with the same apparatus, but he did not touch the board. His hand was held over the diaphragm about 10 inches from its surface, and after remaining in this position for about half a minute the index at the further end of the lever was seen to move. "The movements were much smaller than in the former case, and were almost entirely unaccompanied by the percussive vibrations then noticed." The curves traced on smoked glass are then given, but as we are told that they are magnified, and not told how much, for any purposes of comparison they are quite worthless. The whole of the obtained results, however, show clearly that the disturbances were very small. Let us now consider what in these experiments would be the effect of the accumulation of small motions. Everyone is aware of the tension which is caused by keeping the hands in a constrained position for even a short length of time. The hands tremble, and their motion could not but be communicated to any delicate membrane in their immediate vicinity, and this membrane would amplify them and transmit them through the lever to the smoked glass. The tremors may easily be too insignificant to be detected by other hands placed on them, but amply sufficient to agitate the membrane. Again, too, the mere presence of a heated body in the air must cause currents in that medium; but the hand is a heated body, and if these currents be caused to circulate round a stretched membrane, some vibration of the latter is almost certain to occur. We by no means assert that the explanations we have advanced are the true explanations of what occurred, but we are surprised that causes of error so obvious should not have presented themselves to the mind of any scientific investigator, and have been duly examined and eliminated.

It was from this very unsatisfactory series of experiments that Mr. Crookes drew the following remarkable conclusion (the italics are his): "These experiments *confirm beyond doubt* the existence of a force associated in some manner with the human organisation, by which force increased weight is capable of being imparted to solid bodies without contact." It is, however, a canon of physical research laid down by Newton that no new cause should be devised to account for phenomena which may be sufficiently explained by causes already admitted. Until, therefore, it has been incontestably shown that these phenomena could not be produced by known causes, it is unphilosophical in the highest degree to explain them by the assumption of a hitherto unknown force. Again, we have seriously to complain of Mr. Crookes when he says that, just as in the case of Electricity, "certain conditions are found to be essential to the production and operation of Psychic Force, and unless these precautions are observed the experiment fails. The conditions required are very few, very reasonable," &c., but he entirely omits to state what they are. Imagine a man of Science describing the extreme importance of conditions under which he wants certain experiments to be tried, and not even stating what those conditions are; it is really too bad. The only condition which apparently seems to us to be absolutely necessary, is extreme credulity on the part of the experimenter.

It had been objected to the first series of Psychic Force experiments, that, according to their author's showing, there must have been a direct creation of force, as apparently no form of energy was expended in giving the additional weight, &c., to the bodies experimented on. Now, however, we are told how this is effected; it had been anxiously looked for, and the application of the principle of the conservation of energy to Psychic Force is the latest phase in this strange history. After these experiments the gifted Mr. Home is to be seen "lying in an almost fainting condition on the floor, pale and speechless, so that the evolution of Psychic Force is accompanied by a corresponding drain on vital force." Now there is something about this which is a little suspicious. How was it that such marked physiological features were not recorded in the first set of experiments, which professed to be a scientific, and therefore of necessity a full and complete, account of them? Was it only when the necessity for some origin for the force expended was pointed out, that Mr. Home's fainting condition was noticed? Are we to imagine that Mr. Home could, without any sign of exhaustion, add 6lbs. to the weight of a heavy mahogany board, and on another occasion be reduced to a state of absolute prostration by doing one-ninth of the same work? Really there is something in this too much for our credulity, the thing is too preposterous. We would, however, in the most friendly spirit suggest to the believers in Psychic Force, that Mr. Home should be tenderly dealt with—he must not be allowed to over-exert himself; for what would happen were he to expend all his "vital force" in a moment of undue enthusiasm? Notwithstanding all the numerous "psychics" supposed to exist, we are much of opinion that Psychic Force itself would cease contemporaneously with the decease of Mr. Home; and on his epitaph would be written: "a martyr to science, died of over-exertion of Psychic Force!"

If we now calmly consider the whole of this second set of experiments, there are many anomalies that strike us. In the first place one feature common to all Mr. Crookes' apparatus is the wonderful degree of stability with which they are endowed. We have touched on this point already in our discussion of the second experiment, but it deserves a word or two more. He says the experiments were performed in his own dining-room, and with apparatus which was more or less improvised from material lying about the house; and yet instruments thus constructed, fitted with indices of extraordinary sensibility, fail to show the slightest tremor or agitation under the most violent tests—such as stamping on the floor, pressing heavily on the table, &c.—to which they were subjected. Everyone who has ever been engaged in physical research, making use of apparatus in buildings specially designed to secure stability, perhaps the one condition most difficult of attainment, must be sensible that even under such circumstances a moderate footfall across the floor will derange a comparatively rough adjustment. Therefore, comparing Mr. Crookes' experiences in this department with our own, we can reach but one of two conclusions—either the intelligent tradesman who constructed Mr. Crookes' private residence must have anticipated the great uses to which that dining-room would one day be put, and adapted it for them, or else the apparatus employed cannot have anything like the degree of sensibility that is ascribed to it. But we think that much of what Mr. Crookes has observed, and of what he has not observed, may be very easily accounted for. In the course of a correspondence on the subject of Psychic Force, which was carried on in the "Echo" and other papers, we find the following passage in one of his own letters in describing a séance (the italics are ours): "*In accordance with my usual habit of taking notes, I was writing almost the whole time.*" If, then, a scientific inquirer delegate the duty of observation to his assistants and friends, and confines himself to the useful but more modest function of reporting what they tell him, we certainly should not be surprised to find that his narrative recorded much that did not happen, and omits to record much that did. Supposing the inquirer to be gifted in the very highest degree with the faculty of accurate and delicate observation; if he is content to adopt as his own the assertions of other people, we cannot refrain from saying that he very seriously trifles with whatever reputation he may have earned. Nay, he does more than this, for he publishes as the results of his own observation, and with the impress of his own authority, statements which he may have only received on hearsay evidence. We must confess, however, that it was with much gratification that we read the paragraph which revealed to us this peculiar feature in Mr. Crookes' method of inquiry. Of his perfect veracity and sincere belief in all that he had written on the subject, neither we nor anyone else, we are persuaded, ever entertained the slightest doubt. Much that puzzled us is now clear, the ambiguity of many of the statements, which we have repeatedly referred to, is now accounted for; for it is impossible for anyone to record observations adequately unless they are his own. We looked in vain at the time for those minute details, those graphic touches of description, which mark the records of true personal experience, but now their absence ceases to be remarkable. There is yet another side to this question. We could not have believed it possible that any scientific man, much less one of Mr. Crookes' reputation, would have ventured to give to the world, under the title of "experimental investigations of a new force," the description of phenomena, which he did not himself observe. If he chooses thus to make shipwreck of his status in the scientific world, we are sorry for it, but that is his affair; we are, however, entitled to protest against a method of procedure which imposes upon the public his formulated credulity in the place of scientific facts. We sincerely hope in the future that, should he continue his experiments, he will give us his own observations, and not be satisfied with recounting those of other people.

Leaving now the detailed criticism of these experiments, we would in conclusion deal with the whole subject in a more or less general manner. We have been blamed in many quarters for what has been called an undue hostility, which verged on personality, towards Mr. Crookes. We would emphatically deny any such assertion, for whilst we have not the pleasure of Mr. Crookes' acquaintance, we have heard nothing which should bias our mind either one way or the other. The personalities contained in the last number of the "Quarterly Review" are as objectionable to us as they can be to anyone, and we express our regret if our first remarks on this subject should have caused anyone to think that we had had recourse to personality and invective instead of argument. But as Mr. Crookes stands so prominently forward and has made himself, so to speak, the champion of this new theory, we were of necessity obliged, in criticising these experiments, to constantly refer to him. And whilst we have endeavoured to show the fallacy of the experiments themselves, so, too, we have thought we were not exceeding our duty when we proceeded to discuss the numerous points on which he has neglected obvious precautions, and failed to eliminate obvious causes of error. For it must be remembered that much was talked at the time about the scientific accuracy of Mr. Crookes' investigations and his position in the world of science, causing more than usual attention to be paid to what he wrote. We cannot but admire to a certain degree the courage which, induced by his firm belief in his own powers of investigation, has caused him to become the propagator and defender of what he really believed he had discovered; for, with all his failings, his veracity and thorough belief in what he has written are unimpugnable. We could have wished it had been possible for us to have discussed the experiments *per se*, without any reference to their author, but it was not so. In such investigations as these, which border on the domain of pure physiology, the importance of what has been termed "subjectivity" is very great. It lies beyond our province, however, to enter into any detail on this point, but we would refer our readers to the article already alluded to in the last number of the "Quarterly Review." They will there find this question discussed from the standpoint of Physiology, and will read the arguments which tend to show that so many of the phenomena of Spiritualism, Mesmerism, Psychic Force, &c., are in reality subjective and not objective. We have only taken up and critically considered certain experiments which had been brought forward as conclusive and in which much confidence had been placed. If by our criticism we have succeeded in showing how far removed they really are from being conclusive, and how carelessly they were performed, our task will not have been in vain. It seems very strange that at the present time, when it is possible to ascertain so easily what true physical research really means, that anyone could be found to place implicit confidence in experiments such as we have criticised. But when this is found to be the case it behoves those who have had the benefit of experience to show the fallacies which underlie such like experiments, and to expose their inaccuracies. Fault-finding is never a congenial occupation, but when necessary it should be pursued with an unsparing and unshrinking hand.

STRANGE NEWS ABOUT THE SOLAR PROMINENCES.

By RICHARD A. PROCTOR, B.A., F.R.A.S.

Author of "The Sun," "Other Worlds than Ours," &c.

[PLATE LXXXI.]

OUR knowledge respecting the sun has increased so rapidly of late, that it is by no means easy for the astronomer to place in their due position all the facts which have become known. Some of these facts are indeed altogether strange and unexpected; they seem almost inexplicable at a first view, and the more carefully they are studied the more striking do they appear. Quite recently we have received from two different sources the narrative of observations which bear in a most important manner on the interpretation of solar phenomena. From Fr. Secchi, of Rome, we receive the records of a long and careful series of researches, confirming the startling announcements made by Zöllner and Respighi, and adding other information of extreme interest. From Professor Young, of America, we have the account of a single solar outburst, but the most wonderful by far that has yet been witnessed, and affording highly significant evidence respecting the mighty forces at work in the sun's globe.

I propose to consider, here, the bearing of the information thus recently obtained, not merely on the subject of the solar prominences, but on those questions respecting the physical condition of the sun's globe on the one hand, and the nature of the corona on the other, which have recently attracted so much attention. For I conceive that the great fact which is becoming more and more clearly discerned as observation progresses is this, that the phenomena presented by the sun's globe, or rather by the photosphere we see, are intimately associated with the phenomena presented by the solar corona; and that the bond of union thus associating the two series of phenomena is to be recognised in the processes at work in the coloured envelope,—the sierra or chromatosphere,—which may be regarded as one of the solar atmospheres. We are waiting at present for further information on this very point from the observers of the eclipse of December 12; but beyond all question very clear information was obtained during the Mediterranean eclipse of December 1870. Spectroscopy and polariscopy did not avail to tell us all we wished to know respecting the corona; and through unfavourable weather photography failed in doing what it would assuredly have done had the sky at Syracuse cleared round the sun only two minutes earlier. In the last eleven seconds of totality, however, one good picture of the corona (the first ever taken) was obtained by Mr. Brothers; and that picture, besides showing what Mr. Brothers's method was capable of effecting, gave evidence of the utmost importance in relation to the physical condition of the sun. Combined with the spectroscopic charting of the prominences by Mr. Seabroke (during the day of the eclipse, but not during totality), and confirmed by the photograph taken in Spain by Mr. Willard, as well as by the direct observation of the inner corona by Professor Watson, this photograph indicates an association between the prominences, the inner corona and the outer radiated corona, which *must* be accounted for in any theory respecting the condition of the matter surrounding the sun's globe. Wherever the prominences were large and remarkable, there the inner corona was brightest and extended farthest from the sun, and opposite those same regions lay the great radial beams of the outer corona. Combining these relations with the well-known fact that the solar spot-zone is the region in which the prominences have their greatest activity, we see that we are on the traces of a law relating to the whole economy of the great ruling luminary of our planetary system.

Now the study of the solar spots, on the one hand, presents difficulties so serious in their nature that we can scarcely wonder at the fact that hitherto no consistent theory has been put forward in explanation of their phenomena; and, on the other hand, the study of the solar corona is simply the most difficult of all the subjects of investigation which the student of solar physics can present to himself. Holding a place between the phenomena of the spots and those presented by the corona, and associating together these classes of phenomena, are the phenomena presented by the prominences; and *these* can fortunately be studied in a systematic and (all things considered) a satisfactory manner. So long as the prominences could be studied only during eclipses, it was almost hopeless to look to them for information respecting the difficult problems of solar physics; but so soon as a method was devised for examining their features when the sun is not eclipsed, the whole subject of solar research assumed a new aspect. Since that day the progress of discovery has been so rapid as to render it difficult to believe that the method was first applied only three years ago.

Passing over the first observations of Janssen, Lockyer, Capt. J. Herschel, and Secchi, and giving less attention to the questions of the condition of the prominences as respects temperature and pressure than to the motions of the prominence matter, we find in the work of Zöllner and Respighi the first clear intimations of the wonderful activity of the glowing vapours surrounding the sun's globe. So far back as the spring of 1869, Zöllner recognised the action of solar repulsive forces —which he regarded and still regards as eruptive—in casting forth enormous masses of glowing hydrogen. In several papers he has discussed the evidence he has obtained respecting the energy of these forces, arriving at conclusions which were regarded at the time as startling in the extreme, but must now be considered as falling far short of the reality. He assigned 120 miles per second as the probable velocity of outrush in solar eruptions, and spoke of eighty or ninety thousand miles as the probable limit of height to which the erupted matter attains before, gradually descending, it spreads itself into the strange forms constituting the cloud-like as distinguished from the eruptive prominences.

Respighi was led to regard the repulsive action of the sun as electrical in origin; but as he agrees with Zöllner in regarding the prominences as solar eruptions, it is a matter of comparatively small importance that he considers the force producing the eruptions as something very different in its nature from the volcanic action believed in by Zöllner. At the present stage of our progress it is much more important to determine the extent and energy of the solar eruptions than the cause or causes to which they may be due. Respighi gave the following account of the appearances presented by the prominences. It is important that his description should be carefully attended to, as it supplies independent evidence of some of the remarkable observations made by Father Secchi. "When there are *faculæ* on the sun there are usually prominences; but over the sunspots themselves, though there are low jets, there are no high prominences. As respects the distribution of prominences round the sun's limb, it is to be noticed that great prominences are never recognised in the circumpolar solar regions, and the prominences actually seen, besides being small, are few in number, and last but a short time. At the solar equator the prominences are less frequent, less active, and less developed than in higher solar latitudes." He found that "the formation of a prominence is usually preceded by the appearance of a rectilinear jet, either vertical or oblique, and very bright and well defined. This jet rising to a great height is seen to bend back again, falling upon the sun like the jets of our fountains, and presently the sinking matter is seen to assume the shape of gigantic trees, more or less rich in branches and foliage. Gradually the whole sinks down upon the sun, sometimes forming isolated clouds before reaching the solar surface. It is in the upper portions of such prominences that the most remarkable and rapid transformations are witnessed; but a great difference is observed in the rate with which prominences change in figure. Their duration, also, is very variable. Some develope and disappear in a few minutes, while others remain visible for several successive days."

Respighi agrees with Zöllner in considering that the well-marked bases of the eruptive jets "proves that the eruption takes place through some compact substance forming a species of solar crust," and also in believing "that the enormous velocity with which these gaseous masses rush through the solar atmosphere implies that the latter is of excessive tenuity." The highest prominence observed by Respighi had an elevation of no less than 160,000 miles.

Secchi's recent researches, or the researches he has recently completed, result in a classification of the whole series of phenomena presented by the sierra and the prominences. In the first place, he remarks that the sierra or chromatosphere presents four distinct aspects. At times it has a perfectly smooth and well-defined outline, and is very little less brilliant at the edge than throughout the remaining portion of its depth. At other times, though the chromatosphere is quite smooth, and as it were calm, its brilliancy diminishes outwards so gradually that no limit can be distinguished; more frequently the sierra is surmounted by filaments all sloped in the same direction. And lastly, and most frequently of all, the chromatosphere has an irregular outline, and is fringed with small tongues of flame having no specific direction.

The prominences may be divided into three general orders—*heaps*, *jets*, and *plumes*.

The heaped prominences are of three kinds. First, there are slight elevations of the corona rarely more than 15 or 20 seconds in height, and having an outline either diffuse like the second form of the chromatosphere, or fringed like the third or fourth forms of that layer. Secondly, there are brilliant masses resembling our cumulus clouds. Thirdly, there are large diffuse masses suspended above or attached to the tops of the larger prominences.

Next in order are the jets, the most interesting of all the prominences on account of the evidence they afford of mighty repulsive or eruptive forces.

Some of the jets are small, quickly variable in shape, and last but a short time. They resemble, in fact, as pictured by Secchi, a mere development and extension of the irregularities seen in the fourth form of the chromatosphere.

Next in order are jets such as are shown in fig. 1 of the illustrative plate. Such jets are not often met with on a great scale. Secchi terms them *cones*. Such cones often extend themselves into curved shapes such as are shown in fig. 2; the transformation from the form shown in fig. 1 to that of fig. 2 occupying only about twenty minutes. Nor is the transformation gradual, but one form passes quickly into the other after a short interval of seeming tranquillity. "The luminosity of *jets* is always very great," says Secchi, "their roots being more luminous than the rest of the solar surface.* Their appearance is extremely beautiful; the most splendid display of fireworks would fall far short of realising to the imagination the magnificent glory of the sublime spectacle they present. Sometimes the branches fall in the shape of parabolas more or less inclined; at other times they are like the heads of immense palms with the most graceful curving branches." In figs. 3 and 4 of the plate are shown some of the forms assumed by these jets. "The branches," says Secchi, "incline sometimes in the direction of the jet, sometimes recoil upon the stalk from which they spring. This kind of jet is always compact, filamentary to the base, and terminated at the apex without any clear, decided outlines. Their light is so bright that they can be seen through the light clouds into which the chromatosphere breaks up. *Their spectrum indicates besides hydrogen the presence of many other substances.*" (The italics are mine, and I invite special attention to the statement here made by Fr. Secchi.) "These I call *sheaves*. I frequently observe in sheaves a great variability in the refrangibility of the rays" (*that is, the indications of very rapid motions*). "Frequently also, when they have attained a certain height, they cease to grow, and become transformed into exceedingly brilliant masses, which after a time separate and form fiery clouds. A characteristic of sheaves as of the flames is their short duration; they rarely last an hour, frequently only a few minutes."

The prominences of the third class—called *plumes* by Secchi—resemble the jets in some respects, but differ from them in being less bright, and in remaining longer visible; in having their extremities sometimes surmounted by or resolved

* Secchi here refers, of course, to the appearance presented in the spectroscope. If the jets were in any part of their extent actually brighter than the sun's surface, they would be visible without spectroscopic aid; which has never happened.

into clouds; in attaining to a greater height; and lastly, in being seen all round the sun's limb, *whereas the jets are limited to the neighbourhood of the spots.*

Plumes are sometimes simple, sometimes compound. Some forms of simple plumes are presented in figs. 5 and 6 (Plate LXXXI.). Amongst other forms, Secchi notes plumes terminated by diffuse clouds, or crossed by two or three sets of clouds, or doubled down upon themselves, or attached to a cloud by a tail. (Most of these forms have been already described and illustrated.) Near the poles, "evidently on account of the absence of directing currents, they take an almost vertical form, with a diffused cloudy summit as in fig. 6." Compound jets form appearances not readily classified. Some are reticulated as in fig. 7, an arrangement due to the interlacing of distinct plumes. "Thus, in fig. 8, the masses of streamers diverging from three centres join and form arcades, through which appear black spaces. When these streamers are more numerous and extended, they appear as in fig. 9, where the crossing of the threads in all directions produces cornered interstices." But where the crossing streamers are diffused the interstices assume an oval or rounded appearance, as in fig. 10. "These masses attain the enormous heights of from 150 to 240 seconds. Their summits are generally very much broken up, and strongly resemble the masses of cirro-cumuli which we see at the borders of rain clouds. One fact with regard to them is very interesting; it is, that however distinctly marked and well defined the separation of the streamers may be at their base, after a certain height they become completely mingled with each other, and form a mass which appears to be quite uniform in structure."

Secchi gives some interesting particulars respecting solar clouds,—as he terms those masses which float above the chromatosphere, fig. 11, Plate LXXXI. "One class of clouds," he remarks, "is produced by the breaking up of plumes; others appear to be plumes which have ceased to be fed by the chromatosphere, and therefore become detached. The very curious phenomenon is sometimes presented of a cloud suddenly forming itself into plumes (figs. 12 and 13), showing that these plumes can take their origin from gaseous matter, and do not require an orifice of projection for their formation. M. Tacchini, of Palermo, has also made this observation, and we have both seen the jet directed downwards like a fiery rain."

Secchi's remarks upon the physical distinction between plumes and jets, as well as upon the association between prominences and the phenomena of the sun's surface, are of extreme interest and importance. "In distinguishing between jets and plumes," he says, "I have no intention to decide as to whether

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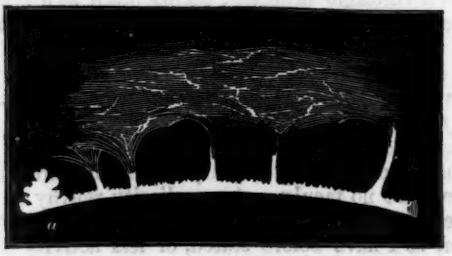
plumes are not also jets. The real distinction appears to be that in jets a part of the photosphere is lifted up, while in the case of plumes it is only the chromatosphere which is disturbed. It does not appear to be established as a fact that all prominences require an orifice of projection, and still less that the height of protuberances can be taken as a measure of the pressure which has projected the gaseous stream, since plumes have been seen to form themselves in the masses suspended in the free atmosphere, far above the possibility of a liquid origin. The persistence of *plumes* is very remarkable as compared with the continuance of the sheaves. In spite of the great mobility of the former, they may be found for two or three days in the same place; towards the poles their existence lasts still longer. On the other hand, the most beautiful sheaves generally last but a few minutes, in very rare cases a few hours. This confirms me in the opinion that sheaves are due to a veritable eruption, taking place at a great depth, the matter composing them having an exceedingly high temperature, and being propelled with immense velocity. The presence of jets and sheaves is the most certain sign that a spot is imminent. As to the connection between protuberances and the *faculæ*, it may be stated that jets, whatever may be their shape, are invariably accompanied by faculæ; but that plumes, more particularly if they are small, are often seen where there are no faculæ. A peculiarity worthy of notice is the feebleness of the light from prominences near the pole—an indication, as I have before stated, of less activity and a less powerful propelling force. The protuberances, both as to number and size, are in accordance with the solar activity as manifested by the spots; the fewer the spots the less numerous and the less extensive are the protuberances likewise. The dimensions of the protuberances are very variable. The largest that I have seen for some time have not exceeded four to four and a half minutes, from which it may be concluded that from the origin of their mass, their measure would be at least five to six minutes, being the height assigned by eclipse-observers to the highest part of the corona. The jets are in general not so high, seldom exceeding one to three minutes."

It will be evident that Secchi's observations bear in a most important manner on the question of an association between the prominences and the solar spots, though they do not make quite clear the nature of the connection. Further evidence is wanted before we can be sure that the eruption-prominences are directly connected with the outbreak of spots on the photosphere. Now Professor Young's researches have supplied (as it appears to me) just the evidence which was required. He has actually witnessed the eruption of matter from the sun, and he has afforded us the means of measuring the energy of ejection and the velocity with which the ejected matter rushes through the lower strata of the solar atmosphere.

On September 7, at noon, he had been examining with the telespectroscope an enormous hydrogen cloud on the eastern limb of the sun. "It had remained," he says, "with very little change since the preceding noon," a long, low, quiet-looking cloud, not very dense or brilliant, nor in any way remarkable except for its size. It was made up mostly of filaments nearly horizontal, and floated above the chromatosphere with its lower surface at a height of some 15,000 miles, but was connected with it, as is usually the case, by three or four vertical columns brighter and more active than the rest. In length it measured 3′ 45″, and in elevation about 2″ to its upper surface" (that is, it was 100,000 miles long by 54,000 miles

Fig. 1.



high). At half past twelve Professor Young was called away for a few minutes. At that time, "there was no indication of what was about to happen, except that one of the connecting stems at the southern extremity of the cloud had grown considerably brighter, and was curiously bent to one side; and near the base of another at the northern end a little brilliant lump had developed itself, shaped much like a summer thunder-head."

On returning, though less than half an hour had passed, Professor Young found to his great surprise that "in the mean time the whole thing had literally been blown to shreds by some inconceivable uprush from beneath." "In place of the quiet cloud I had left," he says, "the air, if I may use the expression, was filled with flying débris—a mass of detached vertical fusiform fragments, each from 10″ to 30″ long by 2″ or 3″ wide, brighter and closer together where the pillars had formerly stood, and rapidly ascending. When I first looked some of them had already reached a height of nearly 4′(100,000 miles); and while I watched them they rose, with a motion almost perceptible to the eye, until in ten minutes (1 h. 5 m. P.M.) the uppermost were more than 200,000 miles above the solar surface. This was ascertained by careful measurements; the mean of three closely accordant determinations gave 7′ 49″ as the extreme altitude attained; and I am particular in the statement because, so far as I know, chromatospheric matter (red hydrogen* in this case) has never before been observed at an altitude exceeding 5′. The velocity of ascent also, 167 miles per second, is considerably greater than anything hitherto recorded. A general idea of the appearance presented when the filaments attained their greatest elevation may be obtained from fig. 2.

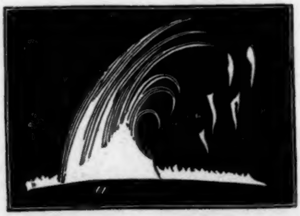
FIG. 2.

"As the filaments rose they gradually faded away like a dissolving cloud, and at 1 h. 15 m. P.M. only a few filmy wisps, with some brighter streamers low down near the chromatosphere, remained to mark the place. But in the meanwhile the little 'thunder-head' before alluded to had grown and developed wonderfully, into a mass of rolling and ever-changing flame, to speak according to appearances. First it was

* Prof. Young probably means that he was observing the red image of the cloud and uprushing matter—*i.e.* the image formed by rays corresponding to the C-line of hydrogen. Father Secchi mentions that he finds the indigo image (*i.e.* the image formed by rays corresponding to the G-line of hydrogen) the most perfect and the fullest in details.

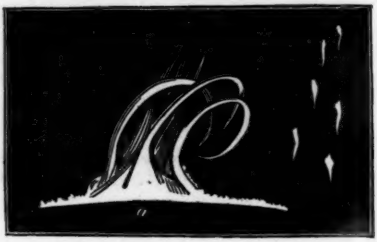
crowded down, as it were, along the solar surface; later it rose almost pyramidally 50,000 miles in height; then its summit was drawn out into long filaments and threads, which were most curiously rolled backwards and downwards like the volutes of an Ionic capital; and finally it faded away, and by 2 h. 30 m. had vanished like the other. Figs. 3 and 4 show it in its full

Fig. 3.



development; the former having been sketched at 1 h. 40 m., and the latter at 1 h. 55 m."* "The whole phenomenon," he adds, "suggested most forcibly the idea of an *explosion* under the great prominence, acting mainly upwards, but also in all directions outwards, and then after an interval followed by a corresponding inrush; and *it seems far from impossible* (the

Fig. 4.



italics are mine) *that the mysterious coronal streamers*, if they turn out to be truly solar, as now seems likely, may find their origin and explanation in such events."

Now, it is to be noticed in the first place, that although the explosion thus described is the only one of the kind that astronomers have yet witnessed, we cannot safely infer that it was an exceptional solar disturbance. It is to be remembered that the sun is not always under spectroscopic surveillance, even in

* Prof. Young mentions that his "sketches" do not pretend to accuracy of detail, except the fourth, the three rolls in which are nearly exact.

suitable observing weather, at American and European stations. Professor Young in America, and in Europe Lockyer, Janssen, Secchi, Respighi, and Zöllner, with the few others who take a more or less systematic part in the work, are unable to devote the whole of the day—or probably even a large portion of the day—to observation of the sun. But apart from this we must take into account the occurrence of unfavourable observing weather, and Lockyer speaks of days seemingly fine, when certain indications in the appearance of the prominence-lines assure him that observation is useless. Doubtless the experience of other observers resembles his in this respect. But this is not all. During a great part of the 24 hours the sun is not above the horizon at any of the European or American observing stations. And then, lastly, even when he is above the horizon, solar outbursts of enormous importance might take place without any possibility that terrestrial observers could become cognisant of the fact; simply because any outbursts in the central parts of the face turned towards the earth and of the half turned directly away from the earth, could not produce prominence-phenomena outside the solar limb. The spectroscope gives us an account indeed of disturbances taking place on the sun's face; but the account can be by no means so easily interpreted as in the case of prominences seen in the ordinary manner.

When we combine these considerations with the circumstance that a solar eruption lasts but a few minutes, and that the observer is unable to examine more than one portion of the sun's limb at a time, so that many important eruptions might occur even while he was engaged in the most attentive observation, we see that outbursts like the one witnessed by Professor Young may occur very frequently and yet be very seldom seen. Again, the jet prominences seen by Respighi, Secchi, Zöllner and others, though not appearing to extend to the height reached by the hydrogen wisps watched by Young, may (many of them) have reached to an even greater height, being reduced by simple foreshortening; and as these are phenomena frequently observed, we may not unsafely infer that eruptions really as important as the one witnessed by Professor Young are by no means uncommon.

But let us consider what the facts observed by Professor Young really imply. This is precisely one of those cases where an observation requires to be carefully discussed in order that its full value may be educed.

Now the main point of the observation is this—that glowing hydrogen was observed to travel from a height of less than 100,000 miles to a height of more than 200,000 miles in ten minutes. To be safe, let us take the limiting heights at 100,000 miles and 200,000 miles; and let us assume that there was no foreshortening. These assumptions both tend, of course, to reduce our estimate of the velocity with which matter was ejected from the sun.

Now we need not trouble ourselves by inquiring whether the hydrogen wisps which moved upwards before Prof. Young's eyes were themselves ejected, or whether their motion might not have been due to the ejection of other matter impinging upon these wisps and forcing them upwards. Some matter *must* have travelled at the observed rate—or (if the hydrogen was not itself ejected, then) at a greater rate.

The question which we have to deal with is therefore this—What must be the velocity of ejection in order that matter may pass between the observed heights in the observed time?

But it may seem that the problem might be simplified by inquiring what must be the velocity of ejection in order that a height of 200,000 miles should be reached. This, however, introduces the question whether that was really the limit of the hydrogen's upward motion. The wisps seemed to dissolve away at that elevation; but we cannot assume quite safely that the hydrogen there ceased to move upwards. On the contrary, it seems more likely that it neither diffused itself (so as to become invisible) nor ceased to ascend, at that level; but simply became invisible through loss of temperature, and therefore of brilliancy. It will be better, therefore, to take simply the flight between the observed levels; for then we shall be attending solely to observed facts. We may, however, inquire as a preliminary process what would be the velocity of ejection necessary to carry a projectile (moving as if *in vacuo*) from the sun's surface to a height of 200,000 miles.

The calculation is not difficult. The formula for our purpose may be thus expressed. Let R be the sun's radius, or 425,000 miles; H the extreme height reached by a projectile from the sun; V the velocity of projection. Then a mile being the unit of length and a second the unit of time—

$$V = 379\sqrt{\frac{H}{R+H}}$$

(379 miles per second is the velocity which would be required to carry a projectile away from the sun altogether); and we have only to put for H 200,000 (miles) and for R 425,000, to deduce the required velocity. We find thus that a projectile must have an initial velocity of about 213 miles per second to reach the height certainly attained by the hydrogen wisps watched by Professor Young.

Now the time in which a projectile with this initial velocity would traverse the upper half of its path is not so readily determined—in fact the formula is not altogether suited to these pages.* I must, therefore, ask those readers who do not care to make the calculations for themselves, to accept on trust my statement that 25m. 56s. would be the time required for the upper half of our projectile's course.

It is already obvious, therefore, that the matter watched by Professor Young did not behave like a projectile *in vacuo*, having 200,000 miles as the limits of its upward course. It traversed a space in 10 minutes which such a projectile would only traverse in about 26 minutes.

Now two explanations are available. We may suppose that the real limit of the upward flight of the hydrogen was greater than 200,000 miles, and that, therefore, the 100,000 miles next below that level were traversed with a greater velocity than would correspond to the case we have just been considering; *or*

* Following, however, the plan adopted in my treatise on 'The Sun,' I give the formula for all such cases in a note, so that those readers whose tastes are mathematical may make the calculation for themselves, if they wish to. It runs thus:—

R being the sun's radius, D the extreme distance of a projectile from the sun's centre, X its distance at time t after starting from rest at distance D (*from centre*, be it remembered), then,

$$\sqrt{\frac{379\mathrm{R}}{\mathrm{D}}}\cdot t=\sqrt{\mathrm{D}x-x^2}+\frac{\mathrm{D}}{2}\left(\pi-\text{vers.}^{-1}\frac{2x}{\mathrm{D}}\right)$$

In the course of my examination of Prof. Young's observation, finding the application of this formula rather wearisome (especially as the formula had to be applied tentatively in dealing with the main problem, for it tells us nothing as to the extreme height, when this is to be determined from the observed time between certain levels), I was led to consider whether a geometrical construction might not be found which would at least afford a test of the calculative results. (For this, be it noticed, is the great value of geometrical constructions; they prevent any serious errors of calculation, by affording a tolerably close approximation to the truth; and in calculation —*crede experto*—great errors are most to be feared).

I presently lighted on the following construction, which may be applied with singular ease, rapidity, and accuracy to all problems such as the one we are upon. Let KQEC be a carefully constructed half cycloid, K being a cusp, E the vertex, and EC the axis. (The same cycloid is to be used for all problems, the remaining constructions being pencilled.) Divide CE in A so that CA represents the sun's radius, AE the flight of a projectile. About centre C draw half circle ADL, cutting half circle on EC as diameter in D. Draw DM square to AC, and let M*m*L, a half circle on ML, cut KC in *m*. Then the time of descent from E to any point P in EA is represented by the ordinate PQ (parallel to KC), where *m*C represents 18 m. 40 s., which is the time in which CA would be traversed, with a velocity of 379 miles per second.

we may suppose that the matter was in reality projected with a much greater velocity than 200 miles per second, and was brought to rest at a height of 200,000 miles by the retarding action of the solar atmosphere cooperating with solar gravity. And, of course, we may conceive that these two explanations coexist, and that the two causes considered operate with any degree of proportional activity, between the relations which would make one or other the sole cause of the observed excess of velocity.

Now, to determine the actual height which must be reached by a projectile from the sun (*in vacuo*) so that it may pass from a height of 100,000 to a height of 200,000 miles in ten minutes, I have gone through a series of calculations which need not be discussed here, leading to the result (which may be accepted as trustworthy) that 350,000 miles is the required height, and therefore 255 miles per second the requisite initial velocity. In this case the hydrogen wisps watched by Professor Young were in reality travelling at a rate of about 150 miles per second when they reached the highest visible part of their course and vanished from view as if by a process of dissolution.

On the other hand, it is not possible to determine the nature of the motion of hydrogen wisps, retarded by the resistance of the solar atmosphere, so as to travel from a height of 100,000 miles to an extreme height of 200,000 miles in ten minutes. We are very far from knowing how to deal satisfactorily with the motion of a solid projectile through our own atmosphere, which may be regarded as appreciably uniform during the projectile's flight, the action of terrestrial gravity being also appreciably uniform. But in the case of the solar atmosphere between the observed levels we have a problem infinitely more difficult, because the atmospheric pressure must be greatly less at a height of 200,000 miles than at a height of 100,000 miles, the solar gravity at these heights being also very different. Nor do we know what the atmospheric pressure is at either level. It would be mere waste of time to discuss a problem all the conditions of which are so vague.

But it will be worth while to consider the general relations which are involved.

In the first place, we may leave out of consideration the motion of the hydrogen before it reached the level of 100,000 miles. The retardation we have to enquire into is something taking place within the observed range of the projectile's motion, and we may consider the moving hydrogen precisely as though its motion had been due to some projectile force operating upon it when already at a height of 100,000 miles. Now we have seen that in order to traverse the next 100,000 miles above that level in ten minutes, it would require an initial rate of motion (at that level) sufficient to carry it to a distance of 350,000 miles from the sun's surface if unretarded. *But* as the matter (on the hypothesis we are considering) did not reach this distance (250,000 miles from its starting place), but, on the contrary, only traversed a distance of 100,000 miles before being reduced to rest, it is obvious that its initial velocity (at level 100,000 miles) must have been greatly in excess of the velocity which, at that level, would correspond to an upward range of 350,000 miles in all. In other words, the hydrogen, when at a height of 100,000 miles, was travelling much faster than a projectile would cross that level if projected *in vacuo* at a rate of 255 miles per second. So that leaving out of consideration all the retardation experienced by the hydrogen before it reached the level 100,000 miles, its motion at that level corresponded to an initial velocity much exceeding 255 miles per second. But, if the retardation was so considerable between the levels 100,000 miles and 200,000 miles, as to reduce the hydrogen to rest at the last-named level, whereas *in vacuo* it would have reached a level much exceeding 350,000 miles, how much more effective must the retardation have been in the first 100,000 miles of the hydrogen's upward course? It is difficult to express how much greater must be the average density of the solar atmosphere between the photosphere and a height of 100,000 miles, than between the height 100,000 miles and 200,000 miles; but the disproportion must be enormous. Apart from this, the retardation being always proportioned to the velocity (though the law of this proportion is not known), would have been much more effective in the lower part of the hydrogen's course, on this account alone. We have, then, this important conclusion (on the hypothesis we are dealing with), that *after traversing a range of 100,000 miles from the sun's surface under the action of a retardation enormously exceeding that operating on the hydrogen in the observed part of its flight, the uprushing hydrogen still retained a velocity far exceeding that due to a velocity of 255 miles per second at the sun's surface in the case of a projectile in vacuo.*

But we have now to consider towards which hypothesis we should lean, or rather which cause we should consider as chiefly operative.

In the first place, it is obvious that we cannot dismiss the hypothesis of retardation entirely, for glowing hydrogen travelling through an atmosphere even of extreme tenuity at an average rate of 167 miles per second must needs be enormously retarded. But I think that, apart from this, we cannot for a moment accept the belief that the hydrogen wisps which Professor Young watched as they slowly vanished at a height of

200,000 miles were then travelling upwards at the rate of about 150 miles per second. So acute an observer could not but have recognised the fact that the hydrogen was still in rapid upward motion at that time. We are compelled then, as I judge, to regard retardation as operative to at least some considerable degree in that upper half of the hydrogen's course.

This being so, I do not know that a single word of what I have said on the hypothesis of retardation being *solely* operative need be altered. The italicised words at the close of the remarks made on that view must still be used in stating the conclusion to which careful reasoning would lead us.

And here I approach the point to which these remarks have been tending. If we regard the hydrogen erupted or in motion in these jet prominences as not less dense than other matter partaking in the motion of primary ejection, the above conclusion, interesting as it is in itself, yet has no bearing on the subject of the corona. The erupted hydrogen reached a certain enormous altitude, and there (so far as the extrusion of matter from the sun was concerned), the work of the solar eruption came to an end. But we have seen that the spectrum of the jet prominences indicates the presence of several other elements—amongst others, several metallic elements in the state of vapour. Now, it is highly probable that at a very early stage of the upward motion a large proportion of the metallic vapour would condense into the liquid form; and if so, such liquid metallic matter would thenceforward meet with far less resistance, and so would travel to a far greater distance than the hydrogen. But without insisting on this point, we may yet feel assured that under similar conditions of temperature and pressure the vapours of the metallic elements far exceed hydrogen in density. Thus they would from the very beginning of their upward course be exposed to a much less effective retarding influence. They would, therefore, retain a much greater proportion of the velocity primarily imparted to the whole body of erupted matter; nor is it by any means an unreasonable or unlikely supposition that at a height of 100,000 miles some of these constituents of the erupted matter would be travelling twice as rapidly upwards as the hydrogen watched by Professor Young. So far, indeed, is this view from being unlikely that it is difficult to entertain any other opinion. Yet, on this view, the matter referred to would be travelling at a rate greatly exceeding 400 miles per second; and a much smaller velocity would suffice to carry it away for ever from the sun's controlling influence. Much more, therefore, would the outrush of such matter suffice to explain the extension of the coronal streamers.

I shall merely note, in conclusion, that it would require only very moderate assumptions respecting the retarding influence of the solar atmosphere, to prove that the least of the jet prominences must have required a velocity of ejection competent to carry the vapours of metals as far as the outermost observed limits of the radiated corona. Now that we have such distinct and incontrovertible evidence of the retardation exerted above a height of 100,000 miles, the opinion * respecting the corona discussed by me in 'Fraser's Magazine' for last April, can no longer be regarded as other than a highly probable theory.

* It would be unfair not to mention, that this opinion had been much earlier urged, and very strong evidence in its favour adduced, by Mr. Matthew Williams, in his interesting work, "The Fuel of the Sun." It was still earlier suggested by the late Prof. Graham. I have been led to it, however, by a perfectly independent line of reasoning.

MADDER DYES FROM COAL.

By EDWARD DIVERS, M.D., F.C.S.

THE beautiful dye-stuffs produced from coal-tar have all proved to be until recently such as were unknown bodies before they were obtained from this source. A new interest has now, however, been imparted to the colour-producing properties of coal-tar by the transformation of one of its constituents into one of the most important and beautiful vegetable dyes that are known—the dyeing matter derived from the madder (*Rubia tinctorum*). As the reader can hardly be assumed to be very familiar with what has been ascertained respecting the production of dyes from the madder-plant itself, it will be well to consider very briefly this production, which, though of some complexity, and still imperfectly comprehended, possesses considerable scientific as well as practical interest.

When powdered madder-root is spread upon an iron plate and cautiously heated so as to avoid scorching it, small orange-coloured crystals are seen to form upon its upper surface, which are the substance that imparts to the root its valuable dyeing power. This substance, discovered by Robiquet, is called *alizarin*, from *alizari*, the name by which madder is known in the Levant.

Alizarin, like indigo, appears, from the microscopic observations of Decaisne, not to exist ready formed in the juices of the living plant. The cells of the root are found to be filled with a yellow substance which increases in quantity with the age of the plant; and this substance gives rise to the true colouring matter when the juice is exposed to the air. Decaisne's observations were, however, disregarded by the chemists who investigated madder until Higgins * drew attention to them again, and, by some very simple and admirably devised experiments, established their accuracy. From these experi-

* In a paper read by him at the British Association in 1848. "Philosophical Magazine," XXXIII. 282.

ments, principally, it became known that cold water dissolves out of madder-root, besides other matters, the colouring matter alizarin, a yellow bitter substance now known by the name of *rubian* after that of the genus rubia, and a highly nitrogenised matter acting as a ferment. The brown aqueous solution thus obtained is at first transparent and of a sweetish bitter taste, but in a short time it becomes turbid and for some hours continues to deposit an orange-coloured, flocculent powder, the liquid at the same time losing its bitterness. The explanation of these phenomena is that the yellow substance, rubian, undergoes *fermentation* by the influence of the nitrogenised and highly putrescible substance which is present at the same time in the freshly-prepared aqueous liquid.

Higgins showed that the colouring matter alizarin already existing in the fresh liquid might be removed from it by suitable means, so as to deprive the latter of active dyeing powers, and that then, by letting this stand in a warm place, fermentation would proceed, all the bitter matter rubian disappear, and a fresh quantity of colouring matter be formed in the liquid. On the other hand, he showed that, by using *boiling* water to make the extract of the madder-root, the ferment was coagulated and deprived of its activity, the liquid remained permanently bitter, and only that quantity of colouring matter present at the time in the root could be obtained. He also established that, as in the ordinary fermentation of grape-juice, the presence of air was non-essential to the continuance of the change in the infusion of madder made with cold water when once it had been started, there only remained to be made the legitimate assumption that, in accordance with Decaisne's observations upon the juice within the cells of the plant, even the dyeing matter always found in the most freshly-prepared infusion of the root is derived from the same source, the bitter matter rubian.

Schunck soon afterwards observed that rubian also yielded alizarin when treated with the mineral acids. Much of the madder used by the dyer is indeed first subjected to the action of sulphuric acid, by which it is converted into what is called in the arts *garancin*. By this treatment not only does the quantity of alizarin appear to be increased, but colouring and other matters which affect the purity of the desired tints are some of them removed and some so far modified as to become inactive.

Alizarin is one of those dyes which require the use of mordants, that is, of substances serving to fix it to the fibres of cotton or woollen fabrics. These mordants also serve to develop the colours which madder (or alizarin) imparts. Thus prepared with aluminum hydrate, cloth takes a Turkey-red colour when boiled with madder, while cloth impregnated with iron hydrate acquires the purple colour so common in cotton *prints*.

The composition of alizarin has now to be considered, for it was this that first suggested that it might be made artificially. Alizarin has been repeatedly analysed by different chemists, but with conflicting results. The variations in the results were no doubt due in part to the alizarin examined being of different degrees of purity, but partly to the fact that it is difficult to determine by unaided analysis the ratio of the hydrogen to the carbon in organic substances of complex constitution with sufficient accuracy to enable it to be expressed by numbers of atoms. Schunck gave alizarin one chemical formula, and Strecker gave it another inconsistent with this; and now that the analogy of alizarin in properties to certain other bodies has led to its being again examined with the powerful aid that such analogy always furnishes, chemists have come to learn its true composition, and that both Schunck's and Strecker's formulæ are incorrect. This brings us to the consideration of the way in which this valuable substance can be obtained independently of the producing powers of the rubiaceæ.

After carrying out some investigations on a different subject altogether, Graebe and Liebermann recognised the analogy just referred to of alizarin to the members of a class of bodies they had been studying, and were in consequence led to try the effect of heating alizarin with powdered zinc. This they found to be the production of a body already well known to chemists, called *anthracene*.

Anthracene is a soft, white lamellated body without taste or smell, which can be obtained from coal or wood. Most persons are aware that when coal is heated in a distilling vessel, besides illuminating gas and other matters, a large quantity of the black, offensive-smelling, viscid liquid known as tar comes over. This tar is a mixture of many different substances, and these, by redistilling the tar, can be partly separated from each other. On account of the value of some of these substances the distillation of tar constitutes an important branch of industry. The substances that first come over are mobile liquids, a mixture of which goes by the name of naphtha. Towards the end of the distillation the bodies that come over are semi-solid: among them is anthracene.

The conversion of alizarin into anthracene at once threw light upon the chemical relations of the former body, and the chemists who had formed anthracene from alizarin next endeavoured *secundum artem* to reverse this transformation, and get alizarin from anthracene.

It was already known that by boiling anthracene with nitric acid it could be converted into a body containing oxygen, and these chemists recognised in this a body intermediate in composition to anthracene and alizarin—a body, in fact, which may be described as anthracene half-converted into alizarin. This body they renamed, in accordance with the results of the researches they had been carrying on, and called it *anthraquinone.**

There being no method known by which it was likely that anthraquinone could be directly converted into alizarin, a change in which it would have to take up as much more oxygen as it already contains, it was acted upon with bromine and converted into a *brominated* anthraquinone. This new substance was then heated with caustic potash, by which the bromine was removed, but only by having potassium oxide left in its place. This was just what was looked for and wanted: more oxygen had been carried into the composition of the body, and the result was a substance that only differed from alizarin in having two atoms of potassium instead of two additional atoms of hydrogen. The last stage of the process presented no difficulty; the potassium-alizarin had only to be heated with an acid in order to replace the potassium by hydrogen, and alizarin was obtained.

The use of bromine being undesirable for manufacturing purposes, Perkin, the discoverer of the first coal-tar dye, determined to try whether he could not replace its use by that of sulphuric acid. In this attempt he proved ultimately successful; as, however, the steps of the process are otherwise essentially the same as those already described, it is unnecessary to follow them out here.

To those familiar with the use of chemical symbols the information afforded by them is so great that we will here represent by these symbols the composition and relations of anthracene, anthraquinone, and alizarin:

Anthracene	$C_{14}H_{10}$
Anthraquinone	$C_{14}H_8O_2$
Alizarin	$C_{14}H_8O_4$.

The identity of artificial alizarin with that obtained from madder having been called in question, the last-named chemist has compared the two bodies together, and enumerates the following points of identity, to show the groundlessness of these doubts:

* Quinone is a substance obtained by oxidising an acid called quinic acid found in the Cinchonas, the coffee plant, the Paraguay-tea plant, &c.; and as Graebe and Liebermann found the body obtained from anthracene to belong to the same class as quinone, they indicated this by the term anthraquinone.

Both the natural and artificial bodies crystallize in needles, which are usually curved, especially when small.

When dissolved in caustic alkali, they both form violet solutions of the same tint.

When applied to mordanted fabrics, they produce exactly the same colours, bearing the treatment with soap equally. They also possess the same tinctorial value.

When dissolved in alcohol, they produce with cupric acetate a purple solution of precisely the same shade of colour.

When examined with the spectroscope, their potassic solutions produce the same absorption bands.

Lastly, the ordinary precipitated artificial alizarin, yields phthalic acid when decomposed with nitric acid, just as alizarin from madder has long been known to do.

There being no other well defined reaction of alizarin, we are, therefore, judging from the above, bound to consider artificial and natural alizarin as identical.

In addition to alizarin another colour, called *purpurin*, is obtainable from madder. The existence of purpurin has indeed been denied by Schunck, but, after all, this denial has not so much practical importance, as he only contends that the substance obtained and called purpurin is nothing but a combination of alizarin with another substance—*verantine*, and not an entirely distinct substance. Now the body called purpurin has been considered to be a material ingredient of the finer colours obtained from madder; and if this were the case, artificial alizarin would prove to be only an imperfect substitute for madder. Schunck, however, also denies that anything but alizarin enters into the formation of madder-colours. Then it has been pointed out by Stokes that the spectrum of purpurin is very different from that of alizarin, so that it becomes easy to detect minute quantities of the former in this way; yet on applying this test to fabrics dyed with madder no purpurin can be detected.

According to Perkin there can be no doubt that the more brilliant the colours dyed with madder, the purer is the alizarin in combination with the mordants. This objection therefore to the substitution of artificial alizarin for madder may be safely regarded as of no value whatever.

The difficulty that remains to be solved is as to how anthracene may be got in sufficiently large quantity to make artificial alizarin an economical substitute for madder. Now that there is a special demand for this substance, we may fairly anticipate that manufacturers will, before very long, succeed in producing it in much larger quantities than at present by the distillation of coal. It is already known that at a sufficiently high temperature various other hydrocarbons, alone or mixed together, are capable of being converted into it; so that it is not improbable that the desired increase may be obtained by some modification in the heating of the coal. When the first coal-tar dye was made from aniline, the latter substance was literally a rare body; it was not long, however, before a method was discovered by which it could be prepared in large enough quantity to meet the demand for it. So, we may expect, will this soon be the case with anthracene.

Even as it is, large quantities of artificial alizarin are manufactured both at home and on the continent, and it is competing in the dye-market with the preparation from madder root.

REVIEWS.

THE ELECTRICITY OF THE BODY.*

IT is unquestionable that the number of workers in pure physiology in this country is extremely small, for example, as compared with either Germany or Italy. Nevertheless, there are a certain number of earnest genuine men among us who have made, and are still making, valuable researches in physiological matters. And foremost in the ranks of these few must be placed the author of the present volume, who is almost alone in England the representative of Matteucci, Du Bois-Reymond, Pflüger, and Eckhard, and other workers in foreign countries. Not recently has he taken up the pursuit of animal electricity, but twenty years ago, and ever since has he devoted the leisure hours obtained from a heavy practice to the pursuit of the electricity of animals. And it is the more remarkable because he has, in the book before us now, given wholly up many of the views which were held as true in his former volumes. We say this because we think that it is infinitely to the author's credit that he has openly admitted his mistake and gone on another tack of scientific discovery. Of course men in the open pursuit of science will readily understand how an author may make mistakes. There is not a day nor an hour when our chemists and astronomers and zoologists are not making mistakes of the most serious character, mistakes which are calculated to leave each of their sciences on the stand-still for years; but we are sorry to say that they do not always confess their errors as candidly as the author of the present volume. We do not mean to say that they knowingly conceal their mistakes, or endeavour to give a semblance of truth to what they know to be false doctrine; but they fail to see their errors till they have spent whole years of their life in bitter and hostile controversy. Therefore we have nothing but praise to award Dr. Radcliffe for the honesty and openness of his avowal.

In the present volume he sets to work in real earnest at his subject, and he has given us the fruits of his later years' researches. These involve work done with Sir W. Thomson's "New Quadrant Electrometer," and the new "B.A. Unit of Resistance," which was devised by the late Dr. Matthiessen, F.R.S., when he was lecturer at St. Mary's Hospital. It is clear, therefore, that through the employment of these two instruments quite a new field

* "Dynamics of Nerve and Muscle." By Charles Bland Radcliffe, M.D., F.R.C.P. London: Macmillan and Co., 1871.

was opened up; and it is this field which Dr. Radcliffe has laboured at so energetically and well. For example, he has found that a single inch of the frog's sciatic nerve exposed a current of electricity to a resistance equal to 40,000 B.A. (British Association) units, or as much as "eight times that of the whole Atlantic cable." But it is with the new and wonderfully exact instrument devised by Sir W. Thomson that his most remarkable researches were conducted, and we may quote a few lines to show this.

"Seeking for tensional phenomena of animal electricity in muscle and nerve by means of the new quadrant electrometer, I soon found that the sides and ends of the fibres were charged differently—the former positively, the latter negatively—and that these evidences of charge disappeared in a great measure during action. I soon found the evidences of the charge for which I had searched before almost in vain; but I found more than I expected. Expecting to find a single charge, I found a double charge; and what to think of this state of things I could not at all see at first. The facts would not chime in with preconceived conceptions, and the end was, that the conceptions had to be modified to suit the facts."

The idea previously held by the writer was quite the opposite of this, for it conceived that the muscular fibres were charged with *one* kind of electricity during rest, and that in this way the molecules were kept in a state of mutual repulsion. But this idea was impossible to reconcile with a belief that there was a double charge of electricity in each muscle, and this latter was unquestionably a fact. Here, then, a new series of views forced themselves on Dr. Radcliffe's attention, viz. that the natural electricity present in the muscle produced the state of muscular relaxation and elongation in a different way from what he formerly supposed; that the tissues presented a great resistance to electricity; that the sheaths acted as dielectrics; that this being the case, a charge of one kind of electricity on the outsides *induced* a charge of the opposite kind on the insides of the muscle, and the electric antagonism of the sides and ends was accounted for by the *induced* inside charge being conducted to the ends by the contents; and, finally, that the fibres might be kept in a state of relaxation by compression of the sheaths, "arising from the mutual attraction of the two opposite charges, disposed, as in a charged Leyden jar, upon the two surfaces of the sheaths." All these views are borne out by ample experiments, which are fully described by the author, but which we have not space for here.

Dr. Radcliffe has found that all the tensional phenomena of the muscular fibre and all the current phenomena also, can be easily imitated upon a wooden model of the fibre left bare at the two ends and at the sides, sheathed with a coating formed of two layers of tinfoil, separated by a thin layer of gutta-percha, if only a charge was supplied to the outer tinfoil layer. He also produced experimentally the elongation of the fibre. This he did on a narrow band of india-rubber, covered on its two surfaces with a thin metallic coating, so as to allow of its being charged in the same manner as a Leyden jar. This appears to be a satisfactory model of a muscle, for it elongates under the influence of a charge of electric fluid, and, on the other hand, it contracts when the charge is discharged from it. This rude example seems abundant in favour of the view which the author works out so elaborately in the pages of the present work.

As regards the *galvanometer*, Dr. Radcliffe thinks it of little use in these inquiries, compared with the *electrometer*. The galvanometer does not tell us, the other instrument does, that the parts between the poles are charged half positively, half negatively. Yet this is intelligence which, the author assures us, cannot be dispensed with; for, as he proves further on, "the workings of voltaic electricity upon muscle are found to be resolvable into those of the charge and discharge of these very charges, and not into those of the constant current."

Another result which the author has arrived at is that the state of the muscles is a state of *electrotonos*, which state further bears out the ideas already alluded to. All through the work will be found abundant accounts of experiments made by the author and an ample description of the results obtained by Matteucci and Du Bois-Reymond, and others who have been engaged in similar researches. In point of style, the book is an exception, for Dr. Radcliffe writes, as few of his medical compeers can, with an elegance and terseness which are most unusual. The physical characters of the volume display the publishers' good taste.

THE SUBTERRANEAN WORLD.*

WE think, of all the works which Dr. G. Hartwig has given us, the present one is unquestionably the best; not only in its style, which is clear and simple, but in the information it conveys, which is full, accurate, and modern. We do not see any fault to find with the book, and we are sure that our readers will study it with a great deal of satisfaction, and with an intense interest in the manifold facts which it relates. The book may really be divided into four parts. First, there is the purely geological and palæontological; then comes physical geography; next we have mines and mining; and, finally, an account of minerals and gems. All this is well done; and the illustrations, although not excessively numerous, are nevertheless good ones of their kind. We shall endeavour to quote something from the principal sections, and thus give the general reader a better idea of the exact nature of the book. After giving a brief account of the different fossil forms which we have yet discovered, the author makes the following remarks on the important question, Has the animal world diminished in size as it has gone on?

"The colossal size of many of the extinct plants and animals might seem to favour the belief that organic life has degenerated from its forme. powers; but a survey of existing creation soon proves the vital principle to be as strong and as flourishing as ever. No fossil tree has yet been found to equal the towering height of the huge *Sequoias* and *Wellingtonias* of California; and though the horse-tails and club-mosses of the carboniferous ages may well be called colossal when compared with their diminutive representatives of the present day, yet their height by no means exceeded that of the tall bamboo of India. No fossil bivalve is as large as the *Tridacna* of the tropical seas; and though our nautilus is a mere pigmy when

* "The Subterranean World." By Dr. George Hartwig. London: Longmans and Co.

compared with many of the ammonites, our naked cuttle-fishes are probably as bulky as those of any of the former geological formations. The living crustaceans and fishes are not inferior to their predecessors in size; and though the giant saurians of the past were much larger than our crocodiles, yet they do not completely dwarf them by comparison. The extinct *Dinornis* far surpassed the ostrich in size, but the mammoth and the mastodon find their equal in our elephant; and though the sloths of the present day are mere pigmies when compared with the *Megatherium*, yet no extinct mammal attains the size of the Greenland whale."

This quotation is a sort of conclusion to the portion of the book devoted to the history of the fossil animals upon the globe. It is followed by a general account of the physical geography of the earth; and herein is contained a long account of the various peculiarities of the globe, of the artificial wells, of earthquakes, volcanoes, and the several underground caverns which are to be found in different parts of the world. The chapter on earthquakes is about the most interesting, for it not only contains an account of the most remarkable of those dreadful commotions, but it gives the leading details of the more general disturbances which have taken place within the past couple of centuries. With regard to these, we may quote the author's remarks on the subject of earthquake shocks.

"Earthquake shocks are either vertical or undulatory. A vertical shock, which is felt immediately above the seat or focus of the subterranean disturbance, causes a movement up and down. Like an exploding mine, it frequently jerks movable bodies high up into the air. Thus during the great earthquake of Riobamba, the bodies of many of the inhabitants were thrown upon the hill of La Culla, which rises to the height of several hundred feet at the other side of the Lican torrent; and during the earthquake of Chili, in 1837, a large mast planted thirty feet in the ground at Fort San Carlos, and propped with iron bars, was thrown upwards, so that a round hole remained behind."

Numerous other examples are given, and the author concludes, with Mr. Mallet, that the rotating hypothesis is not correct, but that earthquake movements are essentially backward and forward movements. We much regret that we have not space for further quotations from this interesting volume. We may, however, just mention that there are hundreds of places from which we could quote with interest, such as the chapters on subterranean water-courses, on cavern animals, on ice-caves, on subterranean catacombs, on mines, and on the several minerals and precious stones. However, we must now conclude our notice, and offer our best thanks to Dr. Hartwig for the very capital treat his pages have afforded us.

A POPULAR HISTORY OF BRITISH INSECTS.*

MR. J. G. WOOD has written us so many and good manuals on different branches of British and general zoology, that we had almost imagined he had come, so to speak, "to the end of his tether," and that we

* "Insects at Home; being a Popular Account of British Insects, their Structure, Habits, and Transformations." By the Rev. J. G. Wood, M.A., F.L.S. London: Longmans and Co., 1872.

should not have any more of his popular works to review in these pages. We are happy to say that we have been mistaken, for the work we have now under notice, if it is not one of his best works, is certainly by no means an illustration of the theory that as a writer advances in public favour, he, as a rule, diminishes in excellence. In point of fact, the "Insects at Home" bears evidence of being carefully prepared by the author, and it is very well illustrated by its publishers. Of course it must be borne in mind that we are speaking of it as a merely popular work; for if we were to criticise under any other view, we should have to call many of the views expressed by the author into serious question. Taking it, then, as a popular book, we may express ourselves as very well pleased with the author's labours. To be sure, he has not dwelt much on the anatomy of the insects, but then he has omitted this specially, because he does not, we suppose, think that so many are interested in it; but on the habits of the several insects he has described, we know of no work, save the immortal "Kirby and Spence's Entomology," which contains so much information.

Of Mr. Wood's method of classification we cannot speak very favourably; but then most probably he would say in opposition to this, that he did not consider the subject at all, and merely gave that classification which he found most generally adopted; and with this argument we can find no fault whatever. There is, however, one suggestion which he makes in his preface to which we must call attention; it is as to colouring the plates. These, he suggests, may be readily coloured by the reader, and he gives the various means by which the process of coloration may be completed. We entirely object to this. If the author had sold with the work a number of plates which, if damaged, would not deface the book, we could not object; but such a suggestion to most readers of the volume looks like a "Yankee" desire to improve the sale of the work by the destruction of copies—a desire which we are sure the reverend author never for a moment conceived of.

As regards the substance of the book, we can only say that it is full to overflowing with accounts of the beauty, intelligence, and peculiar habits of all species of insects which have their home in these islands, and that it concludes with some practical hints on the subject of mounting the specimens which the young entomologist has captured. Although the anatomy of the work is, as we have said, remarkably deficient, still here and there it finds a place, as, for instance, in the account of the great green grasshopper, in which the author says: "Among other points, one of the most interesting is the 'gizzard.' Before it is opened it looks very like a swelling of the digestive tube; but if it be carefully slit horizontally and spread flat, a number of narrow parallel bands will be seen. On placing one of these bands under the microscope, it will be seen to consist of a number of very small teeth, arranged with perfect regularity, and admirably calculated for triturating morsels of the leaves which have been cut off by the powerful jaws, and then swallowed. As for the jaws themselves, their power may be easily ascertained, for the insect is generally given to biting." We thoroughly coincide with Mr. Wood in his recommendation of Lowne's "Anatomy and Physiology of the Blow-fly;" and we can go further than he does, and say of it that it is the most important monograph, with some noted exceptions, that is to be found in any language on the anatomy of a single insect. We say this because it forms, as it were, a sort of companion, and a very essential one, to any work on the general natural history of insects. But on everything that relates to all the peculiar habits and manners of the insect world there is no better book than that of Mr. Wood which we have thus briefly noticed.

MARVELS OF POND LIFE.*

THERE are very few men whose position better qualifies them for a task like the present one than the eminent and active Secretary to the Royal Microscopical Society. But apart from this, Mr. Slack possesses a style as a writer which is at once clear, comprehensive, and to the point; whilst his numerous researches upon the Protozoa and Rotifera constitute him an excellent authority on the subject he has taken up in this excellent little handbook, which we are glad to see is now in its second edition. The reader may be sure he will not find in Mr. Slack's work anything that is erroneous; while, on the other hand, he will be treated with the latest intelligence on the subject that is treated upon; and, moreover, he will find that the writer states his opinions in clear and forcible language, about which it is impossible to have any doubt as to his meaning. In the pages of this little work, which is amply illustrated by the author's wife—a lady who takes a more than common interest in the scientific teaching of the microscope—there is given a very full and fair account of the inhabitants of our ponds. But the reader must not imagine that he will not find the creatures described here in ponds of his own neighbourhood, for Mr. Slack has been careful to describe almost solely those forms which are universally abundant in all kinds of ponds. It is remarkable, too, that the author, besides describing and figuring minutely the several species of animals which he deals with, gives minute details as to the readiest method of capturing them and holding them in a living state during examination. The chapters are thirteen in number; and, with one exception, each deals with a separate month's investigation, so that the young naturalist can pursue his researches all through the year. The woodcuts are numerous—especially those of the Hydra, which is generally depicted from the imagination—and very good; and the plates, which are coloured, are seven in number, and to those who know what is seen under the microscope they are admirable and truth-like representations. *Tout entier*, the book is everything we could desire, so far as it extends, and we can only hope that Mr. Slack will add to it, and make it a much larger and more comprehensive volume.

* "Marvels of Pond Life; or a Year's Microscopic Recreations among the Polyps, Infusoria, Rotifers, Water-bears, and Polyzoa." By Henry J. Slack, F.G.S., Secretary to the Royal Microscopical Society. 2nd edition. London: Groombridge & Sons, 1871.

THE ROYAL INSTITUTION.*

THE Royal Institution is so well and so favourably known as it exists at present to the majority of Londoners who are in the least degree interested in scientific research, that we think an endeavour to publish its history, so that it might be known to every enlightened Englishman, was one in the right direction. We are glad that it has been undertaken, and we know of no one who could do it better, from a longer experience, or with a more loving hand than Dr. Bence Jones, the author of the volume now before us. We wonder that it has not been attempted before, and we cannot but congratulate the author upon his very successful task, and upon the general plan he has adopted in his volume. We say this especially from the outspoken manner in which he speaks of Davy as compared with Faraday, and from his opinion, in which we heartily concur, that Davy was "in originality and in eloquence" far superior to Faraday, while he was not inferior to him in his love of research. The book which Dr. Jones has given us is not unlike the account of Faraday's life which was published some two or three years ago, but it is very much smaller in extent. It tells us of the life of Count Rumford before the Institution was founded, of his subsequent life, of the early history of the Institution; of the lives of Professors Garnett, Young, Faraday, Sir Humphry Davy; of original papers relating to the American war; of original letters from Dr. Thomas Young; and, finally, of the income and expenditure of the Royal Institution to 1814. In general plan it is by no means unlike the Life of Faraday; for example, when possible, the author has allowed each life to tell its own tale by the multitude of documents which he has by most laborious endeavours congregated together. Thus there is hardly a third page of the book which does not contain some one or other important letter bearing upon the subject and printed in full. Of course we cannot say how fully these letters tell of the life of the Royal Institution. No one who is not connected with the place can say whether the book is or is not a full record; and indeed we imagine that very few of those who are there could offer any opinion on the subject. But so far as we have been able to judge, Dr. Bence Jones has played the historian's part with admirable skill, just laying out the principal parts of the story before the reader, and keeping other parts of no interest and of no importance in reserve. And he has done so with an openness and a display of candid criticism which to our minds is greatly to be praised. Although the history is one which of course we cannot dwell further on in these pages, it will nevertheless prove of great interest to a large number of intelligent scientific readers, and with such we wish it every success.

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* "The Royal Institution; its Founders and its first Professors." By Dr. Bence Jones, F.R.S., Honorary Secretary. London: Longmans and Co., 1871.

GEOLOGY, HISTORICAL AND PHYSICAL.*

HOWEVER much may be said about the importance of a student pursuing his study of Geology in large treatises, such as Lyell's and Murchison's, there can be no doubt that the expense attendant on the purchase of these works which are generally dear, is sufficient to prevent a good many students from purchasing them. This cannot be said of volumes such as those now before us, which are issued at such a price that they are within the hands of everyone. Weale's series are too well known to need any general explanation at our hands. But they are not at all equal in value, some of them being the merest trash, while others, by far the majority, are the very best works we can put into a beginner's hands. Unquestionably the two works now before us belong to this category, and we should not be sorry to see them in any beginner's hands. Furthermore, Mr. Ralph Tate, whom we are glad to see directing himself to geological science, has spared no pains in bringing out new editions of the two geological manuals to make them as fully representative of the progress geological science has lately made as possible. He has introduced new passages into the books which prove beyond question that he has been at pains to bring the volumes up to the present state o science; and in cases where a doubt existed he has been open enough to state both sides of the question. This is clearly the case in his chapter on the coal-measures, in which he has introduced us to the more recent facts, those especially connected with the Kilkenny coal formation and its remarkable fossil fauna. It is further shown in his few remarks about *Eozoon*, where he candidly owns the difference which is known to be between the general opinion and the view of Messrs. Rowney and King. In the work devoted to purely Physical Geology, the author is even happier in his construction of the book. It seems quite marvellous what a mass of the opinions of those who have written upon the subject he has introduced. Invariably, too, we find him, when a question is known to have two sides, giving both fully and fairly. Indeed, in all respects, we are much pleased with the manner in which Mr. Ralph Tate has discharged his task, and it is very pleasant for us to be able to avow it.

LONGMANS' TEXT-BOOKS: THE THEORY OF HEAT.†

ASSUREDLY few could have been better selected for the authorship of a manual of "Heat" than Dr. Clerk Maxwell, and, in choosing him, the editor of the Text-books is especially to be congratulated. Yet it is to be regretted, in our opinion, that the author should have so fully developed one part of the work to the exclusion of other points of quite as much importance to the student who goes in for examination, though possibly not of

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* "Historical Geology," and "Physical Geology." By Ralph Tate, F.G.S. London: Lockwood & Co., 1871.

† "The Theory of Heat." By Dr. Clerk Maxwell. London: Longmans & Co., 1871.

such special interest. The author's aim has been clear throughout, and, although he explains it in the preface, it is perfectly clear to those who understand anything of the science. His object has, as he says, been "to exhibit the scientific connection of the various steps by which our knowledge of the phenomena of heat has been extended." Thus, for instance, first of all, the thermometer, or the measurement of heat; then the calorimeter; then "the investigation of those relations between the thermal and the mechanical properties of substances which form the subject of thermodynamics." Then follow chapters on the Dissipation of Energy, and on the hypothesis that the motion of the molecule constitutes the heat of bodies. There is certainly a good deal to be discussed, as Dr. Maxwell treats them, in the space of a small manual. We can therefore understand why the author has been obliged to omit many other questions in the science of heat. But we do not think that he was quite right in doing so in a book intended especially for the working class, and we cannot quite congratulate him on the result, excellent though we admit it to be.

PHRENOLOGY.*

OF the works which have been published on this subject for the last few years, the present one is at once the most presumptuous and the most ignorant. Since the time of Gall and Spurzheim there have been very few, with the exception of Dr. Combe, who have attempted a scientific argument for phrenology; and of all the writers who have since touched the subject, we fancy the author of the present treatise is at once the most presumptuous and the most ignorant of all those purely scientific data on which the argument for phrenology should rest. If ever a subject required an intimate acquaintance not only with anatomy and physiology, but with insanity in its every phase, assuredly that subject is phrenology. But these subjects have virtually no place in the present volume. We do not deny phrenology in the abstract. There may, of course, be such a thing as an organ of the brain for special faculties, but assuredly no such organ has as yet been made out. Indeed there has been a search of late for an organ of speech, which is supposed to lie in the anterior part of the cerebral mass; but as yet, though much valuable matter has been written upon the subject, it is not yet clearly shown whether it is in the right or the left half of the brain that it is situate. And assuredly there is no further attempt at a specialisation of the brain's functions. Even that at one time supposed function of the cerebellum is now thrown into very serious doubt. If there was the least shadow of scientific method about the book, we should have attempted a slight notice of it; but there is really nothing in its pages which would satisfy the merest tyro in medical science. It is a miserable attempt to justify a department of scientific thought which, whatever may have been the claims of its first originators, has now dwindled down to the very

* "Phrenology, and How to Use it in Analysing Character." By Nicholas Morgan. London: Longmans & Co., 1871.

lowest depths of an all but defunct philosophic speculation. We are more surprised at Messrs. Longmans issuing such a work than we are at finding the author's portrait forming the frontispiece.

TABLES OF LOGARITHMS.*

THIS is a small work, but calculated to be exceedingly useful to computers, and more particularly to nautical men and surveyors. We find in it proportional parts of all numbers up to 100; three-place logarithms of numbers and the trigonometrical functions; logarithms of numbers to four places; Gaussian logarithms; traverse table; correction for mid-latitude; meridional parts, &c.; as well as a useful table of constant, with their logarithms. Prof. Peirce mentions an interesting result of experiments conducted at the office of the American Ephemeris. The times occupied in doing the same piece of work by tables of 4, 5, 6, and 7 places, were found to be proportional to the numbers 1, 2, 3, and 4. For many purposes four-place tables ensure a perfectly adequate degree of accuracy. We are much pleased with the way in which the practical questions of size and form of type, arrangement of page, quality of paper, &c., have been attended to. The guiding lines are numerous and well-marked; in fact, that fruitful source of error in computation—mistakes in following lines or in running down columns—seems practically eliminated. Some of the symbols are not altogether pleasing. The use of the symbol *n* for *factorial n* may appear unusual to English readers, though not unknown to continental mathematicians; *csc* for *cosec* is obviously an improvement; but why *ctn* for *cot.*?

A MANUAL OF SCIENTIFIC ENQUIRY.†

HERE is a scientific work which has been some time in existence—seeing that the present is the fourth edition—which is a most valuable book, and which we should wish to see somewhat enlarged, and far more generally circulated among naval and military medical officers, and other medical men who are travelling abroad. It is a book which is essential to every traveller who wishes to profit by his trip, for we know of no other source whence he can obtain the amount of useful information which is contained in the pages of its several departments. The present edition has been brought out by the Rev. Robert Main, M.A., F.R.S., and in most of its departments it is as full as its space will admit of with facts, and with facts arranged in such a manner as to give the reader the best general idea possible on the subject. But all the chapters are not alike, some being very good and others equally

* "Three and Four-Place Tables of Logarithmic and Trigonometric Functions." By James Mills Peirce. Boston: Ginn Brothers, 1871.

† "A Manual of Scientific Enquiry, prepared for the use of Officers in Her Majesty's Navy, and Travellers in general." 4th edition. By the Rev. Robert Main, M.A., F.R.S. London: John Murray, 1871.

defective. That on medicine and medical statistics, by Dr. Aitken; that on seismology, by Mr. Robert Mallet, F.R.S.; that on botany, by Dr. J. D. Hooker, F.R.S.; that on terrestrial magnetism, by Sir Edward Sabine, F.R.S.; that on astronomy, by G. B. Airy, P.R.S.; and that on hydrography, by Admiral G. H. Richards, F.R.S., are all excellent of their kind. But those on zoology, geography, ethnology, and some of the others, are not at all as advanced as we should like. In our opinion, the work is too small, and we cannot but believe that a larger and more important work on the same general plan would be of great advantage to the young medical man, whether in the army or navy. As the book is, it is a very capital one, and deserves great praise. In conclusion, we may mention that it is published by authority of the Lords Commissioners of the Admiralty.

SHORT NOTICES.

The Amateur's Flower-Garden, by Shirley Hibberd. London: Groombridge, 1871. Although we do not very much approve of the plans of arranging a garden which Mr. S. Hibberd suggests, we admit that we are in the minority, for the great mass of people adopt the plans, or something like them, which he recommends. This fact, and the circumstance that he gives abundant information about the garden and its plants, must contribute to make the present work a very popular one. It is essentially addressed to the amateur, and its numerous woodcuts and very excellent coloured life-size illustrations will, we doubt not, make it a very popular book. It is elegantly turned out by the publishers.

The Mysteries of the Vital Element. by Robert H. Collyer, M.D. 2nd edition. London: Renshaw, 1871. It is a pity that Dr. Collyer has written so much on the subject he has taken up. He writes to show us that he was really the discoverer of chloroform, but he has thoroughly mystified his case by the alarming amount of matter he has brought to bear upon it. Supposing we admit him to have been the discoverer, what then? If he had not been in existence, it would still have been discovered, and that, too, about the same time. Why did he not follow up his original discovery, instead of travelling about every part of the world. We give him the credit of discoverer; but we would ask him to go a little further, and deserve something better. We should not like to put down the multitude of deaths that have arisen from the use of chloroform. Dr. Collyer thinks he will do something with nitrous oxide. We hope he may, but we have not very much hope. It is useful for short operations, but we fear it can never be made safe for long and serious ones.

Notes on Comparative Anatomy, by William Miller Ord, M.B., Assistant Physician to St. Thomas's Hospital. London: Churchill, 1871. Dr. Ord proposes to divide this book into three parts, and to give one in each of three successive years. The plan is a good one, and likely to be attended with successful results. The work is essentially a syllabus of lectures, and for this reason is more a work for the lecturer than for his students. At all events, it is quite unfitted for a junior student to read. We have gone through it carefully, and we are thoroughly satisfied with the manner in which the author has discharged his task. No more space is given to one group than to another, and in all there is every reason to be satisfied with the method of classification. Dr. Ord has given very fairly, it seems to us, the anatomy and physiology of the different groups. It will greatly facilitate good lectures for students, and will greatly expedite the labours of those who are reading for honours. If the author had appended to each chapter the titles of the books and journals where the principal points were to be found he would have done well. The Protozoa and Cælenterata are especially good, and they are generally atrociously badly given in most similar works.

The Discovery of a New World of Being, by George Thomson. London: Longmans, 1871. We have failed to discover the new world which Mr. Thomson has found out for us, but we suppose it is all right, nevertheless, and that Mr. G. Thomson is fully acquainted with it. It has struck us through our reading the book which Mr. Thomson has written, that it is singularly like the arguments we have heard urged in grave seriousness by men who were about the last we should have considered as authorities on the subject in question. Listen to his views of Mr. Darwin. "We shall not enter upon a discussion of his development theory; it would occupy too much time. We may say by the way, however, that there is a great deal of truth in his observations of facts, *but nothing new; his inferences, on the other hand, are outrageous.* There are such things as a struggle for life, a natural selection, and a development. These, however, have been observed and repeated *in a thousand forms* before Mr. Darwin was born. Development, however, is always *one-sided*;" and so on to quite another subject. We merely give the quotation as a sample of the author's general knowledge of scientific subjects. As to the general tone of the book, we should be afraid to give a candid opinion.

A Manual of Anthropology, based on Modern Research, by Charles Bray. London: Longmans, 1871. Mr. Bray gives a most absurd title to his book. No one could possibly imagine from it the character of the 350 pages which constitute it. It is not at all anthropological. It is a most absurd defence of phrenology by a man who has no claim to be considered either an anatomist or physiologist. Of phrenology may well be said what one of our ablest modern thinkers has said, viz. "that those who have carefully investigated the structure and functions of the nervous system should have long ago turned their backs on phrenologists is not to be wondered at." Mr. Bray's phrenology is not based on modern research, nor on ancient investigation; it is merely on the author's reasoning from facts which he is insufficiently acquainted with.

A New View of Causation, by T. S. Barrett. London: Provost & Co., 1871. This is a small but clever book by one who very fairly appreciates the views of Mill, Bain, and Lewes. Whether he succeeds in his argument for a new view of causation we shall not say. The book can easily be read in the course of a single evening, and we heartily commend it to our subscribers. It is a well-written and ably thought-out work.

Profitable and Ornamental Poultry, by Hugh Piper. London: Groombridge, 1871. This is a very good book, and those who care for poultry will find it full of valuable information on the subject of the different breeds, and upon the rearing, fattening, laying, feeding, and general mode of keeping all kinds of fowl, from bantams to guinea-fowl, turkeys, ducks, and geese. There are several coloured plates, which are most creditable to the artist.

Spiritualism and Animal Magnetism, by G. G. Zerffi, Ph.Dr. London: Hardwicke, 1871. While we heartily agree with the author in believing that spiritualism is a humbug which ought to be put down, we cannot accept his doctrine of electric action. It is all very well as an hypothesis, but there is not the shadow of a physiological or anatomical proof in its favour. His diagram in the commencement is not creditable. It is based upon a poor assumption.

Description of an Electric Telegraph, by Sir Francis Ronalds, F.R.S. 2nd edition. Williams and Norgate, 1871. The author shows that so early as 1816 he had anticipated many of the discoveries since made. It is very creditable to him that he should have been so early in the field, but it is a pity he did not carry on his researches more fully.

The Discovery of the Nature of the Spleen—by Dr. H. R. Silvester. London: Churchill, 1870—we think we have noticed before. The author tries to prove that the spleen is the left remnant of the liver. And, in fact, his diagram, which completes the intestine by making it come from both sides of the stomach, and one of the tubes end in the vermiform appendix, is very ingenious. We fear, however, that comparative anatomy is against the doctrine, which nevertheless deserves consideration.

Darwinism—by Chauncey Wright. London: Murray, 1871—is a very clever but short essay, attacking Mr. Mivart's "Genesis of Species." We shall notice it more fully in a future number.

Flint's Fancies and Facts—by Dr. Robinson. London: Longmans, 1871—is a pamphlet on a subject which the author sadly wants education in.

Genesis and Geology—by the Rev. G. Henslow, M.A., F.L.S.—is, though short, a very masterly and readable sermon by a thoughtful student of development.

Natural Disease. London: Longmans, 1871. This is a most thoughtful pamphlet on the subject of epidemic disease, and especially of small-pox. It will well repay perusal, and though it is anonymously, it is nevertheless ably written.

A Review of Mr. Darwin' Theory of the Origin of Man—by James B. Hunter, M.D. New York: Appleton, 1871—is a clever and thoughtful essay.

Natural History Transactions of Northumberland and Durham. Vol. IV. Part I. Williams and Norgate, 1871. A most valuable work. As good as any preceding number. Full of valuable papers on zoology and palæontology, and illustrated by a series of most admirable plates.

SCIENTIFIC SUMMARY.

ASTRONOMY.

THE *Total Eclipse of December* 12, 1871.—We have most promising news from the eclipse expeditions to India and Ceylon. Mr. Pogson, the Government astronomer at Madras, stationed during the eclipse at Avenashy, telegraphs to the Astronomer Royal that the weather was fine, and the telescopic and camera photographs successful, that good sketches were taken and good polariscopic work achieved. His telegram adds that "many bright lines were seen in the spectrum," but whether the spectrum of the prominences, sierra, or true solar atmosphere, the telegraphist sayeth not. If the corona is referred to, the news is of extreme importance, provided always that an analysing and not an integrating spectroscope were employed. Colonel Tennant had not quite such favourable weather, for he speaks of a thin mist (the same sort of weather as he had in the eclipse of 1868); yet six good photographs were taken. He was stationed on Dodabetta, near Ootacamund, the favourite sanitarium of the Madras Presidency. The peak of Dodabetta is 8,640 feet above the sea-level, and it is probable that the actual station of Colonel Tennant's party was higher than any spot at which the phenomena of a total solar eclipse have hitherto been observed. The haze cannot have been very thick when good photographs could be taken at the rate of six within two minutes. As respects the comparison of the photographs taken by Pogson's and Tennant's parties, it is fortunate that a sufficient distance separates Avenashy and Dodabetta to prevent any suggestion that the same atmospheric peculiarities would be observable from the two stations. The stations are about forty miles apart. The circumstances of elevation also are so different as to preclude all possibility of deception from this cause; and it appears from the telegrams that the condition of the atmosphere was unlike at the two stations. So that since several photographs were taken at both places, it is reasonable to expect that the question of the corona will now at least be disposed of. If photographs have been taken in South Australia, another kind of evidence, bearing closely on the question of the corona's constitution, will probably have been secured. From Mr. Lockyer's party in the north of Ceylon we have the announcement that splendid weather prevailed, and that most satisfactory and interesting observations were made. But Mr. Lockyer does not vouchsafe any information as to their nature. We hear, however, from Mr. Davis, the photographer sent out with this party at Lord Lindsay's expense, that five photographs were taken, showing an extensive corona, with persistent rifts. M. Janssen telegraphs that the spectrum of the corona demonstrates the existence of matter outside the solar atmosphere.

The spectroscopic observations will probably prove to be scarcely less important than the photographic work. Colonel Tennant, in particular, announces the complete confirmation of Professor Young's observation, that hundreds of the Fraunhofer lines—if not all—are reversed at the moment of totality. It may now be accepted as certain that the true solar atmosphere lies above the photosphere, and not below, as Mr. Lockyer supposed. This, indeed, was accepted as demonstrated, by nearly all who read Professor Young's account of his observation. But it seems to be becoming a rule that all facts relating to the solar surroundings should be demonstrated two or three times before being definitely accepted.

Records of former Total Eclipses.—A most important addition has been made to our knowledge respecting the phenomena of eclipses by the collection of all narratives and pictures relating to the eclipse of December 1870, and to the "Himalaya" eclipse of 1860. This work has been mainly carried out by Mr. A. C. Ranyard, one of the honorary secretaries of the Organising Committee for the former eclipse. We understand that great light has been thrown on the question of the corona (so lately a *vexata quæstio*), by the comparison of a vast array of pictures.

But then the Treasury unfortunately declines to sanction any expenditure of the public money for publishing these valuable records, embodying not only the results obtained by the two expeditions which the Government was good enough to assist, but also the fruits of many months of patient labour. Surely the capacity of our Government for "declining to sanction" is worthy of attention—not, perhaps, altogether admiring.

Amazing Solar Outburst.—On September 7, 1871, Professor Young, the eminent American spectroscopist, whose discoveries during the American and Mediterranean eclipses have been honoured by such careful European confirmation, observed the most remarkable solar eruption yet witnessed by astronomers. The circumstances are detailed elsewhere in these pages. It is probable that they will receive confirmation ere long; but in the mean time they may be received without a particle of hesitation by all who are not working in the same field.

The Solar Corona.—In the second part of his paper on this subject (supplementary number of the "Monthly Notices") Mr. Proctor considers the evidence derived from the microscopic and chemical analyses of meteorites in favour of the theory that matter is propelled from our sun and his fellow suns in such sort as to pass away beyond their domain. Combining this evidence with Zöllner's observations of brilliant linear flashes passing over the whole length of the dull spectrum on which the prominences are seen by Dr. Huggins's method, and with the results of the researches of De la Rue, Stewart, and Loewy into the behaviour of the solar photosphere, he infers that there is sufficient reason for considering with attention the eruption theory of the corona. The bearing of Professor Young's observation on this somewhat startling theory will at once be recognised. At the last meeting of the Royal Astronomical Society Mr. Proctor read a paper discussing this point; and afterwards Mr. Ranyard mentioned that Professor

Stokes regarded the polariscopic observations of the corona as seemingly implying the existence of minute metallic crystals, situated and moving much as the theory advocated by Mr. Proctor appeared to require. It should be added that Mr. Ranyard had himself inferred, from polariscopic observations of the zodiacal light by Mr. Burton, that either "it consists of matter in particles so small that their diameters are comparable with the wave-lengths of light, *or* it consists of matter capable of giving specular reflexion." Either view (or the two views accepted together) would singularly confirm the expulsion theory.

The imagined Change in the Nebula surrounding Eta Argûs.—Mr. Lassell, President of the Astronomical Society, has expressed the opinion that Mr. Abbott's evidence is altogether insufficient to establish the imagined changes of this wonderful nebula. Mr. Proctor, who had been (as was the Astronomer Royal) one of the believers in a change, has expressed his full agreement with Capt. Herschel's explanation that Mr. Abbott had completely misunderstood what he was looking at. The *lemniscate* is, in fact, far too minute a feature for the aperture and low powers employed by Mr. Abbott. It is a most unfortunate thing that the time of Sir J. Herschel, Airy, Lassell, and many others should have been wasted through Mr. Abbott's carelessness and over-confident assertions.

Encke's Comet.—Mr. Carpenter, of the Greenwich Observatory, remarks respecting this object, as seen with the transit circle and the great equatorial, "I was able to make out a considerable extension of the illumination beyond the bright fan-shaped condensation, but on one side (the spreading side) only. On the opposite side this diffused illumination appeared to be cut off nearly in a straight line immediately behind the apex of the fan." He adds that "the comet was easily seen in the finder of the great equatorial."

Inferior Conjunction of Venus.—On September 26 Captain Noble observed Venus 1 h. 37 m. after she had passed her inferior conjunction. The state of the atmosphere was too bad to admit of any attempt at micrometrical measurements, but he estimated that her illuminated crescent occupied a little more than a third of her circumference. He remarks, "My chief reason, however, for putting this observation on record is that I quite failed to see the dark body of the planet, which, under analogous conditions, has always been visible enough before in a constricted field. Doubtless the wretched state of the air may have had something to do with it; but is it possible that the bright background (whatever it may be) on which Venus may be projected, varies in lustre?" We should say it is not merely probable, but certain, that the lustre of the corona is variable.

The Construction of the Heavens.—Mr. Proctor, speaking of the evidence respecting the construction of the heavens derived from his chart of 324,198 stars, remarks ("Monthly Notices" for November), "Struve's general conclusion that the stars of the first nine or ten orders of magnitude are more densely aggregated along the galactic zone is abundantly justified. But instead of a gradual increase of density such as his statistics suggested, we recognise in the chart a distinctly marked aggregation within those very regions of the heavens where the Milky Way is brightest to the eye. In other words, we have clear evidence that it is not towards a certain zone that the stars are gathered, but into those irregular cloud-like masses, those streams, projections, and interlacing branches, which constitute the Milky Way as it is actually presented on clear nights to our study." "In the chart, however, we see the projections carried much farther away from the main branch." "A circumstance of some interest is to be recognised in the fact that the branching extensions are found to lead, in almost every instance, towards regions of the heavens where many nebulæ exist."

A New Southern Observatory.—It is probable that before many years are passed astronomers will be able to extend the system of charting advocated and practised by Mr. Proctor, to the southern heavens. At the Cape observatory Mr. Stone is already busily at work reducing the observations made during the last fourteen or fifteen years. Dr. B. A. Gould writes to the Astronomer Royal respecting the present position of the work at the new observatory of Cordoba. The special instrumental observations which Dr. Gould has in purpose have not been commenced, the observatory not being yet sufficiently complete; but considerable progress has been made in Uranometry. Dr. Gould has a catalogue of over 7,100 stars visible to the naked eye on good nights, reaching to the magnitude 6·6. The space within which these stars lie is about ·587 of the complete celestial sphere; and therefore the numerical richness corresponds to about 12,100 stars for the whole heavens—a much larger number than is usually supposed to be visible to the naked eye. It will be interesting to notice whether the seemingly remarkable richness of the southern heavens as thus newly surveyed corresponds to the peculiar distribution noticed by Mr. Proctor, who remarks, in the Preface to his larger Star-Atlas, that 1,132 stars may be counted in the south polar maps, as against fewer than 400 in an equal northern area.

On October 17 the observatory is to be formally inaugurated, and Dr. Gould has good reason to believe that within a month from that day he will be able to begin the work upon the zones.

He remarks incidentally that hitherto he has only been able to study the great Argo nebula with "a telescope of about five inches in aperture" (much larger than Mr. Abbott's), "placed upon the roof of his house. But such observations as I have hitherto been able to make, compared with the drawing in Sir J. Herschel's 'Cape Observations,' have tended strongly to impress me with the conviction that the alleged change is altogether imaginary."

Planets for the Quarter.—Venus will be a morning star, and, on the whole, favourably placed for observation at the beginning of the quarter. Jupiter and Uranus come to opposition respectively on January 15 and 19, and are therefore very favourably situated for observation. Neither Mars nor Saturn will be well placed, Saturn being in conjunction on January 3, and Mars passing onwards to conjunction (which will take place on May 17). Mercury reaches his greatest westerly elongation on January 24, his greatest easterly elongation on April 5.

BOTANY AND VEGETABLE PHYSIOLOGY.

The Flora of Brazil.—Part 50 of the great work of Martius, continued by Eichler, was issued some time since. It is occupied by Mr. Bentham's continuation of the *Leguminosæ* (viz. *Swartzieæ* and *Cæsalpineæ*), in 254 pages of letter-press, fully illustrated by 66 plates. The *Swartzieæ* form now only the ultimate tribe of *Papilionaceæ*. Among the figures are to be found one of *Guilandina Bonducella* and of a related species, now reduced to *Cæsalpinia*, of *Parkinsonia aculeata*, and of *Cassia Chamæcrista*. There are 189 Brazilian species of *Cassia*, no small part of the genus, the much desired monograph of which, prepared more than two years ago by Mr. Bentham, is probably now printed.

The Structure of Lepidodendra and Stigmaria appears to have been very fully made out in a paper read before the Royal Society by Professor Williamson. In this paper he describes the structure of specimens of *Lepidodendra selaginoides*, and appears to make good the conclusion that it has an imperfect exogenous structure. He observes that it has a central medullary axis, which is closely surrounded by a second and narrower ring also of barred vessels, but of smaller size, and arranged in vertical radiating laminæ, "which are separated by short vertical piles of cells believed to be medullary rays. In a transverse section the intersected mouths of the vessel form radiating lines," and the structure is pronounced an early type of an exogenous cylinder. From this cylinder alone the vascular bundles going to the leaves are given off. He describes *Stigmaria* ("well-known," he says, "to be a root of Sigillaria") as having "a cellular pith without any trace of a distinct outer zone of medullary vessels such as is universal amongst the *Lepidodendra*. The pith is immediately surrounded by a thick and well-developed ligneous cylinder, which contains two distinct sets of primary and secondary medullary rays." Other facts stated tend to show that these plants are of the Lepidodendroid type, and Professor Williamson therefore includes the *Lepidodendroid* and *Sigillarian* plants in a common family, making them, along with the *Calamiteæ*, to constitute an *Exogenous* division of the vascular Cryptogams, while the *Ferns* belong to an *Endogenous* division, "the former uniting the Cryptogams with the Exogens, through the Cycadeæ and other Gymnosperms, and the latter linking them with the Endogens through the Palmaceæ.

Coccoliths not Animals, but Plants.—The recent investigations of Mr. Henry Carter, F.R.S., would seem to put it beyond doubt that these are vegetable organisms. They are what Professor Huxley first thought them to be, not what he subsequently supposed in connection with his *Bathybius*. Mr. Carter says, considering that the coccolith is so abundant in the Laminarian zone, and so voraciously fed on by the Echinodermata and Ascidiæ; also that it is so nearly allied to *Melobesia calcarea*, that it forms the bed of the Atlantic, and is found fossilised in the chalk, he cannot help inferring that it is a vegetable organism which contributes chiefly to form the calcareous deposits of the present day as it has done in the past, at all events in the chalk.

Sculpture of Seeds.—Professor Lange, of Copenhagen, has published in *Botanisk Tidsskrift* (in Danish) an interesting paper upon this subject, which, as botanists know, often furnishes excellent characters to distinguish otherwise similar species. He treats here of *Pyrolaceæ*, *Droseraceæ*, *Cerastium*, and especially of *Pedicularis*, with illustrations. In two plates, filled with beautiful coloured figures, the seeds of 25 species of *Pedicularis* are strikingly depicted.

The Study of Minute Fungi.—Some very useful practical remarks on this subject appear in the "American Naturalist," by Dr. J. S. Billings. He says the attempt at a physiological classification of these organisms is as yet premature, the mere morphological classification being still so very incomplete that it is impossible, from published descriptions, to identify much more than half of the minute fungi which have been described, while a vast number have been collected and named which have never been described at all. He does not, therefore, recommend the microscopist who proposes to undertake this study, to try to do more at first than to recognize genera, and he furthermore advises him to confine his work for a time to half-a-dozen species which he can get named for him by some one who has the necessary facilities for so doing in the shape of identified specimens. For instance, having ascertained that he has a specimen of *Valsa stellulata*, let him first see whether he can get the spores to germinate. First, he may try them with a little water on some form of growing slide, the simplest form of which is to take the slide with the spores on it covered with a piece of thin glass just as he has been examining it under the microscope, and laying it across a narrow dish of water (a soap-dish or toothbrush-dish is just the thing) let two or three threads lead from the water to the edge of the thin glass cover. The growing slides of Hoffman, De Bary, Dr. Maddox, and those described by Dr. Curtis and the author in their report on fungi in connection with the Texas cattle fever, are all good and useful. The spores should be tried not only in water, but in fluids which will afford them some nutriment, such as juice of fruits or plants, Pasteur's fluid, or on such media as a slice of potato, or blotting-paper soaked in lemon juice.

Hypercotyledonary Germination is a somewhat rare event. Dr. Asa Gray says (in "Silliman's American Journal") his attention has been called, by Mr. Guerineau, the gardener of the Cambridge Botanic Garden, to a remarkable instance, which occurs in all their seedlings of *Delphinium nudicaule*, the unique red or red-and-yellow-flowered species of California. As this species is now in European cultivation, and a probable variety of it, *D. Cardinale*, was raised and figured in England several years ago, the peculiarity in question is likely to have been noted; but he has seen no account of it. In germination, the slender radicle elevates a pair of well-formed ovate cotyledons in the usual way. These acquire full development; but no plumule appears between them; consequently the primary axis is here arrested. Soon a nassiform thickening is formed underground at the junction of the lower end of the radicle with the true root: from this is produced a slender-petaled 3-lobed leaf, which comes up by the side of the primary plantlet; soon a second leaf appears, and so on, setting up the permanent axis of the plant from a bud which thus originates from the very base of a well-developed radicle, if not from the root itself.

Baillon's "Histoire des Plantes."—This fine work, though it has made considerable progress during the year 1871, is yet unfinished. Two monographs were issued—those on *Menispermaceæ* and *Berberidaceæ* with 73 illustrations, and on *Nymphæaceæ* with 34 illustrations. M. Baillon follows our chief English botanists in referring the *Lardizabaleæ* to the *Berberidaceæ*. Some other peculiarities are of importance. The author does not seem to have noticed the fact pointed out in "Silliman's American Journal," that Podophyllum occasionally exhibits more than one carpel, and he signifies a doubt whether the pulpy investment of the seeds is an arillus. There are some other peculiarities in the work which are worthy of notice by the English botanist, more especially as Lovell Reeve and Co. publish an English edition of it.

The Arrangement and Morphology of the Leaves of Baptisia perfoliata.—A paper on this subject has been read by Prof. Gray for Mr. Ravenel at the last meeting of the American Association. It had been hastily supposed by Prof. Gray that the leaves were five-ranked, and became one-ranked by a continuous torsion of the stem. Mr. Ravenel points out that the phyllotaxis of the plant in question is really of the two-ranked order, which inspection of the growing shoots makes abundantly clear, and that they become one-ranked by the alternate twisting of the successive internodes right and left, *i. e.*, one twists to the right, the next as much to the left, the next in the opposite direction, and so on, thus bringing the leaves into a vertical position all on one side of the horizontal branch. It occurred to Mr. Ravenel that this vertical position of the leaves was correlated with the remarkable alternate torsion of the axis, namely, that the leaves on the reclining branches were adjusting themselves so as to present their two faces as equally as possible to the light, as is done by those of the compass plant in a different way; and that it was therefore probable that the stomata would be found to be as numerous on the upper face of the leaf as on the lower. A microscopical examination proved the correctness of Mr. Ravenel's conjecture; the stomata are about equally numerous on the two faces. Whether the leaves take a vertical position because the stomata occupy both surfaces, or whether the stomata are so distributed because the leaves stand edgewise to the zenith, is a question. The fact is that the two are thus correlated, and such correlation is ordinarily essential to the well-being of the plant. It may be remarked, however, that the stomata do not manifestly appear until the leaf is pretty well developed. Also that this distribution of the stomata is peculiar to the species in question.—*Silliman's American Journal of Science*, October.

The Sun-dew (*Drosera*) *as a Fly-catcher.*—A lady, Mrs. Treat, writes of the properties of this plant as follows. She says, "For several summers I have taken *Drosera rotundifolia*, *D. longifolia*, and *D. filifolia* from their moist beds, and placed them in sand and water in such a way that they made most charming window-plants. What I take for *D. longifolia* has spatulate, oblong, reddish leaves, and long erect reddish petioles covered with glands like those of the leaf. This species I find a much more effective fly-trap than *D. rotundifolia*, the name of the plant in my window this summer: almost every leaf held a common housefly prisoner until it died, and it did not take the leaf very long to fold amply round its victim. My husband was terribly shocked, and thought it the most cruel thing he ever saw in nature; but with my prepossessions and habits, both as an entomologist and a housekeeper, I was contentedly interested to see the work go on."

Changes in the Position of Grains of Chlorophyll by Sun-light.—M. Borodin has found that this takes place in the higher cryptogamia and in the phanerogamia both aquatic and terrestrial [see *Ann. des Sciences naturelles*, sec. 5 to 12]. *Lemna, Ceratophyllum* and *Collitriche* are among the aquatic plants in which the phenomenon has been observed, and *Stellaria media* among terrestrial. *Lemna trisulca* is one of the best plants for these observations. Under diffuse day-light the grains of chlorophyll are distributed over the cell-walls parallel to the surface of the leaf or frond. Under the direct light of the sun they are rapidly (within 15 minutes or less) transported to the lateral walls. There they are at first uniformly distributed. But upon longer insolation, say for three-quarters of an hour, they became grouped in clusters. In darkness the chlorophyll is likewise upon the lateral walls. Thus absence of light produces essentially the same effect as direct sunshine, but less strikingly. Whether these changes are passive and caused by movements of the colourless protoplasm, as Sachs supposes, or active, is not made out. But the movements, according to Borodin, are in response only to the more refrangible rays.

Decomposition of Carbonic Acid by Leaves.—M. Deherain has published an important paper on this subject and *synopsis* of the amount of water which evaporates. Among some of his more important conclusions are the following:—(1.) That the transpiration of water continued indefinitely, and quite constantly, in a saturated atmosphere. (2.) This evaporation, copious in light and almost null in darkness, is determined by the light, and not by the heat of the sun. (3.) It is much greater from young leaves than from older ones. (4.) And is mainly caused by the luminous rays (yellow and red). (5.) The difference in this respect is manifest even when the less refrangible and more refrangible rays are brought to an equal luminous intensity. (6.) The evaporation of water is much more copious from the upper than from the lower face of the leaf.

CHEMISTRY.

Action of Gun Cotton on Camphor.—Professor C. A. Seeley recently called the attention of the Lyceum of New York to a property of gun cotton which he considered new, or at least unrecorded. It was well known that gun cotton was soluble to a very considerable extent in alcohol which held gum camphor in solution. A knowledge of this fact had been made use of in the arts, for the manufacture of an artificial ivory, which was reported to be fully equal, if not superior, to the genuine article. In making this substance, the gun cotton is ground up with the gum camphor, by means of water, into a pulp, and then pressed into a solid mass, whilst being heated to a temperature of about 300° F. Taking this fact into consideration, he had thought of ascertaining what would be the effect of exposing gun cotton to the action of the vapour of gum camphor. Therefore a small quantity was thus exposed in a glass tube; the camphor being heated to a temperature just high enough to volatilise it. He was surprised to find that after a short time the tube became filled with red vapours, and ultimately the gun cotton exploded. Now, as in the manufacture of the artificial ivory mentioned, the mixture of gun cotton and camphor is exposed to a much higher heat than that he had made use of, it was a fact worth taking into consideration, as to whether there might not be danger of explosion resulting. It is true that, ordinarily, there is water present, but towards the end of the process this is all pressed out, and a dry mass is left. Therefore, as a precaution, it is as well to remember this fact thus ascertained.

Chlorine in Meteors.—Mr. J. W. Mallet, in an important paper on meteors in "Silliman's Journal," says he feels satisfied that the chlorine is not of meteoric origin—not an essential constituent of the original masses—but has been derived from the soil in which the iron has lain imbedded. The exudation of watery drops containing metallic chlorides is observable only at points on the outside and on cut surfaces along the lines of fissures communicating with the outside. Although chlorine is mentioned above as found in the general analysis of the planing-machine shavings, he failed altogether to detect it in a specially-selected solid piece of some fifty grams taken from a part destitute of fissures or flaws.

Synthesis of Oil of Rue.—Herren Gorup-Besanez and Grimm have lately succeeded in forming oil of rue synthetically ("Ber. Berl. Chem. Ges.," iii. 518, and "Chemical News"). Starting from the fact first suggested by Hallwachs, and confirmed by Harbordt, that this oil is a mixed ketone of the formula $\mathrm{CO}\left\{\begin{matrix}\mathrm{C_9H_{19}}\\\mathrm{CH_3}\end{matrix}\right.$ since it is oxidised with difficulty and yields capric acid, they distilled together equal molecules of pure dry calcium caprate—prepared from a Hungarian wine-fusel-oil—and calcium acetate. The mixture melts, swells up, blackens, and evolves at first a fluid smelling like acetone, but afterwards an oil which solidifies in the neck of the retort. By fractioning this distillate, three products were obtained: one boiling below 200°; another boiling between 210° and 245°; and a solid body, caprinon, boiling above 300°. The second fraction was purified by conversion into the double salt of ammonium sulphite, crystallizing from alcohol, and decomposing by sodium carbonate; a colourless, strongly refracting oil rose to the surface, which, when dried, distilled completely between 223° and 224° and had at 17·5° C. a specific gravity of 0·8295. Commercial oil of rue, treated in the same manner, afforded a liquid distilling between 224° and 225·5° and having at 18·7° C. a specific gravity of 0·8281. Analysis of the ammonium double sulphite prepared from both, and of the pure oil both artificial and natural, gave the same result. Hence oil of rue consists essentially of methyl-caprinol or nonyl-methyl ketone.

Impurities present in Reduced Iron.—The "Neues Jahrbuch für Pharmacie" has a paper by Herr E. Schering, which is abstracted in the "Chemical News." While this substance (reduced iron) has hitherto not been obtained quite free from a more or less large quantity of sulphuret of iron, the author calls attention to the fact—an important one, indeed—that he has met with samples of this medicament which, in addition to the impurity spoken of, also contain oxides and carburets of iron, and even cyanide of potassium, due in all likelihood to the reprehensible practice of preparing the reduced iron from the residues of the preparation of cyanide of potassium from the ferrocyanide of potassium. It is therefore advisable for pharmaceutists to test the *ferrum hydrogenio reductum* they purchase for the presence of the cyanide alluded to.

Influence of Heat on Bromine.—Professor Andrews read a paper before the British Association on "The Action of Heat on Bromine." If a fine tube, he said, is filled one-half with liquid bromine and one-half with the vapour of bromine, and, after being hermetically sealed, is gradually heated till the temperature is above the critical point, the whole of the bromine becomes quite opaque, and the tube has the aspect of being filled with a dark red and opaque resin. A measure of the change of power of transmitting light in this case may be obtained by varying the proportion of liquid and vapour in the tube. Even liquid bromine transmits much less light when heated strongly in an hermetically sealed tube than in its ordinary state.

A Manganese Deposit in a Well.—Dr. Emerson Reynolds submitted to the British Association (Edinburgh) the result of an analysis of a singular deposit from well water. He stated that in the examination of a sample of well water used in mashing paper pulp, in a mill near Dublin, he had found that a black deposit formed in the water consisted almost wholly of an oxide of manganese. This deposit he found arose from the gradual oxidation of manganous carbonate, present in extremely minute proportion in solution in the water.

The Direct Substitution of the Alcohol Radicals for the Hydrogen in Hydric Phosphide.—In the "Berichte der Deutschen Chem. Gesellschaft" (4ter Jahrgang, p. 205), there is an able account of the above peculiar process. Absolute alcohol heated with iodide of phosphonium yields hydric phosphide, ethylic iodide, and water. Hofmann has employed this reaction in a beautiful process for obtaining the iodides of triethyl and tetrethyl-phosphonium, which consists in simply heating one molecule of iodide of phosphonium with three molecules of absolute alcohol in a sealed tube for six to eight hours at 180° C. Under these circumstances, the ethylic iodide acts directly upon the hydric phosphide to form the iodides of the substituted phosphoniums. After cooling, the tube is found filled with a beautiful snow-white crystalline mass, which dissolves in water to a perfectly colourless solution. The crystals are a mixture of about equal proportions of the iodides of triethyl and tetrethyl-phosphonium. A solution of sodic hydrate separates triethyl-phosphine as a colourless layer of liquid. The solution then gives, on evaporation, beautiful crystals of the iodide of tetrethyl-phosphonium; the triethyl-phosphine, as separated by means of a funnel, is chemically pure. The iodides of trimethyl and tetramethyl-phosphonium were easily prepared by the same process. In like manner allylic alcohol, phenol, and glycerin gave promise of a rich harvest of new results.

GEOLOGY AND PALÆONTOLOGY.

What is the cause of Thermal Springs?—This question is asked in a paper in the "Geological Magazine," by Mr. Henry Woodward, F.G.S. He thinks that water descending to deep levels in the strata meets at some point with steam at a high temperature, which, being converted into water by contact, raises the temperature of the water, which in turn, as the store of heat is accumulated, rises by rents and fissures to the surface in the form of thermal springs. There seems no doubt that hot springs have a direct connexion with volcanoes. 1. Hot springs are present in all volcanic areas. 2. Where not connected directly with volcanoes, they are found situated, as in the Pyrenees, the Alps, and the Himalayas, upon lines of dislocation and disturbance, where volcanic force, if not visible at the surface, has been in operation far down beneath. 3. Hot springs distant from volcanic disturbances are nevertheless affected by them. Thus the "Source de la Reine," at the baths of Luchon, in the Pyrenees, was raised suddenly during the great earthquake of Lisbon in 1755, from a tepid spring to 122° Fahr., a heat which it has since retained. Although springs, as a rule, carry carbonate of lime and sulphate of lime in solution, the hotter thermal springs alone contain large quantities of silica in solution. For example: the hot spring of St. Michael, in the Azores, having a basin 30 feet in diameter, is surrounded by layers of travertin many feet in thickness, composed of siliceous matter deposited on wood, reeds, ferns, &c. The hot springs of New Zealand are, perhaps, the finest, exceeding even the Great Geyser in Iceland, which also deposits enormous quantities of silica from its waters on cooling, originally held in solution.

Injection of Crinoids by Silica.—A Silurian limestone which was recently examined by Dr. Dawson, was, says Dr. Sterry Hunt, F.R.S., found by him to consist almost wholly of comminuted organic remains, including fragments of trilobites, gasteropods, brachiopods, and joints of small encrinal stems and plates; the whole cemented by calcareous spar in a manner similar to many organic limestones. He observed, however, that the pores of the crinoidal remains were injected by a peculiar mineral, readily distinguishable in thin transparent sections, or on surfaces which had been exposed to the action of an acid, which dissolves the carbonate of lime and places in relief the injecting mineral. The minute structure thus revealed is precisely similar to that of recent crinoids studied by Carpenter, and will soon be described and figured by Dawson. Decalcified specimens exhibit a congeries of curved, branching, and anastomosing cylindrical rods of the replacing mineral, sometimes forming a complex network, which under the microscope resemble the coralloidal forms of aragonite known as *flos ferri*, and present a frosted crystalline surface. The same mineral, as observed by Dr. Dawson, occasionally occupies larger interstices among the fragments, and was evidently deposited before the calcareous spar which cements the whole mass. When this limestone is dissolved in dilute hydrochloric acid, the residue, washed by decantation, equals from five to six per cent. of the weight of the mass, and is seen under a microscope to consist entirely of the casts composed of the mineral just noticed, mixed with about one-fourth of coarse silicious sand. This matter is pale greyish-green in colour, but when calcined becomes of a bright reddish-brown, without change of form.

The Boulder Drift and Esker Hills of Ireland.—Sir Richard Griffith, F.R.S., described these to the British Association (Edinburgh), and also the position and composition of the erratic blocks of that country—a subject to which he has paid much attention for the last sixty years. By the aid of a map, he described the rocks, and showed that the boulder drift was older than any other drift. The Esker drift, he said, was formed subsequently. These drifts are a remarkable feature in the landscape in the Irish counties in which they exist. They are in the form of long ridges, which traverse the centre of Ireland from east to west, and on reaching the valley of the Shannon take a north-westerly direction. After showing the peculiar composition of these drifts, he described the immense erratic blocks which are met with in many parts of the country. All of them are angular, and had evidently been transported on ice by a current from the north-west, as the most remarkable of them are the porphyrite granite of West Galway.

Dr. Carpenter's Speculations considered by the Americans.—Dr. Carpenter's idea that we are living in the chalk formation does not find many supporters in America. In a recent paper read before the Lyceum of Natural History of New York, Dr. Newberry, the president, said "that the conclusion drawn from those discoveries, that they overturned geological classification, was simply absurd. These explorations in the depths of the ocean had proved only this: that there had been less change in the fauna of the depths of the ocean than in that of the shores, and that a few forms characteristic of the fauna of the Cretaceous and Tertiary periods continued to exist there while they had disappeared in shallower water; but these were the most insignificant possible fragments of great life-groups, that had almost entirely passed away. The finding of a crinoid or foraminifer of the Chalk living in the ocean depths did not recall the race of the great reptiles, winged, swimming, and walking; the huge ammonites and the other infinitely-varied forms of the Cephalopoda, which characterise that period. So with all the other geological ages. They were chapters in the life-history of the globe which were distinct and well-defined, holding each its relative place. Human history may repeat itself, but geological history never can."

The Carboniferous Fossils of West Virginia.—Mr. F. B. Meek, in the "Report Regents of University W. Virginia," describes some new species of fossils from the district of Monongalia Co., viz.: *Macrodon obsoletus*, of the lower coal-measures, *Nucula anodontoides*, *Yoldia Stevensoni* and *Y. Carbonaria* of the coal-measures, and *Phillipsia Stevensoni*, from the Chester group of the subcarboniferous. From a survey of the species collected, he concludes that the Chester group (of the Illinois Reports) is represented in Western Virginia by at least six Illinois species, and along with ten or a dozen other species which he could not identify because of the imperfect state of the specimens. The beds also contain *Hemipronites crassus*, a coal-measure species, and a *Cyrtoceras* and *Bellerophon*, closely like species of the coal-measures. He observes that Monongalia County is the farthest point eastward at which the Chester group, or indeed any other of the divisions of the subcarboniferous limestones of the West, has yet been recognised. The species from the lower coal-measures are mostly the same that occur in the coal series of Indiana, Illinois, Missouri, Kansas, Nebraska, &c., though few of them have before been found so far eastward. In some of the States mentioned, nearly all of the species range through the whole of the coal-measures, showing, as Mr. Meek remarks, that species lived on through a great length of time, and consequently that the climatic and other physical conditions of the era must have remained remarkably uniform.

Relation of Orders of Reptiles to Strata.—Professor Cope, who has already so much distinguished himself upon American palæontology, gives in a recently-published memoir the following tabular arrangement of this class, as regards its rocks, which contains these remarks: *Present Time*—Rhynchocephalia; Crocodilia; Testudinata; Lacertilia; Ophidia. *Pliocene*—Crocodilia; Testudinata; Lacertilia; Ophidia. *Miocene*—Crocodilia; Testudinata; Lacertilia; Ophidia. *Eocene.*—Crocodilia; Testudinata; Lacertilia; Ophidia. *Cretaceous.*—Ornithosauria; Dinosauria; Crocodilia; Sauropterygia; Testudinata; Lacertilia; Pythonomorpha.

