THE

POPULAR SCIENCE REVIEW.

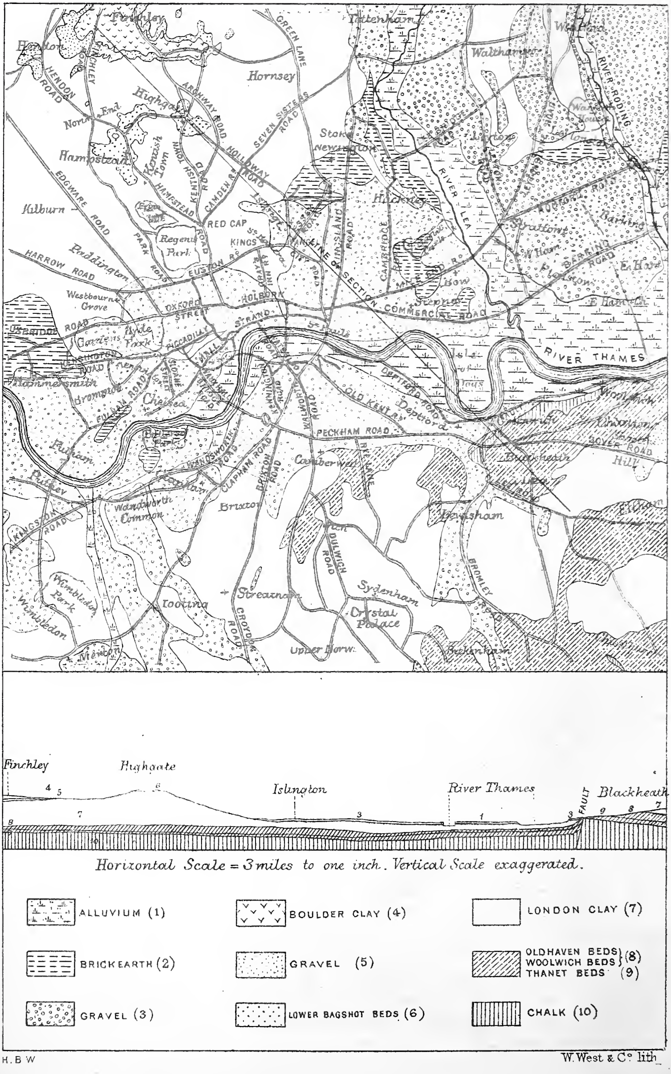
A QUARTERLY MISCELLANY OF
ENTERTAINING AND INSTRUCTIVE ARTICLES ON
SCIENTIFIC SUBJECTS.

EDITED BY HENRY LAWSON, M.D.

VOLUME XIII.

LONDON:
ROBERT HARDWICKE, 192 PICCADILLY;
AND ALL BOOKSELLERS.
1874.

CONTENTS OF VOL. XIII.



H.B.W. W. West & Co. lith.

Geological Map of London & its neighbourhood.

POPULAR SCIENCE REVIEW.

THE GEOLOGY OF LONDON.

By HORACE B. WOODWARD, F.G.S., of the Geological Survey of England and Wales.

[PLATE CV.]

ALTHOUGH the Geology of London may at first thought appear a somewhat barren subject, yet, when we come to consider all its aspects, we find it in reality a very fruitful and interesting study. If we were to take the "Town Geology," as Canon Kingsley has so charmingly described it, we might examine the different building stones and road-materials, inquiring whence they came and how they were formed, so as to learn those portions of the ancient history of the earth of which they are the records. We do not, however, propose to do this on the present occasion, but to treat the subject in its purely natural aspect; we shall look deep into the earth, and endeavour to show what changes the particular area, now embraced by London and its suburbs, has undergone during comparatively recent geological times.

We cannot draw our readers to romantic cliff-scenery, nor to many quarries teeming with fossils, but we can study phenomena which reveal many astonishing changes in the area, from conditions of deep-sea to those favourable to the formation of sand-banks and pebble-ridges; and from a climate almost tropical to one of intense cold, when glacial deposits were formed.

First of all, we must examine into the different kinds of "rocks" or strata found in and around London. These will be recognised as chalk, and varieties of clay, sand, and gravel, which we all know to occur in particular places. (See Pl. CV.)

We must, however, determine their relations one to another, and this can be done in one way by obtaining the records of some of the London deep-wells; and in another way by studying the numerous pits and railway-cuttings, which frequently show the junction between two or more of the different rocks. Classifying the observations so made, we find the following is the order in which the strata occur; and although of course all are not continuous over the area, this order is never inverted :—

Alluvium } Thames Valley deposits.
Gravel, sand, and brickearth }
Boulder clay } Glacial deposits.
Gravel and sand }
Lower Bagshot beds.
London clay.
Oldhaven beds.
Woolwich and Reading beds.
Thanet beds.
Chalk.

Thus we find from well-sections that the chalk is everywhere present at great depths beneath London. (See Section.) Again, in whatsoever direction we go by rail on the main-lines, we come to the outcrop of the chalk, as near Ipswich, Bishop's Stortford, Hatfield, St. Albans, Watford, Maidenhead, Basingstoke, Guildford, and Croydon. The chalk, therefore, crops up on the north, west, and south of London, and forms a sort of basin in which the London clay and other beds now lie. Hence this geological area is called the "London Basin."

The chalk is usually divided into the upper chalk with flints, the lower chalk without flints, and the chalk marl; the total thickness of which is estimated to be about 500 feet at Croydon.

The pits at Charlton, Gravesend, and Grays are no doubt well known to Londoners; whilst the hollow flints containing sponges, the "sugar-loaves," or sea-urchins, and the "thunderbolts" (as sometimes the Belemnites, at others the nodular masses of iron-pyrites, are called), which are found in the chalk, are equally familiar. Besides sponges and sea-urchins, the chalk yields many forms of ammonites, oysters, and other mollusca, a few corals, a number of fish and reptiles, amongst which the *Mosasaurus* is estimated to have been 24 feet in length. But the most abundant fossils found in the chalk, and of which indeed it is to a great extent made up, are the Foraminifera, minute many-chambered shells, which belong to the lowest class of the animal kingdom. "The best method of finding them is to scrape a small quantity of the chalk to be examined very finely with the penknife, and after moistening it with turpentine, to let a drop of Canada balsam fall on it, warming it at the same time over a lamp. Being now covered with a piece of thin microscopic glass, we may soon ascertain (by the aid of the microscope) whether we have been successful in obtaining specimens." *

The chalk is considered to have been formed in a deep and open sea, and indeed the researches which have been carried on in the North Atlantic Ocean show that materials for a continuous bed of limestone, with flint nodules, is now being deposited there. Professor Huxley, who has described the ooze derived from depths between 1,700 and 2,400 fathoms, considers that 85 per cent. of the whole belong to one species of *Globigerina* (a genus of Foraminifera), 5 per cent. to other calcareous organisms of at most four or five species, and that the remaining 10 per cent. consist partly of granules of quartz, and partly of animal (*Polycystineæ*) and vegetable (*Diatomaceæ*) organisms provided with siliceous skeletons and envelopes.† These siliceous organisms give us the clue to the formation of the flints. They consist almost entirely of silex, which is generally aggregated round some nucleus of sponge, sea-urchin, or mollusc, and, as we now find them, they occur in nodules and bands. Their fantastic shapes gave rise to the publication, a few years ago, of a humorous, though professedly scientific work, in which the author figured a number of curiously-formed flints which he had collected, and identified them according to the objects to which they had an apparent resemblance. Amongst these were a bird of paradise, some doves, hippopotami, sheep, and a pair of shoes! These he brought forward as evidences of a universal deluge.‡

The Thanet beds, which overlie the chalk at London, but thin away northwards, § consist generally of pale-coloured quartzose sand, and were so named by Mr. Prestwich from their being best exhibited in the Isle of Thanet. Immediately beneath London they have a thickness of about twenty feet; to the south Mr. Whitaker has noticed sections near Epsom, Cheam, and on the eastern side of the Croydon Valley. He remarks that the great pit east of Charlton railway-station shows a very long face of Thanet sand. The deposit is not very fossiliferous, but contains some remains of fish, mollusca,

* W. Hislop, "Proceedings of the Geologists' Association," vol. i. p. 388.

† See "Manual of Geology," by Jukes and Geikie.

‡ "Facts and Fossils adduced to prove the Deluge of Noah." By Major-Gen. Twemlow.

§ Their occurrence at Sudbury, on the northern outcrop of the London Basin, where they had not previously been detected, has lately been made known by Mr. Whitaker.

plants, &c. It indicates shallow-sea conditions, and although in marked contrast in this respect to the deep-sea chalk formation, yet no positive evidences of unconformity, to indicate a lapse of time, exist between the two.

Resting upon the Thanet sands, we find, throughout the London basin, a series of sands, mottled clays, and pebble-beds, to which the name of Woolwich and Reading beds has been applied by Mr. Prestwich. Under London the beds are from 80 to 90 feet thick. The best known section is that at Charlton, near Woolwich, where the beds rest on the Thanet sand, and are overlain by the Oldhaven beds.

At Greenwich, the Woolwich and Thanet beds are faulted against the chalk, there being a downthrow on the north of about a hundred feet. (See Section.) North of London, the Hertfordshire puddingstone, a well-known rock, is formed by the consolidation of the pebble-beds of the Woolwich and Reading series. This was to be seen three years ago, in a pit near New Organ Hall, east of Radlet's station on the Midland Railway

The fossils of this series show that the conditions of the area varied, the mollusca sometimes indicating estuarine, at others purely marine conditions. Besides the mollusca, of which the species are numerous, many fish remains occur; bones of turtles and scales of crocodiles have been found at Dulwich, as also remains of a tapir-like animal called the *Coryphodon.*

The Oldhaven beds, so named by Mr. Whitaker, consist chiefly of rolled flint-pebbles in a sandy matrix, and are locally developed south and east of London, with a thickness of about 20 feet. In the immediate neighbourhood they are best seen at Bromley, Croydon, and Blackheath. Mr. Whitaker considers that these beds were not formed as a beach along a chalk-shore, as in that case they should contain many flints but partly worn; and he is led therefore to infer that they must have been deposited some way off the shore, as a bank to which no flints could get without having been long exposed to wearing action. They contain many species of mollusca, some fish, and bones of turtles.

Above these Oldhaven beds comes the London clay, a deposit which is probably the most familiar to Londoners. It consists of brown and bluish-grey clay, with occasionally bands of septaria or cement-stones; these latter, which are dredged off Harwich for the manufacture of cement, and which occur in great numbers on the shore at Southend, are composed of clayey limestone with divisions or septa of carbonate of lime. According to Mr. Whitaker, in the neighbourhood of London, its thickness averages 420 feet. Sections are common in the brick- and tile-yards, but perhaps the most noted locality in London is at Highgate Archway, where Mr. Wetherell obtained a valuable series of fossils. To an ordinary observer it would not appear by any means a fossiliferous deposit, as the fossils occur in particular bands or zones, and are not very often displayed in the clay-pits. Sheppey is one of the best localities for fossils, and it has yielded a number of fossil fruits which were described, many years ago, by Dr. Bowerbank. The London clay is indeed very rich in fossils—crabs and lobsters, mollusca, fish, turtles, crocodiles, and a few mammals. Professor Owen remarks that the number of species of turtles obtained from Sheppey exceeds that of the species of *Chelone* now known to exist throughout the globe.