The Geology of Salt-Lake City.—Mr. W. P. Blake has written a letter to Professor Silliman in which he describes briefly, but pretty generally, the geology of the Salt-Lake City. He says that he left New Haven hurriedly to reach the Emma Mine and examine it. It is a remarkable mine. Within a little more than a year it has yielded ore worth over $2,000,000, and this without any special outlay. It is a great mass of soft earthy-looking ore, the result of the decomposition of argentiferous galena. It is dug out with shovels and picks, sacked, and sent to Liverpool, where it sells for about $175 per ton. The mass is between strata of limestone, the middle members of a series of strata over a mile thick. The lower members are slate and quartzite, and rest upon the immense masses of syenitic granite which form the picturesque Alpine-like peaks of the Wahsatch. These strata are all much uplifted and contorted, some of the harder beds surging up into peaks at least 11,000 feet above tide. The mine is at an elevation of 8,500 to 9,000 feet. At the head of the cañon upon the side of which it is situated, there is a fine exposure of syenitic granite for about a mile, with rounded polished backs—*roches moutonnées*—probably 9,000 feet above tide. These rocks give conclusive evidence of the former existence there of a large glacier. Much of the polish upon the surface has been removed by the action of the weather. The patches that remain are dark brown in colour, while the syenite is light grey, and they show the same peculiar scale-like crusts seen on the partly weathered glaciated surfaces above the Yosemite.

Geology of the Rocky Mountain.—"Harper's Weekly," an American literary journal, gives the following account, which may be of interest to our readers. The geological expedition to the Rocky Mountain region under the charge of Dr. Hayden, after completing the survey of the Yellow Stone Valley, left Fort Ellis on September 5, passing down Gallatin Valley to the Three Forks, and thence by the Jefferson to its very source, exploring many of its branches, and pursuing a direction nearly parallel to that which the party had traversed in the June previous. The valleys of the Gallatin, Madison, and Jefferson Forks of the Missouri, with all the little branches, were found occupied by industrious farmers and miners—a contrast quite striking to the doctor, who, twelve years ago, in exploring that same region, met with not a single white inhabitant. The Rocky Mountain Divide was crossed at the head of Horse Plain Creek, from which the party passed over into Medicine Lodge Creek, following this down into the Snake River Plain.

Results of the Coal Commission.—This important Commission, which has been sitting for some years, has just published the first volume of its Report.

The English coal-fields are, of course, most especially dealt with—the Scotch coal-fields being somewhat briefly treated by Mr. J. Geddes, and those of Ireland by Professor Hull. The vast amount of practical information contained in this the first volume of the Report, shows at once the great value of the results which have been brought about by the Coal Commission. We learn that we have an accessible supply of coal that will last about 276 years, but after that the coal that will remain could never be worked except under conditions of scarcity and high price. As we approach this exhaustion, the country will by slow degrees lose the advantageous position it now enjoys in regard to its coal supply; for although other countries would undoubtedly be in a position to supply our deficiencies, it may well be doubted whether the manufacturing supremacy of this kingdom can be maintained after the importation of coal has become a necessity. It is to be regretted that the Commission express rather an adverse opinion as to the finding new coal formations, &c.

The Foraminifera of the Chalk of Gravesend and Meudon.—Professor Rupert Jones and W. K. Parker, M.D., F.R.S., have been publishing some work together, with a review of Professor Ehrenberg's researches on this subject; this paper is of peculiar value, but is too technical for further abstract.—*Geological Magazine*, November.

Death of Sir R. Murchison.—Geology has sustained a severe loss—a loss indeed which it would be difficult to calculate—in the death of Sir Roderick Impey Murchison. Although at the ripe age of eighty years, it is a loss which geologists and geographers are alike called upon to mourn. "In relation to both these sciences, he has for many years justly occupied the most prominent positions. But, apart from his high social and scientific standing, he was a man full of genial and kindly feeling, who could be readily approached; and those who knew him most intimately acknowledge that he was never known to fail his friends in the hour of need, but was ready to aid them with his advice, his influence, and his purse, as many a young scientific man amongst us can testify. Born at Tarradale, in Ross-shire, he receive his early education as a boy at the Grammar School at Durham." The work that he has done is enormous; the titles alone occupying several pages of the "Geological Magazine."

The Irish Coal-measures.—Mr. G. H. Kinahan, of the Irish Survey, read a paper on this subject before the Royal Geological Society of Ireland. This communication was a reply to a statement made before the Society in January last, in a paper "On the Ballycastle Coal-field," by Mr. E. Hull, in which it is asserted that there were true coal-measures in Connaught, while none exist in the provinces of Munster or Leinster, as laid down in the Geological Maps published under the direction of the late Mr. J. B. Jukes. The author of this paper showed that the coal-measures of Leinster, Munster and Connaught, were identical; therefore, if Mr. Hull's statement respecting Connaught was correct, his assertion as to Munster and Leinster must be wrong. He pointed out that the late Mr. Foot and himself wished to divide the coal-measures into four series, but that the late Mr. Jukes objected, and stated, "If we were to seek to force these coal-measure series into a strict analogy with those of other districts, we might look upon these lower black shales with marine fossils as the representatives of the upper limestone shale of Derbyshire; and the set of sandstones and flagstones No. 2 as the representative of the millstone grit of that county. It would, however, be *impossible* in the south of Ireland to draw any recognisable boundary subdividing the coal-measure series, and the attempt would, therefore, only tend to confusion." The author next pointed out that palæontologically the coal-measures of Kilkenny, Queen's County, Limerick, Clare, Kerry, &c., were similar to those of Coalbrookdale, Staffordshire, and other places in England.

The Geology of Clermont, Auvergne.—Mr. R. G. Symes, F.G.S. (Royal Geological Society of Ireland), has published an account of his enquiries in this district during the summer of 1870, when he visited the district in company with Mr. Leonard. The paper describes the result of their observations on the plutonic, aqueous, and volcanic rocks. The country chiefly examined was that between four and five miles west of Clermont. The granite is described as generally consisting of two micas (margarodite and lepidomelane, the latter predominating in nearly every case over the former), one felspar (oligoclase), and the other glassy quartz. It was found to decompose the more readily as it approached volcanic rocks. The aqueous rocks, of Upper Eocene or Miocene age, are briefly described as consisting of grits, marls, and indusial limestone or travertin. The grits are for the most part composed of the débris of granite and basalt, bound together by a siliceous cement. Mr. Symes obtained a specimen containing a well-rounded pebble of basalt: a fact of some importance, as Scrope and Lyell remark that no traces of volcanic rocks occur in these beds. In regard to the volcanic phenomena, the inferences drawn are: that the condition to which the volcanos are referable is that in which eruptive paroxysms of intense energy alternate with lengthened periods of complete inertness; that the cinder cones, domitic hills, and recent lavas, are all due to one violent paroxysm spread, over an area twenty miles long by two broad; that the presence of two such different rocks as basalt and trachyte, in close juxtaposition, can only be accounted for on the supposition that the rocks from which they are derived, namely hornblende rock and some highly felspathic rock (such as granite), were in contact prior to their being reduced to the forms we now find them in; that the granite plateau was very much in the same condition prior to the deposition of the lacustrine strata, as it is now; that prior to the deposition of the lacustrine strata this district was probably the seat of volcanic eruption.

Life of Professor J. Beete Jukes, M.A., F.R.S.—The life of this eminent geologist has been prepared by the loving hands of his sister. It possesses an excellent portrait, and extends over about six hundred pages. It is full of matter interesting to geologists and to those who were acquainted with Professor Jukes, but of course we cannot abstract it here. It is published by Chapman & Hall.

A new Fish, Phaneropleuron elegans, has been discovered by Professor Traquair, of Dublin, and is described by him in the "Geological Magazine" for December. It seems to be a fish very closely resembling the *Phaneropleuron Andersoni* of Huxley, but differing from it in its smaller size, in its somewhat more slender form posteriorly, in the smaller depth of the lower lobe of the caudal fin, and apparently also in the greater extension forwards of the dorsal fin. As it is evidently at least specifically distinct, he proposes to bestow on it the title of *Phaneropleuron elegans*. The genus *Phaneropleuron* is thus common to the Upper Devonian and Lower Carboniferous formations. It is one of the most interesting of the Palæozoic Ganoids, as showing the intimate relations subsisting between the ancient Crossopterygians with acutely lobate pectorals and ventrals, and the remarkable recent types *Lepidosiren*, *Protopterus*, and *Ceratodus*. One cannot fail to be struck, as Professor Huxley has already indicated, with the many points of resemblance which this genus bears to *Lepidosiren*, in the general form of the fish, in its thin circular scales, and in many points in the structure of its internal skeleton. But from the true *Dipnoi*, with which Dr. Günther now unites the true Devonian *Dipterus*, and the carboniferous *Ctenodus*, it differs, as is well known, materially in its dentition; and the position of the nasal openings, so peculiar a character in the recent *Dipnoi*, and in the fossil *Dipterus*, as Günther has pointed out, remains yet to be definitely settled. *Phaneropleuron* must therefore remain, as Professor Huxley has placed it, the type of a distinct subfamily of Crossopterygidæ, viz., *Phaneropleurini*, and not very far removed from *Holoptychius*, and other acutely lobate-membered cycliferous Ganoids of Palæozoic times.

Death of Mr. Chas. Babbage, F.R.S.—Although Mr. Babbage was more of a mathematician than a biologist, still we may notice his death in these pages, because he did, many years ago, some good work in geology. He was born December 26, 1792, and died at his residence, Dorset Street, Marylebone, on the 20th Nov., in his eightieth year. He was the inventor and partial constructor of the famous calculating engine or machine, which the world has associated with his name, and which is now preserved in the Museum of King's College, London. As a writer in the "Dictionary of Universal Biography" remarks: "The possibility of constructing a piece of mechanism capable of performing certain operations on numbers is by no means new; it was thought of by Pascal and other geometers, and more recently it has been reduced to practice by M. Thomas, of Colmar, in France, and by the Messrs. Schütz, of Sweden; but never before or since has any scheme so gigantic as that of Mr. Babbage been anywhere imagined." His achievements, says the "Times," were twofold; he constructed what he called a Difference Engine, and he planned and demonstrated the practicability of an Analytical Engine also.

Relics of the Carboniferous and other old Land Surfaces.—Mr. Henry Woodward, F.G.S., F.L.S., has contributed to the "Geological Magazine" for November an admirable essay on this subject. It is far too long for an abstract, but it will well repay a perusal. It deals with an immense multitude of facts, which are most admirably arranged together.

MECHANICAL SCIENCE.

Mr. Bessemer's Gun.—Mr. Bessemer has been directing his attention to the subject of heavy ordnance, and has matured some very novel and ingenious plans, which he proposes to put to the test of experiment. The great difficulty with heavy ordnance is the enormous initial pressure generated by the explosion of the powder, a pressure which may reach 60,000 lbs. per sq. in. This can be modified to some extent by the use of large grain or pebble powder, which burns more slowly than powder of finer grain. Mr. Bessemer, however, proposes to secure any required reduction of the initial pressure in another way. Instead of having a single large charge of powder, he proposes to have a series of 20 to 100 smaller charges, ignited successively as the shot travels along the bore of the gun. But this successive ignition of the powder would be less efficient than the explosion of a single charge in propelling the shot, unless means were taken to keep the shot under the action of the powder-gas for a longer period than in an ordinary gun. Hence Mr. Bessemer proposes to increase the length of the gun to 50 feet. By these two changes, Mr. Bessemer is said to hope to propel shot, the weight of which may be measured by tons, not by pounds. Mr. Bessemer's gun, illustrated in "Engineering," September 15, consists of inner tubes of welded wrought-iron, strengthened by steel rings, shrunk on with suitable initial tension. The gun is made in lengths, connected by flanges. The cartridge-chamber is chambered, and the charges may be successively ignited by fusees, or by means of electricity. Further, Mr. Bessemer suggests that rotation may be given to an elongated shot, not by rifling, but by the reaction of jets of powder-gas issuing from tangential orifices at its circumference. The powder-gas is supplied by a charge of powder burning in its interior, and the rotation is caused by the reaction of the jets, on the same principle as the rotation of Barker's mill or the Scotch turbine.

Wood-engraving by Machinery.—A process for engraving on wood by the cutting action of a sand-blast, is described in the "Journal of the Franklin Institute." A photographic copy of the drawing or object to be engraved is formed on a suitable matrix. This is then acted upon by a jet of sand, the particles of which have a very high velocity, so as to cut away to varying depths the surface of the block. The block is then electrotyped, and the engraving is printed from the electrotype. For various cutting and polishing purposes, the sand-jet seems likely to prove extremely valuable.

Gunpowder Gauges.—M. le Commandant de Reffye has used a form of gauge for determining the pressure in the bore of large guns, the principle of which has been suggested by M. Tresca's experiments on the flow of solid bodies. A block of lead is placed in a cylindrical hole bored into the gun, and is supported behind by a steel block through which a smaller cylindrical hole is bored. When the pressure acts on the lead, it forces it in part into the cylindrical cavity behind, and the volume of lead thus forced in forms a measure of the pressure in the bore of the gun.

St. Gothard Railway.—It is said that the St. Gothard Railway, with a tunnel about as long as the Mont Cenis tunnel, will be commenced very soon, and that the Mont Cenis staff will be transferred to undertake its construction.

Flow of Liquids.—Canon Mosely in the "Philosophical Magazine," and Professor Colding in the "Copenhagen Transactions," have been investigating the laws of the motion of water in pipes, conduits, &c. The generally accepted formulæ are known to be defective. They are founded on the assumption that the water moves in plane layers at uniform velocity.

The new researches seek to determine the motion of the liquid threads, that motion varying with the depth when water flows over a plain surface. These researches are too technical to be described here, but as they are of very great importance both practically and theoretically, it may be useful to mention that a translation of the abstract appended to Colding's paper has been printed. Colding has applied his results in the study of ocean currents.

Floating Breakwaters.—Mr. T. Cargill has proposed the use of a particular form of floating iron breakwater, in preference to the immense expensive solid-masonry structures now employed. He supposes the influence of the waves not to extend to a greater depth than 15 feet from the surface. To break the force of the waves he proposes an iron floating structure composed of sections each 100 feet in length. Each section is in the shape of a triangular wedge with the apex towards the sea. The wedge is right angled and has one horizontal and one sloping surface. The horizontal surface is nearly level with the water. The section is composed of a series of screws of lattice wrought-iron framework. The theory is that in passing through these screws the waves would be gradually disintegrated and strained of their violence. The idea of floating breakwaters is not new, and many plans of such structures have been proposed, and in one or two cases they have been tried, but without success. Nothing but experiment can determine whether Mr. Cargill's designs would accomplish the end proposed in a practical manner.

Domestic Steam Engines.—MM. Mignon and Rouart, of Paris, are supplying, on the system of M. Fontaine, a small domestic steam-motor, of $\frac{4}{15}$ to 1 horse power. They are intended to be employed in driving lathes, sewing-machines, pumps, jewellers' tools, &c. If the use of these engines becomes general, it will affect many industries by enabling the workmen to labour at home at a variety of employments which now are carried on in large shops. The steam is raised by gas. The boilers are vertical tubular boilers, with Bunsen burners. The products of combustion can be led to an ordinary chimney. The boiler holds water enough for several hours' work, so that it does not require constant feeding, but can be filled up twice a day. There is a self-acting arrangement for regulating the gas flames. The machine weighs about 2 cwt., and costs 500 francs.—Paris Correspondent of "Engineering."

Strains on Arches.—Mr. W. Bell has communicated to the Institution of Civil Engineers a paper on the determination of the stresses in metallic arches under unsymmetrical loads by combining graphic methods and calculation. The paper is one of great interest.

Pneumatic Despatch.—At the Institute of Civil Engineers, Mr. Siemens read a most interesting paper on his system of pneumatic despatch tubes now in use in Berlin, and also in London between the General Telegraph Office and Charing Cross. Mr. Siemen's system differs in principle from that originally introduced by Mr. D. K. Clarke. On Mr. Clarke's system a single tube was used between any two points, the carriers containing the messages being blown through in one direction and drawn back in the other by exhausting the air before them. Hence, in this system, the direction of the current of air is reversed at intervals. On Mr. Siemens' system a pair of tubes are laid down, one taking the carriers in one direction, the other those in the other direction, and the current of air is a steady continuous current always flowing in one direction. Along the line of pipes are stations at which carriers containing messages can be introduced into or taken out of the circuit, and a beautiful piece of apparatus has been contrived by which the carriers can be introduced into or taken out of the tubes almost instantaneously, and without in any way interfering with the steady flow of the air-current. In order to prevent any possibility of the blocking up of the tubes by two carriers jamming, an electrical system is adopted, like the block system of railways, by which each station telegraphs on the arrival and departure of each carrier, and the station to which it is sent prepares to receive it. Although, of course, the passage of the carriers through these tubes is very slow compared with the speed of electricity, yet as some thirty messages fully written out can be sent in each carrier, it is found quicker and cheaper to use pneumatic tubes than to use electric wires in cases where the number of messages to be sent daily is very great. Mr. Siemens even thinks that in large towns we shall ultimately employ the pneumatic system to distribute the letters from the Branch to the General Post Office, and *vice versâ*. He thinks that if a complete pneumatic system were in operation in London between the post offices, every letter might be delivered at its destination an hour after being posted.

As a subsidiary invention Mr. Siemens has perfected a new form of steam-jet for exhausting the air from the tubes. As an engine with its boilers and air-pumps is an expensive item in the erection of a system of pneumatic tubes, it appeared to him that some cheap substitute was desirable, even if its efficiency were less than that of an engine and air-pumps. He has found such a substitute in the well-known steam-jet, but he has so improved that instrument that now its efficiency appears to be very little less than that of an engine with exhausting cylinders.

MEDICAL SCIENCE.

Abnormal Larynx.—Sir Duncan Gibb described some very curious forms of the above at the late meeting of the British Association. In one he described a rare instance of absence of both arytenoid cartilages in a girl of 18. Likewise, one in which the epiglottis possessed the shape of a trefoil leaf, and two others in boys of fissure of the same cartilage.

The Ohio Twins.—These are said to surpass in peculiarity the Siamese twins and the "Two-headed Nightingale," recently exhibited in town. The "Lancet" says, that the Ohio twins have been examined by Drs. Williams and Little, and they are described as being united in a direct line from the occiput downwards along the spine. On one side are perfectly developed hips, thighs, legs, and feet. On the other side there is one large, imperfectly formed leg, presenting the appearance of the consolidation of two legs. There are eight toes on this limb, two of which have the appearance of great toes, being much larger than the others. Each child has a well-formed head and features, good arms and hands, lungs, heart, liver, stomach, &c. The lower portion of the trunk is said to be common to both. While these physicians were making their examination both cried, but, a few minutes after, one went to sleep, while the other remained awake. When either head would cry the perfect leg which was nearest that head kicked and drew up, while the leg nearest the other head remained quiet. When either cried, the toes on the imperfect foot would move, but the limb remained stationary.

Metamorphosis of Tissue while Fasting.—At a recent meeting of the Naturwissenschaftlichen Klasse of the Viennese Academy of Sciences, Prof. Seegen communicated a series of investigations upon the metamorphosis of albumen during fasting, which are thus given by the "Lancet." The subject of his experiments was a young girl, who, in consequence of a stricture of the œsophagus, was only able to consume very small quantities of nourishment. During a whole month the daily ingesta amounted to but 35 grammes of milk, and about 20 c.c. of water. A teaspoonful of this mixture was taken at intervals of about an hour. After lasting for four weeks, the difficulty of swallowing gradually disappeared, and the quantity of milk swallowed rose to 210 grammes per diem. The most important results obtained were the following:—1. The quantity of urine excreted daily amounted to 185 c.c. It was of very dark colour, possessed a strong acid reaction, and frequently deposited a considerable quantity of urates. 2. The amount of urea excreted daily, on an average of twelve days, was 8·9 grammes = 4·1 N. The amount of N. ingested with the milk was 0·29 gramme; the excess of N. excreted, therefore, amounted to 3·8 grammes. This quantity of nitrogen is contained in about 25 grammes of dry albumen, and the patient must thus have used up that weight of the dry albumen of her own body. The chief tissue furnishing this would of course be the muscles, and if that amount of dry albumen be calculated as coming from fresh muscle, 112 grms. of the latter would be used up. 3. The excretion of water is not covered by the water of the metamorphosed tissue. The organism, therefore, is dryer or poorer in water during fasting. 4. With increased supply of food the excretion of water through the urine rose, whilst the excretion of urea remained almost unchanged. 5. The metamorphosis of the albuminates during fasting, so far as they are represented by the excretory products of the urine, bears the proportion to the normal metamorphosis of 1 : 4 or 1 : 5.

Inoculation of Cancer in the Lower Animals.—At the meeting of the British Association in Edinburgh, Dr. John Chiene read a paper entitled "An Experimental Inquiry into some of the Results of Inoculation in the Lower Animals." The paper described a series of experiments, in which rabbits were inoculated with cancerous matter obtained from the human subject. The result may be shortly stated in the following words:—(1) That cancer cannot be produced in rabbits; (2) that cysts, containing cheesy matter, arise at the points of inoculation; (3) that these cysts do not differ from the local appearances which arise after the application of any irritant to the subcutaneous tissue of the rabbit.

The Functions of the Uvula.—A paper on this subject was read at the meeting of the British Association at Edinburgh, by Sir Duncan Gibb, Bart. Anatomists describe the action of the uvular muscle as an elevator which shortens the uvula. It is, however, a sentinel to the fauces, especially in the act of deglutition; for when any substance comes into contact with it, it excites the action of all the neighbouring muscles until it is got rid of. But it possesses a function of not less importance, in holding the soft palate tense and firm in the medial line against the wall of the pharynx during the act of deglutition itself, and thus prevents the passage upwards of fluid or solid substances behind the nose. This was supported by experiments upon a person who had lost the bones of the nose, permitting of a view of the action of the soft palate from its nasal aspect during deglutition with or without food. Under either circumstance, a double arch was seen in the form of two convex swellings, held in a state of firm tension by the action of the uvula passing down the centre of the soft palate, with its end resting flat against the wall of the pharynx. The tension ceased the moment that the constrictors of the pharynx had fully exerted their influence over the substances swallowed. Whilst the uvula has its special uses in the act of deglutition, it exerts a not less decisive influence upon the voice when uttered in a very loud tone, or in singing the higher registers, in both sexes. Then its character as a levator or shortener is exerted. If this power is impaired by removal of the muscular (not the membranous) end, then the singing powers are damaged. Some remarks were made upon elongation of the uvula and its effects, a distinction being made between its elongated membranous end and the true muscular tip, which should not be meddled with. Speech, the author said, was modulated by the soft palate and uvula, and the motor power of the latter is unquestionably exerted in pronouncing the letters K, Q, and X, with their associations, more especially the gutturals of the various languages.

Sir Thomas Watson on the Treatment of Cholera.—The "British Medical Journal" contains a communication from Sir Thomas Watson on the subject—now exciting so much attention—of cholera. Sir Thomas has altered his opinions as to the treatment of this disease. The following concluding passage will show this effectually:—"In the face of this and of much similar evidence, I feel bound to say that the rules laid down by Dr. Johnson for the treatment and prevention of diarrhœa and cholera seem now to me safer and better than the less discriminating advice which heretofore I gave you: 'whenever a suspicion arose that cholera was present in the community, not to try, in cases of diarrhœa, to carry off the presumed offending matter, but to quiet the irritation and to stop the flux as soon as possible, by astringents, aromatics, and opiates.' No doubt, the true indication of treatment is, to stop the flux as soon as possible; but this may sometimes be best effected (as also in 'crapulous diarrhœa,' and in the summer cholera of Sydenham) '*by* carrying off the offending matter.'"

Muscle-grafting.—Skin-grafting came into fashion a few years since, and has made much progress among advanced surgeons. Now muscle-grafting appears to be making its way. The *New York Medical Journal* for November contains an article upon the subject by Dr. B. Howard, and relates some interesting cases in which, by grafting, he obtained increased power of healing in several wounds. These were cases in which skin-graft had been first attempted, but without sufficient success. The paper is of some importance.

Nucleated Blood-cells in the Leucœmia of Infants.—Dr. Neuman has done what none of his predecessors have attempted—he has found the white blood-corpuscles in the blood of a living patient, by puncturing with a needle for a drop of blood. Besides numerous colourless granular cells of 0·005—0·012 mm. in diameter, which under healthy circumstances present no nuclei, there were found single homogeneous, pale-yellow cells of 0·006—8 mm. in diameter, with a colourless round or spindle-shaped nucleus, or with numerous granules (remains of nuclei). By the addition of acetic acid, these latter cells lost their colour, and within their contour, which appeared as a fine circular line, the somewhat yellowish-tinged nuclei and granules stood out with a sort of fatty glitter. Dr. Neuman is inclined to regard the presence of these transition forms between colourless and coloured blood-cells, which are produced by the diseased marrow of the bones, as a diagnostic sign of the disease of the marrow in leucœmia, since in a normal state they are found only in the marrow, and there is no evidence that in leucœmia they occur also in other organs, provided they are not carried into the same. In proof of this, he asserts that he has found nucleated cells in the general circulation of newly-born infants at term, and not alone (as has already been made known) in the pancreas, spleen, liver, and bony marrow. How long they remain after birth is not certain; they were absent in a child which died of peritonitis, sixteen days after birth.

Passage of the White Blood-corpuscles through the Vessels.—Dr. Norris, Professor of Physiology in Queen's College, Birmingham, presented lately a valuable paper on this subject to the Royal Society. It is abstracted in the *Monthly Microscopical Journal* for November. Briefly, his conclusion may be summed up thus: 1st. Both white and red corpuscles pass out of the vessels through apertures which can neither be seen before their ingress into or egress from the vessel wall, but only during the period of transit. 2nd. An essential and primary step in the process is, that the corpuscles shall adhere, or, more properly, cohere to the wall of the vessel. 3rd. These cohering corpuscles shall subsequently be subjected to pressure from within.

The Epithelium of the Cornea and its Regeneration.—The *Lancet*, which has of late years adopted the excellent custom of occasionally translating good papers from foreign journals devoted to histology, recently gave an account of a paper in Virchow's *Archiv*, by J. Arnold, and an opposition paper by Dr. H. Heiberg, of Copenhagen, on the above subject. Arnold's conclusion is that the new cells which replaced the old, when these had been detached, were derived from a finely-granular blastema that changes into protoplasm, and that in this protoplasm the new cells arise by a process of free cell-formation. The correctness of this conclusion is contested by Dr. Heiberg, who maintains the view that young cells are developed from the old, in which certain changes have taken place. His mode of procedure was to scratch the surface of the cornea with a cataract needle in animals (frogs, birds, rats), and, after the lapse of from eighteen to forty hours, to remove the eye and examine the cornea both by means of fresh sections and after careful preparation in solutions of chloride of gold (maceration for from three to five minutes in a one-half per cent. solution of the salt). In certain preliminary experiments it was found that the injured part *immediately* after the injury presented sharply-defined irregular borders; after six hours the margins were considerably flattened, so that the boundary of the abrasion was much less distinct. After eighteen hours it was difficult to tell the seat of the injury with the naked eye, and its diameter had become reduced to one-half or one-third; and after forty hours recovery was complete. He convinced himself by microscopic investigation that the process of regeneration of the epithelium proceeds from the margins of the abrasion, the layers of cells immediately bounding the seat of injury becoming elongated, and, as it were, sending forth processes towards its centre; so that the margins are rendered very oblique, whilst at the same time the exposed surface of the cornea is raised considerably above the level of that which is still covered by the cells. These views are different, of course, but neither can be yet accepted as conclusive.

A Cheap and Constant Galvanic Battery for Medical Purposes.—Professor A. Hammond describes a battery which has recently been devised for him, in the *New York Medical Journal* for November. Each cell contains about half-a-gallon of fluid. A disc of sheet copper is laid flat on the bottom of each cell. To the under side of this is affixed a copper wire covered with gutta-percha. The copper sheet forms the *negative plate*; the insulated wire which rises to the top of the cell is the *positive pole*. Two or three inches below the upper margin of the cell is suspended by a brass hanger a thick disc-shaped plate of zinc, concave on the lower side, with a round aperture in the centre. This is the positive plate. To this hanger is attached a binding screw, and this forms the *negative* pole. The body of the battery fluid is formed of a solution of sulphate of zinc. Occasionally, as required, crystals of sulphate of copper are dropped through the central aperture in the zinc to the bottom of the fluid. These dissolve, and produce a layer of blue liquid, which covers the copper. Thus, we have copper in the bottom of the cell, immersed in a solution of copper, zinc suspended above, immersed in a white liquid, the solution of zinc. The mode adopted in other batteries to separate the fluids consists in using a porous diaphragm, or cup, within, and surrounding which are placed dissimilar metals and fluids. The porous septum, it was thought, would allow the current to pass, and yet prevent the admixture of the diverse elements. It has, however, been demonstrated that, when two such liquids, and even two gases, are thus separated, they will invariably become mixed. In this battery, without the intervention of any diaphragm, the denser liquid, the blue, remains in the bottom of the cell, the lighter one overflows and rests upon it; thus arranged, there is less liability to diffusion or mixing than if the two liquids were placed side by side, in vertical columns, with a porous partition between them. The central aperture in the zinc plate also admits the introduction of a hydrometer to measure the density and strength of the liquid. Provision is also made for preventing too rapid evaporation of the fluid. The occasional addition of a little water, and every three or four days dropping in a few crystals of sulphate of copper, is nearly all that is required in the management of this battery.

MINERALOGY, METALLURGY, AND MINING.

The Working of Hæmatite Ores.—Among the many papers at the late meeting of the British Association at Edinburgh, was one by Mr. Thomas Ainsworth, on "The Facts developed by the Working of Hæmatite Ores in the Ulverstone and Whitehaven Districts, from 1844–71." He referred first to some correspondence that had taken place between the British Association and the Government on the subject of the investigation of the hæmatite ore-fields. As this had resulted in the investigations being abandoned on the part of the Government Geological Survey, he thought it was the duty of every one who knew anything on the subject to make it public, seeing that when the Bessemer royalty expired—which would shortly be the case—this hæmatite would become much more valuable than it was at present. He quite agreed with all that had been done hitherto in this matter, and the facts he was now to present had come under his notice during the last thirty years while residing in Cumberland, near the Ulverstone and Whitehaven districts. Contrary to what had been generally supposed, he had found the hæmatite ore did not confine itself to the neighbourhood of limestone rock, but was to be found in many kinds of rock, and even between two different kinds of rock. He had also found that the hæmatite had some relation to the coal-fields, and was always found in close proximity to these. A peculiarity of the hæmatite ore-fields was that they seemed to run almost exactly from north-west to south-east. Another fact he had discovered was, that carbonic acid was to be found only in very small quantities in hæmatite, while it was found in large quantities in other ironstone. He was aware that hæmatite was to be found also at Haddington and Bristol. The paper was illustrated by several large diagrams.

Ralstonite, a new fluoride, from Arksutz Fiord (U.S.A.).—This is fully described by Mr. George J. Brush, in a recent number of "Silliman's American Journal." It was observed, a few months since, by the Rev. J. Grier Ralston, of Norristown, Pa. Mr. Ralston found a mineral in minute octahedrons associated with thomsenolite, and being unable to identify it, he sent it to Professor Dana, by whom the specimens were passed over to Mr. Brush for examination. The crystals of the new mineral are octahedral; and in some cases they are very minute, but occasionally one to one-and-a-half millimeters in diameter. They are often implanted on the thomsenolite crystals, and also apparently intercrystallised with this species, making it extremely difficult to separate the new mineral sufficiently pure for analysis. The planes of the octahedron are often tinged slightly yellow, and many of them are dull and iridiscent, owing to an excessively thin film of oxide of iron, and hence exact measurement of the inclinations of the faces cannot be made. But they appear to be symmetrical with equilateral faces, and in some cases have all the solid angles replaced by a minute plane.

The Quarterly Metallurgical Report, by David Forbes, F.R.S., is a most useful periodical, and we trust it may receive such support as will induce the proprietors to carry it on. The third number, now before us, is full of important and interesting news from all parts of the Continent and America. Besides, it has just commenced a list of works published in all languages on the subject of metallurgy and mineralogy, which will make it most valuable to those interested in the subject. We have so high an opinion of this periodical, that we prepare our summary almost altogether from its pages.

Iron and Iron Ores.—A pamphlet has been issued by Dr. Sterry Hunt, F.R.S., entitled "Notes on Iron and Iron Ores," being a report addressed to Sir W. Logan, F.R.S., late director of the survey (published by Dawson Brothers, Montreal, 1870). The contents of this pamphlet are arranged under the following heads:—1. A comparison of the iron ores of Canada with those of Sweden and Norway. 2. Chemical analyses of the Canadian iron ores. 3. Notes on the iron sands of Canada. 4. A sketch of the different methods of making iron and steel direct from the ore. 5. A description of the Ellershausen process for making malleable iron. Some of the facts brought forward by the author specially connected with the Canadian iron manufacture are considered of sufficient interest to warrant their being reproduced in Mr. Forbes's report.

Blast Furnace Sublimates.—A chemical examination of the sublimate or incrustation (Cadmia) which attaches itself to the inside of the upper portion of the lining of the blast furnaces at Stenay (Meuse) has been made by MM. Nivoit and Létrange, whose report, published in the "Annales des Mines," 6, Ser. XXVIII., p. 113, gives its composition as follows:—

Oxide of zinc	89·10
Oxide of lead	5·91
Sesquioxide of iron with alumina	1·90
Lime	0·97
Sulphuric acid	0·13
Silica	0·56
	98·57

Phosphorus in Iron and Steel.—According to Salet, the presence of phosphorus in iron and steel may be detected by examining with the spectroscope the flame obtained by burning the hydrogen produced on dissolving the metal in hydrochloric acid.

The Magnet in Practical Mineralogy.—For the purpose of separating the magnetic oxide of iron from the magnetic titaniferous iron ore, quartz, &c., which gives so much trouble and expense in working the Canadian iron sands, a simple, ingenious arrangement has been patented by Dr. Larue, of Quebec; in this the ore in the state of sand or powder, is delivered from a hopper, arranged so as to open and close at proper intervals, and to allow the ore to spread in a thin and uniform layer, upon a series of aprons, arranged with interspaces, between two parallel endless bands, which pass over two horizontal cylinders. These aprons, charged with ore, are made, by the movement imparted to one of the cylinders, to pass from beneath the hopper, under a series of permanent horse-shoe magnets, 800 in number, each capable of sustaining about five pounds; arranged upon transverse bars, in five rows of 160 magnets each. Beneath these is a tympan, covered with muslin, which, when the iron ore is passing beneath them, is in contact with the poles of the magnets; as soon, however, as the magnetic portions of the ore have arranged themselves by attraction, in adhesion to the under side of the tympan, and the apron has moved from beneath, and gone forward to discharge the nonmagnetic portion of the ore at the foot of the machine, the tympan is momentarily withdrawn a short distance from the poles, and the adhering magnetic ore falls in the open space, between two aprons, into a receptacle placed below. This process of loading and unloading the magnets, can be repeated sixty times in each minute.

MICROSCOPY.

The Beading of Diatomaceæ is well described by Mr. Henry J. Slack, Secretary to the Microscopical Society, in the "Monthly Microscopical Journal." Mr. Slack says that the uniformity of plan in the silicious deposits of diatoms is to a great extent shown, and to a still further extent suggested, by examining one of Möller's admirable "type slides," with a good immersion $\frac{1}{8}$th or higher power. The gradations from large beads distinctly separated, to smaller beads closely approximating, are readily and instructively exhibited, so that it is easy to trace a series, beginning with large forms that present no difficulty of resolution, and concluding with the most delicate that tax the utmost power of the optical apparatus. When beading appears minute under high magnification, and each bead seems in contact with its neighbour, the outline of a section made by a plane passing through the bead rows perpendicular to the uppermost point of their circumference, would exhibit a delicate wavy line, the depressions of which would be extremely small, as they would correspond with the radii of the little spheres, while the width of the curves would correspond with their diameters. All that can be done under these circumstances, by the best adjustments, and the most careful unilateral illumination, is to exhibit minute, and often very faint, alternations of light and shade, indicating rather than demonstratively showing the character of the structure. When the best has been done with any objective, it becomes evident that a slight increase of the difficulty, from greater minuteness of the structure, would render it invisible, and make the surface look plane.

Tolles' Binocular Eye-piece.—Professor Smith has written to the "Monthly Microscopical Journal," explaining that he is not the inventor of this instrument. He exhibited it here, and hence the mistake arose. It is really Mr. Tolles' invention. Mr. Smith says that Dr. Carpenter has made a mistake as to the inventor of it.

An Erecting Mirror, which is made to surmount the eye-piece, has been devised by Mr. C. Richards. It consists of a glass reflector, platinised on the front surface, thus getting rid of the second image, always seen when an ordinary silvered surface is employed. The dissecting needles or knives are not reversed by it, as might have been expected; for after having adjusted the focus, by simply turning the mirror on its axis one quarter from the right hand, the needle held in the right hand is immediately brought into a proper position. The definition or perfection of the image is in no way impaired, and therefore it will be found useful in drawing as well as dissecting.—*Monthly Microscopical Journal*, November.

How to Grind a Diamond to a fine Point for Micro-writing.—This is thus excellently explained in a letter from Mr. Wenham to Mr. Slack, which appears in full in the "Monthly Microscopical Journal." It is briefly as follows:—"A fragment of diamond was imbedded in a short piece of copper wire, $\frac{1}{10}$th in diameter, in the way described in my paper 'On the Construction of Object-glasses.' This was chucked in the bow lathe or 'jigger,' and another splinter of diamond, similarly mounted, was held against it as a turning tool. Both were, I suppose, about equally ground away, and you could see the dust flying off; in fact, diamonds rubbed together abrade each other just like two pieces of slate pencil will do. It is *very* easy with a delicate touch at last to bring the rotating diamond to a point as fine as a needle. *This* is the right thing for glass ruling, and I have no doubt that Nobert uses the same. In Peter's writing machine-turned points are employed, as these only will mark in every direction. At first he used to buy his turned points from the diamond workers at one guinea each, and few of them good even at that. I explained my way of turning the points, at which he succeeded at the first attempt, and ever after that made them with his own hands. He told me afterwards that what before cost him 21s. did not now cost him 1s."

Tolles' immersion $\frac{1}{5}$th Object-glass.—In a letter which Mr. Slack received, and which he published in the "Monthly Microscopical Journal" for December, Dr. Woodward says that the Tolles' immersion $\frac{1}{5}$th, by which the Amphipleura pictures which he sent to the Microscopical Society were made, works either dry or wet, the compensation being effected simply by altering the distance of the front lens from the other two, by means of the screw collar. There is also a low-angle extra front for ordinary work. He finds, with the high-angle fronts, the following measurements:—

Angle.	Magnifies at 4 ft. focus; Micrometer screen and without eye-piece.
Dry: uncovered, 110°	225 diam.
„ for thickest cover, 140°	250 „
Immersion: uncovered, 140°	250 „
„ for thickest cover, 170° upwards .	275 „

With central light, and on Podura, or anatomical objects, he finds this objective admirable. He wishes he could speak as favourably of Mr. Tolles' higher powers. They are very good indeed, but he has *yet to see one of them* which will rival the so-called $\frac{1}{16}$th immersion of Powell and Lealand.

A Machine for Ruling Glass has been devised by Mr. Stanistreet, of Liverpool, and is excellently figured and described in the "Monthly Microscopical Journal" for December. Mr. Slack has given a very full account of it. The machine is constructed for ruling lines from $\frac{1}{1000}$th to the $\frac{1}{10000}$th of an inch apart, and the inventor has added to it the means of further subdivision to the $\frac{1}{100000}$th of an inch, but he has not yet been able to procure any diamond fine enough for ruling distinctly more than about 5,000 lines per inch. Mr. Wenham's letter in the preceding paragraph was called out by this statement.

How to Mount Diatoms.—Readers will study with advantage the following method, described by Capt. Lang, in the "Microscopical Journal" for

November. Place, says the author, the balsam-mounted slide on the hot-plate, and when it is sufficiently warmed tip over the cover by means of a needle; the diatoms will be either on it or the slide, it matters not which. Apply over them at once, whilst still on the hot plate, a drop of turpentine. Remove the slide to the stage of the dissecting microscope, and add more turpentine. Have ready a clean slip of glass, on which has been placed a drop of turpentine. In the case of large discoid and other forms, having applied plenty of turpentine, they can be easily transferred by means of a fine sable-hair brush from the original slide to the pool of turpentine on the clean one. In the case of finer forms, it is better to place less turpentine on the original slide, collect the diatoms into a heap, allow the turpentine to dry a little, and then by a twist of the brush to transfer them *en masse* to the new slide. In either case, having got them there, push them together and mop up the superfluous turpentine, and then, still under the dissecting microscope, slant the slide by placing a piece of folded paper under one end, and apply a little benzole, either by means of a clean brush or brass rod, immediately above them, that is, on the end of the slide that is raised, and allow it to float *gradually* over them, care being taken that it does not flow with too great a rush and carry away the diatoms with it. Repeat this process some half-dozen times, till the whole of the turpentine and balsam has been washed away, and till the valves are left dry and black after the benzole is evaporated. They can then be transferred in the usual way to any other slide, and even with greater ease than from an ordinary dry gathering.

Scales on the Body of Culex.—Mr. Jabez Hogg has an interesting paper in the "Monthly Microscopical Journal" for October on this subject. He says that the body of *Culex annulatus* is entirely covered with alternate rings of dark brown and white-coloured battledore scales; and the hairs projecting from its sides are longer and more numerous than in *C. pipiens* or *C. musquito.* The "feathered antlers" pectinate antennæ of the male insects, although destitute of scales, are exceedingly handsome objects: they surmount the most brilliant set of compound ocelli it is possible to find. In short, he wears finer clothes and is better dressed throughout than his female companion. When the piercing apparatus is sent into the flesh of a victim, the proboscis appears to divide; it is then thrown up and turned back upon the head like the trunk of an elephant.

Microscopical Papers of the Quarter.—The following is a list of the several papers which have been published in the "Monthly Microscopal Journal" of October, November, and December:—"A Rare Melicertian; with Remarks on the Homological Position of this Form, and also on the previously-recorded new species Floscularia coronetta." By Charles Cubitt, F.R.M.S.—"On an Improved Method of Photographing Histological Preparations by Sunlight." By J. J. Woodward, Assistant-Surgeon, U.S. Army.—"Hæmatozoa in Blood of Ceylon Deer." By Boyd Moss, M.D. —"Microscopical Fissures in the Masticating Surface of Molars and Bicuspids." By J. H. M'Quillen, M.D., D.D.S., Professor of Physiology in Philadelphia Dental College.—"Transmutation of Form in certain Protozoa." By Metcalfe Johnson, M.R.C.S.E., Lancaster.—"On Gnats' Scales." By Jabez Hogg, Esq., Hon. Sec. R.M.S.—"The Examination of Nobert's

Nineteenth Band." By F. A. P. Barnard, of Columbia College, New York. —"An Incident in the Life of a Chelifer." By S. J. McIntire, F.R.M.S. —"On the Form and Use of the Facial Arches." By W. K. Parker, F.R.S., President R.M.S.—"On the Angular Aperture of Immersion Objectives." By Robert B. Tolles, of Boston, U.S.—"Note on Pedalion Mira." By C. T. Hudson, LL.D.—"Another Hint on Selecting and Mounting Diatoms." Communicated by Capt. Fred. H. Lang, President of the Reading Microscopical Society.—"The Monad's Place in Nature." By Metcalfe Johnson, M.R.C.S.E., Lancaster.—"Mapping with the Micro-spectroscope, with the Bright-line Micrometer." By H. G. Bridge.—"Some Remarks on a 'Note on the Resolution of Amphipleura Pellucida by a Tolles' Immerson $\frac{1}{8}$th. By Assistant-Surgeon J. J. Woodward, U.S. Army.'" By Edwin Bicknell. —"Infusorial Circuit of Generations." By Theod. C. Hilgard.—"Notes on Prof. James Clark's Flagellate Infusoria, with description of New Species." By W. Saville Kent, F.Z.S., F.R.M.S., British Museum.—"Note accompanying Three Photographs of Degeeria domestica, as seen with Mr. Wenham's Black-ground Illumination and a Power of 1,000 diameters." By Dr. J. J. Woodward, U. S. Army.—"Note on the Above." By F. H. Wenham. —"On Bog Mosses." By R. Braithwaite, M.D., F.L.S.—"An Instrument for Micro-ruling on Glass and Steel." By J. F. Stanistreet.—"On the Conjugation of Amœba." By J. G. Tatem, Esq.—"Crystallisation of Metals by Electricity under the Microscope." By Philip Braham, Esq.— "Infusorial Circuit of Generations." By Theod. C. Hilgard.—"On the Connection of Nerves and Chromoblasts." By M. Georges Pouchet.

PHYSICS.

The Movement of the Ocean.—This question has recently been brought under notice by the valuable researches of Dr. Carpenter. It has raised a number of supporters in different parts of the world; among others Professor J. D. Dana, in the United States, who makes the following remarks [at least we take it for granted that the signature is Professor Dana's], in "Silliman's Journal:"—"The view which Dr. Carpenter advocates, that the movement of the ocean affects the whole body of water to its very bottom, is recognised by the writer in his 'Report on Crustacea of the Wilkes Exploring Expedition' (4to. 1618 pp., 1852, 'Silliman's Journal,' II. xvi. 1853), and the general system in this circulation is there pointed out—this system according with the views previously held by the distinguished meteorologist, W. C. Redfield. The conclusions are sustained by facts relating to the temperature of the ocean observed in the course of the cruise of the exploring expedition just mentioned, and others from various sources, presented on an isothermal chart prepared for illustrating the geographical distribution of marine life, and especially the crustacea; and a brief statement of this system is given in his 'Manual of Geology' (1861). The facts from the deep ocean remove all remaining doubt with regard to the universality of the movement and the oneness of the system. At the same time there does not appear to be any good reason for separating from the system the Gulf Stream, as done by Dr. Carpenter. Given the vertical circulation, and the north and south movement, advocated by him, and then the revolution of the globe will make it, as has been long recognised, a *westward* movement in the tropics and an *eastward* in the middle and higher temperate latitudes, such as is found, in fact, in all oceans. Then, secondly, whenever, in the flow of these waters, they approach the continents, where the depth diminishes, the rate of flow will be increased in proportion (approximately) to the decrease of depth; and hence comes the stream east, not only of North America and there called the Gulf Stream, but of South America, and also those east of Asia and of Australia; and also that in the higher latitudes west of South America. The Gulf Stream and all those other streams are parts of the general system, modified by proximity to the continents; the action of the trades is not in any case their origination, though it may well be their accelerator. Neither is an Indian Ocean current the origin of the current in the South Atlantic up the west side of Africa, though contributing to it. Dr. Carpenter also combats Mr. Croll's position, with regard to the 'thermal work of the Gulf Stream.'"

The Temperature-equilibrium of an Enclosure in which there is a Body in Visible Motion.—This is the subject of one of the most important papers brought before the British Association at Edinburgh, by Professor Balfour Stewart, of Owen's College, Manchester. He said—taking the illustration of the room in which they were assembled—they knew that in an enclosure, the walls of which are kept at a constantly uniform temperature, every substance will ultimately attain the very same temperature as those walls; and we know also that this temperature-equilibrium can only be brought about by the absorption of every particle being exactly equal to its radiation, an equality which must separately hold for every individual kind of heat which the enclosure radiates. This theoretical conclusion is supported by numerous experiments, and one of its most important applications has been the analysis of the heavenly bodies by means of the spectroscope. The Professor then asked the Section to suppose that in such an enclosure we have a body in visible motion, its temperature, however, being precisely the same as that of the walls of the enclosure. Had the body been at rest, we know from the theory of exchange that there would have been a perfect equilibrium of temperature between the enclosure and the body; but there is reason to believe that this state of temperature-equilibrium is broken by the motion of the body. For we know, both from theory and experiment, that if a body, such, for instance, as a star, be either rapidly approaching the eye of an observer or receding from it, the rays from the body which strike the eye will no longer be precisely the same as would have struck it had the body been at the same time at rest, just as the whistle of a railway engine rapidly approaching an observer will have to him a different note from that which it would have had if the engine had been at rest. The body in motion in the enclosure is not, therefore, giving the enclosure those precise rays which it would have given had it been at the same temperature and at rest; on the other hand, the rays which are leaving the enclosure are unaltered. The enclosure is therefore receiving one set of rays and giving out another, the consequence of which will be a want of temperature-equilibrium in the enclosure; in other words, all the various particles of the enclosure will not be of the same temperature. The consequence will be we can use those particles of different temperature so as to transmute part of their heat into the energy of visible motion, just as we do in a steam-engine; and if it is allowable to suppose that during this process the moving body has retained all its energy of motion, the result will be an increase of the amount of visible energy within the enclosure, all the particles of which were originally of the same temperature. But Sir W. Thompson has shown that this is impossible; in other words, we cannot imagine an increase of the visible energy of such an enclosure, unless we acknowledge the possibility of a perpetual motion. It is not, therefore, allowable to suppose that in such an enclosure the moving body continues to retain all its energy of motion, and consequently such a body will have its energy of motion generally stopped. In this argument the use of the enclosure had been to enable them to deduce proof from the known laws of heat and energy. We may alter the shape of the body without affecting the result; in other words, we should expect some loss of visible energy in the case of cosmical bodies approaching or receding from one another.

Experiments on Vortex Rings.—Professor Ball made some very interesting experiments before the British Association at Edinburgh, on this subject. A stream of light was admitted through a lens in one of the shutters, and along this stream Professor Ball projected smoke rings, formed on the ejection of smoke from a box in which vapour was generated. Professor Tait, during the progress of the experiments, alluded to the vibration of the rings when those later projected overtook those which had been ejected previously, and the latter of which continued to vibrate after the shock. When Dr. Ball had concluded his experiments, Mr. D. H. Deacon proceeded with his paper and illustrations on vortex rings in water, demonstrating both by diagram and actual experiment the formation of secondary and tertiary nodes. The latter were made in vessels filled with water, and by means of drops of coloured fluids passed thereinto from a tube. These experiments were watched with great interest, and were also loudly applauded. The rings were projected downwards into the vessels that were standing upon the table, but in answer to a question, Mr. Deacon said he had in some cases projected the rings upwards through the fluid, but the phenomena were much more difficult to produce. It was also said that similar experiments to those of Professor Ball had been made twenty years ago by Professor Boswell Reid, and still earlier by Professor Daniell, of King's College. It was, however, further urged that priority in such a matter as this was not of much account, and that the value of the communication should be tested by the results attained.

Unequal Loss of Acid and Salt near the Poles of a Battery.—This is the subject of a paper in the "Annales de Chimie." It is by M. E. Bourgoin, who first gives a review of the labours of Daniell, Miller, Pouillet, Hittorf, Grotthuss, d'Almeida, and others on this subject, and next describes at length a series of experiments, from which the following main results may be deduced:—When the galvanic current passes through acidulated water, it will be seen that, though the total effective work done by each pole is the same, the quantity of acid found in each compartment at the end of the experiment will be found to vary considerably. The three following cases may be distinguished:—(1) The acid is accumulated regularly at the positive pole; this occurs with sulphuric, nitric, phosphoric, benzoic, succinic, camphoric, &c., acids. (2) There is no loss at the positive pole; the loss is only experienced at the negative pole, since half of the electrolysed acid is regenerated in the other compartment. (3) The two compartments become simultaneously poorer; this occurs with lactic, tartaric, citric, and, in general, all the very readily oxidisable acids.

ZOOLOGY AND COMPARATIVE ANATOMY.

The Anatomy of the Skunk.—Dr. J. S. Parker, in a paper published in the "American Naturalist," says, in regard to the glands which secrete the well-known odour emitted by this animal: "When I resumed operations, on the parts now weighing only about two ounces out of a Mephitis of nine pounds, I had a strip of skin with the anal lips, the suspicious calices or cones in their cup-like cavities, and the pouches. Microscope was at hand, magnifying glasses, spectacles, and dissecting case that had done much human duty. I began by severing the two muscular pouches, and found no connection between them. Books say, 'The animal gives its peculiar and penetrating odour from two glands, situated external to the pelvis.' I found the 'glands' to be *clear muscular fibre,* with not a particle of smell, or a trace of any glandular structure. So much truth there is in old sayings, repeated for years or ages past! Further to test the matter, I cut slowly to the middle of the mass of *muscular,* not *glandular,* fibres, and came upon a thick, white, leathery capsule, like the crop of a chicken, with the source for the contents, provided by the little glands about it. Now putting on old clothes, and sitting to the windward, I cut through this white capsule; a bright yellow fluid came out, and I instantly felt that distance would 'lend enchantment to the view.' But I was not to be baffled. So I dipped the point of my scalpel in the yellow fluid, put the tenth or twentieth of a drop of it on a glass, covered it with another strip of glass, and placed it under a power of forty diameters in my microscope. The appearance was peculiar. It looked like molten gold, or like quicksilver of the finest golden colour. Pressure on the strips of glass made it flow like globules of melted gold."

The Anatomy of the Fin Whale.—This was a paper read before the British Association at Edinburgh by Professor Struthers. The author set out by stating that the whale in question, which measured 68 feet in length, was found in the North Sea, some thirty or forty miles off Aberdeen. On its being towed into Peterhead, he proceeded to examine it, and its plaited breast at once showed it to be one of the rorquals. The colour was white on the belly, dark on the sides, and black on the back. In examining the bones and muscles, he found a sixteenth pair of ribs. The position of these was a remarkable one for a rib to occupy in a mammalian animal, suggesting a sternal rather than a vertebral rib, and somewhat resembling the abnormal ribs of the crocodile. The muscles he spoke of as rudimentary structures, whose function was not distinct, but low. As abnormal or unusual rudimentary structures could be understood only through variability and inheritance, so normal rudimentary structures were to be explained, not by the fictions of final cause, or of so-called type, but by the law of inheritance and the influence of function. The one, as part of a great scheme of evolution, had brought them into existence, and the other, by fitness and use, had preserved them from becoming extinct.

Further Deep-Sea Exploration.—Dr. W. B. Carpenter, V.P.R.S., stated, at the meeting of the British Association at Edinburgh, that he had received a letter just then from the First Lord of the Admiralty with reference to an application to him for Government assistance in carrying out deep-sea explorations, which he was sure the Section would be glad to hear. The proposal submitted to Mr. Goschen was for a circumnavigation voyage of four years for the purpose of carrying out such explorations; and at first he had said the proposed scheme was too considerable for the Admiralty to deal with alone. He (Dr. Carpenter), however, had consulted Mr. Lowe, and the result was a reply from Mr. Goschen in the following terms:—"With reference to our interview some weeks ago on the subject of the proposed exploration of the deep sea in the Atlantic, the Indian, Southern, and Pacific Oceans, I am happy to be able to tell you, after consultation with my colleagues, that on receipt of a formal application from the Royal Society, we shall be prepared to take a favourable view of your request, and to give such assistance as may be possible in the manner indicated in your letter of the 17th ultimo." It would be for the committee of this Section to make a recommendation to the general committee to appoint two delegates from the Council of the British Association—who would probably be the past and present Presidents—to co-operate with the Council of the Royal Society in promoting such an application. This recommendation was subsequently adopted.

Parthenogenesis in a Chelifer.—In a paper which was read before the Royal Microscopical Society in October last, Mr. S. J. McIntyre gave an instance which certainly looks like parthenogenesis in a chelifer. The evidence, so far as it went, seemed to prove the possibility of parturition without any connection with the male since parturition occurred previously.

The Embryology of Scorpio Italicus.—This has been recently worked out by Dr. Metschnikoff, whose paper originally appeared in Siebold's "Zeitschrift," and has been analysed in the *American Naturalist.* The embryology of insects and crustacea as pursued at the present day by zoologists, who are directing especial attention to the provisional membranes of the egg and embryo, depends almost as much on the skilful use of the chemicals as the microscope itself. The author says, "The methods which I employ in these researches are not complicated. I study the eggs removed from the ovarian tubes; or place the living embryo in a drop of a weak solution of salt (Salzlösung); or I at first submit them to the influence of solutions of chromic acid of different strengths, and then examine them either with a simple or compound microscope. Out of embryos hardened in this way I can make sections. Much of the time I have to work with dissecting needles, while the embryos or portions of them treated in this way, and in an equal mixture of fresh and salt water, afford very good objects for study." The embryology of scorpions was sketched out in a general way by the distinguished German embryologist Rathke. Metschnikoff extends these researches very greatly, and considers as the most important results of his studies the discovery that "in the embryo of the scorpions three embryonal membranes are developed, which in many respects are very strikingly similar to the Remakian embryonal membranes of the vertebrates."

General Natural History of Madagascar.—This has been recently studied by M. Grandidier, who made numerous astronomical, meteorological, and magnetical observations. He also studied closely the ethnology of the inhabitants, having taken a great many measurements upon the living body, and having collected notes of the habits, language, and traditions of the people. His natural history collections embrace over fifty new species of vertebrates, together with numerous insects and plants. Large numbers of alcoholic specimens were also gathered, for the purpose of further investigation into the anatomy and structure of the entire animal.

The Sponges, and Mr. Carter, F.R.S.—"Silliman's Journal" publishes the following note from Mr. Carter. He says, "I send you a line just to tell you what you will be glad to learn, viz. that I have confirmed all that Professor James Clark, of Boston, has stated about the sponge-cell, and much more too. It is after all only what was published and illustrated in the 'Annals' in 1857. Indeed, I am astonished now at the accuracy and detail of that paper, now *all* confirmed by an examination of a *marine* calcareous sponge. I have not only fed the sponge with indigo, and examined all at the moment, but the sponge so fed was put into spirit directly afterward, and *now* shows all the cells (monociliated) with the *cilium attached and the indigo still in the cells.* This, I think, will break down Häckel's hypothesis, which is as imaginative and incorrect as it is beautiful. His 'Magosphæra' too is figured in the 'Annals' (1856), and described *in extenso* as the amœboid cell which inhabits the mucus of the cells or internodes of the Bombay great *Nitella.* But there are no people in England, if on the Continent, who seem to be able to show this, if even they be cognizant of it. *Ex oriente lux* used to be the old phrase; the light is now being *reflected* back from America. It is from there we must expect novelties now."

How to make Local Museums.—These are generally such unmitigated heaps of rubbish that it is well to find some hints of usefulness thrown out. Mr. Townsend M. Hall, writing to the "Geological Magazine" (November), says that it is well to bear in mind that, in default of local collections, a great deal of good can be done by means of local catalogues. Let each member of a Naturalists' Club undertake whatever branch of natural history he is best acquainted with, and compile a list of the species occurring in his own neighbourhood,—one might catalogue the fossils, giving the names of characteristic species found in each quarry; another might devote himself to the minerals, and others would take in hand the botanical and zoological departments. Thus, by a well-organized division of labour, an immense amount of valuable information would be accumulated, and the result would be a record of the distribution of species throughout the country, more lasting perhaps, though less attractive, than that afforded by many a local museum.

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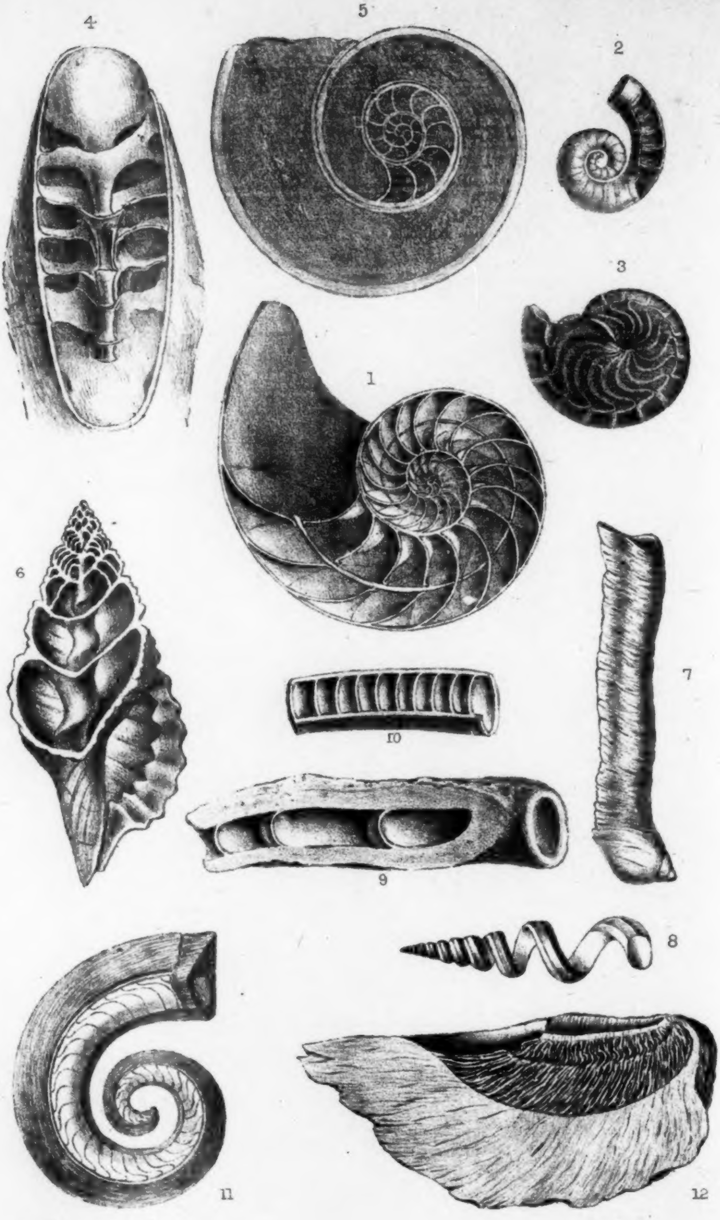

H. Woodward. W. West & Co imp.

Camerated Shells.

ON THE STRUCTURE OF CAMERATED SHELLS.

By HENRY WOODWARD, F.G.S., F.Z.S.,
Of the British Museum.

[PLATE LXXXII.]

AMONG the various natural objects which the ingenuity of man in all ages has converted into articles of use or ornament, both in savage and civilised life, none have attracted a greater amount of attention, or have been more in request, than the shells of Mollusca, especially sea-shells.

Their bright colours and diversity of form are among their chief charms to the uninitiated; whilst, to the student of natural history, they offer ample materials for scientific research.

The shell in the Mollusk may be regarded as a calcified portion of the mantle specially provided (like the enclosing ribs of the vertebrata) to afford protection to the breathing organs and heart. Indeed, when reduced to a mere rudiment, as in *Limax*, *Testacella*, *Carinaria*, &c., it forms only a hollow cone or plate protecting these organs.

This structure, which has sometimes been called a Pneumoskeleton, is so characteristic of the Mollusca as to have obtained for them the title of *Testacea*,* and the common name of "shell-fish" very well expresses the leading feature in the group.

Nevertheless, in one whole class, the *Tunicata*, and in several families, the shell is either wanting altogether, or is internal, or so rudimentary that it would never be popularly recognised as a shell. When fully developed, the shell of the Mollusk subserves to protect the soft parts of the animal from injury, and the animal itself from the attacks of enemies, and, in some of the *Gasteropoda*, from those variations of temperature and moisture to which the *terrestrial* species are peculiarly exposed.

* From *testa*, a shell.

Where the shell is absent or rudimentary, its protective function is supplied either by greater activity on the part of the animal itself, as in the case of the "squids," or by organs of concealment, as in the case of the inky fluid possessed by the Poulpes and cuttle-fishes, with which they cloud the water, and so favour their escape.

In the present paper I propose to point out certain peculiarities in the shells of the Cephalopoda, and to inquire whether there is any ground for the *still-prevalent* idea that these structures are unlike any of their congeners in the Molluscan sub-kingdom.

The external-shelled division of the Cephalopoda, represented at the present day by the "Pearly Nautilus' alone, but in the past by the Nautilus, Ammonite, Goniatite, Orthoceratite, and a host of other forms, belongs to the Tetrabranchiata (four-gilled), and were once as extensively represented in the ancient seas of our globe as the naked or internal-shelled Dibranchiata (two-gilled) are in the seas of to-day.

Only one other siphonated shell is now known amongst living Mollusca, besides the pearly Nautilus; it is the beautiful little *internal* pearly shell of the *Spirula* (fig. 2), a small form of Cephalopod belonging to the Dibranchiate division of the class in which are placed the internal-shelled, active, free-swimming squids, cuttle-fishes, and the shell-less *Octopus*, so largely represented at the present day in the seas of all parts of the world.

The chambered character of the shell, with its siphuncle, appears to be a unique molluscan structure entirely confined to the Cephalopoda. Nevertheless, many shells, belonging to widely-different families of the Mollusca, are *camerated* (*i.e.* divided into chambers, or are vaulted).

Thus, for example, certain species of Helices (as the *Helix decollata*) always lose the apex of their spire on attaining their full growth. But before this takes place the animal has already formed a septum or septa within its shell, effectually shutting off the upper whorls from the inhabited portion.

Vermetus (see Plate, figs. 8, 9), *Euomphalus* (fig. 5), *Triton* (fig. 6), and *Turritella*, all form internal septa to their shells.

Aged Oysters (fig. 12), *Gryphæa*, *Spondylus*, *Ætheria*, and *Teredo* (fig. 10), also form partitions within their habitations.

The genus *Caprinella* (fig. 11), one of the Hippuritidæ, is remarkable for the symmetrical regularity of its septal partitions. These cavities in the lower, *deep* valve, of bivalve shells, generally contain water, and are called "*water-chambers*."

The adult *Magilus* (fig. 7), on the contrary, fills up its shell, not with septal partitions or cancellated shell-structure, but with a solid, compact mass like arragonite.

No siphuncle, however, exists in any of these shells.

The shells of *all Mollusca* are composed of carbonate of lime; but, so different is the mode of its arrangement in the several orders, that their texture is as various and characteristic as that of arragonite and calcite, although, like these minerals, they have precisely the same chemical composition.

Some, when broken, present a dull lustre, like marble or china, and are termed *porcellanous;* others are pearly, or *nacreous*; some have a *fibrous* structure; some are horny; others are *glassy* and *translucent.*

In Nautilus the inner layer and septa are *nacreous*; the outer layer is *porcellanous.* The composition of the shell in Nautilus is, then, the same as in the other Mollusca. As in them, also, it is coated, when alive, with a layer of *epidermis* or *periostracum*, which is not a living membrane, and can only be reproduced around the mouth of the shell, or where it is within reach of the margin of the mantle. It is the umbonal portion of bivalve shells and the spires of univalves which first become eroded and injured; and one object, no doubt, in the formation of septa in all shells, is to shut off the damaged and untenantable part of their abodes.

Dr. S. P. Woodward, in his "Manual of the Mollusca" (p. 82), observes: "With respect to the purpose of the *air-chambers*, much ingenuity has been exercised in devising an explanation of their assumed hydrostatic function, whereby the Nautilus can rise at will to the surface, or sink, on the approach of storms, to the quiet recesses of the deep. Unfortunately for such poetical speculations, the Nautilus appears on the surface only '*when driven up by storms*,' and its sphere of action is on the bed of the sea, where it creeps like a snail, or perhaps lies in wait for unwary crabs and shell-fish, like some gigantic sea-anemone, with outspread tentacles."

Mr. Frederick Edwards, in his Monograph of the Eocene Cephalopodous Mollusca (Palæontographical Society, 1849, p. 12), says: "It is obvious therefore that the *hydrostatic balance* would be destroyed if any one of the deserted chambers were so injured as no longer to act as a float."

In Woodward's "Manual of the Mollusca" we find it also stated that "the *use* of the *air-chambers* is to render the whole animal, and shell, of nearly the same specific gravity with the water" (p. 82).

But no such buoy would be required for a *bottom-feeder*; indeed, it would prevent it from remaining below. I believe the facts of the case tend to show that, like the "water-Spondylus," the chambers were filled, or partially filled, with sea-water, which would be certain to find its way into the chambered portion of the shell, through its pores, thus displacing the air in spite of the animal. The Pearly Nautilus is said to inhabit a depth of from twenty to thirty fathoms. The pressure therefore of such a column of water must be very considerable, equal indeed to more than six atmospheres.

Mr. George Bennett, M.R.C.S., F.L.S., through whom Professor Owen obtained the first specimen of the animal of the Pearly Nautilus, described by him in 1832, states: "On laying carefully open that portion of the shell which contained the chambers, it was found to contain *water*, which of course immediately escaped." *

In 1870 I had an opportunity of opening the chambered portion of the shell of a *Nautilus umbilicatus*, which had been preserved, *with the animal*, in spirits of wine.† The last three chambers preceding that occupied by the animal were laid bare for a distance equal to half the circuit of the shell-whorl.

The siphuncle (when the chambers were laid open) was quite entire, and sheathed in a thin nacreous investment, which, however, attains to considerable thickness near to each septum.

The chambers contained a large quantity of fluid of which I did not specially take note at the time; but on reading Professor Owen's Memoir, I have no doubt that its presence in this, and also in Mr. Bennett's specimen, was *not abnormal* (as I had supposed), but in accordance with the natural state of *all* camerated shells, and that it is a *misnomer* any longer to call them "*air chambers*."

"How, then," Mr. Frederick Edwards inquires (op. cit. p. 12), "is the necessary communication between the animal and the 'air-chambers' maintained, and the vitality of the deserted shell preserved?" It has been shown that the siphuncle traverses the chambers to the extreme nucleus of the shell, and that it is provided with a small artery and a vein; and we also learn, from Professor Owen's Memoir, that in the *Nautilus* 'a delicate pellicle, distinct from the tube, is continued over the outer part of the testaceous tube, and also over the whole inner surface of the chambers.' "May we not, then," Mr. Edwards adds, "reasonably regard the siphuncle with its artery and vein, and the pellicle lining the air-chambers, as the organ destined to maintain the vitality of the shell, and feel ourselves justified in considering this office to be in fact the primary function of the siphuncle?"

Professor Owen, however, does not state that he detected the artery and vein beyond a few lines distance from its origin at the mantle. He writes as follows:—

* Extract from Mr. Bennett's "Journal," part x., quoted by Professor Owen, in his "Memoir on the Pearly Nautilus," 1832, p. 7.

† "British Association Reports," Liverpool, 1870, p. 128.

"From the extremity of the sac is continued a small tubular membranous process, which passes through the siphonic apertures in the septa of the shell, and is continued, there is reason to believe, to the innermost chamber. This tube has been surmised to be tendinous or muscular; but the attachment of the shell to the soft parts proves to be effected by much more adequate means. Rumphius appears to have been acquainted with its true structure, for he calls it an artery (*een langen ader*), and in fact within the external thin membrane are included a small artery and vein. How far these vessels are continued within the chambered portion of the shell, or in what manner they are distributed, remains for some future investigation; for in the present instance the only part of the shell that was preserved was the small portion adhering to one of the horny tendons, and the membranous tube had been ruptured, in removing the animal, *at a few lines distance from its origin at the mantle.* This tube appears to be contracted at its origin, and its diameter at the wider part is one line and a half." *

Admitting, however, that the purpose of the siphuncle is to maintain the vitality of the shell during the long life of the animal, how, we venture to ask, can this vitality be maintained in a non-vascular body? If the siphuncle be a means for repairing the shell, we ought to find some connection between it and the shell, but such does not exist; indeed, the fossil species have, in many instances, enormously thick nacreous or shelly tubes.

In fact, when the shell of the Nautilus, or of any other Mollusk, is once formed, it is extravascular, or *dead matter*, in the same sense that nails and hoofs and hair of higher animals are so—being incapable of repair, save at the *growing end* or where in contact with the shell-secreting mantle.

In the specimen of *Nautilus umbilicatus* already referred to, which I had the good fortune to examine, I observed the thin pellicle of membrane, described by Professor Owen, lining the chambers; but as it is only a film and presents no structure under the microscope, I conclude it to be deposited or left behind by the secreting surface of the mantle when the nacreous septum was formed. And this opinion is strengthened by Dr. Carpenter's statement "that in every distinct formation of shell-substance there is a single layer of membrane," and "that this membrane was at one time a constituent part of the mantle of the mollusc." †

The nacreous covering of the siphuncle was entire, and on removal it was found to enclose a simple membranous tube, composed of an extension of the *periostracum*, and exhibiting no structure even under a one-tenth objective.

* Professor Owen's "Memoir on the Pearly Nautilus," p. 10.
† "British Association Reports," 1844, p. 9.

As I could not detect any artery or vein, I conclude that they probably do not extend beyond the first chamber. This view coincides with Professor Owen's statement * that "neither the contents nor the vital properties of the siphon are, however, yet known; an artery and vein are assigned for its life and nutrition, and to extend a low degree of the same influence to the surrounding shell: but the structure of the membranous siphon, in the specimens from which I had the opportunity of examining it in a recent state, presents, beyond the first chamber, an inextensible and almost friable texture, apparently unsusceptible of dilatation and contraction: it is also coated beyond the extremity of the short testaceous siphon with a thin calcareous (nacreous) deposit."

We know that the body of the animal in Nautilus is attached to the shell by means of the two adductor muscles, and by a continuous horny girdle around the mouth of the body-chamber. The suggestion, therefore, of Von Buch, that the function of the siphuncle was to hold the animal into its shell, loses much of its significance.

But may it not have been the most important point of attachment between the animal and its shell in the *earlier forms* of the Tetrabranchiata?

In support of this view we may notice that in the fossil Nautili it was a shelly tube of considerable size and thickness (Plate LXXXII. fig. 4), whilst in *Orthoceras* it attained to a great magnitude—as, for instance, in the genus *Huronia*, in which the siphuncle is as large as a human vertebral column. In *Actinoceras*, *Gyroceras*, and *Phragmoceras*, the siphuncle is also very large, and contains in its centre a smaller tube, the space between the two being filled up with radiating plates, like the lamellæ of a coral.

Speaking of the connection between the *Nautilus pompilius* and its shell, Professor Owen † says: "A third point of attachment is to the bottom of the shell by the posterior extremity of the mantle, which probably presents a conical form in the embryo Nautilus."

If, then, the siphuncle in the young stage forms the main point of attachment between the animal and its shell, we may reasonably argue that the siphuncle in the adult Nautilus is simply the evidence of an aborted embryonal organ whose function is now fulfilled by the shell-muscles, but which, in the more ancient and straight-shelled representatives of the group (the Orthoceratites), was not merely an embryonal but an important organ in the adult.

* "Lectures on the Invertebrata," XXIII., 2nd edition, 1855, p. 594.

† Ibid. p. 592.

The formation of the septa is undoubtedly due to the constant onward growth of the shell, season by season, and in the female to the periodic development of the ova within the ovary of the parent; producing when discharged from the shell, a corresponding reduction in the size of the soft parts of the animal, and necessitating an equal reduction in the space of the body-chamber.

In youth these septa represent periods of rest in shell-growth; in middle-life periods of fertility; in age reduction of the shell to suit the reduced size of the Mollusk.

In this respect the septa in Nautilus agree with those found in other Mollusca.

"The line of attachment of both the muscles and the cincture progressively advances with the growth of the animals. A certain portion of the fundus of the shell thus becomes vacated, and the Nautilus commences the formation of a new plate for the support of the part of the body which has been withdrawn from the vacated shell. The formation of the plate proceeds from the circumference to the centre, and there meeting the conical process of the mantle, which retains its primitive attachment, the calcification is continued backwards for a short distance around the process which now forms the commencement of the membranous siphon, and acquires the partial protection of the calcareous tube. An air-tight chamber is thus formed, traversed by the siphon, which perforates its anterior wall or septum; by a repetition of the same process a second chamber is formed, included within two perforated septa; and similar but wider partitions continue to be added, concurrently with the formation of the new layers which extend and expand the mouth of the shell, until the animal acquires its full growth, which is indicated by the body having receded for a less distance from the penultimate septum before the formation of the last septum is begun." (Owen, op. cit. p. 592.)

In *Aturia zic-zac* (figs. 3, 4), the siphuncle is not continuous, but is composed of a series of funnel-shaped tubes inserted into each other.

In *Ostrea* I have observed that the shell-muscle appears to be the *last point* to be elevated upon the new shell-layer or septum formed within the lower valve, so that a section of an aged oyster exhibits a series of incipient siphuncular depressions, each fitting into that preceding it. (Plate LXXXII. fig. 12.) The attachment of the muscle in the oysters may perhaps offer the true explanation of the mode of the formation of the shelly tube or siphuncle in the Pearly Nautilus.

If we will only bear in mind this fact, that the animal of the Oyster and the Nautilus are each alike compelled by the constant, though almost imperceptible, growth of the mouth or border of the shell, to which it is attached by the margin of its mantle, *to move forward* in its habitation, and that its hinge-ligament and shell-muscles are absorbed behind, and added to in front, to accommodate themselves to the onward growth of the shell-border; of necessity, therefore, the animal cannot let go its muscular or its siphuncular point of attachment to the old septal surface until the new one is made ready: hence the dipping down from layer to layer of the oyster's shell-muscle (fig. 12); hence also the curious funnel-like tubes in *Aturia* * (fig. 4).

Although, as we are quite ready to admit, no analogy can perhaps be drawn between these two forms, occupying, as they do, the one the highest and the other the lowest place in the Testaceous scale, yet there nevertheless seems to be a definite mode of growth in all shell-structures, which even the highly-developed Cephalopoda share with the rest.

EXPLANATION OF PLATE LXXXII.

Camerated Shells.

Fig 1. Section of the shell of the Pearly Nautilus, *Nautilus pompilius.* Showing water-chambers (?) body-chamber, and siphuncle (Tetrabranchiata).

„ 2. *Spirula lævis*, New Zealand.

„ 3. *Aturia zic-zac.* London clay, Highgate.

„ 4. Broken shell of *Aturia zic-zac.* Miocene, Dax, to show funnel-shaped siphons.

„ 5. Section of *Euomphalus*, showing septa. Carboniferous limestone, Bolland, Yorkshire.

„ 6. Section of *Triton corrugatus*, showing septa formed in apex of spire.

„ 7. *Magilus antiquus*, Red Sea. The *magilus* lives fixed amongst corals, and grows upwards with the growth of the zoophytes in which it becomes immersed; it fills the cavity of its tube with *solid shell* as it advances.

„ 8. *Vermetus lumbricalis* (young), West Africa.

„ 9. Part of an old tube of *Vermetus maximus*, showing septa.

„ 10. Part of tube of *Teredo antenautæ*, London clay (cut to show septa). Wetherell collection. Sheppey.

„ 11. Shell of *Caprinella triangularis*, Dem. (one of the *Hippuritidæ*). U. Greensand, Rochelle.

„ 12. Section of an aged oyster (*Ostrea Boblayei*), showing indentation in each shell-layer where the muscular attachment was fixed.

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* The same structure is noticeable in the delicate little nacreous and internal shell of *spirula* (fig. 2).

ON THE TEMPERATURE AND MOVEMENTS OF THE DEEP SEA.

By Dr. W. B. CARPENTER, F.R.S.

BEING THE SUBSTANCE OF A LECTURE DELIVERED BEFORE THE PHILOSOPHICAL SOCIETIES OF LEEDS, BRADFORD, AND NEWCASTLE-ON-TYNE, IN FEBRUARY 1872.

UNTIL a recent period, the bottom of the Deep Sea has been —if I may make use of an Irish "bull"—an unknown land to us; for the means of research into its condition were very unsatisfactory. For example, in the first place, with regard to temperature. If we let down a self-registering thermometer, which should give the lowest or the highest temperature which is there encountered, there is this source of error in the indications of the thermometer—that the enormous pressure of the water upon the glass bulb will very probably so alter the shape of the bulb as to force up the mercury in the tube, so as to cause it to register a temperature several degrees higher than that which it actually encountered. Now it has only been recently—through the ingenious contrivance of my late excellent friend, Professor Miller, of King's College—that this difficulty has been overcome. We found, on putting thermometers of ordinary construction into the water-chamber of an instrument constructed on the principle of the Bramah press, with a powerful force-pump that should subject these thermometers to pressure of any amount up to three tons to the square inch, that the very best instruments that had been previously relied upon were raised from *eight* to *ten* degrees by the pressure of the water forced in; and we found that inferior thermometers, such as had been used in many deep-sea soundings on former occasions, were raised from *twenty* to *fifty* degrees. So that you see there is no reliance to be placed on any previous deep-sea soundings as to temperature, except in this, that we know that the error of their thermometers could not have been *less* than a certain amount. For instance, when Sir James Ross and his companions carried on their deep soundings in the Southern seas, and found, as they very often did, at a depth of from 1,500 to

2,000 fathoms, that their thermometers indicated a temperature of 39 or 40 degrees, we now know that the smallest error of their thermometers being *seven* or *eight* degrees at those depths, the true temperature could not have been higher than about 32 degrees—that is, about the freezing point of fresh water. The means which Professor Miller suggested for overcoming this difficulty was extremely simple. It was merely to enclose the bulb of the thermometer in an outer bulb, sealed round the neck, a space being left between the two bulbs. Now that space was not left entirely empty; it was about three parts filled with fluid. You may ask, Why was the fluid introduced there? For this reason—if only air had been left in that space, the inner bulb would have been a very long time in taking the temperature of the water round the outer bulb; the air being a bad conductor, it would have been necessary to allow the thermometer to remain perhaps an hour before the mercury or spirit of the inner bulb would have taken the temperature of the water outside; but by introducing between the bulbs some spirit, that spirit conveyed the heat or the cold from the outer to the inner. Still the intervening space was not *filled* with the spirit, because if it had been, the pressure upon the outer bulb, and its consequent change of form, would have acted in the same manner upon the inner bulb; but by leaving void a part of that space, any reduction in the capacity of the outer bulb which pressure might produce merely diminished that void, and produced no alteration in the shape of the inner bulb. We subjected thermometers, which were thus protected, to the pressure of three tons to the square inch, and found that they did not rise more than about one degree; and that small rise was really due, we have reason to believe, to an increase of heat in the liquid occasioned by the pressure to which it was subjected. That is the mode in which the thermometer has been adapted to the purpose of obtaining the real temperature of the deepest ocean waters; and I shall show you what very important information we have derived from its use.

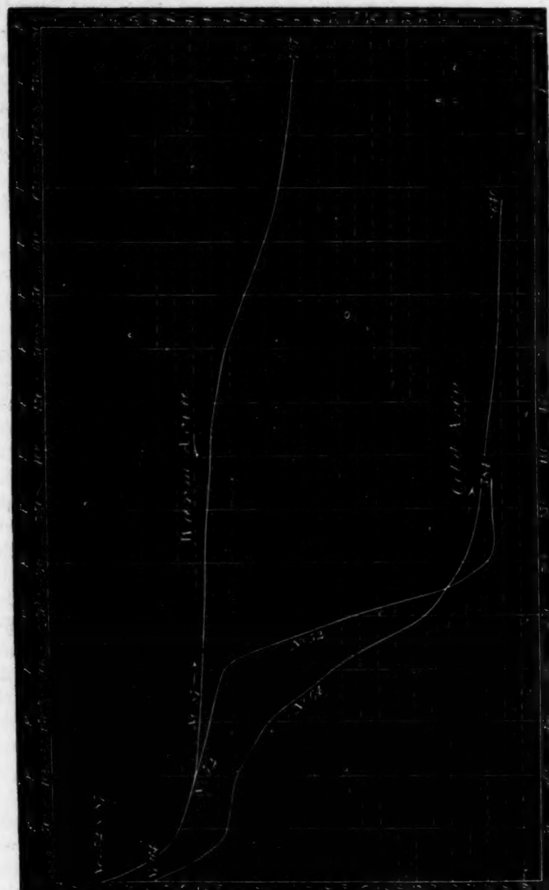
The pressure which is caused by a column of water of course varies with the height of the column—that is to say, with the depth of the water; and in round numbers we may say that at 800 fathoms the pressure of a column of water is one ton upon every square inch; therefore, at 2,400 fathoms, which was nearly the greatest depth to which our soundings extended, the pressure is three tons to the square inch; and that is just the pressure to which our thermometers had been tested. Therefore we know that we had within a degree (we always used two thermometers) the real temperature of the bottom of the ocean. Now I shall show you what very curious and important information we derived from ascertaining the temperature, not only of the bottom of the ocean at different depths, but also of different portions of the column of water in going down to the bottom. This we ascertained by letting down our thermometers to a certain depth, and then taking them up; then letting them down to a greater depth; and so on. In that manner we got what I term "serial soundings"—that is, a series of temperatures of different depths in the same spot; and those corresponded very closely indeed with the bottom temperatures that we got at like varying depths. As a rule, the lowest temperature was always the bottom temperature. I shall presently explain to you how this comes to pass.

Our first expedition was a very short one. We had very bad weather in a very stormy region, between the North of Scotland and the Faroe Islands, and we were not able to make many soundings or many dredgings; and yet, by a piece of extraordinary good fortune, the temperatures of the soundings that we obtained were as curious as any we have obtained since; and they suggested to me a general doctrine in regard to Oceanic Circulation, that all our subsequent researches have tended to confirm. The general facts of the case you will see by this map and the table by the side of it. Here is the north point of Scotland, the Orkney Islands, and Stornoway, the little port of the Hebrides from which we started. Here are the Faroe Islands. This dotted line is what is called the "hundred fathom line"—that is, the line which bounds that curious platform, so to speak, of which the British Islands constitute the highest part. So that dotted line around the Faroe Islands represents water which is under 100 fathoms. Now between this and the Shetland Islands is a deep channel reaching down to 600 fathoms, which is a depth nearly equal to the height of Snowdon. Our soundings in the first expedition were made along this line, where we found, in a part of the channel, very low temperatures, such as 33, 32·2, and 32 degrees. But at the like depth in another part of this channel, the soundings, as marked in the upper part of the table, show a temperature of 45 to 48 degrees. Here was a very marked and curious contrast; for within a short distance of each other, in one instance *only twenty miles apart*, we found two very different climates *at the same depth.*

Now the existence of these two very different climates showed itself, when we carefully worked it out afterwards, in two very distinct kinds of animal life, and in two very distinct kinds of deposit on the bottom of the ocean. I will first show how our next year's work in the same region filled up and completed this inquiry, and gave us some very curious points in addition. You may imagine with what interest we went over this ground again, provided with our superior thermometers; for the first year's work was done with the old thermometers, only the depths were not so great as seriously to interfere with their performance. And you will observe that whether those thermometers had been in error or not (which we did not know till we tried), the same effect would be produced in raising the mercury at 500 fathoms, whether it was in the warm or the cold area; so that the *difference* of the warm and the cold—between about 32 and 47 degrees—would be just the same. These thermometers having been a couple or three degrees too high—as they proved to be—we found that the temperature of the first year, which had been 32°, became 30°, and that which had been 47° was really 45°. But the difference of 15 degrees was exactly the same; and the conclusions at which we had originally arrived in regard to it were verified in the very careful, numerous, and elaborate inquiries which we prosecuted over this area the next year. The most remarkable contrasts of *bottom*-temperature were shown at different depths on the southern slope of the channel. Thus, at a depth of 190 fathoms the temperature was 48°·6; whilst *only eight miles* to the north, where the depth had increased to 445 fathoms, the thermometer sank to 30°·1; thus showing a difference of bottom-temperature to the amount of 18½° within that short distance, with a difference of only 255 fathoms in depth.

Again, we took what I have called "serial soundings;" that is, we let down our thermometers at different depths, for instance at 50 fathoms, then at 100, then at 150, then 200, then 250, and so on every 50 fathoms; and the results we got are shown in the diagram, which is so constructed that a curve indicates the descent of the thermometer, and the depths are expressed by the horizontal figures, which run from 50 to 100, 150, &c., marking every 50 fathoms.

In all this area, whether it was warm or cold at the *bottom*, we found nearly the same *surface*-temperature—a very curious fact. If we went north it was a little less, and if south a little more; but about 52 degrees was the average. We found that in all parts of this area the descent through the lowering of the thermometer in the first 150 fathoms was the same; and in the warm area, when we got below 150 fathoms, there was very little more lowering of the temperature. You see that the line in the *warm area* continues nearly horizontal till we pass about 500 fathoms; but from 150 to about 500 fathoms there was very little lowering of the temperature, the reduction being from 52° at the surface to about 45° at 500 or 600 fathoms. But now see what takes place in the *cold area*. This upper line, which at 100 fathoms is but a little below the other, begins to drop rapidly, so that at 200 fathoms it is very decidedly below; and then it goes down still more rapidly, so that within 100 fathoms it dropped about 15 degrees; and all the water in that particular sounding below 300 fathoms was of a temperature below the freezing point of fresh water. The bottom was there struck at 384 fathoms; but in another part we got a much deeper sounding, down to 640 fathoms, which was taken at a



Temperature-Curves in the Warm and Cold Areas; the Depths being represented by the *vertical* lines, and the Degrees of Fahrenheit's Thermometer by the *horizontal* lines.

point a good deal north: there the surface-temperature lowered to between 49 and 50 degrees; it went down in much the same manner as in the other, until it got to 350 fathoms, which was below the freezing point of fresh water; and from that point to the bottom (640 fathoms) was a river, so to speak, of glacial water nearly 2,000 feet deep—below the freezing point of fresh water. Now that was the very curious fact which our investigations of this channel between the Faroe Islands and Orkney and Shetland brought to our knowledge. That channel I have been accustomed to designate the "Lightning Channel," "Lightning" being the name of the vessel assigned to us in our first expedition. This cold stream must have come straight into this channel from the Polar area; but over it there was another stream proceeding north-east, consisting of water warmer than the normal water of the latitude; for this last would have been about 40°, while the temperature of the surface was about 52°, and even at 500 fathoms it only sank to 45°.

Now, then, what is the meaning of this? When I speak of a "stream" and "flowing," you must understand that there is nothing like a visible movement. I say that this cold stream must be flowing, because if it were not flowing it could not retain its temperature; it would soon give up its warmth to the water above. It is quite a physical necessity that it should be in movement; and of course if it is in motion, only by coming from the Polar area could it have brought this cold temperature with it, for at the bottom it was about 29½ degrees. You are aware that 32 degrees is the freezing point of *fresh* water; but it is not the freezing point of *salt* water. Sea water freezes at about 27°; if it is kept very still it will not freeze till 25°; and there is a most important difference in the condition of sea water and fresh water as regards temperature below 39·2 degrees. You all know perfectly well that when a frost acts upon the surface of a lake, river, or pond, the water freezes on the surface; and if you put down a thermometer into the water below, you will find that its temperature is about 39 degrees. Now, why is this? You know that the ordinary rule of the contraction of water is that it shrinks, just like the mercury in a thermometer, with cold, and expands with heat. As it shrinks it becomes denser, and therefore heavier, bulk for bulk; consequently when a low atmospheric temperature is acting upon the surface of a pond or lake, the water as it is cooled at the surface becomes heavier and goes down. So it keeps on going down, while the warmer water beneath, which is lighter, comes up to the surface, till the whole is cooled down to about 39·2 degrees; but then continued cold does not produce the same effect, for below 39·2 the water begins to expand again, the greater cold making it lighter instead of heavier; consequently the water which is cooled to below 39·2 degrees remains on the surface, and by continued exposure to the action of the cold atmosphere it freezes and forms a layer of ice. But that is not the case with salt water. Sea water continues to contract down to its freezing point; the more it is cooled the heavier it becomes, because its bulk diminishes; it therefore sinks in proportion to its degree of coldness; and in this manner it is that the coldest water nearly always comes to be at the bottom.

This has a most important relation to the doctrine of Submarine Climate. I have shown you here a sort of little compact pocket edition of a set of phenomena, which, as I am now going to explain, probably prevails over the whole of our great Oceans. In our soundings a few months ago on the coast of Spain and Portugal, we came upon this fact; the surface temperature was very high, about 65 degrees; in the first 100 fathoms we lost about 10 degrees of this, which we may call the *super-heating* of the surface, produced by the powerful rays of the midsummer sun. Then the temperature from a depth of 100 fathoms down to 800 lowered very slowly, just as it does in the "warm area;" so that at 800 fathoms it only got down to 49 degrees. But in the next 200 fathoms, between 800 and 1,000, there was a loss of 9 degrees, the temperature falling to 40; in another 100 fathoms, it fell another degree; and over the deeper soundings which we took in the previous year, extending down to 2,435 fathoms, or nearly three miles—a depth about equal to the height of Mont Blanc—we got a temperature as low as 36½ degrees; and still lower temperatures have been obtained elsewhere, even near the Equator. The recent temperature-soundings made by Commander Chimmo with the "protected" thermometers, in Lat. 3° S. and Long. 95° E., have given 35°·2 as the bottom-temperature at 1,806 fathoms, and 33°·6 at 2,306 fathoms. Here, then, you see we have in our great Oceans a condition just comparable with that which we found in the Lightning Channel: first we have an upper stratum of warm water; then we have what I have designated a "stratum of intermixture;" but below 1,000 fathoms, the water ranges from 39° nearly down to freezing point. Near the Pole it is quite down to freezing point; but when it is nearer the Equator, where it has had a long way to flow from the Pole, it will have acquired a certain slight degree of warmth; but still, you see, the finding a temperature of 33 or 35 degrees under the Equator, shows clearly that that water must have come from one or other of the Poles.

Let us now inquire what account can be given of this remarkable phenomenon. Here we have in the deep Oceanic basins this layer of water extending more than a mile deep—water which must have been derived from the Polar area. What account can we give of it? How does it come to be there? and how does it come to retain its low temperature? Now I think it may be said with perfect certainty, that it could not long retain its low temperature unless it was continually supplied from the Polar area. I will show you how this supply takes place. Here, for instance, in this Lightning Channel, we found that we could distinctly trace it along near to the corner of the Faroe Banks; and though we had not the means (which I hope we may at some future time) of measuring its movement, yet by the nature of the bottom we felt pretty sure that it was a running stream, for the pebbles there instead of being angular were round—which you know is a distinct indication of a current. Well, then, we have every reason to believe that this stream ran on and discharged itself into the great Atlantic basin. For about 100 miles to the westward of this there is a deep slope, going down to 1,500 or 2,000 fathoms; and thus it would be one of the feeders, so to speak, of the great mass of Polar water in the Atlantic basin. Then between the Faroe Islands and Iceland there is a shallow bank; but between Iceland and Greenland, again, there is a wide and deep channel, through which a very large mass of Polar water can come down. And though no temperature-soundings have yet been made (so far as I am aware) in this channel, yet the character of the bottom, as shown in the "Bull-dog" soundings, corresponded so closely with that of our own *cold* area, as to justify the belief that the deep water is glacial. Now water cannot be always flowing out of the Polar basin, without water from some other source flowing into it; so that if there is such an *out*flow at its bottom, the circulation must be completed by a constant *in*flow of *surface*-water. While, then, the *deeper* water is coming *from* the Pole, there must be *surface*-water going *towards* the Pole.

You have all heard of the Gulf Stream. It is a great mass of water issuing from the Gulf of Mexico through the channel between the peninsula of Florida and the Bahama Islands, and flowing in a north-easterly direction. The very powerful current that passes through that narrow channel, flows at first at the rate of three or four miles an hour in a direction which carries it towards the Banks of Newfoundland and the Azores; and it is popularly believed to flow on towards the northern coast of the British Isles, and thence to Spitzbergen, Iceland, and even Nova Zembla. Now I have every reason to believe, from careful inquiries lately made, that this Gulf Stream really has not much to do with the phenomena of which I have been telling you, and that its influence pretty much ceases not far to the eastward of the banks of Newfoundland. The Gulf Stream is part of the *horizontal* circulation in the North Atlantic. I think you will easily understand the difference between a horizontal circulation and a vertical circulation. Look at the wind ruffling the surface of a pond. It blows the water in a particular direction, and produces little ripples. If it drives away the water, of course water must come in to fill up its place from some other part of the pond. That is a horizontal circulation; and the horizontal circulation in the Atlantic is produced in this way. The Trade Winds are always blowing between the tropics from east to west; they move along an enormous mass of water, excessively heated by the action of the sun, constituting the Equatorial Current, and drive it into the Gulf of Mexico; it circulates there, and comes out from the Florida channel as a rapid current. But that rapid current, there is strong reason to believe, is not as deep as is commonly supposed; and the amount of the heat it carries has been very much over-estimated. As it passes along the coast of the United States (separated from it by a current of cold water that comes down from the north), it spreads itself out, becoming proportionally thinner, and at the same time slackening in its rate of movement. Its temperature progressively falls, especially in winter; and when the stream is reduced to a mere surface-film, it cannot retain a temperature much above that of the atmosphere. About half of it, when it come to the Azores, or Western Islands, turns round again, goes near the African coast, and returns into the Equatorial current; completing therefore one portion of the circulation I have spoken of. The other half goes on past the Banks of Newfoundland; there it meets the surface of the Arctic stream, which breaks it up or "inter-digitates" with it—this word expressing an action like that of passing one set of fingers through another. I admit that a portion of the Gulf Stream goes north, but the greater part of it is stopped and cooled by this Polar current coming down; and it is the southward continuation of this cold *surface*-current from the coasts of Greenland and Labrador, which gives the low winter temperature to the seaboard of the United States, and which forms the complement of the northern half of the Gulf Stream. It is known that Polar water also underlies the Gulf Stream; for if you send the thermometer sufficiently deep, you find a very low temperature beneath this extraordinary surface-current, even in the Florida channel.

I have adverted to the Gulf Stream, because I want to show the important influence of the upper movement of warm water of which I previously spoke, which is quite independent of the Gulf Stream. Suppose that the narrow peninsula of Mexico, or the narrowest part of it, the Isthmus of Panama, which connects North and South America, were broken through—as it will be in course of ages by the action of the sea—so that a free course should be given to the Equatorial current; it would then go right through into the Pacific Ocean, and we should have no Gulf Stream at all. But even in that case, I think our climate would not suffer so much as most persons believe;

because though we should lose some portion of our warm south-westerly winds, this constant flow of warm water which is taking place in the whole mass of the North Atlantic—from the southerly area directly towards the north and north-east, so as to enter the Polar area—will still continue, carrying with it a temperature which, taken altogether, is very much greater than that of the Gulf Stream. For the last we know definitely of the Gulf Stream shows that it is thinned off to a layer of certainly not more than 50 fathoms, and perhaps less, and reduced to a temperature of about 65 degrees; whereas this great slowly-moving mass of water carries a temperature higher than the temperature of the latitude down to 500 or 600 fathoms' depth; and as the surface is cooled, warm water from below will come up to take its place; and in this manner will carry into the Polar area a great body of heat derived from the general surface of the Temperate and Tropical oceans. And this I believe has taken place in all Geological periods, quite irrespective of any such local accidents as those which produce the Gulf Stream. There must have been in all Geological periods a movement of this warmer water from the Equatorial towards the Polar area, and conversely (and this is most important geologically) a movement of cold water in the depths of the oceanic basins, from the Polar towards the Equatorial area, bringing with it the characteristic animals of the Polar climate.

But you will ask, and very properly, "What evidence have you of this movement?" and "What produces this movement?" Now, the evidence of such a movement lies in the fact that cold water could not remain cold water at the bottom of these Oceanic basins, if the supply were not kept up from the cold basins at the Poles. I will give you an illustration. We were at work this last summer in the Mediterranean; and we found its condition most curiously different in regard to temperature from the condition of the Atlantic. The Mediterranean is a basin which, to use a Scotch word, is "self-contained;" it is shut in almost entirely, the Strait of Gibraltar being its only communication with the outside; and that Strait is so shallow at its outlet, that no communication between the deep water of the Mediterranean and that of the Atlantic can possibly take place. The Mediterranean goes down in some parts to a depth of 2,000 fathoms; we ourselves sounded to above 1,700, that is from about 11,000 to 12,000 feet. We found the surface very hot, being there in August and September; the temperature of the surface of the sea rose to 78 degrees in some instances. But we found that hot temperature limited to a very shallow layer indeed; we lost 10 or 15 degrees of that heat in 30 fathoms; at a depth of 30 fathoms we found the temperature perhaps 63, or sometimes as low as 60 degrees. Then a further loss of temperature would be experienced in going down to 100 fathoms. At that depth we came almost invariably to 54 or 55 degrees; and whatever was the temperature at 100 fathoms, that it was down to the very bottom; depth there made no difference at all; if it was 55 degrees at 100 fathoms it would be 55 at 1,700 fathoms; and if it was 56 degrees at 100 fathoms it would be the same at the greatest depth. There was a little difference in different parts of the area, which can be explained by local causes; but, as a rule, whatever the temperature was at 100 fathoms, that it was at the bottom.

Now what is the cause of this difference between the Mediterranean and the Atlantic? In a basin of very great depth, like the Mediterranean, why should the temperature be thus curiously uniform? Simply because it is entirely cut off from this General Oceanic Circulation, so that the water takes the temperature of the crust of the earth at that particular part. I will give you some curious evidence that such is the case. Thermometers buried deep in the soil in Central Europe are found to vary very little indeed during the different seasons. At about 20 or 30 feet from the surface they are not deep enough to be influenced by what is called the "internal heat of the earth," which you experience when you go down into a deep coal-pit, for instance, or which shows itself in the hot water from very deep springs; and at that depth they are covered with a layer of earth which is a sufficiently bad conductor to prevent their being much influenced by season changes; they therefore take the *permanent temperature* of the crust of the earth, and that permanent temperature in Central Europe is found to be about 51, 52, or 53 degrees. Now I found that there was a cave in a little island which we visited between Sicily and the coast of Africa, which has the reputation of being "icy cold." I was very anxious to visit it, but circumstances did not allow of our doing so; however, I had afterwards the opportunity of learning that the temperature of this cave is 54° through the whole year. Then a Maltese gentleman, the collector of customs at Valetta, a very intelligent and well-informed man, told me that it is the practice among the natives to let down their wine to cool it in the deep tanks which they have excavated in the rock. I asked him if he happened to know the temperature in these deep tanks, and he said, "Yes, it is 54 degrees." So you see we have several pieces of confirmatory evidence, showing us that the bottom-water of the Mediterranean takes exactly the temperature of the crust of the earth on which it rests.

If, then, it were not for the *vertical circulation* of the water in our great Oceanic basins, the temperature of the bottom of the Atlantic would be 55°, like that of the Mediterranean within the Strait of Gibraltar. But see what we get a little outside that basin. Near the coast of Spain, only 100 or 200 miles from Gibraltar, we found the temperature 49° at 800 fathoms, and we got down to 39° at 1,100 fathoms. Now this shows perfectly clearly that such a low temperature could only be sustained by a constant flow of water from the Polar basin towards this southern region. Then, as I have shown you, that outflow could not continue without an inflow into the Polar basin. And that brings me to show you what is the force that maintains this circulation. It is produced by the continual cooling of the water which flows into the Polar area; for it becomes heavier and falls to the bottom, displacing the water previously there, pushing it away as it were. Thus, there is a constant sinking of water in the Polar area exposed to a much colder atmosphere; for every fresh layer of water that comes in from the warmer sea around is cooled in its turn; it then sinks and goes down, down, down; and this colder and denser water creeps gradually along the deepest parts of the great Atlantic basin, and now and then, by some peculiar conformation of the bottom, it will come nearer to the surface, as it does in this Lightning Channel. If we are ever able to trace the Lightning Channel further north, it will be a most interesting point to determine what it is that sends up the cold water so much nearer the surface there than it has been found anywhere else in the same latitude. But we have a parallel fact in the case of Gibraltar, where I have lately been able to prove very distinctly that the water from the deeper portion of the Mediterranean basin is passing as an under-current outwards through the shallowest part of the Strait, beneath the surface-current that is continually flowing inwards from the Atlantic. Thus, then, you see what is the moving force. It is this constant reduction of temperature, which increases the density of the water and disturbs the equilibrium too. Suppose we had a Polar column of water of a certain height at this end of the room, and an Equatorial column at the other end. As this Polar column is cooled down, it contracts and becomes denser; thus its level is lowered, and the water will flow towards its surface to bring up that level. When this column of dense Polar water has on the top of it the additional water which has flowed in to maintain the level of that column, it becomes considerably heavier than the corresponding Equatorial column at the other end. What is the consequence? Why, that a portion of the lower part of it must flow away. Thus there will be a tendency to a renewed lowering of the level, which must draw in water from the Equatorial region; and there will always be, as that water flows in and is cooled down, a tendency to the maintenance of a greater weight or downward pressure ofw ater in the Polar area; so by these two influences—the lowering of the level, and the increase of the density of the column—we have this constant disturbance of level and disturbance of equilibrium, producing an inflow from the Equatorial towards the Polar regions on the surface, and an outflow from the Polar towards the Equatorial area at the bottom.

This is the doctrine of the General Oceanic Circulation to which I have been led. I say "I," because it has happened that I have been the member of the Expedition to whose share this part of the inquiry fell, and I have applied myself to all the points bearing upon it. I have taken the opinion of some of the most eminent Mathematicians and Physicists of this country, with regard to the validity of the principles I have advanced; and I am glad to say that I do not bring them forward merely on my own authority, but am assured that this doctrine will stand the test of very rigid inquiry. A short time before the death of Sir John Herschel, I had the satisfaction of receiving a letter from him, fully accepting the doctrine I have propounded; and his acceptance is the more significant, since he had previously repudiated the doctrine of Captain Maury, that an Oceanic Circulation (of which he regarded the Gulf Stream as a part) is maintained by the expansion produced by Equatorial heat.

"Assuredly," wrote Sir John Herschel, "after well considering all you say, as well as the common sense of the matter, and the experience of our hot water circulation-pipes in our greenhouses, &c., there is no refusing to admit that an Oceanic circulation of some sort must arise from mere Heat, Cold, and Evaporation, as *veræ causæ*; and you have brought forward with singular emphasis the more powerful action of the Polar Cold, or rather the more *intense* action, as its maximum effect is limited to a much smaller area than that of the maximum of Equatorial Heat. The action of the Trade and Counter-trade Winds in like manner cannot be ignored; and henceforward the question of Ocean-currents will have to be considered under a twofold point of view"—namely, as he goes on to explain, the *horizontal* circulation produced by the action of Wind on the surface, and the *vertical* circulation dependent on opposition of Temperature.

At a meeting of the Royal Geographical Society I was enabled, by the kindness of Dr. Odling, to exhibit an illustrative experiment, which was considered extremely satisfactory; and I think I can explain it to you in such a manner that you will easily understand its value. We had a trough, with plate-glass sides, about six feet long and a foot deep, and the sides not more than one inch from each other. At one end of this trough a piece of ice was wedged in between the two sides; that represented the Polar area. At the other end we applied heat at the surface, not at the bottom—to imitate the exact conditions of the case—the heat being applied by a bar of metal which was laid on the surface of the water, and then carried over the end of the trough and heated by a spirit lamp; that represented the Equatorial area. Then we put in some colouring matter, red at the warm end, and blue at the cold end. What happened? The water tinged with blue put in at the surface of the Polar area, being chilled by contact with the ice, immediately fell down to the bottom; it then crept slowly along the bottom of the trough, and at the Equatorial end it gradually rose towards the surface; and, having done so, it gradually returned along the surface to the point from which it started. The red followed the same course as the blue, but started from a different point. It crept along the surface from the Equatorial to the Polar end, and there fell to the bottom, just as the blue had done, and formed another stratum, creeping along the bottom and coming again to the surface. Each colour made a distinct circulation during the half hour in which the audience had this experiment in view.—Now that was a very beautiful experiment; and I can myself see no flaw in the application of the argument, that what is true on a small scale in this trough is true of a mass of water extending from the Equatorial to the Polar area.

Lastly, let us return for a moment to the subject of Deep-sea Climates. You see that this *vertical* circulation is a great Cosmical matter—not a mere local phenomenon, and not confined to the present time as the Gulf Stream is. It is a phenomenon which must have had its place in all Geological history. The Gulf Stream, and the superficial Arctic current which brings its water back again, constitute a *horizontal* circulation, the continuance of which depends on the interruption of the Equatorial Current by the coast-line of Central America. But wherever there were deep seas, and the Polar water and Equatorial areas were in communication, there must have always been this *vertical* circulation.

One very curious consequence of this vertical circulation, which I believe to be very important in relation to the Life of the ocean, is this—that by its means, if this doctrine be true, every drop of water in the ocean will, in its turn, be brought from the bottom and exposed to the surface. Now, in the Mediterranean there is no such circulation; and we found in the great depths of the Mediterranean an extraordinary paucity of animal life, instead of finding the abundance which we encountered in the great depths of the Atlantic. I will not say that this is the sole cause of the difference, but it has a good deal to do with it. These depths are stagnant; there is nothing to change them; for they are completely cut off from the depths of the Atlantic; and the only vertical circulation to which they are subject consists in the descent of water which has been concentrated by evaporation on the surface, and which, becoming heavier by concentration, will go down, but will soon diffuse its excess of salt, so as not to reach any great depth. Thus it is obvious that the condition of any "self-contained" basin, like the Mediterranean, must be extremely different Biologically, and therefore Geologically, from that of an Oceanic basin forming part of the great Water-system of the globe.

THE ECLIPSE OF LAST DECEMBER.

BY RICHARD A. PROCTOR, B.A., HON. SEC. R.A.S.,
AUTHOR OF "THE SUN," "OTHER WORLDS," ETC.

A VERY remarkable degree of success rewarded those who undertook the long journey to the Indian Peninsula for the purpose of observing the eclipse of December 12. The last of a series of important solar eclipses—for two years must now elapse before eclipse expeditions are again thought of—the recent eclipse did not promise to add much to our knowledge of solar physics. The totality did not last nearly so long as in the great Indian eclipse of August 1868; the track of the moon's shadow was less favourably situated; and there were reasons for fearing that monsoon weather would wholly mar the prospects of the observers at not a few of the stations which it seemed desirable to occupy. As it chanced, however, favourable weather prevailed at all the stations in India and Ceylon, save one only, and the only really unfortunate event in the whole history of the eclipse was the complete failure of the Australian observing parties, stationed on the shores of the Gulf of Carpentaria.

In summing up the results obtained during the late eclipse, it seems desirable—as well to save space as to give prominence to the more important achievements—to discuss only those observations which either confirm disputed results or appear to establish new ones. The arrangement, therefore, which I shall adopt, will have reference neither to the various parties into which the several expeditions were distributed, nor to the complete work of particular observers, nor to the classifications of results as obtained by spectroscopy, polariscopy, or photography, but solely to the solar features which have been revealed or rendered more clearly discernible by the work accomplished last December.

Beginning with the immediate neighbourhood of the photosphere, we find that the existence of the complex and relatively shallow atmosphere, whose existence was suspected by Secchi, but first actually demonstrated by Professor Young of America, has been confirmed by several of the observers of the late eclipse.

It may perhaps appear strange to speak of the confirmation of an already demonstrated circumstance, but the present is not the only occasion in the recent history of eclipses, when observers have been invited to demonstrate the demonstrated. We may now assume that the important discovery in question will be admitted by all. Let us consider its nature. Father Secchi had suspected, from the circumstance that the spectrum of the extreme edge of the solar disc is continuous, that the atmospheric layer whose absorption causes the dark lines of the solar spectrum, must lie immediately above the photosphere, and be of relatively limited extent. Thus, in full daylight, the detection of this atmosphere by the bright lines which must in reality constitute its spectrum, is rendered almost impracticable * by the extremely minute angle subtended by the apparent breadth of the ring it must really present. In total eclipses, however, this difficulty is momentarily removed when the moon just conceals the sun's disc, or at the moments of second and third contact. The edge of the moon, at the place of contact, acts for the moment the part of an analysing instrument, revealing the shallow atmosphere but hiding the photosphere. Taking advantage of this circumstance, Professor Young, in December 1870, was able to see the beautiful bright line spectrum of the complex atmosphere, hundreds of bright lines replacing the solar dark lines so soon as the continuous background of the spectrum had disappeared.

One of the first telegrams received from the observers of the recent eclipse announced the confirmation of Professor Young's observations by Colonel Tennant. Captain Maclear, who was at Bekul, succeeded also in seeing the bright line spectrum. Professor Respighi (who adopted a novel method of observing) failed at the second contact, and was but partially successful at the third, being in doubt whether what he saw was a spectrum of bright lines, or simply a discontinuous spectrum; but we shall presently see that his method of observing, though admirably suited for the chief purposes he had in view, was not well adapted for the recognition of this particular phenomenon. Mr. Mosely at Trincomalee, and Mr. Lockyer at Bekul, did not recognise the bright line spectrum—a circumstance not very surprising, since the appearance of the lines is little more than momentary. It need hardly be said that the positive results obtained by other observers can by no means be called in question because in all cases the bright lines were not caught sight of.

It was not expected that any fresh information would be

* Possibly quite impracticable; but the question is one depending on the telescopic aperture and defining power.

obtained during the recent eclipse respecting the prominences and sierra; for the study of these objects proceeds under far more favourable conditions when the sun is not eclipsed. Yet the method of observation employed by Professor Respighi enabled that careful observer to enunciate some interesting facts bearing on the condition of the coloured appendages. His method was specially intended to reveal the true extent of the self-luminous gaseous atmosphere or atmospheres surrounding the sun. He thus describes it: 'It appeared to me that the form and dimensions of the corona might be very conveniently studied by means of a large prism fixed in front of the object-glass of the telescope, whereby the several chromatic images of the corona would be distinctly formed in the focal plane. If the prism has but little dispersive power, and the eye-piece does not magnify too much, all the chromatic images of the corona may in this manner be observed simultaneously in the same field, and their form and dimensions directly investigated." The essential point in which this method (originally employed by Fraünhofer) differs from the ordinary method of studying celestial objects with the telespectroscope, consists in this, that in the ordinary method we examine prismatically the image or part of the image formed by the telescope, whereas in Respighi's method the prismatic images of the object are examined through the telescope. The prism used by Respighi was made by Merz of Monaco in 1868. "My conviction," says Respighi, "of the great advantages which would be afforded by this instrument in the observation of the eclipse, induced me to carry it to India for that purpose," and he adds that he was glad to hear that Mr. Lockyer "had in like manner resolved to observe the corona by means of a spectroscope without a slit, being persuaded that this would be the most convenient method of solving the questions related to the corona itself.* Mr. Lockyer used the ordinary form of telespectroscope, only that a simple train of prisms without any slit was employed, according to a suggestion made early in 1871 by Professor Young of America.

It will be obvious that so long as the visible solar crescent was considerable, the solar spectrum seen by Respighi would be

* By some mistake it was stated in an interesting and ably written account of the eclipse in the "Daily News," that Professor Respighi adopted his method as the result of a consultation with Mr. Lockyer. It will be seen from Professor Respighi's account that this was not the case. It may be added that in the same account the open-slit method of studying the prominences is called by mistake the Janssen-Lockyer method. Huggins and Zöllner independently devised the method, which was first successfully applied by Dr. Huggins. (See Schellen's "Spectrum Analysis," English edition, p. 425.)

impure, simply because multitudinous images of the visible crescent would overlap each other. But so soon as the sun's crescent had become an exceedingly fine sickle the solar dark lines came into view, and were even more strongly marked than by the usual method. It was when this sickle was on the point of disappearing that Professor Respighi hoped to see Young's bright-line spectrum. He failed, as we have said, though the solar spectrum became continuous immediately before the totality (which, after all, demonstrates as effectually the existence of the exterior absorptive atmosphere as though the bright lines had been seen). At the end of totality, just as the sun's limb appeared, a stratum of bright lines was seen projected upon the solar spectrum; but Respighi 'could not determine whether they were due to a general or partial reversal of the spectral solar lines, or to a simple discontinuity in the spectrum, since they were too soon immersed in a flood of light which put an end to the totality.'

'At the very instant of totality,' says Professor Respighi, 'the field of the telescope exhibited a most astonishing spectacle. The chrom(at)osphere at the edge which was the last to be eclipsed—surmounted for a space of about 50° by two groups of prominences, one on the right the other on the left, of the point of contact, was reproduced in the four spectral lines,* C, D^3, F, and G, with extraordinary intensity of light and the most surprising contrast of the brightest colours, so that the four spectral images could be directly compared and their minutest differences easily made out. In consequence of the achromation of the object-glass, all these images were well defined, and projected in certain coloured zones, with the tints of the chromatic images of the corona. My attention was mainly directed to the comparison of the forms of the prominences on the four spectral lines, and I was able to determine that the fundamental form, the skeleton or trunk, and the principal branches, were faithfully reproduced or indicated in the images, their extent being, however, greatest in the red, and diminishing successively in the other colours down to the line G, on which the trunk only was reproduced. In none of the prominences thus compared was I able to distinguish, in the yellow image D^3, parts or branches not contained in the red image C.'

It must be pointed out, however, that some of the phenomena here described were, in all probability, merely subjective, and that the reliance placed by Professor Respighi in the particular method of discrimination he employed, does not seem altogether justified. The G image, or indigo image,

* That is, in the colours corresponding to these lines.

which seemed to him ill-defined and imperfect, is the very image which Fr. Secchi finds most perfect when the open-slit method is employed on the uneclipsed sun. Nor is it difficult to understand why the indigo image should have appeared less perfect during Respighi's eclipse observations than during Secchi's researches in full daylight. For Respighi had the full light of the red and yellow images, as well as that of the less brilliant blue-green image, shining in the same field as the faint light of the indigo image, and therefore rendering his eye insensible to the feebler parts of this image. But when the open-slit method is employed in full daylight, the indigo image is observed under circumstances even more favourable than in the case of the red and orange-yellow images. For the part of the atmospheric spectrum which then forms the background of the indigo image is the indigo part of that spectrum; no red, yellow, or green light is admitted at all into the field of view.

It is more important to notice the effect of glare or strong light in the field of view than might at first sight appear. For though no very great importance can be attached to what Respighi noticed respecting the prominences (save as illustrating his method of observation), yet we learn, from comparing what he saw with what is known about the prominences, to estimate the effect of light in obliterating the fainter portions of circum-solar appendages, and this is a matter of paramount importance in its relation to Respighi's observations of the corona.

Before passing to the corona, however, it will be well to notice that Mr. Lockyer also saw the prominences and chromatosphere separated into four coloured images. But the four colours mentioned by Mr. Lockyer were not the same as those mentioned by Respighi, and there seems reason for believing that the indigo image lay outside the field of Mr. Lockyer's instrument, or else was too faint for recognition, while the extra image seen by Mr. Lockyer was not an image of the chromatosphere at all, but of the brighter part of the inner corona. The account first announcing Mr. Lockyer's observations ran thus, "Four circles, 1474 same size and faint," and subsequent explanation revealed that he had seen four images of the chromatosphere, one red, one yellow, one yellow-green and faint, corresponding to Kirchhoff's 1474, and the fourth blue-green. The faint image did not appear to him to extend more than two minutes from the moon's limb.

Respighi was favoured with better success, however. "The coloured zones of the corona became continually more strongly marked," he says, "one in the red corresponding with the line C; another in the green, probably coinciding with the line 1474 of Kirchhoff's scale; and the third in the blue, perhaps coinciding with F. The green zone was the brightest, the most uniform, and the best defined. The red zone was also very distinct and well defined, while the blue zone was faint and indistinct. The green zone was well defined at the summit, though less bright than at the base; its form was sensibly circular, and its height about 6′ or 7′. The red zone exhibited the same form and, approximately, the same height as the green, but its height was weaker and less uniform. . . . These coloured zones shone out upon a faintly illuminated ground without any marked trace of colour. If the corona contained rays of any other kind, their intensity must have been so feeble that they were merged in the general illumination of the field. . . . . The green and red zones were well developed at the western as well as at the eastern limb, while the blue remained faint and ill defined."

From the evidence it appears clear that, as had been surmised by Young, the gaseous atmosphere containing the 1474-matter extends to a height of more than two hundred thousand miles from the photosphere; but hydrogen also is present in this deep atmosphere. The well-defined outer limit of the green image would seem to show that Respighi had recognised the real extension of the atmosphere. Yet it must be admitted that some doubts rest on this conclusion. For the recognition of a well-defined boundary might have been held to be a reason for regarding the apparent limits of the chromatosphere as the real limit of the solar atmosphere; whereas we now see that the atmosphere is very far deeper than the sierra. May it not well be that, as the observed extension of the sierra indicates, *not* the extension of the hydrogen atmosphere, but that of the D^3-matter, so the observed extension of the green zone indicates, *not* the extension of the 1474-matter, but of the hydrogen atmosphere? There is a sudden degradation in the brightness of the hydrogen images of the sierra where the D^3-matter is limited; and in like manner there may be a sudden degradation of the 1474 image of the corona where the hydrogen is limited.

Whatever opinion we form on this point, no doubt can exist that circumsolar matter of some sort extends to distances much greater than the 6 or 7 minutes mentioned by Respighi.

The naked eye and low power telescopic views of the corona were in some instances obtained under conditions unusually favourable. At Bekul Captain Maclear could trace the corona to a distance of upwards of 45 minutes from the moon's limb. In the finder the corona had a much smaller extension; in a large telescope six inches in aperture, he could see nothing but a bright light round the moon, not much higher than the largest of the prominences visible at the moment. Nothing could more forcibly indicate the extreme faintness of the outer coronal light. Captain Maclear describes the corona as six-pointed. Mr. Lockyer at the same station gives the following account:—"There, in the leaden-coloured, utterly cloudless sky, shone out the eclipsed sun! a worthy sight for gods and men. There, rigid in the heavens, was what struck everybody as a decoration, one that emperors might fight for, a thousand times more beautiful than the Star of India (even where we now are), a picture of surpassing loveliness, and giving one the idea of serenity among all the activity that was going on below, shining with a sheen as of silver essence (*sic*), built up of rays almost symmetrically arranged round a bright ring, above and below, with a marked absence of them right and left, the rays being composed of dark radial lines separated by furrows of markedly less brilliancy." Further on he describes the structure of the corona as "resembling Orion," though how that statement is to be understood is not altogether clear: possibly it means that the corona occupied a space on the heavens resembling in shape that occupied by the constellation Orion!

Mr. Holiday, the artist, "sketched the corona with his right eye while the left was at the telescope." He removed the dark glass when Baily's beads appeared, but the glare was too strong, and he at once replaced it. In this momentary glance then obtained, he had a view of the rays commonly seen before totality, which he imagined to be "the two great lines marking the limit of the advancing shadow." "After a few seconds the glass was removed, and there, in all its glorious beauty, was a grand corona of the most fantastic type, not unlike the one given by Liais. To outline this carefully was the work of a few seconds." "Curiously enough," proceeds the account (though we should have thought "naturally enough" the more correct reading), there are points of difference and points of agreement between this drawing and the photographs, which will doubtless, when the time comes, undergo the most searching examination. After the middle of the eclipse another drawing was made, showing that the corona had become much more diffuse" (or rather could be traced much farther) "than at its first appearance, and it maintained the same form nearly till the reappearance of the sun."

The photographic results are among the most important successes achieved during the recent eclipse. It will be evident, however, that, until the pictures are published, it is not possible to give any sufficient idea of the inferences likely to be deduced from them. At present it must suffice for us to mention the following particulars. At Bekul Mr. Davis took five excellent photographs; Colonel Tennant's party took three; Mr. Pogson's, at Avenashi, took three; while lastly the photographers at

Jaffna took six. None of the photographs show so great an extension of the corona as is seen in Mr. Brothers's photograph taken at Syracuse in December 1871: but, on the other hand, the coronal features are better defined; and necessarily the great number of the photographs renders the value of the series singularly great. The agreement between the views, as well those taken at different times during totality as those taken at different stations, is greater even than the solar theory of the corona absolutely requires, for it is not incompatible with recognisable variations. However, not even such difficulties, or at least none worth noticing, present themselves. We have in all the views the same extensive corona, with persistent rifts, similarly situated.

Let us now turn, however, to the results which must be regarded as the great success of the eclipse observations, unless Respighi's observations be thought to have a rival claim.

Janssen, who had been sent out to observe the eclipse at Java, preferred, after careful enquiry, to take up his station on the Neilgherry Mountains, at Sholoor, on the north-eastern flank of the range. To this elevated station (the highest we believe at which the corona has ever been observed telescopically) Janssen carried a fine telescope 14 inches in aperture and only 4½ feet in focal length, showing images twelve or sixteen times brighter than in an ordinary astronomical telescope. The spectroscope, also, was so constructed as to utilise all this light. It will be understood, therefore, that Janssen observed the corona under exceptionally favourable conditions, more particularly as he was favoured with a sky of almost absolute purity.

And first as to the direct observation of the corona. Janssen was able to recognise those peculiar forms which, as drawn by Liais, had so long been regarded with doubt, if not with ridicule. "Nothing could be more beautiful, more luminous," he says, "with special forms excluding all possibility of a terrestrial atmospheric origin." "Without entering upon a discussion which will form part of my report, I shall say at once that the magnificent corona observed at Sholoor showed itself under such an aspect as to render it impossible for me to accept in explanation either the phenomena of diffraction, or reflection on the lunar globe, or the mere illumination of the earth's atmosphere."

It is, however, the spectroscopic work of Janssen which deserves our chief attention. "The reasons," he says, "which militate in favour of an objective and circumsolar origin acquire an invincible force when we examine the luminous elements of the phenomenon. In fact, the spectrum of the corona has not shown itself (in my telescope) continuous, as it has hitherto been found, but remarkably complex. I have discovered in it the bright lines (though much enfeebled) of hydrogen gas, which forms the principal element of the prominences and sierra; the bright green line which has already been noted during the eclipses of 1869 and 1870, as well as some other fainter lines; and the dark lines of the ordinary solar spectrum, notably that of sodium. These dark lines are much more difficult to perceive. These facts prove the existence of matter in the sun's neighbourhood—matter revealing itself in total eclipses by phenomena of emission, absorption, and polarisation. But the discussion of the facts leads us yet further. Besides the cosmical matter independent of the sun, which must exist in the neighbourhood of that orb, the observations demonstrate the existence of an atmosphere of excessive rarity, mainly composed of hydrogen, extending far beyond the chromatosphere and protuberances, and fed from the very matter of these—matter erupted with great violence, as we perceive, every day. The rarity of this atmosphere, at a certain distance from the chromatosphere, must be excessive; so that its existence is not in disagreement with the observations of the passage of certain comets close to the sun." In a letter to Professor De La Rive,* Janssen proposes to call the atmosphere which he and Respighi have thus independently recognised the *coronal atmosphere*, a title which indicates that it is this atmosphere which produces the chief part of those luminous phenomena which have been designated hitherto by the name of the *solar corona*. "*In announcing this result*," he adds, "*I do not for my own part forget all we owe to those labours which have prepared the way for it, notably those of the American astronomers during the eclipses of* 1869 *and* 1870"—a worthy declaration, and likely to remove in a great degree the impression produced by the somewhat too persistent doubts which had been suffered to rest on the accuracy of the results obtained by the American observers.

In the above summary of Janssen's observations, the chief place must be assigned, I conceive, to his recognition of the existence of dark lines in the spectrum of the corona. The fact that faint bright lines were perceived other than those belonging to hydrogen and the 1474-matter, is interesting and significant, while the recognition of the hydrogen lines is an important result, the credit of which, however, M. Janssen must share with Respighi; † but the recognition of the dark lines was a

* "Bibliothèque Universelle," January 15, 1872, p. 103.

† Perhaps also with Mr. Lockyer; but on this point, until a more exact account of his observations has been rendered, I am uncertain. Mr. Lockyer has not definitely stated the height at which he recognised the hydrogen lines; and his own statement that he had independently proved the existence

work of far greater difficulty, and immensely more important. More important, because no one could gravely question the fact that the prominences exist within an atmosphere, while the inference that that atmosphere was mainly hydrogenous seemed altogether probable, whereas the existence of the dark lines in the continuous spectrum of the prominences throws an altogether new light on a theory which had long been disputed.

Whence do these dark lines come? They cannot belong to the inherent light of the corona, nor can they be due to absorptive action exerted by the outer parts of the corona on the white light producing the continuous background of the spectrum. Those dark lines which would be the most marked in this latter case, are precisely the lines which appear bright in the coronal spectrum—the lines of hydrogen. Moreover, it is incredible that the cool vapour of sodium exists in quantities adequate to account for the presence of the sodium double D line; so that even though no other dark lines were discernible, it would be certain that we have not a phenomenon of coronal absorption to deal with. No other theory remains, then, than this—that the portion of the coronal light which supplies the faint continuous spectrum crossed by dark lines is reflected sunlight. But how reflected? Not by matter in the earth's atmosphere, or between the moon and earth, for that is physically impossible, apart from the independent evidence just obtained against the glare theories. Not by gaseous matter near the sun, for Professor Tyndall has proved that matter in the gaseous state is incapable of reflecting any light whatever. How, then, can this light be reflected, save from that cosmical matter independent of the sun, to which Janssen refers, and which, as he truly remarks, *must* exist in his neighbourhood? It seems to me that this discovery of Janssen's of itself suffices to *demonstrate* the meteoric theory of the origin of at least a considerable portion of the coronal light. I have always regarded this theory as irresistibly strong, altogether apart from spectroscopic evidence—as strong enough in any case to resist the negative evidence based on the failure of spectroscopists to recognise the solar dark lines in the coronal spectrum. Now that the dark lines have been seen, the basis of the meteoric theory, already strong, has acquired double width and firmness.

of hydrogen above "even the prominences" by Professor Young's method (that is, by the use of a simple train of prisms), is in direct contradiction to his own account of his observations by this method, in which he distinctly mentions two minutes as the apparent extension of the four circular images. By the old method—which, however, Mr. Lockyer himself denounced last year as untrustworthy—he obtained the hydrogen lines at a greater height from the sun than this.

As to the distance to which this meteoric corona, so to call it, extends, we need entertain no doubt that in reality it is coextensive with the solar system itself. The zodiacal light probably indicates its more condensed portion; but, inasmuch as our earth encounters meteors which come from beyond the orbits of Uranus and Neptune, we can entertain no question that the real limits of the meteoric matter lie far outside the apparent extension of the zodiacal light. How far the existence of light-reflecting matter can be recognised during eclipse is a totally different question. Nor is it one to which any definite reply can be given—for the apparent extension of the corona depends upon the condition of our atmosphere, the height of the station, the visual powers of the observer, and other circumstances of the sort. It may be mentioned, however, that during the late eclipse Captain Tupman recognised radial polarisation in the coronal light at a distance of from forty to fifty minutes from the moon's limb, showing that there was light-reflecting matter at a distance of more than a million miles from the sun's surface.

I cannot better close this paper than by quoting the words with which Janssen concluded his first letter to the President of the Paris Academy of Sciences: "The question whether the corona is due to the terrestrial atmosphere is disposed of (*tranchée*), and we have before us the prospect of the study of the extra-solar regions, which will be very interesting and fruitful." A pity only that this study should have been put back for two years through the enunciation of an erroneous theory, and that now, when the real significance of the corona has at last been recognised by all, there should be no prospect of effective eclipse observations for years to come.

THE LITHOFRACTEUR.

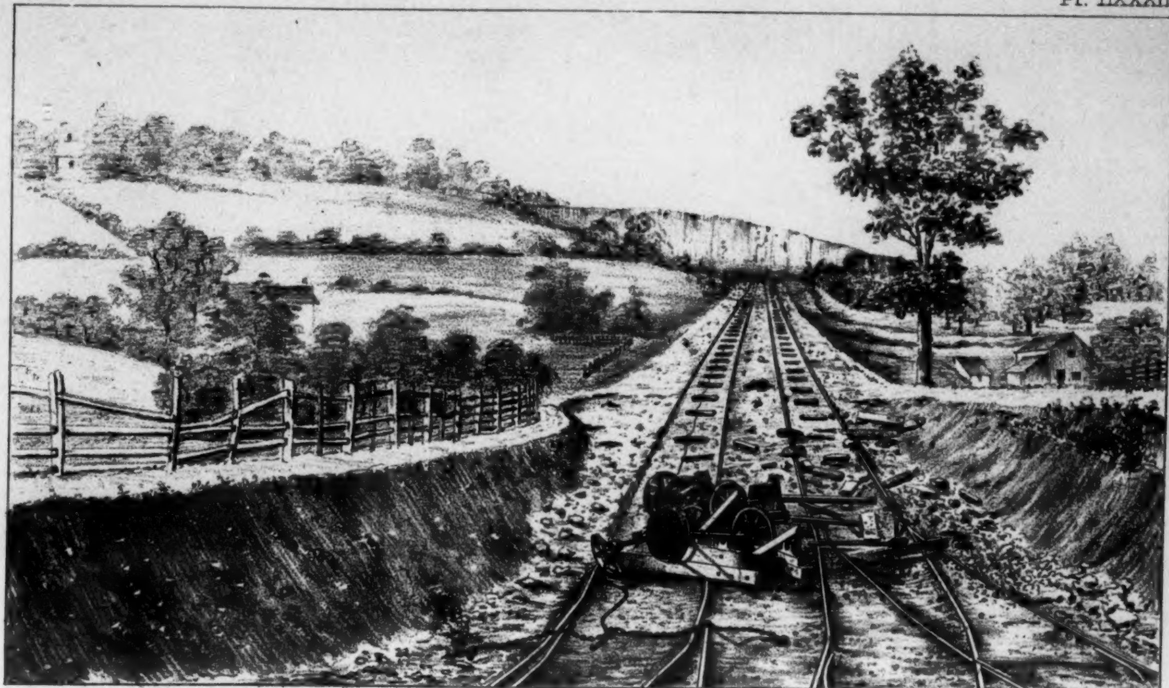
By S. J. MACKIE, F.G.S.

[PLATE LXXXIII.]

IT is not a little remarkable that the compounds of nitro-glycerine—a substance so mercilessly decried in this country for years past—should now have come to the front as the safest as well as the most powerful of explosives. Nitro-glycerine, or nitrate of glycerine, was discovered in the year 1847 by the Italian chemist Sombrero. It is a pale yellow oily fluid, of 1·6 specific gravity, which commences to stiffen and solidify when the temperature falls below 8° Centigrade, or 47° of Fahrenheit. It is insoluble in water, but can be mixed with methylated spirit, benzole, nitro-benzole, or a mixture of alcohol and ether. Against the use of pure nitro-glycerine a strong prejudice still exists in consequence partly of the poisonous nature of the substance, but mainly on account of its violent and sensitive nature and the many accidents which happened in its earlier applications. The bad character given to it by the Newcastle and Caernarvon explosions, however undeserved it may have been (for the treatment it was subjected to was such as gunpowder itself might have rebelled against), has undoubtedly retarded not only its own progress, but that of all blasting substances into which it entered as a base. Nobel, the Swedish chemist, was the first to tame its violence and to put it under complete control, so far as safety was concerned, by mixing it with the fine infusorial earth so well known to microscopists, and thus forming a simple mechanically plastic substance, handier as well as safer for use for mining and quarrying purposes. This substance, consisting of 75 per cent. of nitro-glycerine and 25 per cent. of siliceous meal, was proved both as to safety and power by numerous most important and convincing experiments, and has attained to a large sale for blasting operations under the name of dynamite. Thus compounded, the plastic mass is of a tawny reddish hue, of a consistency approaching that of putty. Set fire to in the open air, dynamite merely deflagrates; it will even burn when enclosed in ordinary boxes, producing only slightly noxious gases and very little or no smoke. It explodes only in completely closed spaces, and with difficulty by means of sparks. It may be, when thus confined, exploded by glowing hot metals, by bullets fired into it, and otherwise by deliberate artificial ignition. The disadvantage urged against it in its practical application is that when the temperature falls below 6° Centigrade, it hardens, and that then, particularly when loose cartridges or a series of cartridges are employed, the whole of the charge is not exploded, but a portion remains altogether undamaged, or is blown out unexploded. The cause of this stiffening is of course due to the natural crystallisation of the nitro-glycerine.

Lithofracteur, the invention of Professor Engels, is an improvement upon dynamite, and derives its advantages from the diminution of the proportion of siliceous meal as well as of the nitro-glycerine, the balance being made good by other ingredients of an explosive nature, the details of which constitute the secret of its manufacture, for neither abroad nor in this country has it yet been patented, Messrs. Krebs, of Cologne, the manufacturers, preferring reticence in this respect to the publicity which patents would necessarily give. It had acquired some time ago considerable reputation in Prussia, but became known in England through the military operations in which it was employed during the late Franco-Prussian war. It also is produced in a plastic state, being black in colour and doughy in consistency. When lit by a match or a cigar, like dynamite, it merely burns; but it is almost insensible to blows or pressure, and can only be exploded by a strong priming cap or detonator composed of the strongest fulminate. Attempts to explode it by charges of gunpowder have failed. How little sensitive it is to violent shocks has been shown by throwing it over cliffs of 150 ft. or 200 ft. in height; as also by firing shells fitted with it from cannon, which exploded only when they reached some hard object of stone or iron. Neither in combustion nor explosion does it generate smoke like gunpowder; but the gases formed are transparent, and only detectable by the smell. They are not pleasant to inhale largely, as headache would follow; but as diluted in the open air they cannot be regarded as poisonous, although it will be better to avoid being too much enveloped in them.

The advantages of lithofracteur over the old blasting gunpowder are very remarkable, and although the latter for certain work will still maintain its ground, the former will have predominance in a majority of operations. Besides its more energetic action and the important savings of time, labour, and tools which is effected by its use, its advantages are especially



W. West & Co. Lith.rs

Lithofracteur Experiment at Nantmawr, nr. Shrewsbury.

notable in its application in wet rocks and under water, as well as in the far less amount of danger incurred in transport from place to place, and in charging the blasting-holes. Lithofracteur possesses this advantage amongst others over dynamite, that it can be exploded at a lower temperature, and it is stated that it can be fired by an energetic detonator even at the temperature of $-12^{\circ}\cdot5$ Centigrade, whilst dynamite ceases to explode at $+5^{\circ}$ to 6° Centigrade.

For mining purposes the great desideratum is the quantity and nature of the work done by the explosive, for the main expense in such work is the cost of boring holes for the charges. If, then, these can be diminished in number as well as in dimensions by the greater power of the blasting material, the saving in the quarrying or mining operation becomes far more important than the difference in cost between the cheaper and less effective blasting powder and the nitro-glycerine compounds, although that difference in price may be considerable pound for pound. One may generally assume, according to the statistics so far attainable, that 1 lb. of lithofracteur acts as powerfully as from 6 lbs. to 7 lbs. of blasting powder; but no exact comparisons have yet been made, and indeed no means have been as yet found for absolute accuracy in such determinations. The difference in the price of the powder is also more than equalled in the saving of human labour in drilling the holes; and it is generally assumed that this gain alone deducts a third from the expense which the manual labour required for blasting powder entails. For the economical working with lithofracteur or dynamite, the holes should be neither too wide nor too deep; in most cases it is not required to exceed from 36 in. to 40 in. in depth, while for breadth $1\frac{1}{2}$ in. at most is sufficient. The harder and stronger the rock the wider must the hole be. One of the great values of lithofracteur is its power of blasting into perfectly flat faces, vertical or horizontal. This was admirably illustrated in the first experiments made in this country at the quarries of Mr. France, at Nantmaur and the Breidden, in May last year. One instance of absolutely horizontal boring into the fair face of the dense limestone quarry cliff at the former place, and one example of perfectly vertical boring into a floor of a tough homogeneous greenstone rock at the latter, jutting out at right angles from the towering cliff face of the latter noble hill 1,200 ft. and over in unbroken height, will suffice to show the really useful as well as powerful energy which this explosive is able to exert in the commercial work of quarrying. In the experiment at Nantmaur, three holes were drilled as absolutely horizontal as human hands could drive them into a bed of solid rock 10 ft. above the quarry floor, and forming a continuous part of the lower portion of the cliff 150 ft.

in height, and near to a salient angle where the quarry workings met at a right angle. The line of least resistance of the first charge was 5 ft. 8 in. from the corner; 4 ft. still further away from this first charge was placed the second charge, and at 4 ft. 8 in. yet again was the third charge. The bores were 3 ft. 4 in. in depth, and $1\frac{1}{2}$ in. in diameter. The thickness of the bed of rock was 18 in. Each hole was loaded with 1 lb. of lithofracteur, and was tamped with water. It need scarcely be said that gunpowder in such holes would merely shoot its solid tamping out like a shot from a gun, and that no work at all would be done in the rock. The middle or No. 2 charge was first fired, and blew out a cavern of triangular form over 1 ft. 6 in. along each of its bounding lines, and the depth of which cavern was 3 ft. 6 in.; the contiguous bed was shivered for a distance of 6 ft. on each side, and shaken and fissured over a length of 20 ft. From such a heading, further convenient workings were at once practicable. The other two shots were also similarly effected. We thus set the superior action of the violent nitro-glycerene detonation over the slower burning explosions of gunpowder. To give now an example of floor work, in which gunpowder would equally do no more than fire a shot. A flat toe of rock ran out from a bight in the Breidden greenstone cliff, and open on one side and in front only. This toe was 12 ft. in length, 5 ft. in width, tapering to a point, with its surface dipping from about 5 ft. extreme height at its junction with the cliff to the level of the floor at the toe; the charge, a little over 1 lb. of lithofracteur, being put midway on the slope in the position of the mean height of 2 ft. The bore hole was 3 ft. 6 in. deep and $1\frac{1}{2}$ in. in diameter. When the charge was fired, splinters of stone were sent into the air more than 800 ft. up; and the mass of the floor shattered completely over an area of 8 ft. 8 in. by 2 feet 2 in., and 3 ft. 6 in. deep, besides which eight considerable fissures were riven into the surrounding rock. Some of the blocks disrupted took five men with crowbars to move. The line of least resistance in this case had been 5 ft. 6 in.; but the explosion was so sudden and effective, that the charge acted in all directions alike; the fissures striking downwards as well as laterally—the full effects of the blast, indeed, not being ascertained until a considerable time afterwards in the course of the quarry workings. It has been said that 45 tons of stone were raised altogether from this one blast.

In Germany, lithofracteur has been used for the breaking up of pig and waste iron. This was first done at the suggestion of Herr Büttgenbach, director of the Neuss Foundry. It has also been similarly used at Krupp's great steel works at Essen. The following illustration of this class of work is from the account in the "Berggeist" of an experiment made in the Iron Foundry of Herr Holtinghausen, of Achenbach. A piece of cast metal 9 ft. long, 6 ft. wide, and 17 in. thick was broken into three large and many small pieces; and a second, 120 centners in weight, was shattered into four pieces. Over each block fifteen rails were laid, in order to protect the neighbouring houses. "These rails were hurled about 300 ft. up into the air, in many little pieces, affording a diverting spectacle." The charge is not recorded; but at the Essen experiment a block of cast iron, 53 in. long, 29 in. broad, and 19 in. thick, was split through at a single discharge by about 6 lbs. of lithofracteur, simply laid on the smooth surface and tamped with a few handfuls of loam.

Independent of the merely commercial trials in this country last year—which were the most extensive and important private experiments ever performed with any of the nitro-glycerine compounds—official investigations were made in February last, at the Nantmaur Quarries, by a committee of military and naval officers, with certain civilian members, under instructions from the Home Office, with a view to the Secretary of State granting a license for the manufacture of this fine explosive in England. On that occasion the celebrated waggon experiment of the former trials was repeated, and all the others requested by the committee carried out with incontrovertible success. The waggon experiment alluded to had reference to a most important subject, namely, the safety or propriety with which this explosive could be carried on the railways. Its safe condition, when merely set fire to, whether open or enclosed in wooden boxes, has in both series amply and easily been demonstrated; but the question of what might occur in the case of railway collisions was of that vital nature that only a crucial test could satisfy. As there is a long incline of some 700 yds., with a fall of 1 in 8, at the Nantmaur Quarries, and as Mr. France had some old stone trucks of no great value, he very properly obtained, by practical proof, a true verdict on this exceedingly important subject. A truck was scotched on the line at 500 yds., and another, with cartridges of lithofracteur, attached to its wooden buffers, was started free from the top of the incline. The pace it attained at the moment of contact was fearful, and the concussion tremendous. The trucks were smashed up and thrown off the rails; but there was no explosion of the lithofracteur. It was simply squeezed out. Another truck was scotched on the line, and another one started against it in like manner, the running truck this time having the buffers, to which the cartridges were attached, plated with iron. Again a smash, but no explosion. Before this run some lithofracteur was put on the rails, for the wheels of the trucks to pass over, which they did, simply squashing the cartridges, and in one or two instances snapping little fragments, like boys' crackers, the stains on the metals not being bigger than half-crowns, the residue, spread in contiguity around, not being fired in any way whatever. Iron to iron was not tried in the experiments of last year, as Professor Engels, the inventor, stated that the lithofracteur would explode confined between metals under such very violent blows. In the recent official experiments not only were those wood to wood and iron to wood experiments repeated with identical results, but a truck was launched having both buffers plated with iron and charged with three cartridges each, and run at the same terrific speed against a scotched waggon with iron plated buffers also. The truck hit true, and, of course, all six cartridges exploded, sending smokeless jets of gas straight up like pistol discharges, in the instant of impact, some 12 ft. or 14 ft. into the air. No splinters were scattered from this discharge, but the iron plates were bent about and much distorted. Our plate (Plate LXXXIII.) shows the incline at Mr. France's quarries, and the wreck of the trucks in this last experiment, which took place on February 24 last.

During the past month lithofracteur has further shown its capabilities under water. The *Alarm*, a schooner belonging to Brixham, capsized in January, in the river Parratt, about three miles below Bridgewater, and in the attempts to raise her she was drawn, heeled over, right across the stream in such a way as not only to fill with sand but to cause banks obstructive to the navigation to be formed. The Company in which she was insured were not only threatened by shipowners with actions, but the town authorities, under a clause in their Act of Parliament, gave notice that if she was not forthwith got out of the way they should clear the wreck themselves and recover the cost. Under these circumstances Mr. France, who has had sufficient practical experience of the explosive in his own quarries, undertook to blow up the wreck, which he did most completely on the 18th ultimo with two charges of 50lbs. each. The first charge was put in a tarred water-tight box, and pushed in atop of the sand under the deck close to the mizen-mast, and by this the after half of the vessel was destroyed and the sand much cleared out of the forward portion. In the second charge the cartridges were put in a mere broken wood case, all loose, with the water running in between them. This was placed under the side of the vessel heeled over; and it effected complete demolition, a deep hole of upwards of ten feet being made in the river-bed where the obstruction previously existed. In the first explosion a fountain of water was jetted, with débris of the planking and timbers, 80 ft. or 90 ft. into the air; in the second the said débris and water boiled up over a large area around for very many yards—indeed, for half a mile of the river's surface the stream was covered with floating splinters and wreck. The charges were in both cases fired with detonators ignited by lengths of from 20 ft. to 50 ft. of Bickford and Bennett fuze, both being burning fuzes of the same nature.

There has thus in every case been demonstrated the control obtained by chemists and manufacturers over that terrible substance nitro-glycerine, which the Government of this country was, in a moment of panic, weak enough to taboo, but which, now held in the bonds of science, will prove a salvation to life and limb in thousands of quarries and mines all over the earth. No stronger condemnation of Government interference in matters beyond the bounds of politics can possibly be instanced than in this particular case. The ban, however, unfortunately is not yet removed; and Englishmen are still debarred the use of those explosives which, under all ordinary conditions, merely burn, and only explode when purposely detonated under a definite and *à priori* purpose of the user's will.

THE PHYSIOLOGICAL POSITION OF ALCOHOL.

By Dr. Richardson, F.R.S.

AT the present moment, the "Alcohol Question," as it is called, is, in various ways, one of the most anxious subjects of out-door controversy. The leaders of the temperance movement, seeing the tide that was once against them hesitating, if not turning in their favour, are redoubling their efforts with a certain improvement of method which bodes better for them and their works: the politicians are beginning to consider the solution of the problem of the successful management, by Act of Parliament, of the "habitual drunkard:" the statist is re-collecting and revising his tables on the influence of alcohol upon the health, the wealth, and the vitality of communities: the actuary is learning that, with an advance of temperance, his calculations may require amendment: the people resident in quiet and respectable villages, or in sections of great cities and towns, are silently but surely conspiring against that old institution, the public hostel, and that older of institutions still, "mine host" of the hostel: the chemists are busy with their analyses of wines, beers, and spirits, and are charged with subtle arguments on the question whether the animal body, by its living force, can turn alcohol into new and different products of the organic series: and lastly, but by no means leastly, the doctors are making clean breasts on the subject of their dealings with alcohol in the sick room, in a strain which partakes rather of sentiment than of reality, or of that serene judgment and reflection so becoming to the professional mind. In the midst of this singular revolution of thought and of revelation of fact against the use of alcohol, it is, I had almost said, appalling to observe how its use extends. What is protest in theory is met by counter-protest in practice. Is theory or practice false? That is the question.

The answer I shall try herewith to give to that question shall consist of fact and suggestion. I have no part in mere controversy. I have tried by experiment and experience to read the physiological action of alcohol, and the reading thus obtained I propose to put forward in this brief chronicle. Let the reader, if he choose, take up the controversy from the narrative.

I would deal now with one part of the science side of the alcohol question, and which, put in very simple language, would stand as follows: Is alcohol good for the health of man and the lower animals? Does it give them strength, readiness for work, endurance for work, length of days, happiness? To answer the question relating to the lower animals first, we may, I think, come to the safe conclusion that alcohol is not good for animals under the rank of man. Calves fed on gin-balls—barley meal and gin—are very soon prepared for the *abattoir*, but are not exalted into anything very sprightly and lovely in the bovine line. On the contrary, they are rendered dull, slothful, sleepy calves, on whose bodies the advice "Rest and be thankful" is morally branded. Cats and kittens are equally deteriorated by alcohol. I knew some young people who gradually taught a favourite kitten to walk round the dinner-table during dessert and taste wine. It was not long before the taste became a luxurious habit with the animal, but she soon began to fail under it. She slept half her life, lost all desire for play, and in the course of a month or two was dropsical and beyond cure. She contracted the liver disease called cirrhosis, and a very perfect specimen of the disorder she presented after her untimely death. I have observed that birds can be made to acquire a taste for alcohol. Pigeons and fowls, after a little training, will pick up peas saturated with spirit, and subsist on such diet. The animals fatten and sleep, but they lose their vivacity, and certainly lose their muscular power. The same rule holds good with fish. These animals, under the feeble but steady influence of alcohol, become indolent and sleepy and soon die.

On the whole, then, we may conclude safely that god Bacchus meant wine and all its allies specially for man. Perhaps he foresaw that it would be too expensive an article for beasts generally, and so wisely limited its adaptation; or perhaps he did not adapt it wisely for the good of man, since, according to the best accounts of him from those his votaries who believe in him, he was not a personage who went deeply into any other subject than wine. Was he wise, and is alcohol bad for beasts, good for man? We will turn to this question.

It is right, before entering on this question, to say that under the word alcohol I mean specifically the alcohol which ordinarily enters into wines, beers, spirits, and upon which their action as stimulants depends. There are many other substances included now, by chemists, under the term alcohols, such as methylic, butylic, and amylic alcohols; and there is another singular alcohol called mercaptan, or sulphur alcohol, in which the element sulphur replaces the element oxygen of common alcohol. Certain of these other alcohols—those above named particularly—I have made, also, subject of physiological study; and the report of their action would afford scope for an article that could not, I think, fail to be of interest to scientific readers whose tastes lean towards physiological research. But, as it will be simplest to begin with the alcohol that is best known, and for good or for evil is most potent in the world in our time, I confine, on this occasion, myself to it—*ethylic or common alcohol.*

Ethylic alcohol will enter the animal body by any channel through which it can be administered. It may be introduced by the stomach, by the connective tissue beneath the skin, into which it can be readily injected, and by the inhalation of its vapour by the lungs. This last method of administration is, however, the most difficult, the quantity required for the production of an effect being considerable, and the time required very long. The animals most susceptible to the action of alcohol-vapour are pigeons; but even they resist the influence of the vapour so determinately, that the same quantity which would produce a profound effect in half-an-hour, were it injected subcutaneously, exerts no marked influence if administered through the air.

But in whatever mode alcohol may be passed into the living body to produce modification of physiological action, the changes it excites are remarkably uniform, and *cæteris paribus* the amount required to induce the changes is also uniform. Thus, I have found, by many researches, that the proportion of sixty grains of alcohol to the pound weight of the animal body is the quantity capable of producing an extreme effect.

The order of the changes induced is, in like manner, singularly uniform, and extends in a methodical way through all classes of animals that may be subjected to the influence; and as the details of this part of my subject are the facts that concern us most, I shall expend some time in their narration.

The first symptom of moment that attracts attention, after alcohol has commenced to take effect on the animal body, is what may be called vascular excitement; in other words, over-action of the arterial vessels and of the heart, or, speaking still more correctly, over-action of the heart and arterial vessels. The heart beats more quickly, and thereupon the pulse rises. There may be some other symptoms of a subjective kind—symptoms felt by the person or animal under the alcohol—but this one symptom of vascular excitement is the first objective symptom, or that which is presented to the observer. I endeavoured in one research to determine from observations on inferior animals what was the actual degree of vascular excitement induced by alcohol, and my results were full of interest. They have, however, been entirely superseded by the observations made on the human subject by Dr. Parkes and Count Wollowicz.

These observers conducted their enquiries on the young and healthy adult man. They counted the beats of the heart, first at regular intervals, during what were called water periods, that is to say, during periods when the subject under observation drank nothing but water; and next, taking still the same subject, they counted the beats of the heart during successive periods in which alcohol was taken in increasing quantities: thus step by step they measured the precise action of alcohol on the heart, and thereby the precise primary influence induced by alcohol. Their results were as follow:—

"The average number of beats of the heart in 24 hours (as calculated from eight observations made in 14 hours) during the first, or water period, was 106,000; in the alcoholic period it was 127,000, or about 21,000 more; and in the brandy period it was 131,000, or 25,000 more.

The highest of the daily means of the pulse observed during the first or water period was 77·5; but on this day two observations are deficient. The next highest daily mean was 77 beats.

If, instead of the mean of the eight days, or 73·57, we compare the mean of this one day, viz., 77 beats per minute, with the alcoholic days, so as to be sure not to over-estimate the action of the alcohol, we find:—

On the 9th day with one fluid ounce of alcohol the heart beat 430 times more.

On the 10th day, with two fluid ounces, 1,872 times more.

On the 11th day, with four fluid ounces, 12,960 times more.

On the 12th day, with six fluid ounces, 30,672 times more.

On the 13th day, with eight fluid ounces, 23,904 times more.

On the 14th day, with eight fluid ounces, 25,488 times more.

But as there was ephemeral fever on the 12th day it is right to make a deduction, and to estimate the number of beats in that day as midway between the 11th and 13th days, or 18,432. Adopting this, the mean daily excess of beats during the alcoholic days was 14,492, or an increase of rather more than 13 per cent.

The first day of alcohol gave an excess of 4 per cent., and the last of 23 per cent.; and the mean of these two gives almost the same percentage of excess as the mean of the six days.

Admitting that each beat of the heart was as strong during the alcoholic period as in the water period (and it was really more powerful), the heart on the last two days of alcohol was doing one-fifth more work.

Adopting the lowest estimate which has been given of the daily work done by the heart, viz., as equal to 122 tons lifted one foot, the heart, during the alcoholic period, did daily work in excess equal to lifting 158 tons one foot, and in the last two days did extra work to the amount of 24 tons lifted as far.

The period of rest for the heart was shortened, though, perhaps, not to such an extent as would be inferred from the number of beats; for each contraction was sooner over. The heart, on the fifth and sixth days after alcohol was left off, and apparently at the time when the last traces of alcohol were eliminated, showed in the sphygmographic tracings signs of unusual feebleness; and, perhaps, in consequence of this, when the brandy quickened the heart again, the tracings showed a more rapid contraction of the ventricles, but less power, than in the alcoholic period. The brandy acted, in fact, on a heart whose nutrition had not been perfectly restored."

It is difficult, at first glance, to realize the excessive amount of work performed by the heart under this extreme excitement. Little wonder is it that, after the labour imposed upon it by six ounces of alcohol, the heart should flag; still less wonder that the brain and muscles which depend upon the heart for their blood supply should be languid for many hours, and should require the rest of long sleep for renovation. It is hard physical work, in short, to fight against alcohol; harder than rowing, walking, wrestling, carrying heavy weights, coal-heaving, or the tread-wheel itself.

While the heart is thus labouring under the action of alcoholic stimulation, a change is observable in the extreme circulation—that circulation of blood which by varying shades of colour in exposed parts of the body, such as the cheek, is visible to the eye. The peripheral circulation is quickened, the vessels distended. We see this usually in persons under the influence of wine in the early stage, and we speak of it as the flush produced by wine. The authors I have already quoted report upon it in definite terms. "The peripheral circulation (during alcoholic excitement) was accelerated, and the vessels were enlarged, and the effect was so marked as to show that this is an important influence for good or for evil when alcohol is used."

By common observation the flush seen on the cheek during the first stage of alcoholic excitation is supposed to extend merely to the parts actually seen. It cannot, however, be too forcibly impressed on the mind of the reader that the condition is universal in the body. If the lungs could be seen they, too, would be found with their vessels injected; if the brain and spinal cord could be laid open to view they would be discovered in the same condition; if the stomach, the liver, the spleen, the kidneys, or any other vascular organs or parts could be laid open to the eye, the vascular enlargement would be equally manifest.

In course of time, in persons accustomed to alcohol, the vascular changes, temporary only in the noviciate, become confirmed and permanent. The bloom on the nose which characterizes the genial toper is the established sign of alcoholic action on vascular structure.

Recently some new physiological enquiries have served to explain the reason why, under alcohol, the heart at first beats so quickly and why the pulses rise. At one time it was imagined that the alcohol acted immediately upon the heart, stimulating it to increased action, and from this idea—false idea, I should say—of the primary action of alcohol, many erroneous conclusions have been drawn. We have now learned that there exist many chemical bodies which act directly by producing a paralysis of the organic nervous supply of the vessels which constitute the minute vascular circuit. These minute vessels when paralysed offer inefficient resistance to the stroke of the heart, and the heart thus liberated, like the mainspring of a clock from which the resistance has been removed, quickens in action, dilating the minute and feebly-acting vessels, and giving evidence really not of increased but of wasted power.

The phenomena noticed above constitute the first stage of alcoholic action on the body; we may call it the stage of excitement; it corresponds with a similar stage or degree caused by chloroform.

If the action of alcohol be carried further, a new set of changes are induced in another part of the nervous system—the spinal system. Whether this change be due simply to the modification of the circulation in the spinal cord, or to the direct action of the alcohol upon the nervous matter, is not yet known, but the fact of change of function is well marked, and it consists of deficient power of co-ordination of muscular movement. The nervous control of certain of the muscles is lost, and the nervous stimulus is more or less enfeebled. The muscles of the lower lip in the human subject usually fail first of all, then the muscles of the lower limbs, and it is worthy of remark that the flexor muscles give way earlier than the extensors. The muscles themselves by this time are also failing in power; they respond more feebly than is natural to the galvanic stimulus; they, too, are coming under the depressing influence of the paralysing agent, their structure temporarily changed, and their contractile power everywhere reduced. This modification of the animal functions under alcohol marks the second degree of its action. In this degree, in young subjects, there is usually vomiting, and in birds this symptom is invariable. Under chloroform there is produced a degree or stage of action holding the same place in the order of phenomena.

The influence of the alcohol continued still longer, the upper portions of the cerebral mass, or larger brain, become implicated. These are the centres of thought and volition, and as they become unbalanced and thrown into chaos, the mind loses equilibrium, and the rational part of the nature of the man gives way before the emotional, passional, or mere organic part. The reason now is off duty, or is fooling with duty, and all the mere animal instincts and sentiments are laid atrociously bare. The coward shows up more craven, the braggart more braggart, the bold more bold, the cruel more cruel, the ignorant more ignorant, the untruthful more untruthful, the carnal more carnal. "In vino veritas" expresses faithfully, indeed even to physiological accuracy, a true condition. The spirits of the emotions are all in revel, and are prepared to rattle over each other in wild disorder; foolish sentimentality, extending to tears, grotesque and meaningless laughter, absurd promises and asseverations, inane threats or childish predictions impel the tongue, until at last there is failure of the senses, distortion of the objective realities of life, obscurity, sleep, insensibility, and utter muscular prostration. This constitutes the third stage of alcoholic intoxication. It is the stage of insensibility under chloroform when the surgeon performs his painless task.

While these changes in the action of the nervous system are in progress there is a peculiar modification proceeding in respect to the temperature of the body. For a little time the external or surface temperature is increased, especially in those parts that are unduly charged and flushed with blood. But it is to be observed that in respect to the mass of the body the tendency is to a fall of temperature. In the progress towards complete intoxication under alcohol, however, there are, as we have already seen, three degrees or stages. The first is a stage of simple exhilaration, the second of excitement, the third of rambling insensibility, and the fourth of entire unconsciousness, with muscular prostration. The duration of these stages can be modified in the most remarkable manner by the mode of administration; but whether they are developed or recovered from in an hour or a day, they are always present except in cases where the quantity of alcohol administered is in such excess that life instantly is endangered or destroyed. In the first or exhilarative stage the temperature undergoes a slight increase; in birds a degree Fahrenheit, in mammals half a degree. In the second degree, during which there is vomiting in birds, or attempts at vomiting, the temperature comes back to its natural standard, but soon begins to fall; and during the third degree the decline continues. The third degree fully established, the temperature falls to its first minimum, and in birds comes down from five and a half to six degrees; in rabbits from two and a half to three degrees. In this condition the animal temperature often remains until there are signs of recovery, viz., conscious or semi-conscious movements, upon which there may be a second fall of temperature of two or even three degrees in birds. In this course of recovery I have seen, for instance, the temperature of a pigeon which had a natural standard of 110° Fahr. reduced to 102°. Usually with this depression of force there is a desire for sleep, and with perfect rest in a warm air there is a return of animal heat; but the return is very slow, the space of time required to bring back the natural heat being from three to four times longer than that which was required to reduce it to the minimum.

In these fluctuations of temperature the ordinary influences of the external air play an important part as regards duration of the fluctuation, and to some extent as regards extremes of fluctuation.

These facts respecting fall of temperature of the animal body under alcohol were derived from observations originally taken from the inferior animals; they have been confirmed since by other observers from the human subject. Dr. De Marmon, of King's Bridge, New York, has specially proved this fact in some instances of poisoning by whisky in young children. In one of these examples the temperature of the body fell from the natural standard of 98° Fahr. to 94°, in another to 93½°.

Through all the three stages noticed in the above, the decline of animal heat is a steadily progressing phenomenon. It is true that in the first stage the heat of the flushed parts of the body is for a brief time raised, but this is due to greater distribution of blood and increased radiation, not to an actual increment of heat within the body. The mass of the body is cooling, in fact, while the surfaces are more briskly radiating, and soon, as the supply of heat-motion fails, there is fall of surface temperature also; a fall becoming more decided from hour to hour up to the occurrence of the fourth and final stage, of which I have now to treat.

The fourth degree of alcoholic intoxication is one of collapse of the volitional nervous centres, of the muscular organs under the control of those centres, and of some of the organic or mere animal centres. It is true that while the body lies prostrate under alcohol there are observed certain curious movements of the limbs, but these are not stimulated from the centres of volition, nor are they reflected motions derived from any external stimulus; they are strange automatic movements, as if still in the spinal cord there were some life, and they continue irregularly nearly to the end of the chapter, even when the end is death.

Through the whole of this last stage two centres remain longest true to their duty, the centre that calls into play the respiratory action, and the centre that stimulates the heart. There is then an interval during which there are no movements whatever, save these of the diaphragm and the heart, and when these fail the primary failure is in the breathing muscle: to the last the heart continues in action.

The leading peculiarity of the action of alcohol is the slowness with which the two centres that supply the heart and the great respiratory muscle are affected. In this lies the comparative safety of alcohol: acting evenly and slowly, the different systems of organs die after each other, or together, gently, with the exception of those two on which the continuance of mere animal life depends. But for this provision every deeply intoxicated animal would inevitably die.

It happens usually, nevertheless, that under favourable circumstances the intoxicated live: the temperature of the body sinks two or three degrees lower, but the alcohol diffusing through all the tissues, and escaping by diffusion and elimination, the living centres are slowly relieved, and so there is slow return of power. If death actually occurs, the cause of it is condensation of fluid on the bronchial surfaces and arrest of respiration from this purely mechanical cause. The animal is literally drowned in his own secretion.

Such are the stages or degrees of alcoholic narcotism, from the first to the last. And with the description of them, and the order in which they come, my present task is well nigh complete. There arise, however, a few thoughts and suggestions deserving of brief notice.

1. In the first place we gather from the physiological reading of the action of alcohol that the agent is a narcotic. I have compared it throughout to chloroform, and the comparison is good in all respects save one, viz. that alcohol is less fatal than chloroform as an immediate destroyer. It kills certainly in its own way to the extent, according to Dr. De Marmon, of fifty thousand persons a year in England, and ten thousand a year in Russia, but its method of killing is slow, indirect, and by painful disease.

2. The well proven fact that alcohol, when it is taken into the body, reduces the animal temperature, is full of the most important suggestions. The fact shows that alcohol does not in any sense act as a supplier of vital heat as is so commonly supposed, and that it does not prevent the loss of heat as those imagine 'who take just a drop to keep out the cold.' It shows, on the contrary, that cold and alcohol in their effects on the body run closely together, an opinion most fully confirmed by the experience of those who live or travel in cold regions of the

earth. The experiences of the Arctic voyagers, of the leaders of the great Napoleonic campaign in Russia, of the good monks of St. Bernard, all testify that death from cold is accelerated by its ally alcohol. Experiments with alcohol in extreme cold tell the like story, while the chilliness of body which succeeds upon even a moderate excess of alcoholic indulgence leads direct to the same indication of truth.

3. The conclusive evidence now in our possession that alcohol taken into the animal body sets free the heart, so as to cause the excess of motion of which the record has been given above, is proof that the heart, under the frequent influence of alcohol, must undergo deleterious change of structure. It may, indeed, be admitted in proper fairness, that when the heart is passing through this rapid movement it is working under less pressure than when its movements are slow and natural; and this allowance must needs be made or the inference would be that the organ ought to stop at once in function by the excess of strain put upon it. At the same time the excess of motion is unquestionably injurious to the heart and to the body at large: it subjects the body in all its parts to irregularity of supply of blood; it subjects the heart to the same injurious influence; it weakens and, as a necessary sequence, degrades both the body and the heart.

4. Speaking honestly, I cannot, by any argument yet presented to me, admit the alcohols by any sign that should distinguish them from other chemical substances of the exciting and depressing narcotic class. When it is physiologically understood that what is called stimulation or excitement is, in absolute fact, a relaxation, I had nearly said a paralysis, of one of the most important mechanisms in the animal body —the minute, resisting, compensating circulation—we grasp quickly the error in respect to the action of stimulants in which we have been educated, and obtain a clear solution of the well known experience that all excitement, all passion, leaves, after its departure, lowness of heart, depression of mind, sadness of spirit. We learn, then, in respect to alcohol, that the temporary excitement it produces is at the expense of the animal force, and that the ideas of its being necessary to resort to it, that it may lift up the forces of the animal body into true and firm and even activity, or that it may add something useful to the living tissues, are errors as solemn as they are widely disseminated. In the scientific education of the people no fact is more deserving of special comment than this fact, that excitement is wasted force, the running down of the animal mechanism before it has served out its time of motion.

5. It will be said that alcohol cheers the weary, and that to

take a little wine for the stomach's sake is one of those lessons that comes from the deep recesses of human nature. I am not so obstinate as to deny this argument. There are times in the life of man when the heart is oppressed, when the resistance to its motion is excessive, and when blood flows languidly to the centres of life, nervous and muscular. In these moments alcohol cheers. It lets loose the heart from its oppression, it lets flow a brisker current of blood into the failing organs; it aids nutritive changes, and altogether is of temporary service to man. So far alcohol is good, and if its use could be limited to this one action, this one purpose, it would be amongst the most excellent of the gifts of nature to mankind. Unhappily, the border line between this use and the abuse of it, the temptation to extend beyond the use, the habit to apply the use when it is not wanted as readily as when it is wanted, overbalance, in the multitude of men, the temporary value that attaches truly to alcohol as a physiological agent. Hence alcohol becomes a dangerous instrument even in the hands of the strong and wise, a murderous instrument in the hands of the foolish and weak. Used too frequently, used too excessively, the agent that in moderation cheers the failing body, relaxes its parts too extremely; spoils vital organs; makes the course of the circulation slow, imperfect, irregular; suggests the call for more stimulation; tempts to renewal of the evil, and ruins the mechanism of the healthy animal before its hour for ruin, by natural decay, should be at all near.

6. It is assumed by most persons that alcohol gives strength, and we hear feeble persons saying daily that they are being kept up by stimulants. This means actually that they are being kept down, but the sensation they derive from the immediate action of the stimulant deceives them and leads them to attribute lasting good to what, in the large majority of cases, is persistent evil. The evidence is all-perfect that alcohol gives no potential power to brain or muscle. During the first stage of its action it may enable a wearied or feeble organism to do brisk work for a short time; it may make the mind briefly brilliant; it may excite muscle to quick action, but it does nothing at its own cost, fills up nothing it has destroyed as it leads to destruction. A fire makes a brilliant sight, but it leaves a desolation; and thus with alcohol.

On the muscular force the very slightest excess of alcoholic influence is injurious. I find by measuring the power of muscle for contraction in the natural state and under alcohol, that so soon as there is a distinct indication of muscular disturbance, there is also indication of muscular failure, and if I wished, by scientific experiment, to spoil for work the most perfect specimen of a working animal, say a horse, without inflicting mechanical injury, I could choose no better agent for the purpose of the experiment than alcohol. But alas! the readiness with which strong well-built men slip into general paralysis under the continued influence of this false support, attests how unnecessary it were to put a lower animal to the proof of an experiment. The experiment is a custom, and man is the subject.

7. It may be urged that men take alcohol, nevertheless, take it freely and yet live; that the adult Swede drinks his average cup of twenty-five gallons of alcohol per year and yet remains on the face of the earth. I admit force even in this argument, for I know that under the persistent use of alcohol there is a secondary provision for the continuance of life. In the confirmed alcoholic the alcohol is in a certain sense so disposed of that it fits, as it were, the body for a long season, nay, becomes part of it; and yet it is silently doing its fatal work: all the organs of the body are slowly being brought into a state of adaptation to receive it and to dispose of it; but in that very preparation they are themselves undergoing physical changes tending to the destruction of their function and to perversion of their structure. Thus, the origin of alcoholic phthisis, of cirrhosis of the liver, of degeneration of the kidney, of disease of the membranes of the brain, of disease of the substance of the brain and spinal cord, of degeneration of the heart, and of all those varied modifications of organic parts which the dissector of the human subject so soon learns to observe—almost without concern, and certainly without anything more than commonplace curiosity—as the devastations incident to alcoholic indulgence. Thus, the origin of such a report as that of Mr. Everrett on the census of America in 1860, related by Dr. De Marmon in the 'New York Medical Journal' for December 1870.

"For the last ten years the use of spirits has—1. Imposed on the nation a direct expense of 600,000,000 dollars. 2. Has caused an indirect expense of 600,000,000 dollars. 3. Has destroyed 300,000 lives. 4. Has sent 100,000 children to the poorhouses. 5. Has committed at least 150,000 people into prisons and workhouses. 6. Has made at least 1,000 insane. 7. Has determined at least 2,000 suicides. 8. Has caused the loss, by fire or violence, of at least 10,000,000 dollars' worth of property. 9. Has made 200,000 widows and 1,000,000 orphans."

When I sat down to write this essay I noted many points of peculiar scientific interest as deserving my attention, and amongst these one specially important, the question:—How alcohol, after it has been taken into the organism, is disposed of, whether by conversion into a new product, by which it ceases to be alcohol, or whether, by leaving the body, as it entered it, an unbroken chemical compound? This question, however important scientifically, is of secondary moment when it is compared with the study of the direct, regular, and almost unvarying action of alcohol upon the body during life, and I have therefore left it in order to place before the mind of my readers the actual influence of alcohol on the body of the animal that takes it, whatever may become of it after it has entered the body. If in this effort I have shown how far alcohol is really good, and how such value as there is in it is limited at most to the first stage of its action; if I have shown how, being a so-called stimulant, it is not a giver of power; if I have indicated by what slight error in the use of it it is a destroyer of power of the most potent character; and if, from experimental research on the physiological action of the agent, I have been able to communicate to the world some facts not before rightly understood, my intention is carried out. I hope, moreover, the intention is carried out with benefit to the greatest of all social efforts, the effort to reduce alcohol to its legitimate application as an instrument for some good and most evil in the possession of man.

THE NATURE OF SPONGES.

(Researches of Clark and Carter.)

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[PLATE LXXXIV.]

THE true nature of sponges has long been a puzzle to naturalists, and various opinions have been held concerning them. No one now supposes them to be members of the vegetable kingdom; but, though their animality is decided, discussions are still going on as to the group with which they are most nearly connected; and much light might be thrown upon the subject if more microscopists would watch the early stages and growth of either marine or freshwater forms. Spongillæ, which belong to the latter, are known to most collectors, but few do more than make a rough examination of their structure, or mount gemmules as cabinet objects.

Recent researches of Professor H. James Clark will help to stimulate enquiry into these interesting organisms; but his last paper, which appeared in "Silliman's Journal," and which is copied into the "Monthly Microscopical Journal" for March, will scarcely be intelligible without some previous knowledge, and reference to other books.

What is now proposed is to supply a sufficient popular explanation of the most interesting points, freed as much as possible from technical difficulties, and from details which beginners will best learn from actual study of the objects.

Professor H. James Clark controverts the opinion of Haeckel and others, that sponges are essentially compound polyps, and he alleges reasons for associating them with flagellate, or whip-bearing, infusoria.

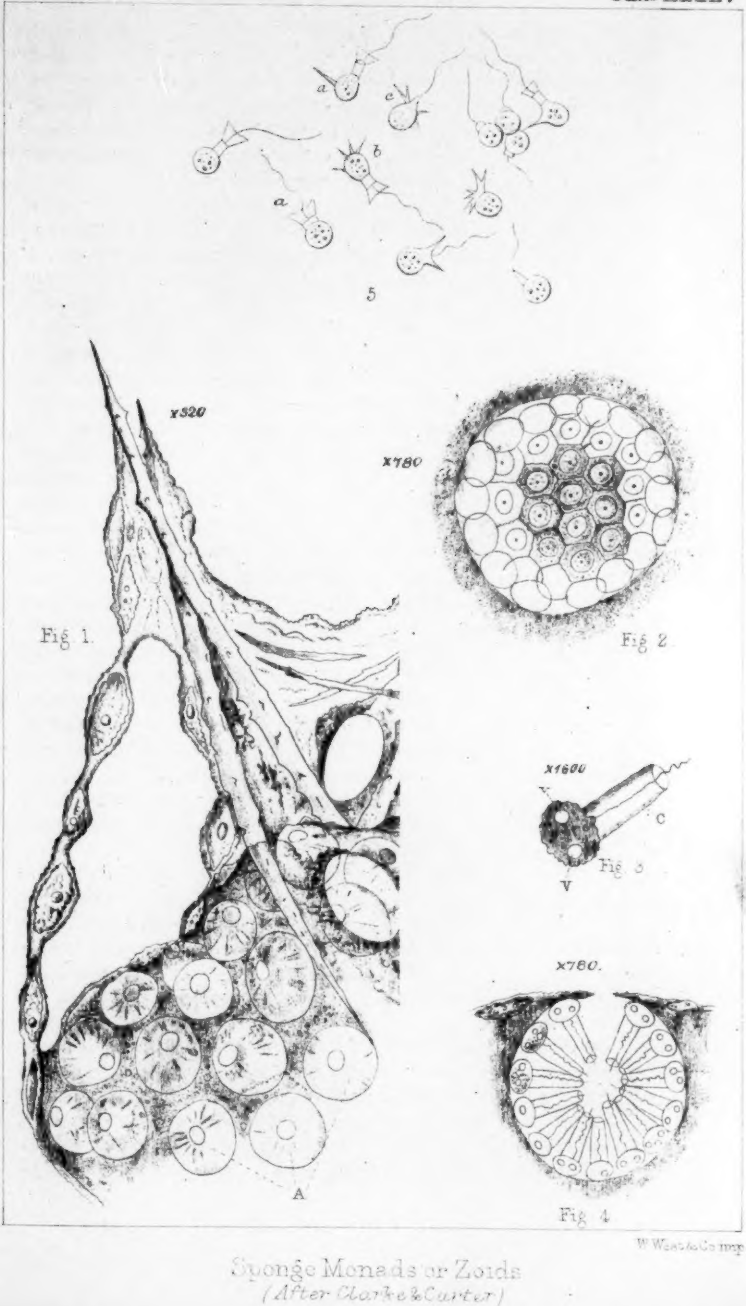
In entering upon these subjects, it is necessary to form some idea of the more obvious characteristics of sponges, polyps, and whip-bearing infusorians. In the first place, it is well for the student to contemplate widely different sorts of sponge. Take, for example, a common toilet sponge—here is the skeleton of a sponge belonging to the keratose group. Its structure is what is commonly meant by spongy, and the material having been likened to horny substances, has received the name of *keratose* from the Greek *keras*, a horn. The term is so thoroughly accepted, that it would be impracticable to change it, although the material somewhat differs chemically and physically from horn. It has, however, the characteristics of an animal substance, and contains sixteen per cent. of nitrogen.

If, after noticing the characters of common sponge—its larger and smaller pores, its tendency to assume regular forms, cup-shaped, flattened, &c.—and the evidence presented by its base of having been fixed or rooted to a rock, the eye is turned to that exquisite and now not uncommon object, Venus' Flowerbasket (*Euplectella speciosa*), quite a different material meets the view. The elegant horn-of-plenty-shaped object is seen to be composed of glassy fibres crossing, and apparently woven together like skilled basket-work, from whence its scientific name, *Euplectella*, well plaited or woven, was obviously suggested. Like the common sponge, this shows that it was rooted by a basal end, and it exhibits a similar variety of larger and smaller pores. The common sponge may be taken as the representative of the keratose group, the Euplectella of the siliceous or flinty group, for the glassy threads of which it is composed are deposits of silex from living threads of animal matter.

A third group of sponges have calcareous or carbonate of lime skeletons, and both these and the siliceous ones are remarkable for the presence of spiculæ of various shapes. In Bowerbank's "British Spongidæ," the most accessible work on the subject, the three orders are ranged thus: Calcarea, Silicea, and Keratosa, and a subdivision of the latter is described as having "spiculated Keratose fibre." It is, however, from the two former groups that the curious and beautiful spiculæ so commonly found in microscopical cabinets are derived; the ordinary shapes of which are well known to many who have never paid attention to the organisms from which they are derived.

A good conception of the more obvious sponge characters may be derived from a passage in Dr. Bowerbank's book. He says: "Whatever may be their form, or however they may differ from each other in appearance, there are certain points in their organisation in which they all agree. In the first place, however variable in its form and mode of structure, there is always a skeleton present, in which the rest of the organic parts are based and maintained. Amidst this skeleton, and intimately incorporated with it, are the interstitial canals, usually of two series: the first appropriated to the incurrent streams of the surrounding water, and the second to the excurrent streams, which they conduct from the interior of the sponge to the oscula at its surface, through which they are discharged."

Plate LXXXIV



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Sponge Monads or Zoids
(After Clarke & Carter)

Every portion of the sponge, in a live state, is covered with fleshy material *sarcode*, much like that of infusorial animals. Detached portions of this sarcode have been described by Carter as moving somewhat like amæbæ, and Bowerbank observes: "I have frequently, at different seasons of the year, taken portions of the sarcode from living and healthy specimens of spongillæ in which I could not, by the closest attention, detect these motions, which are so readily seen at other periods of their existence: and even at the same period of the year, the sarcode of some specimens exhibits these motions, while in others they could not be detected." This passage suggests interesting experiments, which our readers may easily make when spongillæ occur amongst their captures on collecting days. According to Bowerbank, a power of 500 or 600 linear enables the microscope to show that in a living sponge the whole of the sarcode abounds in nutrient particles, "some simply adhering to the surface, while others are imbedded at different degrees of depth." It should be added, that this sarcode substance is sensitive to external impulses, and contracts under conditions that would create alarm in creatures of higher organisation capable of a mental impression. It has muscular power, nervous power, and digestive power, without any distinction of organs being visible.

The roots of sponges are simply means of adhesion, or anchoring, whether they present the simple form of a sort of cement attaching them to rocks, or of glassy fibres, like the Glass Rope Sponge, penetrating into mud. In no case have they anything to do with nutrition.

Sponges are not exactly like plants, dependent entirely upon what air or water may bring to them for nutriment. They possess a ciliary apparatus, by which they can make artificial currents subservient to the purposes of food collecting, aeration, &c. Sponge gemmules also, by means of cilia, can travel about until they find an appropriate resting-place, and commence a sedentary life. The propagation of sponges takes place by eggs (ova), by buds (gemmules), and by divisions, such as is common with the infusoria.

The preceding statements may excite curiosity to know more of these organisms, and prompt many readers to consult various writings on the subject. For the present object of introducing Professor H. James Clark's researches, we have perhaps said enough concerning the general characters of sponges, and shall now pass to a brief consideration of polyps, to which Haeckel and others assimilate them, and which notion Mr. Clark disputes, as many English observers have done and still do.

All microscopists know the common freshwater polyps. They belong to the sub-kingdom Cœlenterata, and to its lowest type.

The bodies grouped under this term by modern naturalists are thus summarily described by Professor Reay Greene: *—"All are furnished with an alimentary canal, freely communicating with the general or somatic cavity. The substance of the body consists essentially of two separate layers, an outer, 'ectoderm,' and an inner, or 'endoderm.' These two membranes, but especially the former, are in general provided with cilia." Another common characteristic is the presence of thread-cells, which are stinging organs. These, in a simple form, are easily seen when the tentacles of a common polyp (hydra) are squeezed in a compressorium, or live-box, and viewed with one-fourth or higher power.

The more complicated and elegant forms found in sea anemones, which belong to the Cœlenterata, are described and figured by Mr. Gosse in his "Devonshire Coast." The Cœlenterata have no proper blood-vessels, but the cilia cause currents of digested matter to flow to various parts.

There are two leading groups of Cœlenterata, Hydroza (polyps simple, or in colonies) and Actinozoa, of which the anemones may be taken as types. In the hydrozoa, the digestive cavity is not distinct from the general body cavity, and "the reproductive organs are external." In the Actinozoa the wall of the digestive sac is separated from that of the somatic cavity by an intervening space, subdivided into chambers by a series of vertical partitions, on the faces of which the reproductive organs are situated." †

Colonising polyps are well represented by the Sertularians, so common near sea-coasts, found adhering to large sea-weeds, shells, bits of rock, &c., thrown up by the tides. In these formations we have elegantly branching trees of a horny-looking substance, bearing numerous cells, in each of which a polyp lives. Each polyp catches its own prey, digests its own dinner, and leads its own life; but all the polyps have a share in the general colonial life, and contribute to its growth and stability. Mr. Hinck's admirable work, "British Hydroid Zoophytes," should be in the hands of all who visit the sea-side, or who wish to know the "fairy tales of science" that are to be told concerning these exquisite and astonishing creatures. For the present it is enough to point to the general character of their organisation, and to the fact of their living in a colonial associated life.

Amongst the compound polyps are the Alcyonaria, a common specimen of which is often seen on our coasts as a gristly-looking, whitish or yellow-brownish mass, with finger-like projections, and popularly called "Dead Men's Toes" (*Alcyonium digitatum*). If alive, and placed in salt water, this repulsive-looking object assumes an aspect of fascinating beauty; star-shaped polyps protruding from numerous pores, and spreading

* Manual of Cœlenterates. † Greene, *op. cit.*

their tentacles like the petals of a glassy flower. Of the Alcyonarian group, Professor Greene observes: "In many, though not all Alcyonaria, the somatic cavities of the separate polypes which make up the compound mass are prolonged into canals, freely communicating with one another, inosculating, and forming a sort of aquiferous system, within which the nutritive products circulate."

We may now come to the first paragraph of Professor H. James Clark's paper on the American Spongilla, with a hope that some of the difficulties which prevent its being popularly intelligible have been removed. He says: "The argument of Haeckel and others, that the sponges are essentially compound polypi, is virtually based upon the assumption that the minor (afferent) and major (efferent) ostioles of the former correspond to the mouths of the latter; and that the profusely branching afferent and efferent canals of the sponges are strictly conformable with similar canals in the polypidom of the Halcyonarians; and, by implication, the cilia-bearing cells of the interior lining wall of the zoophyte find their homologies in the ciliated cell-like bodies of the interior chambers of the porifera. If now it should turn out that these last are not altogether mere cell components of a tissue, but are each severally an independent body, although closely connected with others in a common bond, the attempted parallelism between the two groups must utterly fail of confirmation. The tendency of Carter's investigations, and of our own too, is to show that this is no vain supposition."

Professor Clark proceeds to give his reasons for believing that the cilia-bearing objects of the American Spongilla, on which he particularly writes, are a sort of individual, and not, as Haeckel's theory would require, mere portions of a ciliated membrane. In doing this he uses terms which are unintelligible without reference to his paper entitled "Polarity and Polycephalism," in "Silliman's Journal" for January 1870, and which needs popularising for the general reader.

Professor Clark's endeavour in this essay was to contribute to the decision that meets the zoologist under various forms, What is an individual? and he endeavoured to establish a new sort of quasi-individuality. In man, the metaphysical question of individuality, or personal identity, becomes so prominent from the dominant character of his cerebral system and the powers connected with it, that it requires considerable effort to contemplate it from a purely zoological point of view. There is, however, no difficulty with any of the higher animals in determining that each one is an individual in the sense of a separate and complete representative of the species, as soon as it is severed from the parental life and has an independent existence; but in many of the lower forms puzzling incidents occur, and a complete animal is as it were divided into separate creatures, or a number of animals are so intimately connected that the associated life seems to swallow up the individual life. Among the hydroid polyps, for example, two kinds of budding are conspicuous; one adding new members to the colony, much as first leaves and branches grow on a tree; and the other acting according to what is technically called "discontinuous gemmation," that is, producing buds that drop off and become distinct organisms, differing from the immediate parent so as to appear creatures of another family. Besides this, true eggs are formed by the action of male and female elements. Zoologically all the products of a single egg are considered as one individual notwithstanding the number of forms that may be assumed, and the intermediate steps that may take place before another true egg is produced, and a fresh zoological individual started upon a new comer.

Professor Clark begins his essay by a protest against that theory of individuality which assumes that the separate Medusoid or jelly-fish forms taken by the reproductive organs of certain hydroid polyps are *individuals* in a higher degree than the polyps are; and he remarks that "in the discussions of late years upon the individuality of the lower, compound, colonial denizens of the water, the main points at issue have always been to determine whether a certain form was, on one hand, an *individual*, either in the highest sense, or one of several independent individuals which constitute a colony; or, on the other hand, an *organ* which formed only part of an individual." In the highest individuals each separate body contains all the parts and organs necessary for the due performance of a complete cycle of life functions, while in lower forms certain organs occupy distinct bodies, and would be taken for complete individuals if their relation to other forms belonging to the same individuals was not known. Professor Clark refers to an observation of Lereboullet to show that even in the vertebrates a tendency towards dualism exists, as he found an instance in a fish's egg of two heads and two tails proceeding from one centre of development. In many of the lower creatures the tendency is towards multiplicity—an extension of duality. Physiologists use the term bilaterality to describe the symmetrical arrangements of parts in a man for example—the growths on each side of a median line corresponding with each other. In the development from an ovum directive forces are exerted, not only to produce this bilateral arrangement, but also to tend to what he terms "antero-posteriority and dorso-ventrality," terms easily understood. These actions sufficiently resemble what the physicist calls *polarity* to justify the use of that word in designating them; but it must be borne in mind that scarcely anything is known concerning the way in which physical and chemical forces are directed and co-ordinated in living organisms, and that when we speak of electric or magnetic polarity we do not really know that ultimate particles of matter have opposite (polar) qualities developed at opposite sides or ends.

A complete organism may be regarded as composed of two parts—the vegetative and the reproductive. These are found united in all the higher animals—divided in many of the lower. "Neither *alone* can represent," says Professor Clark, "the *individual* unit;" so he employs the term *cephalism* to indicate "the potentiality of these subdivisions to live apart." One he calls a *vegetative*, and the other a *sexual cephaloid*." Strong objections might be taken to this use of the term "cephaloid," as there is no actual *kephale* or head in the case, and etymology furnishes a hindrance rather than a help to understand the word. The present purpose, however, is not criticism, but exposition; and the reader will now be prepared to understand what is meant when Professor Clark terms certain forms he meets with in spongillæ *cephalids*, and why he calls sponges *polycephalic* animals. The American spongilla, which he names *arachnoidea*, differs from the English form. It resembles an irregular spider-web. "It lives in fresh-water streams and ponds, usually at the bottom of the stems of water plants, or wherever there is considerable shade; apparently avoiding the light, as we seldom if ever found it in open water. In size it varies from a few inches to half a line in diameter; of no definite shape; and has a uniform fuscous or yellowish colour, and is wrapped about by a filmy transparent colourless envelope (investing membrane of Carter). The brown colour is inherent to the interior mass, in which groups of monads are imbedded. The investing membrane is also slightly tinged with amber colour by the large and small spicules which are embedded in it." It is only in minute specimens, free from extraneous matter, that the structure can be well observed.

He describes the whole sponge as furnished with a double envelope, the "outer and the inner parts of which are directly continuous into each other at many points." "The outer division lies at a considerable distance from the monadigerous mass, and, as it were, suspended on the points of the larger or projecting spicules, just as a tent canvas is supported on the ends of poles. The inner division closely embraces the monadigerous mass like an epidermis, and even plunges beneath the hollow groups of monads, forming to them a basis of support. . . . . In brief, we might say the sponge is covered with a miniature colonnade, whose ceiling is the outer division of the envelope, the pillars are the bundles of spicules, and the floor is tapestried by the inner division, which, about the pillars, hangs from the ceiling in lofty folds. The continuity of the outer division of the envelope is broken by numerous round or oval openings of various sizes, sometimes very large, which allow a free ingress of the water to the space just beneath. These are the *afferent* ostioles, through and into which a constant current of floating particles may be seen moving with considerable vivacity. Here and there, scattered at wide distances, finger-like hollow processes from the outer division arise singly and at various angles. Each is terminated by a large aperture, the *efferent ostiole*, from which a current of water and floating matter emerges with more or less spasmodic irregularity. The smaller individuals, from half a line to half an inch in diameter, possess only one such osticle, and those an inch in diameter seldom have more than two or three like conduits; but they are very large, sometimes a quarter of an inch in length, and of the proportions and taper of the human forefinger." Focussing lower shows the inner division pierced with more numerous openings, "smaller, methodically arranged, and each one corresponding to, and overlying a hollow group of monads." These monads, or "monad-cephaloids," are the chief objects to be considered. They are described as found in "deep spherical hollows," or "monad chambers," mere cavities without lining walls. The monads are stated to be "not cells but heads of a polycephalic individual," corresponding functionally to the tentaculated heads of polyps. Each monad chamber has a single small circular aperture, which perforates the inner investing membrane and allows egress into the circulatory apartment. The aperture varies in size at times, and may be completely closed." The monads are arranged "radiatingly from and about the centre," and the interior of the cavity appears filled by a collection of vibrating bodies, an effect arising from their ciliation. The converging bodies are, we are told, soon recognised to be craspedote * flagellate monads, so closely packed together, side by side, as to form a continuous stratum."

These monads Professor Clark finds very similar to those he found in *Leucosilenia*, and like those described by Carter in *Grantia compressa*. Each one has a single flagellum or whip filament. "The body proper of a cephaloid is a little shorter than it is broad; on the whole spheroidal in shape. Its posterior end is broadly rounded, and so is its anterior extremity. In front rises a cylindrical membranous collar, which tapers slightly and projects forward to a distance equal to considerably more than twice the length of the body." The flagellum rises from the anterior end of the body, and looks like a *black* thread.

There are in the body of the monad two or three clear spots

* From the Greek *kraspedon*, an edge or border.

which are contractile vesicles, which make the systole and diastole movements very slowly. The spiculæ are "very slender, slightly curved, small-shaped bodies." From the position of the cilia of these monads Professor Clark considers that they only produce local currents, and that the general circulation is maintained by the contraction and expansion of the body mass in general. By consulting the figures in the plate and the references thereto at the end of this article, the reader will understand the preceding descriptions, and know what to look for if such an organism falls in his way.

In "Annals Nat. Hist. 1859," vol. iii. p. 14, Mr. Carter describes monociliated cells of spongillæ, and he appends figures of those belonging to *S. plumosa* and *S. alba*, the former having two ear-like points projecting forwards, and showing particles of indigo it had taken in.

In another paper in the same volume Mr. Carter describes "spherical bodies" in spongillæ, and asks, "Are they each animals of the spongilla, and analogous to the polypes of a polypidom; or is this typical portion to be considered as a single animal, and the spherical bodies as the digesting organs situated in the cavities of the gelatinous mass, or a stomach with the branch canals as a vent?" Further remarks appear to show an inclination to the former view.

In vol. vi. p. 329, 1870, will be found another of Mr. H. J. Carter's papers "On the Ultimate Structure of Marine Sponges," in which the following passage occurs: "Ampullaceous sac is the term I applied to certain groups of unciliated and monociliated sponge cells, or monad-like bodies, which are tessellated together in a globular form, and scattered plentifully here and there throughout the sponge, so as to make up the greater part of its bulk. The globular form presents a circular opening, or transparent area, through which the cilia may be observed to play internally; and when the young spongilla is grown from the seed-like body, and a solution of carmine is put into the water around it, the new globular bodies alone become coloured. . . . . Hence the little globular bodies are clearly the animal expression of the sponge in particular, as they are respectively the only mouths and stomachs of the sponge—in short, the nutritive apparatus, all the rest being subsidiary." He proceeds to say that when spongillæ fed with carmine are torn to pieces, the "monad-like bodies," which he calls "sponge cells," of which the ampullaceous sac is composed, are found to have taken in the carmine, while the absence of the cilium in some and not in others may be explained, as just stated, by its being retractile.

Carter likewise states that he detected the "ampullaceous sac" in marine sponges, both siliceous and calcareous, and in each of them it is the "eating organ.'

Mr. Carter refers in this paper to Professor Clark's view expressed in 1868, that "the monociliated sponge cell is a distinct flagellated infusorium," not related to the rhizopoda; and that a sponge is a colony of such infusorians; and he observes that while he cannot altogether endorse it, he did not then wish to dispute it.

In a later paper ("Annals," July 1871) he refers to this passage, and adds, "it is with great pleasure that I *can* now endorse them—that is, that I am now able to confirm all that Professor James Clark has stated of the flagellated sponge-cell in the valuable memoir to which I have referred."

Examining specimens of *Grantia compressa* he found that the "ear-like points," mentioned in a passage we have cited, noticed in spongilla were, as Clark had suspected, right and left profiles of the cylindrical collar described by him.

Feeding *Grantia compressa* with indigo and then tearing it to pieces, he found the collared monociliated cells more or less filled with indigo, and in entire vitality."

Mr. Carter, up to the date of this paper, is disposed to regard the sponge-cell as something *sui generis*, and neither an amæba nor a flagellate infusorian. He says, "So long as the collar of the sponge-cell is present with the cilium, all particles of food may go in and out of the body through the collar; but as every part of the sponge-cell is polymorphic, it may put forth pseudopodia from one part in particular, or from any part of the body, like amæba."

Here then is plenty of work for microscopists, and it is hoped that students to whom these questions are novel will find the preceding remarks smooth their way.

EXPLANATION OF PLATE LXXXIV.

FIG. 1. Portion of Spongilla arachnoidea in section × 30. *a*, the monad chamber.

2. Interior of a monad chamber × 780.

3. Single monad as seen in profile in monad chamber. Two contractile vesicles *v v*. Cylindrical collar *c* extended to the utmost × 1,600.

4. Sectional view of monad chamber bringing the aperture, and monad on the same level into profile, showing their convergence towards central space × 780.

The above from Prof. Clark's paper, see "Monthly Microscopical Journal," March, 1862.

5. A, Sponge cell from *Grantia compressa*.
   B, " with pseudopodia at fundus of cell.
   C, " with collar transformed into pseudopodia.

REVIEWS.

VOLCANOES.*

ONE hardly expected that a book which first saw the light of publication no less than forty-seven years ago, should come to a new edition, and that a second, just thirty-seven years subsequently. Yet such is the case in the present work of Mr. Poulett Scrope. The book before us is hardly a newer one than that which appeared ten years ago, yet it contains a series of introductory remarks which extend over nearly twenty-four pages. But then it is as compared with the first edition that we have got to consider it, and assuredly it is a great improvement on that, as naturally follows from the great amount of work done since that period, and the vast number of earthquakes and volcanic eruptions which have since occurred. In fact to notice fully so vast a work is simply an impossibility, for we have not space for a discussion of even the very simplest question which it raises. Our opinion must therefore be expressed generally as to the qualities of the book, and the nature of the views which it puts forward. First, we must give every credit to the author for the vast nature of his researches and the extent of his observations, for he seems to have taken in nearly the whole world under his immediate observation. Secondly, his work must be considered, no matter what its faults are, as unquestionably the ablest treatise extant in any language upon the subject. But having made these admissions, we must find fault with the author for not having made any effort to explore the chemistry of this vast subject. Assuredly, if any science is able to explain thoroughly the nature of volcanic action, and the constitution of its products, it is chemical science; yet nowhere in Mr. Scrope's volume can we find the slightest allusion to chemical work done in the way of elucidating any of the problems of volcanic science. But if we leave this subject beyond our consideration, we are bound to confess that the book is an excellent one—so good that it need hardly be better. It is essentially a work which everyone who considers himself a geologist must procure and study, and he will find in its nearly five hundred pages the history of every volcano which is known in the world, and of every earthquake which has occurred since history became anything exact. There is one point which deserves

* "Volcanoes, the Character of their Phenomenon, &c. &c., with a Descriptive Catalogue of all known Volcanoes and Volcanic Formations." 2nd edition. By Poulett Scrope. London: Longmans, 1872.

notice, and which will perhaps astonish those good folk who are pleased to consider the earth a mass of liquid fire which has a slight crust above it on which we live. It is that Mr. Scrope does not share this view of the earth's constitution. He says: "It is no doubt an attractive sensational idea, that a molten interior to the globe underlies a thin superficial crust, its surface agitated by tidal waves, and flowing freely towards any issue that may here and there be opened for its outward escape; but I do not think it can be supported by reasoning based on any ascertained facts or phenomena ..... If, indeed, we are to theorise at all on the hypothesis that the earth originated from the condensation of a mass of nebulous vapour into an incandescent state, it would seem probable, as Mr. Hopkins has suggested, that solidification would begin at the centre and advance toward the surface, and that only after a long continuance of this process a time may have arrived when the remaining liquid matter, being of inconsiderable thickness, the surface also would begin to solidify by radiation of its heat into space; from which time the further solidification of the interior would proceed in two directions—outward from the central hardened nucleus, and inward from the external crust." There are many other points we should wish to notice, but we cannot. We must, therefore, conclude with our best thanks to the author for his invaluable book, and with advice to our readers to judge it for themselves.

LYELL'S PRINCIPLES OF GEOLOGY.*

THE great master who stands at the head of all geologists, whether European or American, has given us once again a revised edition of his incomparable "Principles of Geology." It is with a feeling of surprise that we look back to the time when Lyell's "Geology" was first issued to the English public. We believe it was somewhere about the year 1838, now thirty years ago; and it is not without a feeling of pleasure that we witness how the man has grown, so to speak, with the times, that he has modified his opinions as he had more light thrown in upon the subject, until he has now, in his eleventh edition, given us a work which for excellence of arrangement, for terseness of style, for beauty of illustration, for comprehensiveness of detail, and for general accuracy of opinion, can find no equal in the publications of the whole world. In this, the first volume of his "Principles," Sir Charles has seen fit to modify and recast some of the chapters which appeared in the former edition nearly five years ago. During the period which has since elapsed, much discussion has taken place on the questions of climate and the temperature of the ocean. Carpenter's great discoveries—of which there is some account by their author in the present number of this journal—have given the geologist many facts on the temperature and shape of the ocean bed, and have shown that some of the animals which he was wont to consider as exclusively fossil are really living still. The physical condition of the Mediterranean has been well defined by these dredging explorations, and

* "Principles of Geology; or the Modern Changes of the Earth and its Inhabitants considered as illustrative of Geology." By Sir Charles Lyell, Bart., M.A., F.R.S., 11th edition, vol. i. London: John Murray, 1862.

hence the geologist has found it necessary to modify to some slight extent certain of his former views. With these exceptions, the present work is the same as the edition produced in 1867. But the reader who has not got the tenth edition will find that this one contains a host of facts and arguments, which do not make any appearance in the earlier issues of this splendid treatise. Among the more important matters peculiar to this edition is Chapter xx., in which the author discusses Dr. Carpenter's opinions on the subject of ocean-temperature. In regard to this point Sir Charles and Dr. Carpenter are at issue respecting the currents of the Mediterranean. The latter affirmed (we do not know that he still affirms) that there is a constant current flowing at the depth of 250 fathoms out of the Mediterranean. This is denied by Sir Charles Lyell, who calls in the subsequent researches of Captain Nares, which seem to prove the correctness of his formerly expressed opinions. In regard to Dr. Carpenter's researches in the North Atlantic, Sir Charles is entirely with him in the opinion that there are two currents, an upper warm one going towards the pole, and an under cold one below 32° Fahrenheit travelling towards the equator. There are many other points of novelty in the volume which Sir Charles Lyell has just issued which we cannot touch upon, but we may conclude with a hearty expression of the delight with which we have read the most charming treatise in existence upon general geology.

DARWIN'S "ORIGIN OF SPECIES."*

IF we had only time and space to give a proper notice of Mr. Darwin's great work, we should do so with feelings of the most intense pleasure. For, even though this is the sixth edition, it is nevertheless a new book; inasmuch as the author has taken up the arguments of his several opponents, and, we may fairly say, has reduced them to little more than misinterpretations; and, on the other hand, he has given a mass of new matter which goes less or more to bear out his views of natural selection being the cause which has produced the present variation in animals from some four or five types. We have not seen the edition which precedes this, so we are not certain whether its type differs from that of the first one, but at all events we may mention that, in its present form, it is in a much smaller and less agreeable type than formerly. In making this alteration, the author, we think, was led into a mistake. We shall just cite the more important additions which the present volume possesses. It has taken up, for example, Mr. Mivart's chief objections, and, so far as we have seen, has answered them satisfactorily. Next it deals with Mr. Ray Lankester's most philosophic essay on morphology. Reasons are given for disbelieving in great and sudden modifications. The author confirms his statement upon the habits of the young cuckoo. He has the discussion on analogical

* "The Origin of Species by means of Natural Selection; or the Preservation of Favoured Races in the Struggle for Life." By Charles Darwin, M.A., F.R.S. 6th edition. London: John Murray, 1872.

resemblances enlarged and somewhat modified. He corrects his remarks on serial homologies, &c. &c. In fact, as we have said, the author has re-written the volume, and in doing so he has not neglected to take up all the various arguments that have been urged against him. He shows, too, that his supporters are vastly greater than they were, and that they gradually increase. His book has an American edition, and has been translated into German, French, Italian, Russian (three editions), Dutch, and Swedish. He may well be proud of the fact, but he cannot feel as great a pride as his English followers feel in him; for they admire his calm philosophy, and they rejoice in the fact that the author of the most philosophic book of the century is an Englishman.

SUPPLEMENT TO WATTS' DICTIONARY.*

A BOOK which is merely a supplement to a regular dictionary, and which contains no less than 1136 pages of very small type, is, it must be confessed, rather a tough piece of work for the reviewer. Of course we give the most sketchy notice of such a work, but we cannot help wondering at the vast labour of the editor and his colleagues. The present volume brings the record down to 1869, but it includes also several additions to and corrections of former results which have appeared in 1870 and 1871. Thus this volume completes perhaps the most valuable encyclopædic publication which the literature of any country possesses. Besides the editor's writing, there are various contributions on Electricity.—Heat, by Mr. G. C. Foster, B.A., F.R.S.; on Proteids, by Dr. Michael Foster; on Beer and the Metallurgy of Iron, by Dr. B. H. Paul; on Light and Spectral Analysis, by H. E. Roscoe, Ph.D., F.R.S.; and on Acetic Ether (in part), the Butyl Alcohols, Butyric Acid (in part), Ethyl and the Ketones, by Mr. J. A. Wanklyn. Among a great number of articles, those which strike us as most worthy of notice are those on Atomicity and Chemical Action; but all are well done and all are of importance. Especially so is a paper on the Aromatic series, in which some of Kekulé's remarkable views are introduced; and that on Electricity, in which are given some good illustrations of Thomson's galvanometer. All through the book is of admirable quality, and we congratulate the editor on its issue to the public.

OUR NATIVE SINGING-BIRDS.†

THOUGH it must be confessed that our literature on this particular subject is far from being barren, still there is room for such a work as the title of the book before us involves. We mean that, for a work dealing

* "A Dictionary of Chemistry and the Allied Branches of other Sciences." By Henry Watts, B.A., F.R.S., F.C.S.; assisted by eminent contributors. Supplement: Longmans, 1872.

† "British Song-Birds;" a Practical Treatise on their Habits, Nidification and Incubation; the Mode of Rearing Young Birds, and their Treatment in Sickness and in Health. By Joseph Nash. London: W. Tegg, 1872.

with the habits, mode of culture, and feeding of our British song-birds, and containing just those hints on management which one who is thoroughly experienced can give so well, there is undoubtedly a want in the English language. We certainly think that Mr. Nash, the author of the little book now before us, has met this want very well, and has fairly put before the public just those practical facts which, in our opinion, were absent from the cheaper works on the subject. In the volume upon our table the author has put into rather less than 100 pages all those practical facts relating to our singing-birds which are so essential to those engaged in rearing the young ones, and which it too often happens that amateur bird-fanciers are wholly ignorant of. He has given, too, about fourteen coloured illustrations of the birds, which are in most instances very well executed indeed. Thus, from a practical point of view, the little work is everything that can be desired. If we were to examine it from a scientific stand-point, of course we should give many examples of erroneous matter; but the author so distinctly explains the nature of his writing, and from his stand-point it is so fairly done, that we have nothing but good words to utter of his labours.

ANTI-DARWINISM.*

WE wonder whether the anti-Darwinists ever think of consulting the various works which they, as a body, have written; for we are sure that if they did, and were solely inspired by the notion of repelling Darwinian doctrines, they would pause ere they gave another volume to the world. These gentlemen, one or two excepted, have attacked Mr. Darwin in every possible manner, with every conceivable form of expression which is not legally libellous. Yet these books have so much in common, i.e. so much ignorance of the Darwinian doctrines, and of the evidence that supports them, that we think it a pity that each writer did not previously read what his own side have said on the subject. What is most manifest to an impartial reader of these books is, the tremendous ignorance of the writers of everything connected with the very seriously difficult questions involved in a true discussion of the subject. Generally speaking, they are written by men who have no knowledge of Natural Science whatever, and who have never written—unless, indeed, in the controversial tract field—upon any subject whatever. It is very melancholy that such essays should find any readers, but we fear that each of them makes its way among hundreds of people who have faintly heard of the disturbance to their religion caused by Mr. Darwin's writings, and who therefore look on a book written by the local schoolmaster, or some other ambitious member of their society, as something which sets their minds perfectly and for ever at rest.

Now, Lord Ormathwaite is, of course, superior to the class to which we have referred, and his essay is therefore equally above the absurd publications which that class invariably produces. In all his pages we find the

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* "Astronomy and Geology Compared." By Lord Ormathwaite. London: John Murray, 1872.

language and style of a gentleman who feels the importance of his subject, and is determined to give full admission to the skill and knowledge of his opponent. But having made this admission, we must confess that it is not a work which can affect Mr. Darwin other than to afford him a good-natured smile at the grave shallowness of his adversary. We do not mean to insinuate that Lord Ormathwaite may not be well versed in other subjects, but unhappily in all that relates to geology and biology he is most deplorably ignorant. Hence we do not find that in a single instance he has really attempted to upset Mr. Darwin's views. He opposes them, of course, but there is not a single page of his volume in which he attempts seriously to disprove Mr. Darwin's ideas. We do not say that he admits them, for he objects to them as forcibly as he can; but what we do say is this, that there is not a single argument which he uses that Mr. Darwin could not readily expose the fallacy of if he wished. A single sentence or so, reproduced from the volume, will show how ignorant of Natural Science is the author, and will spare us the trouble of dealing further with his work, and our geologic readers of purchasing it. Alluding to Mr. Darwin, he says, p. 73: "He speaks of millions of years, and in his diagram he represents 10,000 or 14,000 generations; *but the existing world, of which either from history or tradition we have any knowledge is comprised within at most* 4,000 *years.* . . . . The whole number of existing generations of man of whom we can have the faintest knowledge have existed, is 120. It is quite evident, therefore, that Mr. Darwin assumes as the basis of his argument periods of time *purely imaginary*; he can have no *shadow of proof* that the world has existed for 14,000 generations, or 470,000 years."

This is enough to show the range of knowledge from which the author seeks to prove his case, and of course it at once excludes his opinions from the consideration of geologists. The book is altogether rather well written, and shows no display of bad taste whatever.

A TREATISE ON INTESTINAL WORMS.*

THOUGH this is strictly a medical work, it is also a scientific one, for assuredly nothing is more scientific zoologically than the pursuit of knowledge in the case of the Entozoa. But Dr. Cobbold, who is at once the most skilled and celebrated English helminthologist, has, in the work now before us, united his scientific knowledge *par excellence* to the patience and care of the physician, in endeavouring to pursue his studies practically on the patient. In a word, then, the volume which he has now written is a practical and scientific one. It is essentially a book which the physician must read if he desires to be *au fait* of the practice of those who know most of the subject, and it is furthermore a book from which the general reader may add materially to his knowledge of the habits and distribution of those various forms of "worms" which attack the human system. Dr. Cobbold's

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* "Worms; a Series of Lectures on Practical Helminthology, delivered at the Middlesex Hospital." By T. Spencer Cobbold, M.D., F.R.S. London: J. and A. Churchill, 1872.

book extends over more than 150 pages, and it contains the full reports of no less than eighty-one cases which have come under the author's care, most of which he has discharged cured, and all of which he has very fully reported. The book is divided into twenty chapters, which correspond to so many lectures delivered before the students of Middlesex Hospital; and it deals with all forms of Entozoa, and with a curious case of Bots which came under the author's notice. The most remarkable and interesting chapter in the book is the last, which describes minutely a case of *Bilharzia* which was under Dr. Cobbold's immediate care for several weeks. Altogether the work is most interesting and instructive, and is a very useful addition to our medical literature.

A NEW STAR-ATLAS.*

ANOTHER of Mr. Proctor's books! Really we wonder how it is that Mr. Proctor finds time for all his work. He is Secretary to the Astronomical Society, which of itself alone should take up much of his time. In addition he is a contributor nearly every month to some one or other of our scientific magazines; and further, we find him bringing out some new book or other within almost every quarter. It is certainly astonishing labour, and all the more that the work is unquestionably well done. Before us is his latest effort in a literary and scientific direction. It is an excellent Star-Atlas which is small enough to fit easily in anyone's coat pocket, and which is smaller than the atlas of the Society for Diffusing Useful Knowledge, and yet has larger maps. This seeming anomaly is explained by the fact that the maximum expansion of the S. D. U. K. maps, owing to their distortion, is no less than fifty-eight times greater than in Mr. Proctor's atlas. Besides, each of the present twelve maps includes a tenth part of the heavens. Of the general plan of the maps the author's prefatory remarks give a clear account:—"The meridians and parallels are drawn in to every fifteenth, instead of every fifth degree [as usual]; but since all the intersections of these lines to every fifth degree are marked in the maps, the places of stars can be determined, from catalogues or the like, as readily as though the lines themselves were marked in. In like manner all the latitude and longitude lines, except the ecliptic and the solstitial colures, are omitted; but then intersections to every fifteenth degree are marked, and any student who is sufficiently advanced to require these lines will be able to recognise very readily where they lie, or as to pencil them in if need be. . . . . The method of indicating the effect of precession is also novel. Instead of a precession-triangle in the corner of each map, with instructions for obtaining compass measurements, I have placed precession arrows over the maps [always on latitude parallels 15°, 30°, etc.]; and these show at once by what amount stars in the neighbourhood are precessionally displaced in one hundred years." Further Mr. Proctor points out the advisability of the

* "A New Star-Atlas for the Library, the School, and the Observatory, in Twelve Circular Maps; intended as a companion to Webb's Celestial Objects for Common Telescopes, &c." By R. A. Proctor, B.A., F.R.A.S. London: Longmans, 1872.

student procuring Webb's "Celestial Objects," which is published by Mr. Hardwicke, as the best companion to the present work. Of course there are some few stars in Mr. Webb's book not figuring here; but there need be no difficulty about this, as Mr. Webb gives in every case the right ascension and declination. We quite approve of Mr. Proctor's plan of omitting the barbarous constellation figures; it is the first step towards getting rid of them altogether, which we hope will soon follow. His instructions to beginners are ample, clear, and to the point, and we do not see why any boy of intelligence might not with the assistance of this atlas make the acquaintance of the heavens in a single winter. The maps, which are twelve in number, are admirably clear, and all contain stars as low as under the sixth magnitude. We approve, too, of the fact that each map contains a table of explanation of the abbreviations; for though they are identical on each, it would have been extremely awkward to have to refer back from the third or fourth map to the first, had the other plan been followed. Mr. Proctor has some amusing remarks about literary authors' ignorance of astronomy, Dickens appearing as an example; whilst he declares that "Tennyson is singularly accurate in all astronomical details." In conclusion, we can only thank the author for a very good book, and wish it the success it so thoroughly merits.

AN INDEX OF SPECTRA.*

WE cannot predict that the author of this work will be rewarded commercially for the labour he has undertaken; but most certainly he deserves the hearty thanks of scientific spectroscopists for the arduous task he has so well discharged, in making some effort to render the labours of those who are engaged in working with the spectroscope, something more systematic than they have been of late. Anyone who knows anything of this very difficult, and we may almost say novel method of research, is aware of the inconvenience arising from the employment of different scales in the mapping of spectra. Now the object of the author is to render spectroscopic research simple by collecting all existing measurements of the spectra of the elements, and presenting them on a uniform scale of wave-lengths. This scale, which is adapted to the measurements obtained from very large spectroscopes, is also as suitable for an instrument which has but a single prism. We hope, therefore, with Professor Roscoe, that this index of Dr. Watts may lead eventually to the adoption of an uniform scale. Some excellent plates accompany the volume. These give the spectra of sixty-one of the elements known to chemists; the first plate is devoted to coloured double spectra of carbon, sulphur, and nitrogen; those at the end of the work give the spectrum of each element, on Bunsen's plan, viz. the intensity of each bright line being represented by the height of the line which corresponds to it. Altogether the book is a careful and well-done piece of work.

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* "Index of Spectra." By W. Marshall Watts, D.Sc., Senior Physical Science Master in the Manchester Grammar School. With a Preface by H. E. Roscoe, B.A., F.R.S. London: Gillman, 1872.

MESSRS. LONGMAN'S TEXT-BOOKS: ARITHMETIC AND MENSURATION.*

ALL Messrs. Longman's series of text-books are excellent, and the present one is by no means an exception to this good rule. It is, if anything, rather better than some of the others in the one respect of style. It is remarkably clear and to the point, while in some respects it resembles Bishop Colenso's scientific works, in that no intermediate stage is left out. Of course it is not simply an ordinary book on arithmetic. The mere elementary matter is much of it omitted. Nevertheless it is a work which the schoolboy can readily understand, and one which we should think he would be improved by, and at the same time which he would like, from the earnest simplicity of its style. The particulars of the work are as follows: The first is the division of fractions into two marked periods, the rule of greatest common measure being placed in the latter division. The second is the "finding the cube-root by substitution in a simple and easily-remembered formula, in preference to giving the rule for extracting the root, a rule which can hardly be understood or remembered by any person unacquainted with algebra." The third portion consists in the introduction of a chapter on mechanical work. This the author considers somewhat a bold step on his part, but he thinks it by no means out of the way in a book which is to be read, as the present one is, by mechanics. For ourselves, we deem it a capital deviation from the ordinary plan, and we think the author is to be praised for his departure from existing ways. The treatise on mensuration is a good one, and the examination-papers will be found useful by the student.

SPECTRUM ANALYSIS.†

ASSUREDLY scientific men did not require such a volume as that which now lies upon our table. What with Roscoe's work in English, and the various foreign contributions to the science of spectroscopy which are extant, they had more than enough of works of reference. Yet is the present an admirable essay, and one which it would have been discreditable to our popular taste to have left untranslated. It is not essentially a scientific book—a work of reference for the worker at the spectroscope—but it is in the highest degree a book which will do much to popularise the pursuit of spectroscopy. For it is remarkably clear and intelligible, and it includes so much general information in connection with the subject, that we do not see why it may not be easily read, and read with profit, by any

* "Technical Arithmetic and Mensuration." By Charles W. Merrifield, F.R.S., Principal of the Royal School of Naval Architecture. London: Longmans, 1872.

† "Spectrum Analysis in its Application to Terrestrial Substances, and the Physical Constitution of the Heavenly Bodies Familiarly Explained." By Dr. H. Scheller, of Cologne. Translated from the Second Edition by Jane and Caroline Lassell. Edited, with notes, by William Huggins, LL.D., F.R.S. London: Longmans, 1872.

ordinarily intelligent person outside the domain of science. Hence, we think, the two translators are to be thanked for having performed an exceedingly arduous task with remarkable skill; and our gratitude is also due to Mr. Huggins, for having helped them with his good advice and with suggestions as to the introduction of several papers which give an increased value to the work. It is a book which, for the general reader, deals with spectroscopy in all its various applications, and with the several questions in astronomy and general physics which have relation to it. Thus we find that the editors have introduced papers by Mr. Stoney on the cause of the interrupted spectra of gases, and by Professor Young on the lines in the spectrum of the chromosphere, and that they have reproduced Angström's maps of the solar spectrum, some of Mr. Brother's photographs of the corona, and certain of the solar prominences as depicted by Professor Respighi. Thus we have everything that bears on the subject brought together into one handsome volume, which is absolutely one mass of illustrations, exclusive of the several admirable page plates which illustrate the volume. And, while we are on this part of the subject, we do not think that too high praise can be awarded to the artist who produced the exquisite plate of the solar prominences observed by Respighi in 1870. These are undeniably the most artistic plates which we have seen upon the subject, and great credit is due to their artist. But, besides these, a number of other excellent plates are spread throughout the volume; those of the total eclipses of August 1868 and August 1869, and of Angström's map of the solar spectrum, being especially to be commended.

We think it was wise of Mr. Huggins to express his dissent from many of the views laid down by the author; for we fear that, in not a few cases, he will be found of a different opinion from the great mass of the leaders on the subject. Mr. Huggins especially differs from the author in his views on "the influence of temperature and density on the spectra of gases," and he expresses his regret that the author in his plates should have reversed the spectra from the position usually given them. Still he speaks well of the book as a popular treatise, and as such we essentially consider it. In this respect it is an admirable and extensive work, well illustrated and translated; and, being intended for the general public, it enters into a number of connected elementary questions which would not be found elsewhere. We recommend it as a capital volume for those who desire to know something about a science as vast as it is novel.

SHORT NOTICES.

A Monograph of the British Graptolitidæ, by H. Alleyne Nicholson, M.D., Professor of Natural History in University College, Toronto. Blackwood and Sons, 1872.—This monograph we have just received. It seems a most valuable essay upon a group which is too little studied. Prof. Nicholson, however, has with Mr. Carruthers and Mr. Charles Lapworth, Mr. John Hopkinson, and Mr. W. Hellier Baily, done much to explore the group. In this work, which covers nearly 150 pages, and has nearly 100 illustrations, he does much to unravel the difficulties which this class presents, and he furnishes an admirable account of the series upon which he means to continue his researches.

Hints and Facts on the Origin of Man and of his Intellectual Faculties, by Pius Melia, D.D. London: Longmans.—This little book—less than 100 pages—is the offshoot of a fearful amount of ignorance and self-conceit. There is nothing in it worthy of a moment's notice from the scientific man. It is full of typographical errors, especially in names, which shows the author to be ignorant of the works referred to. Thus, we hear occasionally of Lemarck, a naturalist. Who is he—will anyone tell us?

Practical Physiology; being a School Manual of Health, for the use of Classes and General Reading, by Edwin Lankester, M.D., F.R.S.; 5th edition. London: Hardwicke, 1872.—In this new edition of his work Dr. Lankester has somewhat modified the title, making it more physiological than it was before. We think the change is a wise one, for the book is essentially a popular manual of physiology. So far as we have seen, the work is a good one. Of course it only touches on functions of life in the most general manner possible, but then its style is peculiarly happy, and is calculated to lead one on from page to page from the interest it creates. Besides the text, there are a number of plates which are new to this edition, and which contain fifty capital drawings illustrative of physiological facts; and there are also a number of questions upon each chapter, which appear to us clear and well arranged. The classification of the animal kingdom, with which the work concludes, contains some typographical errors which it is a pity were not seen before the sheets went to press. Otherwise, the work is a good one, and calculated to serve the ends in view.

A Synonomic Catalogue of the Diurnal Lepidoptera, by W. F. Kirby. London: Van Voorst, 1871.—We regret that owing to pressure on our space we were obliged to omit a notice of this work from our last number. It is a marvellous essay, covering nearly 700 pages of print. It is, of course, a special work in which few of our readers will be interested, but it is essentially a treatise which must be regarded as an authority on that branch of the butterfly order on which it treats. It may give the uninitiated some glimpse of the vastness of the subject of zoology, when they see a large book like the present one, filled with little more than the names of species of a branch of a particular order of the class of insects. The index alone covers more than thirty pages of small type. The names of the genus and species are first given, then reference is made by page, &c., to the book referred to, and then follows the country in which it has been observed. It is to the entomologist a most invaluable work, and one which must have cost Mr. Kirby many years' earnest labour.

Remarks on the Prevailing Epidemic of Small-pox: its Cure and Prevention, &c. London: Longmans.—This pamphlet bears no date on the title-page, but from the date of the preface we take it to have been written last year. It is an anonymous and able essay on the general sanitary condition of society and the remedies which offer themselves. It will be read with advantage by all who are in any way connected with public sanitary matters.

Principal Economic Products from the Vegetable Kingdom. London: Hardwicke, 1872.—Here we have a classification which may be useful to either the student of botany or to him who is working at "Materia Medica." In a three-fold table we have first the natural order, then second the economic product and name of plant, and thirdly, the parts of the plant used. It is a small pamphlet, but it seems, in most cases, well done.

Darwinian Pamphlets of Wright, Hunter, Cope, and Mivart.—A series of these we have collected, and we think it well to name them for our readers, but we cannot possibly notice them. They are really clever essays, written by men who clearly comprehend the nature of the subject they are writing upon, and having in them nothing that is not worthy of being read. We at one time thought of noticing them, but we soon found the space at our disposal was infinitely too brief, so we are compelled to urge our readers to get them for themselves (the expense is a mere nothing), and pass their judgment upon them. They are in the order of their publication, as follows: "Darwinism: being an Examination of Mr. St. George Mivart's 'Genesis of Species,'" by Chauncey Wright, Esq. London: John Murray. "A Review of Darwin's Theory of the Origin and Development of Man," by James Hunter, M.D. New York: Appleton & Co. "On the Method of Creation of Organic Types," by Edward D. Cope, A.M. Philadelphia: M'Calla & Stavely. "Evolution and its Consequences," by St. George Mivart, F.R.S. From the "Contemporary Review" for January, 1872. This, Mr. Mivart's last essay, we regret, as it develops a quarrel between himself and Professor Huxley; but it is, as asserted by those who think with Mr. Mivart, a justifiable reply to Professor Huxley's attack. It deals with religious questions almost exclusively.

Hymns of Modern Man, by Thomas H. Noyes, Jun., B.A. London: Longmans, 1872.—This is a book quite out of our line. Still it has some bearing on science, so we may notice it. The "Philosopher's Stone" is rather forcible; the other pieces are not so. We take the following from a notice which accompanied the book. "The opening hymn, entitled 'The Philosopher's Stone,' distinguishes between Theology and Religion, which it attempts to define, and shows that sound philosophy, which takes cognizance of the conditions of human happiness and of the Divine laws of creation, sanctions the view that the performance of those loving duties towards our fellow-men, by which Christ taught us to prove our love to our common Father in heaven, is the only religion which is worth its salt, and commends itself to reason."

Chemical Notes for the Lecture-room, Chemistry of the Non-Metallic Elements, by Thomas Wood, Ph.D., F.C.S. 3rd edition. London: Longmans, 1871.—From the fact of this being the third edition, we are led to suppose that the work must have had a considerable success. Yet it is a book essentially adapted for junior students alone. It has been considerably improved in its present form, and but for the peculiarity of the style would doubtless be a very good book for the beginner in chemical science. What we object to, is the particularly loose fashion in which it is written; for example, take the description of the blowpipe, which is unaccompanied by a woodcut. The author says, "The blowpipe is a tube bent at right angles, and drawn to a point at one end. Through the fine opening of the pipe, air is forced by the mouth or bellows while the pipe is in the centre of the flame." Now we should like to know what idea a student would gather from such an account? We also observe that the author is fond of "popular" descriptions, which, as in the case of hairs, which he says, "are tubes," are generally incorrect. The want of illustrations is an obvious defect in a book like the present, intended exclusively for the young. Still we must not be too severe, for the book is, on the whole, a good one.

New Theory of the Figure of the Earth, considered as a Solid of Revolution; founded on the direct employment of the centrifugal force, instead of the common principles of attraction and variable density, by W. Ogilby, M.A., F.G.S. London: Longmans, 1872.—Readers must really judge for themselves of the value of such a work as the present. For ourselves, we prefer to conceal our opinion of the book, as it would not be complimentary to the author were we to express really what we think about it. Mr. Ogilby says, that "the principles of the method here proposed for treating the problem of the figure of the earth are extremely simple. I make no hypothesis of any description, nor require any other data than those which are furnished by observation and experience. Starting from the admitted phenomenon that the earth is a heterogeneous solid whose mean density, magnitude, and periodic rotation are known quantities, I thence proceed to examine the action of the centrifugal force in producing its present figure, determining the law of gravity at its surface, the variation of curvature, the length of the terrestrial axis, and the change of local ellipticity at every point on the surface; with other phenomena relating to its present and original structure and condition. This mode of treatment furnishes definite results without doctoring the process either by the introduction of extemporised assumptions, or the rejection of unmanageable quantities."

Reports on Observations of the Total Solar Eclipse of December 22, 1870, conducted under the direction of Rear-Admiral B. F. Sands, U.S.N. Washington Government Printing Office, 1871.—Of very different material to the above-mentioned volume is the able series of essays contained in the work now under notice. It is the combined series of reports of the different American astronomers who witnessed the eclipse of December 1870. First is the report of the editor of the volume; then those of Professor Simon Newcomb, on observations at Gibraltar; of Professor Asaph Hall, of Professor William Harkness, and of Professor J. T. R. Eastman, of their several observations at Syracuse, in Sicily. The volume is accompanied by a couple of excellent coloured plates of the solar eclipse; one by Captain Tupman, of the entire phase of the eclipse, and the second by Professor J. R. Eastman, of the corona and protuberances on the western limb of the sun, near the end of the total phase of the same eclipse. These accounts are all capital and very full, some being possessed of details absent from the others, some have descriptions of the minute details of the spectrum examination, others having to do more with the meterology of the observations. All are most exact and comprehensive, and *tout entier*, they form a volume which no modern astronomer should be without.

Elementary Treatise on Natural Philosophy, by A. Privat Deschanel. Translated and edited by J. D. Everett, M.A., D.C.L., F.R.S.E., Professor of Natural Philosophy in Queen's College, Belfast. Part III. Electricity and Magnetism. London: Blackie & Son, 1872.—Assuredly this is the best of the three volumes which Professor Everett has edited and translated. It is so for the simple reason that the editor has taken great pains to make it a different work from the French edition. It is certain that our English electricians are far before their continental brethren in their knowledge of the branch of Natural Philosophy which is included in the present volume. Hence, of course, it was essential for the editor to introduce into a work intended for English readers all those results which have been so prominently brought before English men of science by the researches of Faraday, and still later of Sir William Thomson. In laying these before the reader, the editor has been at considerable trouble, for he has written no less than two entrely new chapters, and he has entirely remodelled all those parts of the original text which in his opinion required it. The elements of the theory of magnetism, which form a part of this volume, have been specially prepared from the several papers of Sir W. Thomson in the "Philosophical Transactions." Thus we find, on the whole, an excellent volume, embodying the latest results in science in a clear style, and with no less than 250 capital illustrations. We must say that we are well pleased with this the third volume of an excellent treatise.

Rudimentary Magnetism, being a Concise Exposition of the General Principles of Magnetical Science, by Sir Snow Harris, F.R.S. 2nd edition, by H. M. Noad, Ph.D., F.R.S. London: Lockwood, 1872.—This is a much improved edition of an old and well-known manual, we think in Weale's Series. The present edition contains matter relating to Faraday's researches and the inquiries of the Astronomer Royal. It seems well put together, and appears to be a very good book indeed.

The Higher Ministry of Nature viewed in the Light of Modern Science, &c., &c., by John R. Leifchild, A.M. London: Hodder & Stoughton.—This is a book which strives to come between the followers of Mr. Darwin and Mr. Spencer's doctrines on the one hand, and the extreme religionists on the other. It is, however, much nearer the latter than the former. The author does not appear to have a thorough understanding of Darwinian doctrines, and hence he cannot argue against them with any force. However, we shall take him up more fully in our next number. For the present we leave him, with an unfavourable opinion of his ability as a reasoner.

SCIENTIFIC SUMMARY.

ASTRONOMY.

Anniversary Meeting of the Royal Astronomical Society.—At this meeting, which took place on February 9, the gold medal of the Society was awarded to Professor Schiaparelli, for his researches into the connection subsisting between meteors and comets. In our summaries during the past four or five years, the details of Schiaparelli's researches, as they have been severally announced, have been fully discussed. At the same meeting the following officers were elected for the ensuing twelve months: President, Professor Cayley; Vice-Presidents, Professor Adams, Drs. De La Rue and Huggins, and Mr. Lassell; Treasurer, Mr. Whitbread; Secretaries, Messrs. Dunkin and Proctor; Foreign Secretary, Lieut.-Colonel Strange.