The most remarkable fossil, however, of the London clay, obtained also at Sheppey, was recently described by Professor Owen. It is the skull of a bird, consisting of the brain-case, with the basal portion of both jaws, the alveolar margins of which are produced into bony teeth. This bird he named *Odontopteryx*, and from a consideration of its characters, he concluded that it was web-footed and a fish-eater, and that in the catching of its slippery prey it was assisted by the armature of its jaws.

The nature of the London clay, according to Mr. Prestwich, indicates a tranquil deposit of no great depth. The fauna, taken altogether, he considers to indicate a moderate rather than a tropical climate; and yet the flora is, so far as can be judged, certainly tropical in its affinities.

Resting in isolated patches upon the London clay, and forming conspicuous hills whence some of the finest views near London are to be obtained, is a deposit of fine mealy sand called the Bagshot sand, about 100 feet in thickness. This is found at Harrow, Hampstead Heath, Highgate, High Beech, and Brentwood. In Essex the sand is overlain by pebble-beds of rolled flints. They are undoubtedly marine deposits, formed in shallow water. Fossils are exceedingly scarce, a few casts of shells having alone been found.

The beds to which we have now alluded may all be considered conformable, that is to say, they lie evenly and regularly one upon the other; although, as we have mentioned, the Oldhaven beds die out over the northern part of the London area. We have no evidence of any deposit immediately following the Bagshot beds in this district. In the Isle of Wight we find them succeeded by beds containing freshwater shells, as at Headon Hill and Whitecliff Bay. While these deposits were forming, and undoubtedly during the succeeding miocene period (which was characterised by a higher temperature in Europe, extending even as far as Greenland, where the fig, the palm, and ferns then grew), the London area was probably dry land. The action of rain and rivers in wearing away the land then came into operation, and probably the main features of this part of the country were formed. Subsequently, during the pliocene period, parts of the eastern coasts of Norfolk, Suffolk, and Essex came within the influence of the sea, which has left its traces in the deposits of shelly sand called crag. The evidence furnished by the mollusca in these deposits shows a gradual lowering in the temperature, while remains of mastodon, elephant, tapir, rhinoceros, hippopotamus, bear, and musk-ox are found in some of the later deposits of the period.

We now come to treat of deposits of which we find traces to the north of London, and which indicate that a glacial climate prevailed over the northern hemisphere. In the railway-cuttings at Bricket Wood (between Watford and St. Albans), and at Finchley, also in pits at Muswell Hill, we find a clayey deposit containing flints, numerous pellets and boulders of chalk, and other rocks, and fossils from almost every geological formation in England. This is called the boulder clay, and it overlies in places beds of sand and gravel, formed of rocks which have come from far distant parts, and containing fossils likewise derived from older and distant geological formations. Indeed, from these deposits at Muswell Hill and the neighbourhood, Mr. Wetherell, of Highgate, made a very extensive collection of British fossils. These sands and gravels, called middle glacial by Mr. Searles Wood, jun., have been proved to contain some organic remains which lived at the time of their deposit. They comprise over sixty forms of marine shells, which he has collected near Yarmouth. The boulder clay owes its origin to very different conditions. The presence of the rolled chalk in such abundance can be due to no other agency than moving ice, according to Mr. Wood. The degrading influence of a vast ice-sheet which covered the northern counties would have formed an immense quantity of material, and this being gradually expelled at the margin of the sea which then stretched over the south of England, it was washed away and dispersed over the midland and eastern counties, where we now find it.

The country must then have been in a condition resembling the polar regions, where vast sheets of ice, due to the accumulation of snow on the heights, press downwards into the lowlands, and so onward to the sea, to break off and form icebergs, bringing with them and dispersing much clayey, sandy, and gravelly material and boulders. Mr. Wood points out, that from the middle glacial deposits to the boulder clay, although there was such an abrupt change in character, there was an uninterrupted succession of deposit.

He believes that the sea depositing the boulder clay extended beyond the limits of England, although the denudation has been too complete to show any traces of the deposit on the southern side of the Thames Valley.

When the area was elevated at the close of the glacial period, some minor traces of gravel were formed, according to Mr. Wood, amongst which may be included the thin deposit upon the Bagshot sand at Hampstead Heath, the traces on Shooter's Hill, Langdon Hill, &c.

There are, however, other and more important deposits of gravel, sand, and brick-earth, which belong to post-glacial times, which we have now to consider. Such are the deposits which cover the greater part of the city, and stretch irregularly out into the suburbs. They border the Thames at Hammersmith, Chelsea, and stretch northwards to Highbury and Hackney. Eastwards, the pits at Ilford are, perhaps, the best known, while southwards the beds extend to Camberwell, and spread over Clapham and Wandsworth Commons. They are called the Thames Valley deposits, and they have been doubtless accumulated by the river itself. When, however, we consider the extent of the old Thames Valley, as indicated by these gravels, and the comparatively small changes that now take place, we cannot but imagine that the denuding and transporting powers were vastly greater in former times than they now are. Professor Phillips* considers that we require the agency of shallow ice-rafts, formed in the valley, to move some of the large blocks which are found in the gravel. He considers that the region was subject to greater extremes of cold than now, with more abundant rain and snow—that there was, in fact, a pluvial period, as Mr. Alfred Tylor first suggested, after which the local climate has been gradually improving, and acquiring more of its insular mildness and comparative dryness, due in some measure to the operations of man in draining and in the cutting down of forests.

Professor Ramsay is of opinion that the area of the Thames drainage has gradually contracted during tertiary and recent times. He points out how the escarpments, both of chalk and oolite, are slowly still changing and receding eastward, and as that of the oolite recedes the area of drainage will diminish, and the Thames decrease in volume.†

Besides many species of land and freshwater mollusca, which are found in the brick-earths at Hackney Down, Stoke Newington, and other places, numerous mammalian remains have

* See "Geology of Oxford and the Valley of the Thames."

† "Physical Geology and Geography of Great Britain," 3rd Edition, p. 222.

also been discovered. These include the bear, wolf, fox, hyæna, cave-lion, bison, Irish elk, musk-ox, elephant (mammoth), rhinoceros,* hippopotamus, beaver, &c. One of the most noted localities for mammalian remains is Ilford, and this is in a great measure due to the energy of Sir A. Brady, who has devoted much time and expense to their collection and preservation.

There are good grounds for believing that man co-existed with these animals. The discovery of flint implements, the works of man, in our valley-gravels, places his antiquity beyond a doubt.

The earliest record (before 1715) of the discovery of any implement is recorded in the Sloane Catalogue (British Museum). "No. 246. A British Weapon found, with elephant's tooth, opposite to Black Mary's, near Grayes Inn Lane. It is a large black flint shaped into the figure of a spear's point."

In 1868 an implement was found by Mr. Norman Evans in the brick-earth of Highbury New Park; and in 1871 Colonel Lane Fox found an implement, eight inches long, at Acton, beneath thirteen feet of sand and gravel. Other implements were found at Ealing, and all, according to Mr. Evans, were of well-marked palæolithic types. Implements have also been found at Hackney Down, Hammersmith, Battersea, Hounslow Heath, &c.†

In regard to the antiquity of man, which is indicated by the discovery of these and other evidences of his workmanship, we may quote the words of Mr. Evans.

"When we remember that the traditions of the mighty and historic city now extending across the valley of the Thames do not carry us back even to the close of that period of many centuries when a bronze-using people occupied this island; when we bear in mind that beyond that period lies another of probably far longer duration, when our barbaric predecessors sometimes polished their stone implements, but were still unacquainted with the use of metallic tools; when to the historic, bronze, and neolithic (newer stone) ages, we mentally add that long series of years which must have been required for the old fauna, with the mammoth and rhinoceros, and other to us strange and unaccustomed forms, to be supplanted by a group of animals more closely resembling those of the present day; and when, remembering all this, we realize the fact that all these vast periods of years have intervened since the completion

* Both elephant and rhinoceros were woolly-haired.

† For these particulars we are indebted to "The Ancient Stone Implements, Weapons, and Ornaments of Great Britain." By John Evans, F.R.S., &c.

of the palæolithic (older stone) period, the mind is almost lost in amazement at the vista of antiquity displayed." *

When we come, however, to discuss these subjects, we begin to trespass on the province of the archæologist, and we must leave them to consider a few more points connected with geology.

Mr. Prestwich † has shown how the earliest settlements in and around London were dependent upon the geological structure, and for this reason. The London clay which occupies so large an area is, as we have mentioned, covered to a large extent by the valley gravels, and in the north of London by here and there an outlying hill of Bagshot sand. Wells sunk through these sandy and gravelly deposits were always supplied with water, which was kept up by the impervious London clay. Hence as the water supply was an all-important question with the early settlers, so they followed the course of the water-bearing strata, while the bare London clay was unoccupied until the New River and other water-works did away with the necessity for wells. Thus the clay districts of Holloway, Camden Town, Regent's Park, St. John's Wood, Westbourne, and Notting Hill received town populations much later than Stepney, Hackney, Islington, Kensington, Chelsea, and Camberwell, which are situated on gravel.

In the same way, Mr. Prestwich has pointed out how on the outskirts of London a succession of villages grew up for miles on the great beds of gravel, ranging on the east to Barking, Ilford and Romford; on the north, following the valley of the Lea to Edmonton and Hoddesdon; and on the west, up the Thames valley, to Hammersmith, Ealing, Hounslow, and beyond. Around Harrow, which stands on the Bagshot sand, a large area of bare London clay extends, which is remarkably free even now from the encroachment of houses, particularly between Harrow and Ickenham on the west, and Edgware on the north-east.

When the supply from the shallow wells through the valley gravels was found insufficient to furnish the demand for water, deeper wells were sunk through the London clay into the sands beneath and into the chalk, which hold a great quantity of water. Owing to the outcrop of the chalk both to the north and south of London, forming, as we have mentioned, the London basin, this formation receives a quantity of water from the rain-fall which, percolating through it, is sustained by the clayey beds at its base, and is prevented from rising up in the centre of the basin, owing to the covering of London clay.

* "The Ancient Stone Implements, Weapons, and Ornaments of Great Britain." By John Evans, F.R.S., &c.

† Anniversary address to the Geological Society of London, 1872.

Consequently, when a deep well is sunk in London into the chalk, the water rises to near the surface from the pressure of the water pent up in the basin. Wells of this kind having originally been dug in the province of Artois (Artesium) in France, have been termed artesian wells.

In giving this brief sketch of the geological history of the area upon which London now stands, we have pointed out but a few of the great changes in its physical geography which have taken place. We have treated of the chalk and overlying strata, but we have not referred to the older rocks, and when we state that these, so far as they have been proved in Britain, constitute a series no less than twenty times as thick as the beds we have noticed, we are as much lost in awe in picturing each successive change during their deposition, as we are at the immensity of time that must have elapsed since the earliest stratum was laid down. Comparing this period to a year, the age of man has been but a second!

We have said enough to indicate the many interesting subjects which a study of the geology of London embraces.* One subject, however, and one, perhaps, more important to the people generally than any other, we have neglected; this is the probability of coal under London, or beneath the cretaceous rocks of the south-eastern and eastern counties. This question was discussed by Mr. Prestwich not long ago in the Popular Science Review. It is now being put to the test by the Sub-Wealden Exploration. A boring is being carried on to prove the palæozoic rocks in Sussex, and has penetrated to the depth of 300 feet, where the Kimeridge clay has been just reached. It seems probable that at least 1,000 feet of rock must be bored before the older rocks can be expected, and then the results will no doubt throw considerable light on the vexed question of the probability of coal in this neighbourhood. It is needless to enlarge upon the benefits that such a discovery would confer, for although according to the Royal Coal Commission we have a supply calculated to last three centuries, yet the high price of the fuel is a great source of disquietude to the nation. It would seem strange indeed to witness mining activity near to London, and probably the finding of coal would re-open the neglected iron-mines of the Weald.