The Eclipse of December 12, 1871.—The observations made during the eclipse of last December were, on the whole, remarkably successful. The Australian observers had bad weather, and one of the observing parties in India was similarly unfortunate; but at all the other stations very favourable conditions prevailed. A full account of the operations of the various parties will be found in these pages elsewhere, and therefore in this place we shall merely summarize the work which has been accomplished. The expedition from England distributed along the part of the shadow track between Bekul on the western coast of India, and the northern extremity of Ceylon, did excellent work. Five good photographs of the corona were taken by Mr. Davis, who accompanied this expedition at Lord Lindsay's charge. They show the corona widely extended, alike in all the pictures, and with permanent rifts. Professor Respighi, the well known Italian spectroscopist, also accompanied this expedition. He took out with him the instrument with which he has drawn his excellent series of solar profiles during the past three years. Using this instrument, he obtained a spectrum of images of the chromatosphere and prominences (seeing at the same time the red, orange-yellow, blue-green, and indigo pictures of the ring of prominences), and a spectrum of images of the inner corona, these images being formed by the yellow-green light corresponding to Kirchhoff's 1474, and by red and blue-green light corresponding to the C and F lines of hydrogen. The yellow-green image extended about eight minutes from the limb of the eclipsed sun, and was well defined at its outer edge, though fainter there than near the sun. It is demonstrated, therefore, that a solar atmosphere extends to at least the observed distance from the sun, and that besides the "1474-matter"* there is hydrogen in the sun's atmosphere to this height, that is to a distance of upwards of 210,000 miles from the sun. Mr. Lockyer endeavoured to see these images of the corona by another plan, suggested by Professor Young early in 1871—the use, namely, of a train of prisms without any slit; but though he saw four images of the circle of prominences, the images of the inner corona, as seen by him, extended from the sun to only about one-third of the distance observed by Professor Respighi, so that it would appear as though the atmospheric conditions had not been so favourable in Mr. Lockyer's case. Mr. Halliday has succeeded in obtaining excellent drawings of the corona. The observing party from the observatory at Madras, under Mr. Pogson, the government astronomer there, did excellent work at Avenashy, obtaining good photographs, and confirming many of the spectroscopic observations made during former eclipses. Col. Tennant's party, at Dodabetta, also obtained six good photographs. Among the spectroscopic observations made by this party must be noted the complete confirmation of Professor Young's important discovery that the Fraunhofer lines undergo reversal at the moment when totality is complete. It is now demonstrated that the region where the absorption takes place to which the Fraunhofer lines are due, is an atmosphere of great complexity, relatively very shallow, and under ordinary circumstances either wholly unrecognisable by the spectroscopist, or only to be recognised by its effect in causing the spectrum of the very edge of the sun's limb to appear continuous. M. Janssen, who was to have gone to Java, preferred, on inquiring into meteorological relations, to take up his station at Ootacamund, on the Neilgherries. Here, with a telescope of larger aperture than had yet been applied to the spectroscopic analysis of the corona, and raised higher above the sea-level than any former observer of the eclipsed sun, he made observations of the utmost interest and importance. His general conclusions respecting the corona are indicated in the following words addressed by him to the secretary and president of the Paris Academy of Sciences. "Nothing could be more beautiful, more luminous, than the corona; with special forms excluding all possibility of a terrestrial atmospheric origin. I think the question whether the corona is due to the terrestial atmosphere is disposed of (*tranchée*); and we have before us the study of the extra solar regions, which will be very interesting and fruitful." . . . "The spectrum of the corona, which contains a very remarkable bright green line already announced, is not continuous as has been asserted, and I have found indications of the dark lines of the solar spectrum, notably D." The recognition of the solar dark lines in the coronal spectrum is a circumstance of extreme importance, as demonstrating the fact, hitherto only suspected, that a proportion of the coronal light is simply reflected sunlight.

Spectrum of the Zodiacal Light.—M. Liais, at Rio Janeiro, has succeeded in observing the spectrum of the zodiacal light. It will be remembered that the eminent Swedish spectroscopist, Angström, had announced that the zodiacal light is monochromatic, its spectrum being the same yellow-green light which appears in the spectrum of the aurora borealis. It would appear that Angström was misled by real auroral light, not recognisable

* The name suggested by Professor Young for the substance producing the line corresponding to division 1474 of Kirchhoff's scale.

as such; for it was noticed, during the aurora of February 4 last, that parts of the sky where no auroral light could be recognised gave the strongly marked green line referred to. In any case, Angstrom's observation had never been confirmed, and had come to be regarded on that account with a certain degree of suspicion. It now appears that the zodiacal light gives a continuous spectrum, exceedingly faint—so faint, indeed, that the solar dark lines could not be expected to be discernible, even though they may really be present in the spectrum. The inference is, that the zodiacal light is simply faint reflected sunlight, as had been always supposed until Angstrom's remarkable observation threw doubt on the matter.

Spectrum of the Aurora.—Although the aurora is not, strictly speaking, an astronomical phenomenon, yet it is so intimately associated with several important solar phenomena, that we may be permitted to notice the results obtained during the remarkable auroral display of February 4. The well-known green line (wave-length 558) was as usual the most conspicuous feature of the spectrum, except that, where the red streamers were exceptionally brilliant, the ordinarily faint red band (wave-length 650) became the brightest. Very faint greenish and bluish bands appeared, having wave-lengths 530, 510, and 490 approximately. Father Perry, who observed the aurora at Stonyhurst, remarks that the green line could always be detected even where the unassisted eye failed to notice any trace of auroral light, and he suggests the "advisability of a daily observation with a small hand spectroscope for those who are desirous of forming a complete list of auroral phenomena. Magnetic disturbances are a sure guide in the case of grand manifestations of aurora; but might not a very slight aurora be observable without the magnets being sensibly affected?"

Temperature of the Solar Photosphere.—Father Secchi has endeavoured to maintain his startling theory, that the solar photosphere has a temperature of 10,000,000° C., against very strong opposition in the Paris Academy of Sciences. MM. Faye, St.-Claire Deville, E. Becquerel, Fizeau, Vualle and Vicaire, agree in adopting 10,000° C. as the probable superior limit of the solar temperature—an estimate falling far short of the value 27,000° C. adopted by Spörer, in advance of which again, at a long interval, lies Zöllner's estimate of 400,000° C. But Secchi and Ericsson had been contending over their rival estimates of ten-million and four-million degrees centigrade; and it is somewhat amusing to find the Paris Academy, invited by Secchi to express an opinion, adopting a value beyond comparison less than the least of the rival estimates. The result is not greatly to be wondered at, however, for Ericsson's value had been obtained by an erroneous system of observation, and Secchi's by an erroneous method of interpreting observations which were in themselves sufficiently trustworthy. The views of the Academicians, being based on a variety of independent considerations, acquire thereby additional weight. Faye bases his opinion on the researches of Professor Thomson; Fizeau adduces the experimental researches undertaken by himself and M. Foucault into the relative intensity of sunlight, and the light of the electric spark under various conditions; Vicaire deduces his estimates from Secchi's observations; and St.-Claire Deville quotes his own experiments on the heat of the oxyhydrogen flame.

The Solar Prominences.—Father Secchi has published a *résumé* of his observations of these objects, tabulating the number, height, and width of the prominences observed in different solar latitudes during nine complete solar rotations. He finds that there are two principal maxima of frequency, placed between 20° and 30° of north latitude, and between 10° and 30° of south latitude; and two secondary maxima between 70° and 80° in each hemisphere. In successive rotations there is no trace of a progressive motion of the principal maxima towards the poles, as had been suspected. Where the prominences are most numerous they are most lofty, and have their widest extent (notwithstanding the circumstance that occasionally we recognise narrow and isolated prominences reaching to a great height.) Comparing with these results the observed distribution of faculæ, Secchi finds that the regions richest in faculæ coincide with the regions where the prominences have their maximum height and width. Among 893 prominences which he observed, 471 were found to have a well-marked slope, resembling inclined plumes; of these 370 were inclined in agreement with the law of movement of the solar atmosphere, and only 101 were sloped in the contrary direction; 40 were vertical at the poles and equator. "Comparing this result," says Secchi, "with that mentioned in former communications, we see that it cannot be accidental. I see that M. Spörer has arrived at the same result later. I may remark here that during the epochs of greatest activity the law is more constant and decided."

The November Meteors.—After all, it would appear that the end of this meteor stream has not been reached. The fact that no meteors appeared on the morning of the 16th was simply due to a displacement of the zone accompanied by a spreading out of the system of meteors. The following list of meteors, observed at Alexandria on the night between November 14 and 15, serves to show that many meteors of the system still remained:—

Between	10	and	11	p.m., November 14,	3	meteors
"	11	"	12	" "	6	"
"	12	"	1	a.m., November 15,	19	"
"	1	"	2	" "	23	"
"	2	"	3	" "	84	"
"	3	"	4	" "	100	"
"	4	"	5	" "	95	"
				Total	330	

As the sky was at no time clear, this result shows that a real meteoric shower was in progress. "It appears," says Professor Denza, who reported the observations made at several Italian and Mediterranean stations, "that the 15th was the true date of the passage of the November stream; so that it was later by a whole day than in former years. Nevertheless the meteor-cloud has appeared not only to be less dense, but displaced from its usual position, perturbed and irregular, and, as it were, a tenuous tail behind the more densely crowded central group." He quotes Schiaparelli as having mentioned that in 1818, or midway between the great displays of 1799 and 1833, a fine shower took place. Mr. Glaisher must, therefore, not suffer his band of observers to rest as he has proposed, but, on the contrary, should extend their nights of watchfulness over a longer period.

The Sun Spot Period.—Messrs. De La Rue, Stewart, and Loewy, have published another valuable paper on the laws of solar spot-frequency. The chief points in this paper are the following:—First, the average period from minimum to maximum spot-frequency is 3·4 years; the average from maximum to minimum 6·9 years. Secondly, a retarded ascent to the maximum is followed by a retarded descent to the minimum, an accelerated ascent by an accelerated descent. Professor Wolf had enunciated the theory that a retarded or accelerated descent to the minimum is followed by a retarded or accelerated ascent to the maximum. Messrs. De La Rue, Stewart, and Loewy, after carefully comparing the evidence for the two theories, consider that their own is fully made out. It should be added that, on *à priori* grounds, their theory appears far the more probable.

Planets for the next Quarter.—Jupiter will be in quadrature on April 10, and is passing too far to the west to be altogether well placed for observation, though he will form a fine object in our evening skies for the next few months. Uranus will be in quadrature on April 16, and in conjunction with Jupiter (very close) on June 5, at 6h. 11m. p.m. Saturn will not be well placed for observation till towards the end of the quarter, when he will be approaching conjunction, which he reaches on July 9. Mars and Venus will not be well placed for observation.

Partial Eclipse of the Moon.—There will be a partial lunar eclipse, visible at Greenwich, on May 22. The following are the mean times of the chief phases at Greenwich:—

	H.	M.
First contact with the penumbra	9	9·8
„ „ shadow	11	40·9
Middle of the Eclipse	11	18·2
Last contact with shadow	11	55·5
„ „ penumbra	13	26·6

The first contact with the shadow will occur at 177° from the north point of the moon's limb towards the east; the last contact at 143° towards the west (in each case, for *direct* image). The magnitude of the eclipse (moon's diameter = 1) will be 0·116.

BOTANY.

A somewhat rare Fungus, the Xenodochus Carbonarius, has recently been presented to the Manchester Philosophical Society by the Rev. J. E. Vize, M.A., who stated that it occurs near Welshpool, in a railway cutting, with a south-westerly aspect, well sheltered by a hill and a wood. The first appearance on the leaves of *Sanguisorba officinalis*, L., was noticed in the middle of May, when the Lecythea-form was in perfection, but the stems and other portions of the Burnet were greatly distorted by it. A month afterwards the magnificent vermilion-coloured spores were well sprinkled over the leaves, the form of which was unaltered. In the middle of July, the intensely black brand-spores made their appearance, many of which had twenty or more articulations, and were plentifully scattered over the leaves in tufts. Mr. Vize stated that he had not watched the transition state from the Uredo to brand-spores, but he hoped to do so if opportunity offered.

A Monograph on Bog Mosses is being presented to the Royal Microscopical Society by Dr. Braithwaite, F.L.S., and is being published in the "Monthly Microscopical Journal." The number of that journal for February contains the first description of the genera and species, beginning with the genus *Sphagnum*. In the preliminary remarks the author gives the following account of the division of the group. Thus, he says, that in the arrangement of the species Bridel adopts two sections, *Obtusifolia* and *Acutifolia*; and this plan is followed by Wilson in the "Bryologia Britannica." C. Müller, in his "Synopsis Muscorum," has *a.* with rounded leaves, *b.* with truncate leaves, and the latter is again divided into two, according as the peduncular leaves have or have not spiral fibres. Sullivant, in Asa Gray's "Botany of the Northern United States," arranges the species by the relative positions of the two kinds of cells, seen on cross section of a leaf, a character far too minute and difficult to be observed to be of practical utility. Prof. Schimper places all the species in two groups, *Monoicous* and *Dioicous*; also a most unpractical arrangement, as apart from the inconspicuous nature of the flowers, the species are so frequently found in a barren state, that such a mode of arranging them affords no help to the student. Lastly, Prof. Lindberg, in his paper which appeared in the "Öfversigt K. Vetenskaps Akad. Förhandlingar" for 1862, has, with a master's hand, distributed the *Sphagna* in natural groups, characterised essentially by the form of the branch leaves, and leaving nothing to be desired. After separating *S. macrophyllum* as the genus *Isocladus*, Prof. Lindberg divides the *Sphagna* into two sections; 1. *Homophylla*, having the stem leaves and branch leaves alike in form, and destitute of threads. *S. sericeum* and *S. Holleanum* from Java and Sumatra belong here. 2. *Heterophylla*, having the stem and branch leaves of different forms, and in this section four groups include the European species. In a letter recently received by Dr. Braithwaite, Prof. Lindberg alters the sequence of these, placing *S. cymbifolium* first, and this order is followed by the author in the paper referred to.

The Terms Endogen and Exogen.—At a meeting of the Royal Microscopical Society in January last, Professor T. Dyer dwelt upon the necessity of abandoning these terms in botanical language. He remarked that Professor Williamson's recent proposal to introduce them into palæontology was very objectionable. Our great English naturalist, John Ray, laid the foundations of a natural classification of flowering plants by dividing them into Dicotyledons and Monocotyledons. De Candolle thought these two groups might be characterised more conveniently by the mode of growth of their stem; he substituted, therefore, for Ray's names, those of exogens and endogens. It had, however, been shown that De Candolle's views involved an entire misconception of the mode of growth. By the researches more especially of Mohl, it had been proved that Monocotyledons were really not endogenous at all; they might, indeed, be more properly described as acrogenous. Then, again, exogenous growth was by no means confined to Dicotyledonous plants. Mr. Berkeley mentioned something very like it in a lichen (*Ipnea*). It was well known to occur in *Lessonia*, the great seaweed of the southern oceans, and, according to Ruprecht, also in the allied *Laminaria digitata* of our own shores. In *Dracæna*, an undoubted Monocotyledon, there were regular concentric zones of circumferential growth.

In fact, exogenous growth was found in vegetable organisms of very different affinities, and was not wholly characteristic of any one. It appeared to be a provision which was correlate with the general form of the whole mass of vegetative organs, and was forced upon the plant, in fact, as a necessary condition of its mechanical stability. For classificatory purposes the terms exogen and endogen were now almost universally abandoned, and Ray's designations, which were found quite valid, were used instead.

The Passifloraceæ and their Modes of Fertilisation have been pretty thoroughly explored by Dr. Maxwell T. Masters, F.R.S., who has published, in the "Transactions of the Linnæan Society," a very fine memoir on the natural history of the whole order. He has also printed private copies of the memoir, from which we have the opportunity of quoting. The memoir should be fully read by all who are interested in the subject; meanwhile, however, we may refer to the author's remarks on the subject of fertilisation of the flowers. It seems that in the young state the anthers are introrse and pressed up against the sides of the ovary and styles, the large stigmas of which project beyond the anthers. When the anthers become sufficiently matured to allow of the emission of the pollen, they undergo a change of position: the filaments spread more or less horizontally, and the anthers become extrorse; so that if the flower-stalk spread somewhat horizontally, as it does usually, or if it be erect, the pollen is likely to be shed on the corona, the styles at this stage being horizontal, with their stigmas quite out of reach of the pollen. In this manner the corona is often found dusted over with pollen; and in *Passiflora cincinnata*, Mast., and in other species it often happens that the stamens are bent downwards to such an extent as to come into direct contact with the corona. The outermost rows of the corona, then, appear to attract insects, the smaller threads proceeding from the throat of the flower-tube catch the pollen, while the membranous or median corona (operculum) shuts off the upper portion of the flower from the nectar-secreting portion at the base. In all cases the object seems to be to detain the insect in its passage to the nectar-secreting portions, and so to enable it the more surely to be dusted over with pollen. Now, when a bee visits an expanded flower, it is easy to see how the insect favours cross fertilisation. The insect alights on the rays of the corona; and if there be pollen on them, some of it must naturally adhere to the hairs on the insect's back. Moreover, if the insect be large, or the stamens, with their now extrorse anthers, be bent downwards, as they usually are at a late stage of the expansion of the flower, it is obvious that the back of the insect is very likely to come into contact (nay, does so, as he has frequently observed), and thus remove some of the pollen from them. In those cases where, from the pendulous position of the flower (*P. quadrangularis*, *P. macrocarpa*, &c.), the pollen cannot fall on the corona, which is now placed above the anthers, the pollen is removed by bees in the manner just indicated. When the pollen-carrying insects alight on the corona of another flower, it may so happen that the stigmas of that flower are so placed as to render them liable to come into contact with the insect, and to remove from its hairy thorax the pollen-grains with which it is bestrewn. The styles, which are erect all the time the anthers are introrse and so placed as to be liable to contaminate the stigmas, gradually assume a horizontal or even a deflexed position when the anthers are extrorse, or bent downwards, so as to render access of the pollen from them an improbable occurrence. All this may readily be seen by anyone who watches the operations of a humble-bee as he flies from flower to flower of any of our cultivated passion-flowers; but it would seem probable, from the length of the gynophore, that in their native haunts the flowers of Tacsonias, for instance, are visited by some larger creatures than bees. Indeed, some travellers state that the honied flowers of the Tacsonias are very attractive to humming-birds; and these elegant little creatures probably act as the carriers of pollen from one flower to another.

A New British Coal Fossil was described to the Manchester Philosophical Society, January 9, 1872, by Mr. E. W. Binney, F.R.S. It resembles somewhat the *Psaronius Zeidleri* found in the Upper Foot Coal Seam, near Oldham. This species has been described by Corda, in his "Beiträge zur Flora der Vorwelt," and figured in Plate XI., but has not hitherto, Mr. Binney believes, been met with in the British coal-fields. The Oldham specimen appeared to him to be a petiole, of about one-eighth of an inch in diameter, and is of a nearly circular form in its transverse section, two-thirds of it consisting of a zone of strong parenchymatous tissue and an internal axis of vascular tissue arranged in four radiating arms of an irregular oval form, resembling a St. Peter's cross. As he could not connect the specimen with a stem of *Psaronius*, he proposed to call it *Stauropteris Oldhamia.* In the above-named coal, as well as that of the Lower Brooksbottom Seam, there is a great variety of beautiful petioles which have not yet been described. Some of them evidently belong to the genus *Zygopteris*, and may probably be discovered in connection with their stems, but most of them have been found detached, and sometimes mistaken for the rootlets of *Stigmaria.* From some specimens in his cabinet he is led to believe that Cotta's *Medullosa elegans* is merely the rachis of a fern or a plant allied to one.

The Plants of Oregon, America.—We learn from Silliman's "American Journal," February 1872, that Mr. Elihu Hall, well known as an excellent and enterprising collector, during the past season made an extensive collection of dried plants in Oregon, which are to be distributed in sets as soon as the materials can be put in order. The full sets will contain five or six hundred species, and Mr. Hall offers them to subscribers at eight dollars per hundred specimens. So far as the examination has gone, a good number of rare and interesting, and some wholly new species, are brought to light. Plants of this region being far from common in herbaria generally, it is thought that these sets will at once be taken up. As Mr. Hall is likely to be very soon engaged in another exploration, intending subscribers may address Mr. Charles Wright, Harvard University Herbarium, Cambridge, Mass.

Structure of the Pistil in Primulaceæ.—In the "Annales des Sciences" of last year is a valuable paper on this subject by M. Van Tieghem. Modifying some of his formerly expressed views, he says that he now finds that the bundles in the placenta of *Primula* and other plants of the same order (as likewise in *Caryophyllaceæ*, &c.) present their liber-cells inward, their spiral vessels outward, contrary to the manner of vascular bundles of stems.

These bundles originate in connection with those which pass into the ovarian walls, and which correspond to the margins of the five carpels; passing into the placenta they ramify, and the ovules are borne at their terminations. He then examined the outer floral whorls in cases where their parts produce an accessory piece or appendage on the inner face or at its base, such as the petals of *Ranunculus*, of Oleander, the crown of the perianth of *Narcissus*, the stamens of *Cobæa*, &c. In all these the vascular bundles of the internal appendage or crown equally have their liber-cells facing toward the axis of the flower, their spiral vessels turned from it. Van Tieghem therefore concludes that, as these accessory pieces are appendages or deduplications of the petals, stamens, &c., so the free central or basilar placenta consists of internal appendages or deduplications of the carpels; also that each ovule answers to the lobe of leaf.

Whence proceed the Flowers of the Larch?—Mr. Thomas Mechan, who has a paper on this subject in the "Proceedings of the Academy of Philadelphia," says that at the flowering time of the larch the male and female flowers proceed from the termination of the spurs—not merely "of the preceding year," according to Gray's "Manual," but in some cases of many preceding years, "the sterile from leafless buds, the fertile mostly with leaves below" (Gray's "Manual," 5th ed., p. 472). Why have the female flowers leaves under them, and the male none? Comparing the male and the female catkins, we see why. The scales of the male are formed out of the leaves which become fully formed in the female one. The pair of anther cells are thus simply on the back of a transformed leaf, just as we find the spore-cases of ferns borne in the same way. The weaker organisation which he has shown in his paper and communications on sex, permits no further development here. But in the case of the female flower, the leaf maintains a separate organisation all through the catkin or cone; and, as shown in his paper on the "Stipules of Magnolia," the midrib of the leaf shortens, and, assuming a stipular character, increases in width, until we have the purple bractea so well known in Larix. As soon as these bractea have been arrested in their development, the carpellary scales, which answer to the phylloidal fascicles of *Pinus*, commence their growth in most species of larch, finally equalling the bracts in length.

The Various Species of Yeast which attack Fruits.—These have been investigated by M. le Dr. Engel, who has written a short memoir on them, that has been presented to the French Academy ("Comptes Rendus," February 12, 1872) by M. Pasteur. He formulates his conclusions thus. (1) He has found a rapid, easy, and certain way of making alcoholic ferments fructify. He has found by this means two very distinct species. (2) He has examined about twenty different species of fruits in order to study the ferments. Generally they (ferments) are found on the surface, and in this state they remain inactive. However, when any part of the skin of the fruit is broken, they make their way, and then they become fully developed, and undergo their development fully. (3) An examination of this kind has proved to him that the fermentation of bread is totally different from that of beer. (4) He has never been able to cause the fungus to germinate when the fruit contains little or no sugar, but in those which contain sugar it germinates rapidly. (5) The two genera to which he referred above are *Saccharomycetes*, which has no less than seven species, and one which he calls *Carpozyma*, which forms but a single species that is met with on nearly all fruits, and is named *C. apiculatum.*

Dr. Braithwaite on the British Mosses.—"Science Gossip" for March contains two very excellent papers by Dr. Braithwaite on the structure of the British mosses. They are accompanied by illustrations, and are admirably adapted to those who do not understand the subject. The author has avoided scientific expressions as much as possible; and has given advice as to the collecting, examining, preserving, and classifying the various species which the collector may have gathered in his country walks. Further, Mr. Hardwick (the publisher) has arranged to supply a mounted specimen illustrative of Dr. Braithwaite's paper and of the list of British mosses now preparing, for the small sum of sixpence.

CHEMISTRY.

Peculiar Production of Ozone.—Professor H. Croft gives, in the "Chemical News," February 23, an account of an anomalous production of this compound which he has experienced. He states, that about six years ago, when evaporating some syrupy iodic acid, prepared according to Millon's process, over sulphuric acid, he noticed that when the acid began to crystallise, the air in the jar (covering the drying-dish) had a strong smell of ozone, or active oxygen. A couple of years afterwards, on again making iodic acid, this observation recurred to his mind, and he carefully tested the air in the jar during the evaporation; no trace of ozone could be detected until the acid began to crystallise, when the smell of ozone became immediately perceptible, and all the usual tests for that body succeeded perfectly. During the last month he has had occasion to convert two ounces of iodine into iodic acid, and exactly the same result has been observed.

Action of Nitric Acid on Charcoal; a New Compound.—Mr. Andrew Scott states, in the "Chemical News," February 16, that having spent a part of the last few months in investigating the action of nitric acid on charcoal from various sources, he has obtained a compound containing over 30 per cent. of carbon, 2 to 3 per cent. of hydrogen, the remainder being chiefly nitrogen. It is a black amorphous substance, very soluble in water, alcohol, ether, &c. It is very deliquescent, absorbing 20 per cent. of its weight of water in a few days, and becoming a dark liquid, the water being again expelled in a short time at the temperature of the water-bath. When heated on platinum foil it takes fire, the combustion spreading rapidly through the mass. Heated in a test-tube it melts, swells considerably, and gives off nitrous fumes. It combines with alkalies, and the solutions give precipitates with most of the metallic salts. Its watery solution is also precipitated by hydrochloric and sulphuric acids. He has prepared this body from willow charcoal, coke from crude paraffin oil, bone charcoal, and from mineral charcoal found in ordinary household coal.

Electrization of Sulphide of Carbon.—In the "Comptes Rendus," January 15, M. Sidot states, that when pure sulphide of carbon is placed along with granulated silver or any other granulated metal in a stout glass bottle, and this vessel vigorously shaken, electric sparks are seen inside the bottle; when, while this phenomenon is observed, water is poured on the bottle, the appearance of the sparks ceases immediately, but the phenomenon is observed again when the shaking is continued. By being exposed for several months to strong sunlight in a sealed tube, pure sulphide of carbon appears to become decomposed, giving rise to a solid, flocculent, red-coloured matter and a peculiar kind of gas, but the author has not yet been able to test the nature of these products.

Tests for Detecting Strychnia.—Dr. Filhol has a paper on this subject in the "Journal de Pharmacie et de Chimie" for January 1872. After having reviewed the various tests described in works of chemistry, the author concludes that as regards the sure detection of this alkaloid in cases of poisoning it should be obtained in a solid state; the alkalinity of its solution should be ascertained as well as its intensely bitter taste; its behaviour with chlorine, and its blue colouration under the influence of sulphuric acid and oxidising substances, should also be seen; while, lastly, as a very delicate reaction, the author quotes that, with chloride of gold, strychnia (in solution) yields immediately a crystalline precipitate, which, although slowly, is yet formed in solutions containing 1-10th of a milligrm. of the alkaloid. This precipitate, and that formed by chlorine, are at once dissolved by concentrated sulphuric acid, and, chromic acid being added, the well-known blue colouration that strychnia yields with this last reagent is produced. The presence of alcohol in liquids to be tested for strychnia should be avoided.

Paralbumen in Serous Exudations.—In the "Chemical News," March 1, 1872, Dr. Hilger states, that by paralbumen he means a modification of serum-albumen, the alcoholic precipitate of which is soluble in water, and only partially coagulated by the addition of small quantities of acetic acid; this paralbumen has been hitherto only found in the hydropsical ovarian cysts, but the author has recently found it also in the fluid abnormally secreted in ascites.

A Prize for best Essay on Sugar-making is offered by the East Bohemian Association of Sugar Manufacturers. It is 50*l.* in value. The work must consist of two sections. The first section, chemico-technical, to contain—(*a*) Well-made and complete researches and analysis of all the raw materials used in the process of sugar manufacture; (*b*) complete analysis of all the products obtained in this manufacture; (*c*) a succinct and explanatory description of the chemico-technical operations required in the sugar manufacturing process; (*d*) a complete collection of all the tabulated forms required to assist the making of calculations for the various manipulations; (*e*) a brief, yet complete, description of the sugar manufacturing process, beginning with the washing of the beet-roots and ending with the refined produce. The second section, mechanico-technical, should treat, in full details, on the plant and machinery required, on the management of the operations, and on the means to be employed for keeping the plant and machinery in good repair. Competitors should send their essays to Herrn Fr. B. Goller, Manager of Sugar Works, at Podebrad, in Bohemia. The essays ought not to contain the names of the authors, which should be sent in a sealed note, bearing on the envelope a motto, also to be placed on the fly-leaf of the essay. The jury to judge on these essays is to consist of a mixed scientifico-technical committee of six members.—*Dingler's Polytech. Jour.*, January, 1872.

Preparation of Pepsin.—This substance is now so much used in medicine that any improved mode of preparing it is of importance. Herr E. Scheffer thinks he has hit on some improvements in the following mode:—The mucous membrane of a previously well-cleaned hog's stomach is dissected off, chopped finely, and macerated in water acidulated with hydrochloric acid for several days, during which time the mass is frequently well stirred; the resulting fluid is strained, and, if not clear, set aside for twenty-four hours, in order to allow the mucus to settle. To the clarified liquid a thoroughly saturated solution of chloride of sodium is added, and the whole thoroughly mixed. The pepsin, separated from the rest of the solution by the addition of the saline solution, is found floating on the surface of the fluid, and is removed with a spoon, put upon cotton cloth to drain, and finally submitted to strong pressure, to free it as much as possible from the salt solution.

Work done in 1871.—A chemical retrospect for 1871 has been issued by Dr. Kolbe, in the "Journal für praktische Chemie" [No. 20, 1871]. Of this the "Chemical News" speaks very highly. It says that the author shows in this paper that he is not only thoroughly well acquainted with science, but also with the conditions existing in different countries; his allusion to the United Kingdom, while courteous and complimentary towards its men of science, contains a very true and correct view of the disdainful contempt with which science in general, and chemistry especially, is treated in certain quarters.

A New Edition of Gmelin's Handbook.—All chemists will heartily welcome a new edition of Gmelin's indispensable Handbook, the inorganic portion of which has long been behind the science of the day, though still often consulted and never superseded. Already there are issued the first, second, third, and fourth Lieferungen of the second division of the first volume, and the first and second Lieferungen of the third volume. The entire work is under the charge of Professors Kraut, with Professors Naumann and Ritter and Dr. Jörgensen as collaborators, to ensure its rapid completion. The favourite old treatise appears with a larger and clearer page and better type, and apparently, with no abatement of its former thorough and conscientious exhaustion of every topic treated.

A Machine for estimating the Starch in Potatoes is described by Dr. A. Schwartzer, in "Dingler's Journal" for January. It is thus briefly noticed in the "Chemical News." The paper, it says, treats on a subject which is not much attended to in this country— to wit, the good quality of potatoes as ascertained by their specific gravity—it being a well-known fact that the more starch (the main constituent of the tuber) potatoes contain the higher their specific gravity. The author describes at length a contrivance (weighing machine of simple construction) with the aid of which samples of potatoes (quantities of at least 5 kilos. together) can be conveniently tested for their specific gravity.

The Devitrification of Glass.—The January number of "Dingler" contains a paper on this subject by a practical glass manufacturer, Dr. H. E. Benrath.

After first briefly referring to the observations of Réaumur (150 years ago) and to the later researches made on the devitrification of glass by Dumas, Pelouze, Terreil, and others, he gives a detailed account of his experiments and analysis of different kinds of glass before and after the devitrification. He concludes this very exhaustive essay by stating that, in his opinion, the behaviour of glass before and after devitrification is, that the silica in glass is not so much in the condition of three- or four-fold combination as in that of solution in glass (perhaps $RO,2SiO_2$), and then, as is the analogous case with all solutions to different temperatures, correspond different maxima of dissolved substance.

Does Dilution cause a Precipitate from a Nitric Acid Solution of Gold?—This question has been long answered affirmatively, but Mr. A. H. Allen denies it. He states that [" Chemical News," February 23] he prepared some pure auric oxide by precipitating a solution of gold in aqua regia with considerable excess of magnesia, boiling, washing with hot water till the washings were perfectly free from chloride, dissolving the precipitate in dilute nitric acid, boiling and washing till the water no longer reddened litmus. The auric oxide so obtained was heated with strong nitric acid, in which it is well known to be soluble, all authorities stating that dilution causes the complete precipitation of the gold as auric oxide. This he does not find to be the case, a perfectly clear and nearly colourless solution being obtained on dilution, in which the gold must evidently be present as nitrate, as argentic nitrate gives no trace of precipitate, and no other salt radical is present. The solution of auric nitrate answers to the ordinary tests for gold, and is but gradually decomposed, with deposition of auric oxide having the same properties as before. The decomposition is more rapid when the liquid is heated.

A New Dye Stuff.—In the "American Journal of Pharmacy" [February 1872], Dr. J. Merrick speaks of flavine, also sold under the name of aurantine, and which, according to reliable information, is a preparation of quercitron bark, well known in Europe, and manufactured in the United States by a process kept secret. According to the researches made by Drs. Bolley, Brunner, and König, flavine is sometimes nearly pure quercitrine, sometimes quercetine, and usually a mixture of the two. By dissolving the dye-material contained in quercitron bark in an alkaline solution, and next treating it with sulphuric acid, Hochstætter and Oehler have obtained a flavine-like substance.

Chondrine in the Tunicata.—According to Dr. Schäfer's researches, these low mollusks contain in their tissues a substance which in its properties and percentage of nitrogen corresponds closely to *chondrine*, which is essentially a vertebrate type of substance. This is another fact in favour of those who bring the vertebrates from the Tunicate class.

Artificial Coniine.—Some further researches have been made on this subject. In the second number of the Proceedings of the Chemical Society of Berlin, Herr Schiff commences a paper with the detailed description of the method of preparation of coniine by artificial means, starting with butyraldehyde exposed to direct sunlight for several months along with alcoholic ammonia solution kept in a well-stoppered bottle. The artificially-obtained alkaloid is, as regards its physiological action, akin to the natural coniine obtained from *Conium maculatum* and *Cicuta virosa*. The boiling point is from 168° to 170°, and also that of the native; the specific gravity varies from 0·893 to 0·899 at 15°. The main difference, as regards the properties of the native and artificially-prepared coniine, is that the latter does not exhibit any optical rotatory action. See also "Chemical News."

The Tungsten Compounds have been very thoroughly investigated by Professor Roscoe, F.R.S., who lately [February 15, 1872] read a paper on the subject before the Chemical Society. The paper is of some length, and should be consulted by those who are interested in the subject.

GEOLOGY AND PALÆONTOLOGY.

Crystals of Calcite surrounding Stalactitic Bitumen.—At a recent meeting of the Literary and Philosophical Society of Manchester [Feb. 6, 1872], Mr. Boyd Dawkins called attention to a remarkable group of crystals of calcite and sulphide of iron surrounding stalactitic bitumen, found at Castleton in Derbyshire, by Rooke Pennington, Esq. The mode of formation was this. When the mountain limestone of that district became charged with bitumen, the latter penetrated into a cavity which it traversed in long stalactite drops. Subsequently the cavity was more or less filled with crystals of calcite and sulphide of iron, which were deposited by the water charged with those substances around the drops of bitumen. The heat by which the bitumen found its way into the rocks must have disappeared before the crystals were formed; for had the latter been the result of hydrothermal action, they may have been coated, but certainly could not have been traversed by the solid bituminous stalactites.

On the Value of Lithology in determining the Age of Rocks.—Clearly Professor Dana is not so much in favour of lithological evidence as Dr. Henry Hunt; for, in a paper in Silliman's "American Journal" for February 1872, he says, in regard to Dr. Henry Hunt, that he has relied, for his chronological arrangement of the crystalline rocks of New England and elsewhere, largely on lithological evidence, and commends this style of evidence, when such evidence means nothing until tested by thorough stratigraphical investigation. This evidence means something, or probably so, with respect to Laurentian rocks; but it did not until the age of the rocks, in their relation to others, was first stratigraphically ascertained. It may turn out to be worth something as regards later rocks when the facts have been carefully tested by stratigraphy. A *fossil* is proved, by careful observation, to be restricted to the rocks of a certain period before it is used—and then cautiously—for identifying equivalent beds. Has anyone *proved* by careful observation that crystals of staurolite, cyanite, or andalusite, are restricted to rocks of a certain geological period? Assumptions and opinions, however strongly emphasised, are not proofs. It is no objection to stratigraphical evidence that it is difficult to obtain; is very doubtful on account of the difficulties; may take scores of years in New England to reach any safe conclusions. It must be obtained, whatever labour and care it costs, before the real order and relations of the rocks can be known. Until then, lithology may give us guesses, but nothing more substantial.

Occurrence of the "Primordial Fauna" in Nevada.—Mr. J. D. Whitney has had placed in his hands specimens of fossils from the above locality by Professor Joseph Le Conte, who obtained them from Mr. J. E. Clayton. (See Silliman's "American Journal," February 1872.) These fossils indicate most unequivocally the Potsdam period of the Silurian age, since they belong to those most characteristic families, the *Lingulidæ* and the *Paradoxidæ*. The specimens contain a great number of individuals, but few species; and these much broken and closely compacted together. Among the fragments are several heads of *Agraulos* (*Arionellus* and *Arion* of Barrande, *Crepicephalus* of D. D. Owen), and the species seems to be the same *A. Oweni* which has been described by Meek and Hayden as occurring in the Big Horn Mountains. This is by far the most abundant species in the specimens obtained by Mr. Clayton; but there are also other fragments, one of which seems almost certainly to be a *Conocoryphe* (*Conocephalites* of Barrande), and others to belong to the genus *Paradoxides*. The class of brachiopods is represented in these specimens by two or more genera, all of the family of *Lingulidæ*. Among these he thinks that he is able to recognise the genera *Lingulepis* (Lingula) and *Obolella*. At all events, the character of this assemblage of fossils is thoroughly and peculiarly Primordial. This is an interesting discovery, since it carries the Primordial fauna much farther west than it had been found before. The most western locality of Potsdam sandstone fossils previously described, is that in the Big Horn Mountains, at the head of Powder River, in longitude 107°, while that discovered by Mr. Clayton is near the 116th meridian. It is indeed wonderful to see the persistence of this grouping of forms which characterises the lowest subdivision of the fossiliferous series, and which has been found recurring at so many points over the vast area of the American continent, as well as on the other side of the Atlantic.

Geological Survey of England.—The new edition of Sheet 7 of the Geological Survey Map of England, showing all the drift deposits on the scale of one inch to one mile, has just been published. It is a considerable improvement on the old map. A glance at the old edition, says the "Geological Magazine" for March 1872, comparing it with this new one, shows at once the great utility of mapping all the superficial deposits, for not only have they a high economic value, but in their relations to drainage and health—an inquiry which we are glad to observe is attracting public attention—their importance cannot be over-estimated. The area comprised in this sheet, about 800 square miles, includes the western part of London. It is bounded on the east by Hornsey and Enfield; on the north it takes in Hatfield Park, St. Albans, Hemel Hempstead, Great Berkhampstead and Wendover; on the west it extends beyond Princes Risborough, Great Marlow, and Twyford in Berkshire; while to the south it includes Windsor and the Great Park, Staines, Twickenham, Wimbledon Park, and Dulwich.

Mineral Veins have their Origin in Solfataras.—This is the view taken by Mr. J. Arthur Phillips in a paper published in the "Philosophical Magazine" [Dec. 1871]. The most interesting of the Solfataras, or fissures giving off steam, are those known as the Steamboat Springs in the State of Nevada, where some of the crevices are over 1000 yards in length, and are often entirely filled with boiling water containing various mineral salts in solution. In the course of time incrustations (sometimes to the thickness of several feet) are formed on each side of the fissures, composed chiefly of hydrated silver, but containing also oxides of iron and manganese, traces of copper, minute crystals of iron pyrites, &c. The author thinks that these phenomena tend to show that the Theory of Ascension, which teaches that veins are the result of deposits of mineral substances which have been introduced into fissures from below, is the most rational method in which to view this formation. For further corroboration he gives analyses of water issuing from lodes in some of the deeper Cornish mines which were found to hold mineral substances in solution.

The Kiltorkan Fossils, co. Kilkenny, Ireland, have recently been explored by Professor Oswald Heer, who read a paper before the Geological Society [Jan. 10, 1872], On *Cyclostigma, Lepidodendron,* and *Knorria* from Kiltorkan. In this paper the author indicates the characters of certain fossils from the Yellow Sandstone from the South of Ireland, referred by him to the above genera, and mentioned in his paper "On the Carboniferous Flora of Bear Island," read before the Society on Nov. 9, 1870 (see Q. J. G. S., vol. xxvii. p. 1). He distinguished as species *Cyclostigma Kiltorkense,* Haughton, *C. minutum,* Haught., *Knorria acicularis,* Göpp. var. *Bailyana,* and *Lepidodendron Veltheimianum,* Sternb. In the discussion on the paper Mr. Carruthers differed from Professor Heer as to his method of arriving at conclusions upon the fossils.

A Geological Map of London is, we are informed, shortly to be published by the Geological Society. This will be a great boon to our metropolitan workers, and we trust it will soon be out, and that it will be well done.

An Earthquake at Malaga is recorded in a letter which the Minister for Foreign Affairs laid before the French Academy at its sitting on February 26, 1872. The shock occurred on January 28, at 3h. 1m. in the afternoon. The undulating movement lasted from four to six seconds; there were subterranean sounds like thunder some moments before. The shock took place from north to south. The weather was unusually cold, the birds were visibly affected, and uttered peculiar trembling notes. No mischief is recorded as having taken place.

MECHANICAL SCIENCE.

Diagrammagraphe.—In investigating the proportions to be given to slide-valves and gear, it is usual to employ various geometrical constructions, by which the action of the valve in opening and closing the steam ports is represented. In the case of valves worked by link motions, these diagrams, though extremely valuable, are in general only approximate representations of the action of the valve; that is, in order to simplify them the influence of the obliquity of the eccentric rods and of the connecting rod is ignored. M. Pichault has invented a beautiful instrument, which he calls a diagrammagraphe, by which any given arrangement of link motion can be temporarily put together, and a pencil moved in precisely the same manner as the actual valve would be moved with the proposed link motion. This pencil records on a revolving plate the motion of the valve, and from the diagram so obtained the action of the valve can be studied, and any improvements in the arrangement of the gear can be at once imitated and tried. The instrument has been described and illustrated in "Engineering."

Navigable Balloon.—The trials of M. Dupuy de Lôme's balloon at Paris have been of great interest, and appear to have been attended with an amount of success which is encouraging. The balloon is of an elongated form, so that it has a horizontal axis of least resistance, which is maintained parallel to the propelling force. The car has a huge rudder, consisting of a triangular sail of an area of 161 sq. feet. In order that this rudder may act, it is essential that the balloon should not drift with the wind, but should be propelled at a sensible velocity relatively to it. To effect this a large screw propeller of two blades is employed, 30 feet diameter, and 26 feet pitch. This screw is rotated by four or eight men. According to M. Dupuy de Lôme's calculations, the resistance of the balloon at five miles an hour is 24 lbs., and at that speed the propeller should revolve twenty-one times per minute, four men having power sufficient to work the screw at that speed. The balloon, inflated with hydrogen, has a gross ascensional force of 3½ to 3¾ tons. The total weight of the balloon and apparatus is 1¾ tons, and of the crew, stores, &c. 1½ tons. In the experimental trial it was found that, with the screw working, the balloon did obey the rudder. It was found possible to direct the balloon at an angle of 12° with the wind's direction.

Flow of Elastic Fluids.—A valuable series of experimental researches on the efflux of air from orifices and its flow through tubes has been carried out by Mr. G. Wilson, and the results are being published in "Engineering."

Steel Gun.—Mr. Vavasseur, of the London Ordnance Works, is constructing a 12-inch built up steel gun, intended to prove the advantages of that system of construction by competing with any other gun of the same calibre. The rifling is a modification of the Lynal Thomas system, the gun being ribbed and the shot grooved. The gun is intended for a 110 lbs. charge of powder with 600 lbs. shot.

Binary Vapour Engines.—Some of our readers may remember that between 1850-60, M. Prospere Vincent du Tremblay constructed some engines, in which steam, in condensing, vaporised ether, the ether vapour being used to work a secondary engine. The object of the arrangement was to economise fuel, and the system of binary vapour engines was worked out by M. Tremblay with great ingenuity, knowledge and thoroughness. The engines constructed on this plan were fairly successful, and realised fair results as to economy of fuel, considering the date of the experiments. But the system was ultimately abandoned, in consequence of some difficulties with the joints, and from the danger of conflagration if there were any leakage of ether. Mr. J. H. Ellis, of Boston, has recently revived the system of binary vapour engines, using vapour of bisulphide of carbon in the secondary engine. The exhaust steam is led into a "vaporiser" constructed somewhat like a surface condenser, except that instead of being surrounded by cooling water the tubes are immersed in liquid bisulphide of carbon. The exhaust steam parts with its heat to this liquid vapourising part of it at a pressure of 40 to 60 lbs. per sq. inch. The vapour thus generated drives a second engine, and is then led into a coil of piping which is wetted by a shower of water and cooled by an air blast. It yet remains to be seen whether any economy can be attained by this plan sufficient to counterbalance the disadvantage of complication in the engine and the danger in using so inflammable a compound as bisulphide of carbon.

Flexure of Iron and Steel.—M. Tresca has made some new experiments on the flexure of iron and steel railway bars, extending the experiments beyond the ordinarily assumed limit of elasticity. The loads were imposed by an hydraulic press, and the deflection at three points measured with great accuracy. Within the limit of elasticity, these experiments confirm the theoretical laws as to the proportionality of the deflections to the loads, and to show them to be inversely as the cube of the span. Beyond the stress ordinarily assumed as marking the limit of perfect elasticity, they show that the imposition of more and more energetic loads causes the limit of elasticity to be extended, so that by repeatedly loading the bar it may approach the point of rupture. The bar takes increasing permanent curvature, but its temporary deflections remain proportional to the loads. The original co-efficient of elasticity diminishes to an extent which may amount to one-tenth part.

MEDICAL SCIENCE.

Determination of the Mode of Flight in Birds.—M. Marey, who has investigated this subject very extensively, adds to his researches a note in the "Comptes Rendus" [February 26th, 1872]. He has already shown that the wing of the bird describes an ellipse. He states that the graphic traces which have enabled him to come to this conclusion have also allowed him to determine the height of the wing at each moment, consequently he has been able to determine the velocity with which either the elevation or depression of the wing is affected. But he says that to determine the work done by the bird, to ascertain the resistance which is opposed by the air each moment to the movements of elevation or depression of the wing, it is necessary also to know the angle at which this organ strikes the air. The resistance, it seems, varies enormously in the case of a bird's wing, according as the motion takes place parallel or perpendicular to its plane. This question is then, with the aid of diagrams, pursued by the author. We believe, too, that a very interesting paper on this subject has been published by Dr. J. Bell Pettigrew. We have not seen the paper itself, but the "Edinburgh Medical Journal" for February contains an extremely favourable notice of it.

How the Nerves end.—Dr. Lionel Beale, F.R.S., has had a paper on this subject in the "Monthly Microscopical Journal" for January, 1872, in which he endeavours to forward his opinion of the termination and distribution of nerve-fibre. This was *apropos* of a controversy which has taken place between him and Dr. Klein, who we believe has a paper on the subject of nerves, which will appear in the "Monthly Microscopical Journal" for April. Dr. Beale states that Dr. Klein showed him some drawings of the termination of the nerve-fibres, but he adds, "His specimens were prepared by the gold process, and give a different idea of the distribution of the nerves to that afforded by mine, some of which are ten years old. My own conclusions on the ultimate distribution of nerve-fibres were formed several years ago, at a time when terminal nerve networks were denied in Germany, and when it was supposed that only in a few exceptional cases did the axis cylinder of a nerve extend beyond the white substance. Not only are my networks of pale nucleated nerve-fibres now accepted, but it is maintained that much finer networks of nerve-fibres ramifying upon and amongst epithelial cells and other elementary parts, and even upon an individual mass of bioplasm (nucleus), have been demonstrated. At present, however, I cannot regard the observations upon which it is thought to establish this view more conclusive than those which a few years since led many to the conclusion that the axis cylinder sprang from the nucleus or nucleolus of the central nerve-cell."

Xylol, the new Remedy for Small-pox.—The "Chemical News" for February 16 contains an interesting note on this subject. It seems that the Berlin "Klinische Wochenschriff" states that Dr. Zuelzer, Senior Physician at the Charité Hospital, had there administered xylol in cases of small-pox, with the most complete success. It is given in doses of from 3 to 5 drops for children, 10 to 15 drops for adults, every hour to every three hours. It is harmless, because as much as a teaspoonful at a time has been taken. The most convenient form of taking it is in capsules, as already supplied by a Berlin firm, and containing 3, 5, 8, and 12 drops each. The specific action is not yet clearly defined, but early information on this point is promised. The theory at present is that xylol is taken up by the blood, and acts as a disinfectant. The absolute purity of the xylol is important, as toluol and other analogous compounds do not possess this peculiar action, and it seems there are some practical difficulties in obtaining xylol absolutely pure.

Antagonism between Physostigma and Atropia.—A very valuable paper on this important therapeutical subject has been presented to the Royal Society of Edinburgh by Dr. Fraser, who we may regard as the first scientific British therapeutist. We regret that we can do no more than refer to the paper, because the "abstract" contains a plate without which the substance of the paper would be unintelligible.

Fatal Dose of Chloral Hydrate.—Dr. B. W. Richardson, F.R.S., has had reprinted from the British Association Reports for 1871 his own special report on the physiological action of organic chemical compounds. This is a most valuable and, we need not say, most interesting paper. It deals with several substances, and first of all comes chloral hydrate. He has endeavoured to ascertain what is a dangerous and what a fatal dose of chloral hydrate. The conclusion at which he has been able first to arrive on this point is, that the maximum quantity of the hydrate that can be borne at one dose bears some proportion to the weight of the animal subjected to its influence. The rule, however, does not extend equally to animals of any and every class. The proportion is practically the same in the same classes, but there is no actual universality of rule. A mouse weighing from three-quarters of an ounce to an ounce will be put to sleep by one quarter of a grain of the hydrate, and will be killed by a grain. A pigeon weighing twelve ounces will be put to sleep by two grains of the hydrate, and will be killed by five grains. A guinea-pig weighing sixteen ounces will be put by two grains into deep sleep, and by five grains into fatal sleep. A rabbit weighing eighty-eight ounces will be thrown by thirty grains into deep sleep, and by sixty grains into fatal sleep. The human subject, weighing from one hundred and twenty to one hundred and forty pounds, will be made by ninety grains to pass into deep sleep, and by one hundred and forty grains into a sleep that will be dangerous; finally, he concludes that a dose of 180 grains is a fatal dose.

Accommodation of the Eye.—Dr. W. E. Dudgeon is one of the last observers who has given us his views on this subject. His opinion does not appear very well supported, but it is briefly as follows:—1. There is no increase in convexity of the surfaces of the crystalline lens. 2. That the required shortening of the focus of the crystalline is effected by a slight movement of rotation of the crystalline from without inwards. 3. That the contraction of the pupil during this act corrects any tendency to the formation of a blurred image on the retina by cutting off the more obliquely impinging, and, therefore, superfluous rays of light. He rather fails in determining the cause of the motion. Still the paper is interesting, and those who consult the Homœopathic Journals, in one of which it appeared, will do well to consult it.

Dinners at Pompeii.—The Pompeian dinners, according to the "Food Journal" for January, usually comprised three courses. The first consisted of eggs, olives, oysters, salad, pickles, &c.; the second of made dishes, fish, and roasts; the third of pastry, confectionery, and fruits. From a painting discovered at Pompeii we have the representation of a large feast in those days. An immense dish containing four peacocks stood in the centre of the table, surrounded by lobsters, one holding a blue egg in its claws, another a stuffed rat, another an oyster, and the fourth a basketful of grasshoppers. At the bottom of the table were four dishes of fish, and above them partridges, hares, and squirrels, each holding its head between its paws. This was all encircled by a sort of German sausage, apparently; and then came a row of yolks of eggs; a row of peaches, melons, and cherries; and lastly, a row of vegetables of different sorts.

The Intestinal Movements.—In one of the Vienna Medical Journals for last year, Drs. Mayer and Basch are led to conclude, after a long series of experiments, that the cause by which the intestinal movements are produced is primarily the presence (especially the afflux) of the venous blood in the intestinal walls. The previously-recognised arrestive action exercised by the nervi-splanchnici upon the intestinal movements may be explained on the same principle by the vaso-motor fibres which they contain.

How Casein is formed in Lacteal Glands.—It is stated in a recent number of Pflüger's "Archiv" that Dr. Daenhardt succeeded in extracting, from the lacteal glands of suckling guinea-pigs, a body which, by the addition of a mixture of the white of egg and carbonate of soda, was capable of forming casein. The doctor deduces from this that, as by a ferment of the saliva sugar is produced from starch, so by this ferment of the lacteal glands casein is produced from the albumen.

A peculiar Aspirator, which is specially recommended for thoracentesis, has been invented by Dr. Vald. Rasmussen, of Cophenhagen. The distinctive character of the instrument depends on the substitution of a two-waterway stop-cock for the two separate and single cocks in Dieulafoy's apparatus; but, more especially, on the insertion of a vent-piece of peculiar mechanism, in the stead of either Dieulafoy's two ordinary stop-cocks, or the two-waterway stop-cock. The most important advantages claimed by Dr. Rasmussen for his instrument are briefly the following:—1. The operation with it is perfectly free from danger. 2. With it the fluid can be completely drawn off, or nearly so, if desired, without the entrance of any air into the pleural cavity. 3. It is possible to remove even the smallest collections of fluid, whether such exist free in the pleura, or are encysted. 4. The fluid can be drawn off slowly or quickly, at pleasure. 5. Relapses are far less frequent than after the performance of the ordinary operation of thoracentesis. 6. The instrument is so small that it does not frighten the patient, and the method of using it is so simple that the operator can almost dispense with any assistance.

METALLURGY, MINERALOGY, AND MINING.

Metallurgy in Ancient India.—In Mr. Forbes's, F.R.S., report on the progress of the iron and steel industries (February 1872), we have given, as a proof of the antiquity of iron smelting in India, and also of the large forgings in wrought iron which could be executed by a people who now appear to have entirely lost the art, the fact that Mr. Mallet has directed attention to a wrought iron pillar situated at the Mosque of the Kutub, near Delhi, which must be more than one thousand, and may be as old as fifteen hundred years, yet is as large as the screw-shaft of some of our largest steamships; that part of the column above the present level of the soil being 48 feet high, with a diameter of 16·4 inches at the base, and 12 inches at the top immediately below the elaborately chiselled capital. It is calculated to contain about 80 cubic feet of iron, and to weigh not less than seventeen tons.

Steel in New Zealand.—It seems [the "Times"] that the iron-sand, as taken from the beach, is mixed with an equal quantity of clay and of the ordinary sea-sand, which contains a large admixture of shell; these materials are worked up into bricks, which are hardened in a kiln, broken up into irregular pieces, and smelted in an ordinary cupola furnace. The product of this simple process is cast steel of the finest possible texture, from which some beautiful specimens of the finest cutlery have been manufactured. These experiments were conducted by a mechanic in the government employ, who was restricted to an expenditure of 100*l.*, and was, therefore, only able to erect a furnace of the most temporary description; he, however, succeeded in producing, at the first and only trial, 5 cwt. of steel in the manner described above, and his success seems likely to lead to further and more extensive efforts to utilise the almost inexhaustible deposits of this ore which exist at Taranaki and elsewhere.

Separation of Phosphorus in Puddling.—According to the "Berggeist," Nos. 92 and 94, for 1871, some trials made at Thale in the Harz, in puddling Ilseder pig iron along with $1\frac{1}{2}$ per cent. of its weight of fluorspar, have resulted in producing a fibrous bar iron when rolled, which is not at all coldshort; a result altogether unexpected, since this pig iron is known as one of the cheapest brands of German iron, for notwithstanding that it contains from 3 to 5 per cent. manganese, and only about 0·1 per cent. of sulphur and silicon, it has, on account of its large percentage of phosphorus, been much more difficult to sell than the common irons, like that made from minette, which only contains 0·2 per cent. of manganese, but is, however, not so rich in phosphorus as the Ilseder pig.—Vide Forbes's *Report*, February 1872.

Chromium Steel.—Cast steel, in which the carbon is in part or wholly replaced by the metal chromium, is attracting considerable attention in the United States, where, says Mr. Forbes's "Report" (February 1872), a company, called the Chrome Steel Company, now produce this steel in large quantities by processes patented by a Mr. Bauer. It is claimed for this steel that it is capable of sustaining a greater degree of heat than ordinary steel, and, consequently, is not so liable to become oxidised or "burnt" in working. It is said to work quite as easily, and to roll much more smoothly than ordinary steel. It is stated to be made in crucibles, but otherwise we have no details of the mode of manufacture. According to a report of Captain Eads, chief engineer to the Illinois and St. Louis Bridge, this steel has been employed in those parts of that bridge where very great strength was required with perfect success, notwithstanding that anchor bolts and staves made from the usual cast steel had, as a rule, failed to sustain the tests fixed by the terms of the contract.

Using up Old Iron.—At the rolling mills of the Southern Railway in Gratz, the old rails, crop ends, and scrap are heated in a gas furnace, and then added to a bath of good grey pig iron, melted with some spiegeleisen in a Siemens-Martin furnace. When the sample shows that the desired quality of steel is attained, the metal is tapped into large pouring pots lined with clay, from which the ingots are filled. The bottom of the furnace is made of quartz sand, so arranged as to be kept cool by a current of air, whilst the sides and roof are of Dinas bricks. ("Zeitsch. d. Deutsch, Oesterr. Eisen. Stahl. u. Maschin. Industrie.")

Specimen of Wolfram from Trumbull, Connecticut, U.S.A.—At a meeting of one of the American societies Dr. L. Feuchtwanger exhibited an interesting specimen of wolfram. It consists for the most part of scheelite (tungstate of lime), but at places this mineral has been replaced by the wolfram (iron and manganese) taking the place of the lime; but the crystalline form of the scheelite has been retained, so that we have a remarkable example of pseudomorphism where large crystals appear having the form of scheelite, but composition of wolfram. This is stated by Dana to take place by the action of a solution of bicarbonate of iron and manganese, or perhaps mainly through sulphate of iron arising from the decomposition of pyrites. He also exhibited a finely crystallised specimen of pyrargyrite, a sulphuret of antimony and silver, containing from 58 to 60 per cent. of the latter metal, from Germany.

What is Onoprite?—At one of the meetings of the Lyceum of New York, Dr. L. Feuchtwanger exhibited a specimen of a mineral which has gone by the name of Onoprite. It is a selenio-sulphide of mercury, and was first obtained by Del Rio near San Onopre, in Mexico. H. Rose has made an analysis of it, showing 6·49 per cent. of selenium, 10·30 of sulphur, and 81·33 of mercury. The specimen exhibited was not from the original locality in Mexico, but from the Santa Clara Mine, in Lake County, California, near Clear Lake. It was of particular interest, on account of its locality being associated with the Cinnabar of California. Lately it has had the name of Tiemanite bestowed upon it.

Crystallised Green-coloured Felspar.—Mr. R. Dinwiddie exhibited to the New York Lyceum of Natural History specimens of crystallised Green-coloured Felspar, from what is known as the Sea Wall of Mount Desert Island, Maine, where it occurs not very commonly in scattered particles or cavities. Many of the crystals were remarkably perfect, and appear upon superficial examination to be peculiar. The colour in most of them is very bright; often light but brilliant, and frequently not extending throughout the crystal.

MICROSCOPY.

The Cilia in Mollusks.—A most interesting paper is that of Professor Wyman in the "American Naturalist," in which he describes a series of experiments which he recently conducted on this subject. The first of the set of experiments were made with water. For these the gills of Unios and Anodontas are well suited. Their cilia are quite active, and vibrate in such directions, that on the inner gill the motion is *from* the free edge, and on the outer *to* it—facts which the experimenter should keep in mind. If an *inner* gill is cut away from its attachment and laid on the bottom of a flat dish, its cilia acting as legs, it will soon begin to move with its *free* edge forwards, and will in the course of time travel the entire length of the dish. Professor Wyman has seen a whole gill move ten inches in four hours. Under similar circumstances the *outer* gill will move with its base or *cut* edge forwards. This difference depends, as will be readily seen, upon the fact that the cilia of the two gills vibrate in opposite directions. The result of ten experiments gave the rate of motion of a piece of gill measuring 12mm. by 14mm., 6mm. a minute. If two outer gills are laid with their free edges towards each other they will at once begin to approach, and it frequently happens after meeting that one crawls directly over the other.

Development of Comatula.—Herr Metschnikoff has lately published, in the Bulletins of the Academy of St. Petersburg, a valuable paper on the above subject. To his surprise, he found no water-system whatever in this Echinoderm, nor could he trace anything in any way homologous to it; also discovered that what constitutes the water-system of adult Crinoids, which has always been homologised with the water-system of other Echinoderms, is developed in a totally different manner. In the free-swimming Comatula larva the bag-like digestive sac is the only organ developed; it becomes the digestive cavity of the adult after the larva attaches itself tc the ground. He noticed the tentacles as diverticula of the digestive sac in the interior of the larva; these subsequently force their way through to the exterior, at the time when the digestive bag has become further differentiated, and is provided with a mouth opening in the centre of the oval disk, and an anus opening not far from it on the side of the calyx. There is formed at this stage a large cavity which divides into two parts: the upper part, uniting the hollow tentacles at their base, forms the so-called circular canal; while below it, and connecting with it, we have a large cavity forming the perivisceral cavity, a mode of development of the circular ring and of the perivisceral cavity totally unlike that observed in Ophiurans, Starfishes, Echini, and Holothurians. Metschnikoff compares the mode of development of the upper and lower cavity to analogous processes in the embryonic growth of Alcyonella and other Bryozoa; he traces a striking similarity in the structure and position of the digestive organs and tentacles with similar organs of Bryozoa.

Number of the Beads in Surirella gemma.—Dr. Pigott, in a paper in the "Monthly Microscopical Journal" for March, says that the *Surirella gemma*, ordinarily a line object, gives up beautiful beads. In the photographs presented to him by Colonel Woodward he counted with a pocket lens *thirty beads*, and carefully inserted the points of a pair of fine compasses into the centre of the *first* and *thirty-first* bead so as to give the exact measurement of thirty beads. This he found to be on Photograph xvii.

$$1 \text{ inch and } \frac{125}{1000} \text{ or } 1''\cdot125.$$

The power marked on the card was 3100 (probably by a clerical error), which gives

83,000 beads per inch, nearly.

This appears rather too great. Fortunately, he says, the Colonel has sent him two photographs of the same diatom. The larger is not quite $2\frac{1}{4}$ times the size of the smaller. If, therefore, the magnifying power employed be as stated upon the smaller one, 1034, the second photograph is magnified not 3100 times, but

2200 times,

which gives nearly

59,000 beads per inch.

The Manufacture of Object-glasses.—A discussion has been for some time going on in the "Monthly Microscopical Journal" between Mr. Wenham, our highest English authority, and certain American microscopists, on this subject. Among other papers is one by Mr. E. Bicknell, in which, observing that Mr. Wenham says: "In such a difficult and complex arrangement as a high-power object-glass it is almost impossible for all the makers to work to the same magnifying standard," he remarks: "I would like to inquire where all the much-talked-of mathematical formulæ are? Does he mean to intimate that the best opticians work by 'rule of thumb?' or that it is guesswork? I have been supposing all the time since I became interested in the use of the microscope, and seeing such terms as 'index of refraction,' 'dispersion,' 'light flint,' 'heavy flint,' and 'Faraday flint,' 'crown glass,' &c., that there *were* some mathematical formulæ involved somewhere in the construction of object-glasses; possibly this statement by Mr. Wenham may account for the discrepancy between the *nominal* and the *actual* power of some object-glasses."

Resolution of Amphipleura pellucida.—Mr. R. B. Tolles, an American microscopist of considerable note, states, as a fact of interest to microscopists, that the resolution of *Amphipleura pellucida is not* any longer a difficult achievement. With a true $\frac{1}{10}$-in. of only 90°, *even less than* 75°, he has repeatedly and plainly lined the valve, *using sunlight and blue cell,* the specimen of *A. pellucida* being one supplied to him as a proper test specimen by Dr. J. J. Woodward, and received by him from London. This performance was not dependent on any special merit in the objective—any good immersion $\frac{1}{10}$th would do the same thing. To accomplish this he used a narrow angled 1-in., 10° swinging under the stage of the microscope as a *condenser.* This 1-in. was placed at the incidence (obliquity to the axis of Mic.) 45° *or less.* The focus for parallel (sun's) rays a *little* outside of the object.—*Monthly Microscopical Journal,* March.

Dr. Beale's Mode of Demonstrating the Microscopic Structure of Tissues.—The mode is that which he has followed for more than ten years, and which in his hands has been most successful. He feels sure that it is capable of further improvement in practical details, and that, upon the principles which he has laid down, delicate structures, which have not yet been seen by man, will be demonstrated by patient and well-practised observers. The process is troublesome, and for this reason it has not been in much favour. In these days investigation must be conducted with such haste, and new facts discovered so quickly, that there is little chance of getting many persons to spend sufficient time in mere practice to enable them to gain the requisite skill for the very much more minute investigation of the structure of the most delicate textures which is now so much required, and which must be carried out before we can hope to arrive at positive conclusions on fundamental anatomical questions of the greatest importance.—*M. M. J.,* March.

Oblique Illumination.—Dr. J. Miller gives, in the "Microscopical Journal" for January, an account of Col. Horsley's cylinder for this purpose. This instrument is $1\frac{1}{2}$ inch long, and $1\frac{1}{4}$ inch in diameter, made of brass tube silvered inside. The light is thrown on to it either directly, or from prism or bull's-eye used as a prism. A dark ground is easily got with low angle $\frac{1}{4}$-inch, and without trouble or loss of time the hemispheres of Angulatum are shown very beautifully. The first apparatus was made by silvering the inside of the tube of the sub-stage which carried the polarizer; this was done by rubbing on it a solution of nitrate of silver, to which hyposulphite of soda had been added. This silvers brass with a good surface, and moderately permanent, in a moment.

Prizes for London Microscopists.—A series of prizes has been offered by the Countess of Ducie to those who work at the ponds and streams of the London neighbourhood. The prizes are three in number: one 5*l.*, one 3*l.*, and one 2*l.* They are to be awarded on March 31, 1873. The conditions may be had by writing to Secretary Natural History Prizes, 100 Fleet Street, E.C. The adjudicators are Henry J. Slack, Esq., F.G.S., Secretary of the Royal Microscopical Society, author of "Marvels of Pond Life," &c., &c., and Walter W. Reeves, Esq., F.R.M.S., Assistant-Secretary to the Royal Microscopical Society.

A New American Microscopical Journal.—Just as we go to "press," the first number of the new Chicago journal reaches us. It is called "The

Lens," and will be published quarterly. This number reproduces a paper and plate of Mr. Johnson's from the "Monthly Microscopical Journal," but it has other non-illustrated papers which are very good indeed. We wish "The Lens" every success, and we feel certain it deserves a hearty welcome from microscopical readers.

A New Erecting Arrangement for Binoculars has been described by Dr. R. H. Ward, U.S., in "Monthly Microscopical Journal" for January. Similarly Mr. Stephenson, F.R.M.S., has described an immense improvement in his binocular arrangement at the last meeting [March 7, 1872] of the Royal Microscopical Society.

A New Micrometric Goniometer Eye-piece has been described by Mr. J. P. Southworth in Silliman's "American Journal" for December 1871.

Microscopical Papers for the Quarter.—The following papers have appeared in the "Monthly Microscopical Journal" for the three months, January, February, and March:—"The Markings on the Battledore Scales of some of the Lepidoptera." By John Anthony, M.D. Cantab., F.R.M.S., &c.—"The Nerves of Capillary Vessels and their Probable Action in Health and Disease." By Dr. Lionel S. Beale, F.R.S., Fellow of the Royal College of Physicians, Physician to King's College Hospital.—"Note on Dr. Barnard's Remarks on 'The Examination of Nobert's Nineteenth Band.'" By J. J. Woodward, Assistant-Surgeon U.S. Army.—"A New Erecting Arrangement, especially designed for Use with Binocular Microscopes." By R. H. Ward, M.A., M.D., Professor of Botany and Microscopy in Rensselaer Polytechnic Institute.—"On a New Micrometric Goniometer Eye-piece for the Microscope." By J. P. Southworth.—"On the Action of Hydrofluoric Acid on Glass viewed Microscopically." By H. F. Smith, Esq.—"On the Relation of Nerves to Pigment and other Cells or Elementary Parts." By Dr. Lionel S. Beale, M.B., F.R.S., Fellow of the Royal College of Physicians, Physician to King's College Hospital.—"Report on Slides of Insect Scales." Sent to the Royal Microscopical Society by the Chevalier de Cerbecq, accompanied by a letter. Examined by Henry J. Slack, Sec. R.M.S.—"On the Structure of the Stems of the Arborescent Lycopodiaceæ of the Coal-measures." By W. Carruthers, F.R.S.—"On Bog Mosses." By R. Braithwaite, M.D., F.L.S.—"The Advancing Powers of Microscopic Definition." By Dr. Royston-Pigott, M.A. Cantab., M.R.C.P., F.C.P.S., F.R.A.S., M.R.I., F.R.M.S.—"Microscope Object-glasses and their Power." By Edwin Bicknell.—"Remarks on a Tolles' Immersion $\frac{1}{15}$th." By Edwin Bicknell.—"Maltwood's Finder Supplemented." By W. K. Bridgman.—"On a New Micro-telescope." By Prof. R. H. Ward.—The President's Address.—"Mycetoma: the Fungus-foot Disease of India." By Jabez Hogg, Surgeon to the Royal Westminster Ophthalmic Hospital, Hon. Sec. R.M.S., &c.—"The Advancing Powers of Microscopic Definition." Part II. By Dr. Royston-Pigott, M.A. Cantab., F.C.P.S., F.R.A.S., M.R.I., F.R.M.S., formerly Fellow of St. Peter's Coll., Cambridge.—"The American Spongilla, a craspedote, flagellate Infusorian." By H. James-Clark, A.B., B.S., Prof. Nat. Hist. Kentucky University, Lexington, Ky.—"The Refractive Powers of Peculiar Objectives." By Robert B. Tolles, U.S.—"A Few Additional Remarks on 'The Examination of Nobert's Nineteenth Band.'" By F. A. P. Barnard, Columbia College, New York.

PHYSICS.

The Spectrum of Uranus.—In the "Comptes Rendus" (February 26, 1872) Father Secchi has a paper which contains the following results. He says that during one of the fine evenings of February he has been examining anew the spectrum of Uranus. He states that he has found the three bands that he formerly observed: one in the red, one in the yellow, and the third in the blue; this latter coincides really with the line as pointed out by Mr. Huggins and M. Vogel, contrary to that which he believed he saw at the period of the discovery of this curious spectrum. But he says that the band of the red requires, in order to be visible, an atmosphere perfectly clear. He is not surprised therefore that it has not been observed.

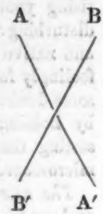
An Apparatus for Studying Vibration.—We hear of an ingenious contrivance for the above, which has been invented by an American gentleman. A disk of white cardboard, with apertures oblong in radial direction, is set on a spindle, so as to be rotated at any requisite speed. To examine, for instance, the flame of a gas light (in a glass tube, to prevent disturbance by air currents), place the disk in front of the light, so that the eye can see the light through each slit as it comes to a vertical position. If the speed of the disk's rotation is such that the interval of time between two slits passing the eye is just equal to the period of a vibration of the flame, the flame appears to be motionless; but if the velocity be reduced, the flame is seen to go slowly through its changes of form. If the interval be equal to, or one-half of, or one-third of, the period of the vibration of the light, the illusory appearance of a disk having as many as, or twice, or three times the number of slits really in the disk is seen. This phantom disk will appear to be motionless when the periods coincide; but when otherwise, it revolves in one direction or the other. It is obvious that the vibrations of the flame can be easily counted by this means. The inventor, Mr. Charles J. Watson, counted with a sixteen-inch tube, 453 vibrations of the flame per second. By this instrument, the undulation of the vibrations of a wire can be seen to travel up and down the wire; and if watched by both eyes through the slits, the spiral course of the undulations can be observed.

A New Source of Electricity has been discovered by Dr. James St.-Clair Gray, and is thus reported. There was prepared a cell containing a solution of caustic-potash, in which phosphorus and sulphur, both in sticks, were placed. Within half an hour the phosphorus was reduced to an oily mass, perfectly mobile, occupying the lower part of the cell; the sulphur was not at first affected. The temperature at first rose considerably—about 20°—but this soon passed off, and the solution returned to the temperature of the surrounding medium, varying from 56° to 60° F. During the first six days there was a constant development in small quantity of phosphuretted hydrogen in the spontaneously inflammable form; but after that time, although phosphuretted hydrogen still continued to be evolved, it no longer ignited spontaneously, this being probably due to the simultaneous development of sulphuretted hydrogen, which began to be exhaled in appreciable quantity about this time. At first the sulphur was little affected, but at the end of ten days it was found that at the point of junction of the phosphorus therewith, there had occurred considerable loss of substance. In the solution there were produced sulphite and phosphite, hyposulphite and hypophosphite, and slight traces of sulphate and phosphate of potassium. At the end of three months the conditions were still much the same; the phosphorus was still fluid and mobile, the sulphur more eroded, the same products present in solution, though increased in quantity and somewhat altered in the ratio which the one salt bore to the other; due allowance being made for increase, the proportion of sulphate and phosphate was greater. The electricity which was subsequently proved to be developed was found to be greater than that produced by an ordinary Daniells cell, and it lasted for more than three months.

The Colour of Fluorescent Solutions.—Dr. H. Morton has sent to a late number of Silliman's "American Journal" a paper describing experiments of his which sustain the interesting conclusion that "all the familiar fluorescent solutions, such as the tinctures of turmeric, of agaric, of chlorophyl, and the solution of nitrate of uranium, emit lights of the same colour of fluorescence—namely, blue, identical with that developed by acid salts of quinine."

The Mode of Production of the Aurora Borealis.—An important paper, by Herr Zöllner, in which those who are engaged in spectroscopic inquiries will be interested, appears, in Poggendorff's "Annalen" (cxli. p. 574). After some preliminary discussion, he says that a layer of ignited air 1^{m} in thickness would, at a height of ten miles, be equivalent to a pressure of 78^{mm}, and at a height of twenty miles, to a pressure of $0{\cdot}01^{mm}$ in a Geissler's tube, in order at the same temperature to give a spectrum as bright as that of the aurora. But as the thickness of the layer of ignited air must be reckoned by kilometers instead of meters, we should have, even at a height of ten miles, so high a value for the equivalent pressure in a Geissler's tube—78^{m} of mercury for a layer 1 kilometer in thickness—that the most powerful induction apparatus could not overcome the resistance of the air. From this it follows that the quantity of particles of ignited air in a Geissler's tube is probably extremely small when compared with the *active* quantity in the case of the aurora. Since, however, the spectrum of a gas in such a tube must have at least the brightness of the auroral spectrum to admit of spectroscopic analysis, it follows that the emissive power of the particles of gas ignited in the tube must be much greater than that of the ignited gaseous particles in the case of the aurora. Such a difference can be produced *only* by differences in temperature. If, therefore, the light of the aurora depends on ignited particles of our atmosphere, the temperature at which the ignition takes place must be much lower than that at which the same gases can be ignited by electricity in Geissler's tubes. From these considerations it appears that all the gas-spectra of the different orders which we can artificially produce belong in general only to high temperatures, since the relatively greater brightness with less quantities of ignited matter renders a greater emissive power necessary, and this can be produced only by a higher temperature. Converseley, as in the case of the aurora, corona, zodiacal light, &c., a great number of active luminous particles must be assumed to exist, it follows that the temperature of the ignited gases must be relatively low.

A New Mode of Producing Stereoscopic Effect.—The following account is taken from a recent number of Zehender's "Monatsblatt" of an experiment of Listing, who has already done so much in physiological optics. It brings out stereoscopic effect with only one picture, which consists of figures arranged in a peculiar way, and seen with vertical double images. The simplest experiment is to view two lines crossing each other at an angle of about 30°, with a prism of 4° or 5°, its base vertical before one eye. No effort must be made to correct the vertical diplopia. If the prism be put before the left eye, its base upward, the line B B′ seems nearer to the eye than A A′. If the prism be turned with its base downward, and before the same eye, the line A A′ seems nearer, and B B′ more remote. It is found that with the base downward the prism must be weaker than when turned with the base upward. In gaining the effect by prisms so weak as these, no double vision is produced except for horizontal lines—the oblique lines appear to be only two. The same phenomenon may be produced in a common stereoscope by having two similar figures, and pushing one alternately up and down. Two rows of the same letters are arranged on a page like the limbs of the letter X, and viewed as above stated with a vertically deflecting prism; a sudden removal of one now takes place to a considerable depth, while this appearance is at once reversed on turning the prism around 180°. These curious effects can be best produced and understood by means of the diagrams accompanying the article.



ZOOLOGY AND COMPARATIVE ANATOMY.

Remarks on Transcendentalism by a former Transcendentalist.—In his annual address as President of the Royal Microscopical Society, Dr. Kitchen Parker, F.R.S., made some observations on the transcendentalists and their school which we think of importance. Thirty years ago, said Dr. Parker, his favourite author in these matters was Sir Charles Bell, a good type of the Paley or *Fitness* school. Such good reverential men loved to trace the handiwork of an infinitely clever *Contriver*, who brought organisms into being in a moment—monads or whales—the plan of each having been perfected beforehand by a mode of thought peculiarly human. Looking from another standpoint, and yet reverent as they, he has no quarrel, even now, with this school. They but saw and understood things in an *ex parte* manner, pleased with a fitness, delighted with a plan. There are deeper meanings than these in the vertebrate organisation; and as early as 1790 the great poet and morphologist, Goethe, began to get some glimmerings of a gorgeous morphological science unimagined by earlier observers and limited thinkers. In 1807 Lorenz Oken took up, worked out in his peculiar way, and went somehwat mad upon this most fascinating subject. He had several disciples, and the greatest of these was our own countryman, Richard Owen. "I could almost have wished," said Dr. Parker, "that the 'transcendentalism' which grew out of these oblique glimpses, partial and fitful, in the dawn of a new era of biological science, had never reached our shores, and that we had passed direct from the labours of the great 'gradationalist,' Baron Cuvier, to those of the German embryologist, such as Baër, Reichert, and Rathke. These men did indeed lay a foundation, deep and wide, by their incomparable labours, for a true morphological knowledge of the Vertebrata. Socrates was accused of corrupting the rising youth of Athens by questioning the truth of received axioms, and disturbing their unfinished minds with doubts. We may accuse a modern and native philosopher, our English Oken, of misleading students—I speak feelingly for myself—in the opposite direction, namely, by dispelling wholesome doubts by interpreting all the hard sentences of Nature for them, and by holding before their charmed vision an *Archetypal Idea,* instead of setting them to work at the various types, with scalpel, and lens, and microscope."

The new views of Spongilla structure.—Perhaps there is no subject in zoology that has been so much explored as that of Spongilla; and even yet, notwithstanding the fine papers of Henry Carter, F.R.S., and Dr. Bowerbank, F.R.S., and Dr. H. James Clark, there appears work to be done. Dr. James Clark has an able paper in Silliman's "American Journal" for December 1871. In this he in great measure confirms Carter's researches. He says that the *monad chambers* are deep spherical hollows, which form the receptacles of the groups of monads. They are mere cavities, and have no lining wall. They may be easily recognised, in young specimens, as clear, more or less circular, areas scattered in pretty close proximity to each other over the "cytoblastemic mass." Each chamber has a single, small, circular aperture which perforates the inner investing membrane, and allows egress into the circulatory apartment. The aperture varies in size at times, and may even be completely closed. He has never seen it open wider than one-third the diameter of the chamber, and very rarely more than one-fifth as wide. That it is a true perforation, and not a clear spot, may be demonstrated by bringing a chamber into profile, so that its aperture lies on the extreme border, and then an actual break in the continuity of the investing membrane becomes evident. Entering this aperture, we do not meet with any obstacle for a little distance around it; there is a clear open space; but pressing onward beyond that, either to the right or the left or directly forward, the cavity appears filled by a collection of vibrating bodies. They seem to be arranged radiatingly from and about the centre. Close inspection, however, modifies this view, and it turns out that they are based upon the periphery of the chamber, and converge towards its centre, where is a small unoccupied space. We presently recognise these converging bodies to be *craspedote, flagellate* monads, so closely packed together, side by side, as to form a continuous *stratum* over the whole concave face of the chamber, excepting immediately about the aperture. Every feature of the monad is strongly marked; even the cylindrical collar is so heavy and conspicuous that its outlines may be seen with as low a power as two hundred diameters. Professor Clark has studied these bodies with a $\frac{1}{8}$th-inch objective, and found it not at all difficult to focus down upon the details of their organisation without pressing upon or even touching the specimen.

Does or does not Cliona burrow?—This question has recently developed the opinion of two naturalists: Mr. Ed. Parfit, who asserts that it does, and

Mr. J. G. Waller, who with equal positiveness asserts that it does not do so. Mr. Parfit's paper is published separately, and Mr. Waller's is printed in the January number of the "Journal of the Quekett Club." Both are of interest. We ourselves think Mr. Waller is nearer the truth than the other observer; but the matter must still be considered in some doubt, as very many able observers are to be found on both sides.

A new French Journal of Zoology has commenced its career in Paris. It started on January 1 in the present year, and will be published quarterly. It is edited by M. H. Lacaze-Duthiers, and is to deal especially with experimental zoology. It is called the "Archives de Zoologie expérimentale et générale."

Australian Polyzoa.—Mr. McIntyre has presented to the Royal Microscopical Society a splendid collection of the above. They came very opportunely, as Mr. P. H. Mac-Gillivray recently read a long paper on those found on the Australian coasts. This paper will be found in "The Transactions of the Royal Society of Victoria for 1869."

The Aquarium at Brighton.—This promises to be a larger undertaking even than the admirable aquarium which Mr. Lloyd so skilfully manages at the Crystal Palace. The following is a brief account of the Brighton Aquarium, which is likely to be open early in the summer. It is taken from the "Daily News" of March 13, which had a long and admirable article on the subject. In the bays between the piers supporting the corbels, whence spring the groining, are the tanks for fish, of which in all there are twenty-eight, the dimensions of which range from 11 feet to 100 feet of frontage. The front works of the tanks is composed of Portland stone to a height of three feet from the floor, with thick plate glass above, six feet high, and cemented into iron columns. The interior of the tanks are being fitted up with rockwork of varied character, some consisting of stratified rock from Nuthurst, near Horsham, other portions consisting of tufa fantastically arranged, and in some of the tanks taking the appearance of caves. The water, which in the majority of the tanks will be salt, in a few fresh, will be introduced from pipes at the back of the tanks, where also, above the water's edge, are the doors affording access to the attendant for feeding purposes. But the water in the tanks will reach higher than the top of the plate glass frontage, and thus the light which will penetrate into the corridor will be entirely peraqueous, a property which, in the words of a local authority, "will afford to visitors that delightful under-water sensation experienced when swimming, and enable them to feel 'quite at home' with the fishes without the inconvenience of getting wet." At night the tanks will be lit from above with argand lamps and powerful reflectors, with which the lights in the corridors, springing as they will do from foliated mouldings in the corbels, are so contrived as in no way to clash. The length of the corridor is 220 feet, but it is broken in the middle by a space known as the "Central Court." This hall, on either side of which is a single large tank, is covered by a raised ornamental iron roof, partly glazed with stained glass, the ironwork being painted in bright colours.

Dust-showers and their Organisms.—A memoir of 150 pages has been written on this subject by no less an authority than the veteran Ehrenberg. The Professor has in it (published 1871) given a very valuable review of the facts relating to organisms from dust-showers and other atmospheric sources, adding also the results of new observations. Plate 1 contains numerous forms of organisms from recent dust-showers: that of Ispahan in 1870; the Dardanelles to Sicily in 1869; Apulia in 1868; Janina in 1870; Island of Sora in 1869; Switzerland in 1867.

Boreal and Arctic Amphipods.—We learn from Silliman's "American Journal" (for January 1872) that Axel Boeck has published a prodomus in Latin of the large work on this subject illustrated with thirty-two plates, soon to be published by the author. These Amphipod Crustaceans have their greatest diversity of forms in the cold latitudes of the globe, and hence a work on Arctic and boreal species has a special interest. This prodomus contains full descriptions of all the species, and a complete synonomy.

Animals of the Mammoth Cave of Kentucky.—Mr. A. S. Packard, junior, has given, in the "American Naturalist" (Dec. 1871), an account of animals recently collected from the above locality. He says, "that of all the animals found in caves, either in this country or Europe, perhaps the most strange and unexpected is the little creature of which we now speak. It is an Isopod crustacean, of which the pill-bugs or sow-bugs are examples. A true species of pill-bug (*Titanethes albus*, Schiödte) inhabits the caves of Carniolia, and it is easy to believe that one of the numerous species of this group may have become isolated in these caves and modified into its present form. So, also, with the blind *Niphargus stygius* of Europe, allied to the freshwater Gammarus, so abundant in pools of fresh water. We can also imagine how a species of Asellus, a freshwater Isopod, could represent the Idoteidæ in our caves, and one may yet be found; but how the present form became a cave-dweller is difficult of explanation, as its nearest allies are certain species of Idotea which are all marine, with the exception of two species: *I. entomen*, living in the sea and also in the depths of the Swedish lakes, as discovered by Loven, the distinguished Swedish naturalist, while a species representing this has been detected by Dr. Stimpson at the bottom of Lake Michigan. Our cave-dweller is nearly allied to Idotea, but differs in being blind, and in other particulars, and may be called *Cæcidotea stygia*. It was found creeping over the fine sandy bottom, in company with the Campodea, in a shallow pool of water four or five miles from the mouth of the cave.

Conditions of Biological Research is the title of a section of Professor Allen Thompson's address to the Biological Department of the British Association. This, like all other parts of the address, is full of thought, and is amply in keeping with scientific progress. Those who are interested in that which unites all branches of natural history and physiology will do well to read this address, which we suppose is given in the usual Report.

The Humble Bee at work as a Fertiliser of Plants.—The "American Naturalist" lately published a very valuable and lengthy paper on Darwinism and fertilisation of plants by insects, in which we find a proof of the functions of the Humble Bee as a fertiliser. Subsequently, says the writer, referring to two German observers (after speaking of a former failure), on the heights of Stromberg, very abundant in Orchis, both he and his son were enabled with ease, and close at hand, to observe many Bombi at work. At a place full of *Orchis mascula* they saw a

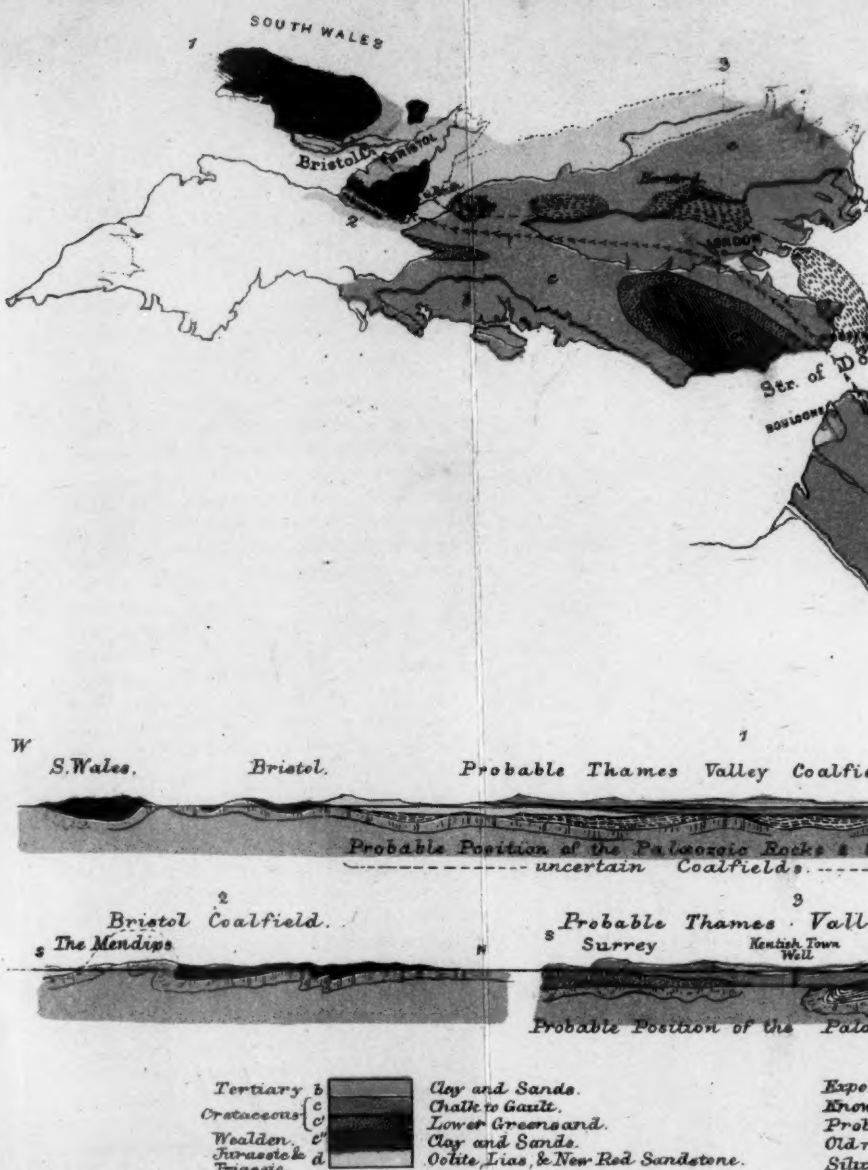
Bombus, which appeared to be *B. terrestris*, fly to the lowermost flower of a spike of this Orchis. It inserted its head into the flower, remaining about four seconds, and then withdrew it with two pollen-masses attached. Ascending from the bottom towards the top, it visited the second and third flowers of the same spike. After withdrawing its head from the third flower, it stopped a short time and endeavoured to brush off the pollen-masses with its legs, but without success. It then continued its visit, climbing up the spike, and visited a fourth flower. At this point he tried to catch it in the net, but failed, and it flew away. After standing a short time they saw a *Bombus hortorum* visit three or four flowers from base to summit of a spike of *Orchis mascula*, after which it flew to another individual of the same species, visiting its flowers in the same way. Upon examining the stigmas of this second individual, they found pollen scattered upon them, and the anther lobes emptied of their pollen-masses. In the space of about two hours, which they spent in observing this fecundation of *Orchis mascula*, they noted two visits of *Bombus lapidarius* and one *Psithyrus campestris*. The *Bombus lapidarius* did not remain in the flowers longer than from two to three seconds. They captured the *Psithyrus* and one *Bombus lapidarius*. Both had a quantity of pollen-masses upon their heads, some of which were already depressed upon their respective stalks, and therefore in a condition to rub against and fecundate the stigmas, while others were yet erect, and therefore not in a condition to effect fecundation. Of ninety-seven bees collected by them in this excursion, thirty-two had pollen-masses stuck upon their heads. Sometimes they observed that the bees succeeded in freeing themselves from some of the pollen-masses, either by tearing them off with their mandibles or brushing them off with their fore-legs. Possibly it is in this way that sometimes in the flowers of Orchis pollen-masses are found in greater or less proximity to the stigma, out of place, and, as it were, wasted.

Annelids of the Gulf of Naples.—There was published in M. Claparède's name a supplement to his "Annélides chétopodes" of the Gulf of Naples. It deals chiefly with the mode of reproduction, and is of especial interest. Malmgren in 1864 was first led to suspect a genetic relation between Nereis and Heteronereis, from a comparison of *Nereis pelasgica* and *Heteronereis grandifolia*, showing nearly an absolute identity, with the exception of the peculiar foliaceous appendages and bristles of the posterior part and other minor characters only developed at the period of sexual maturity. This led him to look upon certain species of Nereis as the agamous stock of sexual individuals appearing as Heteronereis. Subsequently having found eggs in this presumed agamous Heteronereis stock, he came to the conclusion that, although all the species of Iphinereis and Heteronereis were only sexual forms in series of generations still unknown, yet that, at some time during sexual maturity, a stage of one of the polymorphous species of Nereis assumes the characters of Heteronereis, to lose them subsequently and return to its agamous stage. Malmgren accounted for the genetic relations by an alternate generation at first, and afterwards by a metamorphosis; both of which hypotheses Claparède shows conclusively are justifiable. Ehlers has shown that a large number of species of Heteronereis were only sexual forms of previously known species of Nereis, and interprets these facts in favour of a metamorphosis of Nereis into Heteronereis. This was the condition of the problem when Claparède resumed the subject, and showed conclusively (from the study of living Annelids) that there is a genetic relation between Nereis and Heteronereis, but he showed as conclusively that all Nereids do not have their Heteronereis form, as had been taken for granted by Ehlers, and that there is in the Annelids of this family a polymorphism almost without parallel in the animal kingdom. Taking the species which he has most carefully studied (*Nereis* [*Leontis*] *Dumerillii*) we have first a sexual form as Nereis, two sexual forms as Heteronereis, and a fourth hermaphrodite form. This is alone of importance; but the paper contains many more facts of interest.

Professor Lacaze-Duthiers in the Academy.—At one of the meetings of the Academy of Sciences, Paris, the seat left vacant by the death of the regretted and eminent Professor Longet was given by election to M. Lacaze-Duthiers, Professor of Natural History at the museum.

The American and European Bison identical.—Professor Brandt of St. Petersburg, in a lately published paper, renews the expression of his opinion in regard to the identity of the American and European Bison, both of them, in his view, being the lineal descendants of the fossil Bison of a now extinct form. The only appreciable difference between the American and European races, is the developed beard of the American animal, a characteristic, which, in view of similar differences in the manes of lions in different regions, not otherwise distinguishable, he considers of little importance. It may be proper, however, to say that a careful comparison of the crania of the two forms exhibits differences of a much more tangible character; the relationships of the nasal bones not agreeing at all, and the muzzle of the American animal being much broader than that of its European congener. According to Mr. Waterhouse Hawkins also, while the tail of the American Bison has the hairs close pressed, with a bushy tuft at the end only, that of the European animal is full and rather bushy from the root, being much the same difference as that existing between the tails of the American Mule or Black-tailed Deer (*Cervus macrotis*,) and the common American eastern Virginia Deer.

The Families of Mollusca.—The "Smithsonian Miscellaneous Collections" contain an important review of the above by Mr. Theodore Gill. In view of the wide diversity of opinion among zoologists concerning the classification of Mollusca, it is not to be expected that any scheme that can be proposed at present will be generally adopted. Yet we are constantly approximating to a true natural classification of these animals, thanks to the numerous anatomical investigations that have recently been undertaken. The author of the present work fully acknowledges the provisional character of the arrangement which he has adopted, and anticipates many changes hereafter. In reality, some improvements made within the past year would doubtless have been adopted, had not the work been in type some six months before its actual publication. It is, nevertheless, the best index to the classification of the Mollusca that has been published hitherto. It gives in a very convenient form an arrangement in accordance with the views of many of the most reliable malacologists. It is probable that in numerous cases too many families have been admitted, or divisions of minor value have been allowed family rank.



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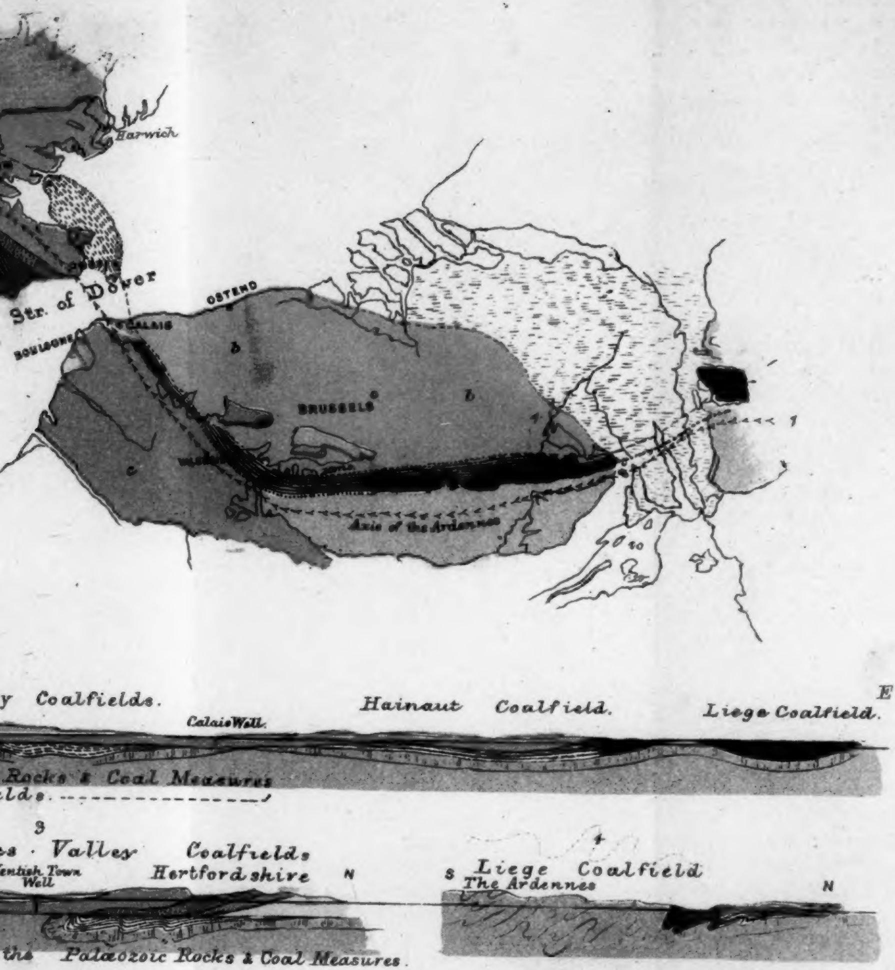
Pl. LXXXV.



Harwich
Str. of Dover
OSTEND
CALAIS
BOULOGNE
BRUSSELS
Axis of the Ardennes
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The Ardennes
N
the Palaeozoic Rocks & Coal Measures.
Exposed Coalmeasures.
Known underground Coalmeasures.
Probable underground Coalmeasures
Old red Sandstone and Devonian.
Silurian series.
Palaeozoic Rocks

ON THE PROBABLE EXISTENCE OF COAL MEASURES IN THE SOUTH-EAST OF ENGLAND.

By JOSEPH PRESTWICH, F.R.S., F.G.S.

[PLATE LXXXV.]

THE question of discovering coal in the south-east of England, so long treated in an empirical manner, has of late years assumed a scientific aspect, but in a direction entirely different from that towards which practice pointed. So many of our Tertiary and Secondary strata contain thin beds of lignite and shale, together with beds of sandstone—the first sometimes not unlike impure coal, and the latter resembling the shales and sandstones of the Coal Measures—that it is not surprising that men, ignorant of scientific modes of investigation, and at a period when geology was little understood, should have been misled by resemblance of parts into a belief of identity of the whole. But the progress of geology has since conclusively shown that although certain beds of lignite of Miocene and Wealden age may be worked for such purposes as lime-burning, and some beds of fair coal are wrought in the Oolites, as at Brora in Sutherlandshire, yet to all practical intent the beds of good and workable coal are confined to certain strata, known as the Coal Measures, forming the upper part of the *palæozoic* series of rocks, the position of which is perfectly well defined, and the organic remains of which serve to render their determination a matter of certainty.

The imperfect lignites of our tertiary strata around London; the lignites and sandstones of our Wealden area; the clays of the Jurassic series, have all at times given rise to the search after coal in the strata far above the Coal Measures; while the sandstones of the Devonian and the shales of the Silurian series have given rise to equally abortive attempts amongst the rocks under the true coal measures, even up to a recent period, by so-called practical men.

Aubry gives a curious account of one of these searches after coal, made in the neighbourhood of Guildford, at the end of the seventeenth century. "The Rev. Giles Thornborough, one of his Majesty's chaplains, digging a boring for coal in Slyfield Green, found, first of sand and gravel 7 ft.; then a spring; within a little of that a bed of stones like square caps and about 2 ft. every way, on the outside whitish, within full of sulphur, out of which was extracted tinn by L. Smyth, of London, engraver. These stones are called at the coal pits at Newcastle, 'catts' heads,' lying always (they say) where coal is. These catts' heads are all full of small pipes for the mine to breathe through. Next under them lay a body of black clay for 15 fathoms; then a rock of stone about a yard thick, which was very hard. Then they came to black clay again for about 3 fathoms, and then another rock; after that, clay mixed with minerals (of which Prince Rupert hath some, as also had King Charles in his closet, which was there placed by the Indian oar); then cockle shells, muscle shells, and periwincle shells, some filled with oar (out of which Prince Rupert extracted tinn and other things), and some filled with clay. After this sprung a bed of oker, 12 ft. thick, a kind of mother-of-pearl; after that, a green quicksand. Then came coal, which how deep it is, is unknown, for here the irons broke: thought by Mr. W. Lilly (astrologer) to be subterraneous spirits; for, as fast as the irons were put in, they would snap off. This is a kind of rocky coal (like that which they call Kennell coal) which burns like a candle." Mr. Thornborough was induced to make this trial, because "there was a kind of stony coal (lignite) that would burn, which he found by grubbing up the roots of an old oak in his ground there." After spending 400*l.* the work was abandoned, to be however revived again about a century and a half later, in the neighbourhood of Worplesdon.

The Wealden strata at Bexhill in Sussex consist of a series of sandstones, clays, and lignites, which led in 1804 to an expensive trial for coal near that village. A shaft was sunk to the depth of 164 ft., and two seams of lignite, designated as smut coal, 2¼ ft., and strong coal, 3½ ft. thick, were met with. The reputed resemblance to coal measures was kept up by the use of such terms as "church clay," "grey bind," "blue bind with iron ore," &c., and the presence of a seam of clay with impressions of wealden ferns not unnaturally assisted the delusion. Fortunately probably for the company, the mine was drowned out, and stopped the further extension of the work.

So recently as about thirty years since, another attempt was made to get up a company to establish a colliery at Worplesdon, near Woking, in the Bagshot Sands, and to sink a shaft to the depth of 150 ft. At that depth the projectors would have reached the London clay; but it is certain that no more profitable material would have rewarded their outlay. Any geologist could at that time have told them what their chances were; nevertheless, they state, "It does not appear that there has been any actual geological investigation or survey of the county of Surrey; therefore it is proper to observe that the non-assignment of coal to that county in the maps which profess to give the geological character of England is not a matter of importance. The most scientific geologists admit that there are various unexplored localities which future research may add to the coal-fields already known." The projectors seem also to have found encouragement in the prospect of being able "to work a mine so near Windsor."

While practice has been making during the last two centuries these tentative efforts—efforts even now continued from time to time almost as blindly as in former days*—science in the meantime has been making slow but sure advances, and is now prepared with an hypothesis respecting the existence of coal in our southern counties of very great probability. William Smith first established the order of superposition—confirmed by succeeding geologists—of the secondary and tertiary rocks of the south of England, the thickness of which at their point of outcrop in or nearest adjacent to the London basin may be roughly estimated as under:—

		Order of Succession of the Strata.	Locality of estimated Thickness.	Average Thickness.
Tertiary Series.		Bagshot Sands	Surrey	300ft.
		London Clay	Middlesex	400
		Sands and Mottled Clays	Surrey	80
Secondary Series	Cretaceous.	Chalk	Hertfordshire	1000
		Upper Greensand	Surrey	30
		Gault	Surrey	120
		Lower Greensand	Surrey	500
		Weald Clay and Hastings Sands	Sussex	2000
	Jurassic.	Purbeck and Portland Beds	Berkshire, Oxfordshire	70
		Kimmeridge Clay	Buckinghamshire	450
		Coral Rag, Oxford Clay	Wiltshire to Oxfordshire	500
		Oolites	Gloucestershire	750
		Lias	Gloucestershire	600
	Trias.	New Red Sandstone	Somersetshire	800
				7,600

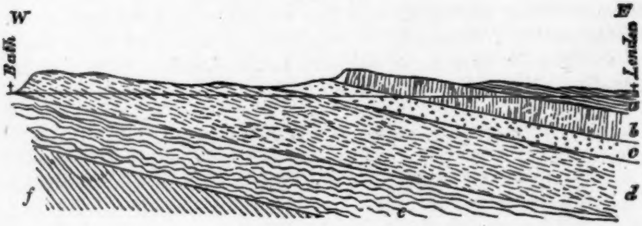
* Even up to the present day a firm belief exists among many, even of men experienced in coal-workings, that in the lower tertiary strata between the London Clay and the Chalk, good coal exists. As hundreds of wells and sections innumerable expose these strata, we well know how futile any such expectations, founded on the presence of irregular seams of lignite, are.

And since then the order of the older underlying rocks has been thus determined by Sir R. Murchison, Professor Sedgwick, and others:—

Palæozoic Series.	Permian		Gloucester, Warwickshire
	Carboniferous	COAL MEASURES	Somerset, Gloucester
		Millstone Grit	Somerset, Gloucester
		Mountain Limestone	Somerset, Gloucester
	Devonian (Old Red Sandstone)		Devon, Somerset
	Silurian		Wales
	Cambrian		Wales

It was for a time supposed that secondary strata maintained in the main their regular sequence and thickness unimpaired over large areas, and in our early geological works the section of the secondary formations, from the west to the east of England, is given as under:—

FIG. 1.



a. Tertiary strata. *b.* Chalk. | *c.* Lower Greensand. *d.* Jurassic series. | *e.* Lias and Trias. *f.* Palæozoic rocks.

In this case, the depth to the *coal measures* under London, supposing they existed there, would have been from 7,000 to 8,000 ft. or more. Speculation was hushed in presence of such depths, nor did it actively revive until the facts more recently acquired showed that the obstacles presented by such an enormous mass of strata had no real existence.

Admitting the variation of thickness, we were hardly, however, until lately, prepared to admit how rapid that variation was. Professor Hull has shown that the Lias and Oolites become much thinner as they range eastward from Gloucestershire. There is reason to believe, in fact, that the oolitic series do not extend far under the chalk hills of Berkshire, and it is known that the Inferior Oolite thins out even before reaching Oxford. Mr. Hull gives a section, from Gloucestershire (the neighbourhood of Cheltenham) to Oxford, in which he shows that all the rocks below the Great Oolite thin out rapidly to the south-east;

and he estimates the thickness of the whole at Oxford at about 600 ft., whereas in Gloucestershire it is 1,880 ft.:—

	Gloucestershire. Feet.	Oxford. Feet.
Great Oolite and Fullers Earth	370	205
Inferior Oolite and Sands	420	0
Lias (Upper and Marlstone)	640	200?
Red Sandstone and Marls (Keuper)	450	200?
	1,880	605

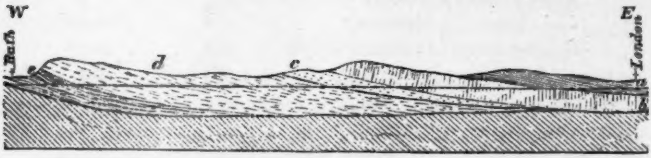
To the north-eastward the Great Oolite and the Oxford Clay become but slightly thinner. On the other hand, the Kimmeridge clay, which is 275 ft. thick at Swindon, is 310 ft. near Abingdon, and increases to 450 ft. at Aylesbury. In like manner, the Portland and Purbeck series increase from 12 to 70 ft.; while the Wealden series, which is altogether absent in the west of England, is about 2,000 ft. thick in Surrey and Sussex. So the Lower Greensand, which is only 50 ft. near Devizes, attains a thickness of about 500 ft. at Reigate. The Gault maintains a mean thickness of about 100 ft.; while the Upper Greensand, 150 ft. thick at Devizes, is reduced to 25 ft. at Merstham. The Chalk, taken at its full development, maintains tolerably constant dimensions from Wiltshire to Dover, viz. of from 800 to 1,000 ft. unless when, as often happens, it has suffered denudation. Notwithstanding, therefore, the large development of the Secondary formations, both westward and northward of the London basin, it was uncertain how many and how much of these might be found to extend under the Tertiary strata of the south-east of England. We did not anticipate, however, the great hiatus which in reality has been found to exist. Whatever might be the case with the triassic and jurassic series, it seemed at all events probable that the lower cretaceous strata, which are so fully developed a few miles both on the north and south of London, would, like the upper cretaceous strata (the chalk), pass under London, and therefore it was a matter of surprise when the boring at Kentish Town, made in 1854, showed that not only were the older secondary strata all absent, but also the lower greensands, which are so well developed in adjoining parts of Kent and Buckinghamshire. In the place of the latter were found a series of red and grey sandstones belonging probably to the Old Red Sandstone, so that the following diagram (fig. 2) may now be considered as representing probably the order of succession and position of the strata from the West of England to London:—

In the same way MM. Dufresnoy and Elie de Beaumont*

* "Explication de la Carte Géologique de la France, 1841," vol. i. p. 727.

have given a diagram showing a similar thinning out and overlapping of the strata from south to north in the north of France, and a like unconformable independence of the secondary

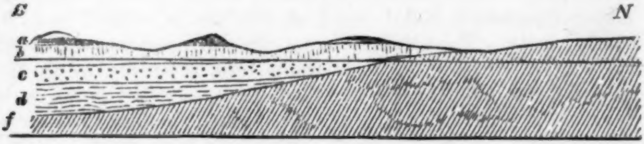
FIG. 2.



a. Tertiary strata. *b.* Chalk. | *c.* Lower greensand. *d.* Oolitic strata. | *e.* Lias and trias. *f.* Palæozoic strata.

and of the palæozoic series, only there the older strata come to the surface, which they do not in south-eastern England :—

FIG. 3.



Westward of this line of diagram in France, the palæozoic rocks sink deeper, and are altogether covered by secondary and tertiary strata ; and while at Lille the *chalk* is found to repose on the *mountain limestone*, at Valenciennes it reposes on *coal measures*. These latter are the prolongation of the great Belgian coal-field, which ranges from Liège westward to and beyond Mons.

It is about two centuries ago that the Belgian coal-basin was found to pass beneath the newer formations westwards in the direction of France, where the Coal Measures were supposed to be lost under these Cretaceous and Tertiary strata. It occurred, however, to some more far-seeing men that they might possibly be recovered, and, after various trials, their search was attended with success, the strike of the coal measures hit upon, and valuable collieries established at Anzin and Aniche, in the neighbourhood of Valenciennes. This encouraged further search, and the coal measures have been gradually followed in a westerly direction under the chalk to within thirty miles of Calais, and at that town a boring for water, made by M. Mulot in 1842, proved the presence of carboniferous strata (but not the coal measures) under the tertiary strata and the chalk, to a depth of 1,138 ft.* Again, under the same strata at Ostend, palæozoic rocks have been discovered at a depth of 985 ft.† On the English coast, a boring for water was carried at Harwich to a depth of 1,070 ft., also through tertiary strata and chalk, which were found to repose upon a fossiliferous slate belonging to one of the lower members of the carboniferous series. We can thus follow at intervals the same Tertiary and Secondary strata overlying *unconformably* Palæozoic strata from Mons to London. The order of superposition and ascertained thickness of the several overlying formations and the nature of the fundamental rock at these points, is shown in the following table :—

	Kentish Town, London	Harwich	Ostend	Calais	Hames, near Calais	Anzin, near Valenciennes	Flines Nord	Ville Ponverneul, near Mons
Tertiary Strata . .	324	76	682	241	6		140	54
Chalk	645	888	210	762	629	265	390	844
Gault and Greensands .	144	61	93	29	33		3	138
	1,113	1,025	985	1,032	668		533	1,036
	OR	ML	OR	ML	OR	C	ML	C

C = *Coal Measures*, ML = *Mountain Limestone*,
OR = *Old Red Sandstone or Devonian*.

There is little doubt from all we now know that, owing to the absence of all the lower secondary formations the great Tertiary and Chalk plains of Belgium and French Flanders repose directly upon a floor of old Palæozoic rocks, and that a like structure obtains in the London basin, at all events as far as London. The reasons we have for believing that the coal measures are associated with this palæozoic base, we will give further on.

In England, south of a line drawn from Bath to Stamford and Yarmouth, no true coal has yet been found. The whole area is occupied by strata newer than the Coal Measures, commencing with the Liassic and Oolitic series to the east of Bath, and ending with the Chalk and Tertiary series of the neighbourhood of London. Nevertheless, in consequence of certain presumed relations between the coal-fields of Belgium and of

* "The Water-bearing Strata around London," p. 208.

† "Bull. Soc. Paléont. de Belgique," Antwerp, 1859. At Vilvorde, near Brussels, Silurian or Devonian rocks were also met with at a depth of about 600 feet.

the north of France with those of South Wales and Somersetshire, an opinion has for some time prevailed amongst geologists that coal probably exists in parts beneath the newer formations of the south of England.

So long since as 1826, Dr. Buckland and Mr. Conybeare, in their excellent account of the Bristol coal-field,* made the following remarks: "Before we close this general account of the south-western coal district of England, we are desirous of noticing its resemblance in geological structure and picturesque features to the country extending along the Meuse between Namur and Liège. There also we are presented with coal basins encircled by Mountain Limestone, and based on Old Red Sandstone, which latter is displayed at Huy. These rocks are all highly inclined, and are covered by overlying formations. The defiles of the Sambre and Meuse present exact counterparts to those of the Avon and Wye." The relation of the two areas was more particularly noticed in the work just quoted (pp. 724-5) by the eminent geologists MM. Dufresnoy and Elie de Beaumont, who thus express themselves on the theoretical question of the original extent of the coal-fields of Belgium and the north of France, and on their probable range:—"The portions of carboniferous strata, which we have reserved as the subject of the last parts of this chapter, contrast in an important manner with those which we have hitherto described, inasmuch as they do not exhibit the characters of deposits formed in circumscribed basins; all indicating, on the contrary, that they have been deposited in an open sea. From the Ardennes on to the mountains of Wales and Scotland, there extended at that period the bays of a sea, in which the Mountain Limestone was formed, which contains a large number of marine remains, and after that the coal measures of the north of Belgium and a part of England."

"This difference in the character of the two classes of basins of the coal measures of which we have just spoken, is not only an interesting fact for science, but it concerns also in a great degree the future of mineral industry, from the views it may suggest relative to the possible subterranean connection between certain basins. In fact, the deposits formed in circumscribed basins present but little chance of their being prolonged for any considerable distance beneath more modern deposits, but the deposits formed in marine basins are generally much more uniform, and susceptible of much greater extension, when they have not been broken up and destroyed."

* "Geological Transactions," vol. i. pl. 2, 2nd ser. p. 220.

Again, M. Mengy, in 1852,* remarks: "However it may be, the considerable thickness of the overlying unproductive strata (*terrains morts*) which exist in the western part of Belgium and Department du Nord, shows that there is a great depression there, which is a prolongation of the deep subterranean substrata above which London stands, and if a coal measure basin exists there, which is not impossible, they may extend to the southern border of that depression, which reaches near to Lille, and may be connected more or less directly with the vast carboniferous system which comes to the surface in England, ranging from Wales to Scotland."

But it was not until 1855, when Mr. Godwin-Austen † brought the question before the Geological Society in an able and elaborate paper, accompanied by a map, in which he showed that the coal measures which thin out under the chalk near Théronanne probably set in again at or near Calais, and are prolonged (beneath the tertiary strata and the chalk) in the line of the Thames Valley, parallel with the North Downs, and continue thence under the valley of the Kennet, into the Bath and Bristol coal area, that the attention of geologists was seriously directed to the subject. Reasoning also on theoretical considerations connected with the extension of the old coal-growth in the west of Europe, Mr. Godwin-Austen concluded that "coal measures might possibly extend beneath the south-eastern part of England," and he showed, upon well-considered theoretical grounds, that the coal measures of a large portion of England, France, and Belgium were once probably continuous, and that the present coal-fields were merely fragments of a great original deposit, which he inferred had been broken up in two directions previously to the deposition of the secondary rocks. He endorsed also the opinion that the main line of disturbance had a general east and west direction—that part of it formed the great anticlinal of the Ardennes, by which the Belgian coal-field had been tilted up, and brought to the surface—and that the Mendips with the Somerset coal-field are on the same line of strike.

These views have been controverted by some distinguished geologists, but they have received the assent of a greater number; and the information we have since acquired of the thickness of the secondary strata and of the existence of palæozoic rocks at Kentish Town and Harwich,‡ and the discussion

* "Essai de Géologie Pratique sur la Flandre Français," 1852, p. 76.

† "Quart. Journ. Geol. Soc.," vol. xii. p. 83.

‡ For detailed particulars of the strata at these wells, see the papers by the writer in "Journ. Geol. Soc.," vol. xii. p. 6, and vol. xiv. p. 249.

of the subject by the Royal Coal Commission,* enables us now to investigate the physical problems with still greater certainty.

It is evident from what we have already said that the age and position of the surface rocks afford no criterion of the thickness of the strata intervening between that surface and the palæozoic rocks underlying the Tertiary and Secondary strata of the south of England; while it is also clear that the relation of the secondary and of the palæozoic group of rocks to one another is perfectly independent, and that the latter must be viewed entirely on their own internal evidence, apart from the bearing of the newer rocks which cover them unconformably.

A glance at the geological map of Europe will show that the Belgian coal-field is but part of a series of great coal-fields ranging from Westphalia to the north of France. These coal-fields are deep, long, and narrow, and their longer axes succeed one to another on the same line of strike.† Omitting a few small unimportant coal-basins, the most easterly of the great coal-fields is known as that of the Ruhr, the second as that of Aix-la-Chapelle, the third as that of Liège, and the fourth as that of Hainaut and Valenciennes. In all these districts the Coal Measures are tilted up or faulted on the south against the Mountain Limestone or the Devonian rocks, or pass northward under Cretaceous and Tertiary strata, beneath which they are prolonged until thrown out by other undulations of the older rocks. The width, north and south, of these coal-fields is always small compared to their length. Thus the coal-field of Liège is only 3 to 8 miles wide, whereas it has a length of 45 miles. So the exposed coal-field of Hainaut, from Namur to beyond Charleroi, is 33 miles long; it then passes under the Cretaceous and Tertiary strata, and is prolonged, with a few small exposures, underground, by Mons to Valenciennes. The length of this other underground portion of the coal-field is 32 miles, making a total of 65 miles, with a width near Namur

* Vol. of Evidence taken before Committee D, and Report to that Committee "on the Probabilities of finding Coal in the South of England" by the writer; Royal Commission on Coal Supply, 1871. Free use is made in this paper of this report and also of the writer's anniversary address to the Geological Society for 1872.

† These are given generally in the map, Pl. LXXXV., the main features of which are copied from Dumont's Geological Map of Europe. To them are added the ascertained range of the Coal Measures under the Chalk of the North of France and their probable range under the South of England. To this I have added a series of hypothetical sections along the line of the coal trough and across it, showing the possible disposition of the Coal Measures and the thickness of the overlying strata.

of 2 miles, increasing to 7 or 8 miles near Charleroi, and continued in France with a width of from 6 to 7 miles, where it has been followed under the chalk to within 30 miles of Calais, and there thins out.

Connected with these coal-fields a great line of disturbance, affecting the palæozoic rocks, has been traced from Westphalia through Belgium to northern France, and it is on the northern flanks of the older rocks of the Ardennes range of hills, which have been formed by this disturbance, that the coal-fields of Belgium lie. The same line of disturbance is again exhibited in the Mendips, and is prolonged even as far as the south of Ireland.

In England and South Wales a similar set of phenomena are met with at this other end of the axis of elevation. From Milford Haven to Tenby, contorted strata of the Mountain Limestone and Old Red Sandstone are flanked on the north by the highly-disturbed Pembrokeshire coal-field, which is 24 miles long by 3 to 6 miles broad. The great coal-field of South Wales is 60 miles long by 15 to 18 miles broad; whilst that of Somerset and Gloucestershire (or Bristol and Bath) shows a length in the direction of the axis of the Mendips of about 12 miles, and in the other direction it measures 26 miles.

The Coal Measures of South Wales are not covered by secondary strata, but a large portion of the Somersetshire coal measures are overlaid by some of the lower Secondary rocks, which in their turn pass a few miles to the eastward under the Chalk. At Clandown, near Bath, the Coal Measures are worked beneath 360 feet of Lias and New Red Sandstone, and they have been followed under these superincumbent strata for a distance of 5 to 6 miles from their outcrop, where they are at a depth of about 500 ft. beneath the surface. But between this point and the well at Kentish Town, no trial for coal or water has been carried to the base of the secondary rocks, or has reached more than about 600 ft. beneath the sea level, and the whole area extending to the channel is occupied by upper Secondary or by Tertiary strata.

There can, however, be little doubt of the continuity of the range of the palæozoic rocks under these newer formations from Belgium to Somerset; but whether or not the Coal Measures were ever continuous between the two districts; and whether, if they were, they have been removed by denudation, leaving only the lower palæozoic rocks, requires further discussion.

So far as the identity of any particular bed of coal or of rock may serve to establish a correlation between the coal measures of Bristol and South Wales and those of France and Belgium, it is not possible, nor should we expect it; for the variation in all the beds of any coal-basin is well known to be so great and rapid, that, in the different parts of the same basin, it is often difficult, and sometimes impossible, to establish any correlation, while in adjacent basins, such as those of Wales and Bristol, or of Hainaut and Liège, such attempts have, with few exceptions, hitherto utterly failed. There are, however, general features which serve to show some relationship. The great central mass of from 2,000 to 3000 ft. of rock called Pennant exists in both the Welsh and Bristol coal-field; and the total thickness of Coal Measures is not very different, being, say, 10,500 ft. in the one, and 8,500 ft. in the other, with workable seams of coal, 76 in Wales, and 55 in Somerset. In the Hainaut (or Mons and Charleroi) basin, the measures are 9,400 ft. thick, with 110 seams of coal; in the Liège basin 7,600 ft., with 85 seams; and in Westphalia, 7,200 ft., with 117 seams. On the other hand, none of our central or northern coal basins, with the exception of the Lancashire field, exceed half these dimensions, and more generally are nearer one-fourth. Further, the difference which exists between the north country coals and those of Wales and Somerset, the preponderance of caking-coals in the former, and of anthracite, steam, and smiths' coal in the latter, equally exists between our north country coals and those of Belgium, which latter show, on the other hand, close affinities with those of Wales and Bristol. I am informed by two experienced Belgian coal-mining engineers and good geologists, who have twice visited our coal districts, that the only coals they found like those of Belgium were the coals of South Wales and Radstock; there was the same form of cleavage, the same character of measures, and the same fitness for like economical purposes. Organic remains afford us little help; and not sufficient is yet known of their relative distribution. The plants, are, as usual, the same; so also are shells of the genus *Anthracosia*, and a number of small *Entomostraca*; and the marine forms are scarcer than in some of our central and northern fields.

That, therefore, which best indicates the relation between the coal-fields of the south-west of England and those of the north of France and of Belgium, is the similarity of mass and structure, uniformity of subjection to like physical causes, and identity of relation to the underlying older and to the overlying newer formations.

These physical features are of much importance and interest. Of the underground prolongation of the axis of the Ardennes through the south of England there can be little doubt; nor can there be much doubt that the same great contortions of the strata which in Belgium folded alike the Coal Measures, the Mountain Limestone, Devonian, and Silurian series, and were the cause of similar folds in the same rocks of Somerset and Wales, were continued along the whole line of disturbance, and that the preservation of portions of the same great supplementary coal trough is to be looked for underground in the intermediate area, just as they exist above ground in the proved area. The intermediate subordinate barriers dividing the coal-basins can, I conceive, in no way permanently affect the great major disturbance, by which the presence of the coal measures is ruled.

Admitting, however, that the coal measures were originally present, the question has been mooted whether they have been removed by subsequent denudation.

It has been urged that the Coal Measures become unproductive, and thin out under the Chalk, as they range from Valenciennes towards Calais, and, therefore, that the coal-trough or basin ends there. It is perfectly true that the Coal Measures do thin out between Béthune and Calais, but not in the sense of their dying out owing to their deposition near the edge of a basin. In that case, each seam, each stratum would gradually become thinner and disappear, but such is not the fact. None of the beds of the Belgian coal-field are thick; the average does not exceed 2¼ft. At Valenciennes it is the same; whereas M. Burat states that the mean thickness of the beds actually increases westward of Béthune to more than 2½ft. With respect, also, to the extreme end of this basin, the lower beds there brought up correspond with the bottom beds of the Hainaut basin, where the lower 650 feet consist altogether of unproductive measures. The thinning-out is, in fact, due to denundation, just as the Bristol coal-field thins out at Cromhall to resume in the Forest of Dean, or the coal-field of Liège thins out at Nameche to resume at Namur in the great fields of Charleroi and Mons.

The deterioration of the coal in the small coal-field of Hardinghen, near Boulogne, has also been adduced against the occurrence of workable coal in south-eastern England; but Mr. Godwin-Austen has shown that this Hardinghen coal-field is one of those small local developments of coal-bearing strata intercalated in the Mountain Limestone, and is of older date than the great Belgian coal-field. It has, therefore, no bearing on this part of the question.

Another objection, to which much weight has been attached, is, that as the coal-field of Bath and Bristol forms an independent basin, cut off both on the east and on the west by ridges of Millstone Grit and Mountain Limestone, we have there reached the eastern boundary of the coal measures. It is probable that such a bounding ridge does exist, though, as the edge of the basin is there covered by secondary rocks, there is all the beds of any coal-basin is well known to be so great and rapid, that, in the different parts of the same basin, it is often difficult, and sometimes impossible, to establish any correlation, while in adjacent basins, such as those of Wales and Bristol, or of Hainaut and Liège, such attempts have, with few exceptions, hitherto utterly failed. There are, however, general features which serve to show some relationship. The great central mass of from 2,000 to 3000 ft. of rock called Pennant exists in both the Welsh and Bristol coal-field; and the total thickness of Coal Measures is not very different, being, say, 10,500 ft. in the one, and 8,500 ft. in the other, with workable seams of coal, 76 in Wales, and 55 in Somerset. In the Hainaut (or Mons and Charleroi) basin, the measures are 9,400 ft. thick, with 110 seams of coal; in the Liège basin 7,600 ft., with 85 seams; and in Westphalia, 7,200 ft., with 117 seams. On the other hand, none of our central or northern coal basins, with the exception of the Lancashire field, exceed half these dimensions, and more generally are nearer one-fourth. Further, the difference which exists between the north country coals and those of Wales and Somerset, the preponderance of caking-coals in the former, and of anthracite, steam, and smiths' coal in the latter, equally exists between our north country coals and those of Belgium, which latter show, on the other hand, close affinities with those of Wales and Bristol. I am informed by two experienced Belgian coal-mining engineers and good geologists, who have twice visited our coal districts, that the only coals they found like those of Belgium were the coals of South Wales and Radstock; there was the same form of cleavage, the same character of measures, and the same fitness for like economical purposes. Organic remains afford us little help; and not sufficient is yet known of their relative distribution. The plants, are, as usual, the same; so also are shells of the genus *Anthracosia*, and a number of small *Entomostraca*; and the marine forms are scarcer than in some of our central and northern fields.

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Another objection, to which much weight has been attached, is, that as the coal-field of Bath and Bristol forms an independent basin, cut off both on the east and on the west by ridges of Millstone Grit and Mountain Limestone, we have there reached the eastern boundary of the coal measures. It is probable that such a bounding ridge does exist, though, as the edge of the basin is there covered by secondary rocks, there is some uncertainty about the disposition of the palæozoic rocks under them. Admitting, however, the basin to be complete and isolated, that is no proof that the older Palæozoic rocks prevail exclusively to the east; for the coal measures of the Somerset basin maintain their full development to the edge of the basin, and are there cut off by denudation, and not brought to an end by thinning out. They form part of a more extended mass, of which we have there one fragment, while on the west another portion exists in the Welsh basin, and another in the newly-discovered small basin of the Severn valley; and there is no reason why on the east the same disposition should not prevail.

The Severn Valley basin is entirely covered by the New Red Sandstone; and as the Welsh basin is bounded on the east and the Bristol basin on the west by Mountain Limestone, the same objective argument might have been used in either case to show the impossibility of Coal Measures occurring in this intermediate area, or of their extending beyond the boundaries of either great basin.

But the fact is, it is the very nature of the great line of disturbance to have minor rolls and flexures of the strata at, or nearly at, right angles to it, and so causing breaks in the coal-trough, which would otherwise flank it without interruption; thus the Aix-la-Chapelle coal-field is separated by older rocks from that of Liège, which is again separated by a ridge of Mountain Limestone from that of Hainaut. So, in the case of south-western England, we have the separate basins of South Wales, Severn Valley, and Bristol—the extremes of the intervening belts of older rocks being two miles at Nameche and eighteen miles in Wales. These barriers are clearly only local; and the division of the coal measures into separate basins appears to be their ordinary condition along this great line of disturbance. The length of the two known portions of the axis included between Pembrokeshire and Frome, and between Calais and Westphalia, is 472 miles; and in this distance we find eight separate and distinct coal-fields. The combined length of these eight coal-fields is about 350 miles, leaving about 122 miles occupied by intervening tracts of older rocks; so that nearly three quarters of the whole length is occupied by coal strata. I consider that a structure which is constant above ground, so far as the axis of disturbance can be traced, is, in all probability, continued underground in connection with the range of the same line of disturbance; and I see no reason why the coal-strata should not occupy as great a proportional length and breadth in the underground and unknown as in the above-ground and explored area. It is certain the basin-shaped form of the Somerset coal field is no reason why other coal basins, fragments of the same great original trough, should not exist underground between Somerset and the north of France and Belgium.

With respect to the possibility of denudation having removed the intervening Coal Measures, enormous as the extent of denudation must have been previously to the deposition of the Permian strata, we cannot admit its exceptional action in this case. Denudation has removed from the crest of the Mendips a mass of strata possibly equal to two miles or more in height, and from that of the Ardennes as much as three or four miles; and it has also worn extensive channels between many of our coal-fields; so that the power of such an agent cannot be denied. (See Sect. Plate LXXXV.). But it is a power of planing down exposed surfaces rather than of excavating very deep troughs. Notwithstanding the extent of its action on the Mendips and Ardennes, deep troughs of coal measures are left flanking their northern slopes. These troughs descend to more than a mile beneath the level of the sea; and I do not think it probable that the intermediate underground portions of the trough through South Eastern England, where the axis lies lower, have suffered more than those on the higher levels, except to the extent caused by the later denudation which preceded the Cretaceous period. But this would not affect the main bulk of the coal measures. The Belgian coal-field, which was exposed to the action of both these denudations, still retains vast proportions.

At the same time the pre-cretaceous denudation was very irregular in its action, giving rise to hills and plains. At one place near Mons the chalk and tertiary strata are above 900 ft. thick; whilst at another, on about the same level, and only a few miles distant, they are not 100 ft. thick—an old underground hill of highly inclined coal measures rising in the midst of the unconformable newer strata, and giving rise to this difference. This shows that in the English chalk area we may possibly find irregular old surfaces of this kind, so that the coal measures may exist at places nearer the surface than we have estimated.

We have alluded before to the great length and small width of the Belgian coal-fields. That of Liège is forty-five miles long, with a mean width of less than four miles, whilst that of Hainaut and Valenciennes, with a width scarcely greater, is 119 miles long. The presence of lower carboniferous rocks so far north as Harwich, and the extent of north range of the Bristol coal-field, render it possible that the coal trough in the intermediate area may have a greater expansion than in Belgium; but we have nothing else to guide us, unless it be that the lateral pressure in the intermediate ground was less than in the Ardennes and the Mendips, where it has exercised its maximum elevatory force. In that case the coal trough in this intermediate area would be less compressed and more expanded, and we might consequently look to find larger coal-basins than those of either Somerset or Liège.

The strata on the south side of the Liège coal-field rise abruptly against highly inclined and faulted Devonian rocks, and on the north side they rise, at a less angle, beneath Cretaceous or Tertiary strata; and to the westward the great palæozoic axis of the Ardennes, consisting of Silurian and Devonian rocks, Mountain Limestone, and Coal Measures, passes westward under the Chalk of the north of France, and has been followed underground as far as Calais, where it lies at a a depth of 1,032 ft.; while in the direction of Boulogne the old rocks keep nearer the surface, crop out from beneath the chalk downs surrounding the Boulonnais, and disappear near the channel under an unconformable series of Jurassic and Wealden strata.

We may, I think, look for a prolongation of this old palæozoic surface of highly inclined, contorted, and faulted rocks at no great depth under the same Cretaceous and Tertiary area of the south-east of England. For, although the old palæozoic surface descends rapidly from 200 ft. above the sea-level in the Boulonnais to 1,032 ft. below it at Calais, it rises at Ostend 47 ft. higher than at Calais, and, crossing the Channel, it is found at Harwich within a few feet of the same depth as at Calais, from which it is eighty miles distant in a northerly direction. Passing westward, we find the palæozoic rocks under London, 105 miles distant from, and 102 feet higher than under Calais, and 106 feet higher than at Harwich. Allowing for irregularities of the old surface as evinced by the well at Crossness, near Plumstead, which was still in the Gault at a depth of 944 feet, or some 14 feet below the level of the palæozoic rocks at Kentish Town, we may still consider that in the area between these three points, and other parts on the same range of the south-east of England, the palæozoic rocks will probably be found not to be more than from 1,000 to 1,200 ft. beneath the sea-level.

Projecting the line another 100 miles westward, we reach the neighbourhood of Bath and Frome, where the coal measures are (as before mentioned) lost, at a depth of about 500 feet, beneath Liassic and Jurassic strata. In the intermediate area between that place and London no trial-pits and no wells have been carried to a depth of anything like 1,000 feet beneath the sea-level. The deepest well with which I am acquainted is one near Chobham, in Surrey, through tertiary strata and chalk to a depth of about 800 feet, or 600 feet beneath the sea-level.

There are, however, in all this area certain indications of the proximity of old land and of pre-cretaceous denudation, in the presence of Quartz and Lydian-stone pebbles, accompanied by extraneous secondary fossils in the Lower Greensands of Surrey, and in the like old-rock pebbles, with the addition of slate-pebbles, in that formation in north Wiltshire; while the banks of shingle, bryozoa, and sponges of the same age at Faringdon, point to still and sheltered waters, probably of no great depth, and to adjacent dry land. Again, on the north of London, we have in the Lower Greensand of Buckinghamshire and Bedfordshire shingle-beds consisting almost entirely of fossils derived from Jurassic strata, with a remarkable collection of larger quartz, quartzite, and other rock pebbles, probably from the old palæozoic axis, which at first stood out in the midst of the Lower Secondary seas, and was only finally submerged in the seas of the Gault and Chalk periods. It was no doubt owing to the gradual shallowing of the old seas as they approached the then palæozoic land that the thinning out of the Lower Secondary rocks from the north-west to the south-east, which we before noticed, is owing.

In this country the newer strata, overlying the palæozoic rocks on parts of the presumed old palæozoic range, have been sunk into without result—in the Wealden at Hastings to a depth of 486 feet, in the upper beds of the same at Earlswood, near Reigate, to about 900 feet, through chalk at Chichester, to 945 feet, and at Southampton, through Tertiary strata and chalk, to a depth of 1,317 feet. Unfortunately all these works fall short of the mark which we as geologists wish to attain.

In a scientific point of view, no experiments could have greater interest, and in an industrial point of view no experiments could be more important, than such as would serve to determine the position of this great underground range of older rocks connecting the Ardennes and the Mendips. We have ascertained that it lies at no great depth beneath the overlying newer strata, and if the strike of the line of disturbance were in a straight line, we should have no difficulty in determining its course; but from what we know of its range in the proved part of the 800 miles, it is certain that while it has a general east and west bearing, it is subject to considerable local deflections. Thus, while Mr. Godwin-Austen is disposed to place the supposed coal trough in the Valley of the Thames or under the North Downs, I am disposed to place it further north, in Essex and Hertfordshire; and while my friend considers it continuous, I consider it to be most likely broken up into basins. Again, if the axis of the Ardennes consisted of an anticlinal line, the problem would be simplified, but it consists of a series of such parallel lines, and therefore whether or not the one which traverses the Boulonnais and is probably prolonged under our Wealden area, is one of the many central ones, or the lateral one immediately flanking the coal trough, is uncertain.

Any attempt made to solve this great problem must be hailed with satisfaction, and we therefore look upon the trial about to be made by the Sub-Wealden Exploration Committee as a most important step in this direction. The site selected for the experiment near Battle is on the line of centre of the Weald, where the lowest Wealden beds come to the surface, and it is no doubt on or near the line of continuation of the axis of the Boulonnais. We may, therefore, there expect to meet with a prolongation of that axis consisting either of the Mountain Limestone, with the subordinate coal strata of Hardinghen, or the Devonian Limestone and grits. Had it been merely a trial for coal another site might have been selected, but the object the Committee has in view is one purely scientific, to determine the thickness of the Wealden and underlying secondary strata, and the depth, nature and position of the underlying palæozoic rocks; and it has been suggested and planned in honour of the next meeting of the British Association at Brighton —of which meeting it will be a worthy memorial. It is not by one experiment, however, but rather by several, that the important question of the line of the great trough of productive Coal Measures will be determined. This experiment will probably be but one of a series. We hope to learn by it the direction of the strike of the older palæozoic rocks, and then judge of the bearing and probably position of the coal trough in relation thereto. No insurmountable difficulties present themselves. The starting-point is the lowest in the series of the known rocks of the south-east of England, and if it should prove to be on the crest of the prolonged ridge of the Boulonnais, the other secondary strata may be of no great thickness. But this is entirely a matter of experiment. We know not what may be the thickness of the remainder of the Wealden, and if beneath that we find the Purbeck and Portland beds, the Kimmeridge clay, and some of the Oolitic series in place, as we do in the Boulonnais, we know not what thickness they may attain.*

* The thickness of the strata may vary within the following limits:

	Probable Minimum Feet.	Probable Maximum Feet.
Asburnham Beds	50	50
Unproved Wealden	100	300
Purbeck and Portland	30	50
Kimmeridge Clay	300	800
Oxford Clay	200	400
Oolites	50	100
	730	1700

PALÆOZOIC STRATA to be proved beneath these beds.

The Committee wisely contemplate the possibility of having to go to a depth of 1,500 to 2,000 feet, and will, we trust, take secure steps against such a contingency. At the same time we hope (and it is perfectly possible) that before they reach half that depth, the very interesting object of which they are in search may meet its solution, and that one more positive fact may be acquired to science in addition to those so fortunately furnished by the trial works at Kentish Town and Harwich.

BUD VARIATION.

By MAXWELL T. MASTERS, M.D., F.R.S.

THE reproduction of plants is effected in one of two ways, either by the contact of one elementary organism with another of a different kind, in consequence of which a spore or an embryo is formed, which ultimately developes into a perfect plant, or by the production of buds.

The word bud is here used in a broad sense to express any separable portion of a plant, not produced by sexual agency, and which when separated has the power of growing into an organism like the parent plant. The process of bud-formation then, reduced to its simplest expression, is a process of segmentation, or subdivision. Illustrations are to be found throughout the whole vegetable kingdom, but in no family are they more frequent, or do they play a more important part, than in the great group of the Fungi, among which are the moulds and blights so destructive to the higher plants on which they grow. One of the most remarkable circumstances about these plants is the varied manner in which they are reproduced. Spores, or reproductive bodies of four, five, or more shapes are met with at different times on the same plant, and, inasmuch as they are often formed at various times and under diverse conditions, it is no matter for surprise that they should have been assigned, not to the same plant, but to different ones, and hence each one has had the misfortune of being separately named.* Now, thanks to the labours of those who have, with infinite skill and patience, succeeded in unravelling the life-history of these plants, all these varied forms are known to be different states of the same plant. Of these spores some are true reproductive bodies in the sense already explained, while others are buds extending and multiplying the plant, but not reproducing it.

We do not know in all cases, indeed we only know in a few,

* See a paper on the subject of Polymorphism in Fungi, in "Popular Science Review," Jan. 1871, y Mr. M. C. Cooke.

the complete life history of these plants and the particular office the bud-like formations fulfil. For our present purpose, however, it will suffice to say that they vary in size, form, and apparently in the conditions under which they are produced. In spite of these diversities, we know that they develop into organisms precisely like those from which they sprung.

Among the sea-weeds the same state of affairs exists; there are true spores and bud-spores, and these bud-spores vary in character on the same plant at different times and in different seasons.

In the Lichens we have a similar formation of true spores and bud-spores, but so far as is at present known, there is not the same diversity in the bud-spores, or "gonidia," of Lichens that there is in the other groups. There is, however, this difference; the bud-spore of the lower plants consists of a single cell, whereas in the Lichens it is made up of several cells: it is an aggregate, not a unit.

In Hepaticæ and Mosses the bud-spores are like those of Lichens, but more highly-organised. In the case of the Ferns and Equiseta there are buds very nearly like those of flowering plants, consisting of a number of minute scales, the outer of which remain scaly, and ultimately perish; the inner gradually develop into leaves, while the central pimple of cellular tissue from which these scales emerge lengthens into a shoot, that shoot into a branch, and so on.

Moreover, that bud, if separated and placed under proper conditions, will form a new plant.

In this way the gardener prepares his cuttings. He takes a "slip" with a bud attached, places it in moist earth, covers it with a bell-glass to prevent undue evaporation, and places it in a sufficiently warm locality. After a time the cutting "strikes," as it is termed; that is, it forms roots, which roots absorb nourishment. The cutting is thus truly a chip of the old block. That which the gardener does by art Nature herself often does unassisted. Many Begonias form buds from almost any portion of their surface, and in prodigious numbers, recalling the way in which similar buds are formed on the Mosses, but in even greater profusion. Other illustrations may be seen in the little bulbs which beset the stalk of the tiger-lily, or protrude from the margin of the leaf in *Bryophyllum*. This process of bud formation occurs also, to some extent, in the animal kingdom, as among the hydras, but is by no means of such general occurrence as in plants.

Under ordinary circumstances all the buds on any particular plant are in all material points alike, and the shoots resulting from those buds are also alike. There are differences in size and vigour and what not, for no two are precisely alike any more than any two sheep in a flock, or any two peas in a pod, are precisely alike; still, for general purposes, we may say that all the buds and all the shoots from those buds are alike. To such an extent is this true that it is the general practice amongst gardeners to propagate, by means of cuttings or grafts, any particular variety they may be desirous of perpetuating, because reproduction by seed does not offer the same certainty of reproducing the particular quality required as propagation by buds does. But it now and then happens that one or more buds on a particular plant, and one or more shoots, are not like the rest, and then we have what in garden phraseology is termed a "sport," but which is more correctly styled a bud-variation.

We propose to cite sundry selected illustrations of this phenomenon, with a view to show how wide the range of variation may be, and in what different ways it may manifest itself. The simplest case, because it involves no appreciable change of form, is that in which a single bud, or a collection of buds in one particular part of a plant, is more precocious in its development than the others on the same tree. Instances of this kind are not uncommon. The buds on one particular branch may be each year considerably in advance in point of development of their neighbours, and this without there being any appreciable reason, such as more perfect protection or shelter on one side than on another. Thus we have seen two shoots of red currants taken from the same branch: on the one spray the flowers were ten days earlier in point of expansion, the new shoots being as much as 6 in. in length, while on the other spray the buds were only just expanding. With reference to this point, it may be remarked that the same phenomenon occurs in the case of seedling varieties. There are certain horse-chestnuts—some of which have almost historical fame, such as the Marronnier du Vingt-Mars in the Tuileries Gardens —which are year by year several days in advance of their kind in their development. But the circumstance of the whole organism exhibiting this precocity is not so striking as is the early development of one particular branch or set of branches, as compared with the rest.

In point of size, whether increased or diminished, there is often great difference in the different branches of the same tree. For some reason or other—what, no one knows—the shoots on a particular branch, instead of lengthening as the rest do, remain stunted and dwarfed. Several curious garden varieties of firs, such as the Clanbrasilian fir, have originated in this way, and are reproduced or propagated by cuttings or grafts at the will of the gardener. The birch affords frequently illustrations of this phenomenon, in the form of those tufted agglomerations of contracted shoots so strikingly resembling birds' nests. A similar occurrence is not uncommon in the wild cherry; and a correspondent—Mr. Webster, of the gardens, Gordon Castle—informs us that he has observed similar growths in the common laburnum, in the Wych elm, and in the Scotch fir. Sometimes the determining cause may be discovered in the shape of an insect or fungus, but in this case the unusual condition ceases with the destruction of the impeding cause, whatever it may be, and the condition cannot then be perpetuated by the art of the gardener.

Variation in the colour of certain leaves or flowers is an equally common occurrence, and is perhaps more easily understood. Each individual cell, to a large extent, lives independently of its neighbours, and the secretions it forms and deposits are very often different from those of adjoining cells. Colouring materials, especially fluid ones, are notoriously liable to be formed in isolated cells. Again, variations in colour so often depend on the mere superposition of cells containing material of different tints, that the changes met with, though striking to the eye, do not seem to indicate so complete a change as in the case of alterations of form or size. Very many of the variegated Pelargoniums, so fashionable now-a-days, have originated as "sports" from some previously existing variety. The intrinsic change between some of these varieties, even where apparently very considerable differences exist, is, in some instances, very slight.

A marked difference in the amount and quality of the pubescence is not unfrequently manifested in some of these cases of bud variation. A plant which ordinarily has its leaves and its younger branches invested with a coating of hairs (epidermal appendages), all on a sudden produces a shoot on which the leaves are destitute of such clothing, or *vice versâ*. Some of the moss roses have originated from plain-leaved varieties in the manner just indicated.

But of all these cases the most striking are those which involve a change of form. We see, for instance, not unfrequently a particular branch bearing leaves very different from those on the rest of the tree, so different that but for their production on one and the same tree, the observer might readily take them to belong to different species. Many trees now cultivated for ornamental purposes have originated in this manner, such as the cut-leaved beech, the oak-leaved laburnum, and very many more, commonly to be found in plantations. Very often the whole "habit" or aspect of the tree is altered by these variations: thus many of the so-called "weeping trees" have sprung from a solitary branch of a tree which presented a pendulous character. Some trees, it may be remarked, naturally produce leaves of very different forms: especially notable in this respect is the Euphrates Poplar, *Populus euphratica*, supposed with reason to be the willow mentioned in the Psalms. Occasionally the variation is confined to one half of the leaves. A remarkable instance of this kind has been noted by A. Braun in a species of *Irina*, where one half of the leaf was undivided, the other deeply gashed into narrow segments.

The history of these variations is pretty much the same in all cases. All on a sudden a tree, which heretofore has produced shoots and leaves of the usual character, emits shoots with leaves of a totally different form. If they be such as the cultivator thinks likely to serve his purpose, he takes care to propagate them by means of grafts or cuttings. Sometimes variations of this character may be reproduced by seed, but there is little certainty as to this. The same kind of variation occurs in flowers and fruits. In the former it is usually associated with distinctly recognisable alterations in the phenomena of reproduction, as in what are spoken of as dimorphic or trimorphic flowers, some instances of which have been so carefully investigated by Mr. Darwin. To this latter class of bud variation we shall do no more than make passing allusion, but there are other cases which have apparently no relation to variations in the phenomena of fertilisation or reproduction, and which are strictly analogous to those already mentioned as occurring in the leaves. Every now and then, for instance, two roses of different forms and colours will be met with on the same stalk, such as a white moss rose in association with a pink one of a different form and destitute of mossy appendages. We have in a former paper in this Journal referred to some of these cases and to the famous *Cytisus Adami*—a laburnum bearing yellow and purple flowers as well as leaves of different character—and have also alluded to the alleged causes of these strange phenomena, on which account it is not necessary now to do more than refer to them. What is a rare occurrence in the rose, and is only known in one or two species of laburnum, is comparatively common in the chrysanthemum. There are indeed particular varieties of this favourite autumn flower which are specially liable to produce flowers of different characters on the same branch. Generally speaking, but by no means always, the change is confined to the colour of the flower only, and colour, as we have seen, is proverbially fickle in flowers. Among commonly cultivated plants azaleas and camellias are peculiarly liable to "sport." In the former plants indeed one may often witness much variation in the shape and colour of individual blossoms, and very frequently parti-coloured flowers and others intermediate between extreme forms. In the case of the fruit similar variations occur—peaches and nectarines on the same bough; black and white grapes in the same bunch; gooseberries of different kinds on the same bush; pears, apples, or cherries, of different shapes, colour, and flavour, on the same bough. All these are, though of course rare, yet familiar occurrences to those on the look-out for such phenomena. It is necessary in some of these cases to investigate closely to see whether or no grafting of different sorts on one stock has not taken place. No doubt some of these cases, recorded by lovers of the marvellous, were simply cases of adhesion or inoculation, but, allowing for these, there still remains a large number which cannot be explained by any such process.

The above-cited illustrations might be largely added to were it necessary to do so. Mr. Darwin's work on "Animals and Plants" contains allusions to many others, and includes many references to the literature of the subject. The horticultural journals, British as well as foreign, contain very numerous records of such cases; * but we have cited enough for our present purpose, and may now pass on to the discussion of some of the alleged causes of the phenomena in question.

It must first of all be premised that these bud variations are not necessarily to be considered as malformations. Their organisation is often perfect, they are not distorted, they are simply variations; and next, they occur not exclusively in plants that have been long subjected to cultivation, but also in wild plants. Now plants that have been long in cultivation have for the most part been hybridised or "crossed" over and over again. Thus in the case of the pelargonium, it is supposed that all the immense number of different kinds now in cultivation have originated from two or three species. These have been hybridised or crossed, their offspring has been crossed in the same way, and so in the pelargonium of the present day we have a plant which has, so to speak, a great deal of very confusedly mixed blood in it.

Bud variation is very often only a reversion—a harking back—to the characters possessed by the parent; it is the result, as the phrase goes, of a *dissociation of hybrid characters*, the consequence of a sort of filtration by which the constituent elements become separated from their previous admixture.† This reversion may be proximate, just as you may see in a family of children that, while most of them resemble both

* A list of many such instances may also be found in M. Carrière's "Production et Fixation des Variétés."

† The papers of Naudin, Braun, Rejuvenescence (Cytisus Adami), and Duchartre, Note sur le Chasselas Panaché, in the "Journal de la Société impériale et centrale d'Horticulture," 1865, should be read in reference to this part of our subject.

parents, some are like the one or the other, while some again present little likeness to either parent, but reproduce the lineament of some remote ancestor. A singular illustration of this phenomenon was brought under the writer's notice by Mr. Wills, and in which two plants of pelargonium showed the characters of three separate ancestors; the exact lineage of one was not fully known, but the history of the other was definitely recorded. The plant in question presented, after the fifth generation by seed (and not till then), various branches bearing leaves undistinguishable from those of the varieties known as "Unique," "Beauty of Oulton," and "Italia Unita"—three very distinct varieties, each of which were known to have been at some time or another ancestors of the plant in question, either as furnishing pollen or as the seed-parent.

Another plant of mixed origin, after retaining its characters for three years, suddenly produced branches some of which had leaves of the form and colouration of those of "Beauty of Oulton," the original seed-parent, while the remainder were bedecked with leaves in all respects similar to those of "Lucy Grieve," the ancestral pollen or male parent. The two varieties in question are widely different. In the cases just alluded to there was not a mere change of colour—an affair of comparatively minor importance—but there was a change of configuration and substance. Other cases of a similar nature have been recorded by various observers, amongst others by Mr. Grieve, the raiser of the popular "Mrs. Pollock" pelargonium.

Of course any plant produced from seed requiring for its development the contact of the pollen tube with the ovule or the germinal vesicle must be held to have mixed characters, and more markedly so in the case of unisexual flowers, either monœcious or diœcious. From this point of view a case lately recorded by Mr. Meehan becomes very significant. That gentleman relates that he obtained cuttings from *Cuphea leiantha*, a diœcious plant, producing its male and female flowers on different individuals. It is not stated whether the cuttings were taken from a male or a female plant, but it is stated that some of these cuttings produced male, others female, plants, and yet all were taken from a plant of one sex only. So, too, it is well known that certain unisexual trees will in some seasons produce male flowers only, in other seasons female flowers only, and *vice versâ*.

To enter into questions relating to the sexuality of plants would, however, lead us too far. We merely now indicate the facts, as proofs of the composite character of the plant.

But dissociation of mixed characters will not account for all the cases of bud variation. Very often we have no evidence at all of previous hybridisation or crossing; or, even where such has existed, the form produced is not like that of either of the supposed progenitors.

Such cases as the fern-leaved beech do not seem explicable by either hypothesis. The sugar-cane, which rarely if ever flowers, and hence offers no opportunity for hybridisation, nevertheless produces new varieties by means of bud variation. Potato tubers, again, vary greatly often on the same plant, but these may be the result of former crossing. A case related by Mr. Meehan, in the sweet potato (*Convolvulus Batatas*), is, however, not open to this objection. The plant in question, it appears, never flowers in the Northern States of America, and yet it has been known to produce tubers of two distinct varieties—the "Red Bermuda" and the "White Brazilian"—on the same root.

Reversion to an ancestral condition is a still more hypothetical cause than dissociation of mixed characters, as we have scarcely ever any means of knowing what the assumed condition was. We have, therefore, to look to other causes. We shall not advance matters much by attributing the changes in question to an innate tendency to vary possessed by buds as well as by seedling plants, which are, in so many respects, analogous with buds. Doubtless there is such a tendency, but we want to get at the "why and wherefore" of the proclivity. The following illustrations may in some slight degree furnish a clue to the attainment of the desired end. In the first place we must not overlook the circumstance that, under ordinary conditions, the several organs of plants often vary according to the part of the plant upon which they grow. Botanists recognise this when they give different names to the root-leaves, stem-leaves, floral-leaves, bracts, &c. Again, there are such cases as the seedless barberry. This plant can be propagated by cuttings, and its seedless condition can be thus perpetuated; but if the plant be multiplied by suckers or shoots thrown up from the underground stem, the fruits produced have seeds as usual. This is an evidence of a difference in the internal organisation of different parts of the same plant. Another illustration of a similar character lately came under observation, in which a sucker from the root of the tree of heaven, *Ailanthus glandulosa*, produced egg-shaped leaves and a dense cluster of flowers while only a foot or so in height (see fig. 1), the ordinary habit of the tree being to grow for several years before flowering, to form a lofty stem, and to produce large compound pinnate leaves like those of the common ash. This, in gardening phrase, would be a "sport," but it is clear it had nothing to do with hybridisation, the form produced being unlike that of any other allied plant. Moreover, there is no evidence to render the occurrence of hybridisation in this particular case at all probable. We can only attribute it to a

FIG. 1.

difference in the organising force manifested in certain parts of the plant as contrasted with others.

Of a similar character are the observations made by practical gardeners as to the difficulty, and in some cases impossibility, of perpetuating a variegated condition of the leaves by dividing the root; plants so produced having green leaves. A French nurseryman, M. Lemoine, notes this in the case of variegated pelargoniums, and in certain forms of *Symphytum* and *Phlox*, and his experience tallies with that of English cultivators. Again, in the common practice of budding roses, if the bud be taken from a long rampant "gross" shoot, with a great tendency to form leaves and little tendency to produce flowers, the bud, transferred to its new home, will reproduce the undesirable characters of the parent shoot: hence the care requisite in budding to take buds from short-jointed flower-bearing shoots.

A similar precaution is exercised by gardeners in the case of fruit-tree grafts.

The different forms which plants assume at different stages of their existence under normal circumstances must also be taken into consideration in speculating on the origin of bud variation. A large number of plants do not immediately assume their wonted habit, they pass through an intermediate stage or stages. This is particularly observable in the case of Conifers, the juvenile state of which is often very different from the appearance presented in the adult state. It now and then happens that, after a plant has lost its youthful characters and assumed its full-grown developement, sundry branches, for some unknown reason, revert to the infantile form. In the common ivy we have a familiar illustration of a similar phenomenon. When the plant is about to produce flowers it assumes an erect bushy habit, its leaves alter in form, indeed its whole aspect becomes changed. If now such branches be taken off and propagated, the characteristic form remains as in what are called tree ivies. If the life history of such a plant were not known, the bud variation just mentioned would appear even more inexplicable than it now does. Again, the leaves and flowers produced on the same plant at different seasons are often naturally different. Dr. Balfour has lately called attention to a remarkable instance of this phenomenon in a species of hawkweed, *Hieracium*, which presents three distinct forms according to the season at which it flowers. Occasionally even a casual observer is struck by the appearance of a second or even a third crop of flowers on laburnums, or pear-trees, Wistarias, and others. In such instances it will generally be found on examination that the adventitious flowers spring from buds which under ordinary circumstances never produce flowers, but only leaves, or that they are placed on portions of the tree usually devoted solely to the production of leaves. How much the aspect of the tree is altered in such cases may readily be surmised: the casual spectator cannot fail to notice it, but the explanation of the phenomenon rarely strikes him.

As might have been anticipated, a change in the external conditions under which a plant lives will often cause very considerable variation in its form: thus a species of fig, *Ficus stipulata*, is commonly grown on the walls of hot-houses, to which it clings ivy-fashion. The same plant grown as a standard in a pot has a totally different appearance. On the wall it has small thin leaves, and it clings to its surface like a large moss or a miniature ivy. Planted out it forms a stout bushy shrub with large coarse leathery leaves, so different from those formed when the plant is growing against a wall that no botanist unacquainted with the history of the plant would hesitate for a moment in ranking it as a distinct species. Some of the Marcgraavias present similar phenomena. In both the plants just named the writer has seen on plants growing against a wall shoots produced of the character of those formed by the plant when growing unsupported. The inference from these facts is that what we call "sports" or bud variations are often only exceptional illustrations of a normal tendency—exceptional in so far that they are manifested at unusual times and places and under unusual conditions.

The individuality or comparative independence of buds—a circumstance often noticed—is also brought prominently into view by such facts as we have recorded. An interesting question arises as to whether there are differences in the plant originating from bud variation as compared with one the produce of variation from seed. It is a matter of every-day experience with gardeners that seedling plants vary greatly—even though the produce of the same seed-vessel, and even though not the offspring of hybridized or cross-fertilised parents. Is there any perceptible difference between a seedling variety obtained as just explained and a bud variety? In other words, are there any means of distinguishing, in the case of a cultivated plant of unknown history, a "sport" from a "seedling"? We have tried in vain to find any such difference. The experience of the most able cultivators furnishes no data on this head. But although this is so, there is an equally prevalent impression that while a variety cannot always be perpetuated "true" from seed, it can be propagated unchanged by cuttings or grafts. The best varieties of apples or pears—to cite only one instance—are propagated by grafts, because there is no certainty at all that the pips will reproduce the desired variety; far more commonly they produce something else. There is, then, a difference between seed variation and bud variation, in the greater degree of permanence of the latter. That this difference is not absolute is shown by the following case recorded by M. Rafarin in a French horticultural journal.

"In 1866, at La Muette, a pelargonium with pale rose-coloured flowers was observed to bear a branch, all the flowers on which were of a deep red colour. Cuttings were taken from this 'sport,' from which 20 plants were raised, which flowered in 1867, when it was found that scarcely two were alike. Thus while some bore rose-coloured flowers like those of the original plant, others had red flowers, like those of the 'sport'; others again had red and rose-coloured blossoms on the same plant and even in the same truss. Nay more, even the petals partook of the parti-coloured nature, for in the same flower were petals of a rose, or a red colour, or of a blended hue. Unfortunately neither the name of the variety nor its genealogy are given, so that we are unable to say positively whether this was a case of reversion or no."

It may perhaps be said that seedling variations such as happen in the apple or the pear are the necessary outcome of the cross-fertilisation to which the plants in question have been subjected for ages, just as the bud variations in the case of the pelargonium are. This may be true in some cases, but can hardly be so in all: for instance, in a bed of seedling conifers, such as Lawson's cypress or deodars, raised from imported seeds taken from wild plants, often from the same cone, the number of seedling varieties is often large. In the case of cultivated plants as of domestic animals, Mr. Darwin has shown how the variations that arise are directly connected with the objects for which the particular plant or animal is cultivated. A plant, for instance, grown for the sake of its fruit is apt to vary in its fruit characters more than in its leaf characters. But although this may and no doubt does apply to a considerable extent in the case of seminal variations, it seems less applicable in the case of bud variations, as will be judged from the illustrations before given, as also from the negative evidence afforded by a plant like the Jerusalem artichoke, which is propagated exclusively by its tubers, and indeed never ripens its seed in this country, and which has produced no variation by "sport" or dimorphism, although so largely grown and for so long a period.

Mr. Darwin attempts to explain the phenomena of bud variation, as of inheritance and reproduction generally, by his hypothesis of pangenesis. This hypothesis proceeds on the assumption that every cell of a living organism gives origin to an innumerable host of "gemmules" in minuteness as in number transcending conception. These gemmules divide and multiply, or they lie dormant possibly for ages. They circulate throughout the organism or they become aggregated together, and so form embryos or buds, and they are transmitted from one generation to another. There is nothing improbable in the assumption of the existence of these gemmules; and, if we take their presence for granted, it is easy to see how they afford an explanation of the phenomenon of reversion to an ancestral condition, such as bud variation so frequently presents. Gemmules derived from a plant's remote progenitors are, according to the hypothesis, circulating in the present generation, and it only requires the occurrence of favourable conditions to determine the revivification of these now dormant gemmules to reproduce the ancestral form. There still remains the difficulty of ascertaining what the favourable conditions are which determine this change. The reason for the prolonged dormancy of the gemmules is also not obvious. But, supposing we admit the gemmule hypothesis as sufficient to account for reversion by bud variation, there yet remain that larger class of bud variations wherein there is no suspicion of reversion.

This latter category, so far as we see, can only be explained by Mr. Darwin's assertion that, in "cases in which the organisation has been modified by changed conditions, the increased use or disuse of parts or any other cause, the gemmules cast off from the modified units of the body will be themselves modified, and, when sufficiently multiplied, will be developed into new and changed structures."

But before we can, with propriety, avail ourselves of this latter explanation, we have to be satisfied that a change of conditions has really been in operation. And this is too often beyond our ken. The majority of bud variations not distinctly referable to reversion, appear suddenly, without any obvious change of external condition, we know not why or wherefore. Suppose we admit, on the ground of intrinsic probability, the operation of changed conditions, even though we may have no direct evidence on the point, we have yet to explain how and why one particular shoot on one particular part of a plant should be acted on in this way, when there is no appreciable reason why it should be influenced more than the rest.

There is still another way of explaining the phenomena on the gemmule hypothesis, and that is, by supposing changes in the number, arrangement, or position of the gemmules; and this supposition, though plausible, is yet based on a number of mere assumptions, and, moreover, it leaves the cause of the altered condition of the gemmules entirely unexplained.

To sum up, then, we may say that there is no absolute difference between bud variation and seed variation. The changes manifest themselves in the same manner and in the same organs in the case of buds or seedlings respectively. The conditions, so far as we know, that produce variation in the one are the same that are effectual in the other. Lastly, apart from the different mode of origin, there is no essential difference between a bud formed as the result of fertilisation, *i.e.*, an embryo, and one formed without the direct agency of the two sexes, *i.e.*, a bud.

AN ACCOUNT OF A GANOID FISH FROM QUEENSLAND (*Ceratodus*).

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[PLATE LXXXVI.]

THE genus *Ceratodus* has been established by Professor Agassiz for teeth which are found in strata of Jurassic and Triassic formations in various parts of Europe and India. These teeth (fig. 3), of which there is a great variety with regard to general shape and size,* are much longer than broad—sometimes 2 in. long—depressed, with a flat or slightly undulated, always punctuated crown, with one margin convex, and with from three to seven prongs projecting on the opposite margin. They have always been found isolated, sometimes with a portion of a bony base attached to them. Yet Professor Agassiz pointed out, from their shape, that there must have been only two of them in the upper jaw and the same number in the lower, that the convex margin was directed inwards, and the prongs outwards. No other part of the fish to which they belong has hitherto been found associated with them; but Agassiz considered it to have been a cartilaginous fish, or more especially a shark—a view not so very far from the truth, as we shall see hereafter.

The discovery of a Ceratodont fish in the recent fauna is due to the Hon. William Forster and Mr. Gerrard Krefft, the Curator of the Australian Museum at Sydney. Years ago the former of these gentlemen had informed Mr. Krefft that there existed in the fresh waters of Queensland a large fish with cartilaginous backbone, but he was thought to be mistaken

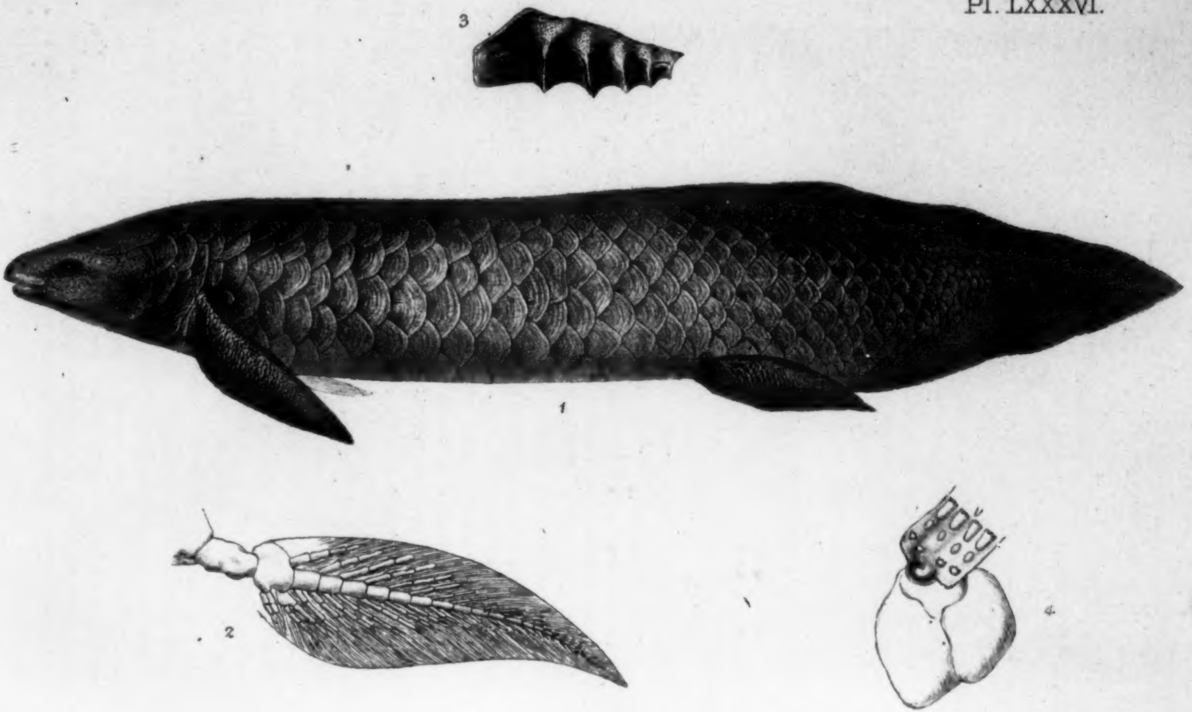
* Mr. Higgins possesses the largest, and probably most unique, collection of Ceratodont teeth from one locality—viz. from Aust-passage near Bristol. Among some 300 specimens there are scarcely two which are sufficiently alike to be assigned to the same individual.

until he succeeded in obtaining for Mr. Krefft a specimen which, although in an imperfect state of preservation, removed all doubts on the matter, and enabled Mr. Krefft to communicate this remarkable discovery to the Zoological Society of London (April 28, 1870). He says: "The discovery of a species of *Lepidosiren* in Australia will, no doubt, take the scientific world by surprise—the more so as this newly-found amphibian has a dentition different from that of *Lepidosiren*, and closely resembling the teeth of certain fossil sharks described by Agassiz under the generic term of *Ceratodus*. On this ground, and being convinced that the various species of animals classed under the name of *Ceratodus* were not sharks, but amphibians, I shall adopt Professor Agassiz's name, and describe the Australian amphibian, in honour of its discoverer, as *Ceratodus forsteri*."

As soon as Mr. Krefft had recognised the importance of this discovery, the Trustees of the Australian Museum took steps to secure well-preserved examples. They sent a collector into the district where the animal was known to occur; and, with their usual liberality, they despatched to the British Museum for examination the first specimens they could spare, by which I was enabled to work out the details of its structure. Some attempts subsequently made to obtain other examples appear to have been unsuccessful, as the fish is locally distributed, and easy of capture at a certain season or at a certain state of the water only.

The specimens hitherto obtained have come from the Burnett, Dawson, and Mary Rivers, some from the fresh water of the upper parts, others from the lower brackish portions. The fish is said to attain to a "weight of twenty pounds," and again to a "length of six feet," the largest example sent to the British Museum being about three and a half feet long. Locally the settlers call it "Flat-head," "Burnett-," or "Dawson-Salmon," and the aborigines "Barramunda," a name which they appear to apply also to another similar fish, the *Osteoglossum leichardti*. I found the intestinal tract crammed full of more or less masticated leaves of various plants (*Myrtaceæ* and *Gramineæ*; they had lost the green colour entirely, being of a uniformly deep black, as if they had lain in water for some time, and were eaten when in a decomposing condition. The quantity of these vegetables is enormous, and there is no doubt that they constitute the principal food of the fish. Shells, fragments of which have been found in the stomach, may have been swallowed accidentally; however, it has been stated repeatedly that the fish can be caught with a hook baited with a worm. The flesh is salmon-coloured, and much esteemed as food.

Pl. LXXXVI.



Dr. Günther

W. West & Co. lith.

A ganoid Fish from Queensland (Ceratodus)

The Barramunda (we will use this probably oldest name) is said to be in the habit of going on land, or at least on mud-flats; and this assertion appears to be borne out by the fact that it is provided with a lung. On the other hand, we must recollect that a similar belief has been entertained with regard to *Lepidosiren*, of which now numerous examples have been kept in captivity, but none have shown a tendency to leave the water. I think it much more probable that the Barramunda rises now and then to the surface of the water in order to fill its lung with air, and then descends again until the air is so much de-oxygenised as to render a renewal of it necessary. It is also said to make a grunting noise, which may be heard at night for some distance. This noise is probably produced by the passage of the air through the œsophagus when it is expelled for the purpose of renewal.* As the Barramunda has perfectly developed gills, besides the lung, we can hardly doubt that, when it is in water of normal composition and sufficiently pure to yield the necessary supply of oxygen, these organs are sufficient for the purpose of breathing, and that the respiratory function rests with them alone. But when the fish is compelled to sojourn in thick, muddy water charged with gases which are the product of decomposing organic matter (and this must be the case very frequently during the droughts which annually exhaust the creeks of tropical Australia), it commences to breathe air with its lung in the way indicated above. If the medium in which it happens to be is perfectly unfit for breathing, the gills cease to have any function; if only in a less degree, the gills may still continue to assist in respiration. The Barramunda, in fact, can breathe by either gills or lungs alone, or by both simultaneously. It is not probable that it lives *freely* out of the water, its limbs being much too flexible for supporting the heavy and unwieldy body, and too feeble generally to be of much use in locomotion on land. However, it is quite possible that it is occasionally compelled to leave the water, although I do not believe that it can exist without it in a lively condition for any length of time.

Of its propagation or development we know nothing except that it deposits a great number of eggs of the size of those of a newt, and enveloped in a gelatinous case. We may infer that the young are provided with external gills, as in the African *Lepidosiren* and *Polypterus*.

Before I proceed to the description of the Barramunda, it

* Gurnards (*Trigla*) and Bull-heads (*Cottus*) are well known to produce a similar noise when drawn out of the water, by the air rushing from the air-bladder through the œsophagus.

will not be out of place to refer here to a remarkable fact in geographical distribution which I have omitted in my previous communications on *Ceratodus*. The division of fresh-water fishes offers not a few instances in which two or more natural families, much differing in their structural characters, have exactly the same geographical distribution. We shall see subsequently that *Ceratodus*, *Protopterus*, and *Lepidosiren*, are members of the same natural Ganoid family (*Sirenidæ*). Now this family coincides in respect of its geographical range with a Teleosteous family which I have called *Osteoglossidæ*, and which comprises the genera *Osteoglossum*, *Arapaima*, and *Heterotis*, as will be seen from the following table:—

Ganoid.	Teleosteous.
Tropical America.	
Lepidosiren paradoxa.	Osteoglossum bicirrhosum. Arapaima gigas.
Tropical Australia.	
Ceratodus Forsteri. Ceratodus miolepis.	Osteoglossum Leichardti.
East Indian Archipelago.	
xx.	Osteoglossum formosum.
Tropical Africa.	
Protopterus annectens.	Heterotis niloticus.

Thus it is only in the East Indian archipelago that we have not yet found the Ganoid representative of the Teleosteous *Osteoglossum formosum*. That it will be found there I have no doubt. *O. formosum* has hitherto been obtained in Sumatra, Banka, and Borneo; and of the inland fishes of the latter island scarcely anything is known at present.

The *body* of the Barramunda (fig. 1) is eel-shaped, but considerably shorter and thicker than a common eel, and covered with very large scales. The *head* is nearly entirely naked, covered with a porous skin, flattened and broad, the eye lateral and small, the mouth in front of the broad snout comparatively narrow, and provided with thick and soft lips. The gill openings are a narrow slit on each side of the head, immediately in front of the fore-paddle. There are no external nostrils. The foremost portion of the trunk is depressed like the head, but it soon passes into the compressed remaining portion, the boundary between trunk and tail being externally indicated by the vent only, which is situated between the hind-paddles. The *tail* varies in length; it is sometimes shortened, and it appears that mutilations of this part, particularly when happening in early youth, are readily repaired. The tail diminishes rapidly behind in vertical dimension, till it ends in a thin point. The entire tail is surrounded by a broad, vertical fin, which commences on the back behind the middle of the trunk, and is supported by innumerable fine cartilaginous rays. There are two fore and two hind *paddles*, similar to each other in shape and size, and very different from the fins of ordinary fishes. They are covered with small scales along the middle from the root to their extremity, and surrounded by a rayed fringe similar to the vertical fin. These paddles are flexible in every part and in every direction, and too feeble to assist in locomotion on land; they may be of use when the animal crawls in water over the muddy bottom of a creek; but the principal organ for locomotion is the tail, as in tailed Batrachians and the majority of fishes.

The *nasal openings* and the *dentition* can be seen only after the mouth has been slit open. The situation of the former within the cavity of the mouth, two on each side, is a very important character, which hitherto had been known in *Lepidosiren*. The number and form of the teeth has been noticed above, and we have only to add that, beside those molar-like teeth, there are a pair of incisor-like teeth in the fore part of the palate, obliquely inserted in the vomer, and without corresponding teeth in the lower jaw. Knowing the kind of food taken by the Barramunda, we can readily perceive that the incisors will assist in taking up or tearing off leaves, which are then partially masticated between the undulated surfaces of the molars.

With regard to the scales, we may add that a slight difference in their number has been observed in the specimens examined, a difference which, on a more extended examination, may prove to be not constant. The specimens from the Burnett River, to which first the name *Ceratodus forsteri* has been given, have the middle of the trunk surrounded by eighteen series of scales, whilst there are twenty-one of these series in examples from the Mary River. Consequently the scales appear conspicuously smaller and more numerous in the latter form, which I have named *C. miolepis*.

The *skeleton* is cartilaginous; where ossification appears, it is in the form of a more or less thin covering enveloping the cartilaginous substratum, but never taking its origin in the interior or by transmutation of the cartilage. Instead of a vertebral column we find a simple long, tapering chord, without any segmentation, but supporting a considerable number of apophyses. Twenty-seven of them are abdominal and bear well developed ribs. A positively defined boundary between the notochord and the skull does not exist, but in a vertical section the tapering end of the former may be traced wedged into the basal portion of the skull. The skull is a completely closed cartilaginous capsule, nearly entirely covered with superficial bones, to which, again, some other cartilaginous elements are appended. In the former the confluence of cartilage is so complete that no distinct divisions are traceable by sutures. The tegumentary bones may be designated as, 1, ethmoid; 2, a pair of ossa frontalia; 3, a pair of ossa pterygo-palatina, bearing the upper molars; 4, a single sclero-parietal; 5, an os basale; 6, an os quadratum; 7, operculum; 8, suboperculum; and 9, mandible. Distinct maxillary and inter-maxillary elements are not developed, but replaced by facial cartilages which pass into the suborbital ring.

On the whole the structure of the skeleton reminds us much of that of the Sturgeons, *Chimæra*, and *Lepidosiren*, and of all the modifications by which it differs from these allied fishes none is of greater interest than the peculiar structure of the paddles (fig. 2). The fore-paddle is joined to the scapular arch by an oblong cartilage (forearm) and by a broad basal cartilage (carpus). The central part, which we have found externally to be covered with scales, is supported by a jointed axis of cartilage extending from the carpus to the end of the paddle; each joint bears a pair of three, two, or one-jointed branches. The skeleton of the hind-paddles is formed on exactly the same plan. This singular structure is of interest from several points of view: 1. The analogy of this framework to that of the caudal portion of the vertebral column is obvious, *Ceratodus* being not only truly diphycercal as far as the termination of the body is concerned, but also with regard to the extremity of its paired fins. The many-jointed pectoral axis may be compared to the series of neural and hæmal apophyses, both forming the base to a system of superadded processes (here two or three-jointed branches, there neural and interneural, hæmal and interhæmal spines), which are destined to serve as supports to the surrounding soft parts, and more especially the rays of the fin. Further, as the heterocercal tail of the sturgeon is justly considered to be a later development of the diphycercal form, so the pectoral fin of those fishes proves to be nothing but the heterocercal modification of the diphycercal Ceratodont paddle. 2. The singular filamentary limbs of *Lepidosiren* prove to be typically the same as the paddles of *Ceratodus*, but there they are reduced to the simple central cartilaginous axis, with the addition of only rudimentary rays in the African species. 3. Professor Huxley has already drawn attention to the affinity existing between the limbs of *Lepidosiren* and certain fossil Ganoids, of which impressions f paddles with scaly centres have been preserved (fringed or lobate fins). *Ceratodus* clearly proves the correctness of this view, and we are fully justified in supposing that those extinct fishes with lobate fins had them provided with a similar internal skeleton.

As in the other Ganoid and Plagiostomous fishes, the *heart* of *Ceratodus* (fig. 4) is provided, in addition to the ordinary two divisions of the fish-heart, with a third contractile chamber, the *conus arteriosus*. The internal structure of the ventricle and atrium is extremely similar to that of *Lepidosiren*, but the valvular arrangement in the interior of the *conus arteriosus* differs considerably, inasmuch as the valves are disposed in two or three transverse rows, of which, however, one only is fully developed.

We have mentioned above that the Barramunda can breathe free air as well as air dissolved in water, and we may infer this from the perfect development of the *gills* and of a *lung*. There are four gills on each side; they are broad, lamellated membranes, free from each other, but attached to the outer walls of the gill cavity, which peculiarity is clearly an approach to the fixed gills of the sharks and rays. The pneumatic apparatus may be described either as a single lung, with symmetrical arrangement of its interior, or as two lungs confluent into a single sac without a dividing longitudinal septum. Its interior is divided into about thirty compartments on each side, formed by strong transverse septa and cellular at the bottom; it is evident that by this arrangement the respiratory surface is much increased in extent. This pulmonary sac extends from one end of the abdominal cavity to the other, and terminates anteriorly by a short duct in a *glottis*, viz. a slit with a valve in the ventral side of the œsophagus, somewhat to the right of the median line. In this respect the Barramunda shows itself to be a true fish, in spite of the presence of a lung, inasmuch as it is obliged to receive the air through the mouth, whilst nearly all Batrachians have the palate perforated by the nostrils, which form a distinct passage for the air used in breathing. When the fish sojourns in pure water, and breathes by the gills, the lung does not differ from the air bladder of other fishes; it then receives arterial blood, returning venous blood, like all the other organs of the body; but when the respiratory function rests with the lung, the pulmonary vein carries purely arterial blood to the heart, where it is mixed with venous blood and distributed to the various organs.

Externally the *intestinal tract* appears as a wide straight sac without divisions or circumvolutions. Internally it is traversed throughout by a spiral valve performing nine gyrations; the cavity before the commencement of the valve must be regarded as the stomach. The liver does not show any peculiarity.

The *kidneys* are paired, the ureters entering a very small urine bladder at the back of, and partly confluent with, the rectum.

The *generative organs* are paired. Their products pass outwards by a paired oviduct or vas deferens. These ducts are entirely separate from the ovaries or testicles, each having a distinct abdominal orifice immediately below the diaphragma. They accompany the ureters in their posterior course, but are nowhere confluent with them, and terminate in a common opening into the cloacal dilatation, immediately in front of the uretral orifice. The ovaries are elongate bands, their outer surface being crossed by a great number of lamellæ, the bearers of the stroma in which the ova are developed. The ova are in very great number, and when mature drop into the cavity of the peritoneum, as in Salmonoids; but instead of being expelled at once by the peritoneal openings, they travel along a shallow gutter *forwards*, enter the much convoluted oviducts, where they receive a gelatinous covering, like the eggs of Batrachians, and are finally expelled through the cloaca. The testicles and vasa deferentia are analogous to the female organs with regard to position, form, and orifices.

Such are the principal points in the organisation of the Barramunda; and it remains now to add some remarks on its affinities and on the bearings which the acquaintance of this singular type has upon the advancement of science.

1. When we direct our enquiry at first to recent fishes, there cannot be any doubt as regards the close relationship between *Ceratodus* and *Lepidosiren*. The latter had been regarded by Joh. Müller (and by most subsequent ichthyologists) as the type of a separate sub-class—*Dipnoi*—which he distinguished from the Ganoids by the presence of a pair of longitudinal valves in the conus arteriosus of the heart, the valves being arranged in transverse series in Ganoid and Plagiostomous fishes. We see now that the valvular arrangement in *Lepidosiren* is merely a modification of the Ganoid heart, and that the characteristic feature of the latter consists in the presence of a pulsating third division—the *conus arteriosus*. Therefore we are compelled to abandon the sub-class, *Dipnoi*, and to refer it as a sub-order to the Ganoids, with its definition altered thus: The *Dipnoi* are Ganoid fishes with the nostrils within the mouth, with paddles supported by an axial skeleton, with lungs and gills, with a notochordal skeleton, and without branchiostegals.

2. But it appears to me that *Ganoids* and *Chondropterygians* (sharks and rays) are much more closely allied to each other than either of them to the sub-class of Teleosteans, which comprises the majority of the fishes of the present epoch, and the members of which have, instead of a contractile conus arteriosus, simply a non-contractile swelling of the aorta, separated

from the heart by two valves opposite to each other. Nor is this the only distinguishing character. A heart with a true conus arteriosus is always accompanied by a more or less developed spiral valve of the intestine (entirely absent in Teleosteans), and by non-decussating optic nerves. The fore and hind limbs of the Chondropterygians are also paddles supported by a cartilaginous framework; the tooth-bearing pterygo-palatine arch of the *Dipnoi* is homologically identical with the "upper jaw" of a shark. And the anotomical evidence in favour of a union of Ganoids and Chondropterygians is rendered complete by the *Holocephala* (Chimæras), which differ in several important points from the other Chondropterygians, approaching the Ganoids by these very characters, and are, in fact, an intermediate form. They are sharks in external appearance and with regard to the structure of their organs of propagation. On the other hand, there is only one external gill opening on each side; the skeleton is notochordal, and the palatal apparatus coalesces with the skull as in *Dipnoi*, which is not the case in any of the sharks and rays; likewise the dentition approaches that of *Ceratodus*. Sir Ph. Egerton has drawn attention to another very important fact; viz. that the dorsal spine is articulated to the neural apophysis, and not merely implanted in the soft parts and immovable, as in sharks. Furthermore, all those modifications which show an approach of the ichthyic type to the Batrachians are found in Ganoids and Chondropterygians, none in Teleosteans; and, finally, the coexistence and development of Ganoids and Chondropterygians in geological epochs when no (or only very few) Teleosteans existed, is a circumstance which seems to confirm a conclusion arrived at from an anatomical point of view only; namely, the conclusion that Ganoids and Chondropterygians should be united in one sub-class—*Palæichthyes*.

3. A third point of the deep interest is the great antiquity of the Dipnoous type. At the commencement of these notes we have seen that there is no evidence to show that the Barramunda is even generically distinct from those fishes, of which, unfortunately, the teeth only have been preserved. But some of the oldest fishes, known from the Devonian epoch, and designated by the names *Ctenodus* and *Dipterus*, prove to be Dipnoous fishes. They had the same dentition as *Ceratodus*, nostrils within the mouth, acutely lobed paddles, a notochordal skeleton, and, with exception of dermal scutes, a very similarly formed skull. Thus, then, we have the following facts before us: The Dipnoous type is represented in the Devonian and carboniferous epochs by several genera (*Dipterus*, *Ctenodus*, *Chirodus*, *Conchodus*, *Phaneropleuron*); it is then lost down to the Trias and Lias, where the scanty remains of a distinct

genus (*Ceratodus*) testify to its presence; no further trace of it has been found until the present period, where it reappears in three genera (*Ceratodus*, *Lepidosiren*, *Protopterus*), one of which is identical with that of the Mesozoic era. Now, at present scarcely any zoologist will deny that there must have been a continuity of the Dipnoous type, and it is only a proof of the incompleteness of the palæontological record that we have to derive all our information regarding it from only three so very distant periods of its existence.

In conclusion I may add a synoptical table, from which the systematic views advocated above, and more especially the position of *Ceratodus* in the system, may be readily understood. After the separation of *Amphiosus* and the Lampreys as types of two distinct sub-classes (*Leptocardii* and *Cyclostomata*), the remaining host of fishes are referred to two other sub-classes:—

SUB-CLASS: *Teleostei*. Heart with a rigid bulbus aortæ; intestine without spiral valve; optic nerves decussating (living species, nearly 9,000).

SUB-CLASS: *Palæichthyes*. Heart with a contractile conus arteriosus; intestine with a spiral valve; optic nerves non-decussating.

- *Order I.—Plagiostomata*, or *Marine Palæichthyes* (sharks and rays; living species, nearly 300).
- *Order II.—Holocephala* (four living species).
- *Order III.—Ganoidei*, or *Freshwater Palæichthyes*.
  - Sub-order 1.—*Amioidei* (one species).
  - Sub-order 2.—*Lepidosteoidei* (three species).
  - Sub-order 3.—*Polypteroidei* (two species).
  - Sub-order 4.—*Chondrostei* (sturgeons, thirty species).
  - Sub-order 5.—*Dipnoi*.
    - Fam. a.—*Sirenidæ*.
      - Sub-fam.—*Ceratodontina* (Ceratodus).
      - Sub-fam.—*Protopterina* (Lepidosiren, Protopterus).
    - Fam. b.—*Ctenododipteridæ* (Ctenodus, Dipterus).
    - Fam. c.—*Phaneropleuridæ* (Phaneropleuron).

GREENWICH OBSERVATORY.

By JAMES CARPENTER, F.R.A.S.

THERE are few scientific institutions whose objects are so little understood, and whose labours are therefore so likely to be misjudged, as an astronomical observatory of the character of the national one at Greenwich. Even those who possess some knowledge of astronomy, who read its literature, and take a warm interest in its salient achievements, are frequently little or not at all conversant with those departments of the science that are perforce pursued in an essentially practical establishment, where the sun may be observed day by day without a moment's thought being given to his spots, the moon watched by night without a care for her physiography, and where the planets and stars are subjects of a system of close observation which, however, gives no heed to questions concerning their physical nature.

It happens, from a circumstance that will bye-and-bye appear, that the present is an opportune time for reviewing the history of the Observatory at Greenwich and its relation to current astronomical science; but it may be mentioned that the appearance of this article at this opportune time is merely accidental.

To start with a just idea of the very definite aims of the Observatory, we should clearly recognise the circumstances that led to its foundation. It was born of a necessity that arose from that extension of British navigation which was, at least partially, a consequence of the passing of the Navigation Act of Charles II. The necessity was a means of obtaining the longitude at sea. The latitude, we may remark, presented no difficulty whatever. A method for longitude had for more than a century existed in theory; for Apian in 1524, Gemma Frisius a few years later, and Kepler subsequently, had proposed the use of lunar distances in the very form that now universally obtains. The method may be thus described. The moon moves rapidly among the stars. Suppose that for a given instant of Greenwich time her angular distance from a near star be known beforehand. Then if an observer at sea measure the distance minute by minute till he makes an observation which shows the distance * equal to the given one, he knows the Greenwich time for the moment of that observation; and this time compared with the local or ship's time of the observation gives, by mere difference, his longitude reckoned, in time, from Greenwich. If the distance of moon from stars be given at fixed intervals of Greenwich time, say hourly,† then it matters not when the navigator takes his distance, for he can always find, by interpolation, the instant of Greenwich time corresponding to the moment of his observation.

Now, to make this method practical two things are necessary. First, the positions of the fixed stars must be exactly known; and second, the moon's place hour by hour must be accurately predictable a long time in advance, so that the mariner may carry the table of predicted Greenwich distances out with him. The plan, as we have said, was proposed more than three centuries ago, and the need of applying it was severely felt just two centuries ago. But there were no catalogues of fixed-star plans accurate enough for the purpose; and the knowledge of the moon's motions was utterly insufficient also. The double want was brought to the King's (Charles II.'s) notice, and he at once ordered an observer to be appointed and an observatory to be founded to meet it. The right man for the post turned up in the person of Flamsteed, and he was commanded forthwith to "apply himself with the utmost care and diligence to the rectifying the tables of the motions of the heavens and the places of the fixed stars, so as to find out the so-much-desired longitude of places for the perfecting the art of navigation." The same object is set forth in the tablet which still stands over what was the entrance-door of the building; and a wording almost identical with that just quoted has been maintained in the warrants of all succeeding Astronomers Royal to the present day.

It is desirable that this definition and its implied limitation of the duties of the Observatory should be borne in mind. There is no provision in its charter for the numerous modern subjects of inquiry which have been classed under the head of *astronomical physics*. It is true such subjects have been occasionally followed up, but only to an extent that precluded

* Certain corrections are necessary to reduce the angular distance as measured at the earth's surface to what it would be if measured from the earth's centre; but these form part of the after calculation.

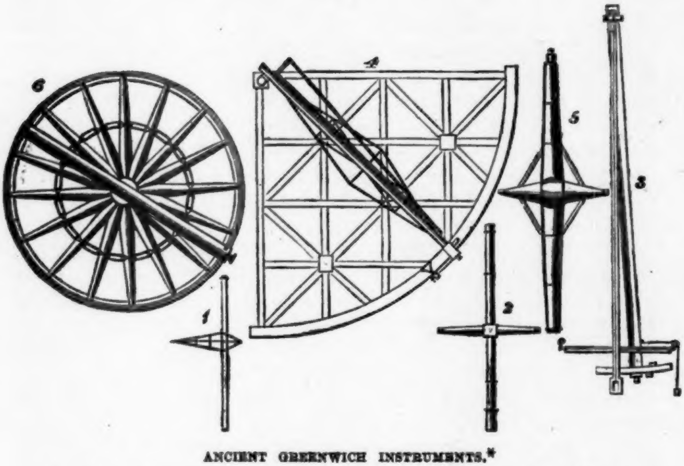
† As a matter of fact, they are given for every third hour in our "Nautical Almanacs."

interference with the primary objects of the institution. From first to last Greenwich has been held to be an institution for the pursuit of mensurative astronomy with utilitarian ends, and its instruments, and, to some extent, its *personnel*, have been provided and organised accordingly. The succession of Royal Astronomers—Royal Observators, as they are officially styled—Flamsteed, Halley, Bradley, Maskelyne, Pond, and the present occupant of the office, have, with one exception, laboured with a consistency truly remarkable, like one long-lived man, to carry out, with the best resources of their times, the strictly fundamental works committed to them: with what results we shall presently see.

Positions of stars for the formation of catalogues of star-places, and positions of the sun, moon, and planets for the ultimate formation of tables of their motions, are the staple observations at Greenwich, and they consist almost entirely of meridian observations, namely, times of transit over the meridian, for determination of objects' Right Ascensions, and measures of the angular distances of the same objects from the celestial pole, for the determination of their North Polar distance; these last being also made on the meridian. The instruments for these observations are the "Transit" and some form of "Circle," both instruments moving in the plane of the meridian only, and the first having it accurately defined by a vertical wire, in modern times a cobweb thread. Flamsteed at first had a make-shift sextant, with which he could only measure one star from another in a straight line, and he had to leave these measures to be ultimately referred to fixed points for determination of Right Ascensions and Polar Distances. But in 1689, thirteen years after his appointment, he procured —from his own resources, by the way, for from first to last he never had a penny for instruments from the Government—a large graduated arc, which was fixed upon a wall in the meridian plane, and upon the centre of which was pivoted a telescope with a vertical wire at its focus; and he took clock-times of transit across this wire for Right Ascension, and read the position of the telescope upon the arc for Polar Distance. Halley had a transit instrument, with a $1\frac{3}{4}$-inch object-glass, generally similar in plan to those of the present day, specially for R.A. observations, and a meridianal quadrant of 8 feet radius specially for those in Polar Distance. Bradley at first had Halley's instruments, afterwards a better transit of $2\frac{3}{4}$-inch aperture, and a second quadrant, so that he could command the whole meridian from the North horizon to the South. Maskelyne used Bradley's instruments from first to last, but the defects of a quadrant, in its liabilities to distortion and errors of centering, were so obvious to him, that just before his death he ordered a complete circle, of 6 feet diameter, to be made, with a telescope, of 4-inch aperture, fixed upon it, graduated around its periphery. This circle turned upon a pivot carried through a wall to which were fixed six micrometers for reading the circle divisions. Maskelyne did not live to use this "Mural circle," but Pond had the advantage of it; and he subsequently had another one made like it, using the two together. Pond also had a new Transit made by Troughton, of 5-inch aperture, which was a masterpiece; and with these means he pushed observing accuracy to a point that had not before been aimed at, and has even now scarcely been surpassed.

The essential parts of all these instruments (except Flamsteed's) are shown in the accompanying sketch, upon a scale of

FIG. 1.



ANCIENT GREENWICH INSTRUMENTS.*

1. Halley's Transit (1721–1749).
2. Bradley's Transit (1750–1816).
3. Bradley's Zenith Sector.
4. Mural Quadrant (1725–1812).
5. Troughton's Transit (1816–1850.)
6. Mural Circle (1812–1850).

about $\frac{1}{4}$ inch to the foot: the cut also shows the form of Bradley's famous Zenith Sector, with which he consummated his immortal aberration discovery.

The present Astronomer Royal, Sir George Airy, used Pond's instruments as he found them until about the year 1850, when finding them too small in their object-glasses for existing and future wants—especially for the observations of the newly-dis-

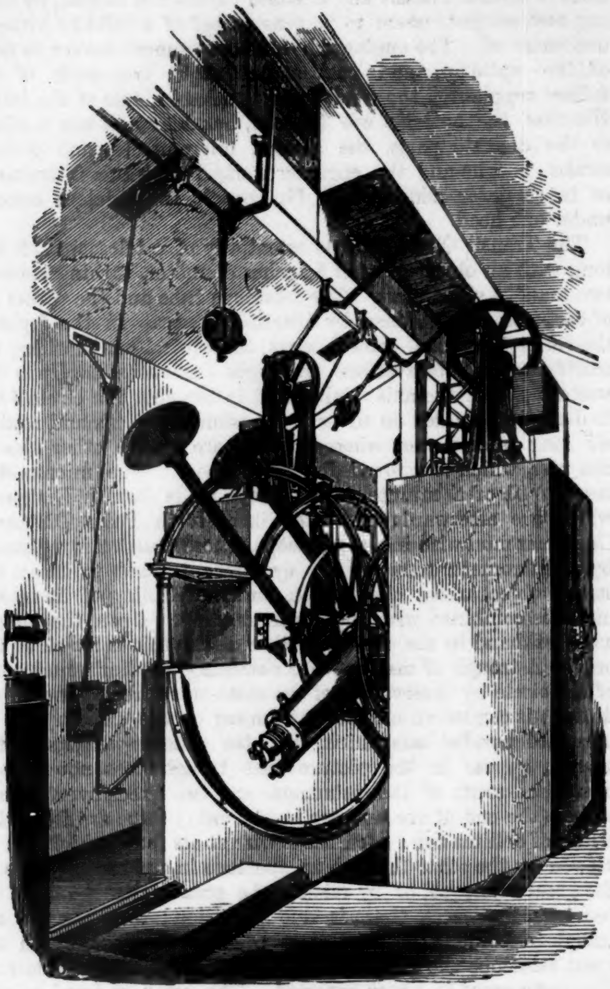
* The woodcuts illustrating this article have been kindly lent by Messrs. Bell and Daldy, and Messrs. Bradbury and Evans.

covered minor planets—he made a revolutionary change of double character, first, by designing an instrument which, while carrying a large object-glass, should in itself combine the functions of both a Transit and a Mural Circle, and second, by causing such an instrument to be constructed of a solidity hitherto undreamt of. The engineer became instrument-maker in place of the optician, and cast and wrought iron-work of the former supplanted the tender and perishable brass of the latter. Not that the optician was ignored, but his work was confined to the delicate parts, the glasses, micrometers, and divided circles; and he and the engineer worked upon this instrument, as in all later additions to Greenwich, in judicious concert under one head.

The Transit Circle consists essentially of a telescope, 12 feet long with an object-glass of 8 inches diameter, turning between two massive stone piers. The telescope tube and the pivots are of cast iron strengthened by internal braces. In the focus is the series of vertical cobweb wires, the central one marking the meridian, for observations in Right Ascension. Upon the western side of the axis (right hand in the cut) is a circle 6 feet in diameter, divided on silver to five minutes of arc, and read by six long micrometers whose eye-ends are on the other side of the pier, which is pierced for the purpose. The micrometers read to ·01 of a minute. The circle reads Zenith Distances, which are afterwards converted into North Polar Distances. One horizontal wire in the telescope, whose position is registered by a micrometer screw on the eye-piece, is used to bisect the star or object under observation, and the reading of its micrometer is combined with the circle reading. Upon the eastern axis (left hand in the cut) is another circle; this is for clamping only. A trough of mercury (for obtaining the horizontal point of the circle by observation of the same star directly and by reflexion) is carried on a circular tramway on the eastern pier by means of parallel bars which, with the counter-weights of the trough, appear in the picture: the trough does not appear. North and south of the instrument are two fixed inverted telescopes (the end of one appears in the cut); these are for giving a line of collimation: each has a wire at its focus which can be viewed by the other through an aperture in the central cube of the great telescope, or by raising the great telescope from its bearings, for which there is due provision; the wires are adjusted to coincidence, and then by observing each with the great telescope the collimation error of the latter is obtained. The instrument is never reversed. The error of level of the axis is found by measuring the amount of non-coincidence of the meridional cobweb with its own image reflected from a trough of mercury placed perpendicularly under the telescope.

In front of the observer, as he looks south, is the *Transit clock* (not seen in the picture) for observing times of passage

Fig. 2.



THE TRANSIT CIRCLE.

of objects over the vertical wires. Formerly these times were taken by ear, the observer counting the clock-beats and noting the second and decimal of a second at which the object passed each wire. Since 1851 they have been registered by an electric chronograph. A sheet of paper on a cylinder rotating uniformly receives a puncture from a point on the armature of an electro-magnet at every beat of the Transit-clock: thus a time scale is formed by the clock. The observer taps a key mounted on the telescope eye-piece as the object under observation crosses each wire; his tap completes a galvanic circuit which works another electro-magnetic pricker; his punctures fall among the clock punctures, and their times can be read off thereby. The prickers travel down the cylinder as it rotates, and thus give a spiral line of registers. Uniform rotation of the cylinder being of the highest importance, it is secured by a driving-clock controlled by a *rotating* in place of a vibrating pendulum.

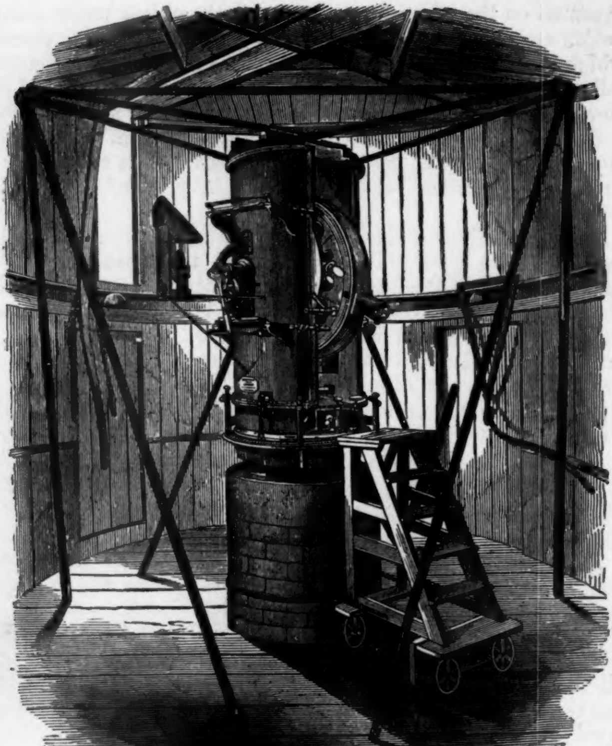
Since the *Transit-clock* is a measuring instrument of the most accurate kind, its excellence and the steadiness of its rate should be of the highest character attainable. The one in the Transit Circle room at Greenwich having grown old and exhibited some slight infirmities, a new Sidereal Motor has lately been mounted, in a basement where the temperature is nearly uniform, at a distance from the Transit instrument. It is, however, in connection with the chronograph, and nearly all Transit observations are made (galvanically) by it. It also gives controlling currents to several subsidiary clocks. Its rate is wonderfully regular; so uniform, indeed, that it faithfully exhibits the small changes which are produced by barometric fluctuations.

The Transit Circle being the chief instrument of the Observatory, it is in almost constant use whenever the sky is clear. A daily course of work with it includes Transits and North Polar Distances of the sun, moon (when visible on the meridian), all the large planets that pass before 3 o'clock in the morning, all the small planets passing between 10 P.M. and 1 A.M. during the first half of the lunation,* transits of a selection of the principal or fundamental stars for error and rate of the clock, an upper and lower culmination of a polar star for azimuthal error, an observation of the reflected meridional wire for level error, and of each collimating mark for collimation error of the Transit telescope; and observations of four stars, two north, two south of the zenith, each by reflexion and directly, for the horizontal-point error of the circle. These secured, as many extra or small stars as can be are observed for the ultimate formation

* By arrangement with the Paris Observatory the small planet observations are divided; Greenwich observing from new to full moon, and Paris from full to new moon.

of catalogues, or for special purposes. Certain instrumental errors, such as flexure of the telescope, errors of circle divisions, and want of circularity of pivots, are determined occasionally. Nothing is supposed for a moment to be in perfect adjustment;

Fig. 3.



THE ALTAZIMUTH.

no error is *mechanically* corrected; everything is supposed to be always wrong, and to be corrected *numerically* in the after reduction of the observations.

But the Transit Circle has an important auxiliary in the Altitude and Azimuth, or, as it is briefly called, the *Altazimuth* instrument, which was designed specially for position observations of the moon. Viewing the primary importance of these observations, and having regard to the impossibility of seeing the moon on the meridian during that critical part of her orbit when she is near the sun, and also to the frequent loss of meridional observations from temporary cloudiness of the sky, Sir George Airy deemed it highly desirable to devise means of securing observations *off the meridian* comparable in accuracy with those made on the meridian. The only form of instrument competent for this purpose was an altazimuth of massive construction. He accordingly designed and mounted (in 1847) the instrument here figured. It consists of a telescope (4 inches aperture) solidly fixed into a broad-rimmed vertical or altitude circle which has a divided silver limb read by four microscopes. This circle turns between two semi-cylinders of cast-iron connected by top and bottom plates to form a frame which turns on vertical pivots, the lower one borne by a stone pier and the upper one by a triangular framework of iron bars. The vertical frame carries on one side the four microscopes which read the altitude circle, and around its base other four which read a horizontal circle fixed to the stone pier for azimuthal measures. There are levels for showing the inclinations of both axes; and they, and the microscopes, are carried by supports which are cast upon (not screwed to) the parts they spring from. The nightly observations with this instrument consist of an altitude and azimuth of the moon, azimuths of stars for zero of the azimuth circle and collimation error of the telescope, and observations of a distant terrestrial mark for zenith-point error of the vertical circle. All observations are made in two (reversed) positions of the telescope; and all times of exact position-determinations are registered by the chronograph. The azimuths and altitudes are in effect reduced to Right Ascension and Polar Distances by after-calculations, which are very laborious, but are, in common with every computation in the observatory, facilitated by skeleton forms of which about two hundred, simple and elaborate, are in constant use.

The Altazimuth secures about two hundred observations of the moon in a year, the Transit-Circle only one hundred; and a large percentage of the former are of the highest value as affording tests of the lunar tables for parts of the orbit when the moon is near conjunction with the sun, and cannot be touched by a meridian instrument.

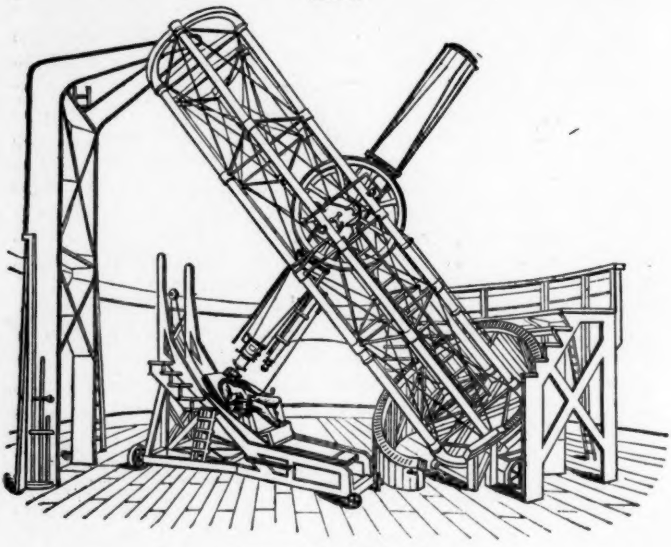
Another auxiliary instrument of the exact class is the Reflex Zenith Telescope, for measuring with great precision the small zenith distance of the star γ Draconis, which passes near the zenith of Greenwich, and is favourably situated for determination of the amount of stellar aberration. It consists of a horizontal object-glass, capable of semi-rotation about a vertical axis, with a trough of mercury at half its focal distance below.

The rays from the star, after passing through the object-glass, are received upon the mercury and reflected up through the glass again, forming a focal image just above it. In the focal plane are the wires of a micrometer by which the star's image is twice bisected, the object-glass being reversed between the bisections. A measure of the star's zenith distance is thus obtained which is remarkably free from instrumental errors. During the past two years a zenithal telescope of 3-feet focus, filled with water, has been used upon the same star to determine whether the aberration is altered in amount by the passage of the star's light through a considerable thickness of a refracting medium—a question which has been raised by some continental astronomers. The observations hitherto have negatived the anticipated alteration.

Equatorially-mounted telescopes have always been regarded as of secondary importance at Greenwich. When Sir George Airy entered office he found the equipment in this department, to use his own word, "contemptible." The best equatorial then was one presented by Sir G. Shuckburgh, made by Ramsden, with a 4-inch object-glass, a shaky mounting, and in a very bad situation. In 1837 a 6¾-inch glass was presented to the Observatory by the late Rev. R. Sheepshanks; it was a somewhat inferior one, and it was mounted with circles too small for accurate measurements. So about fifteen years ago Sir George Airy planned the Great Equatorial, deeming it essential that the Observatory should have a first-rate instrument of the class, even if it was for a time regarded as a luxury. The instrument (without the covering dome) is figured in the annexed cut. Its object-glass, by Merz, of Munich, is of 12¾-inch aperture, and 17 feet 6 inches focus. The mounting is of the English form, which admits of large circles, and such were regarded as essential; the hour circle is 6 feet diameter, that for Declination 4 feet. The polar frame is formed of iron columns firmly braced, and connected at top and bottom by oval iron frames. Except the tube, which is of wood, every large part is of iron, and the whole was designed with the highest engineering skill. Gas is carried to all the micrometers about the instrument, and to the eye-piece; and galvanic wires are led to a chronographic touch-piece at the eye-end and to a chronometer there also, which is always in front of the observer at the telescope, and is controlled by the Sidereal Motor Clock. An observing chair, moving up and down upon a travelling frame, enables the observer to command all positions without leaving his seat. The driving-clock is mounted in a lower apartment; it consists of a water turbine controlled by a conical pendulum.

We have spoken hitherto solely of the tools of the Greenwich observers. Let us now pass in review the works that have been actually accomplished thereby in furtherance of the objects for which the Observatory was constituted. We begin with the formation of star catalogues, for these are the foundations of exact astronomy. Flamsteed formed a catalogue of 3,310 stars, that remained the standard work of its kind for half a century, and has, in respect of selection and nomenclature of the stars it comprised, served as the basis of every catalogue since produced. Halley did nothing in this department. Bradley worked wonders; he laid the foundations of the present fabric

FIG. 4.



THE GREAT EQUATORIAL.

of mensurative astronomy. In 1750, when his new instruments were mounted, he began the daily course of meridional observations of principal stars, sun, moon, and planets which has continued without interruption (save by bad weather) to the present time. His star observations were incorporated into a catalogue (which comprised 3,222 stars) by Bessel, and, with values of the constants of refraction, aberration, precession, and other elements deduced therefrom, was brought forth by the illustrious Königsberg astronomer in a great work, whose title expresses its character—we allude to the "Fundamenta Astronomiæ." The rich harvest that Bradley reaped has not yet had all its good grain thrashed out, for at this moment several German astronomers are engaged in a re-reduction of his observations, believing that they can get from them even more than Bessel obtained. Maskelyne did not aim at producing a great catalogue; he confined himself to thirty-six of the principal stars, whose places he sought to fix with the utmost possible exactness, in order that they might serve as reference-points of the first order. Pond accumulated a long series of observations, and produced a catalogue of 1,112 stars, which was the most valuable contribution to the sidereal astronomy of the time, and is second in accuracy to no modern catalogue. The present Astronomer Royal has already produced four large catalogues; the first from the observations during the twelve years 1836–47; the second from the six years 1848–53; the third from the seven years 1854–60, and the fourth from the seven years 1861–67. The numbers of stars in these catalogues average over 2,000 each; each contains all the fundamentals, and together they include the greater portion if not all the stars visible at Greenwich down to the fifth magnitude, and the majority of the sixth. They are sought all the world over, wherever accurate astronomy is pursued for its own sake or for geographical or geodetical purposes, and they have served and will serve again as the best materials for researches upon the proper motion of the stars or of the solar system.

Then as to the moon; Greenwich has alone sufficiently supplied the investigators of her movements from the time of Newton to the present. At so critical a period was the Observatory established that Flamsteed actually fed Newton, so to speak, from hand to mouth with places of the moon for perfecting the lunar theory which the latter was then deducing from his theory of universal gravitation; and there is an entry in Flamsteed's note-book of the author of the "Principia" coming down to Greenwich one Sunday evening for twelve observations of the moon of which he was in urgent need. Halley, though he did no star work, made some moon observations, and compared them with his own lunar tables; it was the only good piece of regular work that he did. He was the exception we previously spoke of to the consentaneous character of the Royal Astronomers; and his case shows that a man may be a great astronomer, and yet fail in the office of Royal Observator, which requires the exercise of no small amount of self-denial. The lunar observations of Bradley, Maskelyne, and Pond, have to be spoken of connectedly, for they were reduced *en masse* by the present Astronomer Royal, and they form the basis of the Lunar Tables that are now in almost universal use. But before this stupendous uniform reduction, the Greenwich Observatory had furnished 1,200 moon observations to improve the tables of Mayer, which were the first generally available for nautical longitudes; 3,000 were employed by Burg for his tables, which so completely satisfied the conditions of a prize for such tables offered by the Consular Government of France, that the First Consul doubled the prize ; and of the 4,000 which were employed by Burckhardt to correct the lunar elements for his famous tables (which served for nearly fifty years prior to 1862 as the basis for all navigational predictions), nearly the whole must have been derived from Greenwich.

The great lunar reductions previously alluded to embraced nearly 9,000 Greenwich observations of the moon, made between the years 1750 and 1830—a series without a parallel. The first fruit of their reduction was a general correction by Sir George Airy of the received elements of the moon's orbit. The next was the discovery by Prof. Hansen, of Gotha, of two inequalities of long period in the moon's motion, depending upon the direct and indirect action of the planet Venus. And what may be considered for the present as the ultimate outcome, was the construction by Hansen of the great Lunar Tables that bear his name, which represent the motions of our satellite with an accuracy surpassing all others, and abundantly sufficient for the preparation of reliable nautical ephemerides. Hansen's tables are used for all the important "Nautical Almanacs" of the world, with one exception, that of America, for which special tables were previously prepared, embodying, however, the corrections derived from the long suite of Greenwich observations.

If we look to other planetary tables, we find the same dependence for their data upon our National Observatory. The tables now used for Jupiter, Saturn and Uranus are those by Bouvard (1821), and they depend mainly upon the observations of those planets by Bradley, Maskelyne and Pond. The tables of Mercury, Venus, and Mars are Le Verrier's, based chiefly upon the Greenwich observations from 1750 to 1830, which were, with the rest of the planetary observations for that period, reduced by Sir George Airy *en masse*, like the lunar observations. For the sun, Le Verrier's Tables are also used ; and they depend upon a century's Greenwich observations. The current tables of Neptune are those by Professor Newcomb, for which Greenwich found the major part of the observational data. Throughout the whole series of Planetary Tables that belong to the period of accurate astronomy, there is such a broad reliance for data upon Greenwich, and such comparatively small support derived from other places, that it is evident there was no excessive flattery in Baron Zach's assertion that our astronomical tables would have been as perfect as they are if no other observatory had ever existed. All this we adduce, not with the idea of glorifying a national institution, but merely to aid the full conception of its mission, which was defined in Sir John Herschel's always happy words to be "to furnish now and in all future time the best and most perfect data by which the laws of the lunar and planetary movements as developed by theory can be, compared with observations."

It will be easily seen that with all things organised to this end there is little room for such work as double-star measures, celestial photography, delineations of planets and nebulæ, spectroscopy, &c. Some of these have occasionally been taken up for a time, but none of them have been, or indeed could be, systematically followed.

Of late, however, there has been a tendency in some subjects of this character to overrun the powers of attention of amateurs, to whom they have been left; and it has been suggested that inasmuch as they ought to be followed by the State, and Greenwich as at present constituted could not undertake the work, a special Observatory ought to be established and devoted to Astronomical Physics. The systematic record of solar phenomena (sun-spots, gaseous eruptions, &c.) has been mentioned as in immediate need of pursuit. Not unnaturally a counter question arose whether all that it was desirable for the State to undertake could not be done at Greenwich, and the Astronomer Royal laid his views upon the general question before the Board of Visitors at their meeting on June 1 last, in the following terms:—

"The tendency of late discoveries and consequent discussions in astronomy has been, not to withdraw attention from the exact departments of astronomy, but to add greatly to the public interest in those which are less severely definite. And this has become so strong, that I think it may well be a subject of consideration by the Board of Visitors whether observations bearing upon some of those trains of discovery should not be included in the ordinary system of the Royal Observatory. The criteria which, as appears to me, may be properly adopted in the selection or rejection of subjects of observation are these: Observations which can be made at any convenient times, which do not require telescopes of the largest size, and which do not imply constant expense, ought to be left to private observers. Observations which demand larger telescopes, and especially observations which must be carried on in continual routine and with considerable expense, can only be maintained at a public observatory. The claims of each subject must be separately considered; but there can be no doubt that a very powerful demand for attention is made when private persons have been induced to continue observations for a long time at considerable current expense, and when plausible evidence is given of the connexion of results thus obtained with other cosmical elements. I think that these considerations exclude measures of double stars at the Royal Observatory, but they leave an opening for the scrutiny of nebulæ, planets, &c., and possibly (but I speak in doubt) of solar spectroscopy. But I have no doubt that they fully sanction the undertaking a continued series of observations of solar spots. The character of the Observatory would be somewhat changed by this innovation, but not, as I imagine, in a direction to which any objection can be made. It would become, *pro tanto*, a physical observatory; and possibly in time its operations might be extended still further in a physical direction."

Upon the effect of these statements it would be premature to speak. It is, however, generally understood that the Board decided upon the advisability of extending the Observatory system so far (for the present) as to include regular photographic record of solar spots and systematic solar spectroscopy. The Visitors are an intermediary body: before full effect can be given to their decision the Treasury must be appealed to for funds for the first cost of instruments and the running expense of an increased personal staff.

Chronometers have such a direct connection with navigation and sea-longitude, that not unnaturally Greenwich has been identified with the testing of these instruments from their invention to the present day; and now it is the chief depôt for Government marine chronometers. All business of their purchase, trial, and repair, is transacted at the Observatory; and usually about 200 chronometers are there under rating for issue to H.M. ships. Every year there is a competitive trial open to all makers, during which the chronometers are exposed to a wide range of temperature: four or six of the best instruments are each year purchased at good prices, and it is doubtless to these trials and to the general Government patronage of the trade that the supreme excellence of British chronometers is due.

Into the department of Time and its distribution we need not enter further than to say that a signal ball is dropped at one o'clock daily at the Observatory, and another ball at Deal is dropped by direct current from the Observatory; and that every hour accurate electric signals go forth from Greenwich, which are variously distributed over the country; one of them, that at 10 A.M., passing through well-nigh all the important telegraph lines in England. This department of the Observatory was, however, fully described in a previous number of the POPULAR SCIENCE REVIEW (October 1870).

A magnetical and meteorolgical department was established in 1840, and till 1847 eye-observations of its instruments were made every two hours, day and night. In 1848 photographic registration was introduced, and from then till now there has been an unceasing record of the movements of the declination, horizontal force, and vertical force magnetometers, as well as of the barometer and the dry and wet bulb thermometers. The anemometers, for direction, force and velocity of the wind, and also a pluviometer, register themselves mechanically. The magnetic observations and registers to 1863 have been discussed, and the epitomised results form the subjects of various memoirs in recent volumes of the *Philosophical Transactions.* It may be mentioned that among other points these discussions negative the existence of a decennial magnetic period related to the period of solar spot activity. A great discussion of temperature records from 1848 to 1868 is now in progress. Within the past few years an important, and we believe unique, addition has been made to the photographic recording department. The spontaneous galvanic "earth-currents" that at times become so intense as to interfere with telegraphic operations, have been made to record themselves perpetually by reflecting galvanometers connected with special wires running in N.-S. and E.-W. directions through the Observatory, and attached to earth-plates at their extremities. A discussion of some of the registers has shown that these currents are related to the earth's magnetism in its disturbed state —as during auroral displays—but apparently not in its tranquil state.

In conclusion, it should be stated that the Greenwich observations of all kinds are published in yearly 4to. volumes of nearly 1,000 pages each, in which every observation is set down in the utmost detail, with every instrumental reading as it is recorded by the observer, and (especially in the case of the astronomical observations) with every step in the reductions exhibited, down to the final results, which are given in such a form as to be directly available to the theoretical investigator.

THE RECENT FOSSIL MAN.

By J. MORRIS, F.G.S.

Professor of Geology in University College, London.

[PLATE LXXXVII.]

THE subject of the antiquity of man has of late years attracted considerable attention, and the terms palæolithic and neolithic have become nearly as familiar as those of the stone and iron age of former years. For preconceived opinions on this point, and the apparent doubtful evidence of the association of the human species with those of the extinct mammalia, strengthened the belief of the appearance of man only after the great physical changes had brought about their disappearance. Hence arose, partly from want of careful observation, much controversy on the subject, and, although maintained by some, the opinion has been confirmed by the re-examination of several of the cases cited, as well as by recent discoveries, that the remains of man lie entombed in earlier graves than those where

The rude forefathers of the hamlet sleep.

Among those who carefully investigated and prominently brought the subject forward were Mr. Prestwich * and the late Dr. Falconer, after their examination of the discoveries near Abbeville by M. Boucher de Perthes and of the Brixham cave; the facts of the contemporaneity of the works of man with the remains of extinct mammals were still contested by Elie de Beaumont and others; but the opinions of Mr. Prestwich were corroborated by Mr. Evans, Mr. Flower, Sir J. Lubbock, Sir C. Lyell, and by MM. Lartet and Christy, and Gaudry in France.

The occurrence of human remains are far more rare in caverns or other deposits than those of the works of man, and hence every additional fact is worthy of careful examination.

* "Phil. Trans." 1860, Pt. II. p. 277.

Dr. Buckland, in 1824,* discovered in the Paviland Cave, Glamorgan, beneath a shallow covering of earth, nearly the entire left side of a human female skeleton, which, he states, was clearly not coeval with the antediluvian bones of the extinct species of elephant, rhinoceros, hyæna, bear, &c. found there; but that this exposed and solitary cave had, at some time or other, been the scene of human habitation, not only from the charcoal and fragments of recent bone, but that the ivory rods and rings in contact with the skeleton were certainly made from part of the antediluvian tusks that lay in the same cave. Besides the skeleton of the "Red Lady of Paviland," human bones have been fouud by Col. Wood in the "Spritsail-Tor" cave, and in the ossiferous fissure of Mewslade, in the Peninsula of Gower.

Dr. Schmerling, however, in 1833, announced the discovery, in the Engi and Engihoul Caves in the Valley of the Meuse, of the bones of man, associated with those of recent and extinct mammals—rhinoceros, horse, elephant, bear, hyæna; the bones were indiscriminately mixed together, and the cave earth did not seem to have been subsequently disturbed.

M. Marcel de Serres, in his exhaustive essay for that period (1838), on "Bone Caverns," cites many instances of the co-occurrence of human remains: in America (Kentucky) with the megalonyx, bear, deer, and bison; in Franconia with extinct species; in France, in the departments of Lozère, Gard, and l'Aude.

M. E. Lartet, in his paper on "The Co-existence of Man and the Great Fossil Mammalia," published in the "Annales des Sciences Naturelles" (Ser. IV. vol. xv. p. 178), after his examination of the Aurignac Cave, near St. Gaudens, states that, not only was man cotemporary with the mammoth and rhinoceros, but, like the natives of Africa at present, used the latter as an article of food; and that the human remains are of great antiquity, as they were associated with the bison, reindeer, megaceros, hyæna, and *Ursus spelæus*, the latter, according to M. Lartet, being the earliest which disappeared of the group of the great mammalia, and which age—the earliest of primitive man—was followed successively by the age of the elephant and the rhinoceros, the age of the reindeer, and the age of the aurochs.

The cavern of Bize and others, in the vicinity of Narbonne, are equally remarkable for the similar association of man with the *Ursus spelæus*, *Hyæna spelæa*, *Rhinoceros tichorhinus*, and reindeer, with about sixteen other species, of which the bones of the ox, deer, and horse were the most numerous,

* "Reliquiæ Diluvianæ," p. 87.

THE NEWLY-FOUND FOSSIL MAN

together with the remains of birds. These were first noticed by M. Tournal in 1827, and subsequently by MM. M. de Serres, Christol, Dumas, P. Gervais, and Brinckmann, the latter two authors giving an account of their observations in the "Messager du Midi" (Montpellier, 1864), and they remark that the occurrence of the reindeer does not indicate so great geological antiquity, although still remote, but that the climate of central and southern Europe was considerably colder, and became subsequently modified, so as in part to cause the retreat of this animal to the northern regions.

In the bone caves of Dordogne, investigated by MM. Lartet and Christy, the most abundant animal was the reindeer, which evidently formed the principal article of food of the cave-dwellers, and, together with the ibex and the chamois, afford evidence that a considerable change of climate has taken place, for the former animal could not now exist in the south of France. "These caves are particularly interesting, because, so far at least as we can judge from the present state of the evidence, they belong to M. Lartet's reindeer period, and tend, therefore, to connect the later, or polished-stone age, with the period of the river drifts and great extinct mammalia, a period about which we had previously very little information." *

The discovery, in the early part of this year, of a human skeleton in one of the great caverns (Baoussé-roussé) of the Italian frontier, has again drawn attention to the subject, and excited much public curiosity at Mentone when announced in "Le Courrier de Menton," of April 7, with a plate of "Le Troglodite de Menton," from which journal the following notes are abstracted.† The cave is one of a series which occur in a compact limestone, and are known as *les grottes des roches rouges*. The caves are from 50 to 150 feet from the sea, and 40 or 50 feet above it, and all open to the south.

The discovery was made by Dr. E. Rivière, who has been appointed by the French Government to examine and study the palæontology and prehistoric period of Liguria. After obtaining an immense quantity of bones and teeth of bears, gigantic stags, hyænas, rhinoceros, and other animals, from the neighbouring quarries, Dr. Rivière commenced the exploration of the caverns. The cavern above alluded to is near the line of railway from Mentone to Vintimille, and the skeleton was found beneath a layer of earth several yards in thickness, and is in a very fine and remarkable state of preservation,

* Sir J. Lubbock, "Prehistoric Times," p. 245.

† The Plate has been reproduced from the "Geological Magazine" for June, whose Editors kindly lent it.

which may be possibly due to the nature of the earth in which it was imbedded and the continued dryness of the spot in which it was placed. The skeleton, which is that of an ordinary-sized man, is entire, with the exception of the ribs, which have been broken by the pressure of the superincumbent earth. The teeth and lower jaw are in a good state of preservation; the skull differs from the rest of the bones in being of a deep brick-red colour, and the part of it resting on the ground is broken. The legs crossed in a natural position, and the arms folded near the head, seem to infer that the man to whom they belonged died in his sleep, and was carefully covered over without disturbing the earth beneath. A great number of small shells, similar to those living in the adjacent sea, and deer's teeth, all pierced with a hole, were close to the skull, leading to the belief that they were either twined in the hair or formed part of a head-dress. Round the skeleton were found a great quantity of stone implements, as scrapers, chisels, and axes, and also bone needles; the form of the latter seem to have been produced by having been rubbed down on some hard substance. Associated with these were bones of animals, and, among others, the lower jaws of herbivora. Behind the loins there was a stone, also one behind the head, and between the latter were two of the largest stone implements which have been found in these caves.

Mr. M. Moggridge, who is well acquainted with the locality, and has visited the cave, has kindly furnished the following points as most important in reference to the skeleton:—

1. The rock in which the cave is situated is Oolitic or Jurassic limestone, which is very cavernous.
2. The cave is narrow and lofty, and deep.
3. The floor at the mouth is 9 ft. above the bottom, but in the interior the depth is much greater.
4. No remains of the extinct animals (*Ursus spelæus*, &c.) occur above the skeleton.
5. I believe it was a case of interment—of a person of some consequence—of the stone age; not early in that period.

Mr. G. W. Nicholl, who has also examined the cavern, states: "To my mind the skeleton proves clearly a case of burial; the stones at the back and in front of it showed this pretty clearly, for they were evidently so placed by design, as if to roughly mark out the place of sepulture. M. Rivière did not seem to think much of them, for he had removed them all before the skeleton was photographed. The sketch in the paper shows the stones at the back very truly, but it does not show those in front, which were placed further from the body and more irregularly than those at the back. If, then, the man was buried, he might or might not have been co-existent with the extinct animals of whose bones so many were found in the cave earth in which the skeleton was interred. But close by, in front of the cave there was, as has been suggested to me, irresistible evidence that man lived in those caves (there are four or five of them) at the same time that animals now extinct were living in the neighbourhood. For in front of the cave is a *talus*, formed of breccia fallen from the cliff above. The stones forming this breccia are as sharp and angular as when they fell from the cliff, and they are cemented by lime and iron into a hard conglomerate. In this conglomerate, whilst making the railway cutting two years ago, were found numerous implements of flint—knives, spear and arrow heads, and cores of flints, from which these had been broken off, also bones of animals now extinct, and bones of animals now existing. Now as the stones forming the conglomerate are so very sharp and angular, it seems to me to be very conclusive evidence that they are lying where they first fell, and that the bones and flints amongst them are also lying just where they were thrown by the inhabitants of the caves above."

The report of Dr. Rivière will no doubt lay all the facts before us, as to this and the other caves, to which attention had been drawn by M. W. De Suiram, in 1869, and also by Dr. Falconer, who, in 1858, after examining at Nice the brecciated mass of human bones discovered near St. Hospice, visited the Rocco Rosso caverns near Mentone, which had previously yielded such abundant relics of long-continued human occupation, upon the exploration of M. François Forel.

In connection with this subject Mr. John Evans, in his recent work on "Ancient Stone Implements," June, 1872, states that the difference in the faunas of the palæolithic and neolithic periods is of great importance, as affording some guide in judging of the antiquity of human remains when found in caverns without any characteristic weapons or implements; such, for instance, as the human skull cited by Mr. Boyd Dawkins as having been found in a cave at the head of Cheddar Pass, in Somersetshire. For it must never be forgotten that the occupation of caves by man is not confined to any definite period; and that even in the case of the discovery of objects of human workmanship in direct association with the remains of the Pleistocene extinct mammals, their contemporaneity cannot be proved without careful observation of the circumstances under which they occur, even if then. Another point may also be here mentioned, namely, that where there is evidence of the occupation of a cavern by man, and also by large carnivores, they can hardly have been tenants in common, but the one must have preceded the other, or possibly the occupation by each may have alternated more than once.

REVIEWS.

MAN'S ORIGIN.*

THERE are some who would doubt the propriety of translating a work like the present one into the English tongue, and we almost fancy that the editor is to some extent of this class. But for ourselves we do not perceive its disadvantage. Indeed, on the other hand, we approve of the effort to introduce into our language so fearless and outspoken and honest a labour as that which Dr. Büchner has performed. In regard to the editor, we think he has been engaged in a task which must in every page have run hard against his conscience; and we wonder therefore that, if he did perform a task which must have so completely gone foul of his ideas, he did not conceal his name, and so have, to a certain extent, prevented the injurious influence—injurious at least to the author's views—which must follow a preface in which he states that he "is by no means inclined to accept all the results at which Dr. Büchner has arrived." Indeed, it seems us that Mr. Dallas must have found almost all of Dr. Büchner's views unacceptable; for it certainly appears that the author's one distinct and clearly-drawn conclusion is, that there is no such thing as a Deity, and his other, that man's existence comes to an end as soon as his life is finished. However, we must thank Mr. Dallas for having given us an excellent and clear translation of a most interesting and forcibly-written book, in which the author puts forward his views as clearly as he did in his former work, "Force and Matter," but without any of that offensive manner which created for his previous essay so many fierce opponents. Further gratitude is due to Mr. Dallas for not inserting a number of sharp, opposing foot-notes, showing what parts of the work he especially objected to. The book contains nothing that will be new to those who have followed minutely anthropological science for the past ten years, though it certainly contains, in a pithy and masterly style, nearly everything that has been achieved by the English and foreign *savants* in that space of time; but to the general reader, or to the dabbler in science, it offers a masterly summary of the facts, reflections, and ultimate conclusions that have been put forward in regard to man's origin during the period referred to. Indeed, in this respect we know of no volume that can be at all compared with it, not only for the facts it contains, but for the scholarly shape in which these facts have been put together and laid before

* "Man in the Past, Present, and Future," from the German of Dr. L. Büchner. By W. S. Dallas, F.L.S. London: Asher & Co., 1872.

the reader. Of course, in such a work the notes and references must be manifold, and we think that the method of placing them at the end of the volume both tends to lend the pages a better shape and to make the notes more carefully studied by the reader who refers to them.