* Those who wish to pursue the subject in detail should consult Mr. Whitaker's "Geology of the London Basin," "Memoirs of the Geological Survey of England and Wales," vol. iv. It contains an exhaustive list of other works on the geology of the district, and records of over 500 well-sections and borings.

Mr. Whitaker has also lately constructed a large model of London and its neighbourhood, embracing an area of about 165 square miles, on the scale of six inches to one mile. This is exhibited at the Museum in Jermyn Street, s.w.

WHAT TO BELIEVE IN SCIENCE: TELEOLOGY OR EVOLUTION.

By the Rev. T. R. R. STEBBING, M.A.

THE Science which deals with the evidences of design or purpose is *Teleology*, the science of final causes. A final cause is that for the sake of which anything is produced or done. The old lady who found a burglar in her store-closet, asked him for the final cause of his presence in that singular situation when she said, "What brings you here, sir?" In answering, "Why, ma'am, one must be somewheres," he evidently adopted the theory that all things, the movements of human beings included, are not by purpose but by chance, and that therefore it was idle under any circumstances to ask the reason why. Of that theory Archdeacon Paley effectually disposed many years ago, in his famous and popular treatise on Natural Theology. His whole argument is an argument from final causes. The same argument, and after much the same method, is pursued in another delightful book, the Bridgewater Treatise on "The Hand; its Mechanism and Vital Endowments, as evincing Design," by Sir Charles Bell. No one, indeed, will believe that the *flexor perforans* of the finger found its way through the *flexor perforatus* only by accident, just where the confined space of the narrow elongated digit made it all but essential that one of these muscles should pass through a hole in the other. No one, understanding the anatomy of the arm, and how the phalanges of the fingers are bent and straightened chiefly by muscles lying along the front and back of the fore-arm, would for a moment admit that the long terminal tendons of those muscles are bound down by the annular ligament at the wrist only through a lucky coincidence.

We are content, and may well be so, to recognise personal agency and design in the construction of a watch, a microscope, a steam-engine, without having seen them made or knowing the makers; and since we are surrounded by contrivances analogous, though in important particulars superior, to these contrivances, many of them long antecedent to the origin of man, and far beyond his skill to invent or even imitate, the inference so often drawn seems a fair and legitimate inference, that personal agency and design underlie all that, for convenience of language or from reverential motives, we call the works of Nature.

From the great conclusion, based on Teleology, that Nature, Creation, the Order of the Universe, has arisen not from chance but by design, we turn then to other conclusions, supposed to be grounded likewise on Teleology, and maintained by the same eminent writers who have shed so much lustre on the first part of the argument.

These authors were concerned to prove not only that design was visible in Nature, but that, out of many ways which the mind *à priori* might conceive as possible, the Designer had chosen one particular way, in preference to all other ways, of effecting his purpose. It is needless to conceal that they were led to maintain this line of argument by the impression existing in their minds that the Designer had Himself declared his choice of plan, and that therefore his honour was involved in the truthfulness of the declaration. They deemed it necessary, then, to their purpose, to show two things: first, that this particular plan had in fact been pursued; and secondly, that upon a broad general view, and as far as the human intellect and human science could judge, this plan was of all conceivable plans the very best. We propose to join issue with them on both these points, and to show that the teaching of Teleology is in favour of a different plan from that which *they* thought must have been followed; different, we say, from theirs, yet equally consistent with supreme wisdom and goodness.

On whatever plan the Universe may have been contrived and ordered, a finite intellect scanning and gauging it, not as a whole, but part by part, observing only infinitesimally small portions of it at any one time, and most of it never, can scarcely fail to be impressed with what some would call imperfection and contrariety in the scheme, but what others would more logically as well as more reverently describe as problems awaiting solution, as mysteries not to be frivolously blasphemed because impenetrable or unsatisfactory to a particular order of intelligence. But this doctrine, undeniable as it surely is, throws its ægis equally over every theory of creation, protecting all equally from *à priori* objections. We are really concerned with nothing except the facts of the case—facts gradually emerging, slowly revealing themselves, or being revealed, to the prophets and apostles and poets of science, with their strange gifts beyond the run of common men; gifts of heroic patience and self-denial, by which, with sure steps though slow, they penetrate the innermost arcana of the world, showing more and more clearly with every advance that no part of it all is useless or uncared for, but all teeming with marvellous work, with the stamp and impress of purpose, with the signs of an omnipresent intelligence.

Opinions about the actual course of Nature have changed many times, and with each change the religious philosopher has still acknowledged, as he was bound to acknowledge, its wisdom and goodness under each disguise or change of guise; whether he thought that the round earth had been made so sure that it could not be moved, or knew it to be engaged incessantly in rapid and varied motion; whether he thought the earth a circular plain dotted with hills and surrounded by an ocean river, or knew it to be an oblate spheroid; whether he believed the sun and moon to have been created three or four days after the earth's redemption from chaos, or believed that particular opinion to be utterly absurd; whether he thought that no other animals had ever existed on the earth than such as we now know, or was aware that multitudes of other genera and species had long ago died out; whether he believed the earth's crust to have been formed only by fiery agencies or only through the instrumentality of water, or knew it to have been formed by neither of these exclusively; whether he believed the granite rocks to be primeval, the strong foundations of the earth whereon all its outer covering rested and had been built up, or knew that granite-rocks had been continually forming in all geological periods;—through all these changes of opinion he continued, as to our view he was bound to continue, stedfast in loyalty to one belief—that, however the world had been made, it had been made wisely and had been made well.

We propose, then, to consider the theory of the world's history which the old writers on Teleology maintained, and to contrast it with the theory which they rejected. Sir Charles Bell says expressly: "Everything declares the species to have its origin in a distinct creation, not in a gradual variation from some original type; and any other hypothesis than that of a new creation of animals suited to the successive changes in the inorganic matter of the globe—the condition of the water, and atmosphere, and temperature—brings with it only an accumulation of difficulties." * But it is now abundantly clear that "the changes in the inorganic matter of the globe" of which Sir Charles Bell speaks have not taken place suddenly and at long intervals, as he supposed; they have been continuous and unceasing; they are working now. We need not witness Etna and Vesuvius in eruption to be aware of these changes. The boy who "in a showerful spring stares at the spate" may see

* Bell, "On the Hand," p. 166.

the chalk or mud washing down the slopes, and gradually wearing away the "everlasting hills." The engineer may run his iron road under the strong cliff and on the lip of the ocean, saying to its liquid mass, "Hitherto shalt thou come, and here shall thy proud waves be stayed;" but with the winter frost and in the winter storm the cliff falls and the waves beat on, turning dry land into sea; while elsewhere the costly light-house, built also on the edge of the shore for a beacon to the mariner, in the course of years is left far inland by the receding waters. By this gradual redistribution of land and water, by gradual changes of elevation and depression, by the slow diversion of hot and cold currents, and by other causes likewise operating slowly, the temperature of the earth's surface is diversified, not as a whole, but by gradual interchange of climate between its several portions. Regions now temperate in other days have known the perennial glacier and the iceberg; and the same regions now temperate, then glacial, at yet another time have reared in their warm enduring summers the grateful shade of palm-trees. The frozen North, treeless as it now is, once abounded in timber and foliage within a few degrees of the Pole.

If, then, Sir Charles Bell could fairly argue that sudden changes in the inorganic matter of the globe, in the condition of the water, the atmosphere and temperature, pointed clearly to successive creations, not the gradual variation of species, may we not as fairly infer from the changes which are now proved to have been gradual instead of sudden that gradual variation of species is more likely to have prevailed than successive creation? Certainly the teleologist cannot claim the point in favour of his argument from design if, while the outward conditions of life are constantly changing, species have been so stubbornly organised that they can make no change in correspondence. But emphatically he can claim the point in favour of his argument if he finds that not only have living organisms at any one period of the world's history been admirably suited to the condition of the world at that period, but that living organisms have been so marvellously constituted that, as time rolls on and climates change, and means of subsistence vary, and the whole face of the earth is altered, species too—which seem to shortlived and shortsighted observers rigidly fixed and unalterable—can adapt themselves by infinite variations to the ceaseless flow of circumstances. What is the adaptation of a few bones and muscles in the arm and hand for the advantage of a single animal, compared with this argument from the adaptation of all the living species on the globe, not to a single set of conditions, but to a never-ending variety.

Paley conceived the possibility of our planet revolving without any permanent axis of rotation. "The effect," he says, "of this unfixedness and instability would be, that the equatorial parts of the earth might become the polar, or the polar the equatorial, to the utter destruction of plants and animals, which are not capable of interchanging their situations, but are respectively adapted to their own."* His idea was, that upon some particular spot of the earth's surface each organism, as we now know it, was abruptly called into existence out of the dust of the earth; in one place a whale, in another a gudgeon, here a monkey and there a man. For instance, in one place he reproaches his fellow-men, saying, "We invade the territories of wild beasts and venomous reptiles, and then complain that we are infested by their bites and stings."† And having read that some extensive plains in Africa are almost entirely covered with serpents, he exclaims, "These are the natures appropriated to the situation. Let them enjoy their existence; let them have their country." According to this doctrine the extermination of wolves from England was an act of impiety; and when we fumigate our houses to rid them of animals smaller indeed than wolves but almost equally objectionable, though obeying the laws of comfort we are defying the prescriptions of Nature. Believing, as Paley did, and as so many persons continue to believe with him, that the ancestor of each species was a fixed and finished design, like a watch as it comes from the hands of the watchmaker, only with the faculty which no human machinery ever had, of producing copies of itself, it was natural for him also to believe, and believing to fancy he perceived, that Nature had a special care for preserving these designs, preserving them in the places for which they were specially designed, and preserving them unaltered. He contemplates arrangements "for the preventing of the loss of certain species from the universe; a misfortune," he says, "which seems to be studiously guarded against." "Though there may be the appearance of failure," he continues, "in some of the details of Nature's works, in her great purposes there never are. Her species never fail." It is certain that he is utterly wrong in the majority of these conclusions. The climates which he thought fixed for the different quarters of the globe have beyond all doubt been continually, or even continuously, varying. Plants and animals have not been destroyed, as he thought they must be, by such changes; one reason, though not the only reason, being, that plants and animals, which he thought were not capable of interchanging their situations, undoubtedly *are* capable of these migrations. The extinction of species is *not* studiously guarded against by Nature, and her species *do* fail. Within historical

* Paley, "Natural Theology," ed. 1837, p. 312.

† Paley, "Natural Theology," p. 378.

times we have the record of such failure. Now if the teleologist believed that fixity of species was nicely accommodated in the scheme of Nature to unchanging climates, to incapacity for migration among the several forms of life, and to contrivances for preventing the extinction of any, can he refuse to admit that, the circumstances being just the reverse of what he had supposed, the accommodation, the adaptation, the completeness and perfection of design for which he is arguing, imply not fixity of species but variation? This or that minute organism may have survived all changes for an incalculable period. Dredging expeditions may bring up from the depths of the ocean forgotten forms of species supposed to have been long extinct; but no researches will give us back in the clothing of flesh the gigantic mammals, birds, and lacertilians with whose fossil bones we are gradually becoming familiar under the awe-inspiring names of *Palæotherium* and *Dinornis* and *Megalosaurus.* It is safe to affirm that the great Meiocene tortoise, *Colossochelys Atlas*, will crawl and creep never again in active existence.