The author regrets that his book had almost gone through the press before Darwin's celebrated volumes on "The Descent of Man" had made their appearance; but he congratulates himself on the fact that he has arrived, without the great English naturalist's aid, at practically the same conclusions. Dr. Büchner, of course, traces man through the different tribes which gradually, step by step, lead down almost to the brute. He gives numerous quotations from travellers of repute, showing the low mental, moral, and physical condition of these savages, and then he points out how, even in their anatomy, they approach the apes. Then he gives the numerous instances which have, during the past ten years, been brought forward regarding pre-historic man, and he shows conclusively that in many of the examples the skeleton—especially the skull—possessed, in a marked degree, characters that are now not exhibited by any living creature, save certain of the quadrumana. In all his observations on this point of course it is impossible to go against him. We mean, that for any man at all conversant with anatomy, such a line of reasoning as would be opposed to Herr Büchner would be in the highest degree absurd. Here you have man possessing the same skeletal formation, or nearly so; the same heart, lungs, thyroid, diaphragm, liver, stomach, and intestines; same kidneys, same spleen, same pancreas, same organs of reproduction, nearly the same hands, and a closer allied brain than that possessed by any other mammal. And notwithstanding all these important details of anatomy you are asked to believe that man was made from clay—though he hardly contains a particle of its most universal constituent, alumina. Assuredly, all reasonable men will go in with Dr. Büchner, and will believe that man must have come from the apes, a doctrine which has so much to support it, rather than the other view, which is merely a questionable hypothesis of the almost untranslatable Holy Writ, and which, besides having nothing to support it, is opposed on the most important chemical grounds to which we have alluded.

There is only one instance in which it seems to us that the author is incorrect, and that is a case that may fairly be forgiven him, for the best authorities in France are of the same opinion. It is in reference to the Moulin-Quignon jaw-bone, which he in common with the Frenchmen pronounces to have been a fossil. We cannot regard the bone as in the least degree antique. It seems to us unquestionable that it was a bone of recent formation, which was imposed upon the French savants who obtained it. In our opinion Mr. Busk's method of examination was the only correct one. He sawed the bone through, and obtained the same peculiar smell which is so well known to those who manipulate recent bones. It is utterly out of the question that this peculiar smell could have been obtained from any but a recent bone, and therefore we regard the Moulin-Quignon jaw-bone as representing a scientific canard. However, there are other specimens whose antiquity is beyond question, and of them Herr Dr. Büchner gives a good and clear account. Let us take the Neanderthal skull and La Naulette jaw as examples:—

"The most important of these remains is the celebrated *Neanderthal* skull already mentioned * * * which Huxley describes as the most ape-like of all the human skulls that he has ever seen, and of which he says that in its examination we meet with ape-like characters in all parts, and also that it has the greatest similarity with the existing Australian skulls, and with the ancient Borreby skulls. Huxley also states that this skull is by no means an *isolated* phenomenon, but that it is only the extreme term of a long series of bestial, or, at least, very lowly-developed human skulls of the past and present periods." Again, on the same subject, the author writes: "in the year 1866 a fragment of a human jaw with very remarkable and animal characters was found by the indefatigable Belgian cave-explorer, Dr. Edward Dupont, in the *Trou de la Naulette*, a lone cave situated on the bank of the little river Lesse, not far from the village of Chaleux. It was in a deposit of river-loam, covered with a layer of stalagmite, and at a depth of about four metres. The most remarkable of its characters, besides the comparative thickness and rounded form of the bone, and its elliptical dental curve, *is the almost entire absence of the chin.* The projecting or prominent chin is so distinctive a character of man that Linné, the great lawgiver of systematic zoology, could name no better bodily distinctions between man and animals, than the upright position and prominent chin of the former. In animals, instead of projecting the chin retreats, and the jaw of La Naulette holds an intermediate position between the two; where the projection of the chin ought to be, it exhibits a line descending perpendicularly. Moreover the cavities destined for the reception of the canine teeth are remarkably wide and large as in animals, although the canines themselves are closely contiguous to the incisors, and *not* molars, and the jaw is thus shewn to be undoubtedly of human origin. But what is still more remarkable than this is the circumstance that the three hinder or persistent molars present exactly the same relative sizes as is usual in the anthro-promorphous apes. Thus whilst in the higher races of man, the three true molars are so arranged that the first is the largest and the last or hindermost the smallest, we find in the dentition of the lower races, such as the Malays and Negroes, that all the three molars are of equal size, and throughout larger than usual. But in the anthropoid apes the first true molar is the smallest, and the last the largest, and this is the case also in this fossil human jaw, the last or hindermost molar of which even appears to have possessed five roots. To all this may be added that the inner surface of the jaw at the point of the so-called suture or symphisis, behind the incisor teeth, forms a line obliquely directed upwards, and consequently leaves no doubt as to the prognathism of its former possessor."

We fancy that the above quotation will suffice to show that the author has not dealt in a slovenly fashion with his book, but has been at care in dealing with points of evidence; he gives them fully, minutely, and thoroughly. The other portions of the book are alike, and they all bear the author along to the conclusion regarding man, which he expresses in his last chapter. We must now leave the work in our readers' hands, begging of them to read it carefully, and not either from enthusiasm or from malice to do this author's efforts any injustice.

THE FALLACIES OF DARWINISM.*

THE opponents of Darwinism are numerous enough, but we may add by no means intelligent enough to do anything but help to extend Mr. Darwin's views by bringing very feeble arguments against Darwinian views before people who are ignorant of the whole subject, but who are intelligent enough to perceive fallacies when put before them, and who, once they are called upon to consider whether man has descended from the other animals or has been *separately* and *similarly* made, very naturally conclude that the former is by far the most probable of the two. There is another quality, too, which the opponents of Darwinism possess, and which certainly by no means tends to render their arguments more convincing, and that is the faculty of abuse. In every chapter of their works there is a summing up in which they express their astonishment at the very existence of unbelievers, and that too in terms usually of the very coarsest abuse that can be conceived. How unlike the whole number of Mr. Darwin's or Mr. Wallace's books. In no single passage do they attempt to revile—as indeed they might most easily—the abortive and often absurd arguments of their opponents. Dr. Bree's is a book of the ordinary class, an abusive review of Mr. Darwin's theory, as unlike Mr. Mivart's able essay as anything possible to conceive, and by no means to be placed alongside it or compared with it in any respect. Yet the author has not been without some reading on the point. He has found out the names of two or three naturalists of repute who do not hold the Darwinian doctrines, but he is by no means to be considered as having Professor Owen on his side, though he evidently considers that the distinguished naturalist leans towards his views. We may not call Professor Owen a Darwinian, but if we could take Mr. Darwin's name from the doctrine, we doubt not Professor Owen would hold to it, for indeed he alleges that he put forward similar views quite thirty years since. Of the other names which Dr. Bree has got, we fail to see any of special importance in a zoological respect except Agassiz, though of course we could ourselves name some few more whom possibly Dr. Bree has never heard of. But if Dr. Bree were to ask to us to name those eminent masters of zoological science, which, at first opponents of Darwinism, have since become its firm supporters, he would doubtless be a little astonished at the multitude of European and American names we could put before him. The book is simply a series of bitter attacks on the views of Mr. Darwin, Professor Huxley, and the leading evolutionists of this country and the Continent. Of course there is some reason given for not holding particular theories arising out of the main doctrine, but these are indeed few, and not particular; while the arguments of the author are of the old-womanish kind, and invariably end with allusion to the Creator, and an assertion that therefore the doctrine in question must be absurd, because it is out of harmony with the author's particular religious belief. Let us take a paragraph from the chapter in which the author exhibits his knowledge of Van Baër's work. Here, after denying Mivart's view, he says:—

* "An Exposition of Fallacies in the Hypothesis of Mr. Darwin." By C. R. Bree, M.D., F.L.S. London: Longmans, 1872.

"We *know* that the organic and inorganic worlds have been formed by a thoughtful reasoning being; but the 'how' or 'why' are hidden among the mysteries of Omnipotence . . . ." and further, Dr. Bree having nearly exhausted his vocabulary of abusive epithets against Mr. Darwin and those who believe with him, says of the Darwinian theory that "if proved in every point to be true, it would still leave the fact of special creation in all its wonderful mystery. The organic *cannot* be formed from the inorganic; nor could the organic, even if it were so formed, be endowed by any physical force with the laws and properties of life. Go on still in speculation, and I ask whence the inorganic—its beginning, its ending, its grand and inexplicable laws, which the physicist in vain attempts to correlate with the vital? whence gravitation, and what? the sidereal system and its movements? the spirit that breathes, through illimitable space, and lives through an eternity of time?"

To a shallow mind it may seem a very grandiose speculation, but suppose we go further, and assuming the existence of the Creator, ask the author whence did he come, what answer will he give? We dislike this kind of speculation intensely, it carries the narrow-minded person off his balance, but to the man of thought it is simply the verbose and shallow wanderings of a mind which has not yet arrived at the conclusion that the whole subject he has been talking of is most probably for ever removed from the speculations of man. It is an absurd wandering from the matter of animal life and from the very simple though yet unproven question, whence comes man, from the apes or from mud?

AIR AND RAIN.*

HERE we have a book which is new in its subject and mode of treatment; and it is a large one too, extending over more than five hundred pages. What shall we say of it? Is it good or bad, too long or too short; too much confined to facts without deductions; or given to over-extended generalisations, to the exclusion of accurate observation? We must answer these several questions by stating that the work appears to us a good one, but unsuited either to the general reader or to the scientific man. It is unsatisfactory for the former because of the quantity of tabular matter which is introduced into its pages, and to the latter because the author indulges in a great series of observations which are not clearly put, and which are intended for general audiences. Still, it seems to us to be a good book: it is one with which we have hardly a single fault to find; and we give the author the highest praise for the manly and fearless manner in which he has in all cases spoken his mind, and for the intense labour which the work must have entailed by reason of the numerous analyses which it contains of air and water made over every portion of the kingdom. It would be a labour without end to attempt anything like a full notice of the author's various and manifold researches. We shall

* "Air and Rain: the Beginnings of a Chemical Climatology." By Robert Angus Smith, Ph.D., F.R.S., F.C.S. London: Longmans, 1872.

content ourselves with a few remarks on the author's opinions relative to that important question whether drains, as we have in London, or middens, which some of the manufacturing towns possess, are, all circumstances considered, the most convenient and healthy. Thus we shall leave the various analyses he publishes out of the question, and shall pass his drawings of the products found by the microscope in the air of certain localities altogether aside. The latter we do more especially because the author has given us little or no idea of the enlargement of the specimens, and we fancy that in most cases low powers of 400 to 600 diameters have been most usually employed. We shall merely notice, too, the fact that he has been at pains, in conducting his analyses, to employ the most recent analytic methods—excluding Dr. Frankland's, and including Wanklyn & Chapman's method. Neither shall we deal with the author's views on the subject of meteorites, which we deem on a par with Sir William Thompson's notion, which was given to the British Association. But it seems to us that Dr. Smith's observations on the subject of crowding are most valuable, and we hope they will be read and carefully remembered by all those who have to do with the building of our towns and the general management of house property. Dr. Smith says, "There is a want of willingness to pnll down dangerous property, but a readiness to rush forward to save the life of the greatest crimimals. Reason is out of the question in the matter; we are misled by an uneducated feeling. We like to save property, forgetful that deadly weapons and poisons are subject to peculiar laws, and their indiscriminate use is forbidden to the nation. Houses that produce death are not property; as well might a man claim his debts as such. If a man sells unwholesome meat, the law interferes; if he sells the use of a room with fever in it, the nation seems not to complain. . . . The time must come—and the sooner the better—when it shall be enacted that no land shall contain more people per acre than we know, by experience in several places, can live healthily thereon. The same thing must be said regarding houses . . . because of the degradation of some of the population." On the midden versus sewer system, the author's opinions clearly lead us to this, that where water is abundantly flushed through the sewer, it is the best; otherwise the midden is infinitely superior, and causes much less death to the population. Dr. Smith says, he has "come long ago to the conclusion, that the water-closet is one of the greatest of luxuries invented in modern times; but also thinks that the midden is better than the bad sewer . . . The question is not a simple one for a *yes* or *no*, but an extremely complicated one, where many conditions must be balanced. This experiment, however, is desirable—the examination of the air outside of the houses of a sewered town and a midden town. I feel almost confident of the answer; indeed, the analyses in this volume may be said to give it, because the backs and fronts of the houses of a midden town give different airs—the backs giving worst; whereas the backs and fronts in a water-system town cannot be different, one would suppose. . . . I think it probable that, as matters are now conducted, the water system will be the worst in all houses not large enough to have sufficient separation. . . . We come to this, that the danger is outside in the midden system, and inside in the water system, where the danger does exist."

Dr. Smith's remarks are judiciously cautious, but they are to the point; and the multitude of analyses by which they are accompanied bear them fully out. Altogether, we are well pleased with the book, which, with a few deficiencies in style, is an admirable essay on a very difficult question.

MILLER'S CHEMISTRY: PHYSICS.*

THE fifth edition of Part II. of this excellent treatise on Chemistry is now before us, and we may just say a word or two about it. We remember well enough the appearance of the first edition of this work when we were at college some fifteen years ago. It was an excellent introduction to chemical physics—clear and intelligible to the lowest intellect, amply illustrated, and printed in a large and bold type. In what, then, does the present work differ from the first one? Well, it is nearly twice the size, contains a bulk of matter which renders it a terrible book to the readers of the first edition, and the matter is excellently arranged, and is all that the mere student of chemistry can require; indeed, it is much more than an ordinary chemical student requires. Still, there are some parts which ought to have additions made to them, and some which, in our opinion, might as well have been cut out. But, doubtless, that was out of the question in editing a work like the present one. Mr. McLeod has, we think, performed his task with extreme caution and excellent good taste; and he has introduced a quantity of matter in relation to spectrum analysis, and to the question which has lately occupied the Chemical Society—that of atomicity; thus bringing the book up to the time, so that it forms an excellent manual of physics for the chemical student.

GEOLOGICAL SURVEY OF OHIO.†

DURING the time occupied by the geological survey of the State, the Chief Geologist is required to make an annual report of the progress of the survey, with such necessary illustrations as may exemplify the same; and the present volume is the result of the field and other work for 1870. The determination of the geological structure of Ohio was not only important as bearing on the character, variety, and distribution of the mineral riches of the district, but also as showing the connection of the geological features of the country lying between the Atlantic and Mississipi. Already the number of formations known to exist in the State has been nearly

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* "Elements of Chemistry, Theoretical and Practical." By William Allen Miller, M.D., D.C.L., late Professor of Chemistry in King's College, London. Revised by H. McLeod, F.C.S. Part II. Chemical Physics. 5th edition. London: Longmans, 1872.

† "Geological Survey of Ohio: Report of Progress in 1870." By J. S. Newberry, Chief Geologist, and Assistants. 8vo., pp. 578, with maps and sections. Columbus, 1871.

doubled, and their general structure determined; but the final report will embody all the local details of the different counties, with the needful maps, plans, and sections necessary for their proper illustration.

From the present report it appears that the coal measures occupy a larger portion of the surface area than any other formation; and, from the number of economical substances associated with them, their character and distribution has been carefully studied. The first part comprises a sketch of the *lower* coal measures in north-eastern Ohio, by Mr. Newberry; from which it appears they contain seven or eight workable seams of coal, all of which lie below the celebrated Pittsburg seam, and include, in fact, the most important coal strata of the State. The coal measures do not form one symmetrical basin, but several troughs in a general way parallel with the axis of the great one of which they are parts. On the east side of each of these subordinate basins the strata rise, or are horizontal, and the easterly dip is neutralised; so that on the east line of the Columbiana county the section of the hills is nearly the same as that found on the banks of Killbuck, one hundred miles west—the average dip in this interval being not more than three feet to the mile. In tracing the strata from the western margin of the coal-field to the Pennsylvania line, some of the coal seams disappear, and others come in; and local changes are discoverable both in the development and purity of the different seams of coal and iron. The coals are of various qualities. The upper seams are well adapted for the generation of steam; below them are the cannel coals, which, although the difference in heating power is not great, contain a larger amount of ash than the English Wigan cannel; still, however, they can yield a large volume of good illuminating gas. The most valuable seam is the lowest, or Briar Hill coal, both for its thickness, purity, and being well adapted, in the raw state, for the smelting of iron ores. With regard to surface features, Mr. Newberry points out that the rivers, as the Killbuck and Tuscarawas, run in parallel synclinal valleys, and that the folding of the strata which formed these subordinate troughs and ridges in the coal basin first gave direction to the drainage streams of the region, and which lines of drainage have retained, through all subsequent mutations, the directions thus given them; and that this direction, as well as that of the main tributaries of the State, have been determined by the same causes that produced the great folds of the Alleghany mountains.

The other portion of the volume contains a report of labours in the second geological district, by E. B. Andrews, accompanied by maps of grouped sections, showing the strata of the lower coal measures in detail. The geology of Highland county, by E. Horton; which county contains a more extensive geological series than is to be found in any other county of the State, as it includes the Lower and Upper Silurian, Devonian, and Carboniferous formations, besides some Drift deposits and evidences of glacial action. The agricultural survey is by J. H. Klippart, and is intended to give a brief exposition of the physical and chemical characters of the various soils, their origin, the part they play in the growth of vegetation, the source of their fertility, and the theory of their impoverishment. The chemical report, by T. G. Wormley, is more specially devoted to the analyses of the coals, iron ores, furnace-slags, fire-clays, soils, limestones, and the processes by which they were effected. Sketches of the geology of Geauga and Holmes counties, by M. C. Read, and of Williams, Fulton, and Lucas counties, by C. K. Gilbert. On the present state of Iron Manufacture in Great Britain, by W. B. Potter. And, lastly, some interesting notes on the present state of the Steel industry, by H. Newton.

MAN'S ORIGIN AND DESTINY.*

A SINGULAR book this; one of its own kind, full of learning, and rather addressed by its style to those who are engaged in similar pursuits than intended for the populace. It is essentially a deistical work, not Atheistic, for the author concludes by expressing a belief that there is a Deity, and that we shall hereafter, when we have passed from this world, enjoy a life of intense happiness. However, it is not that portion of the book which contains the speculations of the author that is the most important. Especially those chapters—and they are numerous enough, as the book extends over more than 770 pages—which deal with the scriptural account of the Lord, and the various other writings, some of them very old, and others the works of the holy men of the middle ages, are full of interest, for they let us into secrets that were unknown to all but those who have made the biblical records the study of their lives. We cannot attempt to review the author's efforts for they are to be judged alone by those few Eastern scholars who can thoroughly follow the writer throughout. Still we see beyond question that he is quite correct in his onslaught on the supposed divine origin of the Pentateuch, and other biblical writings. Starting with Origen's question, he asks, "What man of good sense will ever persuade himself that there has been a first, a second, and a third day, and that these days have each of them had their morning and their evening, when there was as yet neither sun nor moon nor stars? What man is there so simple as to believe that God, personifying a gardener, planted a garden in the East? that the true tree of life was a real tree, which could be touched, and the fruit of which had the power of preserving life?" It is certainly too true that the great mass of people believe the tales of Moses, whilst they would not think of believing them if they were in the writing of a Greek philosopher, or a Rabbi, or a Mahometan, because they believe Moses to have been inspired. But we certainly agree with the learned author, that when we see that these books of Moses are full of repetitions and contradiction, we must give up all notion of their being inspired. A few of these contradictions may not be known to all, so we give them from the author: "The hesitation of Moses when he received the order to deliver the Israelites is mentioned twice in different terms. The miracle of the cloud on the tabernacle is related twice with different particulars. Jacob is made to be 84 years old when he took Leah to wife, while Dinah was scarcely seven years of age when she was violated by Shechem, and Simeon and Levi

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* On "Mankind, their Origin and Destiny," by an M.A. of Balliol College, Oxford. London: Longmans, 1872.

were scarcely twelve and eleven years old when they ravaged a city and put all the inhabitants to the sword. Some of the laws are mentioned twice, and each time they are different in it." These are but a sample of a host of such errors which the author dwells on. The book throughout is full of interest, and the plates representing various objects, chiefly from Eastern temples, are remarkably interesting. Altogether we are much pleased with the book, which is, however, of a class unsuited to our pages. We wish the author had given his name, for then we could more thoroughly thank him for the good efforts which he has made, and for which, even though he be unknown, we thank him.

RADIANT HEAT.*

HERE we have a book which cannot be termed a popular work in any sense of the word, but which may be read, we think, with interest even by those who have little knowledge of the subject, but who have minds capable of taking up questions of this kind. It consists of a series of memoirs chiefly on the subject of radiant heat. These have for the most part been contributed to the "Philosophical Transactions," and the "Philosophical Magazine," and they together form such an admirable essay on the whole subject that we fancy Professor Tyndall did rightly in reproducing them. Furthermore, they will certainly tend to let many understand who were ignorant before, of the nature of the dispute between the author and Professor Magnus. They will, from this point alone, be of service, for they will show how convincingly strong are the arguments Professor Tyndall advances, and how admirably his experiments have been devised and carried out. To our minds, individually, the most popularly interesting paper of the whole is that on calorescence, which is headed by those aptly-chosen lines from Lucretius. We see no reason why any of our readers should be unable to follow the author through this portion of his book, which is written in Professor Tyndall's usually eloquent, forcible, and clear style, and which is amply illustrated by woodcuts. Altogether the book is of course an excellent one, and we sincerely hope its sale will fully recompense its publishers.

GANOT'S POPULAR NATURAL PHILOSOPHY.†

WE do not know whether this book is to be regarded as superior to all our English books for beginners in natural philosophy; indeed, we think ourselves that it is not. Still, it must be admitted that it is a very

* "Contributions to Molecular Physics in the Domain of Radiant Heat," by John Tyndall, LL.D., F.R.S., Professor of Natural Philosophy in the Royal Institution. London: Longmans, 1872.

† "Natural Philosophy for General Readers and Young Persons," Translated and Edited from Ganot's "Cours Élémementaire de Physique." By E. Atkinson, Ph.D., F.C.S., Professor of Experimental Science in the Staff College. London: Longmans, 1872.

good book of its kind, and that it is by the same author as that of, perhaps, the best-known manual of physics. It is carefully prepared by the English editor, who has discharged his task of translation very well indeed; there being, as far as we have seen, an utter absence of any of that peculiar French idiom so objectionable in a translation. The book is nicely printed, and its illustrations are, in our estimation, the very best part of the whole volume. These are excellently done, and are no less in number than 414. We fancy we notice a few errors here and there in the optical illustrations; but they are defects by the absence of special rays rather than otherwise. In the next edition, too, it would be as well if Dr. Atkinson would look it over carefully, so as to avoid those errors which are absolutely inevitable in the first. We would point to one, for example, that certainly appears so to us. It is on p. 339, eight lines from the bottom. The author, speaking of the immense value of the spectrum analyses, says that "their extreme delicacy constitutes them a most valuable help in the *quantitative* analysis of the alkalies," &c. Surely he meant qualitative in this case. There are others also in the volume, but of much less importance. Altogether, the translation is very creditable to the editor.

ESSAYS ON ASTRONOMY.*

MR. PROCTOR has so long and so frequently contributed papers to our columns that we suppose our readers will think we are prejudiced in his favour. It is not so, however. We see Mr. Proctor's good qualities, and we see his faults also; but the latter are so very small in comparison with the former that they are almost insignificant. He has eminently the faculty—the too dangerous acquirement in the case of a scientific man—of writing. Let him take up any subject which he understands, and he is able to write there and then a long paper on the subject. Whilst most men would require to collect material, and, having done so, to arrange it, and, having settled the matter, would have considerable difficulty in committing their thoughts to paper, with Mr. Proctor it is done at once. This gives him a great advantage over his fellow-workers. But it has its disadvantages. It leads a man to contribute something to nearly every journal, and this is what Mr. Proctor does, and it is where, we think, he errs. If even it be granted that in the majority of cases he is but popularising that which he has aired in scientific arenas at an earlier date, he is, at all events, doing that which the majority of scientific men refrain from, and which is generally thought to give a man a character for light thinking rather than for serious heavy work. However, in the essays before us at present this idea will not occur to anyone. They are all serious papers, a few taken from our own pages, and others from various other journals, the majority of

* "Essays on Astronomy: a series of papers on Planets and Meteors, the Sun and Sun-surrounding Space," &c.; preceded by a sketch of the life and work of Sir John Herschel. By Richard A. Proctor, B.A., F.R.A.S. London: Longmans, 1872.

them appearing to be excerpted from the "Proceedings of the Royal Astronomical Society." All the papers are good, some of them indeed are excellent; but they have all come before scientific readers before, and are now addressed to the general public. The opening chapters are about the most generally interesting in the volume, as they deal with the life and labours of the late Sir John Herschel; but all will be found interesting and profitable reading.

BOTANY FOR BEGINNERS.*

WE think that Dr. Masters has not been guilty of a commercial mistake in publishing the book before us, the greater part of which originally appeared in the pages of the "Gardener's Chronicle." But altogether apart from the monetary aspect of the question we have reason to be grateful to the author for the trouble he has taken to write a genuinely popular work; a book, be it understood, which is absolutely devoid of the abominable twaddle of most popular treatises, but which is couched in language intelligible to every reader, which is so arranged that each difficulty is provided for, and each technicality is explained as the young student goes on, and which, nevertheless, does not keep the pupil for days learning the interminably dry details of the flower and stem in the abstract, but which plunges *in medias res* of botany at once. The first six chapters deal with various plants so common as to be accessible to every one even in this huge city, and the remaining four treat of the physiology and classification of vegetables. The illustrations are most of them novel to the student, and are nearly eighty in number. We certainly thank the author very heartily for so admirable a little book as the "Botany for Beginners."

ANTI-DARWINISTS.†

WE have before us two books whose whole aim is to show us the errors of the Darwinism doctrines, and the truth of the older views. We cannot agree with the conclusions of either. In fact, neither has a single argument to adduce that, resolved to its simplest elements, can resist the general doctrines of evolutionists; but the one is written in a true scientific spirit: we do not say that it has a great deal of science in its pages, but the spirit of science is there, calmness, coolness, patience. The other has not an atom of scientific value, and very little of mere literary worth either.

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* "Botany for Beginners: an Introduction to the Study of Plants." By Maxwell T. Masters, M.D., F.R.S. London: Bradbury and Evans, 1872.

† "The Higher Ministry of Nature, viewed in the light of Modern Science, and as an aid to advance Christian Philosophy." By J. R. Leifchild, M.A. London: Hodder and Stoughton, 1872.

"Esse and Posse: a Comparison of Divine Eternal Laws and Powers, as severally indicated in Fact, Faith, and Record." By H. T. Braithwaite, M.A. London: Longmans, 1872.

Both books we disapprove of because of the views they contain, but the one, that of Mr. Leifchild, we can commend to those who wish to read such a book, while for the other, which bears the name of a Mr. Braithwaite, we cannot say a syllable in favour of. In style it is affected, in reasoning fearfully shallow, in substance it has nothing upon its naked bones. It is, therefore, an empty, vacuous work, in no way to be recommended. On the other hand, Mr. Leifchild's work, though it is large, calmly written, dealing with its opponents in a truly Christian spirit, and above all things nicely written, is a poor argument against the thousand reasons which the evolutionists can urge against it. In any case, we commend it to the notice of our readers, but we cannot honestly recommend the empty book which Mr. Braithwaite has given us.

A MARVELLOUS CHART.*

TRULY a wonderful work that which gives in the space of an ordinary map—a circle of about one foot two inches across—every star which is to be found in Argelander's wondrous star-maps of the Northern Hemisphere. In other words, this map of Mr. Proctor's displays at once in the proportionate size and in their exact position, no less than 324,198 heavenly bodies. It is certainly a marvel of work, and is best studied by means of a good large reading lens. Indeed, without this it will look to many aged persons as simply a dirty circle, which has a filthier aspect in some parts than others. Besides this there is published with it a key map, in which the larger constellations of the Northern Hemisphere are distinctly represented, and the figures on this can be very readily distinguished in the photographic map—for it has been done by photography, as being more exact. Truly enough has Mr. Proctor said, that "to the general student of science the chart is chiefly of use in affording the means of enlarging his conceptions respecting the glories of the celestial depths. If he remembers that every one of the dots in the chart represents in reality a sun—a sun perhaps exceeding our own in magnitude and splendour—he cannot fail to be impressed with a sense of the grandeur of the stellar universe." This admirable map we, through some mistake, omitted noticing in our last number, and we beg to offer our ample apologies for the fact to both our readers and the author.

* "A Chart of the Northern Hemisphere, on an equal-surface projection; showing all the stars in Argelander's series of forty full-sheet charts—324,198 in all, with a key map on the same projection." By R. A. Proctor, B.A., F.R.A.S. Manchester: Photographed and Published by A. Brothers, 1871.

SHORT NOTICES.

Conversations on Natural Philosophy. By Mrs. Marcet: revised and edited by her son, Francis Marcet, F.R.S. 14th edition. London: Longmans, 1872. Those who like this book—and we believe it can number its admirers by thousands—will be pleased to find a new edition of it. It is, so far as we have examined it, tolerably accurate, and it is fuller than many text-books on the subject. The illustrations are both numerous and good, and the chapter on spectrum analysis is singularly excellent.

On the Antagonism between the Actions of Physostigma and Atropia. By Thomas R. Fraser, M.D., Lecturer on Therapeutics at Surgeons' Hall, Edinburgh. Edinburgh: Neil & Co., 1872. This is a work reprinted from the "Transactions of the Royal Society of Edinburgh." It contains nearly 200 pages, accompanied by numerous charts, and is certainly the most voluminous account which has ever been published upon the action of two drugs. It has an interest of the highest kind, not for the general reader, but for the medical man; and we hope there are few who are interested in the opposite action of drugs who will not carefully study Dr. Fraser's important results. The general conclusion at which the author has arrived from hundreds of experiments, though not exactly novel, is nevertheless of importance. He says (page 617) that "the conditions of the experiments, and the symptoms that were observed, render it certain that atropia prevents the fatal effect of a lethal dose of physostigma, by so influencing the functions of certain structures as to prevent such mortification being produced in them by physostigma as would result in death."

The Sun: Ruler, Fire, Light, and Life of the Planetary System. By R. A. Proctor, B.A. 2nd edition. London: Longmans, 1872. We need only say that this volume has gone to a second edition, and has sold more than 2,000 copies in the year. The present issue contains several new illustrations and some important additions.

Popular Physiology. By E. D. Mapother, M.D. London: Longmans, 1871. This book was not noticed, through some omission, in our recent number. It is a very small book of over 100 pages. Contains a good many illustrations. It is only intended for the most elementary possible of classes.

The Earth's Crust: a Handy Outline of Geology. By David Page, LL.D., F.G.S. Blackwood & Sons, 1872. This is the sixth edition of a good elementary outline of the science. It is just the sort of book for a class-book in school-rooms.

The Year Book of Facts in Science and Art. By John Timbs. Lockwood & Co., 1872. This contains just the usual amount of matter, with the usual degree of printer's blunders or other erroneous matter. We cannot at all approve of so badly edited a volume.

The Fairfield Orchids cultivated by Messrs. James Burke & Co. Bradbury & Evans, 1872. This is, of course, a publication got up for the purpose of selling the orchids of this firm. We believe that Mr. Leo Grindon had a share in its production.

The Stone-Age in New Jersey. By Charles C. Abbott, M.D. Salem, U.S., 1872. This is unquestionably the best paper and most elaborately illustrated which has appeared on flint weapons in any part of the world. It is admirably done, and we are glad it has been reprinted from the "American Naturalist," in which it originally appeared in last March or April.

Optical Illusions Explained. By A. B. Lacy, 1871. Is simply an explanation which exists in every treatise on natural philosophy in existence.

The Testimony of the Rocks, &c. By T. Callard. London: Elliot Stock, 1872. The whole matter is explained in about twenty-five pages of the smallest duodecimo. The author has been fortunate, we must say.

The Existence of Projectile Forces in Nature. By John Parker. New York: Wiley, 1872. The author simplifies the whole of astronomical questions, in twelve pages 8vo. ! !

The Hygiene of Air and Water. By W. Proctor, M.D., F.C.S. London: Hardwicke, 1872. This is a sensible little book on two important points in Hygiene. We recommend it to our readers' notice.

Human Progress in Medical Education. By W. Aitken, M.D., Professor of Pathology in the Army Medical School. London: Griffin & Co., 1872. This is a very capital address, given by the Professor of Pathology at Netley. It will repay attention. Dr. Aitken seldom speaks without saying something to the point.

SCIENTIFIC SUMMARY.

ASTRONOMY.

PROPOSAL for new National Observatories.—Col. Strange has called the attention of the Royal Astronomical Society to the insufficiency of existing national observatories, in respect of what may be called the physics of astronomy. He proposes that Government should be applied to for money to found other observatories than those at present in existence, and that in these new observatories the study of the physical features of the sun and moon, planets, comets, nebulæ, and stars, should be prosecuted systematically. At present the feeling in scientific circles is in favour rather of an extension of the work done at existing observatories, than of the foundation of new observing stations.

The Eclipse of December last.—In the last number of this journal will be found the chief particulars of the observations made upon the recent eclipse. It was hoped, however, that at the March meeting of the Astronomical Society full reports would have been made by the English observing expedition, and that many details of interest would then be announced. To the astonishment, and indeed to the disgust, of the crowded meeting which assembled on the strength of this expectation, no report whatever was then made! Nor was any report delivered at the April or May meetings! The silence of those whose duty it was to report the proceedings of the English expedition has naturally given rise to much comment, by no means favourable to the nominal chief of the expedition; the more so that the said head of the expedition has made free use of the information handed in to him by his fellow-observers, as well in lectures as in papers published at a price. It is felt that (unless there be some explanation as yet unpublished) it is altogether unworthy of a student of science to refrain from making due announcement of discoveries effected by the aid of Government money, and by the skill of fellow-workers who have unsuspectingly entrusted the records of their work into his hands, unless when the announcement can be so made as to be repaid with so many pounds, shillings, and pence. We trust for the honour of British science that some explanation may yet be forthcoming.

In marked contrast to the action of the reputed head of the English expedition, is that of Col. Tennant. So soon as he had reached England, he laid his statement before the Royal Astronomical Society, and submitted to examination the splendid photographs which were obtained at Dodabetta. It should be noticed in passing that the photographs, scarcely (if at all) less perfect, obtained by Mr. Davis, who at Lord Lindsay's charge accompanied the English expedition, escaped as by a miracle from the tight grip which laid hold of everything else obtained by that party,* and were thus available for comparison with those of Col. Tennant. Nothing could possibly be more convincing than the evidence given by either series separately, and by the two series when taken together. The details of the corona in these photographs are very numerous, and wonderfully distinct; *all of them can be recognised in all the photographs.* We cannot wonder that even those who had been most opposed to the solar theory of the corona have at last given in their adhesion to it. We do not say that all of them have, for that would be expecting too much, human nature being as it is. But that any of the opponents of that theory should have adopted it, speaks volumes. Now Dr. De la Rue, who, it will be remembered, had urged arguments in favour of Oudemann's theory, which may be called the lunar theory of the corona, admitted, with a candour and frankness which did him infinite honour, that the photographs obtained last December demonstrate the solar nature of the phenomenon.

Densities of Jupiter's Satellites.—Mr. Proctor has called attention to the incorrect values of Jupiter's satellites which have found their way into our text-books of astronomy. These values have led to erroneous assumptions as to the condition of these bodies. The following table shows at once the incorrect values formerly adopted, and those which Mr. Proctor has calculated:

Satellite	Mass that of Jupiter =1	Density that of Earth=1		Density that of Water=1	
		Text-book value	True value	Text-book value	True value
I.	0·0000173	0·0202	0·198	0·114	1·148
II.	0·0000232	0·0302	0·374	0·171	2·167
III.	0·0000885	0·0698	0.325	0·396	1·883
IV.	0·0000427	0·0393	0·253	0·223	1·468

It will be seen that the errors are enormous, nor is there any recognisable explanation. For the density of the first satellite is given in our text-books at about a tenth of its true value, but that of the third at rather more than a fifth. One obtains something very like the text-book values, however, by supposing all the satellites equal in size to the first, and by reducing the thence estimated density to one-tenth of its value. Assuredly, supposing this to be the true explanation—or, indeed, whatever may be the true explanation—the error is a most monstrous one, to be repeated, as it has been, from one hand-book of astronomy to another. It may fairly be asked what confidence can be placed in such hand-books when blunders like these are suffered by their authors to escape detection.

Orbit of the Double-star Castor.—Mr. Wilson, M.A., of Rugby, enunciates the somewhat startling theory that the orbit of Castor is hyperbolic, "a form

* Probably because, being obtained at Lord Lindsay's expense, there was a private claim upon them. The public claim upon the observations of the members of the expedition could be ignored, it would seem; but, fortunately for science, Lord Lindsay successfully urged his claim upon his own property.

of orbit," as he truly remarks, "which has not been shown to exist in the case of any binary system." It may be remarked that Sir John Herschel, nearly forty years ago, deduced the period 253 years, while Smyth obtained a period of 240 years. In the Monthly Notices for December 1845, Mr. Hind gives elements "entirely differing from those previously computed by Herschel and Mädler," the difference being "materially owing to the great influence exerted by recent measures at Mr. Bishop's observatory by Mr. Dawes." Mr. Wilson's result involves a somewhat startling advance in the same direction. He finds that the apparent orbit is part of an hyperbola of eccentricity 2·2. "The real hyperbola may be shown by a graphical construction to have an eccentricity of 3·16, and its line of nodes nearly coincides with the axis major." "If this orbit is correct, the angle of position will decrease to the limit 188°. It is now about 238°·12, according to the observations of Mr. Seabroke and myself," he adds, "and a little less by the interpolating curve—about 237°·85."

An Unsuspected Cause of Diffraction Phenomena in Telescopes.—Captain Noble makes the following remarks in the Monthly Notices for April:—"Some little time ago, in observing Jupiter and his satellites, I remarked certain emanations which appeared to have their origin in diffraction. I was very much puzzled to imagine in what way these phenomena could arise. My 4·2-inch Ross object-glass is simply perfect; my eye-pieces were carefully cleaned, and, so far as I could see by removing them, and looking up the tube at the Moon or Jupiter, the tube itself was free from any obstruction. Since, however, the general definition of the instrument was sensibly unimpaired, I took no further action in the matter, and let things take their course until about the middle of last month. I was observing the sun one morning at that period, when I removed the eye-piece for some reason, and happened to glance obliquely up the tube. To my astonishment I saw, brilliantly illuminated by the sun, a perfect *grating* of excessively fine spider-webs, spun vertically across the interior of the telescope, somewhat within the focus of the object-glass. A light, in more senses than one, suddenly broke in upon me, and I very speedily removed the offending lines. I had the pleasure, that same evening, of viewing the Jovian system shorn of all optical appendages. I am too ignorant of the Arachnida to be able to guess at the species which produces a web of such extraordinary tenuity; but it certainly must be an extremely minute one, not only on account of the excessive fineness of the filaments which it spins, but also in order that it should have found its way inside of a tube so thoroughly and carefully closed as (I should think) to prevent the existence of interstice or aperture whatever whereby an entry might be effected."

Spectrum of the Zodiacal Light.—In our last Summary we mentioned that Liais had found the spectrum of the zodiacal light to be a faint continuous one. Before that statement had appeared in these pages, news was received from the skilful Italian spectroscopist Respighi, to the effect that the spectrum of the zodiacal light consists in the main of the bright line forming the chief constituent of the spectrum of ordinary green aurora. "Formerly," he says, "I made spectroscopic observations on the zodiacal light, in the East Indies, but I could not see Angström's line, and I had obtained no result because I did not take the necessary precautions, protecting the eye from external light whose brightness was sufficient to veil the line and the bright neighbouring zone." But his observations on the night following the great auroral display of February 4, serve, so far as they go, to prove the very reverse of what Respighi infers. He says: "Thinking that the aurora-borealis would re-appear on the next evening after the disappearance of twilight, I set myself to observe the sky, and I found it illuminated in all parts by a feeble light which produced the effect of a general phosphorescence. While waiting for marked phenomena, I directed the spectroscope provisionally towards the zodiacal light, then tolerably bright, and soon I could distinguish the green light and the neighbouring zone of light apparently continuous, and which embraced the space occupied by the lines of the auroral spectrum. Next turning the spectroscope on the faint light which illuminated the heavens, first towards the magnetic meridian, and then towards all azimuths and at all heights, I was surprised to find still the same spectrum, more or less marked, but everywhere as distinct as in the zodiacal light. Moreover Dr. D. Legge, one of the assistants at the observatory, distinctly saw this spectrum in all parts of the heavens. These observations were made towards seven or eight o'clock. Later, towards ten, I could not detect this spectrum in any part of the heavens. This fact, which confirms a similar observation made by Angström in March 1867, seems to me somewhat important, for it would tend to show the identity of the light of the aurora-borealis and the zodiacal light, and hence the probable identity of their origin." It seems to us, on the contrary, that Respighi's observation tends to show that the appearance of the auroral line when the spectroscope was turned upon the zodiacal light was due to auroral light in that direction, and not to the zodiacal light at all. It certainly was a suspicious circumstance that a certain bright line could be seen as distinctly in all parts of the sky as towards the zodiacal light; and one cannot understand why, under the circumstances, Respighi should judge this line to belong to the zodiacal light, since unquestionably any auroral phosphorescence must have extended over the zodiacal region as well as over the rest of the heavens.

But all doubts on the subject seem to be finally removed by Professor Piozzi Smyth's recent observations in Sicily. In the first place, it should be noticed that Prof. Smyth attended much more carefully to the structure of his instrument than any of his predecessors in zodiacal observation. He was also particularly careful to exclude extraneous light. He had an instrument so contrived that he could examine simultaneously the spectrum of any auroral light and the faint linear spectrum of an alcohol flame. Now with a narrow slit he obtained from the zodiacal light no spectrum at all, though the very same instrument had, with the same slit-opening, shown the chief auroral line even from very faint aurora. As the slit was widened no trace of a linear or band spectrum could be recognised, but with sufficient opening a faint continuous spectrum, exactly like that obtained from faint twilight, or indeed from the ordinary night-sky when there is no aurora. Here we have absolutely perfect proof of the fact that the zodiacal light gives the same spectrum as faint reflected sunlight. But Prof. Smyth's observation proves more than this. It shows that, under the very same circumstances, the zodiacal light gives no spectrum where the faintest auroral light gives the characteristic green line spectrum; or, in other words, that spectroscopically dealt with, the slit being fine, the zodiacal light gives a background on which the faint auroral line (if aurora is present) may show itself as perfectly as though there were no zodiacal light at all. Hence Prof. Smyth's observation not only demonstrate the real nature of the zodiacal spectrum, but shows how the mistake of those arose who have supposed the zodiacal spectrum to be identical with that of the aurora.

Proper Motions of the Stars.—Dr. Huggins has been able to continue his researches into the proper motions of the stars in the direction of the line of sight. It will be remembered that the telescope he formerly used did not possess sufficient light-gathering power to deal with any star except Sirius. The instrument Dr. Huggins is now using has a light-gathering power four times as great. The first fruits of its employment in this line of research confirm in a very interesting manner the anticipations as well as the theories of Mr. Proctor. Dr. Huggins finds that certain stars are moving as if in systems or families, since they possess a common motion either of recess or approach. Among such instances may be mentioned one of a very remarkable kind. It may be remembered that Mr. Proctor, nearly three years ago, announced that the five stars β, γ, δ, ϵ, and ζ Ursæ Majoris, as well as Alcor close by ζ, and the telescopic companion of ζ, are moving in a common direction; and at a lecture delivered in May, 1870, at the Royal Institution, Mr. Proctor expressed his conviction that whenever Dr. Huggins applied the spectroscopic method to these stars, he would find that they are either all receding or all approaching. Many, unaware of the evidence on which this conviction was based, considered so definite a prediction altogether unwise. It has, however, been amply confirmed by the event, since Dr. Huggins finds these five stars to be all receding at the rate of about thirty miles per second. On the other hand the star ζ, which Mr. Proctor had indicated as *not* belonging to the set, is found to have a spectrum differing in character from that common to the five stars, and though receding, has a different rate. The star α, also as marked by Mr. Proctor distinct from the rest, is found to have a totally different spectrum, and to be approaching. Thus the prediction referred to has been more than fulfilled; it has been found not merely that all the stars of the set are receding at the same rate, but that other stars excluded from the set are not moving in the same way, and are furthermore distinguished by spectral differences from the members of the drifting-star family.

Planets for the Quarter.—Saturn is the planet best placed for observation during the approaching quarter. He comes to apposition on July 9, at 11 h. 13 m. Jupiter will be in conjunction with the Sun on August 3, 4 h. 5 m. p.m., and is unfavourably situated for observation throughout the quarter. None of the other planets (except Mercury) will be well placed during the quarter. Mercury will be at his greatest eastern elongation on August 3rd, and at his greatest westerly elongation September 15.

August Meteors.—We remind our readers to look out for the famous August meteors on the nights of August 9, 10, and 11.

BOTANY AND VEGETABLE PHYSIOLOGY.

Dispersion of Seeds by the Wind.—A good paper on this subject has been contributed by M. A. Kerner, Director of the Botanic Garden of Innsbruck, to the "Zeit. des Deutsch. Alpen-vereins." He made a thorough inquiry into the flora of the glacier moraines, and the seeds found on the surface of the glaciers themselves, believing that these must indicate accurately the species whose seeds are dispersed by the agency of the wind. Of the former description he was able to identify, on five different moraines, 124 species of plants; and a careful examination of the substances gathered from the surface of the glacier showed seeds belonging to thirty-six species which could be recognised with certainty. The two lists agreed entirely in general character, and to a considerable extent, also specifically; belonging, with scarcely an exception, to plants found on the declivities and in the mountain valleys in the immediate vicinity of the glacier; scarcely in a single instance even to inhabitants of the more southern Alps. M. Kerner's conclusion is, that the distance to which seeds can be carried by the wind, even when provided with special apparatus for floating in the air, has generally been greatly over-estimated; and this is very much in accordance with the view advanced by Mr. Bentham in his anniversary address to the Linnæan Society of London in 1869. Along with the seeds M. Kerner found, on the surface of the glacier, more or less perfect remains of a number of insects belonging to the orders *Lepidoptera*, *Hymenoptera*, *Diptera* and *Coleoptera*, which, like the seeds, belonged almost exclusively to species abounding in the immediate neighbourhood of the glaciers.

The Fertilization of Coniferæ has been very carefully studied lately by Signor Delpino, who is now Professor in the State Forest School at Vallombrosa. He has been paying much attention to dichogamous flowers, and to the difference between those fertilised by the wind (anemophilous), or by insects (entomophilous), or by animals of whatever sort—zodiophilous, as he terms them. *Coniferæ*, as is well known, are anemophilous, that is, their fecundation is entrusted to the wind; their light and most abundant pollen is correlated to this, and the structure of the fertile inflorescence is such that the pollen reaches the very orifice of the ovule. In yew and cypress, and in other, if not all other genera of the sub-orders they represent, Delpino finds that, at the time when the ovule is ready for fecundation, a minute clear drop of liquid appears at the orifice of the ovule; grains of pollen falling upon this are retained, are incited by it to develop the pollen-tube into the liquid first, thence into the ovule, and the drop is then re-absorbed or dries up. Alph. de Candolle, in a recent number of the "Arch. des Sciences de la Bibl. universelle," calls attention to the fact that this droplet was known, as to its appearance, function, and re-absorption, to his late venerable townsman, Vaucher, and is described in his "Physiology of the Plants of Europe," published in 1841.

Action of Foreign Pollen on the Fruit of the Fertilised Plant.—"Silliman's American Journal," May, states that Maximowicz has collected in a Russian journal the observations and experiments on this subject, and recorded some observations of his own. He mutually crossed *Lillium davuricum* and *L. bulbiferum*. Now, these have been taken for one and the same by late writers, but are really characterised, according to Maximowicz, by the form of their capsules and bulb-scales. In the single experiment the pistil of *L. bulbiferum* fertilised by the pollen of *L. davuricum*, set fruit, but failed to mature it. That of *L. davuricum*, fertilised by the pollen of *L. bulbiferum*, matured well; but, to the surprise of the observer, it formed the long capsule of *L. bulbiferum*, instead of the short one of the species. This is an important experiment, but it requires repetition.

The Phænogamous Plants of the United States east of the Mississippi, and the Vascular Cryptogamous Plants of North America north of Mexico.—This is the second edition, revised and corrected, and published now by Mr. B. Pickman Mann. It is, however, merely a reprint, with corrections and a few additions, of the catalogue known familiarly as "Mann's Exchange List," which has been of great service to all collectors of American plants. Since the running numbers have not been changed, it will be possible to use the old and new editions interchangeably. The "typographical and other errors," which have been found and corrected, are over 150, and Mr. Mann, in his preface, renews his brother's request that persons using the catalogue would send him notice of all errors discovered.

A Fungus-like Growth on the Leaves of Coleus Plants has been thoroughly investigated by Mr. H. J. Slack, F.G.S., who has read a paper upon the subject before the Royal Microscopical Society. In the first place, he says, a number of leaves were taken from coleus plants of various colours, and carefully examined in their natural state, both by transmitted and reflected light. It became apparent that every leaf, whatever its age or tint, exhibited chiefly, if not entirely on the under surfrce, a number of globular bodies of a beautiful yellow colour, highly translucent and refractive, most of them marked with a cross like that impressed upon the well-known cross-bun. These bodies differed in hue from any yellow of the leaf, and they were distributed pretty uniformly without any regard to the variegations of the leaf-colouring matter. From damaged specimens, it was obvious that they were the bodies alluded to by Mr. Howse, who in a recent paper imagined them of fungous origin. The colour of these bodies, when looking healthy, and well filled with their refractive matter, varied from rich topaz to a pale sherry tint, and they glittered like jewels when well lit up. Empty cells had a rude resemblance to a mushroom in form, with a stout stem and a round head marked with the cross, but the texture did not look in the least fungoid, nor could any mycelium be discerned in or on the leaves.—*Vide* "Monthly Microscopical Journal," May.

Death of M. de Brebisson.—Those who are familiar with the large amount of work done by this gentleman will regret his death, which took place on April 26, in his seventy-fourth year.

Structure of the Diatomaceous Frustule.—Professor H. L. Smith gives his views on this subject in the April number of the "Lens." He believes that all the frustules are "siliceous boxes," with either one portion (the cover) slipping over the other, as in *Pinnularia*, or with edges simply opposed, as in *Fragilaria*. If we take a frustule of *Melosira*, it may be compared to a pill-box—one portion slipping on to the other. The great majority of diatoms are thus constituted. It is perfectly evident that, in the case of the formation of a new valve, in the processes of self-division, this new part, which slips out from the older, must be somewhat smaller. Inside of the box is a membrane, inclosing the internal coloured or colourless substance, imbedded in which may always be seen, at least in the larger forms, a distinct nucleus, sometimes two, and sometimes a "germinal dot," with numerous fine threads radiating from the nucleus, or the germinal dot. As the frustule increases in width, one portion slips out from the other, and sometimes successive additions of siliceous matter are made to the edges of the box, somewhat analogous to the successive additions to the edge of the shell of a mollusc. When the widening of the frustule has reached a certain extent, the lining membrane, at the places which would be exposed if the two portions were wholly to slip apart, infolds. He has reason to believe that, previous to this, a double membrane of extreme tenuity has been formed, commencing its growth *at* the nucleus (which itself is divided), and extended to the margins of the cell, which is thus divided into two nearly equal parts; for, as soon as this infolding commences (perhaps now accelerated by the admission of water), the line of division can be seen progressing steadily inwards by the parting of this thin double membrane, so that in fifteen or twenty minutes the fissure is complete. He has, in very large *Pinnulariæ*, witnessed the whole phenomenon, from its inception up to the final self-division. While the *fissure* occurs in the short period of time he has named, to complete the *self-division* requires about six days.

Is there Alternation of Generations in Fungi.—Mr. M. C. Cook, M.A., believes that it is questionable whether this phenomena occurs in fungi, as Professor Œrsted alleges. He thinks it takes place in the same plant, as in the case of *Bunt*; but he feels great difficulty in believing in this process, where the generations were passed in different plants, until confirmed by other observers. If the spores of *Æcidium Berberidis* were taken from the barberry and sown upon young wheat plants, and all these plants became infected with corn mildew (*Puccinia graminis*), to which wheat is but too prone, it certainly seemed premature to say that the spores of the *Æcidium* caused the *Puccinia* to be developed as a second generation; whereas it is much more probable that the germs of the mildew already lay dormant in the wheat, and, at most, the sowing and growing of the *Æcidium* spores only stimulated the mildew to a more rapid development.

Altering the Name of a Bog-moss.—Dr. Braithwaite is sufficiently conscientious in giving a new name to a species. In his last paper in the "Monthly Misroscopical Journal," June, he gives the following observations on this subject. In Phænogamic Botany, Entomology, and other departments of natural history, the adoption of the first name by which a species has been described (dating from the establishment of the binomial nomenclature by Linnæus) is considered imperative; yet the synonymy of mosses is wofully confused, for Hedwig and others gave a new specific name as often as they changed the genus—a rule not sanctioned by the best authorities. Others may object with greater reason that the brief discriptions of the older authors are not sufficient to identify the species with certainty, yet it must be remembered that the actual specimens of very many of them are in existence, and their examination by a competent authority in most cases settles the question. Prof. Lindberg, who has worked so indefatigably at this unattractive department of botanical literature, has shown, in his "Rev.

Crit. Ic. Musc. Fl. Dan.," that this species is in the St. Petersburg herbarium named *tenellum* in Ehrhart's own handwriting; this, however, without description, might not be allowed to stand, but the same species received the same name from Persoon, as proved by a specimen from him. preserved in Swartz's herbarium; and a description is given by Bridel in his "Mantissa Musc." (1819), the leaves indeed being described as recurved at the point, which might perhaps refer to them in a dry state. Bridel also admits *S. molluscum* into the Bryol. Univ., but he only copied the description of Bruch (1825), without having seen a specimen. Dr. Braithwaite, therefore, has no hesitation in adopting the name first given to the species.

The Breathing Pores of Leaves.—A good popular paper on this subject is that which Prof. T. D. Biscoe read before the Troy Scientific Association, and published in the "American Naturalist," March, 1872. If, he says, the outer layer or skin be stripped from the surface of the green-coloured parts of plants, and examined under a low power of the microscope, the stomata, or breathing pores, will appear as green specks in the otherwise colourless membrane. Their object is to open and close communication between the intercellular space always existing between the individual cells and the outer atmosphere. The sausage-shaped cells constituting the essential part of the organ are called the pore cells. They have the power of separating from each other in the middle, thus opening a free way for the air to the interior tissues; or in certain conditions of light and moisture they approach each other so as to narrow or entirely close the slit between them. They are filled with protoplasm, chorophyl and starch granules, while all other cells of the outer surface are filled only with air and water. Apparently with the object of placing these pore cells as free as possible from all constraint or pressure, so that they may correspond sensitively to all the changes in the atmosphere, they are at times situated on a level with the epidermis cells, sometimes raised above, at others sunk beneath this level. If the epidermis cell-walls are thin and flexible, the stomata will generally be found in the same surface with them; but when the epidermis walls are thick and stiff, the stomata will generally be found sunk deep under the surface, or raised above it, or surrounded by a ring of smaller cells with thinner walls than the remaining epidermis cells. Immediately under the stomata are empty spaces, of irregular form and varying size, called breathing-rooms. They are in connection with, and form a part of the intercellular space which ramifies through the entire structure of most tissues. It is an interesting question in what way the stomata have been formed. Were the pore cells at first a pair of ordinary cells, which have gradually changed their form and contents until endowed with all the peculiar properties of their natural state? Or were they always existent in their peculiarities, only smaller as the leaf was younger? Or, have they grown out of a single cell by the process of subdivision and after-growth? Do they belong to the epidermis, or to the chlorophyl-bearing tissues beneath?

CHEMISTRY.

The Chemical Society's Faraday Lecture.—On Thursday, May 30, Professor Cannizzaro, of Palermo, delivered the "Faraday Lecture" before a large audience, including a number of ladies. The Lecture Theatre of the Royal Institution had been kindly lent to the Chemical Society for this purpose, and the learned Professor's discourse was entitled "Considérations sur quelques Points de l'Enseignement Théorique de la Chemie." On the following Friday a dinner was given to the Professor, at which about 150 were present, including the Italian ambassador and the Right Hon. the Chancellor of the Exchequer. We think it is a pity that some arrangement is not made by which the lecturer could address his audience in English, for we are certain that very few of them apprehend French sufficiently well to take in even the substance of the lecture.

Chemical Analysis of the Meteoric Rain in Sicily of March 9, 10, *and* 11. —M. O. Silvestri has an important memoir on this subject in the "Comptes Rendus," April 9, 1872. This memoir contains the results of the researches made on rain-water along with which fell a kind of sand; the water, having been filtered, was found to be colourless and free from smell, but exhibited a saline taste; it was neutral to test-paper; hardness, 17·5 degrees (ordinary rain-water, 1 degree). By long-continued boiling, it gave off $19\frac{1}{2}$ c.c. of gas, consisting of: nitrogen, 83·959 per cent.; oxygen, 13·070; CO_2, 2·971. On being evaporated to dryness, this water was found to contain, in 1,000 parts: bicarbonates of lime, 0·129; of magnesia, 0·035; of iron, traces; sulphate of lime, 0·041; chloride of potassium, traces; sulphate of soda, 0·009; chloride of sodium, trace; organic matter, 0·063. The sand, very finely pulverised and dust-like, has a sp. gr. −2·5258, and contains, in 100 parts: clay, 75·08; carbonate of lime, 11·65; organic matter, 13·10.

Dr. Hoffmann on Phosphuretted Hydrogen.—On Dr. Hoffmann's recent visit to this country, he went to the Chemical Society as a matter of course. On April 18 he there experimentally exhibited the formation of phosphuretted hydrogen by the action of water on iodide of phosphonium, and the decomposition of iodide of ethyl-phosphonium by water, liberating the ethyl-phosphine, E_2HN, which has the characteristic odour of the phosphorus bases. He also explained Baeyer's method of preparing iodide of phosphonium on the large scale by the action of water on iodide of phosphorus, stating that it was necessary to employ a large excess of phosphorus, three or four times the theoretical, since much of it is converted into the amorphous state, and thereby rendered inactive.

Chloride of Sodium in Liebig's Extract of Meat.—In the "Annalen der Chimie" for May, Baron Justus von Liebig has written a paper with the view to refute the allegation made by a Dr. Godefroy, who appears to have published, in an Austrian scientific paper, a statement to the effect that Liebig's extract of meat should contain 2 per cent. of chloride of sodium, purposely added as a fraud. The author refers Dr. Godefroy to his (Liebig's) essays, published in the "Annalen" twenty years ago, "On the Constituents of the Fluids contained in Meat," and emphatically denies that at Fray Bentos, where the extract of meat is made, any common salt is added to it. Chloride of potassium is largely contained in the extract.

Determining Carbonic Acid in Sea-water.—Professor Himly, at the meeting of the Chemical Society, April 18, after pointing out the difficulties which beset the determination of carbonic acid in sea-water—Jacobsen having shown that the whole of the gas present is not given off by boiling, either *in vacuo* or under the ordinary pressure at 100°, even when a current of air is passed through the liquid—said, however, that the whole of the carbonic acid could be readily estimated by adding baryta water or barium nitrate, ammonium nitrate, and ammonia, to a measured quantity of the sea-water, thus obtaining the whole of the carbonic acid in the precipitate. After the supernatant liquid had been removed, the carbonic acid might be estimated in the precipitate. In order to collect sea-water for the determination of the carbonic acid at great depths, and consequently under great pressures, it was necessary to sink a cylinder open at both ends to the place where the water was to be collected, and then to securely close it there. The apparatus for that purpose, closed by valves, had been found to be very defective; but he had employed one which answered admirably, consisting of a cylinder furnished with a large stop-cock at each end. When this cylinder had been sunk to the required depth, the stop-cocks were closed by powerful springs released at the proper moment by means of an electro-magnet set in action in the usual way. (See also "Chemical News.")

A New Organic Pigment has been obtained from a spot above the eyes of the moor-cock. It is called *Tetronerythrin*, and has been described by Dr. Wurm in "Poggendorff's Annalen." It seems ["Chemical News"] that a statement was made in the "Wiener Jagdzeitung" to the effect that the red warty spot met with above the eyes of the mountain-cock and moor-cock (*Tetrao tetrix*), when rubbed with a white handkerchief, imparted thereto a beautiful red colour. The author was inclined to disbelieve this, and accordingly made some microscopical and microchemical researches on this subject, the result being that he discovered a pigment which he terms *Tetronerythrin* (from *Tetraon* and *erythros*, mountain-cock red). A very small quantity of this pigment, which is soluble in chloroform, was sent by the author to Dr. J. von Liebig, who states that it is a peculiar substance which has nothing in common with the colouring matter of the blood; it is soluble in ether and sulphide of carbon, not soluble in cold caustic alkaline solutions, and soluble in hot nitric acid, but decomposed simultaneously, leaving a waxy residue.

How to Know Fruit-wine from Grape-wine.—According to Dr. F. Vorwerk ["Neues Jahrbuch für Pharmacie"], the phosphoric acid present in genuine grape-wine is combined with magnesia, while in fruit-wines it is present in combination with lime. The simple addition, therefore, of ammonia (1 part to 9 parts of wine), will produce in genuine wine, after twelve hours' standing, the well-known precipitate of ammonio-phosphate of magnesia.

Dr. Letheby and Dr. Frankland on Water-analysis.—The editor of the "Chemical News" has quite recently given a leading article on this important subject. Referring to the controversy which has come up upon the matter, he states that if it had arisen in France there would have been a commission of the Academy to report on it. In this country we manage things differently, and resolve questions of this description after our own fashion, and we have to collect the testimony of competent scientific men who live and work in isolation. Mr. Way, who was formerly on the Rivers' Commission, has published his judgment on this question in a report on the analysis of a sample of water. It is to the effect that he uses and has confidence in the rival process of Wanklyn and his colleagues, which, it is admitted, gives results in opposition to Dr. Frankland's. Dr. Angus Smith, who, as our readers possibly know, has been for some time engaged in a very important investigation of the organic matter existing in the atmosphere, has also given in his adhesion to the other process, which he employs in his researches. The late Dr. W. A. Miller rejected Dr. Frankland's process and employed Mr. Wanklyn's in his later investigations, undertaken for the medical department of the Privy Council. Dr. Voelcker, chemist to the Royal Agricultural Society, rejects Dr. Frankland's process and adopts the other. Dr. Letheby has often expressed a like opinion. Indeed, says the writer in conclusion, we scarcely know a single chemist of reputation who approves of Dr. Frankland's water-analysis ! !

The asserted Alkalinity of Carbonate of Lime.—Mr. William Skey, Government Analyst, New Zealand, re-asserts the alkalinity of the above. He says, in a paper of his which appeared in the second volume of the "Transactions of the Wellington Philosophical Society, that he asserted the alkalinity of carbonate of lime, but the correctness of this assertion having been disputed by Mr. Charles R. C. Tichborne, F.C.S., M.R.I.A., &c., of the Laboratory of the Apothecaries' Hall, Ireland, in a communication to the editor of the "Chemical News" (vol. xxii. p. 150), he has re-investigated this subject and extended his researches upon it, by which he has arrived at results corroborative of the correctness of his statement, and which show, besides, that a large number of salts hitherto maintained to be neutral, or respecting which nothing has been affirmed, are in reality alkaline. This is important, for it may be regarded as conclusive so far as Mr. Skey's researches are concerned.—*Vide* "Chemical News," March 28.

The new Hydrocarbon: Abietene.—Mr. W. Wenzell, who writes in the "American Journal of Pharmacy," March, 1872, says that this hydrocarbon is the product of distillation of the terebinthinate exudation of a coniferous tree indigenous to California, viz., the *Pinus sabiniana*, a tree met with in the dry sides of the foot-hills of the Sierra Nevada mountains, and locally known as the nut-pine or digger pine, owing to the edible quality of its fruit. A gum resin, or rather balsam, is obtained from this tree by incisions made in its wood, and the balsam submitted to distillation almost immediately after having been collected, owing to the great volatility of the hydrocarbon (or essential oil, because abietene really stands in the same relation to the balsam alluded to as oil of turpentine stands to the exudation derived from other *Pinus* species). The crude oil, as usually met with for sale at San Francisco, is a colourless limpid fluid, requiring only to be redistilled to obtain it quite pure. The commercial article is used under different names—abietine, erasine, theoline, &c.—for the removal of grease and paint from clothing and woven fabrics, and likewise as an efficient substitute for petroleum-benzine. The ultimate composition of abietene is not stated, but the author points out at some length the differences existing between abietene and terebene (oil of turpentine).

Influence of Pressure in producing Chemical Change.—An important paper on this subject has been read before the Chemical Society, May, 16, by Mr. H. T. Brown. In his investigation the author found that during the alcoholic fermentation of grape juice or malt wort, besides carbonic anhydride, that nitrogen, hydrogen, a hydrocarbon of the paraffin group, and sometimes nitric oxide, are evolved; moreover, the proportion of the gases unabsorbed by potassium hydrate is largely increased when the operation is carried on under diminished pressure. At the ordinary pressure by far the larger proportion of these gases is nitrogen (70 to 90 per cent), but under diminished pressure, 400 to 459 m.m., the hydrogen preponderates (60 to 90 per cent). Nitrogen, however, does not occur when the solutions contain no albumenoids, even if ammonium salts are present in considerable quantity. The increase of the proportion of hydrogen, resulting from diminution of the pressure, is accompanied by formation of a comparatively large amount of acetic acid and aldehyde, so that it would seem that water is decomposed during the alcoholic fermentation, and that this result is facilitated by the diminution of the pressure. The presence of nitric oxide in the evolved gases was found to be due to the reduction of nitrates originally present in the solutions.

Fearful Adulteration of Whisky in Ireland.—At a recent meeting of the Chemico-Agricultural Society at Belfast, under the presidency of Dr. Knox, late Poor Law Inspector, the subject of whisky adulteration was brought under consideration by Dr. Hodges, Professor in Queen's College, Belfast, who exhibited a specimen of that liquid brought to him by two men who had been physically incapacitated by drinking a small portion of it in a public-house. He found, on analysis, that it contained a large amount of naphtha. He had also discovered that ingredients of even a more deleterious character were used in the process of adulteration—mixtures containing sulphate of copper (blue stone), cayenne pepper, sulphuric acid (vitriol), and a little spirits of wine. One specimen submitted to Dr. Hodges by a number of provision-cutters and curers, was composed of naphtha and a slight colouring of whisky. The men who had imbibed a small quantity of it were affected with serious symptoms; and this, said Dr. Hodges, was a fair specimen of the drink sold in low-class public-houses. The trade in this noxious compound is carried on with impunity, no local authority in Belfast or in the Province of Ulster caring to interfere with it.

A Substitute for Soda in washing Linen.—This, which appears to be extensively used abroad, will, we doubt not, prove of great service if introduced into the English laundry. It is described by Dr. Quesneville, in the *Moniteur scientifique Quesneville* (March). The very common use, says Dr. Quesneville, especially in England, of soda for washing linen is very injurious to the tissue, and moreover has the effect of yellowing it in the long run. The author states that in Germany and Belgium the following mixture is now extensively and beneficially used:—2 lbs. of soap are dissolved in 25 litres (5·5 gallons) of water, as hot as the hand can bear it; there are next added to this fluid three large-sized tablespoonfuls of liquid ammonia and one spoonful of best oil of turpentine. These fluids are incorporated rapidly by means of beating the soap-suds and other fluids with a small birch-broom. The linen, &c., is then put into this liquid and soaked for three hours, care being taken to cover the washing-tub with a closely-fitting wooden lid; by this means the linen is readily cleansed, requires hardly any rubbing, and less brushing, and there is a saving also of time and fuel. Ammonia does not affect the linen nor woollen goods, and is largely used as washing-liquor in the North of England, of course along with much water, as above indicated.

GEOLOGY AND PALÆONTOLOGY.

Fossil Turtles.—At the meeting of the American Philosophical Society, March 1, 1872, Professor Cope read a paper "On *Protostega*," a genus of extinct Testudinata. A detailed account of the osteology of *P. gigas* from the cretaceous beds was given, by which it appeared that the genus had separate ribs, as in *Sphargis*, and that the carapace was formed by large radiating plates of bone in the skin. Two other species were described—*P. tuberosus* and *P. neptunus*. The latter, the largest known marine turtle, from New Jersey; the former, from the cretaceous of Mississippi, had been referred by Leidy to the Mosasauroids.

Mr. Waterhouse Hawkins' efforts in New York have, we regret to learn, been completely overthrown by an ignorant manager of the Central Park Museum. In answer to an inquiry made of him, Mr. Hawkins says that all he had done during twenty-one months to restore the skeletons of the extinct animals of America (of the Hadrosaurus, and the other gigantic animal, which was thirty-nine feet long), was destroyed by order of Mr. Henry Hilton, on May 3, with sledge-hammer, and carted away to Mount St. Vincent, where the remains were buried several feet below the surface. The preparatory sketches of other animals, including a mammoth and a mammoth and a mastodon, and the moulds and sketch-models, were destroyed. Mr. Hilton did this, said Mr. Hawkins, out of ignorance, just as he had a coat of white paint put on the skeleton of a whale which Mr. Peter Cooper had presented to the Museum, and just as he had a bronze statue painted white. Mr. Hilton told the celebrated naturalist who had come from England to undertake the work that he should not bother himself with "dead animals;" that there was plenty to do among the living. This illustrates the policy of having such men as Hilton at the head of one of the most important departments of the City government. When the skeletons were dug up again, by order of Colonel Stebbins, they were found broken in thousands of pieces. Prof. Henry, of the Smithsonian Institution, when he heard of this piece of barbarism, would not believe it. "Why," he exclaimed, "I would have paid them a good price for it." Mr. Hilton, however, preferred to destroy the work of the naturalist, which had cost the city at least 12,000 dollars.

Flint Arrow-heads.—We beg to recommend our readers to an excellent article on these weapons, which appears in the "American Naturalist" for April. It is certainly the ablest paper on this subject that we have seen, and it is abundantly illustrated.

New Species of Cretaceous Birds.—A very able paper on this subject appears in "Silliman's American Journal" for May, by Mr. O. C. Marsh. The few remains of birds hitherto described from the cretaceous deposits of that country, although of much interest, all pertained to comparatively small species, and belonged, apparently, to families still existing. It is fortunate, therefore, that the existence of a fossil bird, so large and remarkable as the one (*Hesperornis regalis*) that forms the subject of the present description, should first be made known by the discovery of such important parts of a skeleton as to afford ample material for the determination of its affinities. This interesting discovery has already been announced in "Silliman's Journal," and the name, *Hesperornis regalis*, proposed by the writer for the species thus represented. The present paper is preliminary to a full description, with illustrations, now in course of preparation. The other species briefly described in this article are likewise of interest, as they add some new forms to the limited avian fauna heretofore found in the cretaceous beds of the Atlantic coast.

Changes of Climate during the Glacial Period.—We observe that Mr. Geikie has communicated his seventh and last paper on this subject to the "Geological Magazine" for June. The series are of very great importance, though of course it is utterly impossible for us to give an abstract of the several papers it includes. However, for our readers' convenience of reference, we give the following, which is the order of appearance of the earlier portions of Mr. Geikie's paper "On Changes of Climate during the Glacial Epoch."

First Paper:	"Geol. Mag."	vol.	viii.	Dec.	1871,	p. 545.
Second ,,	,,	,,	ix.	Jan.	1872,	p. 23.
Third ,,	,,	,,	,,	Feb.	,,	p. 61.
Fourth ,,	,,	,,	,,	March	,,	p. 105.
Fifth ,,	,,	,,	,,	April	,,	p. 164.
Sixth ,,	,,	,,	,,	May	,,	p. 215.
Seventh ,,	,,	,,	,,	June	,,	p. 254.

The last Eruption of Vesuvius.—We merely record the fact here that there has been a severe eruption since our last issue. Mr. G. Poulett Scrope has given an account of the eruption in the "Geological Magazine" for June. However, until Professor Palmieri gives us his version of the tale, no other can have any very great value. It is not a little amusing to note, as is done by Mr. Scrope, F.R.S., how the peasantry have considered the Professor's bravery:—"Signor Palmieri, who watched throughout with creditable constancy the progress of the eruption, from his observatory on the Crocelle, appears by so doing to have gained a character for almost superhuman heroism among the frightened population of Naples and its environs. The philosopher must have been much amused at the fervour of his extravagant admirers, who raised him almost to the level of their adored St. Januarius; knowing as he well did, of course, the very small amount of danger that he incurred while he remained at his post, under a substantial roof, above the possible reach of any lava-stream, in a building founded on a portion of old Somma, which has certainly never been seriously disturbed for the last 1,800 years. He, better than any one, knows that the phenomena of the late eruption were by no means so exceptional as our newspaper correspondents would persuade us, but of the ordinary type of moderate Vesuvian paroxysms, such as the mountain has exhibited perhaps a dozen times within the last hundred years. That, indeed, is the judgment he is said to have passed upon it.

Eocene Fossil Wood.—This subject receives the attention of Professor T. Dyer, and, as is customary with Mr. Dyer's labours, it has been gone into fully and exhaustively. He explains fully the nature of so-called *tylose*, a subject upon which we have not been very clear before. He says that many instances of tylose are now known amongst recent plants, and have been repeatedly made the subject of investigation by foreign writers. Malpighi, indeed, in his "Anatome Plantarum," gives a very fair representation of them in the oak, remarking, "fistulæ frequenter *pulmonares* quasi *vesiculas* trachearum substantia excitatas continent." Without going into the literature of the subject, which is considerable, it is sufficient to state that the investigations of an anonymous writer in the "Botan. Zeit." for 1845, confirmed by Mohl and Reess ("Bot. Zeit." 1868), appear to leave little doubt that the "Thyllen," as the first-mentioned writer named them, are hernioid protrusions into the vessel from adjacent cells. In the words of Reess, "each young thylle makes its appearance as a bulging of a wood-parenchymatous or medullary-ray cell forced through a pore in the vessels." This process would be inconceivable in the case of the prosenchymatous cells; but parenchymatous cells, which surround the ducts, and those which form the medullary rays, do not undergo the same amount of speedy induration.

Sauropus Unguifer, a new Carboniferous Batrachian, has been just described by Dr. J. W. Dawson, F.R.S., of Montreal. He states that the principal specimens are several large slabs of brownish sandstone, bearing series of footprints in relief. Of the largest and most distinct series 40 to 50 footprints have been preserved, and are arranged in two rows, about 5½ inches apart. They were probably produced by a large Labyrinthodont Batrachian walking on a muddy shore, near the edge of the water, and are not very dissimilar from those described by Sir C. Lyell as found by Dr. King in the carboniferous beds of Pennsylvania. They also closely resemble, in size and form, the footprints found by Mr. R. Brown, F.G.S., in the coal-field of Sydney, Cape Breton, and described by Dr. Dawson in the second edition of "Acadian Geology," p. 358, under the name of *Sauropus Sydnensis*, and still more closely those found by Mr. Jones, F.L.S., at Parrsborough, N.S., and noticed in the same work. With these they may, in the meantime, be included in the provisional genus *Sauropus*.

The dimensions of the footprints are as follows:—

Hind foot, breadth	2·71	inches.
" " length	4·24	"
Fore foot, breadth	2·63	"
" " length	2·77	"
Length of stride	11·53	"
Average distance between the rows of footprints made by right and left feet	5·48	"

These measurements correspond very nearly with those of his *Sauropus Sydnensis* above referred to. He has given it the name of *S. unguifer*.

Fossil Plants from Queensland.—In the course of the discussion at a recent meeting of the Geological Society, upon a paper by Mr. R. Daintree, Mr.

Carruthers stated that he had examined the vegetable remains brought over by the author, which were of great importance. Some of those from the Devonian rocks appeared to be identical with species found in North America. From the remains of one of these, which he could not separate from one described by Dr. Dawson, *Leptophlæma rhombicum*, he had been able to reconstruct it in its entirety, of which he exhibited a drawing. The plant was lycopodiaceous, and its remains served to show that erroneous conclusions had been drawn as to the characters presented by the North American specimens, which had been regarded as having a *Sternbergia*-pith. There were specimens also of *Cyclostigma*, of the stipes of ferns, and of a doubtful calamite. With regard to the supposed *Glossopteris*- and *Tæniopteris*-epochs, which by some had been regarded the one as Palæozoic and the other as Mesozoic, he was not convinced that they could be distinctly separated, but thought rather that they might both belong to different portions of one great period. Systematically the two forms might be very closely related, the venation of the fronds on which the genera are founded occurring in two forms, which by Linnæus had been included in one genus, *Acrostichum*. He thought that neither was of a date earlier than Permian.

Death of Dr. Auguste Krantz.—We regret to announce the death of this distinguished collector of rocks, fossils, and minerals, which took place at Berlin on the 6th of April last. The "Geological Magazine" says of him that he represents one of the longest established and most able members of that rare class, a scientific merchant in rocks, fossils, and minerals—one, who not only knew accurately the commercial value of his collections, but was intimately acquainted with the scientific worth of every specimen which passed through his hands. Indeed, there are few museums which have not been enriched from his cosmopolitan repository. He leaves an immense and valuable collection both of minerals and fossils, the result of the labours of a long life devoted to these pursuits. Dr. Krantz was in his sixty-second year. We believe it is the intention of Madame Krantz to carry on her husband's business, with which she is well acquainted.

Amount of Coal in Austria and Hungary.—A very valuable and trustworthy report is that of Herr F. Foeterle, which has been recently published. It is accompanied by a large map, a glance at which will convince every one of the scanty distribution of coal over the enormous surface of the Austro-Hungarian dominions, and that most of it belongs to the western and the central districts. *a. True Coal-measures Coal* is found in Bohemia, in Moravia, and Austrian Silesia, in the Alps and in the Hungarian dominions. *b. Trias and Lias Coal* in the Alps, in Hungary and in the Banat. *c. Cretaceous Coal* in Moravia, in the Alps, and in Hungary. *d. Eocene Coal* (sometimes still showing the structure of the wood, then called Lignite, but generally a good black coal, which, when burnt, cakes, and is excellent for gas manufacture) is chiefly found in the *Alps*, where it is embedded in Cosina beds, below the Nummulite Limestone; Carpano near Albona, the large coal-basin of the Marburg district, Sotzka, Eibiswald. The coal of Häring, in Tyrol, belongs to a higher horizon of the Eocene, as does also the coal of Monte Promina and of Sebenico in Dalmatia. The coal of Gran, in Hungary, is also of Eocene age. *e. Neogene Coal* forms large basins in Moravia, Bohemia, Galicia, Bucovina, and in the north and south zones of the Alps and in Hungary. A glance at the accompanying map of the distribution of fossil fuel in Austria shows at once how insignificant is the extent of her coal-basins in comparison with the coal-formations of England, North America, or even Prussia.

England has	8,960	square miles of coal.
North America	100,528	" " "
Province of Silesia in Prussia .	1,280	" " "
Austria (as near as possible) . .	1,200	" " "

The whole produce of coal of all formations in Austria and Hungary amounted during 1868, in round figures, to 6,300,000 tons.

Phaneropleuron and Uronemus.—Professor Traquair, M.D., writes to the "Geological Magazine" to say he has now satisfactorily determined that his fossil fish *Phaneropleuron elegans* is identical with the *Uronemus lobatus* of Agassiz. Of course it is well that Professor Traquair has made this discovery, but it would have been better had he taken more time at first to inquire into the facts of the case ere he gave a new name to the fish.

MECHANICAL SCIENCE.

Paper Armour.—Colonel Muratori, at present in this country, has been endeavouring to introduce paper as a material for resisting bullets, and even projectiles of greater weight. A cuirass which he has invented, made of this material, and weighing no more than the ordinary metal cuirass, is said to possess a much greater power of resistance. Experiments on this material were made at Chalons in 1868, under the direction of the late Emperor of the French. The war stopped the experiments, and Colonel Muratori is now seeking to have them resumed in this country.

Torpedo Warfare.—Mr. C. W. Merrifield, F.R.S., has suggested, at the Institute of Naval Architects, that structural means of resisting torpedo attacks should be provided in armour-plated vessels. Of possible means of meeting torpedoes, he thinks that a rope or wire netting outrigged at a distance of 6 or 8 ft. from the ship's skin would afford the best protection in cases where a line of torpedoes is known to exist. But such a netting offers so great an impediment to speed, that it would be impracticable to employ it when the object is to cruise at a risk of meeting torpedoes. Hence he is led to suggest the following device as better than armour-plating the ship's bottom. Let the ship have three skins, each divided into cellular spaces of moderate size, the middle skin representing what is now the outer skin of ordinary double-plated ships. Each cell between the middle and inner skin to have an airtight manhole by which access can be gained to it from the interior of the ship, and a stopcock and union collar in its upper corner. The space between the middle and outer skins is also to be divided into cells by frames breaking joint with those between the middle and inner skins. Water is to be freely admitted to the cells between the middle and outer skins, so that in fact the bottom of the ship would have a kind of water-casing surrounding the middle skin. The middle skin is to be deliberately weakened near the bottom of each inner cell. When a torpedo explodes against the outer skin, it is expected that the shock will break through the middle skin at its weak points. Then the outer skin will be driven in, forcing the water into the cells between the middle and inner skins against the cushion of air contained in them. The work so expended will, it is hoped, save the inner skin from injury, except with very powerful torpedoes. After the explosion the inner cells may be cleared of water by attaching hose to the union joints and forcing in air. Nothing but actual experiment can decide on the value of such a plan, but it is believed to be the first suggestion yet made for providing structural means of resisting torpedo attack.

New Technical Journal.—Mr. E. J. Reed, C.B., the late Chief Constructor of the Navy, has started a new quarterly magazine dealing with subjects relating to naval architecture and marine engineering, which bids fair to render very great service, not only to those professionally engaged in the construction of ships and engines, but to the much larger circle of readers requiring information on such subjects of a reliable character. The articles in the first number of the new magazine are many of them by writers of eminent experience and knowledge, and range over a field including both special technical subjects, and others of more general interest. The articles on the stability of ironclads, the structure of iron ships, and the stowage of merchant ships, are of the former character. Those on the proposed Naval University, on naval tactics, and on the necessity of forming a naval staff, with the very severe review of the criticisms on the navy contained in Mr. Hawksley's presidental address, at the Institute of Civil Engineers, will be of interest to a very wide circle of readers.

H.M.S. Thunderer.—This powerful ironclad, a sister vessel to the *Devastation*, has recently been launched at Chatham. She is one of the mastless ironclads designed by Mr. Reed in 1869. The armour generally is 12 in. thick, but 14 in. in the neighbourhood of the port-holes. On the sides of the breastwork, in parts where a shot penetrating would do no harm to the machinery of the turrets, the armour is reduced to 10 ins., and it is also reduced to 10 ins. in the lower strake on the sides of the hull. The vessel has a sharp spur for ramming, and is short and handy. She is driven by engines guaranteed to give 5,600 indicated horses power, and will be armed with four 35-ton guns.

Wind Pressure on Inclined Planes.—Mr. Wenham and Mr. Browning have carried out some new experiments with a very delicate instrument on the pressure of a current of air on inclined planes. The results have been communicated to the Æronautical Society, and will be of interest not only to those who are studying the mechanism of flight, but also to engineers who have often to calculate the effect of wind pressure on their structures.

The Westinghouse Air-break.—This is a form of continuous break, or break applicable simultaneously to all the carriages of a railway train, which has been in use for about three years in America, and is now being tried in this country. An air-compressing pump is fixed on the locomotive engine, delivering the air into a reservoir under the foot-plate. The speed of the pump is self-adjusting, the valves being so set that it just keeps moving against the full pressure in the reservoir, and when the pressure in the reservoir falls it immediately accelerates in speed. Duplicate lines of ¾ in. iron gas-piping extend beneath the train, with ingenious indian rubber connections between the carriages. The breaks can be worked by sending the air through either line of pipes, so that if one fails the other remains serviceable, and further with two lines of pipes the carriages may be turned end for end, if necessary. The pipes not being in the centre line of the carriage, this could not be effected unless two symmetrical lines of piping were provided. The breaks are actuated by the air pressing on a piston in an air cylinder placed under each carriage. Hence the breaks can be instantly put in action by turning a cock and admitting air from the air-reservoir to the piping. The pressure of air in the reservoir is 60 to 70 lbs. per square inch, but it is usually regulated not to exceed 10 to 30 lbs. in the air-cylinders. The details of this break, which are most ingenious and most carefully worked out, cannot be understood without drawings, and for these we must refer the reader to "Engineering" for May 24, where the invention is very fully described.

MEDICAL SCIENCE.

The Physiological Action of Tobacco has been very carefully studied by Herren Vogl and Eulenberg. They investigated the physiological action of (1) those bases volatile below 160° and (2) of those volatile between 160° and 250°. Both portions act like nicotine, producing contraction of the pupil, difficult respiration, general convulsions and death. They act more quickly by the stomach than when sub-cutaneously injected, but even then are not as prompt as nicotine. On post-mortem examination, the lungs and air-passages were found to be highly congested. They think that the disagreeable symptoms produced in the incipient smoker, and the chronic affections which excessive smoking produces, as well as the poisonous effects of tobacco-juice when swallowed, are due to the pyridine and picoline bases, and not to nicotine. They explain the fact that stronger tobacco can be smoked in cigars than in a pipe, by finding that more of the volatile bases are present in the smoke of a pipe; more especially of the very volatile and stupefying pyridine; while in a cigar, little pyridine and much collidine are formed. The authors compared this action with that of the bases obtained from other plants used for smoking; with those from dandelion, willow-wood and stramonium, and with pure picoline from Boghead coal. The action was entirely similar, but, with the exception of the willow wood bases, they produced no contraction of the pupil. Picoline in vapour is extremely poisonous, producing great irritation of the air-passages, convulsions and death. From these results the authors believe that the different effects of smoking opium are due simply to a difference in the proportion of the bases produced by its combustion.—*Arch. Pharm.* II. cxlvii. 130.

The Supposed Syphilis Corpuscles.—The "Monthly Microscopical Journal" of April, quoting from the "Allg. Wiener Med. Zeit.," says that the latter contains a serio-comic article on this discovery of Lostorfer, which, for a few moments, shook such men as Stricker, Hebra, and Skoda off their balance. Dr. Wedl, the author of the article, says:—"These corpuscles will, at the next meeting of the Medical Society, be solemnly buried in Dr. Stricker's museum. The sympathy of the profession is requested under these painful circumstances, and the Society will doubtless institute special masses for the repose of the departed. It is very lucky that the members did not, in their hurry, have medals struck for the discovery; and there is time left to send counter-orders to Paris and London, to stop enthusiastic researches which might bring some blame on the Vienna Medical Society. The latter may derive from this mishap the lesson to beware of allowing itself to be made a trumpet to ephemeral discoveries." The "Lancet," however, finds it difficult to believe—and the "Microscopical Journal" quite coincides with it—that observers like Stricker and Hebra would have been carried away by imperfect experiments. It is clearly stated that different kinds of blood were placed under Dr. Lostorfer's microscope (he not knowing whence the blood came), and he constantly recognised his peculiar corpuscles in blood coming from patients affected with syphilis.

Colourless Bile.—In the "Comptes Rendus," March 18, M. E. Ritter quotes the results of a series of analyses made by him on colourless bile, taken from the gall-bladders of men and animals submitted to autopsy. As an instance of the composition of such bile (as yet hardly ever analysed, since the colourless fluid has been taken to be mucus) we mention here the following, in 1,000 parts:—Water, 923·5; salts, 12·4; fat and cholesterine, 6·8; organic matter, 2·1; salts of the bile acids, 55·2. It appears that colourless bile and fatty degeneration of the liver are somehow connected together.

Ancient Egyptian Perfume.—Dr. Personne states in the "Journal de Pharmacie," March, that he accidentally obtained a small piece of a chocolate-brown substance, which originally was apparently a paste, but is now hard. On further examination it was found to consist of a lime-soap, mixed with myrrh, olibanum, benzoin, and probably some essential oil. Dr. Personne states that at the present day there is sold in Egypt as a perfume a substance of similar composition, and locally known as *Bouh Kourre-bare,* which means perfume from the Arabian frontier.

A Mechanical Means of Lowering the Temperature, which is peculiar, is described in a late number of Pfluger's "Archiv," by Herr Manassein. He states that, if rabbits, seated at ease in a box, were swung in a transverse direction to their length, and with a rapidity of twenty-eight to thirty double swings, at a pendent length of 117 ccm., that the temperature taken in the rectum after the swinging was, by 0·3° to 1.2° C., in the mean by 0·66°, lower than before. The depression of temperature continued from a half to two hours, and was most decided after fifteen minutes of swinging; a longer swinging did not increase the effect. The maximum of the depression of temperature occurred, at first, some time (about thirty minutes) after the cessation of the swinging. The last-named circumstance, as well as that the wrapping of the animals in cotton-batting in no wise hindered the effects, and, on the other hand, that a more rapid swinging appeared less effective, prove that the current of air which is produced by the swinging is not the cause. Swinging in the longitudinal diameter made the animal more afraid and more restless; it had, however, the same influence upon the temperature. The effects of the swinging on rabbits were greater where the eyes were blinded, and less, on the other hand, in animals in which the respiration was moderately disturbed by the tightening of a cord around the neck, and also in animals only slightly narcotized by morphine. In injections of an ichorous fluid, the feverish increase of temperature produced was lessened by the swinging, and indeed, by repeated swinging, brought to a stop.

Influence of Dr. Wright's Morphiæ Compounds on the Animal Body.—Some experiments on this subject were carried out by Dr. Reginald Stocker, Pathologist to St. Mary's Hospital, and are of interest. He says that doses of 1 decigramme of the compound $C_{68}\ H_{81}\ I\ N_4\ O_{10}$, 4HI from codeia, and of the similar compound from morphia, were given to an adult terrier by the mouth without producing any perceptible effect whatever; when the dose was increased to 3 decigrammes, in each case repeated defæcation in the course of a few hours was produced, the stools being more loose than ordinarily and frequently of a dark greenish colour; no other symptom was noticeable, and no appreciable difference in the action of the two compounds was perceptible. Doses of 5 decigrammes of the compound $C_{68}\ H_{82}\ I_2\ N_4\ O_{10}$, 4HI from each of these sources were given to the same dog by the mouth, with the result of producing similar repeated defæcation in the course of two or three hours; the sole difference discernible between these and the former experiments being that the effect was produced somewhat sooner and was of longer continuance in the latter cases, a result probably produced solely by the larger dose. No material differences were observed between the codeia and morphia derivative. The same dog was employed throughout, two or three days being allowed to intervene between each experiment, so that the animal had recovered from the effects of a former dose before the administration of another.—*Proceedings of the Royal Society*, for April, 1872.

Gastric Juice and Pepsin applied to Wounds.—The "New York Medical Journal" states that there have been performed recently a number of experiments with the above fluids, applied as follows: the gastric juice of dogs was pencilled, at short intervals daily, fifteen to twenty times upon the wounded surface, or small pledgets of cotton were applied, and upon them a second larger layer of wadding dipped in a very dilute solution of muriatic acid. Several experiments were made, especially upon chancres, upon soft chancres in particular. After five to eleven days, commencing cicatrization followed as a rule. The remedy is chiefly indicated in soft chancre, in diphtheria, phagedæna, and nosocomial gangrene.

A Curious Memento of Jenner.—A very interesting memento of the discoverer of vaccination has recently been presented to the Royal College of Physicians by Sir John William Fisher. It consists of a cow's horn, beautifully polished, presented to Sir J. W. Fisher, in the year 1813, by Dr. Jenner, and polished by himself. The gift was made in grateful acknowledgement of services rendered to Jenner's sick children by Mr. Fisher, then a medical assistant in Soho. The horn is now mounted in silver, and bears an appropriate inscription, stating the circumstances under which it was presented to the college. Dr. Burrows, the President, in asking the acceptance of the horn, stated that it was probable—though there was no record of the fact—that the horn had been taken from one of Dr. Jenner's favourite cows on which he made his experiments on vaccination.

Atheroma in the Arteries.—Dr. Moxon has been making some recent researches on this subject, which are of importance. He shows the connexion between inflammation of the arterial tunics, atheroma, and aneurism, and he dwells upon, and accounts for, the relatively greater frequency of the latter affection among soldiers. Dr. Moxon holds (1) that what is called atheroma of arteries is sub-inflammation of various degrees, of which the lower degrees end in fatty degeneration of the coats, along with the inflammatory products; and (2) that the determining cause of this change is mechanical strain, a general altered nutrition—such as obtains in gout, syphilis, &c.—being regarded in the light of a predisposing cause.—Vide *Lancet*, June 8.

Chemical Composition of Pus.—The "British Medical Journal" states, in a recent number (May 28, 1872), that Hoppe-Seyler has obtained results which are interesting in reference to the question of the origin of the pus-corpuscles and their identity with the colourless and lymph corpuscles. He introduced fresh crystalline lenses of the ox into the abdominal cavity of dogs, and analysed them after a period varying from one to fourteen days. As was expected, the lenses became infiltrated with lymph-corpuscles. Glycogen was found in greatest abundance at the eighth day, at which period they contained the greatest number of contractile corpuscles. The glycogen is due to these corpuscles. If the lenses were not plunged immediately into boiling water, but allowed to stand for some time, no glycogen was found, but in its place sugar. In the pus of congestion-abscesses, no glycogen occurred. The occurrence of glycogen, therefore, may be taken as a means of distinguishing lymph from pus-corpuscles. When glycogen is found in abscesses, it will be found to coexist with the presence of numerous contractile corpuscles. Lymph-corpuscles, therefore, by their transformation into rigid pus-corpuscles, become deprived of their glycogen.

Animal Starch.—The "British Medical Journal" in one of its May numbers, gives an account of M. Dareste's researches on this point. It says he has observed granules which present the optical characters of starch in the hen's egg, both when newly laid and during the process of incubation. The granules in the new-laid egg give a blue with iodine; while those observed during incubation sometimes give a blue, but often give a red. The granules appear and disappear several times. The formation of the area pellucida is partly due to the disappearance of the third series of granules. The fourth series form the glycogen in the liver. He attributes the disappearance of starch to its conversion into grape-sugar, and its reappearance to the change of grape-sugar back to starch.

Action of Alcohol on the Body.—This subject is yet far from being exhausted. Dr. Subbotin gives in the "Zeitschrift für Biologie" (Band vii., Heft 4), also "Lancet," June 8, 1872, the details of a considerable series of experiments he has recently performed on rabbits. The mode of detection of the alcohol he employed was its acetification by chromic acid, or rather by bichromate of potash and sulphuric acid, the quantity being determined by the subsequent estimation of the distilled acetic acid by means of soda solution. The respiration experiments were conducted in an apparatus lent to him by Voit, and constructed on the plan of the large apparatus of Pettenkofer and Voit. Alcohol of the strength of 29 per cent. was injected into the stomach through the exposed œsophagus, and this tube at once ligatured. The results at which he arrived were: That, during the first five hours after the introduction of alcohol into the stomach, a considerable amount escapes by the skin, lungs, and kidneys; that at least twice as much escapes by the lungs and skin as by the kidneys; that the amounts he obtained, showing that from 6·79 to 7·4 per cent. were thus eliminated, were, from various considerations, certainly below the quantities really discharged from the system. These conclusions he arrived at in 1870. Quite recently, however, he instituted another series of experiments, the object of which was mainly to determine for how long a time after ingestion alcohol continued to be excreted by the skin and lungs. In one of these experiments he found that 12·6 per cent. of the alcohol was eliminated in eleven hours and a half through all these channels; in another instance 16 per cent. was eliminated in twenty-four hours, either in the unaltered condition or only changed into aldehyde. Subbotin maintains that alcohol cannot be regarded as in any sense a food, since, under its influence, the metamorphosis of tissue diminishes, the temperature falls, the amount of carbonic acid excreted lessens, and a smaller quantity of urea is discharged. Its action, he thinks, is direct upon the nervous system.

METALLURGY, MINERALOGY, AND MINING.

An Improvement in Blowpipe Operations.—MM. Armin, Junge, and Mitzopulos, of Freiburg, have greatly improved the blowpipe by an apparatus of which the following is a description:—A common wide-mouthed bottle is carefully fitted with a caoutchouc cork bored with two holes, into each of which passes a piece of glass tube bent at a right angle. On to one of these tubes is slipped the caoutchouc tube coming from an ordinary caoutchouc bellows, whilst the other is put in communication with the blowpipe nozzle by means of four pieces of caoutchouc tubing joined by three pieces of glass tube, drawn to a fine point at each end. This forms the main peculiarity of the arrangement. When air is forced into the bottle by the blower, in jerks, it finds a difficulty in escaping as fast as it comes in, on account of the six fine openings in the glass tubes that it has to pass through on its way from the bottle to the nozzle, and it thus acquires a certain pressure in the bottle, and flows out towards the nozzle as a regular blast. The bottle may be about 6 inches high by $3\frac{1}{2}$ inches wide, with a neck $1\frac{1}{2}$ inches in diameter; but of course the dimensions are of no great importance. On the whole a somewhat large bottle is better than a small one. The pieces of glass tubing used are 2 inches long by $\frac{1}{5}$ of an inch in diameter. The apparatus will be stronger if instead of a glass bottle a tin cylinder is used, about 4 inches high by 2 inches in diameter, with two tin tubes opening into its top. Small metal cylinders, with a fine hole at each end, may be used instead of the little glass tubes. A blowing apparatus constructed in this manner will deliver a perfectly regular blast, and will be of practical interest to those who are thinking of working in places where it is impossible to repair the ordinary instruments.—See *Chemical News*, June 7th.

The Metallurgy of Lead.—Mr. John Jex Long read the first part of a paper on this subject before the Glasgow Philosophical Society, March 25, 1872, in the course of which he described the operations which he had personally witnessed at the works of the London Lead Mining Company, at Middleton-in-Teesdale, a few miles from Barnard Castle, where the mining operations were commenced about 170 years ago. He explained the geological position of the lead-bearing rocks in Teesdale, describing the direction, extent, and richness of the mines, and their mode of occurrence in flats, pockets, strings, &c. The annual product of the mines referred to is about 2,000 tons of metallic lead, containing about 9 ozs. of silver per ton, which is separated in the metallic form by Pattinson's process. Before the company obtained any pecuniary return from the mines, they had to expend about 30,000*l.* Mr. Long described his exploration of the Coldberry Mine at Middleton, the mode of working it, and the various mechanical operations by which the mineral is prepared for smelting; and he promised, on a subsequent occasion, to devote the second part of his paper to the consideration of the smelting and refining processes and the extraction of the silver. The paper was profusely illustrated by specimens.

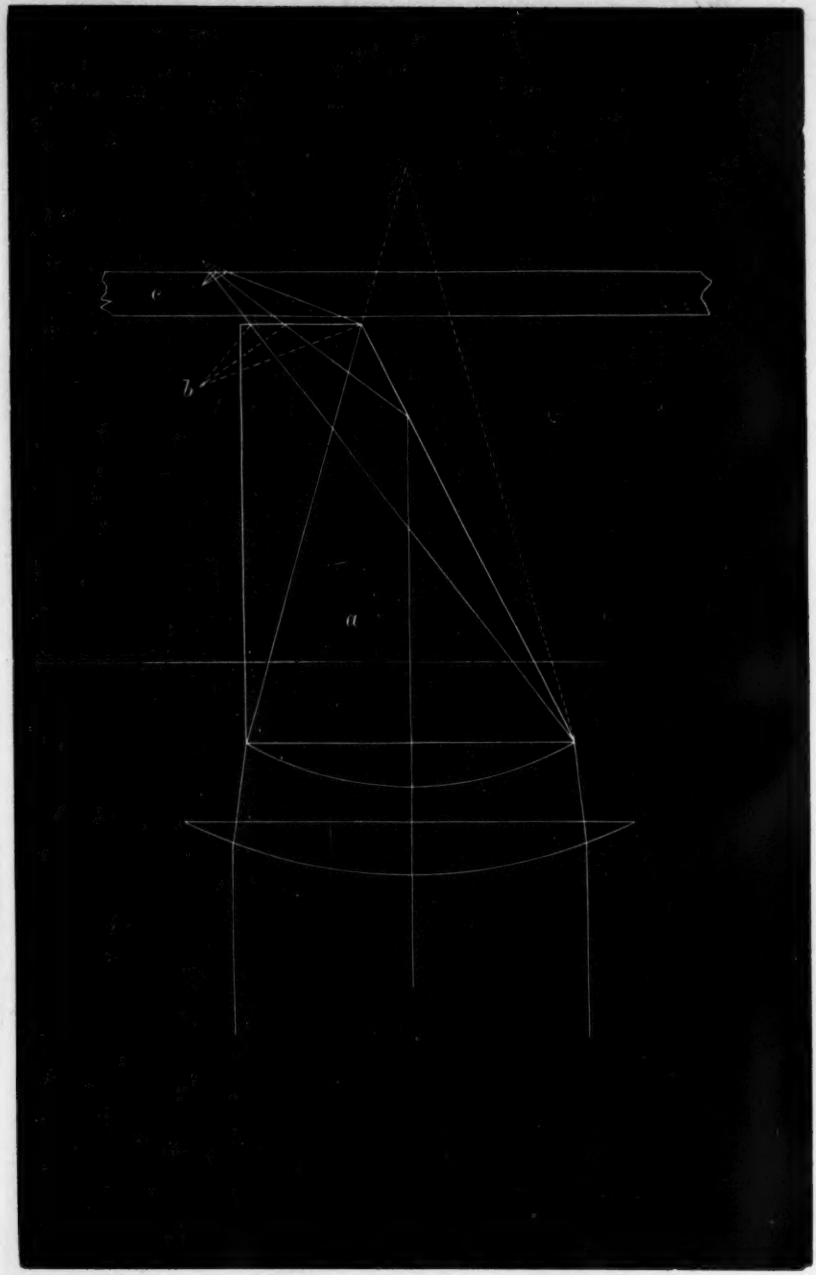
Waste of Sulphur in Mining.—Dr. W. H. Tayler writes to the "Chemical News," May 24, upon this subject. He states that whilst minerals of every description are rising in value, it will scarcely be credited, although such is the fact, that in several of the tin mines in Cornwall, at the present time, a large source of what ought to produce wealth is allowed to be wasted. Large quantities of sulphurous fumes are allowed to pass off daily in calcining the tin stuff, instead of manufacturing it into sulphuric acid. He states that he knows of an instance where three tons of sulphur are daily allowed to escape, which if manufactured into sulphuric acid, the present price of which is 3*l.* 10*s.* per ton, would yield a revenue of more than 12,000*l.* a year. While Spain and Portugal and other parts of Europe are ransacked to find sulphur ores to supply the manufacturers of sulphuric acid, in Cornwall all these sources of wealth are allowed to be wasted.

Crystalline Dissociation.—MM. Favre and Valson have published the second part of a very valuable monograph on this subject. It contains so large a series of tabulated forms, the results of experiments, that an abstract would be impossible.

MICROSCOPY.

Wenham's improved Reflex Illuminator for the highest powers of the Microscope.—We cannot resist giving our space to this excellent invention of Mr. Wenham's for microscopic illumination. The diagram (p. 329), five times the size of the instrument, illustrates the plan he has adopted to overcome the defects of the olden apparatus. In this *a* is a cylinder of glass half an inch long and four-tenths in diameter, the lower convex surface of which is polished to a radius of four-tenths. The top is flat and polished. Starting from the bottom edge, the cylinder is worked off to a polished face at an angle of 64°: close beneath the cylinder is set a plano-convex lens of 1¼ focus. Parallel rays sent through the lens, after leaving the lower convex surface of the cylinder, would be refracted to the point shown by the dotted lines if continued in solid glass; but by impinging on the inclined polished surface (which is far within the angle of total reflection) they are thrown on the flat segmental top; here they would be totally reflected and beaten down again to the point, *b*, outside the cylinder; but if an object-slide, *c*, be laid over the flat top with an intervening film of water, the rays proceed on till the focal point reaches the upper surface or is slightly beyond it; here total reflexion now takes place; all the light is concentrated to a minute spot in the centre of the field of view of the microscope, and most of the rays are available for any object brought there by traversing the slides over the water top of the illuminator, which must be kept full without allowing any to run down the reflecting surface. It will be seen, in order to get the focal point in the centre of the microscope, that the lens centre must be excentric; but this does not involve the slightest inconvenience, as the excentricity only amounts to a little over two-tenths of an inch, and is so small that the same adjustment of the mirror serves during an entire revolution. The apparatus rotates on the focus as a centre. The management of this illuminator is very easy and simple; its fitting goes into the ordinary sub-stage, and has an independent rotary movement of its own, like that of Nachet's prism. The cylinder is brought up nearly level with the stage. The centre of rotation is set true by a dot on the fitting, seen with a 1½ object-glass. A drop of water is then placed on the top, upon which the slide is laid. The required objects *on the slide* are found by a low power, and may be distinguished by their brilliant appearance, while those on the cover are nearly invisible. The light is thrown up by either the plane or concave mirror. The former is generally the best and most controllable. The lamp should not be placed much beyond the stage, else its direct rays will get underneath and mar the blackness of the field. Having got the best effect, say on a *diatom*, or insect scale, by tilting the mirror, we now proceed to rotate the illuminator. During this the most exquisite unfolding of structure takes place, opening out as it were into detail the form of bosses or ribbings. On that superb test, the *Podura*, for example, when the light is thrown from the apex to the quill, the whole scale is dotted over with bright blue spots laying in a zigzag direction; these are the most prominent parts or the club-end of the markings, which are nearest in contact with the glass. Fuller details are contained in the paper from which these remarks are taken, which should be referred to by the reader.—Vide *Monthly Microscopical Journal*, June 1872.

The Best Way to see the Markings on Battledore Scales is the following, according to Dr. Anthony ("Monthly Microscopical Journal," June, 1872). He says that scales are best seen by "transmitted" *ordinary* light, when a "pin-hole" stop is placed like a small cap on the usual wide-angled condenser, and by being very particular that both flame of the lamp and object are in focus, or very nearly so, at the same time. When scales are looked at by "reflected" light, then they are to be seen at their best by bringing up a little transmitted light at the same time, such light being quite subordinate, and only for the purpose of rendering the black shadows transparent. A similar effect of course can be got by using a second lamp and "bull's-eye" at the other side of the microscope, or even in a minor degree by a bit of white card, placed in the stage beneath the object, so as to reflect light, but



c
b
a

on the whole he prefers the first form of "double illumination" as equally satisfactory and far less troublesome.

Mr. Collins' Light Corrector.—An ingenious bit of apparatus has been devised by Mr. Charles Collins (17, Great Portland Street). It consists of a brass stage-plate with a groove in which rotates a diaphragm of four apertures, one of them being open and the other containing blue glasses of special tint, and one with a finely-ground surface. These effectually correct the yellowness of all artificial illumination, making the light soft and agreeable to the eyesight as well as improving the definition. It is in fact an improvement on Rainey's Light Modifier so as to obtain more varied effects, and does not require any special fitting, as it can be used on any microscope.

A Curious Fact in Bichromatic Vision has been observed and described by Mr. J. W. Stephenson, F.R.M.S. He says, as of course is known to most students of optics, that, by the aid of a double-image prism and a film of selenite, two images may be shown in the field of the microscope, the colours of which will be complementary the one to the other, and that when these images overlap, the resulting image will be, as far as the overlapping extends, of white light; but it is not, he thinks, so well known that when, by a suitable arrangement, different colours are made to occupy the two fields of a binocular, the resultant is a combination of such colours, and that if these are complementary to one another, the sensation of colour induced in the brain by the retina of one eye, is neutralised by that which reaches it through the instrumentality of the other, and that by the combination of the two the sensation of colour is entirely lost. This, however, Mr. Stephenson observed in the most convincing way. The fact is a little curious, and we think, as yet, is insufficiently explained.—*Monthly Microscopical Journal,* May 1872.

The Two Best Test Objects.—Dr. Col. Woodward says that *Amphipleura pellucida* is a useful and valuable test for immersion objectives of $\frac{1}{8}$th inch focal length or less. Lower powers can only hope to resolve it if possessed of excessive angular aperture. When, however, it is desired to discriminate between small differences in the excellence of objectives intended for the most exquisite resolution, a more subtle test is required, and this will be found in the nineteen-band plate of Nobert, by those who take proper precautions in its use. Those, however, who believe they have secured resolution whenever they see lines in the higher bands of the plate, without duly considering their number, must not be surprised if objectives they have accepted as resolving the ultimate bands of the plate fail to show the striæ on even the coarsest frustules of the *Amphipleura pellucida.*—*Monthly Microscopical Journal,* April.

PHYSICS.

Death of Professor Morse.—We regret being obliged to announce the death of Professor Samuel Finley Breeze Morse, which took place at his residence in New York city, on April 4, at the advanced age of eighty-one years. Few Americans have attained so world-wide a renown as Mr. Morse, growing chiefly out of his success in devising and introducing the system of electric telegraphy which bears his name. Mr. Morse was the eldest son of Jedidiah Morse, D.D., an American clergyman, better known as a geographer, whose writings were the first devoted to the elucidation of American geography, his educational works of this character remaining for more than a generation in general use. Prof. Morse was born in Charlestown, Mass., April 27, 1791. He graduated at Yale College in 1810.

An Improvement in Barry's Singing Flame has been devised in America by Mr. W. E. Geyer, and is described by him in "Silliman's American Journal." He states that a simple addition to the apparatus described by Barry last year has given him a flame which, by slight regulation, may be made either: (1) a sensitive flame merely; that is, a flame which is depressed and rendered non-luminous by external noises, but which does not sing; (2) a continuously singing flame, not disturbed by outward noises; (3) a sensitive flame, which only sounds while disturbed; or (4) a flame that sings continuously, except when agitated by external sounds. The last two results, so far as is known to him, are novel. To produce them it is only necessary to cover Barry's flame with a moderately large tube, resting it loosely on the gauze. A luminous flame six to eight inches long is thus obtained, which is very sensitive, especially to high and sharp sounds. If now the gauze and tube be raised, the flame gradually shortens and appears less luminous, until at last it becomes violently agitated, and sings with a loud uniform tone, which may be maintained for any length of time. Under these conditions, external sounds have no effect upon it. The sensitive musical flame is produced by lowering the gauze, until the singing just ceases. It is in this position that the flame is most remarkable. At the slightest sharp sound, it instantly sings, continuing to do so as long as the disturbing cause exists, but stopping at once with it. So quick are the responses, that by rapping the time of a tune, or whistling or playing it, provided the tones are high enough, the flame faithfully sounds at every note. By slightly raising or lowering the jet, the flame can be made less or more sensitive, so that a hiss in any part of the room, the rattling of keys even in the pocket, turning on the water at the hydrant, folding up a piece of paper, or even moving the hand over the table, will excite the sound. On pronouncing the word "sensitive," it sings twice; and in general it will interrupt the speaker at almost every "s" or other hissing sound.

A new Seismograph has been described in a paper before the Wellington Philosophical Society, New Zealand, by Mr. Wm. Skey. The following is a partial account of the instrument. A small block of metal, having a thin slip of platina attached, or a small wire of this metal projecting a little apart from it horizontally, is connected with an electro-magnet with keeper suspended, and this with a single cell of a battery. A very fine silver wire (that used for sewing wounds), 3 ft. long or so, joined at its lower extremity by a little platina wire, depends from a point above, so that the two platina wires may intersect; a firm adjusting screw or other apparatus set contiguous to the point of suspension enables one to put this point in such a position that these wires are allowed to press but very slightly upon each other. The silver wire is connected with the other pole of the cell through this point of suspension through a vertical galvanometer. The shock-receiving part is placed underground to avoid the interference of winds or that of violent detonations, the metal block being set upon a wooden pile driven some distance in solid earth. When properly set, a single make and break contact of this kind is so sensitive that the impact of 3 pounds of stone, falling from a height of 5 ft. upon the ground, at a distance of 50 ft. from it, moved the needle of the galvanometer very determinately. The intervening ground was clay.

Coal-gas for Lighthouses.—Mr. J. Wigham recently gave a lecture in Dublin on this subject. He said that coal-gas was first used in lighthouses in 1865, by Mr. Samuel Bewley of the Irish Board of Lights, who tried some experiments at Howth. The first burner used was called the "crocus." The principles involved in this burner, and the means taken to economise the gas, proved that by the crocus there was an immense saving, taking light for light, as compared with the gas usually used in our houses. The lecturer then alluded to the economy as compared with oil. There was a saving of about 50*l.* per annum on each lighthouse in which the gas had been tried, and, in the case of intermitting lights, the difference was much greater as regards economy. Dr. Tyndall has been sent down to investigate the whole matter at Howth, and that gentleman had reported favourably. There were five lighthouses at present on the Irish Coast illuminated by gas, and they are about to try it on two of the English lighthouses. In speaking of the electric light, Mr. Wigham said that though the latter was very intense, yet it was deficient in quantity, and it was not so good as coal-gas for penetrating fogs. Mr. Wigham then proceeded to explain the mechanical arrangements and the construction of the lenses.—*Royal Dublin Society,* March 18, 1872.

Estimating the Intensity of Light.—The *Chemical News,* April 12, quoting from an American journal, states that Dr. Vogel proposes *nitroprussid iron* as a suitable salt for determining quantitatively the intensity of light. For the preparation of this reagent, dissolve chemically pure oxide of iron, best obtained from the oxalate, in hydrochloric acid, and evaporate nearly to dryness to expel the excess of acid; and after filtering, add an aqueous solution of nitroprussidnatrium in proportion of three of the iron to two of the latter. There is usually a slight precipitate produced by this mixture, which can be collected on a filter; but this operation must be performed in a dark room. We have now a liquid excessively sensitive to the action of sunlight. By exposing a small quantity of a known specific gravity to the action of light, a precipitate of prussian-blue will instantly begin to fall; and, on redetermining the sp. gr. in the dark chamber, its decrease will be found to be proportional to the precipitate; and we have thus the data for measuring the intensity of light. It was found by Dr. Vogel that the liquid, exposed for forty-eight hours before a kerosene lamp, was not in the least affected, but a piece of magnesium wire, when burned, immediately produced a precipitate. By employing a long instrument graduated in millimetres, it would appear to be possible to measure the intensity of the light by the number of millimetres occupied by the precipitate. The invention has an important bearing upon photography.

How to bend Glass Tubes so as to fit Apparatus.—Mr. J. Laurence Smith states that it is well known that it requires some tact to bend a tube with an even curve and without collapsing its sides, and many chemists never do succeed in bending them skilfully. Although having no particular skill in this matter, he never fails to bend them perfectly satisfactorily, by using a flame different from the one usually employed; the flame is one given by the Bunsen burner described in his article on alkali determination in silicates (see *Chemical News*, vol. xxiii., p. 235). The burner is very commonly used now in all laboratories, where the extremity of the burner is flattened out so as to give a short and thin but broad flame, something like the flame of an ordinary gas-burner. The tube is placed in this flame and turned round and round, until a good heat is given to the tube; it is then withdrawn from the flame and bent, when it does so with a perfect curve and no collapse of the sides of the tube. Of course this is only intended for the smaller tubes, but a tube of 1 centimetre and more can be thus bent very readily.