But, it may be argued, that power of migration which you speak of would have sufficed to preserve species from the effects of variation in climate and other conditions without variation in the species themselves. It would help; it is not true that it would suffice, because the power is limited. Something more was required to maintain that wonderful diversity which we perceive both in the present and the past—plasticity, namely, in the species themselves.

One of the grand arguments urged repeatedly against the variation of species is this—that no one has ever seen one species change into another; that there is no known instance of such an occurrence. It is a wonderful argument, especially wonderful from the lips whence in general it proceeds—from lips ever prone to exalt the decisions of faith above the decisions of sight. By the same argument no man can believe in God, since no man has ever seen Him. The very same argument tells equally against what Sir Charles Bell calls *distinct creation*, since no man has ever seen a single instance of that method of production.

To prove design in the works of Nature, Paley compared, as well as he might do, the construction and action of living organisms with the construction and action of machinery made by human art, especially one beautiful, ingenious, and well-known piece of machinery—a watch. He propounded further the conception of a watch "possessing the property of producing, in the course of its movement, another watch like itself," inferring justly that the effect would be to increase an observer's "admiration of the contrivance, and his conviction of the consummmate skill of the contriver." But not a single specimen of human art does, in fact, possess this faculty, while it is common not only to many, but to all, the species of living organisms. With this essential difference, then, between the designs of art and of Nature before his eyes and noted by his pen, he was almost bound to suspect that there would be some corresponding difference in the manner of production. Anthropomorphism is the attributing to non-human powers or agents the actions and feelings of mankind. What with the imperfection of language and the feebleness of reason, we cannot wholly escape from the fallacies into which anthropomorphism is apt to lead us. Analogies of the painter and the tailor and the mechanician have overmastered Paley and many others when contemplating the colours and the drapery given by Nature, and all those levers and valves and syringes and tubes and engines and circulating fluids, within the body, which the same Nature has concealed under robes almost endlessly varying in texture, hue, and pattern. New fashions are invented by human tailors and dressmakers to satisfy human caprice; and artificial clothing will not adapt itself to changes of season or the exigencies of travel. To meet variety of circumstance man must resort to variety of design, to many a "distinct creation," so to speak, in the sphere of art. But Nature is not to be thought of as a mortal artist, as a human mechanic. Nature is not to be charged with caprices. Nature is not bound by all the limitations which affect the contrivances of man. Obviously, and by the confession of all, in organic life there is a power of reproduction; the recurring process of generation. That a living organism is adapted to produce an organism like itself, we all admit; but what to many seems so impossible, so heretical, so derogatory to God and man, is simply this, that an organism should be adapted by Nature to produce an organism not like itself. This is the crime of the Evolutionist. The head and front of his offending, that he attributes to the Author of Nature a power of contrivance so far beyond man's, a foresight, an adaptation of means to ends, not only immeasurably beyond what appears in the achievements of art, but beyond all that art has ever attempted or even imagined. Men heap scorn upon the process of development, as though it were a light thing for Omnipotence, in a moment, abruptly, by an act of distinct creation, to call into existence, out of the dust of the earth, a man with his eyes and arms and brains and gastric juices and all the other curious chemistry and mechanism of his body; but they seem to think that the same Omnipotence would have been baffled in the attempt, however long pursued, to derive a man from another creature already organised, such for example as an Ascidian. They might just as well say that generation is impossible, and that every individual fish which swims in the ocean must be the result of an act of distinct creation. Each fish presents evidences of special design. Every single argument which has been used to prove that living creatures could not have attained their present forms through a process of development will prove equally that they could not have been generated in any but their mature forms. This absurd conclusion would no doubt have been acceptable enough, had we been familiar with none but adult forms of life, had we known nothing of that admitted and undeniable course of development which leads from the germ and embryo to the full organisation of the creature in its prime. A world of butterflies would plausibly argue that their own development out of grovelling cabbage-eating caterpillars was about as contrary to common sense as any theory that could well be devised, insulting to the Author of Nature, as supposing that He would bring a clean thing out of an unclean, and degrading to themselves, the children of the sun, fed on nectar and clad with the rainbow. This argument against development based upon the superior dignity of one creature above another creature will not bear a moment's examination. The largest brain, the fairest beauty ever found among mankind, have been nurtured by food. A Newton and a Cleopatra could not have been clever or beautiful without some such sustenance as beef and chicken. They fed on the ox, and the ox fed upon grass, and the grass fed upon manure; or if they washed down the tender flesh of pullets with Mareotic wine, the wine that gave the grapes had its roots in compost, and the chicken picked worms out of a dunghill. Of this dignified creature, man, so punctilious about his origin, it is said in a certain place, "The worm shall feed sweetly on him." You will perceive, perhaps, from what has been said, that the worm is only taking a just revenge—devouring its devourer.

In reality, however, opposition to the development theory founds itself not upon argument but upon authority. Men suppose that they have sound historical witness that man was produced a few thousand years back in a perfect state by a distinct act of creation. Moral failure, they think, first made him liable to bodily pain, and that he was exiled from the external Paradise because he had destroyed the Paradise within him of his own integrity. It will be interesting, therefore, to hear what so eminent a teleologist as Sir Charles Bell—a religious philosopher above suspicion—says on the subject of pain, bearing in mind that the Bridgewater Treatises to which his work was contributed are specially "on the Wisdom, Power, and Goodness of God, as manifested in the Creation." "To suppose," he says, "that we could be moved by the solicitations of pleasure and have no experience of pain, would be to place us where injuries would meet us at every step and in every motion, and, whether felt or not, would be destructive to life. To suppose that we are to move and act, without experience of resistance and of pain, is to suppose not only that man's nature is changed, but the whole of exterior nature also. There must be nothing to bruise the body or hurt the eye, nothing noxious to be drawn in with the breath: in short, it is to imagine altogether another state of existence, and the philosopher would be mortified were we to put this interpretation on his meaning. Pain is the necessary contrast to pleasure: it ushers us into existence or consciousness; it alone is capable of exciting the organs into activity; it is the companion and the guardian of human life."*

To argue, therefore, that man in Paradise was free from pain is to argue that he was without the necessary companion and guardian of life, without that which alone is capable of exciting his organs into activity; that he was liable at every step and in every motion to destructive injuries; and that he was rewarded for sinning by then first becoming capable of pleasure. Or you can avoid these conclusions and still cling to the old opinion that physical pain and death were introduced into the world through man's transgression, by maintaining that until that event the lion roared as gently as any sucking dove, and that beak and claw and talon and envenomed fang were only prospective contrivances, the ingenious apparatus of punishment beneficently designed before any fault had been committed. What tenderness and benevolent wisdom we should recognise in a human parent who, as soon as his child was born or even sooner, provided a large series of rods to chastise its anticipated offences!

We turn, in conclusion, to a class of cases which appeal to sentiment more forcibly than any others. Such an appeal has no proper cogency in rigid argument, but the use of it has a definite value and adequate justification when the minds of men have been previously closed to the reception of purely logical inference by sentimental objections. There is a strong popular bias in favour of the old hypothesis of distinct creation. That hypothesis conceives of each living creature as having been specially designed and constructed for its place in the world and for certain methods of operation, as a watch, a steam-engine, a microscope, a guillotine, might be designed and constructed by man. With this hypothesis before our minds, let us take the case of the Hermit-crab. This animal encases its soft defenceless body in the unoccupied shell of a mollusk. Its abdomen is furnished with hooked appendages to enable it to attach itself to this tenement. Here, then, we

* Sir Charles Bell, Bridgewater Treatise, "On the Hand," p. 190.

have the death of the mollusk distinctly contemplated, in anticipation, no doubt, of the Fall of Man, which would react upon the lower animals, shorten the span of existence for the Turritella and the Whelk, and so at length accommodate the shivering Crustacean with a house and a holdfast for its tail. But this, it seems, was likely to make the Hermit too comfortable; so another special creation presents itself in the shape of certain Rhizocephala, which have a free existence of a few days, and then attach themselves to the Hermit's abdomen. In this attachment, by Fritz Müller's account, these objects of distinct creation "remain astomatous (*i.e.* without mouths); they lose all their limbs completely, and appear as sausage-like, sack-shaped, or discoidal excrescences of their host." Closed tubes, ramified like roots, sink into his interior, twist round his intestine, or become diffused among the sac-like tubes of his liver. But distinct creation has not yet done with the Hermit and his guest; for in the case of *Sacculina purpurea* the roots of the parasite are made use of by two other parasites, which take up their abode beneath the *Sacculina*, and cause it to die away by intercepting the nourishment conveyed by the roots; and when the *Sacculina* itself is dead its roots continue to flourish and abound, at the expense of the Hermit and for the benefit of the besieging *Bopyrus*.

The distinct creation of this series of animals—of a crab not fitted like other crabs to produce a shell of its own, but adapted only to occupy the shell of a dead mollusk; of a *Sacculina*, not furnished like most animals with a mouth and limbs, but with roots suited only to steal away the vitality of the crab; and lastly of a *Bopyrus*, not designed for independent existence, but contrived to depend for its life upon the destruction of all but the roots of the *Sacculina*—seems to me as unlike our ordinary notions of wisdom and benevolence, as contrary to all analogy of human art and contrivance, as anything that could easily be conceived. Would any man in his senses raise a building specially suited to one set of machinery, and at the same time specially contrive a different set of machinery to suit that style of building; and at the same time devise a third set of engines which could not work apart from the previous set, nor yet work with that set except by stealing away its products; and at the same time invent a fourth set to rob the third set of what it stole from the second? From such a group of designs we should infer either that there had been several designs hostile in purpose, or, if on other grounds we were sure that one wise master-artist was the author of them all, we should feel equally assured that the working out of these designs had not been contemporaneous but a gradual process, worked out in correspondence with gradually varying circumstances, as different materials and different facilities came successively to hand.

We will take another group of animals—the Entozoa. These are animals which, as the name implies, live within other animals. The manner and course of life in at least one well known instance is as follows:—The egg is deposited on the ground; it is swallowed by some herbivorous animal, in the stomach of which the embryo escapes from the egg. It makes its way through the walls of the stomach, and by the general circulation may be carried into any part of the tissues of its host. It there developes into a form known as *cysticercus* or bladder-tail. Its nutriment is of course derived from the animal in which it resides—a pig it may be, or some other. But it is not designed to end its life either in this form or in this position. Let the pig be killed and eaten by man, it may be, or some other carnivorous animal, and then the cysticercus, drawing its head out of its bag, with hooks and suckers fastens on to the intestine of its new host, and budding out an immense series of segments, becomes a tape-worm. If, therefore, the tape-worm is the result of distinct creation, it must have been distinctly created with a view to the death of one animal and the disease of another. Nay more, distinct creation of the adult tape-worm must have been itself the distinct creation of disease, and of disease in one of its most repulsive forms. An animal of so low an organisation as to be little more capable of happiness than a cabbage is thus supposed to have been invented, by the direct exercise of an ingenuity that one would scarcely dare to call divine, to be the scourge of pigs, and of all pig-eating carnivores. These are not solitary instances; they might be supplemented by hundreds more.

Nevertheless, throughout the whole animal creation, not a single creature has been found with endowments injurious to itself. Paley especially insists on this. "The world," he says, "abounds with contrivances; and all the contrivances which we are acquainted with are directed to beneficial purposes. Evil, no doubt, exists, but is never, that we can perceive, the *object* of contrivance. Teeth are contrived to eat, not to ache." And again, " We never discover a train of contrivance to bring about an evil purpose. No anatomist ever discovered a system of organization calculated to produce pain and disease; or in explaining the parts of the human body, ever said, this is to irritate, this to inflame." His point is, that no creature possesses contrivances expressly for its own injury. He might have added that no creature possesses contrivances expressly for the benefit of others—a circumstance, as inconsistent with the idea of special design as could well be imagined. But Paley's own point is almost equally inconsistent with that idea, since it raises the question which it cannot answer—Why in a guilty world, why in a world of limited space, why in a world where creatures must prey upon one another, none have contrivances for self-immolation or self-punishment, notwithstanding the obvious advantage to the general system of things which such appendages would entail? It is impossible to set this down to the benevolence of special design, while Nature abounds in contrivances like the hairs of the stinging-nettle and Venus' fly-trap, and the sting of the hornet, and a whole host besides of stings and fangs and prickles and thorns and horns and cruel jaws and jagged teeth and claws and talons and irritating surfaces and noxious breaths and odours.

The wonderful voracity of some animals is matched, indeed, by the wonderful fertility of others; but if both are the results of special design, there is little to excite our admiration. If some animals were specially designed to be the food of others; if some were specially contrived to inflict upon others weakness, disease, and death; if the necessity of keeping under the number of individuals in every species was distinctly contemplated at the creation, there is nothing to admire in the fact that no species presents any special contrivances to enable its pursuers to capture and destroy it, nor any for self-torture, nor any for the limitation of its own numbers. Rather these are defects in the plan, scarcely credible inconsistencies. Allow, on the other hand, that the species of animal life have been gradually developed through wisely-ordained processes of variation, with natural selection thereupon attendant, and the difficulties at once vanish. We see then why creatures in general present no contrivances injurious to themselves, because such are the least likely to be preserved, and why they present many injurious to other animals, because such contrivances are beneficial to the possessor; we see why the most highly organised animals are most susceptible to pain, because pain is the most efficient monitor and guardian of animal organisation; so that every variation unprotected by its warnings would soon be lost. The voracity of some animals, so hard to accept as the benevolence of special design, is easily intelligible as an advantage developed in the struggle for existence, carrying with it superior strength and courage; and while some species are gradually developing voracious appetites, others will find a corresponding advantage in increased fertility. A pair of common plaice will become the parents of a hundred thousand or a million young ones, and out of that vast crowd not more, on the average, than two will survive to the prime of life. As the French general said of Englishmen's irregular bravery, "It is magnificent, but it is not war," we may say of the extraordinary fertility which enables many species of fish not to increase in numbers but to survive, it is magnificent, but assuredly it is not the result of special design.

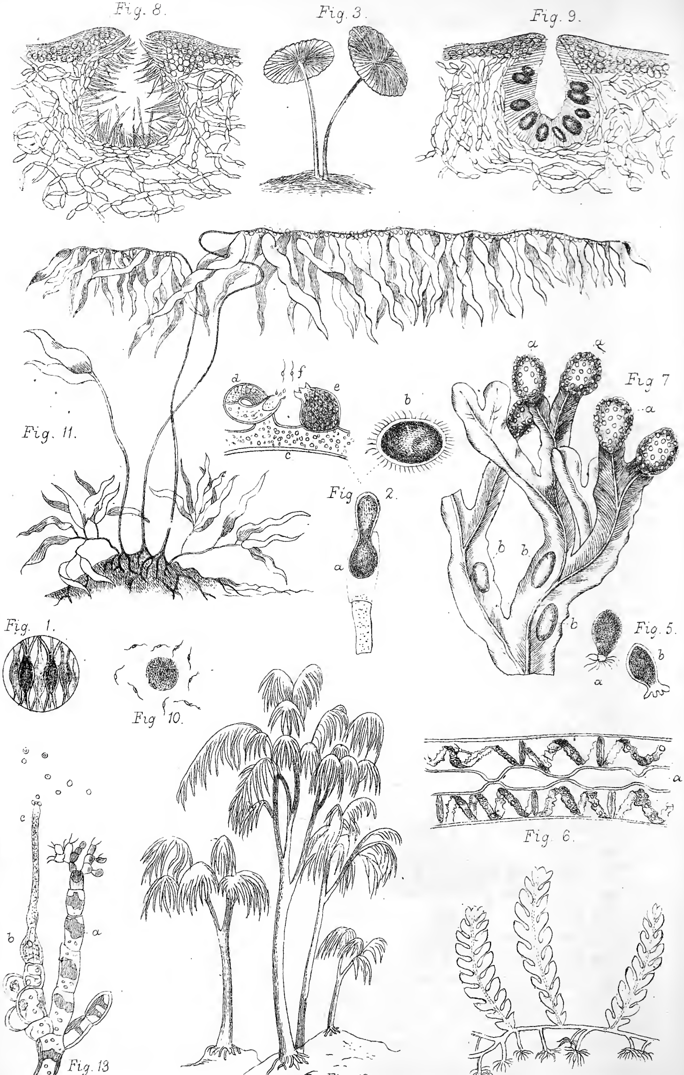
Sir Charles Bell speaks of "the force of our conviction that all that regards man's state is ordered in perfection." Bishop Wilson, on the contrary, calls "the remembrance of our own Infirmities and Miserys an excellent Antidote against ye Poison of Vanity." We stand highest in the scale of visible Nature; and because we see nothing higher than ourselves, we vainly and ingloriously think that "the force of Nature can no further go." We claim to be autochthons, sprung from the ground itself, investing the old pagan boast with the dignity of a divine utterance. We cannot embrace the idea either of a past when man was something less than man, or of a future when man shall have become something more than man. We tell our children pretty fables about the man who wished for eyes with the powers of the finest microscope, and the man who wished to be able to read the thoughts in the breasts of his brethren, and the man who asked to know the number of his days, that he might be certified how long he had to live, and the man who soared to the sky on cloud-piercing wings; and the moral is that all these wishes and strivings for faculties enlarged and ennobled, for something better than our present selves, something more perfect than our present perfection, ended in disaster and shame. Silly little moral! craven, ignoble, and pernicious, unintelligent of man's origin, with no penetration into the destiny which that origin foreshadows! If teleology, the science of design, teaches anything, it teaches this, that the world as it exists for humanity could never have been so good nor so bad, that the race of man could never have been either so blessed or so cursed as it is, if the whole complex design had been from the first stationary, unprogressive, incapable of improvement, finished out of hand by an act of distinct creation.

The Irishman said that one man was as good as another, and a great deal better. Numbers of persons, who are not Irish, maintain that the creatures inhabiting our globe are just the same as they were at the Creation, and a great deal worse. This, in fact, is the old, time-honoured, orthodox, popular opinion. There is an opposite opinion held by a small set of fanatics, known as men of science, that forms of life have been continually changing, that they are still changing, and are likely to continue to change. They believe that the changes have been on the whole for the better, and that the laws of Nature made it almost impossible that they should have been or should be on the whole for the worse. They think this conception of Nature quite as worthy of an Artist supremely wise and good, as that which imputes to Him a fixed design, beginning well and in the sequel going bad.

Those who hold these opposite opinions may be compared to the two armourers in the ancient legend, one of whom boasted, as the orthodox boast, that his coat of mail was impenetrable; the other, like the scientific men, that he had a sword which no coat of mail could resist. Just as some persons stake their religion on the truth of some favourite prejudice, the one champion agreed to stand the buffet of his rival's sword in the armour which he thought could never be pierced. The blow descended, and he stood unmoved, smiling with scorn and triumph at the smiter, as some perhaps are even now smiling at the inefficacy of my argument. But the other said, "Is it possible, my friend, you do not know that I have cut you in two? Just shake yourself.' And the triumphant boaster shook himself, and fell to pieces. The keenness and temper of the blade had done what scientific reason has done with a large group of popular prejudices. They still stand upright and look science in the face and laugh at it. But science has already cut them in two, severed their heads from their feet. Even now Science is saying to Superstition, "Shake yourself," and presently Superstition will shake itself and fall to pieces. But do not fear that the death of Superstition and the ruins of prejudice will involve any damage or hurt to religion. The death of the one is the life of the other. As in the Laureate's allegory, when the gigantic and horrible figure of Death has been cleft through helmet and through skull,

> "Out from this
> Issued the bright face of a blooming boy,
> Fresh as a flower new-born;"

so, when Superstition has been slain, Religion stands forth, no longer trammelled by vain armour not of proof, no longer distorted by ghastly imageries, and misrepresented under form and features not its own, but in unclouded majesty and grace.



K. Bennett del. W.W. & C° lith

The Anatomy & Physiology of Sea-weeds.

ALGÆ: THEIR STRUCTURE AND MODE OF REPRODUCTION.

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

Lecturer on Botany, St. Thomas's Hospital.

[PLATE CVI.]

MAGNUS in maximis, maximus aut in minimis is a saying which has a large amount of truth in its application to natural science. As long as naturalists confined their attention to the largest and most perfectly developed representatives of the two kingdoms into which organic nature is divided, they could obtain but a very shallow insight into some of the most important phenomena of life, whether animal or vegetable. It is the investigation—and especially since the microscope has been brought to its present pitch of perfection—of organisms either extremely minute or very low in the scale of organisation, of the Amœba, the Ascidians, the simplest and often microscopic fungi and algæ, that has led to those problems of the genesis and development of organic life that are now engaging so large a share of the attention of the scientific world. Into the discussion of these problems we do not propose now to enter, but rather to give the reader a general account of the peculiarities of structure which occur in one of the largest and lowliest divisions of the vegetable world, the algæ, and especially of that section of it known popularly as sea-weeds.

The first great natural division of the vegetable kingdom, as far as any division can be considered really natural, is into cellular and vascular plants. Plants belonging to the former division are characterised by their tissue always remaining in a cellular condition, and not developed into the fibro-vascular or any other of the tissues characteristic of the higher plants; they are therefore, as a rule, destined to only a very short period of existence. It includes algæ, fungi, lichens, Characeæ, Hepaticæ and mosses. Vascular plants, in which the cells are transformed into various kinds of vessels, and tissues thus formed calculated for an enduring term of life, are represented by ferns, horse-tails, club-mosses, and all flowering plants. Among cellular plants, the lowest place is generally assigned to the group of algæ, though the lowest forms of fungi are equally elementary in their structure with the lowest forms of algæ. To give a concise technical description which shall draw the line between algæ and fungi is not easy; and indeed, as we shall see presently, the two groups overlap at several points. Several well-marked characters which hold good in the large majority of cases, may, however, be mentioned. Algæ and fungi agree in being "Thallophytes," *i.e.* in the fact that they do not in general show any differentiation of their parts into distinct stem and leaves; they differ, however, in the locality in which they are found, algæ being generally water-plants (either fresh or salt) or, if not, growing at least in very damp places, while fungi are aërial plants, and very commonly parasites. A more important distinction is that algæ almost invariably form chlorophyll, the green colouring matter on the presence of which depends the power of the plant to decompose the carbonic acid of the atmosphere (or of the air dissolved in fresh or salt water) and hence to carry on an independent existence; while fungi never form either chlorophyll or starch, and hence have no power of independent existence, but live as parasites or epiphytes on either living or decaying vegetable or animal substance. In sea-weeds, however, it is very common for the chlorophyll to be overlaid and obscured by some other colouring material—red, brown or purple. The mode of reproduction is, as we shall see, extremely variable.