Spectra of Manganese in Blowpipe Beads.—Manganese may be easily detected in this manner according to Mr. Charles Horner (*Chemical News*, March 22). The following is the best way of preparing the beads and examining their spectra. Sufficient chlorate of potash should be volatilised in the loop of platinum wire to form a bead about the size of a pin's head, then take up the merest trace of the oxide and fuse it; next add enough chlorate to fill the loop, and very gently flame the bead for a few seconds and withdraw, when it crystallises a delicate pink colour. In adding the second portion of chlorate care must be observed not to volatilise the salt, and the best result is when the bead does not much exceed the thickness of the wire. If after adding the second portion we volatilise the chlorate, we immediately obtain a greenish-coloured bead of manganate of potash, and more transparent than the pink bead. In order to see the spectra of these beads, they should be examined by the spectrum microscope and strongly illuminated. The pink bead exhibits several absorption-bands more or less definite according to the amount of manganese present. The three most distinct bands, however, lie between D and *b*, and may be seen when the bead is scarcely coloured. This spectrum very closely resembles that given by the crystals of perchlorate coloured by permanganate of potash, but the bands are slightly more refrangible in the former. The green bead gives a spectrum of two bands, one broad band covering the sodium line, and a very narrow band in the orange ray. This spectrum test is most useful in the examination of minerals, for although the pink colour is sometimes disguised by the presence of other substances, as in rhodonite, which communicates a yellowish tint to the bead, yet the three principal absorption-bands are plainly visible.

ZOOLOGY AND COMPARATIVE ANATOMY.

Zoological Nomenclature.—This subject has excited considerable attention in the United States. "Silliman's American Journal," May 1872, contains an important review of Mr. Lyman's recent and somewhat novel opinions on the subject. We of course cannot enter on so long a question. We may, however, refer the reader to Mr. Lyman's work, "Illustrated Catalogue of the

Museum of Comparative Zoology," in which, under the heading of "Notes on Nomenclature," his peculiar views are expressed. It is remarkable that some years ago, the American Association for the Advancement of Science appointed a committee to reconsider the canons of biological nomenclature, and to report whether, with the growth of science, they required any additions or alterations. No report has yet been made, nor, so far as we are aware, is any likely to be presented, until the subject is again brought prominently forward and new instructions given. Professor A. E. Verrill has since republished the Revised Rules of Zoological Nomenclature adopted by the British Association for the Advancement of Science in 1865, and has accompanied them by a few apt comments: in England, Mr. W. F. Kirby, in a paper read before the Linnean Society of London, has called attention to the extensive changes which a strict adherence to the laws of priority would cause in the generic nomenclature of butterflies; and quite recently has put the same into practice in his catalogue of these insects.

A New Crustacean: Tomocaris Peircei has been discovered by Professor Agassiz. This, which is named as above, was dredged in 45 fathoms about 40 miles east of Cape Frio. It is described as very like *Serolis*, with the marked difference, that the thoracic rings are much more numerous, and the abdomen much smaller; and it is said that its resemblance to Trilobites is unmistakable and very striking, and that it can be referred to no one of the orders or families in Milne Edwards' or Dana's classification. From the details of Prof. Agassiz's description, the animal is evidently one of the Serolidæ, apparently congeneric, perhaps specifically identical, with the *Brongniartia trilobitoides* of Eights (Trans. Albany Institute, vol. ii., p. 53, pl. 1, 2, 1833), which is referred to the genus *Serolis* by Audouin and Milne Edwards (Archives du Muséum d'Hist. nat., tome ii., p. 29, pl. 2, fig. 11, 1839), and retained in the same genus by Milne Edwards in his great work (Hist. nat. des Crust., tome iii., p. 232, 1840). To make this apparent it is necessary to observe that what Prof. Agassiz calls the head includes the first thoracic segment, which in the *Serolidæ* is anchylosed with the head; that what he considers the three posterior segments of the thorax, have been regarded by carcinologists as belonging to the abdomen; and that, as a result of this first homology, what have been regarded as the anterior legs are called maxillipeds. The only point in the whole description which can militate against the view here expressed is in the description of the nine pairs of legs which are said to be "all alike in structure;" the six anterior pairs, however, are "larger than the three last, which are also more approximated to each other," thus agreeing perfectly in position with the three anterior abdominal legs of the ordinary Serolidæ. The perfect agreement in all other respects, however, leaves little doubt of the close affinity between *Tomocaris* and the *Brongniartia* of Eights. It may be well to notice that among the species referred to *Serolis*, there are several genera, distinct from the typical *S. paradoxa*, and that the species described by Eights represents one of these, although the name *Brongniartia* is preoccupied.—*Silliman's Journal*, May.

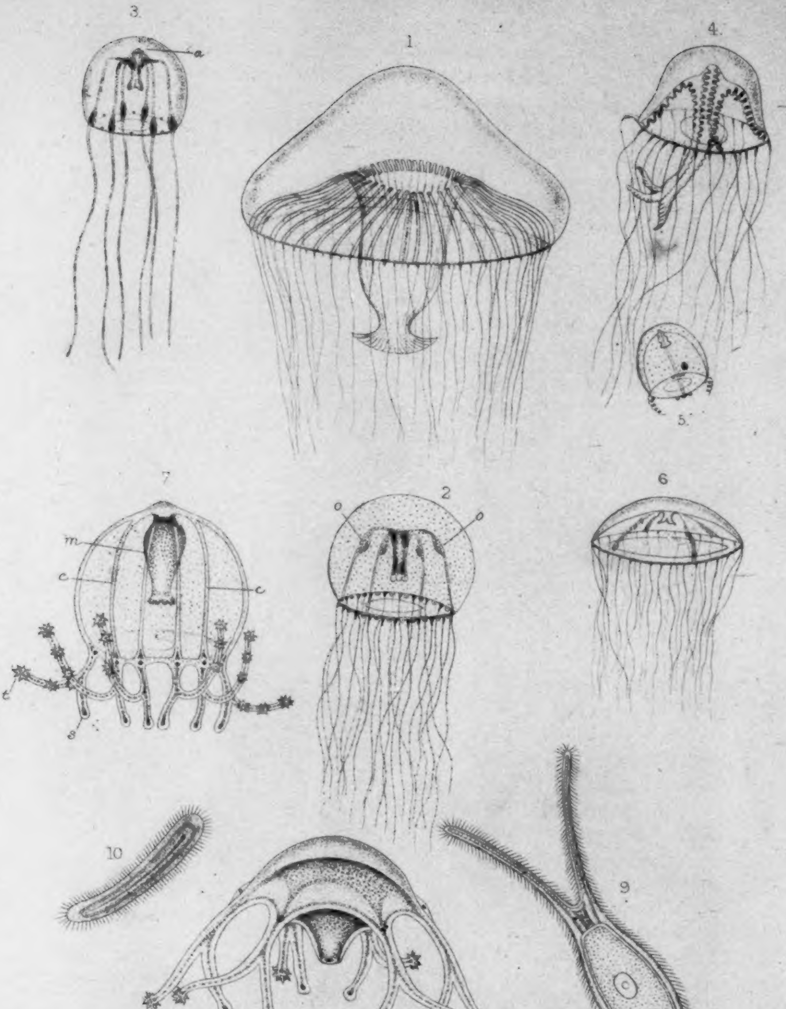
The Great Public Aquarium at Naples.—An account of this immense undertaking is given by a contemporary, and is of sufficient interest to have a place in our columns. The building, which is under the direction of M. Anton Dohrn, is rectangular, measuring 100 ft. by 70 ft., with a height of 40 ft., and is 100 ft. from the sea. The lower part is to be occupied by the tanks of the great aquarium, to be opened to the public; and the upper will contain 24 rooms for laboratories, a library and collections, with lodging rooms for three or four zoologists. There will be 53 tanks in the lower story, one of them 32 ft. long, 10 broad and $3\frac{1}{2}$ deep, another, 26 ft. long, and twenty-six 3 ft. by $3\frac{1}{2}$ ft. The tanks throughout are furnished with a continuous current of sea-water. Upstairs, the library room is large enough to hold 25,000 volumes. The principal laboratory room will contain 20 to 30 tanks of different sizes; and besides there are private laboratories for the chief zoologist and the first assistant, and other small laboratory rooms, and rooms for collections.

Three Additional Zoological Publications have lately made their appearance, and if we may judge from the first numbers and from the character of the editors, they are likely to commend success. The one, "Archives de Zoölogie Expérimentale et Générale," issued by Professor Lacaze Duthiers, will take a high place among scientific periodicals, and is likely, in French zoological literature, to take the position which Siebold and Kölliker's "Zeitschrift" takes in Germany. Professor Lacaze Duthiers, so well known for his thorough researches upon the Invertebrates of the Mediterranean, contributes an introduction to the first number, stating the aims of the publication, and concludes the number by an elaborate article on the auditive capsules of Gasteropoda. The other original article of this number is written by Mr. Perrier, who has given us an excellent paper on the Natural History of a fresh-water worm (Deto) allied to Nais. This periodical, as well as the other "Journal de Zoölogie," published under the auspices of Professor Gervais, both have notes and reviews on scientific works published in countries outside of France, a feature which thus far has received but little attention from French scientific journals. Holland, which already publishes so many scientific memoirs and periodicals of great excellence, adds a purely zoological archive to its list, edited by Professor Selenka. The first number contains a tolerably complete embryology of one of the naked Gasteropods by Selenka, and a long paper by C. K. Hoffman on the anatomy of Echinoderms; both these papers are excellently illustrated. Professor Selenka intends to issue his Niederländische Archiv für Zoölogie whenever sufficient material is at hand; he solicits articles either in German, French, or English.

Appearance of Colour in Fish kept in Alcohol.—Mr. Richard Bliss, writing in the "American Naturalist," April, states that a short time since while examining a number of alcoholic specimens of Cyprinoids from Ogden, Utah, collected by Mr. J. A. Allen last September for the Museum of Comparative Zoology, he noticed a species of *Richardsonius* distinguished by a bright vermilion spot on the abdomen. The size of the spot varied in different individuals: in some it was quite small, in others it extended from the base of the pectoral fin to the anal opening. Calling Mr. Allen's attention to this fact he stated, greatly to his surprise, that this colour was not present in the living fish when he caught them, but appeared after the fish had been in alcohol a short time. A dissection of one of these fishes showed Mr. Bliss that the colour was deposited in the areolar layer or derm, and was therefore a true pigmentary colour. The only explanation he can offer to account for this peculiar appearance of colour is this:—It is well known that during the breeding season fishes frequently take on the most brilliant colours, which disappear when that season is past. It is not therefore improbable that this colour may have been one, at least, of the colours assumed by the fish during the reproductive period, and that the alcohol served in some way to bring out the colour thus abnormally. Whatever may have been the cause, the fact that colour can so appear in fishes will serve as a caution to ichthyologists when describing species from alcoholic specimens alone, lest they confound abnormal or seasonal colours with those that are permanent.

Parthenogenesis among Lepidoptera.—It seems that the Dutch naturalist, M. H. Weizenbergh, jun., has performed a series of experiments on this interesting subject, the insect placed under observation being *Liparis dispar*, and concludes that it is possible for at least three successive generations to be produced without access of the male to the female. The following are the results of his very careful experiments:—(1) August 1866, eggs laid by impregnated females; April 1867, caterpillars appear, and in July perfect butterflies. (2) August 1867, eggs laid by females of this year are without impregnation; April 1868, caterpillars appear, and in July perfect butterflies. (3) August 1868, eggs laid by females of this year without impregnation; April 1869, caterpillars appear, and in July perfect butterflies. (4) August 1869, eggs laid by the females of this year without impregnation; April 1870, no results, the eggs all dried up. The power of reproduction appeared to decrease year by year when impregnation was prevented. Similar results have been noticed in other butterflies, in bees, and notably in aphides.

An Error of Mr. Darwin's.—The "American Naturalist" (May 1872) states, that in the last edition of his "Origin of Species," Mr. Darwin misstates Hyatt and Cope's law of acceleration and retardation in the following language: "There is another possible mode of transition, namely, through the acceleration and retardation of the period of reproduction. This view has lately been insisted on by Prof. Cope and others in the United States. It is now known that some animals are capable of reproduction at a very early age, before they have acquired their perfect characters," &c. A writer, who signs himself "Z," states, that Prof. Cope and others have not insisted on the above propositions, which he imagines to be supported by very few facts. Their theory of acceleration and retardation states, that, while the period of reproductive maturity arrives at nearly the same age or period of the year in most individuals of a single sex and species, the portion of the developmental scale which they traverse in that time may vary much. That an addition to the series of changes traversed by the parent would require, in another generation, a more rapid growth in respect to the series in question, which is *acceleration*. A falling short of accomplishing that completeness would result from a slower growth, hence the *process* is termed retardation. Vast numbers of observed facts prove that this is the great *law of variation*, towards which little progress has yet been made by students who are yet chiefly occupied with the co-operative law of natural selection.



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Hydroid Medusæ.

THE HYDROID MEDUSÆ.

By the REV. THOMAS HINCKS, B.A., F.R.S.

[PLATE LXXXVIII.]

THE title which I have given to this paper is not as strictly accurate as I could have desired, but it is the best that occurs to me. It is my purpose to sketch the history of those exquisite medusiform organisms, which, long regarded as independent animals, are now known to be the wandering reproductive buds of the hydroid zoophytes.* To style them Medusæ, however, is to run the risk of suggesting the idea of absolute individuality, and so concealing their true zoological significance. They are the floating flowers, so to speak, of the plant-like animal, detached from the parent structure at a certain point of their development, but still a mere term in a single life-series; elements of a perfect being, but not themselves complete existences, and only comprehensible when referred to the *whole* of which they form a part. The title must be accepted with this explanation.

Other medusan forms, identical in structure with those to which I have just referred, but which have not yet been traced to any fixed hydroid stock, also come within the scope of the present paper. These are very numerous, and offer a wide field of interesting research to the naturalist. It is remarkable that in the case of so many species the reproductive bodies only should be known, while the plant-like colonies that gave them birth elude our search. Of the forty or fifty forms described by Edward Forbes in his classical Monograph on the "British Naked-eye Medusæ," † but few have yet been traced to their origin. Of the North American species, so admirably described and figured by Alexander Agassiz, many of which are wonderfully beautiful, the same remark holds good. We know a large proportion of them merely as ocean-wanderers, scattering the seed of new generations, beautiful and ephemeral, like

* Allman has given them the expressive name of "planoblasts"—"wandering buds."

† Published in 1848.

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their analogue the flower; but, so far, can neither trace their lineage, nor follow the fortunes of their offspring. These unattached medusæ are mere fragments of a life-history; they are like a few exquisite lines from a lost poem, which make us long to recover the missing context.

Perhaps an apology is needed for isolating one element of the Hydroid, and treating of it apart from the individuality to which it belongs. But inasmuch as the medusiform zooid detaches itself altogether, at a certain stage, from the colony that has reared it, and thenceforth leads a perfectly independent and original life, with marked characteristics of its own; and further, as it is the representative of one grand department of the hydroid economy, it seems permissible, as it is certainly convenient, to make it the subject of a separate study. We do not quarrel with a paper on flowers, apart from the plants that bear them, nor do we resent it as a sin against scientific accuracy. Besides, as I have just mentioned, a large proportion of the hydroid medusæ are still without pedigree, and must be treated provisionally as isolated beings.

A hydroid colony (for associated life is the rule of the order) consists of at least two classes of zooids. They may not bóth be present at any given time, just as leaves and flowers are not always found together on the plant, but they are both essential elements of the perfect commonwealth. One class is charged with the alimentary, the other with the reproductive function. Both are evolved in the same way, as buds from the common substance of the zoophyte. The polypites or feeders of the colony are always permanently attached to it; the reproductive buds also often pass through their various stages *in situ*, and wither away, like the seed-vessel on its stalk, after the liberation of the ova. But in many cases they take on a more highly specialised form, and are equipped for a free and locomotive existence. Assuming a medusan guise they part from the sedentary colony, and exchange their vegetative ways for the customs of a vagrant life. The structural elements of the polypite are modified and readjusted in the free zooid; the tentacles, which had only served for prehensile purposes, are webbed so as to form a contractile float, and the zoophyte which in its alimentary phase is the most vegetative of animals, appears, in its higher reproductive phase, as a restless ocean-wanderer.

Let us take up the history of the so-called medusa, the "swimming polypite," the reproductive member of the hydroid colony, at the point when it is about to cast itself loose, and enter upon its proper work of maturing and diffusing the winged embryos that are to perpetuate the species. The sexual buds of the zoophyte are borne in various positions; sometimes they originate on a special offshoot (a *quasi* flower-stalk), which is now naked, and now protected by a transparent urn; sometimes they spring singly or in clusters from the body of the polypite; sometimes they pullulate from a portion of the common substance which links together and binds into one organism the many zooids composing a colony.

Let us examine a cluster of them, hanging from the body of one of the naked polypites. Commonly each bud is inclosed in a delicate capsule, which protects but only half conceals the exquisite structure that is ripening into perfect form and "tender grace" within it. In some cases, however, this envelope is wanting, and the bud is freely exposed through the course of its evolution to the surrounding water. In each cluster we find buds in all stages of development; but one is always much in advance of the rest, and attains maturity while some of its companions are merely rudimentary. And here let me pause for a moment to celebrate the beauty of the group of animal flower-buds, already showing traces of the vivid colouring that adorns the adult medusa; some closely folded up, and giving scarce a hint of the marvellous form that is being moulded within, some heaving with the pulsations of the imprisoned zooid, while one, it may be, has opened, and the full-blown medusa is visible hanging from its slender pedicle, and struggling with the bond that still detains it.

Through the investing capsule the general form and leading features of the contained zooid may be dimly traced; but so tightly is it packed in its little case, and so much are the parts compressed and thrown together, that it is difficult to realize its appearance until it emerges from its captivity. In the later stages of development, the contractile movements that are so characteristic of the tribe become frequent and vigorous, and at length the capsule is ruptured, and the medusa unfolds itself; the tentacles, which had been stowed away within the swimming-bell, are cast forth, the bell itself assumes its true proportions, and it hangs as if on the point of starting into free life. But it is still attached, and arduous and often protracted struggles must precede its final liberation.

Up to this point that portion of the bud which is to form the digestive sac of the medusa has been in direct connection with the common cavity of the zoophyte, and has received from it constant supplies of nutriment. But the communication is now to cease, and all the energies of the zooid are engaged for the time in severing the link that binds it to the common life of the colony. (*Vide* Plate LXXXVIII., fig. 3; *a* marks the point at which the medusa was attached, and in communication with the general chymiferous cavity.) The struggle for freedom is often long, and, to the watching naturalist, wearisome enough; the bell contracts and expands rapidly, jerk succeeds jerk, tug follows tug, these spasmodic efforts alternating with long periods of quiescence, until at last the connecting link suddenly gives way, and the medusa drops into the water.

Before we finally dismiss it from the colony, and while it still continues an integral portion of it, let us pause to consider the somewhat perplexing question of individuality with reference to these composite organisms, in which many zooids, similar and dissimilar, continuous and discontinuous, are the product of a single ovum.

Philosophically regarded, *the whole series of forms* evolved by budding, and intervening between two generative acts is, no doubt, the equivalent of the "individual" in other classes; and this is the case, even if some of these zooidal forms detach themselves, and lead an independent existence. In this sense the polypite and the medusa are as undoubtedly not "individuals." Yet it must be accounted unfortunate that this term should have been applied to the zoological conception. It would be better, surely, to speak of the *life-series* of the zoophyte, of which the various zooids are so many units, with the understanding that this corresponds with the "individual" of other tribes, than to perplex the ordinary mind by asserting that the free and independent medusa is *not* an individual at all, but that a hundred polypites and a company of a hundred medusæ together constitute an individual! The medusa is not the immediate and single product of an egg. It is developed as a bud from another structure, which *is* the immediate product of the ovum. It is not, therefore, an "individual," biologically considered. But in the ordinary sense of the words, as Professor Allman has remarked,* "every zooid has an individuality of its own," which it is important to recognize. The medusa, with its original and distinctive manner of life, and its independent ways, has a very marked individuality of its own, which I shall endeavour to exhibit in the following pages.

To resume the history, the liberated medusa after a brief period of quiescence begins to move rapidly through the water, propelling itself by the alternate contraction and expansion of the gelatinous disc or swimming-bell, which constitutes its most striking feature. It certainly presents a remarkable contrast to the sedentary kindred from which it has lately parted company. Its whole organisation fits it for active locomotion, the polypite is rooted to one spot for life; is a restless floater, ranging widely through the waters of the sea, the latter is a fixture: it pursues its prey, the latter waits for it; it is mercurial, the latter vegetative; and yet after all it is but a

* In his great work on the "Tubularian Hydroids," 1871.

polypite disguised in a dress which is suitable to the needs of a free existence. The general plan of structure will be best understood by a reference to the plate. It is of this kind. A delicate, more or less transparent disc or bell, serves as a float to which the various organs are attached; it is eminently contractile, and by a regular systole and diastole propels itself through the water. In figure it is variable, but always bounded by lines of beauty; sometimes it is almost globular, sometimes hemispherical, sometimes shaped like a watch-glass, and as translucent, sometimes of more fantastic form. Commonly it is of the slightest, filmiest material, often colourless, and so crystalline as to be hardly visible; as often tinted with the most delicate hues, which only the flower can match; frail as the bubble, and brilliant as the bubble when touched by the sunlight. Within the cavity of the bell, and suspended from its summit (Plate LXXXVIII., fig. 7, *m*), hangs the *digestive sac*, terminating below in a *mouth*, and at its upper extremity opening into a number of delicate *canals* (Plate LXXXVIII., fig. 7, *c*, *c*), which traverse the walls of the swimming-bell longitudinally, and empty themselves into a *circular vessel* running round its margin. We have here the simple nutritive system, which corresponds essentially with that of the fixed and plant-like elements of the hydroid colony. The digested and diluted food is forced from the pendent stomach into the radiating canals, and conveyed by them and the circular vessel that unites them throughout the organism, the stream flowing back periodically to the central depôt for fresh supplies. The number of the canals varies in different species; commonly it does not exceed four (Plate LXXXVIII., figs. 3, 4, 6,), but six (fig. 3), eight, ten (fig. 7), and twelve are met with, and in some cases as many as a hundred (fig. 1). The vessels are generally simple, but occasionally they bifurcate or are slightly branched, and in one rare instance give off short, lateral diverticula; never exhibiting, however, the remarkable complexity which occurs in the parallel group of the *Discophores*.* From the free margin of the bell hang a variable number of tentacles, some of which are a direct continuation of the radiating canals (Plate LXXXVIII., fig. 3); these which are first developed and always present, may be regarded as the primaries, and the spaces between them are often occupied by large numbers of secondary tentacular appendages (figs. 1, 2,), forming a beautiful fringe of delicate interlacing threads. The tentacle is of course a prehensile organ and instrument of offence; it is usually covered

* Vide a paper by the author in the POPULAR SCIENCE REVIEW for April 1871, in which the points of agreement and contrast between the two groups of the *Hydroida* and *Discophora* are fully presented.

with prominent groups of poison-bearing thread-cells, which give it a pretty beaded appearance and a deadly touch (fig. 7, *t*). When the medusa swims, the arms are coiled in a spiral, and are borne "tightly twisted like a corkscrew;" when it is at rest they hang passively pendent, or are cast out to several times the length of the bell, and float in undulating lines through the water. The opening of the bell below is closed by a membranous film (the *velum*), with a circular orifice in the centre, through which the water finds access to the interior. This *veil* is a continuation of the muscular layer which lines the entire cavity of the swimming-bell, and endows it with its remarkable contractility.

There only remain to be noticed the simple organs of sense with which the locomotive zooids of the Hydroid are generally furnished, but which of course are altogether wanting amongst the fixed and vegetative members of the colony. These are of two kinds: the first consists of a collection of pigment-cells, forming a coloured spot or ocellus, and inclosed by a delicate membrane (figs. 7, 8). In some cases a crystalline body, a refracting lens, is embedded in the pigment-mass, and we are naturally led to regard the whole structure as an eye. Even when the lens-like body is wanting, the elements that remain may possibly constitute a light-perceiving organ of the simplest and most rudimentary kind. The ocelli, which are often dark-red or black, are borne on the bulbous enlargements from which the tentacles spring (fig. 3). The remaining organs of sense that occur on the medusa are certain minute sacs developed on the margin of the swimming-bell, in each of which one or more refractile spherules are inclosed (fig. 2; in this species they are very numerous). They have been regarded as organs of hearing, from their supposed resemblance to the auditory sacs that occur in other classes; but there seems to be no real analogy between the two, and accordingly some of the ablest observers assign them a visual function. It may be impossible to determine the precise significance of these primitive structures; but we shall hardly err in viewing them as sense-organs of the simplest type correlated with the habits and needs of a free and active existence. Though almost universally present, there are a few known cases in which they seem to be wanting. The medusa represented in our fig. 3—a remarkable form, which I have lately obtained, and which is still undescribed—is destitute, at least in its earliest stage, of both ocelli and marginal sacs.*

The nervous system of the medusa, if such should exist in any specialised form, has certainly not been detected in the vast majority of known Hydroids; and though such a system

* So far as is known at present, the two sense-organs (the ocellus and the marginal sac) never exist together on the same medusa.

has been described as occurring in a few cases, we are justified, I think, in waiting for further testimony before coming to any positive conclusion on the subject. As sense-organs make their appearance along with a free existence, we should naturally look for the dawn of the nervous system at the same point.

So much may suffice as to the general plan on which the Hydroid medusa is organized. I wish to avoid unnecessary detail, and to fix attention on the cardinal points of structure, and especially to emphasize what may be called the artistic and æsthetic aspects of my subject. No technical description, however minute and accurate, can enable us to realize the distinctive beauty of these fragile beings. Not merely are the forms graceful, and the curves faultless, and the colours vivid, but an additional charm is imparted to all of them by the delicate and transparent material in which they are presented.

Perhaps the portrait of an individual carefully executed may give the best idea of the kind of beauty that belongs to these "wandering buds," and may serve to justify the enthusiasm of the naturalist about them. I will select as my "sitter" a medusa, certainly one of the most lovely of its kind, which made its appearance for two successive years in a friend's tank, but which we were never able to trace to the parent stock (Plate LXXXVIII., fig. 2). No doubt in some chink or cranny a charming little colony of polypites was concealed, which supplied the annual medusan brood, but it baffled our keenest search. I am not aware that the pedigree of the form which I am about to describe has ever been made out; it is probably one of the unattached.* Imagine, then, a minute crystal globe, the surface of which is thickly dotted over with thread-cells, making it appear as if delicately frosted; from the free margin hangs a graceful fringe, composed of twenty beaded tentacles, springing from as many bosses of a brilliant green with metallic lustre, which girdle it as with a belt of emeralds; amongst them are placed a number of the supposed eyes, each with its refractile corpuscle; the pendulous sac in the centre of the dome is dyed with the richest carmine, and tipped with the purest white, while the ovaries on the course of the radiating canals, shower masses of pink through the walls of its translucent bell. The colours are wonderfully brilliant, and the green tentacular bulbs almost glitter like gems. It was the prettiest sight to watch this little hydroid, with its painted and jewelled disc, now darting through the water with contracted arms; now sinking slowly, like a balloon, with some of its tentacles extended laterally as if to regulate its descent, and some hanging

* It is, no doubt, the reproductive zooid of a Campanularian Hydroid, and may belong to the Genus *Campanulina*, or a near ally.

below it; now anchored to the bottom of the vessel and casting out its lines in search of food. When engaged in fishing, it attached itself by means of a few of its tentacles, while the rest were thrown out in all directions and to amazing distances, and appeared as the finest and most attenuated threads. The extremities at such times exhibited a constant tremulous movement, as if they might be feeling about for the minute organisms on which the medusa feeds. Then it would suddenly gather up its fishing-lines, and rise by a series of rapid jerks to the surface of the water. This exquisite species, as I have stated, has not yet been referred to a fixed stock; it is one of a large company, which remains to stimulate and reward the researches of the zoologist. These unattached medusæ are single chapters of as many charming biographies still unread; they suggest to us the existence of a host of graceful forms, which, when discovered, will fill many a gap in our systems, and add many an interesting page to Hydroid history.

The medusa, at the time of liberation, is in most cases far from having attained its perfect form. Remarkable changes subsequently take place in a large proportion of the kinds, which so completely alter its aspect that it might pass, and has often passed, in its adult state for a different species from its young self. Indeed several species have been formed out of the various stages of one and the same hydroid. The changes affect the size and form of the swimming-bell, and the *number* of many of the other organs. In fig. 5 we have the early, and in fig. 6 the adult state, of the same medusa. The deep bell of the one gives place to a flattened and expanded disc in the other. The two tentacles present at birth multiply into about forty, and there is a corresponding increase in the number of the marginal organs of sense. In some cases (Plate LXXXVIII., fig. 1), the change is carried still further; the radiating canals, as well as the tentacles, multiply, so that the *four* with which the young is (probably) furnished, are represented by sixty or eighty in the adult.

One of the largest of the hydroid medusæ (*Zygodactlya*), closely related to the form represented in fig. 1, which measures some fifteen inches in diameter and is furnished with 100 radiating vessels, is not larger than the head of a pin in its early condition, has only four canals, and is moreover developed from one of the smallest of polypites. Growth must proceed rapidly in these ephemeral beings, which run their whole course in a single season; they are like the annuals of the vegetable world, which pass through the entire cycle of their existence and perish in a summer. It must be noted that these changes are merely illustrations of the vegetative tendency to a repetition of parts, which is characteristic of the zoophyte, and is so strikingly manifested in the complex ramification and the many polypites of its plant-like colonies.

Besides the changes that have been enumerated, others occur in the digestive sac, which sometimes attains an extraordinary development in the adult, and can be extended far beyond the opening of the swimming-bell (Plate LXXXVIII., fig 4). In one genus (*Syncoryne*) it stretches out to two or three times the length of the body, and is capable of the most active movement. This remarkable development of the stomach is accompanied by a voracious appetite.

It is of this form that Edward Forbes writes: "An animal that can pout out its mouth twice the length of its body, and stretch its stomach to corresponding dimensions, must indeed be 'a Triton among the minnows,' and a very terrific one too. Yet is this ferocious creature one of the most delicate and graceful of the inhabitants of the ocean—a very model of tenderness and elegance."

Reproduction by budding is a familiar fact in the history of the zoophyte, and it enters into that of the free as well as of the fixed element. Some of the medusæ produce buds which assume the form of the parent, and probably repeat its life. They are borne in various positions; in some cases springing from the bulbous bases of the tentacles, or from the tentacle itself; in others from the base of the digestive sac; and in one instance, at least, from the margin of the bell between the tentacles. And these buds may themselves produce other buds, so that two generations may hang from the body of the primary zooid before its separation from the parent stock. Development takes place rapidly; three or four days suffice for the completion of a brood, and before it is mature it is itself budding. We do not know the limits of this process, but the number of generations originated by a single medusa is probably very great. With these facts before us, we cease to marvel at the myriads of these organisms that swim in certain seasons near the surface of the sea. Indeed, apart from this reproduction by budding, which has only been noticed in certain species (though probably it is commoner than we suppose), there is ample provision for an abundant supply of these "floating nurseries." They are cast off in incalculable numbers from the hydroid colonies. In some species each polypite bears a brood; in others each shoot is laden throughout with graceful urns, in which not a single medusa, but a whole tribe, is nurtured. Probably there may be more than one brood in a season. Then consider the immense acreage of the zoophyte crop on the shore and the sea-bed, the forests that overspread the algæ, the populations that hide in well-nigh every chink and cranny, and plant themselves on almost every spot where they can get a footing, and you will understand how the myriads of medusæ cover the surface of the sea beneath the summer sun, and make it glow through the summer night.

Alexander Agassiz tells us that one of the budding kinds, appropriately named "*fulgurans*" ("light-flasher"), is sometimes so abundant off the American coasts, that the sea, when disturbed, is brilliantly lighted by its blue phosphorescence. And of another species he says that he had often found the whole surface of the water, for several miles in the Gulf of Georgia, thickly covered with it.

To return to our budding, the case which we have been considering is simple enough, and in harmony with the whole course of hydroid history. But the facts now to be noticed are anomalous and difficult to interpret. A medusa has been observed to bud off young within the cavity of the stomach, and these, when developed, have taken on a form *which is totally unlike the parent*; which belongs, indeed, to the medusa of another, and very different Family. In other words, two distinct types of medusa originate one from the other and form parts of one and the same life-series. These observations, if correct, are certainly startling; we are scarcely, I think, in a position at present to determine their true significance. It would be satisfactory to have them confirmed, eminent as the authorities are to whom we owe them, and to know more than we now do both of the earliest and of the later stages of the remarkable life-history. If it should prove to be a fact that one hydroid medusa may originate another by gemmation of an entirely different generic type, we must materially enlarge our conception of the polymorphism of the zoophyte, and modify our systematic views. So remarkable a divergence from the direct line of development would open the way for much curious speculation. The observations of Haeckel and others on this point are deeply interesting, and indicate to the zoologist a most fruitful field of research.

I have spoken of the swimming-bell as the characteristic feature of the medusa; but there exists a small group of forms in which it is suppressed, and its place is filled by a totally different locomotive organ. In *Clavatella* (Plate LXXXVIII., fig. 8) the contractile float is wanting, and we have an ambulatory medusa, which moves about leisurely on suctorial feet, or climbs by their aid among the algæ. In this remarkable creature, the general form of the medusa is preserved, the radiating canals are traceable in the walls of the hemispherical body, the ocelli are conspicuous at the base of the tentacles, the portion of the central sac bearing the mouth hangs below the disc; but the beautiful contractile float is missing. In its absence, however, a new compensative element makes its appearance; the tentacle gives off a branch, which terminates in a sucker, and by means of these suctorial appendages the *Clavatella* traverses the small rock-pools in which it finds a home, or mounting the tufts of sea-weed, seeks its food among their branches. A change in habit, mode of life, and range of distribution, accompanies the modification of structure, and the *Clavatella* appears to be separated from its kindred by a much wider gap than really exists. We have an interesting transitional form in *Cladonema* (fig. 7); in this case the bell is fully developed, and the creature is an active swimmer, but the tentacles (fig. 7 *t*) are furnished with the sucker-bearing branch (fig. 7 *s*), and it thus enjoys the means of rapid locomotion and attachment at pleasure, in combination. It does not appear to use its suckers as feet. In its adult state the tentacles of *Cladonema* bear several branches, and at least two of the suctorial appendages; but, as I have lately had the opportunity of observing, in an early stage they are as simple as those of *Clavatella*, from which they only differ in the presence of a larger number of the groups of thread-cells (vide fig. 7 *t*). It should be remarked in passing that the free zooid of *Clavatella*, though of medusan type, makes a near approach to the polypite, and is an interesting link between the two principal elements of the Hydroid colony.

Besides the ambulatory medusa, only one other deviation from the normal condition is known; it occurs in the locomotive sexual bud of the genus *Dicoryne* (fig. 9). In this extraordinary form the medusan structure seems to have vanished altogether. It consists of a closed sac covered with cilia within which the generative elements are developed, and bearing at one extremity two ciliated tentacles. It is clearly an intermediate form between the *fixed* buds, which in many hydroids discharge the generative function, and the ordinary medusa. In brief it is the equivalent of the central sac (fig. 7, *m*) of the medusa, minus a swimming-bell and its appendages, made locomotive by the aid of cilia; and the two tentacles may be regarded as rudiments of the contractile disc, the development of which has been, as it were, arrested *in limine*.

But I must pass on to notice briefly the most important phase of the life of the medusa. It is its specific function to give origin, directly or indirectly, to the generative elements, and with the scattering of the seed its work is accomplished. In one section of the Hydroid medusæ, the ova and spermatozoa are produced in the walls of the digestive cavity (fig. 7, *m*); in another, they originate in special sacs, developed on the course of the radiating canals (fig. 2, *o*, *o*). These sacs have long been regarded as the *ovaries* (or spermaries); but Allman has shown that in many cases at least, their structure is identical with that of the sexual buds, which continue attached to the colony, and mature their products *in situ.* He therefore views them as definite zooids, co-ordinate with the polypite and the medusa, and not as mere organs. In such cases the medusa is a mere carrier and nurse of the sexual buds, which form a separate term in the complex life-series of the species. But leaving these transcendental matters, whether in ovaries or zooidal sacs, the medusa originates the seed of new generations; and with the escape of the embryos, and their dispersion through the waters of the sea, its existence probably terminates.

The Hydroid embryo (or *planula*) (fig. 10) is an elongated, cylindrical body, thickly clothed with cilia, which after a short term of free life, fixes itself by one extremity, loses its locomotive organs, and is moulded gradually into a polypite—the first term of a new series, from which by repeated buddings the complex, arborescent colonies, and the graceful medusan forms, will be evolved in due succession.

Towards autumn chiefly the medusæ seem to congregate for the purpose of spawning, and on warm, still days, certain species swarm in immense shoals near the surface. Their fragile forms are ill-suited to face the tumult of the waters, and large numbers of them are found floating dead after storms. The smaller kinds sometimes fall victims to rain-water; and Agassiz has noticed that their total disappearance from the neighbourhood of the wharves, about which they congregate, "uniformly coincides with heavy rainfalls, while the larger species survive." Some are so frail as to be unable to resist the action of the light and heat near the surface, and consequently swim at a considerable depth below it. There seem to be diversities of temperament amongst them, but most of them are vivacious in their habits, moving about "with the greatest freedom and energy;" and it must be added that, with all their grace and tenderness, they are indubitably voracious!

The phosphorescence, for which they have long been celebrated, is confined to certain species; and in them it is localized in certain portions of the structure. The painted bulbs of the tentacles sometimes glow with vivid lights, under the stimulus of irritation, without which the fires refuse to kindle; and the margin of the crystal bell is girt with a wreath of brilliants. The phosphorescence also connects itself specially with the reproductive system, and sometimes the central pendant is all ablaze, and thousands of little lamps illuminate the dark waters around. Some species emit flashes of coloured light.

The Hydroid medusæ, like a host of other marine floaters, can only be obtained in their adult state by the use of the tow-net. On calm warm days in summer and autumn—*dreamy* days are the best—a bag of fine muslin fastened to a stout ring of wire, and towed at the stern of the boat, will gather a rich harvest of beautiful forms. The student will have a fair chance of obtaining some specimens in which the ova are far advanced towards maturity, and may possibly be fortunate enough to keep them until the embryos are discharged. If he can rear these into polypites he may complete an imperfect life-history, and fill up one of the many gaps in our knowledge of the tribe. This has been done in a few cases; by care and patience it might no doubt be done in more.

And even those who are not prepared for any profound study of the structure and history of the medusæ may find the tow-net a not uninteresting resource during their visits to the sea; and if they have any sensibility to beauty in their souls, can hardly fail to derive the highest gratification from watching the exquisite forms and movements, and the singular habits of these "animated bubbles."

DESCRIPTION OF PLATE LXXXVIII.

Fig. 1. *Crematostoma flava*, A. Agassiz.
„ 2. A Campanularian medusa, not yet described (?).
„ 3. The free sexual zooid of *Lar Sabellarum*, Gosse.
„ 4. *Tima formosa*, Agassiz.
„ 5. *Oceania languida*, A. Agassiz (the young). Probably the gonozooid of a *Campanulina*.
„ 6. The same, in the adult state.
„ 7. *Cladonema radiatum*, Dujardin. The medusa in an early stage of its development.
„ 8. *Clavatella prolifera*, Hincks. The ambulatory zooid.
„ 9. *Dicoryne conferta*, Alder. The female natatory zooid; an ovum, with its germinal vesicle and spot, is visible within the cavity of the body.
„ 10. The Hydroid *planula* or embryo.

[Figs. 1, 4, 5, and 6, are after Alexander Agassiz; fig. 9, after Allman; and the rest from drawings by the Author.]

THE FIRST CHAPTER OF THE GEOLOGICAL RECORD.

By DAVID FORBES, F.R.S., &c.

THE term Geology, which signifies the science of the earth, being derived from the two Greek words, Γῆ, "the earth," and Λόγος, "a word or argument," has been variously interpreted by different writers on the subject.* During the last generation, geology, as a science, was studied altogether from a purely mineralogical and physical point of view; an interpretation which became completely reversed when the introduction of paleontology, called in as an aid to its study, so absorbed the attention of the majority of geologists, to the exclusion of almost all other branches of the science, that most of the later works on geology, especially here in England, may be regarded rather as histories of the development of life upon our globe than treatises on its geology in its more extended sense.

A perusal of most, even of our best-known manuals of geology, will show that their contents are almost entirely devoted to the fossiliferous strata, commencing their descriptions either with the most recent formations, and proceeding backwards until they stop at those more ancient ones, in which only traces of organic remains have as yet been discovered; or *vice versâ*, beginning with the lower Silurian or Cambrian rocks or in later years (since the discovery of that most perplexing organism the Eozoon Canadense) with the Laurentian formation, and treating the others in ascending order up to the present time: a system, which in either case makes the student feel the evident want of a beginning or first chapter in the geological record, whilst at the same time it imposes, as it were, a dictatorial boundary to his field of research in a similar manner to what it would be, if he was told, when studying

* Including the strangely inappropriate application of the term by M. Meunier, who writes of the "*Geology* of the Heavens!" and has lately published a work entitled "Le Ciel géologique." Paris, 1871.

ethnology or the history of mankind, to ignore everything connected with the subject before printed records existed, or as if an astronomer was advised to discard all discoveries of which he had not *tangible* evidence as to their correctness. Just, however, as the recent advances of the collateral sciences have cleared up so many difficulties, and have added so much to our knowledge of prehistoric times, and the condition of the human race in earliest periods, or of parts of the cosmical system, which the astronomer of old could never even have imagined to be within man's power of investigation; so it is to be expected, with the aid of our daily improving information and appliances, that proportionate advances may also be made in our knowledge of what may be termed the prozoic history of the earth; that is to say, of the different stages through which our globe has passed before it became fitted for the habitation of organisms even so low in the scale of life as are met with in the previously mentioned formations, which modern geologists appear so often to regard as the very *ultima thule* of their investigations.

On the present occasion it is proposed to make an attempt to sketch out such an introductory chapter in geology as is here referred to, premising, however, that from its very nature it cannot be other than in the highest degree theoretical, and must be regarded only as an essay, in which the more recent discoveries in physical and chemical science are appealed to in elucidation of a subject which, without their aid, would be all but unapproachable; and this is here brought forward in the belief that attempts made from time to time, to generalise and put into shape the somewhat disconnected facts and observations relating to this subject, cannot but do good, notwithstanding that it must at the same time be self-evident that the views herein expressed will require to be modified from time to time, according as the progress of scientific investigation furnishes more reliable and extended data for generalising upon than are in our possession at the present moment.

As is well known, even the most ancient philosophers entertained the opinion that our globe had not always been what it was in their age; that it had passed through varied phases, and that it once upon a time had even had a commencement to its present career. Later on, when astronomy came to be studied as a science, astronomers went still further, and reasoned from a consideration of the earth's form, &c., that it must at a remote period have been in a fluid, or at least plastic condition; a result which the subsequent observations on the temperature of the earth in depth and the products of volcanic action confirmed, and led to the conclusion that our globe must once have been a sphere of molten matter, which had solidified on its exterior, owing to the cooling action of the surrounding air. The celebrated Laplace went still further, and, from a consideration of Herschel's researches on nebulæ, propounded his so-called nebulous theory of the earth's origin, according to which our sphere owed its existence to the aggregation and condensation of nebulous matter. The state of the natural sciences of the period was not, however, sufficiently advanced to furnish means by which this theory of Laplace could be either confirmed or disproved, so that it was long looked upon as a visionary hypothesis which was never even imagined as likely to be so far confirmed by future discoveries in science, as to become at this moment the most plausible explanation of a beginning of our world which has as yet been put forward.

This being the case, our chapter of genesis commences by assuming the nebulous theory of the origin of our globe as the starting-point; and the first stage in the history of the earth is consequently the act of aggregating or segregating the nebulous matter in space or, in other words, of gathering together in a gasiform condition the chemical elements of which the earth, with its surrounding atmosphere, is actually composed of.

The consequence of the coming together of these elements would, as chemistry teaches us, result in their reacting upon one another with intense energy, giving rise to the development of both light and heat, and forming numerous chemical combinations, the nature of which would be dependent upon the mutual affinities of the elements themselves, and the relative proportions in which they were respectively present in this admixture of gases and vapours. The more simple or binary compounds would naturally be formed first, such as the oxides, sulphides, chlorides, &c.; but these in turn would combine *inter se* producing salts and other compounds, amongst which the silicates played a very prominent part.

The final result of this great display of chemical energy would be to change entirely the nature and appearance of the original nebulous gathering of gasiform matter, for as soon as the chemical action had come to an end, by far the largest proportion of the newly-formed substances would no longer be able to retain the gasiform condition at the lower temperature which then ruled, and would be condensed into fluids, when the whole would assume the shape of a sphere of molten matter surrounded by an intensely heated atmosphere of such of the other compounds and free elements as could still remain volatile at this temperature.

This period might be termed the second stage in the history of the earth, and if examined into more closely, it would be found that neither the molten sphere nor the atmosphere surrounding it was of uniform character throughout; but owing to both of them being made up of a number of dissimilar substances, the first impulse of the newly-formed compounds would be to obey the laws of gravity by arranging themselves more or less completely in strata, or more correctly speaking, zones, in accordance with their respective densities, and the study of the composition of the rocks now forming the earth's exterior, and of those brought up from its depths by volcanic forces, along with that of the relative specific gravities of the parts accessible to our observation as compared to the density of the earth's mass as a whole, leads to the deduction that the molten sphere might at this period of its history be regarded as composed of some three great zones (probably with subzones), having the following general mineralogical characters:—

1st. An exterior of molten rock of comparatively little density which consisted of silicates, in which an excess of silica was to a great extent combined with alumina and alcali, but containing very little of the other bases, such as lime, magnesia, oxide of iron, &c.

2nd. A middle zone, also of molten rock (silicates), considerably heavier than the former, and in which the silica, present in minimum proportion, existed in combination with a large amount of the bases, lime, magnesia, oxide of iron, and alumina, with but comparatively little potash, and,

3rd. A central nucleus of very much greater density and of metallic nature, the outer part consisting of compounds of the heavy metals with sulphur, arsenic, &c., whilst in the centre the metals themselves are probably in a free state, or as alloys.

The constitution of the sphere of molten matter as thus arranged would now present a general character of stability maintained even after its solidification, due to the loss of heat radiated from its surface, and the cooling action of the external air, had commenced. In the atmosphere, however, the arrangement of the gases and vapour in zones would be much less permanent, as by degrees the zones would be more or less broken up by the tendency which gases have to diffuse themselves throughout one another, as well as the condensation in succession of the different vapours contained in it, in proportion as the temperature of the whole became more and more lowered. In the first instance, however, that stratum of the atmosphere next to the earth would be composed of dense vapours of such compounds as are only volatile at very high temperatures, amongst which several of the chlorides, and especially the chloride of sodium or common salt would be most prominent; above this a great zone of carbonic acid gas would prevail, then one of nitrogen with possibly the admixture of some oxygen and above this again the vapour of water in enormous quantity.

The third stage in the history of the earth may now be said to have commenced, when the earth as a molten sphere surrounded by a furnace-like atmosphere began to cool down owing to the loss of heat radiated from its exterior into space; by degrees a thin crust would commence to form on the surface of the molten rock which soon consolidated and extended over the exterior of the entire sphere, becoming thicker and thicker over the nucleus of molten matter until it offered more and more resistance to the passage of heat from within outwards, and thus caused the rate of further cooling to diminish greatly, and the more so from its being composed of a highly nonconducting material. In time, therefore, the external surface of the earth would come to be barely red hot, and as soon as this was the case we should find it become coated with a layer or incrustation of the chlorides and other vapours hitherto held in suspension in the heated atmosphere, but which now owing to the lowering of temperature would be condensed and precipitated on to the now consolidated crust of the earth. From the amount of the salts contained in the ocean and known deposits, it has been estimated that the quantity of common salt alone would be sufficient to cover the entire sphere with a layer some ten feet in thickness.

As the process of cooling went on, as soon as the temperature of the atmosphere had become so lowered as to be below that of the boiling point of water, the enormous amount of steam hitherto pervading its uppermost regions would naturally become condensed into water, and at once fall down from the heavens as a deluge of hot rain upon the saline crust covering the sphere which it would instantly dissolve, forming the ocean which would thus be salt from the very first appearance of water upon the face of the globe.

The atmosphere now freed from the vapours previously diffused throughout it, would still be very different from what it now is, as although it might contain precisely the same gases, these would, however, be present in vastly different proportions; it would mainly be composed of an admixture of nitrogen and carbonic acid gas, free oxygen if present at all being but in very small amount, for it must be remembered that the total amount of nitrogen and carbon contained in the entire animal, vegetable, and mineral kingdoms of the future, were at this moment held suspended in the atmosphere in the gaseous form.

This state of things brings us down to the fourth or last stage of the prozoic history of the earth since it required but a comparatively short period to lower its temperature sufficiently to enable many of the lower organisms to exist upon it or rather it should be said, in its ocean, for as yet we only recognise the earth as a sphere externally coated, as it were, with a uniform sheet of water. Certain changes have, however, been going on in the solid crust of the earth which here demand our consideration since they tended to completely revolutionise its external features as well as prepared the way for its future career. In the first place as soon as the stony crust had completely consolidated over the molten nucleus within, it would present itself with a comparatively uniform and smooth surface externally, when, however, this crust increased in thickness and became colder, contraction would take place in its mass, which would result in the production of cracks and fissures in the crust itself, the sides of which, becoming dislocated, would bring about elevations and escarpments to interrupt the previously regular contour of the sphere, whilst by the subsidence of portions, some of the still fluid rock below would be forced up along the lines of such fissures and so form dykes of eruptive rock traversing the original crust. All these effects would, however, be immensely augmented, when the exterior had so far cooled down as to be covered with the ocean, since then, owing to such fissures allowing the water to penetrate down to the molten mass within, internal forces would be called into play, giving rise to volcanic phenomena which would result in the elevation of mountains, and the upheaval of islands and continents, thus forming the first dry land on the surface of the earth.

Instead of the previous uniform sphere we should now have its surface varied by elevations and depressions, valleys and mountain ranges, which by giving direction to the movements of the salt water in the ocean, and the fresh water from the heavens, by which the lakes and rivers are supplied, would at last set in action those external or secondary forces which have played so important a part in modifying the outward configuration of our globe and the distribution of organic life over its surface.

The primitive crust of the earth thus ruptured, along with the mineral matter ejected from below as before described, would now become still further broken up and pulverised by the mechanical force of water, powerfully assisted by the decomposing action of the great excess of carbonic acid gas present in the atmosphere of this period, and would in process of time become so comminuted as to allow of the particles being carried off and sorted by the action of rivers and the sea, which would deposit them as sedimentary beds of varying character. It should however be remembered that the exact mineral composition of the original crust of the earth must ever remain an open question, for when we take into consideration the great changes it must have experienced during countless ages, and the vast amount of "débris" directly or indirectly the result of its wear and tear, which has been scattered all over the globe, we can have no reason to expect to meet with any portion of it *in situ* in any part of the world; as, however, quartz is found to be the most preponderating of all the minerals composing the most ancient rocks, this fact confirms the view that the original crust must have been extremely rich in silica, much of which no doubt would separate out from the other constituents in the form of quartz during the act of solidification.

From this time up to the present age all the various changes, whether of mechanical or chemical origin, which have taken place in our globe have been brought about by agencies identical with those which we now see in operation, although possibly on a somewhat different scale; stratified rocks became formed from the wear and tear of the primitive crust by aqueous action, precisely as at the present moment we see them reconstructed from the "débris" of preexisting rocks of all kinds; the quartzites of the older rocks were formed from the comminuted quartz out of the primitive crust, just as the later sandstones and grits, whilst the associated silicates owing to the action of carbonic acid and water would be more or less decomposed, thereby producing beds of clay and others of areno-argillaceous character, whilst the largest proportion of the alkalies contained in them, would in the state of carbonates be carried off in solution by the water to the ocean, where they would react upon and decompose any chlorides or other salts of the metal, or earths which they might encounter.

Whilst all these changes were in progress, outbursts of fluid mineral matter from the still molten interior of the earth would from time to time continue to break through and disturb the primitive crust, and the rock strata in course of construction above it, exactly as we at present see similar eruptions from volcanic centres, and as many of these would then as now take place at parts of the crust covered by the ocean, they would result in production of vast volumes of submarine tufas and breccias which by the action of the waves would at once assume the form of ordinary stratified formations.

With the exception, however, of some minor occurrences of calc-tufas and precipitated carbonate of lime, no calcareous or limestone beds were deposited during this early period, nor were carbonaceous beds of any kind in course of formation, for the simple reason that both these classes of deposits owe their origin to the action of animal and vegetable organisms.

The atmosphere of this stage in the earth's history was, however, vastly different from what it is at present: instead of being as now composed mainly of oxygen and nitrogen, along with a small admixture of carbonic acid gas, it, on the contrary contained so overwhelming an amount of carbonic acid and nitrogen gases with only a minute proportion of oxygen (if any), as to be totally unfitted for the respiration of air-breathing animals, for which reason we find the first development of life of our globe represented by submarine organisms of the lowest type, and these followed by a great development of vegetation, which by absorbing the carbonic acid gas, and decomposing it so as to assimilate the carbon contained in it for the benefit of future generations, whilst at the same time the oxygen was returned to the air, so purified the atmosphere as to render possible the existence of still higher types of animal life on the surface of our globe.

ELECTRICAL SIGNALLING AND THE SIPHON RECORDER.

By J. MUNRO, Assistant to Sir W. Thomson.

PERHAPS most, if not all, animals have, in some degree or other, the power of communicating intelligence. For this purpose birds and mammals chiefly exercise the vocal organs, and many of these give manifest signs of a considerable acquaintance with the "art of talk." The parrot and prairie-dog, indeed, are reputed gossips; and, as all the senses can be made channels for intelligence to pass along, we shall perhaps find the voiceless fishes and serpents to be, like many other people, more sociably inclined when we know them better.

Man, quickly cognisant of his own physical attributes, and finding his needs far exceeding their utmost avail, employs the subordinate forces of nature, and extends himself. Ordinarily one man cannot speak intelligibly to another beyond a distance of 200 yards, and the movements of a man 850 yards away are barely distinguishable. So no wonder we find that from very early ages man availed himself of penetrating sounds and conspicuous exhibitions of motion, form, and colour for the purposes of telegraphy.

For obvious reasons night signalling by fires and torches was, in early times, much commoner than day signalling; and we are told in the classics that Clytemnestra watched the beacon fires light up the hills across the Ægean Sea, announcing the return of the heroes from "windy Troy." But even in these present days of the brilliant electric light, when a sunbeam can be flashed nearly 100 miles from a mirror, and the commercial marine code of flags provides for 78,000 distinct signals, the wonders of the electric telegraph far transcend all other modes of signalling; and, although apathetically regarded by Brown, Jones, and Robinson, would not fail to excite the wildest surprise, even superstitious awe, in the breasts of those more impressionable ancients who passed away before the dawn of this the zinc and copper age. Now neither eye nor ear is hampered by distance, there is no interference from fog or wind, the great sides of the world are laced with a system of magic nerves which convey intelligence among its living members by an influence only less crude and gross than the subtle vehicle of animal sensations.

The mere idea of a means of transmitting intelligence to any distance independently of the limitations of the senses does not seem to have readily occurred to our ancestors, not even to deep-thinking astrologers and those visionary alchemists who prosecuted so diligently their mythical researches after the *elixir vitæ* and philosopher's stone. It seems, indeed, to have suggested itself only after an apparent means had been discovered, and was, so to say, forced upon the notice of the world.

Up to the beginning of the eighteenth century, in spite of all its latent wonders and unexplained mystery, the science of electricity, represented by the observed attractive power of excited amber, while being a source of marvel to all, was as yet a forsaken portion of the country of science, apparently too barren and without promise to engage the notice and investigation of more than a few philosophers, among whom were Dr. Gilbert of Colchester and Sir Isaac Newton. But into this neglected and lowly field there went forth, as if by a natural similitude, an humble explorer to toil after truth. Honour be to Stephen Gray, pensioner of the Charterhouse, who devoted the last years of his life to this work; and, besides discovering that some bodies conduct electricity better than others, he successfully transmitted electricity, in 1727, along a wire 700 ft. long. At this time the only sources of electricity were glass and amber rods, so that the supply was very limited; but we find Gray, with the prophetic penetration of genius, while revealing upon his death-bed the secrets of his labours, anticipating "that there may be found a way to collect a greater quantity of the electrical fire, and consequently to increase the force of that power which, by several of these experiments, *si licet magna componere parvis*, seems to be of the same nature with that of thunder and lightning."

"The inventions of the electrical machine and the Leyden phial," says Sir William Thomson, "immediately fulfilled these expectations as to collecting greater quantities of electric fire; and the surprise and delight which they elicited by their mimic lightnings and thunders, and, above all, by the terrible electric shock, had scarcely subsided when Franklin sent his kite messenger to the clouds, and demonstrated that the imagination had been a true guide to his great scientific discovery, the identity of the natural agent in the thunderstorm with the mysterious influence produced by the simple operation of rubbing a piece of amber, which, 2,000 years before, had attracted the attention of those philosophers among the ancients who did not despise the small things in nature."

The discoveries and experiments of Stephen Gray awakened fresh enquiry into the long-neglected phenomenôn of the amber tear; but, although simultaneously we find Watson in England, Du Lac in Switzerland, and Franklin in America, transmitting the "electric fire" to great distances, it does not seem to have occurred even to the reflective mind of the latter that an intelligent and obedient thunderbolt was forged and already in their hands. The first published appropriation to the purposes of telegraphy of Gray's experimental proof that electricity can travel along wires, is to be found in the "Scots' Magazine" for 1753. One "C. M.," writing from Renfrew, there details a scheme for the construction of a land telegraph; but, although this was sufficiently startling to turn attention to the realisation of the idea it put forth, all efforts to effect a successful consummation were unavailing, owing to the insurmountable difficulty of preventing the frictional electricity they had no choice but to employ, from escaping out of the wires they used. But Nature, with a kindly accordance to our wishes which we sometimes see in her, had provided a less refractory and more docile kind of electricity, which was all around us, lurking like a shy fairy among the atoms of all metallic and non-metallic bodies, whatever their guise, in the great kingdoms of solid and fluid matter. And, with a praiseworthy liberality, she seems to have decreed that some familiar products of industry should yield us the best supply. In the minerals zinc, iron, copper, and carbon, water and the common acids, we have a source of electricity which far-seeing Stephen Gray could scarcely have dreamt of, and which we might still have been ignorantly stumbling about in the dark, but for the fact that there occurred to a man of philosophic spirit one of those chance discoveries which seem to be inseparably connected with the progress of electrical science. As early as 1767, we are told, Sulzer had discovered that pungent sandwich of the tongue and two dissimilar metals, as zinc and copper, which now "every school-boy knows;" but in this case Nature had been somewhat careless in the casting of her pearl, and twenty-three years passed away ere she astonished the eyes and fired the enquiring mind of Galvani with one of her coy revelations. Madame Galvani, having caught a slight cold, was recommended to try frog-broth; and while, like a good housewife, she was inspecting the skinned victims, which, by the way, occupied the somewhat unattractive position of a place on an operating table in the professor's laboratory, she observed startling convulsions in the dead frogs' limbs every time an assistant, who was present, drew sparks from an electric machine standing by. Of course she told her husband; and, strangely enough, six years after he noticed a similar effect in some frogs' limbs which hung by a copper rod from an iron balustrade, independent of the agency of the electrical machine. Judging from subsequent experiments, he explained the last phenomenon by supposing a specific kind of electricity to reside in the nerves, and this, flowing through the metals into the muscles, threw the latter into convulsions. But to Volta, professor of physics at Pavia, we are indebted for the true explanation that the two metals (iron and copper) of the skewer and balustrade were the real electromoters; and, in proof of this, Volta constructed the progenitor of a great race of important agents of civilisation—the zinc and copper "pile."

It was not a difficult thing to insulate or confine the electricity of the voltaic pile, and the progress of electric telegraphy was thenceforth rapid, but not until Professor Oersted of Copenhagen, in 1820, discovered that a current of electricity deflected a magnetic needle in its neighbourhood, and Schweiger of Halle applied the fact, did the heyday of telegraphy dawn.

Since then, the extent to which the science in its theoretical and practical bearings has developed, and the perfection it has reached, are extraordinary. Puck could put a girdle round the earth in forty minutes, but we know that a telegraph clerk, or, indeed, any one amongst us, could greet the antipodes ere Puck had bidden us good-bye. Submarine cables are being laid in every sea, and cable stocks are a safe investment. Very soon a fourth cable will be laid direct to America, and, perhaps before next summer, Mother England will give her apron string direct to Bermuda, and extend it to New York and Brazil. Land lines, with that daring often displayed in spiders' webs, cross each other in all directions, and penetrate across the wildest continents.

Of all men connected with the progress of electrical science as a whole, and particularly of submarine telegraphy, we are, as is well known, chiefly indebted to Sir William Thomson, not only for his brilliant mathematical investigations and deductions, but for the utility of his practical inventions in the shape of electrometers and other instruments more closely connected with the practice of telegraphy. It was his delicate mirror, or reflecting galvanometer, which lifted the first Atlantic Cable Company out of the mire into which they had fallen for want of a more sensitive interpreter of their messages. In this instrument there is an arrangement of small magnets with a miniature mirror attached, weighing about a third of a grain, the whole suspended by a single fibre of unspun silk; and the current coming from the sending station being passed round and round it, deflects the magnets and the mirror with them. A beam of light proceeding from a lamp is reflected off the mirror on to a screen, and moves when the mirror moves. The clerk at the receiving end patiently observes the motions of that spot of light upon the screen, and from them interprets the message.

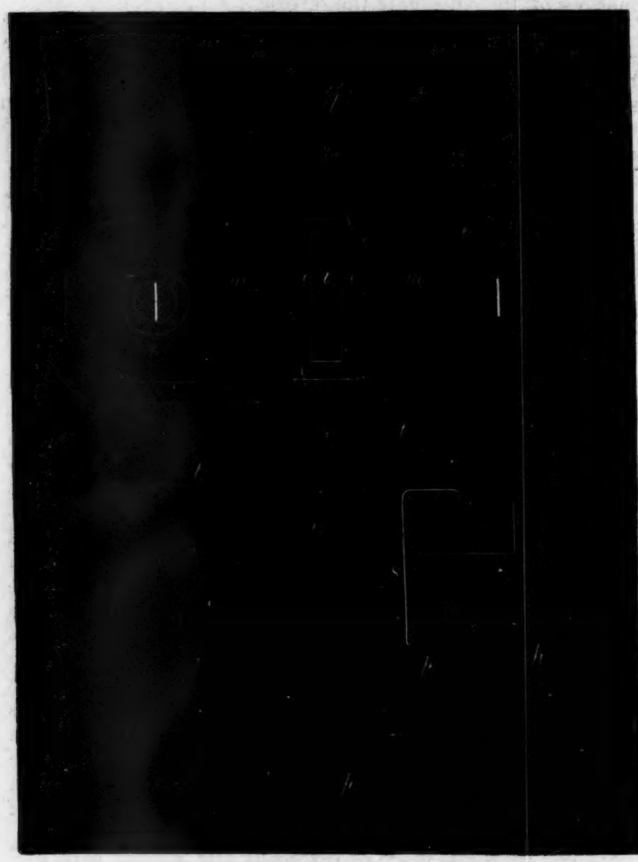
There have been recording instruments for land lines of great diversity and utility. There are instruments in use at present which record or write down the message by chemical action, which emboss it or daub it in ink, which print it in Roman characters, and which can even reproduce a sketch of a criminal's features. Of non-recording instruments, those generally employed are the "single-needle" instrument, in which the right and left movements of an upright needle involve the message; the "sounders," in which two bells of different pitch are struck; and a receiving machine, in which a moveable pointer indicates letters on a dial. But until lately the mirror galvanometer was the only instrument used upon long cables. The inventor of that invaluable current detector has, however, designed and patented a new self-recording instrument, destined to relieve the weary watches of the telegraph clerks, and to emancipate those "slaves of the lamp." Sir William Thomson's patent siphon recorder received its name from the pen which writes the message being a little siphon, which draws off ink from an ink bottle by one end, and squirts it upon a running strip of paper by the other; but the essential principle of the instrument is that of the galvanometer, so to say, inverted. Whereas in the latter the coil of wire through which the current of electricity from the distant station travels is fixed, and the magnets moveable, in the recorder the magnets are fixed, and the coil moveable. Just as Oersted found that every conductor conveying an electric current acts upon a magnet, so Faraday perceived that a magnet acts upon a conductor carrying a current. Since the earth behaves as a great magnet, and the animal body is a conductor, though—luckily for us in thunderstorms—a poor one, it is a fact that one cannot walk in an east-and-westerly direction, so as to cut the lines of terrestrial magnetic force which run approximately north and south, without establishing a difference of "electric potential" between his two extremities, which, if joined through a current indicator delicate enough, would give rise to an electric current from the one extremity to the other. On the other hand, if a current is sent through a person from his head to his feet or from his feet to his head, he will be subjected to a real mechanical impulse in a direction across the magnetic meridian, but all too feeble to be sensible. In the siphon recorder a coil of wire is suspended between the poles of a powerful electro-magnet, and the electricity coming from the cable is led through this coil, which thereupon experiences a force, and is deflected. The siphon, with ink continually jetting from it in a fine rain, is connected with the coil so as to move when it moves from side to side across a narrow riband of paper which travels past the point of the siphon. The paper moves with a certain velocity, and receives upon its surface the track of the ink-shower in a straight line when no message is being sent, but in an irregular, wavy line when the siphon is recording.

The siphon (s) upon fig. 1 [next page] is about the thickness of a horse-hair, and is made of capillary tubing so fine in the bore that ink will not run in it owing to the influence of capillary attraction. In order to make it squirt upon the paper, the ink requires to be electrified to a higher (positive or negative) potential than the paper. This is effected by an arrangement which generates electricity of high potential, and, as its form in the recorder bears a strong resemblance to one of those drums turned by white mice, and the force necessary to rotate it has been reckoned in "mice-power," it has been termed the "mouse-mill."

The principle of the mouse-mill is that of statical electric induction—the name given to that property by which electricity on a body is enabled to draw forth electricity of an opposite kind to itself upon the nearest parts of any neighbouring conductor, and to drive electricity of the same kind as itself into the parts remote. The mill-wheel is a nonconducting disc of ebonite, with composite carriers of brass and soft iron set all round its circumference. The poles of an electro-magnet are placed so as to attract the iron parts of the "carriers," and there are special arrangements for effecting this so as to keep the wheel rotating uniformly in one direction at certain velocities. Part of the electricity stored upon the inductor A is milked off by a wire and led to the ink, which it electrifies strongly, and, by virtue of a repulsion among its own particles, the ink escapes down the siphon, and breaks in spray upon the paper. A rapid vibratory motion, given to the point of the siphon by electric attraction and repulsion between it and the paper, helps also to produce the dotted character of the markings.

The coil itself *c* is made of extremely fine copper wire, and is so light that one of 100 ft. in length of this wire weighs only about 26 grains. It is made of a rectangular form, about 2 in. long, and ¾ broad, being stiffened with shellac varnish. The sides of a central piece of soft iron *i* and the poles of the electro-magnet *m m* outside, are hollowed out so as partly to surround the convolutions of the coil and concentrate upon them as much as possible of the full power of the magnetism. The top and bottom of the coil are attached to vertical stretched wires *l e*, which serve to keep the coil stiff and form an axis round which it is free to oscillate, while they at the same

FIG. 1.



time serve to lead the electricity coming from the cable into and away from the coil. These wires are kept stretched by such a sensibly constant force as an adjustable spring or arrangement of weights properly attached can give. The siphon is connected to the coil by a light fibre *f*, and this fibre is kept in a state of strain by the torsion upon the platinum wire *t*, from which the siphon depends. When the coil is deflected so as to increase the tension on the fibre, *f*, the top of the siphon is pulled nearer to the coil, and the writing extremity moves across the paper *p p* further from the coil; and when the coil is deflected so as to decrease the tension, the motions of the siphon are the reverse.

The two electro-magnets, which have each a pole *m m* shown upon the figure, are simply rods of soft iron wrapped round with copper wire, which is insulated by a covering of spun silk. So long as a current of electricity is sent through this wire the iron core about which it is wound becomes a magnet, but so soon as the current ceases to flow the magnetism vanishes almost entirely if the iron be very soft. The amount of deflection of the coil is proportional to the strength of the electro-magnetism, to the length of wire in the moveable coil, and to the strength of the current which flows through it; so that, *cæteris paribus*, the stronger the current upon the electro-magnets the greater the range of the siphon point across the paper. There is, however, a certain convenient range which can always be procured by regulating the battery power upon the electro-magnet and the current which comes from the cable. In the instrument itself every contingency in the working is provided for, and there are adjustments to regulate its every part, but the description of these would be uninteresting to many.

Suppose now that it is desired to transmit a message along the submarine cable from Falmouth to Lisbon. The source of the electricity to be employed is, we shall say, the ordinary Daniell battery, each element of which consists of zinc immersed in a solution of sulphate of zinc and a plate of copper immersed in a solution of sulphate of copper, the two solutions being kept separate by a porous separator. All the elements are joined up "in series," that is, taking them in order the zinc plate of each is connected to the copper plate of the next. If the last copper plate be joined outside the battery by any conducting circuit to the first zinc plate, a current will flow from it to the zinc. This conducting circuit may be a very complicated one, but still the current will find its way along it, always from the copper plate to the zinc, even although its route may take it round the world. In submarine telegraphy this conducting circuit is formed by the cable, the receiving apparatus, and the earth itself. If the zinc plate of a battery at any station, as Falmouth in England, is well connected to the earth—by one of the city water-pipes, for instance—or a large metal plate buried deeply in the ground, and an insulated wire be led from the copper plate to a distant station, such as Lisbon, then so soon as the conducting circuit is rendered entire by putting the wire to "earth," there the current will flow from Falmouth along the wire to Lisbon, just as if the earth itself had led the current back to the zinc plate. On the other hand, if the copper plate be earthed and the zinc plate be connected by wire to Lisbon, where it is "earthed," just the reverse will take place, and the current will seem to flow from England to Lisbon through the earth, and will return to England by the wire. The sender at Falmouth connects the first zinc and last copper plate of his battery to a "signalling key," which is an arrangement for enabling him, by simply pressing down one or other of the two levers like piano keys, to control the battery so that he can apply the zinc plate or pole of the battery to "earth" and the copper pole to the cable, or *vice versâ*, for any length of time he chooses. The receiver at Lisbon sets the "mouse-mill" running, and by a battery, also joined up "in series," magnetises the electro-magnets of the recorder. He connects the cable by means of the wire to the moveable coil, and the coil by the other wire to the earth; and so completes the conducting circuit between the zinc and copper poles of the sender's battery at Falmouth. Then, when the paper is running and the ink squirting so as to mark a straight line upon it, the instrument is ready to record the message from Falmouth. The sender there controls the electricity by means of the key according to a code of signals. The code universally adopted now is the Morse code, so called after the inventor. The fundamental basis of this code is two elementary signals, usually called the dot and dash. Every letter of the alphabet is made up of one or more of these signals. In the application of the Morse code to the recorder it is arranged that a dot and dash is transmitted when the sender presses the left and right-hand levers of the key respectively, just as in the case of the "needle instrument," and a dot is recorded by a motion of the siphon point towards the coil; and the paper being in motion, the ink traces out a small wave, while a dash is recorded by a wave on the opposite side of the axis of the paper.

The message being composed of a combination of such waves, with short intervals between each letter and longer intervals between each word, during which the siphon marks a straight line up the middle of the paper, presents a very serrated and, to the uninitiated, very unintelligible appearance, but an expert clerk can translate it as fast as ordinary handwriting.

There are of course various contractions for words conventionally employed, which facilitate communication in electric telegraphy, but without the aid of any of these, and by the use of an automatic sender—for few clerks can send more than thirty-five—as many as 120 words a minute have been recorded through a land line by this instrument, each word averaging five letters and each letter three distinct signals. Owing to an inductive effect between the electricity in the interior conductor of a submarine cable and the sea water round its exterior, there is a retardation of the velocity of the electricity which is not experienced in land lines, and as a consequence the rate of transmission of signals through the latter is much greater than through the former, so that the number of words recorded from a cable will depend more upon the speed capabilities of the cable for transmitting signals than upon the receiving qualities of the recorder. Twelve words a minute is the working speed of the Brest and St. Pierre Atlantic Cable, whereas the longest land lines will transmit signals as fast as it is possible to send them. The retardation due to induction is made very apparent by the motion of the coil of the recorder. When the electric signals are sent through a land line into the coil it oscillates quickly, and the waves traced by the siphon are sharp; but when the signals are sent through a cable the motion of the coil is slower and more prolonged, the waves traced upon the paper being flat curves.

When one stands alone by this beautiful instrument while it is at work, and observes the violet fire sparkling from the whirring "mouse-mill," the sure obedience of the coil to Nature's mysterious and inviolable laws, the apt inscription of the pen which,

> With pulses electric,
> Scatters its fine jet of ink,

and is guided by Nature's own fingers to the dictation of man many thousand miles away, he experiences one of those mental glimpses of admiration for human powers which he sometimes feels on viewing mighty enginery or triumphs of literature and art.

However much in after years telegraphy may extend its domain and connect the centres of human life throughout the world, it is scarcely too much to say that the electrical instruments of the future will be no important innovation upon those of our own time, unless indeed some hitherto unknown phenomena provide new laws to be the basis for invention no less fertile.

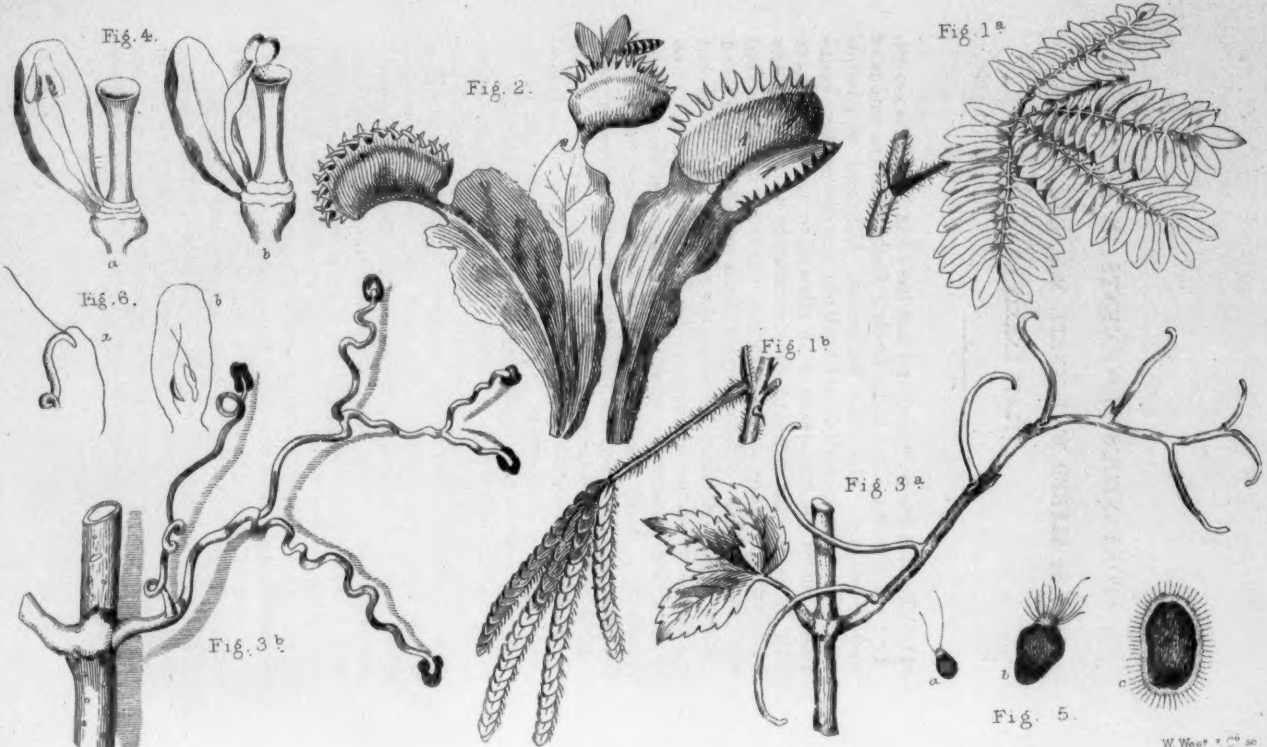
SPONTANEOUS MOVEMENTS IN PLANTS.

By ALFRED W. BENNETT, M.A., B.Sc., F.L.S.

[PLATE LXXXIX.]

THAT there are no "hard and fast lines" in Nature is a truth which is more and more forcing itself upon the minds of men of science. The older naturalists delighted to circumscribe their own special domains within sharply-marked boundaries, which no trespassers were allowed to pass. We have long given up the attempt thus accurately to map out the kingdom of Nature. Her varied productions are connected with one another by innumerable links and cross-links; and our systems of classification, even the most "natural," are but an imperfect human contrivance for bringing together those forms which present the most evident marks of resemblance or affinity. While the truth of this law is most familiar in the case of those smaller subdivisions of the animal and vegetable kingdoms—classes, orders, and genera—which are connected with one another by innumerable intermediate forms, it is none the less certain in the line of demarcation which separates these two great kingdoms themselves from one another. In attempting to draw up a definition which shall serve accurately and infallibly to distinguish between the Animal and Vegetable Kingdoms, we find ourselves compelled to abandon one supposed crucial test after another, and to content ourselves at last with framing, as in the case of the lower subdivisions, an assemblage of characters, by the *tout ensemble* of which we must decide whether our organism is an animal or a plant. So great is the uncertainty as to the actual boundary-line, that large groups of lowly organisms, such as those known as Diatoms and Desmideæ, have been regarded by experienced authorities as belonging to each kingdom; and one of the ablest of living naturalists, Ernst Haeckel of Jena, has proposed the division of the material universe not into three but into four kingdoms—animals, plants, protista, and minerals, the new kingdom of Protista including the most lowly organised forms of what are generally considered animals and plants, from the Flagellate

Plate LXXXIX.



K. Bennett. del

W. West & Co sc.

Spontaneous Movement of Plants.

Infusoria to the Fungi, distinguished by the absence of sexes, and the mode of reproduction by gemmation or fission alone. The soundness of this new classification is not however admitted by the best remaining authorities in England or Germany.

One of the most obvious distinctions between the Animal and Vegetable Kingdoms consists in the possession by the former of a power of voluntary motion of either the whole or a part of the body, dependent on the presence of a distinct nervous system, which is absent in the latter; a distinction obvious enough when contrasting any of the higher forms of the two kingdoms, but which, like all other individual characters, fails when pressed to too rigid a test. There are animals, so regarded by the best naturalists, and possessing other characters which compel us to refer them to this class, whose power of motion is confined to the "contractility" common to all protoplasmic substance, and which are absolutely devoid of a nervous system; and there are plants, unquestionable plants, which possess powers of spontaneous motion strictly comparable to those exhibited by the lower animals. It may be interesting to collect together a few illustrations of this last-named fact, some of which appear to the writer scarcely explicable by the application of any of those laws which govern inert unorganised matter.

The movements to which reference is here made belong in most cases to a part rather than to the whole of a plant; in some cases, however, we find the whole organism endowed with spontaneous motion of a very remarkable character. An instance of this occurs in the case of the regular undulating motion, exceedingly similar to that of some of the lower animals, characteristic of a class of Algæ hence called Oscillatoriæ. The mode of reproduction of the Algæ, the lowest class of the vegetable kingdom, to which the sea-weeds and the freshwater confervæ belong, is often obscure, and in some cases different distinct processes exist in the same species. In certain freshwater Algæ, reproduction takes place by the formation of "Zoospores," (fig. 5), which are the results of the separation and isolation of the protoplasmic contents of certain special cells. According to the observations of M. Thuret, who has paid great attention to this subject, these zoospores, which are of extreme minuteness, are ovoid in form, and are furnished, either over their whole circumference or towards one extremity, with very fine cilia, varying from two to a large number. As soon as these minute bodies free themselves from the cell in which they are enclosed, the cilia begin to vibrate with great rapidity, the vibration being accompanied by a movement of rotation of the bodies themselves on their axis, occasioned apparently by rapid and spontaneous contractions; the result being a quick motion of the body through the water—undistinguishable in fact from that of some of the lower forms of animal life—continuing for a period varying from half an hour to several hours, at the expiration of which they settle down, reassume the characters of ordinary vegetable cells, lose their cilia, and give rise, by cell-division, to new individuals resembling the parent-plant. Those zoospores which are furnished with cilia at one extremity only, direct that extremity, which is destitute of chlorophyll or green colouring matter, towards the light. Closely resembling these zoospores are the "spermatozoa" of the higher orders of cryptogamic plants, ferns, equisetums, and mosses. These bodies (fig. 6) are produced in the antheridia or male organs, again by a modification of the protoplasmic cell-contents; they are filiform bodies of various forms, mostly presenting one or more spiral curves, and furnished with vibratile cilia. When released from the parent cells, they move about with great activity until they come into contact with the opening of the archegonium or female organ, which they enter, and thus fructify the germ of the new plant. Pringsheim describes the process by which the spermatozoa enter the archegonium as a very peculiar twisting motion, due to the action of the mucus or protoplasm of the germ-cell. He has seen a large number of spermatozoa enter a single cell, forming a kind of chain.

In describing these curious bodies, of the connection of which with the vegetable kingdom there is no room for doubt, one is irresistibly reminded of these lowly forms of animal life known as *Amœba* and *Gromia*, consisting apparently of shapeless masses of protoplasm, possessing indeed far more restricted powers of locomotion than the zoospores and spermatozoa, their faculties in this respect being confined to the protrusion and retractation of arms or pseudopodia, by means of which a slow movement is effected. If the possession of consciousness and of a voluntary control over the movements of the body belongs to the animal kingdom even to its lowest forms, it is difficult to frame any cogent reason for denying these faculties to the vegetable organisms which we have been considering. A very interesting problem also presents itself for solution in the almost perfect identity of constitution between these lowest forms of animals and the protoplasmic elements in the constitution of more highly organised forms. If the *Amœbæ* and *Gromiæ* are admitted to be distinct individual animals, the same line of reasoning would almost compel us to admit to the same rank the white corpuscles of the blood of mammalia, which present almost the same characters and possess the same power of protrusion and retractation of a portion of their substance.

The instances above cited illustrate the faculty of spontaneous motion possessed by detached portions of protoplasm endowed with the power of forming themselves into new individuals. This phenomenon appears, however, to be but a form of the property possessed by all protoplasm of constant motion in some form or other. The circulation of the protoplasmic mucous fluid within the cells of plants is one of the most beautiful phenomena of vegetable life revealed by the microscope, and one of which the explanations at present offered appear quite inadequate. A favourite object for exhibiting this circulation or rotation is formed by the jointed hairs which cover the stamens of the Virginian Spider-wort (*Tradescantia virginica*). The movement is rendered visible by the presence in the otherwise colourless fluid of minute opaque granules of chlorophyll or other colouring matter; and is observable with great ease in the semi-transparent tissue of certain water-plants, as *Chara*, or the *Valisneria* commonly grown in fresh-water aquariums. It consists of a slow movement of the protoplasmic fluid up one side of the cell, across the ends, and down the other side; not perpendicularly, but in an oblique or spiral course. The subject has been carefully investigated by three French physiologists, MM. Prillieux, Roze, and Brongniart, who find that the rotation is directly influenced in a remarkable manner by the presence of light. M. Prillieux kept a moss in the dark for several days, when the cells presented the appearance of a green net-work, between the meshes of which was a clear transparent ground. All the grains of chlorophyll were applied to the walls which separate the cells from one another; there were none on the upper or under walls which form the surfaces of the leaf. Under the influence of light, the grains, together with the thin mucous plasma in which they are embedded, change their position from the lateral to the superficial walls, this change taking place, under favourable circumstances, in about a quarter of an hour. On attaining their new position, the grains do not remain absolutely immovable, but continually approach and recede from one another; and if again darkened, they leave their new position, and return to the lateral walls. Artificial light produces the same effect as daylight.

Analogous to the circulation of the protoplasm within the cell is that of the sap or nutritive fluid through the whole plant, passing through the permeable walls of the cells. This circulation of the sap, by which fluid is conveyed equally to all parts of the plant, apparently in opposition to the laws of gravity, is no doubt explicable to a certain extent by the application of known physical laws, of which the most important are capillary attraction, osmose, or the law by which a less dense fluid passes through a permeable diaphragm to mingle with a denser fluid, and the upward pumping force to supply the partial vacuum occasioned by the evaporation of water from the leaves. Allowing, however, full scope to all these physical forces, there would seem to be a residuum of energy still unaccounted for connected with the vitality of the plant itself. In particular, the selective power of plants in absorbing from the soil a larger portion of those ingredients which are required for the formation or healthy life of their tissues, is an absolutely unexplained phenomenon. A familiar instance of this is furnished by the difference in the amount of silica absorbed by corn-crops and by leguminous plants, amounting in the former case to 2·5 per cent., in the latter to ·3 per cent. of the dry foliage. Indeed, if any two plants are grown together, side by side in the same soil, the constitution of the ash, i.e., of the solid ingredients derived from the soil, will be remarkably different; while in the same plant in the same soil the constitution is constant. It was pointed out by the Duke of Argyll, when criticising Darwin's "Origin of Species," how unavoidable it seems, in describing the phenomena of nature, to use language involving the idea of contrivance and design. In the same manner it seems impossible to describe the process of vegetative life without appearing to attribute to the plant some conscious power of its own. A striking instance of this, as well as of the liability to consider a mere statement of an obscure law in other terms as an explanation of that law, occurs in an admirable treatise on the growth of plants—Johnson's "How Crops Grow." * "The cereals *are able to dispose* of silica by giving it a place in the cuticular cells; the leguminous crops, on the other hand, *cannot remove it* from their juices; the latter remain saturated, and thus further diffusion of silica from without becomes impossible, except as room is made by a new growth. It is in this way that we have a *rational and adequate* explanation of the selective power of the plant." The "rational and adequate explanation" seems to me, on the contrary, to be merely a restatement of this selective power of the tissues in other terms. Because the tissues want the silica, is no explanation of how they get it.

The curious and interesting movements of climbing plants have been investigated by Palm, Mohl, and Asa Gray, and form the subject of one of the most charming of Mr. Darwin's works. It is well known that climbing plants, such as the hop, honey-

* "How Crops Grow:" A Treatise on the Chemical Compositions, Structure and life, of the Plant, for Agricultural Students. By S. W. Johnson. Revised and adapted for English use by A. H. Church and W. T. T. Dyer. London: Macmillan & Co., 1869, pp. 345.

suckle, or major convolvulus, always twine round the stem or other object which supports them in one direction, that is, always either from right to left or from left to right; but few probably have reflected, and fewer still attempted to observe, by what process the end of the growing shoot contrives to change its position from one side to the other of the stem. If the extremity of a living stem, say of convolvulus, growing perfectly free and in a normal position, is observed, it is seen to hang over from its support in a horizontal direction; and this horizontal portion is found, if observed at intervals of some hours, to point in different directions. The end of the growing shoot has, in fact, the property of revolving in a large circle round the support, always with the same species in the same direction, either with the sun or opposed to the sun. The rate of revolution varies with different plants, and with the same plant at different periods of its growth; it is much quicker in warmer than in cooler weather. With the hop Darwin found it to vary from two and-a-half hours to nine hours. The object of the climbing power of plants is no doubt to reach the light and to expose a large surface of leaves to its action and to that of the free air; but the mode by which this power of motion is gained is by no means clear. The late eminent physiologist Mohl supposed that it was caused by a dull kind of irritability in the stem, which caused it to bend towards the support when in contact with it. Mr. Darwin has, however, carefully tested this theory experimentally, and always with negative results. He rubbed many shoots much harder than was necessary to excite movement in any tendril or in any foot-stalk of a leaf-climber, but without result. This view seems also entirely negatived by the fact that not only do the stems of climbing plants revolve when they are not in contact with any support, but even more freely under such circumstances than when climbing. When a climbing plant first springs from the ground, the extremity of the shoot performs slow gyrations in the air, as if, as Darwin expresses it, it were *searching* for a support. I do not here discuss the question whether this habit may be the result of a tendency transmitted and enhanced through thousands of generations; the movement itself is, in the individual plant, entirely "spontaneous" in every sense of the term; that is, is not the necessary result of known physical laws acting upon the individual. Darwin's paper "On the Movements and Habits of Climbing Plants" published in the Journal of the Linnean Society, contains a number of the most interesting observations on this class of plants; and the language employed is everywhere suggestive of some hidden sentient controlling power in the plant itself.

The same purpose as that served by a climbing stem is answered in other plants, as the vine, Virginian creeper, and passion-flower, by tendrils; and the phenomena of spontaneous motion in tendrils, are, if possible, still more curious. Some tendrils display the same power of rotatory motion possessed by the extremities of the shoots of climbing plants, others do not revolve, but are sensitive, bending to the touch. The curling movement consequent on a single touch continues to increase for a considerable time, then ceases; after a few hours the tendril uncurls itself, and is again ready for action. A tendril will thus show a tendency to curl round any object with which it comes into contact, with the singular exception that it will seldom twine itself round another tendril of the same plant. It is also very curious that with some exceedingly sensitive plants, the falling of drops of rain on the tendril will produce no effect whatever. The mode in which a tendril of a *Bignonia* catches hold of a support is thus described by Darwin:—"The main petiole is sensitive to contact with any object; even a small loop of thread after two days caused one to bend upwards. The whole tendrils are likewise sensitive to contact. Hence, when a shoot grows through branched twigs, its revolving movement soon brings the tendril into contact with some twig, and then all three "toes" bend, (or sometimes one alone) and, after several hours, seize fast hold of the twig, exactly like a bird when perched." The Virginian creeper has another mode of attaching itself to a wall or other solid support, by the formation at the extremities of the branches of the tendril, of little disks or cushions, very similar to the disks on the foot of the house-fly by which it is enabled to attach itself to our windows and to walk along the ceiling. These disks secrete a glutinous fluid which attaches the tendril to the support with such strength that it is often impossible to detach it without destroying the tendril or even removing a portion of the wall itself. As soon as the attachment is accomplished the tendril gradually thickens and contracts spirally, as shown in Fig. 3, *a*, *b*. This spiral contraction, indeed, is always the result of the tendril meeting with a support; and if no support is found, the tendril soon shrinks and withers away. Some tendrils exhibit a most remarkable power of selection, which, to use Mr. Darwin's words, "would, in an animal, be called instinct." The tendrils of a species of *Bignonia* slowly travelled over the surface of a piece of wood, and when the apex of one of them came to a hole or fissure, it inserted itself; the same tendril would frequently withdraw from one hole and insert its point into a second one. Mr. Darwin has seen a tendril keep its point, in one instance for twenty hours, and in another instance for thirty-six hours, in a minute hole, and then withdraw it. After the record of this fact on such unexceptional evidence, we are the more prepared to credit the statement of Mr. Anderson-Henry that a climber will, in running up a wall, carefully avoid contact with another climber which it dislikes; and even the account by M. Paul Lévy * that the *lianes* of tropical forests have an affinity for certain trees, towards which they direct their growth, and not towards those nearest to them; carefully drawing themselves away when they encounter one of the objectionable trees.

We may conclude our account of climbing plants with the following remarks by Mr. Darwin:—"It has often been vaguely asserted that plants are distinguished from animals by not having the power of movement. It should rather be said that plants acquire and display this power only when it is of some advantage to them, but that this is of comparatively rare occurrence, as they are affixed to the ground and food is brought to them by the wind and rain. We see how high in the scale of organisation a plant may rise, when we look at one of the more perfect tendril-bearers. It first places its tendrils ready for action, *as a polypus places its tentacula.* If the tendril be displaced, it is acted on by the force of gravity, and rights itself. It is acted on by the light, and bends towards or from it, or disregards it, whichever may be most advantageous. During several days the tendrils or internodes, or both, spontaneously revolve with a steady motion. The tendril strikes some object, and quickly curls round, and firmly grasps it. In the course of some hours it contracts into a spire, dragging up the stem, and forming an excellent spring. All movements now cease. By growth, the tissues become wonderfully strong and durable. The tendril has done its work, and done it in an admirable manner."

The phenomenon known as Sensitiveness is of by no means uncommon occurrence in the vegetable kingdom. It consists of a sudden movement of the leaf, a portion of the flower, or the whole plant, on contact with, or even on the approach of, a foreign body. One of the most familiar examples is that of the Sensitive Plant, *Mimosa pudica* and *sensitiva*, fig. 1, *a* and *b*, in which three distinct movements are observable when the leaf is touched by the hand or the warm breath. First, the numerous leaflets close in pairs, bringing their upper faces together, and also inclining forwards; then the four branches of the leaf-stalk, which were outspread like the rays of a fan, approach each other; at the same time the main leaf-stalk turns downwards, bending at its joint with the stem. The explanation offered in one of our best botanical text-books of this phenomenon is as follows:—"There is a swelling at the base

* Bulletin de la Société Botanique de France. Translated in the "Gardener's Chronicle," March 19, 1870.

of the petiole, the cells of which constitute, as it were, two springs acting in contrary directions, so that if the one from any cause be paralysed, the other pushes the leaf in the direction of least resistance. These springs, if they be so called, are set in action by the rush of fluid creating a turgid state of the one set of cells and an empty state of the other. What circumstances regulate the turgescence are only imperfectly known." It will be obvious, that, even if this is correct as a statement of facts, it offers no real explanation of the phenomenon; for it is quite as difficult to understand how the mere approach of the hand, which gives rise to a sensitiveness commencing, it will be remarked, at the *extremity* of the leaf, will account for a "turgescence" of the springs at the *base* of the leaf, which then causes the movement. It should be observed also that we are unaware of any use which these movements are to the plant. Similar sensitiveness occurs in the leaves of some other leguminous plants, in several species of *Oxalis*, &c. M. Bert has observed that the sensitiveness is destroyed by the continual application of chloroform, and also by placing the plant constantly in the dark or in green light.

Similar movements to that of the Sensitive Plant, but occurring spontaneously, may be observed in other plants. Thus in the *Desmodium gyrans* or "Telegraph Plant," sometimes grown in our hot-houses, belonging to the same order, Leguminosæ, the leaf consists of three leaflets, a large central, and two smaller side ones. The motion is especially observable in the small side leaflets, which on a warm summer's day may be seen to rise and fall by a succession of jerking movements; now stopping for some time, then moving briskly, always resting for a while in some part of their course, and starting again without apparent cause, "seemingly of their own will," as Prof. Asa Gray remarks. The movement is not simply up and down, but the end of the moving leaflet sweeps more or less of a circuit. It is not set in motion by a touch, but begins, goes on, and stops, of itself.

An exceedingly remarkable instance of sensitiveness occurs in the case of the "Venus's Fly-trap" of North Carolina, *Dionœa muscipula*, represented in fig. 2. The mid-rib of each leaf serves as a kind of hinge. When the inside of the blade of the leaf, or the fine bristles which grow on its surface, are touched by any foreign substance, the hinge suddenly closes, and if the intruding substance be a fly or other small object, it is immediately imprisoned as represented in the figure, the teeth on the margin of the leaf closing firmly upon one another like a steel trap, the sides of the trap then flatten down and press firmly upon the victim, and it now requires a very considerable force to open the trap. If nothing is caught the trap presently re-opens of itself, and is ready for another attempt. With regard to the object of this strange proceeding, there can be no doubt that the insect is retained until the softer parts of the body are completely dissolved in the thick mucous fluid which is exuded by the leaves: and Professor Asa Gray considers that the evidence is nearly complete that the animal matter is actually absorbed in the leaf itself. It is even stated that pieces of raw beef are digested by the leaf in the same manner! Seeing, however, that it is now generally admitted by physiologists that even pure water is not absorbed through the pores of leaves, which serve only for the *exhalation* of vapour, this explanation is very hard of belief. The "pitchers" of the *Nepenthes*, or pitcher-plant, act also as fly-traps, large numbers of insects being enticed into them by the fluid they secrete, and are then unable to extricate themselves.

The sensitiveness of the leaves of plants is but an excessive development of the phenomenon known as the Sleep of plants. In the case of the Sensitive Plant the position assumed by the leaf and leaflets in the night is the same as that which they assume when disturbed in the day-time; and with many other plants, such as the clover and the *Robinia* or "acacia" tree, the change in the position of the leaflets, morning and evening, is a familiar fact. The Sleep of Plants extends also to the flowers, many plants opening their flowers only at particular times of the day. Thus the major convolvulus of the gardens and the goat's-beard open at sunrise and always close by about noon, the evening primrose opens only in the evening, and many others last for but a single day. So regular is the time of opening and closing of some flowers, that Linnæus drew up a list, which he termed a "floral clock." The singular part of the affair is, that with many flowers the time of opening and closing is determined, not by the degree of light, or by the temperature or humidity of the atmosphere, but absolutely by the hour of the day. The giant water-lily of the Amazons, the *Victoria regia*, opens, for the first time, about 6 P.M., and closes in a few hours, then opens again at 6 A.M. the next day, remaining open until the afternoon, when it closes and sinks below the water. Other plants, again, open their flowers only in the bright sunshine, as the beautiful yellow centaury or *Chlora perfoliata*, the sundew, *Drosera rotundifolia*, &c. In the latter plant, belonging to the same natural order as the Venus's Fly-trap, and possessing a slight irritability of the leaves, Mr. Worthington Smith has noticed also a strong sensitiveness in the petals, the flowers closing suddenly when touched.

Irritability or sensitiveness, similar to that of the leaves of the Sensitive Plant, is not uncommon in the flower. An instance has been alluded to in the petals of the sundew; it occurs also in the lip of the corolla of several of the orchis tribe. It is, however, more common in the proper organs of reproduction, as the style of *Stylidium*, the stamens of the berberry, &c., and is then directly connected with the process of fertilisation of the ovule. In *Stylidium*, an Australian genus, the style and filaments are adherent into a column, which hangs over on one side of the flower. When touched, it rises up and springs over to the opposite side, at the same time opening its anthers and scattering the pollen. The stamens of the various species of *Berberis* and *Mahonia*, to the former of which our common berberry belongs, exhibit this irritability to a remarkable degree. If touched with a pin or other object at the base of the inside face of the filament, the stamen will spring violently forward from its place within the petal, so as to bring the anther into contact with the stigma, as shown in fig. 4, and will after a time slowly resume its original position. At first sight it may seem as if this contrivance were intended to ensure the fertilisation of the pistil from the pollen of its own flower. In reality, however, the reverse is the case; the excitation takes place in nature when an insect entering the flower for the sake of the honey in the glands at the base of the pistil, touches the inside of one of the stamens. The pollen is thus thrown on to the head or body of the insect, which carries it away to the next flower it visits, and leaves some of it on the stigma, and thus cross-fertilisation instead of self-fertilisation is secured. Similar motion of the stamens towards the pistil, but spontaneous, takes place in the case of the London Pride, and other species of *Saxifraga*.

Elasticity is, indeed, a common property of organised tissue, though it is not often developed to so evident an extent. In the "touch-me-not," or *Impatiens*, we have a familiar instance in the seed-vessel, which, if touched when nearly ripe, suddenly coils back, throwing the seeds to a considerable distance. The "squirting cucumber" (*Momordica Elaterium*) marks the period of ripeness by the fruit separating from its stalk, and expelling the seeds and juice with great violence. Mr. Thomas Meehan described a remarkable instance of elasticity at a recent meeting of the Academy of Natural Sciences of Philadelphia. The seeds—or, as would appear from his description, more correctly the embryos of the seeds—of the American "witch-hazel" (*Hamamelis virginica*) are thrown out with such force as to strike people violently in the face who pass through the woods. Collecting a number of the capsules, and laying them on the floor, he found the seeds or embryos were thrown out generally to the distance of four or six feet, and in one instance as much as twelve feet.

Many of the instances of spontaneous motion or irritability we have now recorded may doubtless be explained by the application of known physical laws. With others this is not so easy; and it is but reasoning in a circle to say that because the organisms which manifest them belong to the vegetable kingdom, therefore the phenomena cannot be the result of a sentient force acting upon, and independent of, matter. Darwin has described how certain movements of the tendrils of climbing plants would be termed instinctive if they were observed in animals. The rapid rotatory motion of the zoospores of the lower Algæ is absolutely undistinguishable from that of certain undoubted lowly organised forms of animal life. It is very difficult to distinguish between the movement of a shoot of a climber performing its circles in the air in search of a support, and that of the tentacula of a coral-polyp in search of food. The mode in which the Venus's Fly-trap seizes and encages its prey is very like that adopted by a sea-anemone. Every fresh addition to our knowledge seems to confirm us in the view that it is unwise to dogmatise by laying down too rigid generalities, and absolutely to deny certain functions to whole classes of animated beings because we do not find them exhibited in the forms most familiar to us. I do not wish distinctly to claim for plants the actual possession of a voluntary or sentient faculty. But I do wish to point out that facts do not support us in asserting that a clear line of demarcation separates the animal from the vegetable kingdom; the power of voluntary motion belonging to the one and not to the other. Taking all the facts we have described into consideration, the statement seems justified which has been made by one of our most experienced naturalists, Professor Wyville Thomson*:—"There are certain phenomena, even among the higher plants, which it is very difficult to explain without admitting some low form of a general harmonising and regulating function, comparable to such an obscure manifestation of reflex nervous action as we have in sponges and in other animals in which a distinct nervous system is absent."†

* Introductory Lecture to the Natural History Class at the University of Edinburgh, May, 1871. See "Nature," vol. iv. p. 91.

† Since writing the above, I have met with the following remarks by the Italian botanist, Prof. Delpino ("American Naturalist," July, 1871, p. 297):—"I must here, as always, declare myself a teleologist and a vitalist. Now teleology and vitalism, far from being vanquished by the Darwinian doctrine, find in it their most solid support. What do teleology and vitalism mean? They mean that we believe that there is in all living things an

EXPLANATION OF PLATE LXXXIX.

FIG. 1. Leaf of Sensitive Plant, *Mimosa pudica*; *a*, in normal position; *b*, after depression by the approach of the hand.
" 2. Leaves of Venus's Fly-trap, *Dionæa muscipula*, one of them closed on an imprisoned fly.
" 3. Tendril of Virginian Creeper, *Ampelopsis virginica*; *a*, before contact; *b*, after contact with a wall.
" 4. Stamen of Berberry, *Berberis vulgaris*; *a*, normal position; *b*, after excitation of base of filament.
" 5. Zoospores of Algæ; *a*, *b*, *Conferva*; *c*, *Vaucheria*.
" 6. Spermatozoa; *a*, *Chara*; *b*, Sea-weed, *Fucus serratus*.

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innate, specific principle, intelligent, free, and teleological. This principle is the hidden cause of the variability of organised beings, as well as the wonderful harmonies which have been established between one being and another."

NEWS FROM THE STARS.

By RICHARD A. PROCTOR, B.A. (Cambridge), Hon. Sec. R.A.S.
Author of "The Sun," "Essays on Astronomy," "Other Worlds," &c.

FROM time to time, during the last three years, I have brought before the readers of this magazine the various arguments and considerations on which I have based certain new views respecting the constitution of the sidereal universe. In so doing I have had occasion to deal chiefly with facts already known, though not hitherto viewed in that particular light in which I sought to place them. Indeed it is an essential part of my general argument that much that is contained in observations already made has been escaping us. In the eagerness of astronomers to ascertain new facts, they have been neglecting the interpretation of facts already ascertained.

But I have long felt that it would greatly tend to advance the new views which I have advocated, if some process of research, pursued by one of those astronomers of our day who possess the requisite means and leisure for prolonged enquiries, should confirm in a clear and decisive way some definite point of my new theories. Thus, if new observational evidence should be found in favour of my theory that the nebulæ are not external to our galaxy, or if new evidence should be obtained to show that the stars are aggregated in certain regions within our system and segregated from others; or again, if my theory of star-drift should be confirmed by new and striking evidence, I felt that a greater measure of confidence in my analysis of former evidence would thenceforward be accorded. I had no occasion, indeed, to complain of cavil or opposition; in fact, a degree of attention had been given to the new opinions I advocated which was certainly much greater than I had looked for. But there must always be such an inertia in the general weight of opinion in favour of accepted views, that only a steady reiteration of reasoning during a long period, or else some striking and impressive discovery, can cause the weight of opinion to tend in the contrary direction.

I cannot but regard myself as most fortunate in finding the first confirmation of my views (i.) coming from one of the most eminent astronomers and physicists of the day, (ii.) bearing upon one of the most definite and positive of my vaticinations, and (iii.) relating to one of the most interesting subjects in the whole range of recent astronomical research.

It will be in the remembrance of many readers of this magazine that, nearly four years ago, Dr. Huggins succeeded in showing that the bright star Sirius is travelling at an enormously rapid rate away from us. In other words, besides that rapid thwart motion which is shifting the place of this star upon the heavens, the star has a rapid motion of recession. In the paper called "Are there any Fixed Stars," in the "Popular Science Review" for October 1868, the nature of the means by which this discovery was effected was fully described and explained. It may be permitted to me to mention, also, that while Dr. Huggins's researches were still unannounced (or rather incomplete) I was so far fortunate as to indicate the possibility of employing the very method of research which Dr. Huggins was then engaged (unknown to me) in applying to Sirius. I propose here briefly to describe and explain the method, referring the reader who desires fuller information on these preliminary points to the paper of October 1868, mentioned above. I am the more desirous of doing this, because I find the principle of the method not readily grasped, and that I conceive the explanation I am about to offer* may remove certain difficulties not uncommonly experienced.

Conceive that a person, standing on the edge of a steadily-flowing stream, throws corks into it at regular intervals—say one cork per second. These would float down the stream, remaining always separated by a constant distance. Thus, if the stream were flowing three feet per second, the corks would be a yard apart (supposing, for convenience of illustration, that each cork was thrown with exactly the same force and in exactly the same direction). Now, if a person a mile or so down the stream saw these corks thus floating past, he could infer that they had been thrown in at regular intervals; and, moreover, if he knew the rate of the stream, and that the corks were thrown in by a person standing at the river's edge, he would know that the interval between the throwing of successive corks was one second. But, *vice versâ*, if he knew the rate of the stream, and that the corks were thrown in at intervals of one second, he could infer that the person throwing

* I am indebted for the illustration on which is based the explanation which follows, to my friend and college contemporary, Mr. Baily, great-nephew of the eminent astronomer, Francis Baily.

them was standing still. For let us consider what would happen, if the cork-thrower sauntered up-stream or down-stream while throwing corks at intervals of one second. Suppose he moved up-stream at the rate of a foot per second; then, when he has thrown one cork, he moves a foot up-stream before he throws the next; and the first cork has floated three feet down stream; hence the second cork falls four feet behind the first. Thus the common distance between the corks is now four feet instead of three feet. Next suppose he saunters down-stream at the rate of a foot per second; then, when he has thrown one cork, he moves a foot down-stream before he throws the next; and the first cork has floated three feet down-stream; hence the second cork falls only two feet behind the first. Thus the common distance between the corks is now two feet instead of three feet. It is clear, then, that the person standing a mile or so down-stream, if he knows that the stream is flowing three feet per second, and that his friend up-stream is throwing one cork in per second, can be quite sure that his friend is standing still if the corks come past with a common interval of three feet between them. Moreover, he can be equally sure that his friend is sauntering up-stream, if the corks come past with a common interval exceeding three feet; and that he is sauntering down-stream, if the common interval is less than three feet. And if, by some process of measuring, he can find out exactly *how much* greater or how much less than three feet the interval is, he can tell exactly how fast his friend is sauntering up-stream or down-stream. It would not matter how far down-stream the observer might be, so long as the stream's rate of flow remained unchanged; nor, indeed, would it matter, even though the stream flowed at a different rate past the observer than past the cork-thrower, so long as neither of these two rates were liable to alteration.

Now, we may compare the emission of light-waves by a luminous object to the throwing of corks in our illustrative case. The rate of flow for light-waves is indeed infinitely faster that that of any river, being no less than 185,000 miles per second. The successive light-waves are set in motion at infinitely shorter time-intervals, since for extreme red light there are no less than 458,000,000,000,000 undulations per second, and for extreme violet no less than 727,000,000,000,000; but these specific differences do not affect the exactness of the illustration. It is obvious that all that is necessary to make the parallel complete is that the flow of light-waves shall reach the observer at a constant rate (which is the actual case), and that he shall know, in the case of any particular and distinguishable kind of light, what is the rate at which the wave-action is successively excited, and be able to compare with this known rate the rate at which they successively reach him. If they come in quicker succession than from a luminous body at rest, he will know that the source of light is approaching as certainly as our observer down-stream would know that his friend was sauntering towards him if the corks came two feet apart instead of three feet. If, on the contrary, the light-waves of a particular kind come in slower succession than from a body at rest, the observer will know that the source of light is receding, precisely as the river-side observer would know that his friend was travelling away from him if the corks came past him four feet apart instead of three.

Now, the stellar spectroscopist *can* distinguish among the light waves of varied length which reach him, those which have a particular normal length. He analyses star-light with his spectroscope, and gets from it a rainbow-tinted streak crossed by dark lines. These dark lines belong to definite parts of the spectrum; that is, to such and such parts of its red, or orange, or yellow, or green, or blue, or indigo, or violet portion. Thus they correspond to light having a particular wave-length. And *many* of these lines in stellar spectra are identifiable with the lines due to known elements. For instance, in the spectrum of Sirius there are four strong dark lines corresponding to the known bright lines of the spectrum of hydrogen. Thus the wave-length corresponding to any one of these dark lines is perfectly well known to the spectroscopist from what he has already learned by examining the bright lines of hydrogen. Now, if Sirius were receding very rapidly, the wave-length corresponding to one of these lines would be lengthened; it would correspond, in fact, to a part of the spectrum nearer the red end or the region of longer light waves, and thus the dark line would be shifted towards the red end of the spectrum; whereas, on the contrary, if Sirius were very rapidly approaching, the dark line would be shifted towards the violet end of the spectrum. All that would be necessary would be that the rate of approach or recession should bear an appreciable proportion to the rate at which light travels, or 185,000 miles per second. For, reverting to our cork-thrower, it is clear that if he travelled up-stream or down-stream at a rate exceedingly minute compared with the stream's rate of flow, it would be impossible for the observer down stream to be aware of the cork-thrower's motion in either direction, unless, indeed, he had some very exact means of measuring the interval between the successive corks.

Now the spectrum of a star can be made longer or shorter according to the dispersive power employed. The longer it is, the fainter its light will be; but, so long as the dark lines can be seen, the longer the spectrum is, the greater is the shift due to stellar recession or approach; and, therefore, the more readily may such recession or approach be detected. But, with the instrument used by Dr. Huggins four years ago, it was hopeless, save in the case of the brilliant Sirius (giving more than five times as much light as any other star visible in our northern heavens), to look for any displacement due to a lower rate of recession than some hundred miles per second (little more than the two-thousandth part of the velocity of light). What was to be done, then, was to provide a much more powerful telescope, so that the stellar-spectra would bear a considerably greater degree of dispersion. With admirable promptitude the Royal Society devoted a large sum of money to the construction of such an instrument, to be lent to Dr. Huggins for the prosecution of his researches into stellar motions of approach and recession. This telescope, with an aperture of fifteen inches, and a light-gathering power somewhat exceeding that usual with that aperture, was accordingly completed, and provided with the necessary spectroscopic appliances. Many months have not passed since all the arrangements were complete.

In the meantime, I had arrived at certain inferences respecting the proper motions of the stars, on which Dr. Huggins's researches by the new method seemed likely to throw an important light.

More than three years ago, I had expressed my conviction that whenever the recorded proper motions of the stars were subject to a careful examination, they would confirm the theory I had enunciated, that the stars are arranged in definite aggregations of various forms—star-groups, star-streams, star-reticulations, star-nodules, and so on.* Making leisure, in the summer of 1869, for entering upon such an examination I was led to several results, which not only confirmed the above-mentioned theory but suggested relations which I had not hitherto thought of. Some of these results are discussed in the article called "Are there any Fixed Stars," already referred to; others are presented in an article called "Star Drift," in the "Student" for October 1870. The special results on which Dr. Huggins's recent discoveries throw light, were first publicly announced in a paper read before the Royal Society, on January 20, 1870.

I had constructed a chart in which the proper motions of about 1,200 stars were *pictured*. To each star a minute arrow was affixed, the length of the arrow indicating the rate at which the star is moving on the celestial vault, while the direction in

* See "Notes on Star-Streams," in the "Intellectual Observer" for August 1867, "Notes on Nebulæ," in "The Student" for March 1868," and "A New Theory of the Universe," in "The Student" for February, March, and April 1869.

which the arrow pointed shows the direction of the star's apparent motion. This being done, it was possible to study the proper motions much more agreeably and satisfactorily than when they were simply presented in catalogues. And certain features, hitherto unrecognised, at once became apparent. Amongst these was the peculiarity which I have denominated "Star drift;" the fact, namely, that certain groups of stars are travelling in a common direction.* This was indicated, in certain cases, in too significant a manner to be regarded as due merely to chance distribution in these stellar motions; and I was able to select certain instances in which I asserted that the drift was unmistakable and real.

Amongst these instances was one of a very remarkable kind. The "seven stars" of Ursa Major—the Septentriones of the Ancients—are known to all. For convenience of reference, let us suppose these seven divided as when the group is compared to a waggon and horses. Thus, there are four waggon-wheels and three horses. Now, if we take the waggon-wheels in sequence round their quadrilateral (beginning with one of the pair farthest from the horses), so as to finish with the one which lies nearest to the horses—these are named by astronomers in that order Alpha, Beta, Gamma, and Delta of the Great Bear. Thus, Alpha and Beta are the well-known pointers (Alpha nearest the pole), and Delta is the faintest star of the Septentrion set. The three horses are called in order Epsilon, Zeta, and Eta; Epsilon being nearest to Delta. Now when the proper motions of these seven stars had been mapped, I found that whereas Alpha and Eta are moving much as they would if the sun's motion were alone in question, the other five are all moving at one and the same rate (on the star-sphere, that is) in almost the exactly opposite direction. Moreover, a small star close by Zeta (the middle horse), a star known to the Arabian astronomers as the "Test," because to see this star was held a proof of good eyesight, is moving in the same direction and at the same rate as Zeta and the rest of this set. And besides this star (which has also been called Jack by the Middle Horse), Zeta has a telescopic companion which also accompanies him in his motion on the celestial sphere.

After a careful consideration of these circumstances, and an

* I include this among "features hitherto unrecognised, though Michell had already noted the fact that the stars are arranged into systems. "We may conclude," he said, "that the stars are really collected together in clusters in some places, where they form a kind of systems; whilst in others there are few or none of them, to whatever cause this may be owing, whether to their mutual gravitation or to some other law or appointment of the Creator."

analysis of the probabilities in favour of and against the theory that the concurrence of apparent motion was merely accidental, I came to the conclusion that the five large stars and the two smaller ones form a true drifting set. I found, on a moderate computation, that the odds were upwards of half a million to one against the concurrence being accidental; and since I had recognised other instances of concurrence not less striking, I felt that it was morally certain that these stars belong to one star-family.