The division of the great group of algæ into orders is attended with great difficulties. One of the most easily understood systems of classification, though not in all respects satisfactory, recognises three orders, recognised by the colour of their tissues and spores; viz. (1) Rhodosporeæ or Florideæ, with a leaf-like or filamentous rose-red or purple, rarely brown or greenish-red thallus, and red spores; (2) Melanosporeæ or Fucoideæ, with a leaf-like, shrubby, cord-like, or filamentous thallus of olive-green or brown colour, and dark spores; and (3) Chlorosporeæ or Confervoideæ, extremely variable in form, and often microscopic, with green spores. Of these, the plants belonging to the first two orders are almost exclusively marine, while those belonging to the third chiefly inhabit fresh water. It will be our object to examine them more in detail, and mainly in respect to their microscopic structure and the physiology of their reproduction, rather than in relation to the mode by which the families or species can be distinguished from one another.

1. Taking these orders according to their grade of organisa-

tion, and beginning with the lowest, we find the Chlorosporeæ or Confervoideæ to be a heterogeneous assemblage of an immense variety of forms, differing most widely from one another, and arranged into a considerable number of families, whose rank is by no means uniform, and some of which might well be raised to the grade of independent orders. We propose to describe some only of the more important and interesting of these.

Among the lowest forms of this protean order are the two families *Diatomaceæ* and *Desmidieæ*. The various and extremely numerous species belonging to these families are so remarkable and peculiar as to have attracted a large share of attention, and to have formed almost a study of their own. We do not, therefore, propose to enter further into their description here. The extreme beauty and fineness of the marking of the siliceous coating of the cells of Diatoms have rendered them favourite test-objects for the higher powers of microscopic lenses. The peculiarities of their structure and mode of reproduction rendered it long a doubtful point whether they belonged to the animal or the vegetable kingdom, the great German authority, Prof. Ehrenberg, having persistently maintained their animal character.

The family of *Palmellaceæ* or *Chroococcaceæ* includes some of the simplest possible forms of vegetable life, comprised under the designation of Unicellular Algæ. In *Protococcus* the individual consists of a single cell composed of cell-wall and protoplasmic contents (but without a nucleus), the latter of various colours. Reproduction takes place by the ordinary process of cell-division, the protoplasmic contents of the cell first becoming constricted at two opposite points, the constrictions advancing till they meet, and thus dividing the protoplasm into two halves, the cell-wall afterwards folding in and finally dividing the parent-cell into two daughter-cells similar to itself. In this instance, however, the daughter-cells which proceed from a single parent-cell remain united by a common gelatinous envelope. *Palmella cruenta* forms the well-known rose-coloured gelatinous patches found on damp walls, which flake off when dry. *Protococcus nivalis* is the well-known "Red-Snow," the wonder of Arctic travellers, a blood-red gelatinous substance, which increases with such extraordinary rapidity that miles of virgin snow will become covered with it in a single night. It is occasionally met with in this country on other substances than snow. The family includes also some other more complicated structures, as *Glœocapsa* and *Merismopœdia*.

In *Nostocaceæ* are included some very remarkable forms of algæ, especially the well-known *Nostoc*. Emerald-green wrinkled gelatinous or very light dry lumps or balls, may frequently be found on damp stones, earth, or moss, gravel-paths, &c. Placed under the microscope, it is seen that these are not homogeneous masses, but that they contain within them a number of necklace-like threads of cells, which form the true *Nostoc*, a number of individuals being collected into a colony surrounded by the gelatinous envelope. In some of the genera the cells of which the necklace consists are of two different forms, the greater number are green and capable of division; between these occur at intervals, or at the ends of the threads, colourless cells of considerable size and incapable of division, called Heterocysts. These latter are probably connected with a yet undiscovered process of reproduction. The ordinary mode of propagation is thus described by Thwaites :— The jelly which surrounds the colony becomes softened by water, the portions of the threads that lie between the heterocysts become detached, separate from the jelly, and straighten themselves, the heterocysts themselves remaining in the jelly. The portions of the threads which become detached are endowed, after entering the water, with spontaneous motion; the cells now grow transversely to the axis of the filament, become disc-shaped, and finally divide into threads at right angles to the original one, and thus form new Nostoc-filaments, which then become enclosed in a new gelatinous envelope. Some species of *Nostoc* are parasitic, these motile threads having been seen by Janczewski * to enter the stomata of certain species of Hepaticæ, of which they were long thought to be peculiar vegetative reproductive organs, and being also known to inhabit the large cells in the leaves of *Sphagnum*. The species of this family are very widely distributed. Several are common in this countty. *Nostoc edule* is dried by the Chinese, and forms an ingredient in their soup. One species abounds to such a degree in the Polar regions as to afford an article of food, consisting of a modification of cellulose without any deleterious admixture. It is produced on soft and boggy slopes bordering the sea, and is carried by the wind in all directions, rolling along the surface. Another species was found by Dr. Hooker in Kerguelen's Land, near the southern pole, while several occur on the surface of the salt lakes of Thibet, and others in the hot springs of India.

Volvocineæ is a small family including only a few genera (*Volvox*, *Stephanosphæra*, *Gonium*, and a few others) which, as the name implies, are in a constant rolling motion. Each individual plant consists of either a single cell or a number of

* "Ann. des Sci. Nat.," 1872, p. 306, and "Quar. Jour. Micr. Sci.," 1872, p. 367.

cells collected into angular or tabular bodies, their motion being caused by fine threads or cilia of protoplasm, which project through the colourless hyaline envelope of cellulose in which they are enclosed; thus differing from the more common "swarm-spores" which we shall afterwards examine, and which are naked or unprovided with a coating of cellulose. The structure and mode of reproduction of *Stephanosphæra* (Plate CVI. fig. 1) are very curious. When fully mature it is a hyaline ball in which lie eight or more green cells, standing vertically to the horizontal diameter, and connected with the enveloping membrane by threads of protoplasm. These cells form together a family which rotates on an axis. Reproduction is effected by each of these cells dividing in succession into two, four, and eight cells; these eight daughter-cells form a family, which becomes clothed with an envelope of cellulose, with cilia protruding through it; and at a certain period, before the rupture of the outermost membrane, eight young families may be found moving in circles within their parent. *Volvox globator* is a pale-green globule, $\frac{1}{50}$th inch in diameter, common in ponds; *Stephanosphæra pluvialis* is found in rain-water in the hollows of large stones.

The *Oscillatorieæ* are a family of obscure plants, often of microscopic size, consisting generally of threads of cells, and characterised by an apparently spontaneous undulating or serpentine motion. To this family are generally referred the *Vibriones* and *Bacteria* which abound in decomposing infusions of vegetable substances; the difficulty of obtaining conditions under which they fail to be produced having given rise to the controversy respecting the spontaneous generation of vegetable organisms, which has recently attracted so much attention in the scientific world. They may, however, be simpler forms of other algæ, or even of fungi.

In the family of *Siphoneæ*, and especially in its typical genus *Vaucheria*, we have a more complicated structure and mode of reproduction than in any of those already passed in review. The individuals consist always of a single cell, often much branched, but never divided by septa into a number of distinct cells; they abound in pools, or on damp soil or mud. There are two distinct modes of reproduction, one sexual and one asexual. The latter is by the production of spontaneously motile zoospores, or "swarm-spores," which are produced by the protoplasmic contents of a branch of a filament accumulating at the end of the branch (as represented in fig. 2 *a*), and finally falling out. The escaped zoospore *b* is an ellipsoidal mass of colourless protoplasm containing grains of chlorophyll, and covered everywhere by short delicate densely crowded cilia, the vibratile motion of which causes the motion of the zoospore or swarm-spore. This mode of propagation by "free-cell-formation," as it is termed, is one which has thrown great light on the constitution of the vegetable cell, clearly demonstrating that the vital principle of the cell resides not in its cell-wall of cellulose, but in its protoplasmic contents, even when, as in the present case, the protoplasm does not possess a nucleus. The escaped zoospore of *Vaucheria* is, for all physiological purposes, a cell not enclosed in an envelope of cellulose, or a naked cell. As soon as the vibratile motion of the cilia ceases, the zoospore comes to rest, rapidly becomes encysted with a cellulose-wall, and grows into a new Vaucheria-plant by the ordinary process of cell-division. The separation of the protoplasm in the filament generally begins in the night, the zoospores escape in the morning, and their germination commences the next night. The sexual reproduction is accomplished by means of two distinct organs (fig. 2 *c*, *d*, and *e*), the oogonia, or female cells, and the antheridia, or male cells, the two kinds being often produced in close proximity. From the protoplasm of the antheridium are produced a number of antherozoids, very minute long bodies, each with two cilia. At the time that these are ready to escape, the contents of the oogonium have swelled up into a jelly and burst the cell-wall; some of the antherozoids enter through the opening, mingle with the protoplasm in the interior, fertilising it, and causing it to assume the form of an oospore or "resting-spore," which then developes into the new plant. The formation of the oogonia and antheridia begins in the evening, and is completed the next morning; fertilisation being accomplished between ten and four in the day. Closely allied to *Vaucheria* is the very simple *Botrydium*; but in other genera of Siphoneæ, the frond, though always consisting still of a single cell, assumes the most remarkable and beautiful forms, from the extraordinary extent to which it branches, as in *Acetabularia Mediterranea* (fig. 3), closely resembling in form a hymenomycetous fungus, and *Caulerpa taxifolia* (fig. 4), where the frond actually simulates differentiation into stem, leaves, and roots.

The *Œdogonieæ* are a small family, including only two generas, *Œdogonium* and *Bulbochæte*, but interesting as furnishing an illustration of a phenomenon more common among fungi than algæ—that of alternation of generations. From the oospores or resting-spores produced by a sexual process, which have remained at rest for a considerable time, several (usually four) swarm-spores are immediately formed; these produce asexual, *i.e.* swarm-spore-forming plants, from which again similar ones proceed, until the series is closed by a sexual generation, with formation of oospores. Swarm-spores are also produced in an ordinary cell of a filament by the contraction of its whole protoplasmic substance. It bursts through the wall of the cell and becomes free as an ellipsoidal body (fig. 5 *a* and *b*), one end of which is chlorophyll-green, and the other hyaline, the latter being crested with a tuft of cilia, by the vibration of which it is propelled rapidly through the water. When it comes to rest, the cilia disappear, at the end where they grew root-like processes are put out by which it attaches itself to a solid body; it then clothes itself with a cell-wall, and developes in the ordinary way.

The *Zygnemeæ* are often united with the Œdogonieæ, Diatomaceæ, and Desmidieæ into the family of *Conjugatæ*. The family includes a considerable number of filamentous fresh-water algæ which may be found in almost any drop of stagnant water, remarkable from the brilliant green of their chlorophyll, which is arranged in the most varied and beautiful forms of stars, plates, or bands, revealed under the microscope from the perfect transparence of their cell-walls. One of the commonest and most striking objects is *Spirogyra longata* (fig. 6), in which the bands of chlorophyll are arranged in the most beautiful spiral form. The Zygnemeæ produce no motile swarm-spores, but ordinary resting-spores or zygospores by a process known as Conjugation, confined to the Conjugatæ and a few fungi, which may be considered as the simplest possible form of fertilisation, the two conjugating cells being alike in form and structure, and therefore not distinguishable as male and female. In *Spirogyra* the conjugation always takes place between the adjacent cells of two more or less parallel filaments. A preparation is made for it by the formation of lateral protuberances (fig. 6 *a*) which continue to grow until they meet. The protoplasmic body, or "endochrome" of each of the two cells contracts simultaneously, detaches itself from the surrounding cell-wall, and assumes an ellipsoidal form. The cell-wall then opens between the two protuberances, and one of the ellipsoidal protoplasmic bodies forces itself into the connecting channel thus formed; it glides slowly through it into the other cell-cavity, and as soon as it touches the protoplasmic body contained in it, the two coalesce. After complete union the united body is again ellipsoidal, and scarcely larger, owing to the expulsion of water, than each of its two constituents separately. It clothes itself with a cell-wall, forming the body known as a Zygospore, which germinates after a period of rest of some months, and developes a new filament of cells.

In the family *Confervaceæ* are included an enormous number of green fresh-water algæ, found in all ponds and running water, reproduced by minute zoospores or metamorphosed joints. Besides the more ordinary forms, a number of others are doubtfully referred to this family, as *Hydrodictyon*, in which the frond forms a beautiful net, *Coleochæte*, which consists of cushions of jointed threads, attached to submerged plants, as Equisetum, and many others.

Sometimes included under the Confervaceæ, but often separated in consequence of the peculiarity of its physiological phenomena, is a small family, the *Saprolegnieæ*, including the two genera *Saprolegnia* and *Achlya*. If the dead body of a fly floats for a little while on water, it rapidly becomes covered on the under side by a coating of fine white hairs, constituting the alga known as *Achlya prolifera*. If, on the other hand, the dead fly is exposed to dry air, as on a window-pane, it also soon becomes surrounded by a number of white threads, the mycelium of the well-known fungus *Empusa Muscæ*. Many cryptogamists are of opinion that these two organisms, the one an alga, the other a fungus, are simply different states of the same plant, produced under different circumstances; while others consider the *Achlya* to be an aquatic form of a *Botrytis*, or of some other fungus. In any case, this remarkable fact proves the extremely close affinity between the lower forms of these two great classes of flowerless plants.

2. The FUCOIDEÆ includes an enormous number of marine algæ, of brown or olive colour, mucilaginous in texture, and of very variable form. To this order belong the largest and noblest of seaweeds, of shrubby or even arborescent form, rivalling the tallest trees in the actual length of their branches; as the *Lessonia fuscescens* and the gigantic *Macrocystis pyrifera*, represented in our Plate (figs. 11 and 12), from the Antarctic Seas, and the historic *Sargassum bacciferum*, or Atlantic Gulf-weed. The detached masses of this sea-weed form a floating meadow, known as the "Sargasso-sea," in the midst of the Atlantic, between 20° and 25° N. lat., and about 40° W. long., and occupying an area estimated to equal in size the whole of France. This enormous field has certainly occupied the same position since the time of Columbus, whose first expedition was so seriously delayed by it that the sailors were on the point of mutiny when a favourable wind carried them beyond it. It consists entirely of floating masses, buoyed up by the stalked berry-like air-bladders, from which it derives its name, but never producing fruit. It forms the home of multitudes of sea-animals, fishes—one of which builds a nest—and crustacea, many of which, we learn from the reports sent home by the "Challenger" expedition, furnish remarkable instances of "protective mimicry," closely resembling in colour the sea-weed in the midst of which they live, this being their only protection against their enemies.

Thuret divided the Fucoideæ, or Melanosporeæ, into two groups, of which the Phæosporeæ, including *Laminaria*, *Macrocystis*, and *Lessonia*, are distinguished by possessing zoospores, while the *Fucaceæ* are destitute of them.

The structure of the various organs of the *Fucaceæ* may be studied in their most perfect form in our common Bladder-wrack, *Fucus vesiculosus*, one of the most abundant sea-weeds of our coast. It is found on all the shores of Europe, and in the North Sea it grows so abundantly that it is used for domestic purposes, such as roofing houses. It is also burnt, and alkali extracted from its ashes, the ordinary term "potash" being derived from this mode of obtaining it. A mature frond of this sea-weed, as shown in fig. 7, presents two conspicuous organs; the bladders (*b*), which serve to float the fronds in the water; and the tubercles (*a*), at the ends of the fronds, which contain the organs of reproduction. These reproductive organs are hollow bodies or conceptacles of two kinds, male and female; in most of our common species the two kinds are found on different plants, which are therefore diœcious. These receptacles are not formed in the interior of the tissue, but as depressions of the surface which become walled in by the surrounding tissue, and so overgrown that at length only a narrow channel remains opening outwards. Among the reproductive organs a number of hairs are produced in the receptacles, which in *Fucus platycarpus* project out of the mouth of the receptacle in the form of tufts. The male conceptacles (fig. 8) produce the Antheridia as minute ovate sacs attached in great numbers to hair-like filaments. Each antheridium consists of a thin-walled oval cell, the protoplasm of which splits up into a number of minute spermatozoids or antherozoids, pointed at one end, and endowed with spontaneous motion caused by two vibratile cilia attached to each antherozoid; in the interior they contain a red or orange speck. They are scarcely $\frac{1}{5,000}$th of an inch in length; the two cilia are of unequal length; the larger one points forwards during progression, producing the motion, while the shorter one trails behind, and appears to act the part of a rudder. The female conceptacles (fig. 9) produce in the midst of the hairs a number of stalked oval bodies, the oogonia or sporanges. These bodies become filled with dark protoplasm, which divides into two, four, or eight spores, which are set free by the bursting of the envelopes or membranes in which they are confined. The process of fertilisation or fecundation of the spores has been carefully watched by M. Thuret, and is exceedingly curious. The process takes place outside the receptacles. At a certain period, when, from the reflux of the tide, these fertile branches are lying outside the water in moist air, the antheridia become detached from the hairs on which they grow, and collect in large numbers outside the mouth of the male receptacle, presenting the appearance of a drop of viscous exudation. At the same time, the spores, surrounded by the inner membrane of the oogonium, are also expelled, and collect outside the mouths of the female receptacles. When they again come into contact with the water, the spores burst through this membrane, and at the same time the antherozoids escape from the antheridia. The antherozoids collect in large numbers round the spores, as represented in fig. 10; and the movement of their cilia is so energetic that they impart to the spore to which they are attached a rotatory motion, which lasts for about half an hour, finally coalesce with it, and thus fertilising it; after which it finally comes to rest. A short time after these processes are completed, the fertilised spore or oospore surrounds itself with a cell-wall, fixes itself to some other substance, and begins, without any period of rest, to germinate, lengthening and dividing at the same time. This process may be followed under the microscope by placing together in a drop of water some of the spores and a small quantity of the viscous matter taken with the point of a needle from the mouth of the male receptacles. The entire field of the microscope may sometimes be seen covered with brownish spheres bristling with antherozoids, which roll themselves about in all directions under the influence of the vibratile cilia.

3. The FLORIDEÆ or RHODOSPOREÆ are almost invariably marine algæ, a very few forms being found in fresh water; they are generally of a rose-red, violet, or purple colour, often with leaf-like fronds, but very variable in size and in the consistence of their tissues. The various families cannot here be described *seriatim*, but one deserves special mention, from the peculiarity of its structure, viz.:—

The *Corallinaceæ* or Corallines. From their external resemblance to the coral-forming polyps, these substances were long supposed to belong to the animal kingdom; but are now known to be algæ which possess a similar power of extracting the carbonate of lime from sea-water. The lime may be removed by weak acid, and the nature of the true tissue revealed. They are reproduced by a vegetative process, by means of Tetraspores, no sexual organs of reproduction having hitherto been discovered. The common species, *Corallina officinalis*, grows everywhere between tide-marks, on rocks, &c., and presents a branched, mostly pinnate, tuft of articulated filaments evenly coated with carbonate of lime.

Two distinct modes of reproduction occur in most families of Florideæ, the asexual and the sexual. The first is distinguished from that known in other families of algæ by the fact that zoospores endowed with free spontaneous motion are never produced; but in their place Tetraspores, which are perfectly immotile. They consist of an oblong or globular external cell or sac, at first filled with granular contents, which subsequently separate into four portions, often arranged as the corners of a tetrahedron, but also frequently in a row, or like the quadrants of a sphere. Very rarely the number is greater or smaller than four. They are not placed in special receptacles, sometimes exposed on the surface of the frond, more often imbedded in its tissue, often in branches of peculiar shape, often congregated in great numbers. In the Corallines they are enclosed in elliptical conceptacles.

The sexual reproduction of the Florideæ presents some remarkable features. The male and female organs, called Antheridia and Trichogynes, are produced on different individuals to the tetraspores. The antheridia are either single cells at the ends of the branches, each producing only one antherozoid, or these parent cells of the antherozoids are congregated together in large numbers on a common axis as the terminal member of a very short branching system.

The antherozoids are minute roundish bodies without cilia and, like the tetraspores, without any power of independent motion, but are moved along passively by the water. In fig. 13 *a* represents the antheridium with a number of antherozoids escaping from it. The female organ, or trichogyne, is placed at the summit of a special organ, the Cystocarp, which, after fertilisation, produces the spores; in these organs it may be said that we have the first rudimentary indication of organs corresponding to the stigma and ovary of flowering plants, the antheridium and antherozoids corresponding to the anther and pollen-grains. The cystocarp consists, in its rudimentary condition, of branches formed of only one or two cells, which subsequently divide into a larger number. One of these cells at the side of the cystocarp subsequently elongates greatly into three, the two lowermost of which form the Trichophore, while the uppermost elongates still further into a hair-like prolongation, the Trichogyne (fig. 13 *c*), which therefore grows up by the side and not at the summit of the cystocarp. The spores are produced in the central cell of the cystocarp, and therefore not in immediate connection with the trichogyne or trichophore. The antherozoids collect round the top of the trichogyne, as represented in the figure; and their fertilising power is transmitted through the tube to the spores, in some unknown way, possibly analogous to the part performed by the pollen-tube of flowering plants. In *Dudresnaya* the process is, according to Thuret and Bornet, still more complicated. In this genus the cystocarps are produced on altogether different branches from the trichophore. After the long spiral trichogyne has been fertilised, branches spring out from it, and from these are produced tubes connecting them with certain special branches. The cystocarps are produced on these special fertile branches, and the tubes apparently convey the fertilising power from the trichogyne to them. The act of fertilisation always consists of a conjugation of the roundish antherozoid with the trichogyne, its protoplasmic substance passing over into the latter. Sachs compares the process with that which produces the fructification of the Pezizæ and Erysipheæ among fungi.

The Florideæ, like all other algæ, contain abundance of chlorophyll, which is, however, concealed from view by the red colouring substance, phycoërythrine. M. Rosanoff extracted this substance by cold water, and examined it accurately. In transmitted light it is carmine-red, in reflected light reddish-yellow. The grains of chlorophyll also show fluorescence, if left behind when the phycoërythrine has escaped in consequence of injury to the cells. The whole plant remains green when the red colouring matter has been extracted or destroyed by heat. Millardet obtained in the same way from the Fucaceæ, by extraction with alcohol, a yellow substance, phycoxanthine, and a reddish-brown colouring matter, phycophæine, which in like manner conceal the chlorophyll in the brown or olive-green sea-weeds.