The reader will perhaps not be surprised to learn, however, that before publishing this conclusion I submitted it (in July, 1869) to one who was, of all men, the best able to pronounce upon its significance—the late Sir John Herschel. I have the letter (dated August 1, 1869) which he sent in reply before me as I write. The part relating to my discovery runs as follows:—"The considerations you adduce relative to the proper motions of the stars are exceedingly curious and interesting. Of late years catalogues have gone into much detail, and with such accuracy that these motions are of course much better known to us than some twenty or thirty years ago. The community of proper motion over large regions (of which you give a picture in Gemini and Cancer) is most remarkable, and the coincidence of proper motion in Beta, Gamma, Delta, Epsilon, and Zeta Ursæ Majoris most striking. Your promised paper on this subject cannot fail to be highly interesting."*

In a letter written on May 11, 1870, and referring not to another letter of mine, but to my "Other Worlds," Sir John Herschel remarked, "the cases of star-drift such as that in Ursa Major are very striking, and richly merit further careful examination."

My first public expression of opinion respecting the star-drift in Ursa Major was conveyed in the following terms:—"If these five stars indeed form a system (and I can see no other reasonable explanation of so singular a community of motion), the mind is lost in contemplating the immensity of the periods

* He proceeds as follows (the passage is removed from the main text as relating to a different branch of the subject):—"I cannot say that I am at all surprised at its being found that the average proper motions of stars of small magnitudes is not less than those of large, considering (as I have always done) that the range of individual magnitude (i.e. lustre) must be so enormous that multitudes of *very* minute stars may in fact be our very near neighbours." Compare my paper on "The Sun's Journey through Space," above referred to, which paper also deals with the point touched on in the next sentence of Sir John Herschel's letter:—"Your remark on the conclusion I have been led to draw relative to the small effect of the correction due to the sun's proper motion, will require to be very carefully considered, and I shall of course give it every attention."

which the revolutions of the components of the system must occupy. Mädler had already assigned to the revolution of Alcor around Mizar (Zeta Ursæ) a period of more than 7000 years. But if these stars, which appear so close to the naked eye, have a period of such length, what must be the cyclic periods of stars which cover a range of several degrees upon the heavens." (From Zeta to Beta is a distance on the heavens of about nineteen degrees.) "The peculiarities of the apparent proper motions of the stars," I added, "lend a new interest to the researches which Dr. Huggins is preparing to make into the stellar proper motions of recess and approach." *

But a few months later, in a lecture delivered at the Royal Institution I pointed out more definitely what result I expected from Dr. Huggins's researches. "Before long," I said, "it is likely that the theory of star-drift will be subjected to a crucial test, since spectroscopic analysis affords the means of determining the stellar motions of recess and approach. The task is a very difficult one, but astronomers have full confidence that in the able hands of Dr. Huggins it will be successfully accomplished. I await the result with full confidence that it will confirm my views." †

It will be manifest that if the five large stars in Ursa are really travelling in the same direction, then, when Dr. Huggins applied the new method of research, he would find that, so far as motion in the line of sight was concerned, these stars were either all receding or all approaching at the same rate, or else that they were all alike in showing no signs of any motion, either of recess or approach.

But in the meantime there was another kind of evidence which the spectroscope might give, and on which I formed some expectations. If these stars form a single system it seemed likely that they would all be found to be constituted alike—in other words, that their spectra would be similar. Not indeed that associated stars always display such similarity. Indeed the primary star of a binary system not unfrequently exhibits a spectrum unlike that of the small companion. But the five large stars in Ursa, being obviously primary members of the scheme they form, might be expected to resemble each other in general constitution. Moreover, since the stars not included in the set—viz., Alpha and Eta—might be regarded as probably very much nearer or very much farther away, it was to be expected (though not so confidently) that these two stars would have spectra unlike the spectrum common (on the supposition) to the five stars.

* "Proceedings of the Royal Society," Jan. 20, 1870, pp. 170, 171.

† "Report of the Royal Institution of Great Britain," Weekly Evening Meeting, Friday, May 6, 1870, p. 7.

Now, Secchi announced that the stars of the Great Bear, with the exception of Alpha, have spectra belonging to the same type as the spectrum of the bright stars Sirius, Vega, Altair, Regulus, and Rigel. This result was in very pleasing accordance with the anticipations I had formed, except that I should rather have expected to find that the star Eta had a spectrum unlike that of the remaining five stars of the Septentriones. Moreover, as the stars belonging to this particular type are certainly in many cases, and probably in all, very large orbs * (referring here to real magnitude, not to apparent brilliancy), the inference seemed fairly deducible that the drifting five stars are not nearer than Alpha, and therefore (since we have seen that it is unlikely that *all* the Septentriones lie at nearly the same distance) the inference would be that the drifting stars lie much farther away than the rest.

It remained, however, that the crucial test of motion-measurement should be applied.

In the middle of May last I received a letter from Dr. Huggins announcing that *the five* are *all receding* from the earth. In all the hydrogen line called F, is "strong and broad." In the spectrum of Alpha the line F is "not very strong" (so faint, indeed, Dr. Huggins afterwards informed me, that he preferred to determine the star's motion by one of the lines due to magnesium in the star's atmosphere. He found that Alpha is *approaching*. As to Eta, Dr. Huggins remarked that the line at F is "not so strong or so broad" as in the spectrum of "the five." He was uncertain as to the direction of motion, and mentioned that "the star was to be observed again." He subsequently found that this star is receding. But whereas all the five are receding at the enormous rate of 30 miles per second, Eta's recession was so much smaller that, as we have seen, Dr. Huggins was unable to satisfy himself at a single observation that the star was receding at all.

It will be seen that my anticipations were more than fulfilled. The community of recessional motion was accompanied by evidence which might very well have been wanting—viz., by the discovery that neither Eta nor Alpha shared in the motion. Moreover, the physical association between the five stars was yet further evidenced by the close resemblance found to exist between the spectra of the five stars. Dr. Huggins remarked in his letter, "My expectation had nothing to do with the above results. At the moment, I thought Alpha was included in the

* Sirius 4 demonstrably gives out much more light than our sun, and according to the best determinations of his distance he must (if his surface is of equal intrinsic lustre) be from 2,000 to 8,000 times larger than the sun. Vega, Altair, and Rigel are also certainly larger and may be very much larger than our sun.

group, and was therefore a little disappointed when I found Beta going the opposite way."

We have at length, then, evidence, which admits of no question—so obviously conclusive is it—to show not only that star-drift is a reality but that subordinate systems exist within the sidereal system. We moreover recognise an unquestionable instance of a characteristic peculiarity of structure in a certain part of the heavens. For, though star-drift exists elsewhere, yet every instance of star-drift is quite distinct in character—the drift in Cancer unlike that in Ursa, and both these drifts unlike the drifts in Taurus, and equally unlike the drift in Aries or Leo. Much more, indeed, is contained in the fact now placed beyond question, than appears on the surface. Rightly understood, it exhibits the sidereal system itself as a scheme utterly unlike what has hitherto been imagined. The vastness of extent, the variety of structure, the complexity of detail, and the amazing vitality, on which I have long insisted, are all implied in that single and, as it were, local feature which I had set as a crucial test of my theories. I cannot but feel a strong hope, then, that those researches which my theories suggest, and which I have advocated during the last few years, will now be undertaken by willing observers. The system of star-gauging, which the Herschels did little more than illustrate (as Sir W. Herschel himself admitted), should be applied with telescopes of different power to the whole heavens,* not to a few telescopic fields. Processes of charting, and especially of equal surface charting, should be multiplied. Fresh determinations of proper motions should be systematically undertaken. All the evidence, in fine, which we have, should be carefully examined, and no efforts should be spared by which new evidence may be acquired. Only when this has been done will the true nature of the galaxy be adequately recognised, its true vastness gauged, its variety and complexity understood, its vitality rendered manifest. To obtain, indeed, an absolutely just estimate of these matters, may not be in man's power to compass; but he can hope to obtain a true relative interpretation of the mysteries of the stellar system. If any astronomer be disposed to question the utility or value of such researches, let him remember that Sir W. Herschel, the greatest of all astronomers, set "a knowledge of the constitution of the heavens," as "the ultimate object of his observations."

* This is a work in which telescopes of every order of power would be useful. The observations, also, would be very easily made and would *tell* amazingly.

LIFE-FORMS OF THE PAST AND PRESENT.

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[PLATES XC. and XCI.]

ONE of the most interesting results at which the naturalist arrives in extending his researches from the living present far back into the remote periods of geological time, is that he finds existing between the life-forms of the past and present, not merely accidental likenesses or analogies, but actual homologies and relationships. There is, in fact, no ground whatever for the old, and to some extent, still prevalent dogma, that the several faunas and floras, which in past ages successively peopled and clothed the surface of the earth, had no direct relationship, either with each other, or with existing types.

Indeed, so strong was this feeling in the minds of nearly all the earlier observers, that they hesitated to compare extinct organisms with living forms, and were content to accept the dictum of those geologists who taught that each series of fossiliferous deposits was a distinct creation, being separated by a universal cataclysm alike from the preceding and subsequent life-periods. It is less than twenty years since the modern doctrine of continuity of life on the earth began to be received and adopted as the basis of all sound palæontological reasoning; and notwithstanding the numerous breaks that still exist, it is nevertheless possible, by reviewing the life-history of any particular class or order, to demonstrate that a real continuity does exist from the earliest representative down to the forms of to-day.

I propose to take, by way of illustration, a few examples from a class which offers perhaps the widest geographical and geological range, combined with the greatest diversity of detail in organisation, of any among the Invertebrate kingdom, namely the Crustacea; confining myself mainly to two of the most ancient orders, the Merostomata and the Trilobita.

In tracing a group like the Crustacea further and yet further back in past time, we find that our enquiry is greatly facilitated, not merely by reason of the reduction of our field of observation to a smaller accessible area of fossiliferous deposits, but also because the objects themselves become more and more reduced, not merely in numbers, but in diversity of forms, until at last we arrive at rocks in which the whole class is included in some two or three orders or families.

Thus in the rocks of Tertiary age Crabs (*Decapoda-Brachyura*) are apparently as abundant as in our recent seas.

But in the Secondary strata we perceive a visible diminution in the short-tailed forms, the earliest of which are, at present, only known in rocks of Oolitic age.*

Lobsters (*Decapoda-Macrura*), however, are abundant in the Oolitic series, and extend back into primary or palæozoic times, the first being found in the Coal-measures.†

Through all these formations we find representatives of the principal living genera, with the exception only of those soft-bodied forms which could not be preserved in a fossil state, such as the "Brine-shrimp," *Artemia salina*; *Cheirocephalus*; and the parasitic *Lerneonema*, *Argulus*, *Nicothoe*, and other specialised forms.

As we scan the record of these old Carboniferous rocks, so rich in organic remains, we seem to stand on some lofty beacon-hill, whence we can cast our glance upwards and downwards along the stream of time. Beneath our feet lie buried the last representatives of those aboriginal races now quite extinct, the Trilobita and the Eurypterida, whose ancient hosts peopled the seas of Devonian and Silurian ages, and reached far away into the Cambrian epoch. Beside them lie the earliest representatives known of our modern Decapods, Stomapods, and Isopods, then but few and feeble, but now the dominant races of the Crustacean class.

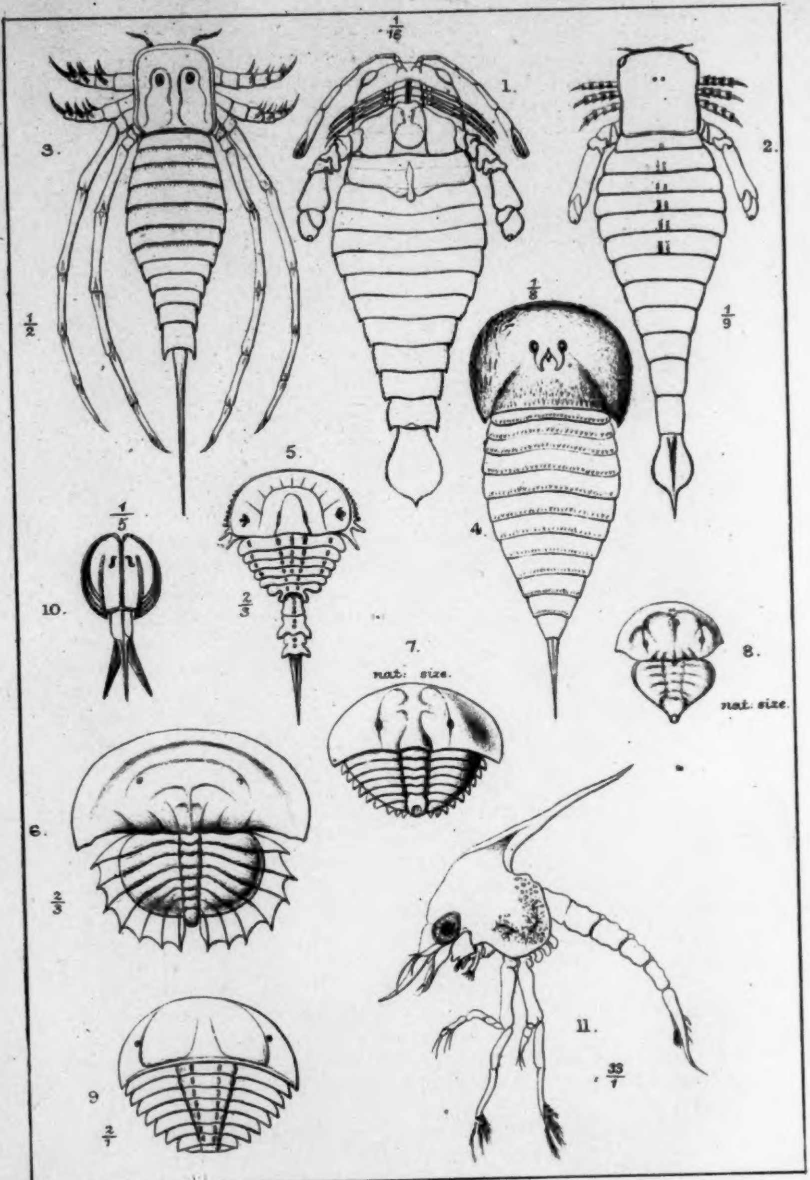
Is this, then, the barrier-reef between the Palæozoic and Neozoic life-periods? Do we indeed find here the beginning of all modern forms of Crustacea, and the ending of all ancient ones? By no means; nor is there, as we have already observed, any period in the whole geological record at which a hard and fast line can be drawn dividing the class into recent and extinct families.

Certain groups, such as the Entomostraca, are represented *throughout*. Others, like the Amphipoda, may perhaps extend

* Oldest known British Crab, *Palæinachus longipes*, H. Woodward, Forest Marble, Malmesbury, Wilts. See "Quart. Journ. Geol. Soc.," 1866, Vol. XXII. p. 493.

† *Anthrapalæmon Grossartii*, Salter, Coal Measures, Lanarkshire. See "Quart. Journ. Geol. Soc.," 1861, Vol. XVII. p. 531.

Plate XC.



D. Blair, del et lith.

W. West & Co. imp.

Types of Palæozoic Crustacea.

into Silurian times; * or, like the Isopoda, may reach back to the Devonian epoch.† The Cirripedia, a most aberrant group of Crustacea (represented abundantly in the seas of to-day by the pedunculated *Lepas* ("Ship Barnacle") and the sessile *Balanus* (or "Acorn Shell"), so common on the bottoms of ships which have been long at sea, and upon the piles of piers, and on sea-walls, and rocks washed by the tide), carry back their history, the latter through the Tertiary rocks to the Upper Chalk,‡ the former to the earliest Secondary rocks, whilst a single form is found in the Wenlock shale of Dudley (Upper Silurian).§

The King-Crabs, next to the Entomostraca, undoubtedly enjoy the most extended range in time; occurring in considerable numbers in the Lithographic stone of Solenhofen, with characters scarcely, if at all, differing from those species now found living on the east coast of North America and in the seas of China and Japan. About seven species occur in the Coal-measures (See Plate XC., figs. 6, 7, 8,), and one actually in the Silurian (See Plate XC., fig. 9) of Lesmahagow in Lanarkshire;‖ these palæozoic forms closely resemble the larval stages of the living *Limulus* (See Plate XCI., figs. 21–24).

The accompanying Table will best exhibit the successive appearance of the chief orders of Crustacea, and beside them are placed the Arachnida, the Myriapoda, and the Insecta, with their representatives in palæozoic strata; thus giving the range of the entire sub-kingdom of the Arthropoda in time.

But omitting the solitary instances, already referred to, of those higher forms which have left traces of their existence in palæozoic times, it is evident that, from the Carboniferous strata downwards, we have to deal for the most part with three great groups of Crustacea, namely the Merostomata, the Trilobita, and the Entomostraca.

The first of these, the Merostomata (or thigh-mouthed

* I believe the form I have described under the generic name of *Necrogammarus*, from the Lower Ludlow, to be an Amphiphod. See "Trans. Woolhope Club, Hereford," 1870, p. 271.

† I have described a part of a giant Crustacean, which I believe to be an Isopod, under the name of *Præarcturus gigas*, from the Devonian of Herefordshire. See "Trans. Woolhope Club, Hereford," 1870, p. 266.

‡ *Pyrgoma cretacea*, H. Woodward, "Geol. Mag.," 1868, Vol. V. p. 258, pl. xiv. figs. 1, 2, 3. From the Upper Chalk near Norwich is at present the oldest.

§ *Turrilepas Wrightii*, H. Woodward, "Quart. Journ. Geol. Soc.," 1865, Vol. XXI. p. 486, pl. xiv. figs. 1–6.

‖ *Neolimulus falcatus*, H. Woodward, "Geol. Mag.," 1868, Vol. V. p. 1, pl. i. fig. 1.

RANGE OF THE ARTHROPODA, SHOWING THE RELATIVE PERSISTENCE IN TIME OF EACH GROUP.

	CRUSTACEA												
	DECAPODA		STOMAPODA	AMPHIPODA	ISOPODA	(TRILOBITA)	CIRRIPEDIA	PHYLLOPODA and OSTRACODA	MEROSTOMATA		ARACHNIDA	MYRIAPODA	INSECTA
RECENT and TERTIARY	BRACHYURA	MACRURA							XIPHOSURA	(EURYPTERIDA)			
CRETACEOUS					Palæga			Well represented throughout.					
JURASSIC	Palæinachus		Squilla		Archæoniscus		Pollicipes		Limulus				
TRIASSIC													
PERMIAN				Prosoponiscus									Eugereon, 1 sp.
CARBONIFEROUS		Anthrapalæmon	Pygocephalus						Prestwichia Bellinurus	Eurypterus	Scorpio, 4 sp. Eophrynus, &c., 2 sp. Protolycosa, &c., 2 sp. Architarbus	Xylobius, 1 sp. Euphoberia, 4 sp.	Coleoptera, 3 sp. Orthoptera, 13 sp. Neuroptera, 14 sp.
DEVONIAN					Præarcturus					Pterygotus Stylonurus			Neuroptera, &c., 7 sp.
SILURIAN				Necrogammarus		Trilobita	Turrilepas		Neolimulus Hemiaspis	Pterygotus Slimonia Eurypterus			
CAMBRIAN													

Crustacea), are divided into two sub-orders, the Eurypterida and the Xiphosura. These may not inaptly be compared with the higher Decapoda, which are divided into Brachyura (short-tailed) and Macrura (large or long-tailed); for in the Eurypterida the segments of the body are distinct and well developed (See Plate XC., figs. 1, 2, 3, 4), whilst in the Xiphosura or "King-crabs," they are coalesced and cephalised (Plate XC., figs. 6, 7, 8, 9). In both, however, the details of their organisation are arranged upon a common pattern.

In the Crab and Lobster the compound eyes are placed on movable eye-stalks; the antennæ, the mouth-organs and the organs of prehension, locomotion, and respiration, are all constructed on a common plan.* But the body-segments in the Crab are reduced to a minimum size, and merely subserve, in the female, as a cradle or marsupium for the eggs before hatching, while in the male they are quite rudimentary. In the Lobster, on the other hand, the body-segments in both sexes are well developed, and serve as a most powerful swimming organ, by which the animal can by a sudden jerk propel itself through the water with great velocity.

All the Merostomata have large compound sessile eyes, or, if pedunculated, the stalk is immovably fixed to the carapace. The larval eye-spots, or ocelli, are usually distinctly seen. The appendages are not specialised and set apart for separate duties, as in the higher forms of Crustacea, but alike fulfil all the functions of locomotion, prehension, and mastication, and for this purpose are arranged around the mouth (see Plate XC., fig. 1). The branchiæ and organs of reproduction are borne upon a series of lamelliform plates, which are in fact the modified pairs of limbs, attached to the under surface of the thoracic segments; there being as many as six plates in *Limulus*, five of which are branchiferous, and probably only two, or at the most three, in *Pterygotus*. The remaining segments are destitute of appendages.

In the Eurypterida the body-segments are largely developed for swimming (as in the Lobster); whilst in the Xiphosura they are reduced to a minimum size (as in the Crab). Lastly, it is the long-tailed division in each order which is the oldest group, the more cephalised type in each being also the more modern.

We have thus seen that *Limulus* and *Pterygotus* are as intimately related as Crabs and Lobsters are to each other at the present day; but nevertheless the type of *Pterygotus* is a very abnormal type. Let us see what we have like it among living forms.

* See an article on the Lobster, by Mr. St. George Mivart, F.L.S., in the "Popular Science Review," 1868, Vol. VII. p. 345, pl. xxii.

If we glance for a moment at the larval stage of that most specialised and highly developed of modern Crustacea the Crab, depicted on our plate (Pl. XC., fig. 11), we find that in its zooid state it has sessile eyes, a long body, destitute of any appendages; it has no walking legs, but it is a free-swimming form, performing its locomotion with its maxillipeds or jaw-feet, which are greatly developed, serving as a pair of long oars similar to those with which *Stylonurus Logani* is furnished. (Pl. XC., fig. 3).

Indeed the larva of this highest form is the most apt illustration of our ancient order the *Merostomata*.

Limulus is living to-day to represent his great ancestor in the Silurian epoch, but we find that the palæozoic King-crabs are still more closely and successfully represented, not by the adult, but by the larval stages of the living King-crab, represented in our illustration (compare figs. 6, 7, 8, and 9, on Plate XC., with figs. 21, 23, and 24 on Plate XCI.

We thus arrive at another interesting deduction, namely, that the stages of development of the individuals of to-day are a reflection of the life-history of the class in past geological time. It is worthy of notice, when speaking of the Merostomata, to point out that we have intermediate forms affording characters between the long-bodied *Pterygotus* and the short-bodied *Limulus*, namely the genus *Hemiaspis* (see Plate XC., fig. 5). There are also three Russian forms named *Pseudoniscus aculeatus*, *Exapinurus Schrenkii*, and *Bunodes lunula*, in which the hinder segments of the body are sensibly diminished in size and reduced in number.

A similar group of irregular forms (the Anomoura) exists among the living Decapoda.

When we turn to the Phyllopoda and Ostracoda, among the Entomostraca, we find numerous forms in the palæozoic rocks, which are readily comparable with those now existing, and differing chiefly in their greater size as compared with living types.

The one figured on our engraving (Plate XC., fig. 10), *Dithyrocaris Scouleri*, McCoy, will serve as an apt illustration of a palæozoic phyllopod, reminding one strongly of the recent *Apus* and *Nebalia*.

The Entomostraca were probably as abundant in past times, as at the present day, their remains often forming almost entire strata.

We do not propose to treat of them in the present article, but merely to point out that they are the most persistent group among the Crustacea, being found from the Cambrian period to the seas of to-day; lowness or simplicity in organisation, with great powers of vitality and reproduction superadded, being the most satisfactory explanation by which to account for their longevity.

The group which has attracted perhaps the greatest amount of attention, and upon which vast labour has been bestowed with little or no result commensurate therewith, is that of the Trilobites.

This is a truly palæozoic group, and, so far as we are aware, an extinct group, although this is always a difficult point to be dogmatic upon.

Look, for example, at the Limulidæ. We have living King-crabs as far apart as on the east coast of North America and along the coasts of China and Japan, &c. We find them again in the Oolitic beds of Solenhofen in Bavaria. Again in the Coal-measures of England and Illinois; again in the Upper Silurian of Scotland.

We cannot doubt the continuity, but the gaps are enormous.

Nor can we, by the same rule, positively assert that the Trilobita are a strictly palæozoic type, and will *never* be found in neozoic strata.

In taking a general review of this great family or order, so widely distributed through the older sedimentary deposits, one is naturally struck by the immense amount of *variation of form*, brought about simply by the modification of a single plastic type, and that, apparently, a very elementary one. Yet by means of diverse ornamentation, in the way of spines, warts, and tubercles, by compression in one direction, by elongation in another, by adding to the normal number of segments of the body, or by subtracting therefrom; by enlargement or reduction of the eyes (the only organs seen upon the dorsal aspect of the body), we obtain—as with one of those amusing human faces in vulcanite india-rubber that children delight to contort—all those endless modifications of expression of form possible to the frame of the highest vertebrate organism.

In the present state of our knowledge it is very difficult to speak at all positively of the organisation of this group, but I incline to the belief that they conceal beneath their apparently simple structures evidence of more than one order.

For instance: they indicate, in some points, a close relationship to the Phyllopoda. We find in both the articulated labrum; and although we have not as yet obtained clear evidence of the maxillæ in the Trilobita, we are justified in concluding from analogy that they possessed such organs.* Again,

* See note on the "Palpus and other Appendages of a Trilobite," by H. Woodward; and a paper on the "Supposed Legs of Trilobites," by E. Billings; "Quart. Journ. Geol. Soc." 1870, Vol. XXVI. See also H. Woodward "On the Structure of Trilobites," "Geol. Mag." 1871, Vol. VIII. p. 289, Pl. VIII.

in both the Trilobites and Phyllopods we find genera in which a greater number of segments is attained than the normal number in the Crustacean type. Thus in *Conocephalus* there are sixteen (reckoning only one each for the head-shield and pygidium), in *Paradoxides* twenty to twenty-two, in *Arethusina* twenty-two, and in *Harpes* twenty-eight; and if we look upon the head-shield and tail-plate as composed of several body-rings united together—which seems certainly to be the correct view—we have forms presented to us in which the multiplication of segments like each other is one of its peculiar features, illustrating that form of growth which Professor Owen has most aptly described as a vegetative repetition of parts.

Barrande, who has made a special study of this group, when writing recently upon the divisions of the body, divides them into four groups:

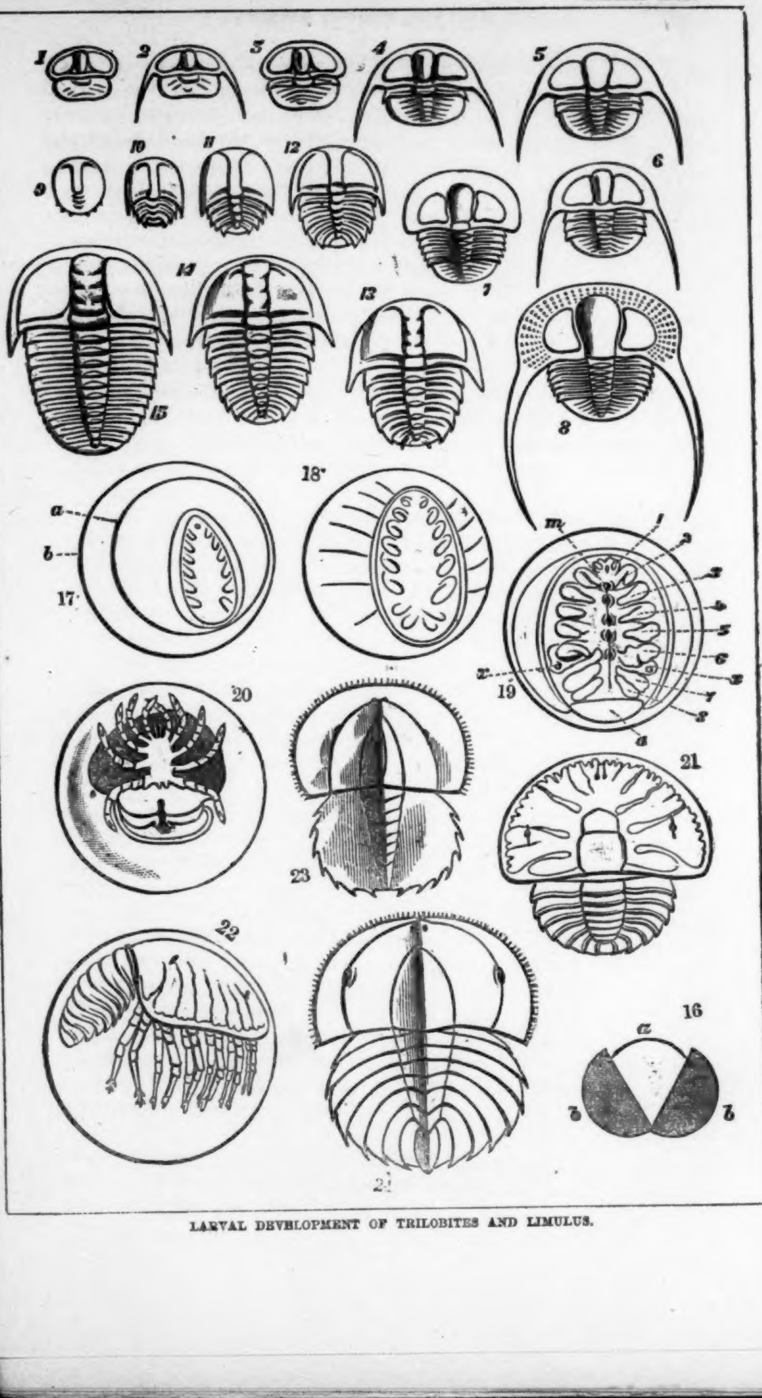
The first with from		1 to 4	free and movable thoracic segments,			containing	2	genera.
The second	„	5 to 9	„	„	„	„	24	„
The third	„	10 to 13	„	„	„	„	32	„
The fourth	„	14 to 26	„	„	„	„	16	„

We thus perceive that those forms of Trilobites having a great excess of *free segments* is not large when we consider the whole as a group.

In the higher and more specialised forms of Isopoda of the present day we do not find the number of segments absolutely adhered to without any variation; on the contrary, we constantly meet with individuals in which more or fewer segments are welded together, so as to conceal the normal number of seven thoracic somites between the head and the abdomen.

Such being the case, we cannot be surprised to find considerable variation in a group like the Trilobita, which, if they really are the remote ancestors of the recent Isopoda, must be, according to the views I have suggested above, the prototypes of the larvæ rather than of the adult stage of the living form.

Dr. Dohrn and other writers upon the Arthropoda have pointed out the remarkable similarity between the larval stages discovered by Barrande of certain forms of Trilobites, of which we have reproduced two sets, namely *Trinucleus ornatus*, Sternb., and *Sao hirsuta*, Barr. (See Plate XCI., figs. 1–8, and 9–15), and the larval stages observed in the living *Limulus* (See Plate XCI., figs. 20–24). But the larvæ of the Trilobites pass through these stages after being hatched, whilst those of *Limulus* take place in the egg. Bearing in mind that in the Trilobites we are dealing with animals possessing in many cases a large number of free thoracic segments covered by a firm calcareous crust or shell (at least it is so in



LARVAL DEVELOPMENT OF TRILOBITES AND LIMULUS.

a large number of genera), it is hardly probable that mere branchial feet would serve for their locomotion.

If, however, each free and movable thoracic segment was furnished with a pair of appendages, as among the modern Isopods, and as is also indicated in the larva of the *earliest stages* of development within the egg of the modern *Limulus* (See Plate XCI., figs. 17–19), then another point is gained in our investigation, and we see that the earliest embryonal stages are those which naturally foreshadow the earliest and simplest adult forms. In other words, all the immense variety of forms in a group are but the expression of the sum of the stages passed through by the highest individual in arriving at perfection

Another relation which the Merostomata and Trilobita exhibit, and upon which much stress has been laid by Dr. Dohrn and Prof. Hæckel, is that between these palæozoic types and the Arachnida; particularly between the Eurypterida and the Scorpionidæ.

And it is a most significant fact that the earliest Arachnides occur as far back as the Coal-measures, where the last of the Eurypterida and the Trilobita are also met with. Anyone who has examined a scorpion, or is acquainted with its form and structure from books and drawings, cannot fail to be struck by the remarkable resemblance between it and the Eurypterida, even to the arrangement of the appendages, the position of the eyes, &c., &c. Indeed, we may very fairly infer that from this division of the Crustacea the Scorpionidæ of to-day were derived. Nor is there any insuperable difficulty in accepting this view on sound physiological grounds. The possibility of an animal passing through larval conditions, casting aside, at even a single moult, its branchiæ, and assuming aërial respiration, quitting the water and inhabiting the land, changing its element, its diet, its mode of progression, and its entire life, is no chimerical speculation. Such cases are familiar to the entomologist,* the carcinologist,† and even to the herpetologist.‡

But the acceptance of this proposition does not, as has been assumed by these writers, necessitate the removal of the Eurypterida from the Crustacea; on the contrary, as Fritz Müller well observes, "If all the classes of the Arthropoda (Crustacea, Insecta, Myrapoda, and Arachnida) are indeed all branches of a common stem (and of this there can scarcely be a doubt), it is evident that the water-inhabiting and water-breathing Crustacea must be regarded as the original stem

* The larval and adult Libellula, Ephemera, &c.

† *Gecarcinus ruricola,* and other land-crabs.

‡ The Batrachia.

from which the other (terrestrial) classes, with their tracheal respiration, have branched off." *

Viewed as a whole, the Crustacea probably present to us the best zoological illustration of a class constructed on a common type retaining its general characteristics, but capable of endless modifications of its parts, so as to suit the extreme requirements of every separate species.

And it is doubtless in great degree due to this plasticity of structure, enabling the species to occupy such diverse positions, and to subsist upon such varied aliment, that the class owes its preservation through the lapse of ages represented by the long series of geological formations, from the Cambrian strata to the present day.

EXPLANATION OF PLATE XC.

FIG. 1. *Pterygotus anglicus*, Agassiz, ventral aspect restored. From the Devonian of Forfarshire (about one-sixteenth natural size).

" 2. *Slimonia acuminata*, H. Woodw., dorsal aspect restored. From the Upper Silurian, Lesmahagow, Lanarkshire (about one-ninth natural size).

" 3. *Stylonurus Logani*, H. Woodw., dorsal aspect restored. From the Upper Silurian, Lesmahagow, Lanarkshire (about one-half natural size).

" 4. *Eurypterus Scouleri*, Hibbert, dorsal aspect restored. From the Carboniferous limestone of Kirkton quarry and Bathgate, West Lothian (about one-eighth natural size).

" 5. *Hemiaspis limuloides*, H. Woodw., dorsal aspect. From the Lower Ludlow Leintwardine, Shropshire (about two-thirds natural size).

" 6. *Prestwichia rotundata*, H. Woodw., Coal-measures, Ironstone, Coalbrook Dale (about two-thirds natural size).

" 7. *Bellinurus Königianus*, H. Woodw. sp. nov. Coal-measures, Dudley Coal-field, in clay ironstone (natural size).

" 8. *Prestwichia Birtwelli*, H. Wood. sp. nov. Coal-measures, Cornfield Pits, near Padiham, Lancashire (natural size).

" 9. *Neolimulus falcatus*, H. Woodw. Upper Silurian Lesmahagow, Lanarkshire (twice natural size).

" 10. *Dithyrocaris Scouleri*, McCoy, Carboniferous, Ireland (one-fifth natural size).

" 11. Larva or Zöea of the common "Shore-crab," *Carcinus mænas*, Penn. 2nd stage, copied from Mr. C. Spence Bate's paper on the "Development of Decapod Crustacea," Phil. Trans. 1858, p. 589, Pl. XL.

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* "Facts and Arguments for Darwin," by Fritz Müller (p. 120). Translated from the German by W. S. Dallas, F.L.S., &c.

[Figs. 1–9, copied and reduced from the plates illustrating Mr. Henry Woodward's Monograph on the Merostomata, published in the volumes of the Palæontographical Society, 1867–72.]

EXPLANATION OF PLATE XCI.

[This plate is reproduced here by permission of the Council of the Geological Society of London from a paper by Mr. H. Woodward, "On the Relationship of the Xiphosura to the Eurypterida, and to the Trilobita and Arachnida." See "Quart. Journ. Geol. Soc.," 1872. Vol. XXVIII. p. 46.]

FIGS. 1–8. *Trinucleus ornatus*, Sternb. sp. (copied from Barrande's "Système Silurien du centre de la Bohême," Prague, 1852, 4to, plate 30). Specimens arranged in series according to their supposed age. (All the stages figured by Barrande are not given here.)

FIG. 1. Young individual, destitute of thoracic segments, composed of head-shield and pygidium only.
„ 2. Another of the same stage, in which the genal or cheek-spines are developed.
„ 3. Individual with one thoracic segment developed, but without the genal spines.
„ 4. Another of the same stage, with the genal spines.
„ 5. Individual with two thoracic segments, and in which the genal spines are present.
„ 6. Individual with three thoracic segments, and possessing the genal spines.
„ 7. Individual with five thoracic segments, but without genal spines.
„ 8. Adult *Trinucleus*, with six thoracic segments and fully-developed genal spines.

FIGS. 9–15. *Sao hirsuta*, Barrande (copied from plate 7 of Barrande's work above cited). Barrande figures twenty stages of this Trilobite, of which we have only reproduced seven.

FIG. 9. First stage. A young individual in which the limit of the head-shield is not indicated as separating it from the pygidium.
„ 10. Second stage. Young individual with the head-shield separated, and having inclinations of three soldered segments to the pygidium.
„ 11. Third stage, in which the genal angles of the head and the spiny border of the pygidium are well seen, and four or five soldered segments indicated.
„ 12. Fourth stage, in which two free thoracic segments are developed behind the head, and two or three soldered segments represent the pygidium.
„ 13. Fifth stage, in which the thorax is longer than the head, and is composed of three movable segments and three soldered segments in the pygidium.
„ 14. Sixth stage, in which four free segments succeed the head, and three or four soldered segments form the pygidium.

FIG. 15. Tenth stage, in which eight free segments succeed the head, and three soldered segments form the pygidium. [In the twentieth stage figured by Barrande the adult has seventeen free thoracico-abdominal segments and two soldered ones (the pygidium).]

„ 16. Egg of *Limulus polyphemus*: *a*, the chorion; *b*, the exochorian (after Dohrn *).

„ 17. Third stage in the embryo of *Limulus*: *a*, chorion; *b*, exochorion (after Packard †).

„ 18. Fourth stage (?) in the embryo of *Limulus* (after Dr Packard's † figure).

„ 19. Fourth stage (?) in the embryo of *Limulus*: 1, antennules; 2, antenna; 3–6, maxillipeds; 7 and 8, thoracic plates afterwards bearing the branchiæ; *m*, the mouth; *x*, the ovarian apertures (?); *a*, the abdomen (after Dohrn *).

„ 20. Fifth stage (?) of embryo of *Limulus* (after Dohrn *). At this stage the exochorion is split, and the chorion is expanded by the admission of water by endosmose, in which the embryo is seen to revolve.

„ 21. Ninth stage (?) of embryo, 'just before hatching' (after Packard †): dorsal aspect.

„ 22. The same: side view of embryo.

„ 23. Larva of *Limulus* recently hatched (after Packard †).

„ 24. Larva of *Limulus* on hatching (the "*Trilobitenstadium*" of Dohrn *).

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* "Zur Embryologie und Morphologie des *Limulus polyphemus*." Von Dr. Anton Dohrn. (Jenaische Zeitschrift, Band vi. Heft 4, p. 580, Tafeln xiv. xv.) Received September 30, 1871.

† "On the Embryology of *Limulus polyphemus*." By A. S. Packard, jun., M.D. Read before the American Association for the Advancement of Science, August 1870. ("American Naturalist," vol. iv. No. 8, 1870, October, p. 498.)

REVIEWS.

LYELL'S PRINCIPLES OF GEOLOGY.*

THE second volume of our greatest geological masterpiece has appeared. It is now in its eleventh edition, and it is, as it has been since its first appearance, unquestionably the largest, fullest, and most interesting work upon the principles of geological science which the world has produced. It is marvellous how an author who has been so long before the public is withal so ready to acknowledge himself mistaken whenever clearly established evidence is brought before him. And this we consider the greatest feature in Sir Charles Lyell's character; for we have not to travel far to find other masters, in their peculiar department, who have their ideas as firmly fixed now as they were when first they appeared upon the literary world. But the author of the splendid volume before us is not of that class. On the contrary, we find in this volume that he has considered all that has been written since the last edition was published, and that he has in every case dealt in the fairest manner, even with antagonists whose rivalry is unquestionable. In this volume the subject of volcanoes forms the contents of the first couple of chapters. Then follows earthquakes, dealing with those of the present and past centuries. Next in order we are treated to the elevation and subsidence of land without earthquakes; and, lastly, to the cause of earthquakes and volcanoes. This completes Book II., and it leads us on to the most interesting as well as most novel portion of the present volume, that which deals with the changes of the organic world which are now in progress. Under this general heading we find about four hundred pages devoted to the following highly interesting subjects:—Lamarck, on the transmutation of species; theories as to the nature of species and Darwin's views; variation of plants and animals under domestication, viewed as bearing on the origin of species; natural selection; the geographical distribution of species; the migration and diffusion of terrestrial animals; insular floras and faunas, considered with reference to the origin of species; extinction of species; man, considered with reference to his origin and geographical distribution; and, lastly, the en-

* "Principles of Geology; or, the Modern Changes of the Earth and its Inhabitants, considered as illustrative of Geology." By Sir Charles Lyell, Bart., M.A., F.R.S. Eleventh edition. Vol. II. London: John Murray, 1872.

closing of fossils in peat, blown sand, and volcanic ejections. The reader may readily perceive that within this list of headings is included, if not all, at least the great proportion of those subjects which are at the present moment of the very highest interest to men who have even the slightest consideration for the great secrets and truths of science. It is clear that, even if we confined our attention to the more distinctly novel portions of the volume, we should far exceed the limits of our reviewing chapter. To deal even briefly with them would be more than we can do. We shall, therefore, just give the headings of the absolutely new additions to the volume under notice, and afterwards we shall treat of one or two subjects which appear to us to merit our special attention. The peculiar novelties of this portion of the work are the following: the New Zealand geysers, and reference to Dr. Tyndall's illustration of the probable mode of geyser-action; Mr. Scrope, on the action of water in volcanoes; Sir John Herschel and Mr. Babbage, on transfer of sediment causing the shifting of the subterranean isothermals; Mr. Wallace, on the single origin of the dog; Mr. Darwin, on sexual selection; the Rev. R. T. Lowe, on the arrival of a flight of locusts in Madeira; Mr. Darwin, on some cases of abnormal structure in prehistoric man corresponding to the structure of the same parts in some lower groups of animals; Mr. Mivart's objections to the theory of natural selection, and Mr. Darwin's reply; temperature and fauna of Lake Superior; and, finally, the depth to which the ocean is inhabited, as illustrated by deep sea-dredgings, and the amount of difference of the oceanic fauna in adjoining warm and cold areas. Now, the matter entirely new that is introduced under these successive heads is very extensive. We shall, therefore, confine our attention to one or two of the recent points. And firstly, of the dog, as to whether he comes from several different progenitors or from one single animal, the wolf. Sir Charles Lyell says, that in regard to the origin of the various canine races which have been domesticated by man in all parts of the world, there is still no small diversity of opinion. Mr. Darwin, after an analysis of all that has been written on the subject, inclines to Pallas's belief, that the dog has a multiple origin; for example, that more than one species has been blended together to produce our present distinct races. John Hunter contended that dog, wolf, and jackall were all one species, because he had found that the dog would breed with the wolf and jackall, and that the mule in each case would breed with the dog. "In these cases, however," says Sir Charles Lyell, "it may be observed that there was always one parent at least of pure breed, and no proof was obtained that a true hybrid race would be perpetuated." "The main argument in favour of different breeds of dogs being the descendants of *distinct* wild stocks is their resemblance," says Mr. Darwin, "in various countries to indigenous species still existing there. For instance, the domestic dogs of the American Indians resemble North American wolves; the shepherd dog of Hungary is extremely like the European wolf; the domestic dog of Asia resembles the jackall." But Mr. Wallace has suggested to Sir C. Lyell that evidence of this nature loses much of its weight when we take into consideration some cases of modification given by him and Mr. Darwin, and cited by Mr. Mivart in his "Genesis of Species." Many distinct species of butterflies are shown to be similarly modified in the same localities; in some districts acquiring more elongated wings, in others losing their tails, in others again becoming enlarged or diminished in size, and so on. Mr. Meehan has shown the same thing in regard to American and European trees. So also has M. Coste shown that English oysters, when placed in the Mediterranean, become like the native oysters, and soon acquire prominent converging ribs like those of the Mediterranean oyster. The same thing may be said of the Cape hunting-dog and the Aard wolf, both of which bear a strong external resemblance to the Hyænas. Thus Mr. Wallace thinks it more probable that the dogs of various regions have been thus modified so as to correspond to native foxes, wolves, and jackalls, than that they should have been descended from such very distinct species, "and have mysteriously acquired the power of breeding together and producing fertile offspring, which these species themselves do not possess."

In regard to sexual selection also Sir Charles Lyell makes some important remarks. Indeed, in this respect he seems almost to have accepted Mr. Darwin's ideas. Thus he says that a considerable number of the most striking external characters of animals are confined to one sex, such as the horns and canine tusks often found in the males only of quadrupeds, the ornamental plumes, gay colours, and musical voices of male birds, and the varied horns and excrescences of male insects. "Mr. Darwin has shown that these characters are often useful to the males in their struggle for mates. Some actually fight together, and the possessor of the greatest strength and the best weapons will be the parent of the next generation; others captivate the females by their beauty or their song, and these, by obtaining the earliest and most vigorous mates, will have the most numerous and healthy offspring. Favoured individuals will thus have an advantage in the transmission of these peculiarities; and in this manner Mr. Darwin believes have been produced the noble antlers of the stag, the sharp spurs of the cock, and the gorgeous train of the bird of Paradise. Sexual selection thus becomes an important supplement to natural selection, and may enable us to account for structures which could not be explained by the mere 'preservation of favourable variations in the struggle for life.'"

There is one more point to which we would refer ere we close our notice of this remarkable work. It has reference especially to the Darwinian view of our origin from the lower animals, and it certainly bears very strongly upon it. To a rational mind there seems to be very little difficulty in believing that man has proceeded from some of the Quadrumana. But to those who are differently constituted it is otherwise, and hence every argument in favour of the view is necessary to be brought forward. The present argument is comparatively a recent one, and assuredly it is very forcible. It is as to the resemblance between the humerus of prehistoric man and that of the Quadrumana. For example, in the latter and the carnivora there is a passage near the lower end of the humerus, called the supra-condyloid foramen, through which the great nerve of the fore-limb passes and often the great artery. Now occasionally this foramen occurs in man with the nerve passing through it, and it is remarkable that the percentage of the occurrence of this variation is greater in ancient than in modern races in the proportion of nearly 30 to 1. This has been ascertained by the examination of large numbers of arm-bones of the Bronze and Reindeer periods, and Mr. Darwin remarks that one chief cause of this near approach of ancient races to the type of structure of the lower animals seems to be "that ancient races stand somewhat nearer in the long line of descent to their remote animal-like progenitors than do the modern races."

But we must draw our notice of this attractive book to a close, and while we do so we cannot help offering our best thanks to the author for his laborious efforts in producing edition after edition of his several works. And we would also observe that our gratitude is due not only for the masterly character of the labour but also for the manly and fearless manner in which our author expresses opinions which, though he must know them to be obnoxious to many, he nevertheless expresses as the only results of calm, careful, and conscientious scientific research.

PATTERNS FOR TURNING.*

THIS is a curious book, by no means a common one, and one which we should think will be very highly prized by all who understand practically the principle of the lathe. For all others it is a book which is, from its indulgence in technicality, utterly unintelligible. It is, however, to the expert a work which he must value from the number of hints and descriptions of novel processes which it contains. Besides, it has nearly seventy plates of figures cut by the lathe, which the author assures us have been executed by himself. To one who is inexperienced in the principles of turning they do not seem particularly attractive, being all more or less like the other. But then we fancy the "Turner" will esteem them very highly, and doubtless they look even better upon the block as cut by the lathe than they do in the engraving. However, we think it a pity the author did address himself to beginners exclusively, for we believe it to be impossible for one who is not already acquainted with the lathe to learn anything whatever from Mr. Elphinstone's book. Still the "Turner" *par excellence* will find it most interesting. The patterns contained in the book can be cut, the author says, on a lathe furnished with a division plate, an ornamental slide rest, an eccentric cutting frame, and an overhead motion. Furthermore it is asserted that most of the patterns can be cut by anyone who can add and subtract numbers containing two figures and the fractions $\frac{1}{4}$, $\frac{1}{2}$, and $\frac{3}{4}$. The book is handsomely turned out.

THE SMITHSONIAN REPORT.†

THE Smithsonian Report for 1871, which has been recently issued, is, though late in its date of publication, a most interesting volume, partly filled of course with the monetary concerns of the Institution which

* Patterns for Turning, comprising Elliptical and other Figures cut on the Lathe *without* the use of any Ornamental Chuck. By H. W. Elphinstone; with 70 illustrations. London: John Murray, 1872.

† Annual Report of the Board of Regents of the Smithsonian Institution, for the year 1870, at Washington. Government Printing Office, 1871.

publishes it, but in great portion occupied with a series of papers of the utmost interest and importance to scientific men. It is indeed an admirable work, and is, we should think, first *par excellence* of all scientific annuals in the United States. Certainly a great deal of credit is due to the general experience of the Editor, and to the skill by which he selects both articles and writers. The articles in the present volume have most of them a high value, and though some of them are translations of not extremely recent papers, still they have an importance from the fact of their being valuable additions to the particular branch to which they belong, and also from the circumstances that few of the great mass of scientific readers are sufficiently acquainted with German or French to take them in satisfactorily in their original shape. The contents of the general appendix are as follows:—A Eulogy on A. D. Bache, by Professor Joseph Henry; Lecture on Switzerland, by the subject of the preceding paper; on a Physical Observatory, by Professor J. Henry; the History of My Youth, an autobiography of Francis Arago; Eulogy on Herschel, by M. Arago; Life and Labours of H. G. Magnus, from the "Archives des Sciences"; The Life of Professor Chester Dewey, LL.D., by M. T. Anderson, LL.D.; On the Nature and Origin of Force, by W. B. Taylor; Induction and Deduction, a discourse by Justus Baron von Liebig; The Relation of Food to Work, and its Bearing on Medical Practice, by the Rev. Samuel Haughton; Hydrogen as a Gas and as a Metal, by Dr. J. Emerson Reynolds; On the Identification of the Artisan and the Artist, by the late Cardinal Wiseman; and on the Diamond and other Precious Stones, by M. Babinet. Now, besides all these, most of which are really papers of exceeding interest, there are a series of smaller, more numerous, and equally valuable papers on Ethnology, Physics, and Meteorology by various foreign and American authors, thus completing a volume which is most creditable to the Institution from which it issues, unhappily a little too late. It seems to us that of all the papers which the work contains those on Gustavus Magnus, on Induction and Deduction, and on the Relation of Food and Work, are unquestionably the best in point of style and in regard to the matter they bring forward for discussion. Whilst, unquestionably, the weakest article in the volume, as well as the most antique, is that of Cardinal Wiseman on the Artisan and Artist; and we say so from no religious prejudice, for some of the Cardinal's writings we particularly esteem. But of the three articles we have mentioned, Liebig's is unquestionably the most interesting, the Life of Magnus being short and very general, and Professor Haughton's paper having been very thoroughly brought before London readers in other journals. But Baron Liebig's paper, while strictly scientific, is withal a contribution which may be read with interest by any intelligent reader. It deals both scientifically and popularly with the subject, and shows that what too many imagine, viz., that a knowledge of logic must precede discovery, is by no means true. In science as well as in common life the operations of the mind are executed, not according to the rules of logic, but the conception of a truth; the idea of a process or the cause of a phenomenon generally precedes the demonstration; the conclusion is not reached through the premises, but the conclusion goes before, and the premises are then first sought out as proof. Baron Liebig says that "in a conversation with a celebrated French mathematician on the part which the imaginative faculty bears in scientific labours, he expressed himself to the effect that by far the greater number of mathematical truths are obtained, not by deduction, but through the inventive or imaginative power, and in this he had a view even to the properties of the triangle, the ellipsis, &c., which is saying little else than that the mathematician as well as the physicist can do nothing for his science without artistic endowment." We have selected the foregoing as a sample of the Baron's ideas, and we doubt not that most of our readers will thoroughly agree with them.

The other papers are many of them of exceeding interest, and make the work a most interesting and valuable one.

DISEASE GERMS.*

THE second edition of this important work has appeared, and it demands a careful perusal by all who are at all interested in the subject. It is first of all, we may say, the only book of its kind in our language, and it is written by a man who must unquestionably be considered the first microscopic observer in the world; for he is one who has gone on steadily with his observations for a considerable number of years, and he is the only one who has *worked* (we use the word advisedly) with the highest microscopic powers, the $\frac{1}{25}$ and $\frac{1}{50}$ of an inch. Now from these circumstances alone considerable attention should be given to his writings. But when we further regard him as a professor in a vast public school of medicine, and as the author of an admirable manual of microscopic enquiry, we find ample testimony to his high character as an observer and a teacher. The views which the author lays down in this volume may in some cases be incorrect, though we do not say so; but assuredly they must be regarded as sound, till some one, with equal microscopic powers and experience, comes forward to demonstrate the contrary. The book is, in fact, an expansion of the Radcliffe lectures which Dr. Beale delivered a couple of years since, at Oxford; and the views it sets forth are given clearly, and while without that emphaticism which so readily suggests inexperience to the critical reader, they are nevertheless put forward as the strong and serious expression of opinion of one who, while he is ready to be corrected, is nevertheless hopeful of the truth of his opinions. The first few pages contain a stinging assault on some of the more recent investigators or rather writers and lecturers on the important subject of this volume. In this we fancy the writer has gone out of his way to take up the consideration of views and opinions which were not considered by the public as of more than a passing interest. We think, nevertheless, that Dr. Beale's defence of the late Dr. Budd is both well-timed and in good taste, which of course we cannot say for the views which he opposes. *Bioplasm* appears to us a very good term, and we think that Dr. Beale has much in his favour as he traces its growth. But

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* "Disease Germs; their Nature and Origin." By Lionel S. Beale, M.B., F.R.S., Physician to King's College Hospital. Second Edition. London: J. & A. Churchill, 1872.

we think that it is yet almost too early to consider as decided the question as to how contagiousness arises. We ourselves imagine that there is more to be said in favour of the fungous origin of disease, though in certain cases it appears almost impossible to trace the connection between the disease and living contagious organisms. But unquestionably, if such are to be found, they will be found by Dr. Beale and those who work under similar conditions; and they have not yet been discovered in particular cases. He believes, so far as we can see, that diseases are caught by the introduction of certain matter of an organic character, but not of the fungoid or algal nature. And, furthermore, he believes that these organisms, which are exceedingly minute, have not always existed, but have been called into life by the possible absence of hygienic care, which is too common among all large populations, and that they may be eventually banished after a couple of generations. It is this question which the book undertakes to deal with, and which is treated of lengthily in 450 pages of excellent type, accompanied by 28 plates, in three colours, of the principal subjects, which the author thinks help to illustrate the subject. Altogether the book is an excellent one, written in a healthy, clear, and forcible style, and containing plates which find their equal in no microscopical publication on this side of the Channel.

HALF-HOURS ON THE SHORE.*

THE love of natural history has now become so prevalent, at least among purely English readers, that we hardly meet a family at the sea-side, one of whose members has not some little knowledge of the wonders of the deep. Now, of course, this love of marine zoology is being vastly increased by the existence of the valuable Aquaria at the Crystal Palace and at Brighton. Still, however, notwithstanding the amount of admirable works on the subject, more especially the excellent treatises of Gosse and others, there was wanted a cheap form of book with good illustrations which should give a clear account of the ordinary creatures one meets with on the sands and in the rock pools. The want no longer exists, for the excellent little manual that now lies before us embraces all that could be desired by those who are entirely ignorant of the subject of sea-side zoology, while its mode of arrangement and woodcuts, which are carefully drawn and admirably coloured, combine to render it both attractive and useful. It is, as its name implies, divided into a number of chapters called half-hours, and which deal with the following subjects:—The Waves; Preparations; Sea-weeds; Sponges; Sea-worms; Corallines; Jelly-fish; Sea-anemones; Sea-mats and Squirts; Sea-urchins and Star-fish; Univalve Shell-fish; Bivalve Shell-fish, and lastly Crustacea. Under these separate headings are given clear and accurate accounts of the several groups which are abundantly figured in 148 cuts. Finally the book is excellently printed in large clear type.

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* Half-Hours at the Sea-side; or, Recreations with Marine Objects. By J. E. Taylor, F.G.S. London: R. Hardwicke, 1872.

THE MARTYRDOM OF MAN.*

MR. WINWOODE READE, in his early work on Africa, while his style was repulsive to many readers, nevertheless gave a good account of his travels. In the present work he has given us over five hundred pages of matter, which is simply a very shallow rendering, in one volume, of the more immoral portions of the popular histories of some four or five nations. The book, while intentionally it has an aim, has really none whatever. It is marvellously constructed, no succeeding chapter having any relation to its predecessor, nor anything in the form of a vein of continuity running through the book. Towards the end we are treated to the author's views upon religion, and in this portion, while we agree with him in some of his views, we must say that he has not the slightest novelty to introduce, and that he argues in favour of his views with a poverty of logic that is somewhat astonishing in one who writes upon such a subject at all. There is but one good thing in the book, and that is the style in which it is written. Evidently the author has been at pains in the mode of expressing his ideas, and has doubtless paused considerably and erased much ere he gave his copy to the printer. So that on the whole a feeling of unpleasantness steals over the writer as he imagines the pain and difficulty of composition. Still we must admit that it is very good, the mode of expression being throughout terse, clear, and sometimes even epigrammatic. But when we have said this we have given every particle of praise which can be bestowed on a book that is really without aim or object of any kind save the gratification of the foolish ambition of having written. It seems to us as though Mr. Reade wanted experience, as though he had but a capacity of seeing himself and his own views alone, and thus he was led to write for others that which they themselves understand infinitely more fully and better than he does. While we agree with him in the doctrines he holds, so far as we can see, we are somewhat astonished at the logical accuracy of some of his stated conclusions. "In the first place," says he, "we shall state as an *incontrovertible* maxim in morality that a God has no right to create men except for their own good." Now we should very much like to know on what principle or by what *data* in the laws of morality Mr. Reade arrives at this conclusion. Most assuredly we fail entirely to see it, as indeed we fail throughout his whole argument to perceive any but the most shallow reasoning, put forward in terse style, and an intense amount of egotism unalloyed by anything in the shape of respect for the opinions of others. Nevertheless it is an amusing, well-written book, which those who agree with the author will be interested by reading. There is a freshness in its want of generalisation, and an amusing absence of consideration for others which we doubt not will prove refreshing to the mind of an average reader.

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* The Martyrdom of Man. By Winwoode Reade. London: Trübner and Co., 1872.

RICHARD TREVITHICK.*

ASSUREDLY it must have been a labour of love on the part of the son, who has given us these two large volumes on the life of Richard Trevithick. If he expects to make money out of them, he must be vain indeed. Still we must hope that to those interested in the progress of mining, engines, and machinery they will have a certain value and importance. Truly the man whose life is here given must have been devoted to the pursuit he was engaged in, for he seems to have been during his whole life attached to the invention of machines for rendering mining more simple and expeditious; and numerous indeed appear to have been his inventions. We do not perceive that he possessed any of that great inventive power so common to Watt and Stephenson. Nor do we find in his life any of those occasions in which, as it were, a man is led back from his special calling to the more general concerns of mankind. This may be, however, from the fact that the present writer had not the materials at his command. Therefore the volumes, while they must have a particular interest for the engineer, are devoid of those incidents which render a book of the kind attractive to the reader of biography. It seems to us that the author had hardly the capacity for a biographer, and that he would have done better had he placed the task in other hands. Still, the book is not without interest to the practical engineer.

SHORT NOTICES.

Annual Record of Science and Industry for 1871. Edited by S. F. Baird. New York: Harper Brothers, 1872. This is the first attempt in America to publish a yearly record of the progress in different branches of science. It includes every branch, and does not, so far as we have seen, give to any one department greater space than another. It seems to be very fairly done; but we must assure the editor, that unless he covers at least three times the space at present occupied, he cannot expect to produce anything like a tolerably faithful record. We would urge on him also the propriety of considerably shortening some of the paragraphs, and of totally excluding others. It seems a good work, and we hope to see the succeeding volumes, on the plan we suggest.

Magnetism and the Direction of the Compass. By John Merrifield, LL.D., F.R.A.S. London: Longmans, 1872. This is a little manual for the use of students in navigation and science schools, and it appears to be very well prepared. It deals both practically and scientifically with the several questions arising out of the deviation of the compass, and is clearly a practical and useful volume.

(*Other notices are pressed out through want of space in this, the index No. of "Popular Science Review."*)

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* Life of Richard Trevithick, with an Account of his Inventions. By Francis Trevithick, C.E. Wood engravings by W. J. Welch. 2 vols. London: E. & F. Spon, 1872.

SCIENTIFIC SUMMARY.

ASTRONOMY.

DR. DE LA RUE'S address,* to Section A. of the British Association, was one of the features of the recent meeting. We quote from it the following passages :—

The Solar Corona.—"The great problem of the solar origin of that portion of the corona which extends more than a million of miles beyond the body of the sun has been by the photographic observations of Colonel Tennant and Lord Lindsay in 1871 set finally at rest," says Dr. De la Rue, "after having been the subject of a great amount of discussion for some years. The spectroscopic discovery in 1869 of the now famous green line, 1474 K, demonstrated undoubtedly the self-luminosity, and hence the solar origin of part of the corona. Those who denied the possibility of any extensive atmosphere above the chromosphere received the observation with great suspicion; but in 1870 and again in 1871 it was fully verified. So far, therefore, the testimony of spectroscopic observations was in favour of the solar origin of the inner corona. Indeed the observations of 1871 have proved hydrogen to be also an essential constituent of the 'coronal atmosphere,' as Janssen proposes to call it—hydrogen at a lower temperature and density, of course, than in the chromosphere. Janssen was further so fortunate as to catch glimpses of some of the dark lines of the solar spectrum in the coronal light, an observation which goes far to show that in the upper atmosphere of the sun there are also solid or liquid particles, like smoke or cloud, which reflect the sunlight from below. Many problems, however, even with reference to the admittedly solar part of the corona, are unsettled. The first relates to the nature of the substance which produces the line 1474 K. Since it coincides with a line in the spectrum of iron, it is by many considered due to that metal; but then we must suppose either that iron vapour is less dense than hydrogen gas, or that it is subject to some peculiar solar repulsion which maintains it at its elevation, or other hypotheses may be suggested for explaining the fact. Since the line is one of the least conspicuous in the spectrum of iron and the shortest, and as none of the others are found associated with it in the coronal spectrum, it seems natural, as many have done, to assume at once that it is due to some

* Dr. De la Rue on "Recent Astronomical Progress."

new kind of matter. But the observations of Angström, Roscoe, and Clifton, and recently those of Schuster regarding the spectrum of nitrogen, render it probable that elementary bodies have only one spectrum: and since in all experimental spectra we necessarily operate only on a small thickness of a substance, we cannot say what new lines may be given out in cases where there is an immense thickness of vapour; and hence we cannot conclude with certainty that because there is an unknown line in the chromosphere or corona, it implies a new substance."

On the photographic evidence respecting the corona, Dr. De la Rue remarks, "If the rays and rifts were really atmospheric, it would hardly be possible that they should present the same appearance at different stations along the line of totality; indeed they would probably change their appearance every moment, even at the same station. If they are cislunar, the same appearances could not be recorded at distant stations. It is universally admitted that proof of the invariability of these markings, and especially of their identity as seen at widely separated stations, would amount to a demonstration of their extraterrestrial origin. Eye-sketches cannot be depended on; the drawings made by persons standing side by side differ often to an extent that is most perplexing. Now photographs have, undoubtedly, as yet failed to catch many of the faint markings and delicate details; but their testimony, as far as it goes, is unimpeachable. In 1870, Lord Lindsay at Santa Maria, Professor Winlock at Jerez, Mr. Brothers at Syracuse, obtained pictures, some of which, on account partly of the unsatisfactory state of the weather, could not compare with Mr. Brothers's picture obtained with an instrument of special construction; * but all show one deep rift especially, which seemed to cut down through both the outer and inner corona clear to the limb of the moon. Even to the naked eye it was one of the most conspicuous features of the eclipse. Many other points of detail also come out identical in the Spanish and Sicilian pictures.

"None of the photographs of 1871, by Colonel Tennant and Lord Lindsay's photographic assistant, Mr. Davis, shows so great an extension of the corona as is seen in Mr. Brothers's photograph, taken at Syracuse in 1870; but, on the other hand, the coronal features are perfectly defined on the several pictures, and the number of the photographs renders the value of the series singularly great. . . . We have in all the views the same extensive corona, with persistent rifts similarly situated. Moreover, there is additional evidence indicated by the motion of the moon across the solar atmospheric appendages, proving, in a similar manner as in 1860 in reference to the protuberances, the solar origin of that part of the corona."

The Sun's Complex Atmosphere.—Dr. De la Rue says, "On the long disputed question of the complex atmosphere below the chromosphere, in connection with the solution of the most prominent questions connected with

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* "Mr. Brothers had, in 1870, the happy idea to employ a so-called rapid rectilinear photographic lens, made by Dallmeyer, of 4 inches aperture and 30 inches focal length, mounted equatorially, and driven by clockwork; and he was followed in this matter by both Col. Tennant and Lord Lindsay in 1871. The focal image produced, however, is far too small ($\frac{3}{10}$ of an inch, about); therefore it will be desirable in future to prepare lenses of similar construction, but of longer focal length and corresponding aperture."

the solar envelopes, it may not be without great interest to allude another point conclusively decided during the last annular eclipse of the sun, observed by Mr. Pogson on June 6 of this year, as described by him in a letter to Sir George B. Airy. In 1870 Professor Young was the first to observe the reversal of the Fraunhofer lines in the stratum closest to the sun. Now, in 1871 doubts were thrown upon the subject. It appears that the reversed lines seem to have been satisfactorily observed by Captain Maclear at Bekul, Colonel Tennant at Dodabetta, and Captain Fyers at Jaffna. The observations of Pringle at Bekul, Respighi at Poodacottah, and Pogson at Avenashi were doubtful, while Mosely at Trincomalee saw nothing of this reversal, which is, according to all accounts, a most striking phenomenon, although of very short duration. Mr. Lockyer missed it by an accidental derangement of the telescope. The reversal and the physical deductions from it are placed beyond doubt by Mr. Pogson's observations of the annular eclipse on June 6. At the first internal contact, just after a peep in the finder had shown the moon's limb lighted up by the corona, he saw all the dark lines reversed and bright, but for less than two seconds. The sight of beauty above all was, however, the reversion of the lines at the breaking up of the limb. The duration was astonishing—five to seven seconds; and the fading out was gradual, not momentary. This does not accord with Captain Maclear's observations in 1870, who reports the disappearance of the bright spectrum as 'not instantly, but so rapidly that I could not make out the order of their going.' Professor Young, again, says that 'they flashed out like the stars from a rocket-head.' But discrepancies in this minor point may be accounted for by supposing differences in quietude of that portion of the sun's limb last covered by the moon."

Dr. Huggins's Spectroscopic Observations of Nebulæ.—On this subject Dr. De la Rue remarks: "Although, as I have stated, I do not contemplate passing in review recent discoveries in astronomy, I must not omit to call your attention to some few subjects of engrossing interest. First, with reference to the more recent work of Dr. Huggins. In his observations he found that the brightest line of the three bright lines which constitute the spectrum of the gaseous nebulæ was coincident with the brightest of the lines of the spectrum of nitrogen; but the aperture of his telescope did not permit him to ascertain whether the line in the nebulæ was double, as is the case with the line of nitrogen. With the large telescope placed in his hands by the Royal Society, he has found that the line in the nebulæ is not double, and in the case of the great nebula in Orion it coincides in position with the less refrangible of the two lines which make up the corresponding nitrogen-line. He has not yet been able to find a condition of luminous nitrogen in which the line of this gas is single and narrow and defined like the nebular line."

Stellar Motions of Recess and Approach.—"Dr. Huggins has extended," says Dr. De la Rue, "the method of detecting a star's motion in the line of sight by a change of refrangibility in the line of a terrestrial substance existing on the star to about 30 stars besides Sirius. The comparisons have been made with lines of hydrogen, magnesium, and sodium. In consequence of the extreme difficulty of the investigation, the numerical velocities of the stars have been obtained by estimation, and are to be regarded as provisional only. It will be observed that, speaking generally, the stars which the spectroscope shows to be moving from the earth, as Sirius, Betelgeux, Rigel, Procyon, are situated in a part of the heavens opposite to Hercules, towards which the sun is advancing; while the stars in the neighbourhood of this region, as Arcturus, Vega, and α Cygni, show a motion of approach. There are, however, in the stars already observed, exceptions to this general statement; and there are some other considerations, as the relative velocities of the stars, which appear to show that the sun's motion in space is not the only or even in all cases the chief cause of the observed proper motions of the stars. In the observed stellar motions we have to do probably with two other independent motions—namely, a movement common to certain groups of stars and also a motion peculiar to each star. Thus the stars β, γ, δ, ϵ, ζ of the Great Bear, which have similar proper motions, have a common motion of recession; while the star α of the same constellation, which has a proper motion in the opposite direction, is shown by the spectroscope to be approaching the earth. From further researches in this direction, and from an investigation of the motions of stars in the line of sight in conjunction with their proper motions at right angles to the visual direction obtained by the ordinary methods, we may hope to gain some definite knowledge of the constitution of the heavens. This discovery supports, in a somewhat striking manner, "proceeds Dr. De la Rue, "the views which Mr. Proctor has been urging respecting the distribution of the stars in space."

BOTANY.

The Geographical Distribution of Compositæ.—Mr. G. Bentham read a paper on this subject at two meetings of the Linnæan Society, in continuation of his paper on the structure of the same order of plants. The genera and species of this largest order of flowering plants are about equally distributed between the Old and New World; of the genera, about 410 are found in the former and 430 in the latter; of species, about 4,400 in the Old World and a rather larger number in the New. Not quite 70 species are common to the two hemispheres, and these mostly belong to the extreme northern regions; a few are common to New Zealand and Antarctic America; not more than a dozen tropical species are found in both the Old and New World, and some of these are coast plants. The form which Mr. Bentham looks on as prototypic, and possibly ancestral to the whole order, includes a few closely allied genera, distinguished by their regular corolla, belonging rather more to the American than the Old World distribution, being found in Chili, with an outlying genus in St. Helena. Other types, apparently of great antiquity, are found in Africa, Australia, and Western America. Since the separation of the Indo-Malayan and Australian regions from one another, there appears to have been a continuity of races of Compositæ across the tropics from south to north. The paper, which enters exhaustively into the distribution of the various tribes and more important genera, is published in the "Journal of the Linnæan Society."

The Formation of Ozone by Flowers.—It is stated by the *Academy* that Mantegazza has found ("Rendiconti del Reale Instituto Lombardo," vol. iii. fasc. vi., abstracted in "Der Naturforscher," April 27) that many essential oils, like that of peppermint, turpentine, oil of cloves, lavender, bergamot, aniseed, nutmeg, thyme, and others, when in contact with the oxygen of the atmosphere in presence of sunlight, develop very large quantities of ozone. The oxidation of these oils is, in fact, a very convenient source of ozone, as they, even in small quantities, ozonise much oxygen. The action is strongest in direct sunlight, far less so in suffused daylight, and very weak or at an end in the dark. The development of ozone which has been begun in the light continues for a long time in darkness. In the same manner act eau-de-cologne, hydromel, and other aromatic tinctures on exposure to the solar rays. Experiments which Mantegazza has made on flowers with powerful perfume, such as the narcissus, hyacinth, heliotrope, mignonette, and others, in closed vessels, proved that they also form ozone. Those with fainter perfume produced less ozone, those without scent none at all. Mantegazza believes that this important source of ozone is of hygienic value for the purification of the air of marshy districts.

Absorption of Water by Leaves under certain circumstances.—The statements of Duchartre and others, that leaves absorb neither water with which they are wetted nor vapour from surrounding air, have been fully confirmed by M. Cailletet so far as respects the foliage of plants established in the soil and supplied with moisture by the roots. But the revival of wilting leaves when sprinkled or enclosed in a moist atmosphere, however ingeniously explained away, always seemed to bear testimony to absorption. And M. Cailletet's experiments go to show, incontestably, that foliage does absorb liquid water (but not watery vapour) when supply by the root fails or is arrested. His experiments were made by introducing a leafy branch into a glass vessel with a double tubulure, filled with water, the increase or diminution of which was accurately and delicately measured by a manometer. A Bromeliaceous epiphyte, which grew under his care for six years, suspended by a fine wire without root, he found, was able to absorb and fix more than a hundredth part of its weight of water upon a short immersion. His note upon the subject was presented to the French Academy, and is printed in "Ann. Sci. Nat.," vol. xiv. p. 243.

Change of Habit.—It is stated in "The Garden," that the *Loranthus macranthus* of New Zealand, parasitic there upon trees of *Rutaceæ* and *Violaceæ*, is deserting these in favour of trees introduced by the European settlers, such as hawthorn, plum, peach, and especially laburnum, which was introduced as lately as 1859. Its flowers are abundantly visited by the European honey-bee.

Double Flowers of Ranunculus Rhomboideus.—A correspondent of the "American Naturalist" has found several perfect double flowers of this plant in Floyd County, Iowa. One of them was transferred to a bed in the flower-garden, where it throve well, and increased to a clump six inches in diameter, which, the next spring, was perfectly enveloped with the little bright yellow flowers. No seeds were produced; and during the two or three seasons in which the plant was cared for there was no sign of change to single flowers. The plant was quite attractive; and as it blooms very early and profusely, it would seem to have some of the qualities required to entitle it to a place among cultivated flowers.

CHEMISTRY.

Detection of Nitrogenised Matter in the Atmosphere.—Mr. A. H. Smee has described ("Chemical News," July 19) a method which he has devised, and which he names "distillation by cold," by which he believes the detection and determination of ammonia and other organic impurities existing in the atmosphere will be greatly facilitated. A glass funnel (usually of 8 or 9 inches) is drawn to a point and closed. It is supported in an ordinary stand, and filled with ice. Condensation of the watery vapour of the atmosphere then takes place; the dew collects into drops, which trickle down the outside of the funnel, and at last fall from the point, under which a small receiver is placed to catch them. The total quantity of liquid collected in a given time is measured, and the quantity of ammonia determined by Nessler's test. By the method of distillation by cold, the author found it possible to distil many substances which are decomposed at a high temperature. Thus many delicate odours of flowers were distilled by placing the flowers under a bell-glass sufficiently large to cover the funnel containing the ice. The odours were found to be more rapidly and completely abstracted by placing a dish with a little ether under the bell-glass at the time of distillation. The paper was accompanied by tables giving the results obtained in 107 experiments, together with the atmospheric conditions prevailing at the time. The experiments were made in a garden, in a bed-room, in hospital wards, in the open country, &c.

A Method of obtaining Potassium.—Professor A. Dolbear has described the following mode:—Some white stick caustic potash of commerce is dissolved in water and then treated with sulphuretted hydrogen in the way commonly described for making potassium sulphide, K_2S. The solution is evaporated until it is solid when cool, when the yellowish mass is mixed with more than its bulk of iron filings and chips, and the whole put into an alembic for distillation. The heat of a furnace is applied till the alembic is of a bright red heat, and the products of distillation are received in common coal oil. The product is rather small, as some of the potassium vapour decomposes the heated vessel; nevertheless the potassium shows itself when the oil is poured off, and the residuum turned upon water, by its characteristic ignition and flame. The reaction is simple, and may be thus represented. $K_2S + Fe = FeS + K_2$.

Chemical Action during a number of Years.—In the "Comptes Rendus," July 8, M. Becquerel has a paper which contains the account of the results of some experiments which prove that, by slow but continued action, curious effects may be obtained; for instance: Crystals of arragonite are formed upon a piece of gypsum (lance-shaped variety) kept in a closed vessel in a solution of bicarbonate of potassa; the gypsum has almost entirely disappeared, having become converted into arragonite. A similar piece of gypsum, placed in a solution of arseniate of ammonia, has been converted into arseniate of lime. With a solution of aluminate of potassa and gypsum, the result is the formation of glauberite (double sulphate of lime and potassa, the latter substituted for the soda in the mineral just named). Pieces of galena, kept for twenty years in a solution of bicarbonate of potassa, have yielded well-defined crystallised carbonate of lead. When pieces of limestone were immersed into a solution of plombite of potassa (solution of oxide of lead in caustic potassa), the result was the formation of beautifully-crystallised hydrated carbonate of lead; malachite has been obtained by the action of limestone upon nitrate of copper, converting it into subnitrate, which, in its turn, is converted into malachite by bicarbonate of soda.

The Teaching of Elementary Chemistry.—At the meeting of the British Association (Brighton), Dr. Wood read a paper on this subject. The sum and substance of his communication was that, in his experience, it was impossible to teach practical chemistry successfully in towns or places where there were no regular laboratories, and he strongly advocated the public appointment of such laboratories, where boys could be sent to gain the necessary knowledge. His opinion was that the recognised public examinations in chemistry were too difficult, that the standard was too high for young boys. Dr. Wood also proposed that the several great examining boards shall agree to use a common standard for elementary examinations, and should rigidly adhere to the conspectus. A very full discussion took place, in which some valuable suggestions were made by Professors Williamson, Lemoine, Barrett, and others engaged in the public teaching of chemistry. The president, in summing up the discussion, alluded to the great importance of teaching chemistry to every educated individual, and furthermore called the attention of the Section to the opening address of Dr. B. Sanderson in Section D, who had given a very high opinion of the necessity of chemical knowledge to everyone, irrespective of their calling.

A new Vegetable Ink.—In "Les Mondes," July 4, the editor states, that experiments are being made to acclimatise in Europe the *Coriaria thymifolia*, or ink-plant of New Grenada. The juice of this plant, locally termed *chanchi*, is at first of a somewhat reddish colour, but becomes intensely black in a few hours. This juice can be used for writing without requiring any further preparations; it corrodes steel pens less than ordinary ink, and has, moreover, the advantage of better resisting chemical agents. When the portion of America named above was under Spanish dominion, all public documents were written with chanchi, which was not removed from paper by sea-water.

On Meteoric Iron.—At the British Association, Prof. Mallett, U.S.A., exhibited three specimens of iron from Augusta, co. Virginia. One had been cut from the original mass by a planing machine, and without further preparation had been forged into a tolerably perfect blade for a paper-knife; another had been heated to redness in a vacuous porcelain tube (for the purpose of examining the occluded gases), and had then been with great difficulty forged into a blade of similar kind, in which cracks and flaws were visible. The third had been heated in like manner in vacuo, but to a white heat, and this specimen could not be forged at all. The conceivable causes of this difference were briefly discussed, such as the more or less complete removal of the occluded gases, changed state of combination of the phosphorus and sulphur, and melting out of phosphide of iron, leaving the metal porous.

The Decomposition of Water.—A paper was read at the British Association, "On the Mutual Helpfulness of Chemical Affinity, Heat and Electricity, in producing the Decomposition of Water," by Dr. Gladstone and Mr. A. Tribe. Some metals are able of themselves to displace the hydrogen of pure water, while other metals are unable. Zinc, if perfectly pure, is incapable of doing so; but if it be brought into contact with another metal still further removed from the power of effecting the decomposition of water, the electric force started by contact of the metals enhances the chemical affinity sufficiently to make it effective—or, otherwise expressed, the electrical tension, *plus* the chemical tension, upsets the state of equilibrium between the oxygen and the hydrogen. The amount of action may be measured by a Thompson's galvanometer. The effect of varying the distance of two plates of zinc and copper was tried, and it was found that the chemical action increased slowly till the plates are within an inch or so of each other, but on continuing to bring them together the action increases at a rapidly accelerating ratio. Heat assists the action considerably. Magnesium is capable by itself of decomposing water, but its action is greatly increased by touching it with a piece of copper, and some of the hydrogen gas makes its appearance on the copper. If, instead of magnesium, a metal less capable than zinc of decomposing water be used, there is still found a deflection of the galvanometer, if it be united with a metal still more negative. The order for pure water seems to be—platinum, silver, copper, iron, tin, lead, zinc, magnesium.

The Quality of the Metropolitan Gas.—Dr. Letheby, the chief Gas Examiner appointed by the Board of Trade, has recently reported to the Corporation of the City, and to the Metropolitan Board of Works, on the quality of the gas supplied to the metropolis by the Chartered, the Imperial, and the South Metropolitan Gas Companies, during the last three months; from which it appears that the illuminating power has never been less than that required by the Act of Parliament. The common gas of the Chartered Company has ranged from an average power of 16·89 standard sperm candles at the Millbank testing-place, to 17·85 candles at the testing-place at Mile End. The Imperial Company's gas has ranged from 15·72 candles at Camden Street, to 16·85 at Oakley Square, Chelsea; and the gas of the South Metropolitan Company has averaged 15·78 candles. The cannel gas of the Chartered Company has had an average illuminating power of 25·74 candles at Millbank Street, and 26·06 candles at Arundel Street in the Haymarket. With respect to purity, Dr. Letheby reports that the gas at each of the testing-places has been constantly free from sulphuretted hydrogen, but that the fluctuation in the amounts of sulphur have been considerable, as from a minimum of 2·2 grains per 100 cubic feet of gas at Mile End, to a maximum of over 40 grains at Beckton, at Cannon Street, at Arundel Street, and at Hill Street, Peckham; the average proportions at the different testing-places having been as follows:—The common gas of the Chartered Company at Beckton, 26·98 grains per 100 cubic feet; at Cannon Street 25·77, grains; at Friendly place, Mile End, 8·71 grains; at Arundel Street, Haymarket, 18·89 grains; and at Millbank Street, Westminster, 26·96 grains; while that of the Imperial Company has been 32·98 grains at Oakley Square, Chelsea; 28·16 grains at Camden Street, Camden Town; and 25·71 grains at Graham Street, Dalston. The average amount of sulphur in the South Metropolitan gas was 36·05 grains per 100 cubic feet. It is a noticeable fact that the quantity of sulphur in the gas of the Chartered Company at Friendly Place has only once contained as much as 20 grains per 100 feet; and that during the last month the gas at Beckton, at Cannon Street, at Friendly Place, and at Arundel Street, has never contained 20 grains per 100 feet; the average for the month having been from 4·63 grains at Friendly Place to 13·64 grains at Beckton. Dr. Letheby regards this as highly satisfactory. The proportion of ammonia in the gas has not at any time or place exceeded the quantity (2·5 grains per 100 cubic feet) prescribed by the referees; the average amount being from nil to 1·28 grains per 100 feet.—*The Chemical News, July* 19.

GEOLOGY AND PALÆONTOLOGY.

The Palæontographical Society Volume for 1871, which has only been a few months issued, contains the following important monographs:—1. "The Flora of the Carboniferous Strata." Part III. By E. W. Binney, F.R.S., F.G.S. Pp. 63–96. Pl. xiii.–xviii. 2. "The Fossil Merostomata." Part III. By Henry Woodward, F.G.S., &c. Pp. 71–120. Pl. xvi.–xx. 3. "Supplement to the Crag Mollusca." Part I. By Searles V. Wood, F.G.S. Pp. 1–90. Pl. i.–vii. Together with an "Introductory Outline of the Geology of the same District," with a Map, by S. V. Wood, jun., F.G.S., and F. W. Harmer, F.G.S., pp. i.–xxxi. 4. "Supplement to the Reptilia of the Wealden." Part IV. By Prof. Owen, F.R.S. Pp. 1–15. Pl. i.–iii. 5. "The Pleistocene Mammalia." Part IV. By W. Boyd-Dawkins, M.A., F.R.S., &c., and W. Ayshford Sanford, F.G.S. Pp. 177–194. Pl. xxiv. and xxv. 6. "The Pleistocene Mammalia." Part V. By W. Boyd-Dawkins. M.A., F.G.S., &c. Pp. 1–30. Pl. i.–v.

Geology at the British Association.—The Geological Section began its sittings with the Sixth Report on Fossil Crustacea, by Mr. H. Woodward, F.G.S., in which various newly-discovered species from the silurian, carboniferous, tertiary, and other rocks were minutely described. The next paper was of great importance, by the Rev. J. Gunn, on the Prospect of Finding Productive Coal Measures in Norfolk and Suffolk, with suggestions as to the places best adapted for experimental borings. The next paper was on the Genera Trimerella, Dinobolus, and Monomeralla, by Thos. Davidson, F.R.S., and Professor King. Mr. John Gunn read a paper on the possibility of finding Coal in Norfolk and Suffolk, with suggestions for an experimental boring. Hunstanton was recommended as the most likely spot where coal would be soonest reached. Professor Hull thought that if there were any places where the chances of finding coal were *nil*, it was in the eastern counties. In this opinion Professor Phillips coincided. Mr. Godwin Austen, F.R.S., protested against discouraging a boring, and thought there might be a continuity over Norfolk of the great central coal-fields. He concluded a long address by expressing his opinion that eventually a considerable coal area would be worked beneath the secondary strata. A paper by Professor Rath was next read on a remarkable block of lava ejected by Vesuvius during the great eruption of April 1872, proving the formation of silicates by sublimation. Mr. Gwyn-Jeffreys, F.R.S., followed with some remarks on submarine explorations. Afterwards, Dr. Adams read a report on the Fossil Elephants of Malta; and Professor Boyd-Dawkins, F.R.S., a very interesting communication on the Physical Geography of the Mediterranean during the Pleistocene age. Mr. Charles Moore also read a valuable paper on the Presence of Naked Echinodermata in Oolitic and Liassic Beds; after which Mr. J. E. Lee noticed some veins or fissures in the Keuper-filled Rhætic Bone Bed at Goldcliff, in Monmouthshire. Dr. Sim's paper on Quartz Nodules in the Crystalline Schists of Perthshire was next in order. There were many others of importance read, but our space does not admit of giving them all.

Professor Nordenskiöld's Expedition to Greenland.—Professor Nordenskiöld himself gives an admirable description of his excursion in the "Geological Magazine" for August, and in the preceding and following numbers. The paper is too long for abstract, but should be read by all who are interested in the subject.

Our London Coal-fields as they may be almost termed, were again the subject of consideration at one of the excursions of the British Association. At the excursion to Battle Abbey, Mr. Prestwich, F.R.S., gave a most interesting address on the more theoretical subject. He showed that long before the formation of any member of the secondary formations, there had extended over North Europe an immense land surface, broken up, like that before them, into hills and dales. This area, on which the vegetation of the coal period grew, extended certainly from South Wales to Belgium. It was eventually submerged, and covered over with the later secondary strata. It was originally thought that the latter extended from Bath into France, and that it would be useless to bore through them for coal. But subsequent researches found that these rocks thinned out in the direction of London. They had to eliminate some of the members, so that this circumstance, and their general thinning out, caused geologists to expect that the primary rocks would be found at no great depth. This proved true, for under London the tertiary and chalk strata had been found at Kentish Town resting on the old red sandstone. In Herefordshire, again, the chalk reposed on beds of the same age. At Harwich, in a well-boring, the lower carboniferous rocks were reached, and at Calais, across the sea, the chalk was found lying on the mountain limestone. Thus, positive evidence of the extension of these ancient Palæozoic or primary rocks had been afforded. At Calais, they reached at a depth of 1,300 feet, at Harwich 1,100 feet, and at Kentish Town about the same depth. This furnished the hope that the primary rocks would be found elsewhere under the chalk at a similar depth. The question was, where to go and look for them? All their efforts were only tentative, and many experiments like that before them would have to be carried out before they found out their way. But every step taken was so much ground gained. Mr. Prestwich then showed how the ancient carboniferous formation had been disturbed, and compared all those that lay in a certain direction. Thus, in Belgium, at the foot of the Ardennes, there was a patch of coal-measures forty or fifty miles long, and running in an east and west direction. In Somersetshire the range of hills had the same direction as those of the Ardennes. The coal basin at the latter place had the same strike as those of Bristol and South Wales. The elevation of these took place before the formation of the new red sandstone. The present surface elevation was no guide whatever to the disposition of the ancient rock. In Belgium the coal dipped beneath the chalk, and the question was what became of it? If the strike always followed the same line there would be no difficulty in answering this, but it did not follow a straight line, but was curved, as in the Ardennes Hills. Mr. Godwin Austen thought that the Ardennes chain passed underneath where they stood, but Mr. Prestwich slightly differed from this, and thought the strike and range of the coal-measures in the South Wales coal-field and that of Belgium extended farther north. This was not an experiment to find coal, as had been asserted, but simply a scientific experiment to determine the position of the primary rocks. With other similar borings it might be possible to discern the actual range of the older rocks, and find out whether coal was to be found in Herefordshire or Kent. Where this boring was going on, they might meet with the mountain limestone, but he did not think the actual coal-measures would be found. If they came on the silurian rocks, they must go farther north to search for coal, so that it greatly depended on what they came to underneath them as to whether they should continue.

MEDICAL SCIENCE.

The Coagulation of the Blood.—A short communication on this point was made by Mr. E. A Schafer to the British Association, detailing a variety of experiments, from which the following conclusions were drawn:—That frog's blood, especially if taken during the winter months, exhibits but very little tendency to coagulate, with the exception of the portion in immediate contact with a foreign surface; that when apparently coagulated throughout, the central portions are very apt to remain fluid, and to impart coagulability to the expressed serum; that the clot, when formed, frequently tends to attain a relatively very small bulk; and, finally, that this diminution in bulk is due to contraction merely—not reliquefaction of the fibrine.

Rythmical Variation of Arterial Pressure.—Professor Burdon Sanderson read a paper on this subject at the same meeting as above mentioned. He pointed out the rythmical irregularities observable in the pulsation of certain animals, stating that the periods of fast beating correspond to inspiration, and that the periods of slow beating correspond to expiration; and explaining how this is recorded by a graphic method. Discussing the question of the relation of these phenomena, he showed that, when the muscles are subjected to paralysis, so that respiration almost ceases, there is no variation in pulsation; whence he concluded that the phenomena in question are not related as cause and effect, but are results of the same cause.

Lobes of the Liver in different Animals.—Professor Flower, F.R.S., recently alluded to the great diversities in the arrangement of the lobes of the liver in different animals, which he illustrated by numerous diagrams. He suggested that, in studying these arrangements, the principal thing is to look carefully to the great blood-vessels around which the hepatic substance is grouped, taking the umbilical vein as the central starting-point. Numerous details were given, and suggestions were made, with a view to systematise the arrangement of the lobes in various animals so that the parts may be more accurately described by anatomists than they have hitherto been. In the course of the discussion on the paper, it was stated by Professor Struthers that the liver in the human subject is very unequally divided, the right lobe being much larger than the left, and he suggested that this might have some relation to the strength of the right hand as compared with the left. It was also stated that the old system of tight-lacing often introduced changes of form in the liver, and sometimes an additional fissure was in this way produced.—*Brighton Meeting of the British Association.*

Cutaneous Absorption.—M. Bernard has recently described very fully to the French Academy his observations on the above subject. They are also fully translated in the "Chemical News," July 12. He used a bath apparatus in his experiments, which consisted of a furnace, a boiler, a chamber in which the steam coming from the boiler was charged with the substance to be applied, and a wooden cage, in which the patient was seated while enveloped in the vapour. He used iodide of potassium in his experiments —(1) because it is not volatile; (2) because its presence in urine is easily determined by nitric acid and chloroform; (3) because, in seizing the iodine set at liberty by the nitric acid, the chloroform takes a rose colour varying in a marked way with quantity; and thus, by comparing with a graduated scale, one may determine pretty accurately, and without quantitative analysis, the quantity of iodide of potassium in the urine. The skin of the subjects experimented on was intact, without wound or scratch. The urine was examined before the bath was taken, and the absence of iodine ascertained. By a respiratory tube, the patient breathed the external air through his mouth, the nostrils being pinched. A thick sheet of caoutchouc was bound by a T-bandage over the anus; the penis was sheathed in the same material; while the hands and feet were wrapped in cotton and gummed taffeta. The subject was then placed in the cage, and subjected for thirty minutes to vapour from the mixing chamber, into which there had been put 20 grms. of iodide of potassium. The temperature in the cage was gradually raised to 45°; the skin of the subject became wet. He was then wrapped in a woollen covering and put in bed, when profuse perspiration took place. The urine analysed two hours after the bath gave a rose colour; some taken three hours after gave a much more lively colour: thus affording clear proof of the absorption of iodide of potassium through the skin, the only way it could have entered the system. Besides, if it had entered by pulmonary passages, it would have been eliminated immediately after the bath.

Vital Movements and Electricity.—This subject was part of that of a paper by Dr. C. B. Radcliffe, read before the British Association. The paper was entitled, "Whether the Causes of Vital Movement are yet clearly Appreciated." The paper commenced with a *résumé* of the opinions of ancient and modern writers on the subject of vital movement. He ridiculed the current doctrine which, he said, assumes that vital movements are distinct from those manifested in inert matter, and which can at best be only regarded as a hasty deduction from superficial observations. The highest aspirations of philosophy, he contended, are in direct contradiction to this assumption: and the doctrine of the correlation of physical and vital forces implies a grand centralisation, by which what have been regarded as separate forces are made to appear as various aspects of some central force. Dr. Radcliffe then referred to some investigations which he had recently made with Sir William Thompson's electrometer, and which had tended to confirm the views of Galvani. He mentioned, by way of illustration, a singular experiment that he had made with a strip of india-rubber, coated on the two surfaces with metal, which was elongated by the attraction of two opposite charges; the discharge causing the india-rubber to contract to its original length.

MICROSCOPY.

A valuable *Table of Objectives* has been published in the "Monthly Microscopical Journal" (August). This Table, by Dr. R. H. Ward, is designed to be complete in regard to American lenses, and to present for comparison, and for the convenience of those who desire a variety of lenses by different makers, such European items as could be obtained at the time of writing. It is believed that the Table will be interesting to others than buyers, as showing the prevailing powers, prices, angular apertures, &c., in different parts of the world. Notwithstanding the general impression, that the prices of American objectives are unreasonably high, it will be seen that they compare favourably with those of the most celebrated English makers. Most opticians prepare, very cheaply, mounted objectives of small angle, and usually second quality, which answer a good purpose where economy is imperative and difficult work is not required. Such lenses are not usually priced and sold separately by the American makers, and are therefore not included in their columns in the "Table of Objectives." They can be obtained, however, at prices proportioned to their angles and quality. The prices and angular apertures given in the Table are generally, though not in every case, official and up to date; but a reasonable allowance must be made for practical variation from the standard angles and powers. There is understood to be a screw-collar adjustment for all objectives of over 50° angular aperture, unless otherwise stated; though, from want of sufficiently distinct description by their makers, this rule cannot be applied to some of the English lenses of medium power and small angle. To persons without great experience in microscopy, the best "test" of an objective is the respectability of its maker.

The Sturgeon's Development.—The *Acclimatisation* Society of Paris lately published in its Bulletin an account of the sturgeon's reproduction (sturgeon of the Volga), by Professor Owsjannikow. It seems that the sterlet (*Acipenser ruthenus*), the smallest of the Russian sturgeons, spawns in the Volga early in May on rocky bottoms, the temperature of the water being at 10° R. (=$54\frac{1}{2}$° F.). The eggs are readily fecundated by the artificial method. After they have been in the water a few minutes they adhere to any object which they touch. The development of the embryo can be observed in progress at the end of cne hour. On the seventh day they hatch. At first the young fish are $0^{m}\cdot007$ (about $\frac{27}{100}$ inch) long. At the age of ten weeks they are nearly two inches long. They feed on the larvæ of insects, taking them from the bottom. Both in the egg and when newly hatched, the sterlet has been taken a five days' journey from the Volga to Western Russia, and in 1870 a lot of the eggs was carried to England to stock the River Leith. This species passes its whole life in fresh water. The other species inhabiting the Baltic, *Alcipenser sturio*, *A. Huso*, *A. stellatus*, and *A. Güldenstädtii*, are anadromous. These species hybridise, and freely, and from this circumstance some Russian savants have pronounced them only varieties instead of species.

How should Objectives be Named.—Dr. Ward says that the following are some of the more important *queries* which still remain *open*. Should the standard 1-inch objective be characterised by magnifying ten diameters as used in the compound microscope, or should it be compared to a simple lens of actually measured focus or foci? Should the objective be named by its equivalent focal length, or by its amplifying power, or both? Should our standard distance of measurement be changed from 10 inches (254 millimètres) to $9\frac{5}{6}$ inches (250 millimètres)? From what point in the objective shall the distance to the scale be measured? At what point of screw-collar adjustment shall the objective be placed for rating its angular aperture and amplifying power? Should the name *ocular* be substituted for "eye-piece" in general use?—*Monthly Microscopical Journal* (July).

Various Microscopical Papers.—The following are some of the papers published in the "Monthly Microscopical Journal" for the past three months, July, August, September:—"Micro-Pantograph." By Isaac Roberts, F.G.S. "On Bog Mosses." By R. Braithwaite, M.D., F.L.S. "Remarks on the Homological Position of the Members constituting the Thecated Section of the Class Rotatoria." By Charles Cubitt, F.R.M.S. "Further Remarks on Tolles' $\frac{1}{6}$th, and Powell and Lealand's Immersion $\frac{1}{16}$th." By Edwin Bicknell, Cambridge, Mass. "On Uniformity of Nomenclature in regard to Microscopical Objectives and Oculars." By. R. H. Ward, M.D. "Notes on some Microscopic Organisms." By Prof. A. M. Edwards, U.S.A. "Notes on Victoria Mollusca and their Palates." By Charles M. Maplestone. "The Nerves of Capillary Vessels and their probable Action in Health and Disease." By Dr. Lionel S. Beale, F.R.S., Fellow of the Royal College of Physicians, Physician to King's College Hospital. "Remarks on the Nomenclature of Achromatic Objectives for the Compound Microscope." By Dr. J. J. Woodward, U.S. Army. "Our Present Medical and Students' Microscopes." By R. H. Ward, M.D. "On Euchlanis Triquetra and E. Dilatata." By C. Hudson, LL.D. "On some Methods for Preparing the Tissues of the Frog Tadpole's Tail."

"On the Employment of Coloured Glass in Microscopy." By M. Mouchet. "Note on Silicious Incrustations from the Geysers of the Yellowstone River, Wyoming Territory." By Prof. Arthur Mead Edwards, U.S.A. "On Angular Aperture of Immersion Objectives." By Robert B. Tolles, Boston, U.S.A. "The Minute Anatomy of Two Cases of Cancer." By Assist.-Surgeon J. J. Woodward. "The Theories of Cell Development." By I. N. Danforth, M.D., Pathologist of St. Luke's Hospital, Chicago, U.S.A. "Draw-Tubes *versus* Deep Eye-Pieces." By M. A. Prazmowski.

MINERALOGY, METALLURGY, AND MINING.

The Datolite from Bergen Hill, N. I., has been described by Mr. E. S. Dana in "Silliman's American Journal" for July. The Bergen Hill tunnel is famous for the abundance, beauty, and variety of the minerals which it brought to light. Datolite, pectolite, calcite, analcite, apophyllite, natrolite, stilbite, and others were obtained there during its excavation in a degree of perfection rarely equalled by the productions of any other locality. The crystallisations of datolite are especially remarkable; some of the surfaces covered with the brilliant crystals being eighteen to twenty-four inches in length. The crystals are in general not over a third of an inch across, though they sometimes have a diameter of one inch. Those of a single specimen have always entire uniformity of habit. The datolite is associated on different specimens with most of the other species found at the same locality, but it was not found possible to obtain any facts which would throw light upon the influence of the associated minerals on the crystalline form. Among the varied forms, four different types may be distinguished.

Ferro-Tungstine. A new Mineral.—Mr. Hugo Tamm describes in the "Chemical News," a mineral, of which he had procured a very small specimen, but had not been able to find out the place where it was originally discovered. He gives the following results of his analysis of it. Ferro-tungstine is a dark steel-coloured, heavy, crystalline powder, formed of separate and distinct crystals, presenting exactly the appearance of crystallised silicium. Its specific gravity, which is considerable, is 12·5. It is a very hard substance, which can only be porphyrised with extreme difficulty, and it is almost impossible to reduce it to an amorphous powder; the powder obtained after a prolonged porphyrisation remaining still bright, like the powder of a crystalline metal or alloy, and crystalline.

Heated in the air at a low red heat, ferro-tungstine slowly oxidises, and is converted into a yellow powder of tungstic acid; and when it is thrown in fused nitre, it burns with brilliancy and is thoroughly attacked.

Ferro-tungstine is composed of—

Metallic tungsten	88·05
Metallic iron	5·60
Metallic manganese	0·15
Undetermined substance	6·20
	100·00

PHYSICS.

Spheroidal State of Water, and Boiler Explosions.—Mr. W. F. Barrett, F.C.S., read a paper on the above subject at the meeting of the British Association at Brighton. He said:—On one occasion, some six years ago, I wanted to cool a red hot copper ball. For this purpose I plunged it into some water in which I had just washed my hands. The hot ball went in without any hissing or visible evolution of steam, and on removing it from the water it appeared as hot as before, in fact it remained brightly incandescent, somewhat below the surface of the water. I was astonished to see this, as I did not know that the spheroidal state of water could be so readily produced and maintained by a body at this temperature. So I tried other red-hot bodies in the same water, and with the same result. I then threw away the soapy water and used plain water; the result was now quite different, the hissing was loud, and the evolution of steam copious. Hence the soapiness of the water was concerned in the phenomenon. Adding a little soap to the water immediately reproduced the result first noticed. Other bodies that dissolved in water were also tried, and the results are briefly these:—Albumen, glycerine, and organic liquids generally facilitate the acquisition of the spherical state, probably by increasing the cohesion of the water, whilst of course bodies such as ammonia, which readily yield vapour, have the same effect but not so marked. Oil shaken up or even placed on the surface of the water has the same effect as the soap. The best method of exhibiting the experiment is to pour a little of Plateau's soap solution into a large beaker of water, and then, by means of a hooked wire, lower into the liquid a white-hot metal ball, some 2 lbs. weight, and of copper is best. The ball smoothly enters the water, and glows white-hot at a depth of a foot or more below the surface. Notwithstanding the considerable hydrostatic pressure it is seen to be surrounded by a shell of vapour, perhaps half-an-inch thick. This vapour shell is bounded by an envelope that resembles burnished silver, and has a most striking appearance. In fact the hot ball blows a soap bubble of steam, from the limiting surface of which the light is totally reflected. As the ball cools (mainly by radiation) the shell of vapour is seen to grow thinner, and finally collapse altogether, when immediately there follows a loud report, volumes of steam are produced, and often the glass is broken. I have heard that traces of oil often get into the boilers of steam engines, and there can be no doubt that dissolved organic matter often finds its way in. If in any way we increase the intensity of the water, we render it possible for a corroded boiler to give way under the pressure of the steam suddenly generated in the way I have indicated.

The Use of Steel Wire for Deep Sea Soundings.—Sir W. Thomson, F.R.S., read a paper on the above at the British Association in Brighton, in which he showed that the great difficulty of deep sea soundings consisted in the resistance of the water to the material used for letting down and raising the weight, and that the only way in which that difficulty had ever been overcome in very deep soundings had been by employing extremely heavy weights. When the depth of three hundred fathoms was passed, the ordinary lead line ceased to be available, or at all events convenient; and until very recently the difficulty of calling up a long line and heavy weight from considerable depths was so great that it had become the practice to leave the weight behind, simply bringing up the specimen of the bottom. The Admiralty had made great improvements in deep sea soundings, but even with the rope now used its resistance to the water when drawn up by hand at considerable speed was so dangerous as to necessitate the use of steam power. When there was great resistance to the line, and the currents carried it away to a distance, it was difficult to know when the bottom was reached. However, he believed that with so great a weight as 3 cwt. the bottom might practically be perceived within a few fathoms, and, although it must be difficult to stop the line all of a sudden, he did not think the error in the sounding could be considered to be serious. To many it had occurred that wire rope would be a great advantage, inasmuch as it would occupy much less space and, therefore, create less resistance to the water. The objections which had been raised to wire were, that it was liable to rust, that the men could not handle it as it would kink, and it would go down in a heap over the weight; but he believed all those difficulties might be overcome by proper care. It had been considered necessary to have a great deal of mechanism, but all that he deemed to be essential was a wheel which operated like a break, and around which the wire should be twined; the wire used being No. 22 gauge, of the quality known as the homogeneous steel wire, which could be manufactured in great lengths, was ·03-inch in diameter, weighed 12 lbs. per statute mile, and broke with a weight of 252 lbs. To the end of the wire was attached a piece of hemp cord, which carried the weight, and by that means the wire was prevented from touching the bottom at all. He had made an experiment in mid-ocean, at a depth of 2,700 fms., experiments with the apparatus and materials he had indicated; and, it having been attended with the most perfect success, he was sanguine that, if wire were allowed to take the place of cord in deep sea soundings, it would be far more economical, and so the calculations themselves would be more accurate.

Aërial Navigation.—This formed the subject of a paper read at the British Association in Brighton by Mr. C. A. Bowdler, in which he expressed the opinion that the autumn manœuvres would be an excellent opportunity of trying experiments, and that aërostation would become an important element in military science. Hitherto, captive balloons only had been used, but it was by no means improbable that circumstances would occur where it would be most desirable to pass over the enemy's position, and it would then be important to have the power of severing or deflecting the balloon from the wind-course, either to right or left as required. Captive balloons could not be used in safety in high winds on account of violent rocking of the car. The writer then proceeded to review the elements of aërostation, and to show that aërial navigation was practical to a certain limit by simple mechanical means. Of the practicability of apply-steam power he had no hope, the weight of a steam engine made as light as possible, consistent with due strength, being much too great for any gas balloon to support. The power he proposed was manual, being, he believed, the only power applicable to gas balloons. But propulsion having been secured, the question arose how the power of direction could be acquired, that being of the utmost importance in actual warfare. That was accomplished by rotating the balloon to any required position, and then, holding it from further motion, the rotation was completely under the control of the aëronaut. A rudder was the instrument to be used for that purpose, a vertical disc fixed in a line with the axis of the propellor. By turning the plane of the disc the current of air forced from the fan on the rudder caused the whole machine to rotate right or left precisely as the rudder of a ship guided the vessel.

Report on Rainfall.—At the meeting of the British Association in Brighton, Mr. G. J. Symons, Secretary of the Rainfall Committee, produced the Report of that committee, which was as follows:—"Your committee have the honour of reporting that every branch of rainfall work continues in efficient working order, and that, notwithstanding the very limited funds at our disposal, and the long illness of our secretary during the winter, all arrears have been overtaken, and, owing to the completeness of the organisation, no hitch or interruption occurred. At the meeting of the British Association in Edinburgh very strong representations were made to your committee respecting the desirability of establishing additional rain-gauge stations in different parts of the highlands, and as your committee had long been aware of the necessity which existed for these stations, and, moreover, as somewhat larger funds than usual were at their disposal, they resolved on taking every means in their power to secure the efficient establishment of these stations. In addition to ordinary correspondence, our secretary took two special steps to secure the most promising distribution of the new gauges. In the first place he wrote to Mr. Buchan, the secretary to the Scottish Meteorological Society, acquainted him with the assent of the committee, and requested him to state what number of gauges he could provide good observers for. On receipt of his reply, ten gauges were sent to him, which he was kind enough to distribute as follows:—1. Springfield, Tain, Ross; 2. Kilmalcolm, Port Glasgow; 3. Arrochar, Loch Long; 4. Strahane, Brodick, Arran; 5. Strathfillan, Perthshire; 6. Samrose, Arran; 7. Kilchoman, Islay; 8. Port Charlotte, Islay; 9. Port Ellen, Islay; 10. Glenharn Abbey, Mull of Cantire."

Powerful Galvanic Battery.—Mr. Highton described, at the British Association in Brighton, a new form of battery capable of giving powerful effects without fumes or other inconveniences. Negative: Carbon packed in a porous cell, with sulphur, peroxide of manganese, and granulated carbon, filled up with dilute sulphuric acid; Positive: zinc in a solution of caustic potash or soda. The potential is nearly fifty per cent. higher than Groves. One cell will abstract magnesium from its salts.

ZOOLOGY AND COMPARATIVE ANATOMY.

Supposed New Marine Animal.—At the British Association Meeting, Dr. Sclater, F.R.S., exhibited specimens of bodies having the general external shape and appearance of long, thin, tapering, white willow wands, which he had received from Captain David Herd, of the Hudson's Bay Company's service, with the information that they had been brought by that Company's vessels from Barrand's Inlet, Washington territory, North-West America. The captain who brought them stated that they were the backbones of a gelatinous fish, shaped like a conger-eel, very common in that inlet, which swam about in shoals with the dog-fish; that in the living animal the backbones were also transparent like the rest of the animal, but became ossified when dried on the beach. Dr. Gray, of the British Museum, recently described these rods as the axis of a pennatulide animal, and referred it to his genus *Osteocella* under the name *O. septentrionalis*; but Dr. Sclater, supposing the facts supplied him to be correct, considered the "rods" to be the ossified notochords of some low organised fish, with the skeleton wholly cartilaginous, partially belonging to the lampreys or to the chinœroid group.

A new Asiatic Rhinoceros.—Dr. Sclater, F.R.S., read a paper on a "New Asiatic Rhinoceros," with remarks on the recent species of the genus. On the 14th of February last, he said, the Zoological Gardens of London received a female two-horned rhinoceros, which had been captured near Chittagong four years previously. This animal had been referred to *Rhinoceros Sumatrensis* of Cuvier by the author and other writers, that being the only species of the two-horned section of rhinoceros hitherto recognised by naturalists. The acquisition of a female of the veritable *Rhinoceros Sumatrensis* from Malacca had enabled the author, after comparison, to conclude that the first-mentioned specimen belonged to a different species, which he proposed to call *Rhinoceros lasiotis*, on account of its most obvious external peculiarity being the long hairs which fringe the ears. The existing number of rhinoceros certainly known he considered to be six, of which four belonged to the Asiatic group and two to the African group.

Rheumatism in Whales.—Professor Struthers made a communication to the British Association on the sternum and pelvic bones in the right whale and in great fin whales, showing great variations in form, even in different species. He mentioned a curious circumstance in the osteology of whales, viz., that these animals are very liable to rheumatism. He had, he said, seen many examples of rheumatic ostitis in whales of different kinds. It had been said that animals were not subject to disease until they were brought into connection with man; but the fact he had mentioned contradicted the theory. It was the more remarkable, seeing that whales were less liable than man to variations of temperature; and the cold water cure (as a witty friend had observed) did not seem to be efficacious in the cure of the disease in question. The Professor made a communication also on the occurrence of finger-muscles in the bottle-nose whale (*Hyperodon bidens*). A dissection of the fin of a whale of this species (a male 20 feet in length) was exhibited, showing the presence of finger-muscles corresponding to those in man, and also (according to the Professor) the biceps muscle transferred from the scapula to the head of the humerus. A piece of the gum of the lower jaw was likewise shown, in which a concealed tooth was sunk about half an inch below the surface. He asked what could be the use of teeth in such a position? He could only infer, from the existence of such rudimentary structures, that the animal was descended from a species possessing functional teeth. Professor Flower attributed the great variations in the pelvic bones and the sternum of the whale to their rudimentary character.

Sir John Lubbock's Tame Wasp.—Sir J. Lubbock exhibited a tame wasp which he had brought with him from the Pyrenees, and which had been in his possession for about three months. The wasp was of a social kind, and he took it in its nest formed of twenty-seven cells, in which there were fifteen eggs; and, had the wasp been allowed to remain there, by this time there would have been quite a little colony of wasps. None of the eggs, however, came to maturity, and the wasp had laid no eggs since it had been in his possession. The wasp was now quite tame, though at first it was rather too ready with its sting. It now ate sugar from his hand and allowed him to stroke it. The wasp had every appearance of health and happiness; and, although it enjoyed an outing occasionally, it readily returned to its bottles, which it seemed to regard as a home. This was the first tame wasp kept by itself he had ever heard of.

Normal and Abnormal Growth of Lymnœus.—Professor Carl Semper read a paper at the British Association, in which he stated that numerous experiments made during the last few years have shown that, by separating individuals of the same generation, and by rearing them under the same conditions, the separated individuals grow more rapidly than those remaining and reared in company together. Through these experiments it became possible to draw certain curves of growth which show that, under the most favourable conditions, the growth may be divided into three distinct phases—the first being of slow growth, the second of very rapid, and the third of a very slow one again. The fact, that isolated individuals brought up under the same conditions—namely, the same quantities of water, the same surplus of food in the same temperature and isolation—acquire a greater length in the same time than those brought up in numbers together under the same conditions, is not a new one. But the explanation given, for instance in the case of fresh-water fishes, that this difference of growth depended entirely on the influence of the greater or lesser quantity of food, may be correct for the fishes, but is entirely wrong for the lymnœus. It is neither the quantity or quality of food, nor temperature, carbonate of lime, or other known influences, which determine the growth of the animal. From known experiments, it may be inferred that there will be found, in pursuing these investigations, that there may exist in the water a substance, the presence of which, at a certain low percentage, will determine the growth of the animal. This substance, which probably will be choloride of calcium, will act in the organism like the oil does in the steam-engine, viz. without its being there the animal will be unable to digest its food, while, being there in an almost imperceptible quantity, it renders growth possible. Thereby it is shown that the forces of molecular activity play an important part in the growth and formation of animals and animal tissues. The important part of physical science called molecular science must now always be taken into consideration in studying the development and growth of animals in general.

The White Coffee-Leaf Miner.—An American naturalist has given an interesting paper on this subject in the "American Naturalist" (June). The habitation of the larva is a mine, which is made in the leaf by eating out the soft green substance (parenchyma) between the upper skin (epidermis) and the framework of the leaf, laying the framework bare, but leaving the epidermis intact, except at the point where (he supposes) the larva enters the leaf. At this point the wound heals up and forms a lenticular scar twenty-five hundredths of a millimeter in length, and fifteen hundredths of a millimeter in breadth, raised a little above the general surface of the leaf. The epidermis which covers the mine becomes rusty brown, sometimes almost black in the centre. The excrement (frass) adheres irregularly to its under surface. Sometimes a portion of the under surface of the leaf opposite the mine also turns brown. When the eggs are laid in sets, the mines of the separate larvæ usually become united, and even the mines of two sets may be united into one. One mine, fifteen millimeters long and ten millimeters broad, contained seven larvæ, the scars arranged in two groups of four and three respectively. Another scar was near. As many as five mines, all inhabited, have been found on one leaf, and even eight mines made by ten larvæ, though in this case some of the larvæ had escaped.

The Colour of Fishes.—A short paper (in French) was read at the British Association by M. Georges Pouchet on the mechanism of the changes of colour in fishes and crustacea. The author referred to the fact that fishes often change in colour according to the colour of the objects by which they are surrounded: but he explained that this does not take place when the fish is deprived of the nerves that preside over the peculiar corpuscles to which the colour is due. The change does not take place in blind turbots; and in the seeing turbot, if the nerves are divided which communicate between the eye and the skin, the change does not occur. If the fifth nerve is divided, the change takes place all over the body except the part to which that nerve is distributed. These experiments, M. Pouchet said, show that the change of colour is dependent upon impressions received by the nervous system through the organs of vision.

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