EXPLANATION OF PLATE CVI.

ALGÆ: THEIR STRUCTURE AND MODE OF REPRODUCTION.

Fig. 1. *Stephanosphæra pluvialis.*

" 2. *Vaucheria sessilis*: (*a*) young filament producing zoospores; (*b*) escaped zoospore with vibratile cilia; (*c*) branch producing reproductive organs; (*d*) antheridium emitting antherozoids; (*e*) oogonium; (*f*) antherozoids, further magnified.

" 3. *Acetabularia Mediterranea.*

" 4. *Caulerpa taxifolia.*

" 5. *Œdogonium vesicatum*; zoospore; (*a*) in motion with vibratile cilia; (*b*) at rest.

" 6. *Spirogyra longata*; (*a*) cells preparing for conjugation.

" 7. *Fucus vesiculosus*; frond with fructiferous tubercles (*a*) and air-bladders (*b*).

" 8. *Fucus vesiculosus*; section of male conceptacle lined with branched hairs bearing the antheridia.

" 9. *Fucus vesiculosus*; section of female conceptacle, containing sporangia.

" 10. Fertilisation of *Fucus vesiculosus*; spore surrounded by antherozoids.

" 11. *Macrocystis pyrifera.* } from Hooker's "Flora Antarctica."
" 12. *Lessonia fuscescens.* }

" 13. *Nemalium multifidum*; (*a*) male branch, emitting antherozoids; (*b*) female branch, ending in the trichogyne (*c*), which the antherozoids reach.

HOUSE MARTINS AS BUILDERS.

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

THE popular notion that all nest-making birds work by instinct, neither controlled nor modified by reason, has not been accepted by many distinguished observers, and has been demolished by Mr. Alfred Wallace, who supplies abundant reasons for his opinion "that the mental faculties exhibited by birds in the construction of their nests are the same in kind as those manifested by mankind in the construction of their dwellings."* If it is said that birds are accustomed to do the same things in the same way, over and over again for years and generations, it should be remembered that this is also true of many races of men, and, to some extent, of all men. Such propositions are only true in a broad and general sense, and it is probable that a great many exceptions would be found amongst building birds if they were carefully looked for. After any building creature has formed a habit of constructing its abode in a particular way, it will most likely continue it until some change of circumstances renders it impracticable or inconvenient, and then whatever powers of reason and observation it possesses will be exerted to get over the difficulty by some alteration in the material or the plan.

Some time ago, M. Pouchet, of Rouen, noticed that the swallows of the present day, inhabiting that picturesque city, had a better pattern for their nests than those of older date which had been preserved in the museum. The new construction is more roomy than the old. Here, then, is a proof of divergence from any supposed "instinctive" pattern, and it is not likely to be a solitary exception.

During the last three or four years the writer has noticed numerous divergencies and varieties in the nests made by house martins round his own dwelling. Instead of saying they all build alike, it would be much nearer the truth to say that each pair have their own notions on the matter, and vary them

* "Intellectual Observer," vol. xi. p. 420.

within certain limits from time to time. At the present moment, on the north side, near the point of a gable, is a nest built against one slope of woodwork, and the rough, cast-wall below it. This nest has an oblique, rough-edged entrance, following the line of the eaves. Another nest was built touching it with an opening in another direction, but, being much exposed to wind and weather, it tumbled down. In another gable nests are built every year, and fall sooner or later from wind and rain. The new nests in this situation have not been exact repetitions of the old ones, but somewhat broader at the base, and with an entrance differently arranged. The birds do not choose to leave this place, but they have not yet succeeded in making a nest to last long, though they may be said to be improving.

On the east side of the house, under the projecting roof, there are now two nests attached to each other, side by side. The first built had a roundish hole for an entrance on the right-hand side, just under the woodwork. The second nest has its entrance on the left side of its curve, not close to the woodwork like the former, but provided with a slightly thickened and projecting rim. Another nest may be roughly likened to a big convex oyster shell, stuck up under a horizontal part of the projecting roof, and open all along the top, with a rough edge. This has been a very common form of nest for three or four years in several situations.

At the point of a southern gable a nest was made this year attached to the right-hand slope of the woodwork as well as to the wall, showing a large sloping opening on that side.

Two nests are fixed side by side, and attached under the projecting window of an upper room, and in the top corner of the window of the room below. When the first nest was built, cats used to sit on the window-sill and look longingly at its inhabitants. This did not trouble the birds—they had apparently satisfied themselves that it would be too awkward a jump for pussy to succeed in, and up to the present they have been right. So tame are the birds when building, and so satisfied of protection, that they did not show any anxiety when workmen were close by them coating the walls with a silica preparation, some of which was washed over their unfinished nest.

The second of these attached nests was made this year. It is much larger than the first, and has a different sort of entrance. The way into the first nest is by a round hole just in the middle and at the top of its convex curve. The second one is entered by a large irregular aperture in the left-hand corner, being a space left in the construction, by not carrying the edge of the nest at that place up to the wall of the house. This mode of entrance might be thought extremely inconvenient, but the birds constantly approach it at a right angle and make a sudden sharp turn into it, with no diminution of their customary speed. This performance will remind the old coach traveller of the way in which four horses and the vehicle were suddenly whisked round at Guildford, and got through an entrance that was barely wide enough for their admission, and at right angles to the road.

Three nests were made two or three years ago under the eaves of a lower part of the house on the north side, but well protected against the violence of wind and rain. The droppings from the young birds being inconvenient at this spot, a board was put under the nests to catch them. The birds did not approve of this alteration, and took the trouble to construct fresh abodes in worse positions rather than put up with it. Perhaps the board was placed nearer to the nests than they approved of. It might also have offered too convenient a resting-place for enemies wishing to attack them, which once happened when one nest was used by other birds.

It is well known that the house martin will often make experiments, before determining the site of a nest, by sticking little bits of mud to a wall; but works of this kind have been noticed for several years when no more nests seemed to be wanted for that season. Were these elementary building lessons for the benefit of the rising generation, or preparations for a subsequent season? The latter may be probable, though why should they put some dozen or more patches all of a row when only a few would be used? Anyhow, those who had not been builders in a previous year would have an opportunity of seeing how the process was commenced.

In "The Birds of Sherwood Forest," an interesting book by Mr. Sterland, the writer, speaks of the eaves of buildings, or corners of windows as the most favourite spots for martins building, "but," he adds, "I have never met with a nest in such places open at the top, as I have frequently seen it represented in works of natural history. In one recent book, the illustrations of which are generally very faithful, the nest is figured as a shallow dish fixed to a wall and entirely open at the top. Surely this must be a mistake, or if drawn from nature it cannot be taken as the type of the nest of this species. All that I have ever seen have had their walls carried up until they met the projection under which they were built, leaving a rounded hole immediately under the angle of the tile, or cornice."

In none of the nests which it is the purpose of this paper to describe could the form be likened to "shallow dishes," but the open tops have been common. Mr. Sterland is not likely to be mistaken in his observations, and if open-topped nests have been unknown in the regions of his observation, it is strange that they should be found elsewhere. The inference seems to be that variations from a normal pattern may be local; and perhaps a careful comparison of the building proceedings of the martins in different counties might throw light upon their ways, and lead to a higher view of their intelligence.

I may be fortunate in having my house frequented by a more experimental race of martins than are common, and there may be an advanced thinker among them, analogous to the reformers who sometimes spring up amongst stagnant tribes of men. I cannot, however, venture to flatter myself that this is the true explanation until I hear the result of careful inquiries in other quarters. All I can say is, that the martins' nests round my roofs exhibit nearly as much variety in form as the houses of the human folks in the village below, and a reason can usually be seen for the variations they display. Open-topped nests have been found in the most sheltered places. When the birds' abodes have been built in couples, like the semi-detached villas in the outskirts of towns, the entrances have been arranged so as not to come too close together. The distances are sufficient to render collisions of out-going and in-going birds improbable, and I notice other adaptations of means to ends.

In summer the martin families seem scarcely to sleep at all. At midnight and at early morn the young ones twitter. Late in the evening the parents keep up their elegant flight, and they are at it again as twilight passes into dawn.

A PHOTOGRAPH IN NATURAL COLOURS.

By J. TRAILL TAYLOR.

JOHN RUSKIN somewhere says that he would rather have a good engraving than the best of coloured prints. He speaks of course of those copies of masters' works which are produced with more or less approximation to fac-simile by the mechanical printing-press. And so far as many of these are concerned he may not improbably be in the right; but there remains the fact that colour, to almost all eyes, is the life of a picture. The black shades that give the form at best put before us a thing that is cold and dead, and it is only when the glow of colour is brought out on face or landscape that its full charm can enter and possess the mind. Except in the case of those who are to some extent unsusceptible to the effects of hue, either through partial colour-blindness or from want of taste or education, this is so true that we are disposed to think that a very indifferently executed picture as to its tints may really carry higher meanings to its beholders than a mere image in black and white. Without colour it is not possible to have true harmony—such harmony as a summer landscape presents, for instance. Abstract from that landscape all its wealth of green, and what would then remain that would tell of the season or express its poetry? It might be either autumn or spring for aught its black and white might say.

Colour, too, is one of the highest elements of expression in the human face, the pale lips speaking of fear or fixed resolve; the flushed brow telling of anger, rude health, or the glow of hope. True, the engraver may manage with little subtleties to hint at some of these things, and the etcher sometimes shows a wonderful faculty for simulating colour, but the fact remains that colour is the life, and that without it we but half know what a face or a landscape has to say.

With regard to photography, the immediate subject of our remarks, it is certain that the want of this element of pictorial beauty was very early felt. The chemicals had done so much; could they not be made to do a little more, and catch the shifting hues of life in all their flush and glow? A dream of this possibility has never been altogether absent from the minds of chemists since the wonders of what photography actually can accomplish were first revealed to view. In spite of many failures which have come of attempts hitherto made to realise this hope, it is by no means yet surrendered by all. At the same time, it has seemed sufficiently near impossibility of realization to induce many to turn aside and seek the fulfilment of it by other than photographic means. The hand of the painter or lithographer has been called in to eke out the shortcomings of the chemical rays; and no insignificant success has been attained in the effort to combine science and art in this field. There are thus two distinct lines of enquiry, which we shall endeavour to trace separately.

It is not a little singular that while the present high state to which photography has already been brought is owing mainly to the inventive faculty and prolific process-engendering research of Englishmen, it is to a few French philosophers that we are indebted for nearly everything that has yet been effected in the direction of photo-polychromy, both in the natural or chemical, and in the mechanical departments.

So long ago as 1839 the late Sir John Herschel noticed the interesting fact that paper which had been rendered sensitive by chloride of silver, and had been exposed to light until it had darkened, was then in a condition to reproduce various colours when again exposed to the action of light under strongly-coloured pieces of glass. Although Sir John Herschel did not continue this line of investigation, he says that he saw enough from his first experiments to warrant him in arriving at the conclusion that photography in natural colours might reasonably be expected to be brought within the range of possible accomplishment. It is an act of justice to our distinguished countryman to place on record the fact that the most successful pictures in natural colours that have been produced in times of more recent date, and by other experimentalists, have been obtained by means of processes based on his discovery of the capability of sub-chloride of silver to receive coloured impressions. Guided by the directions of M. Poitevin, we have taken many pictures in natural colours by a method discovered by the latter gentleman, but under which there ran as a substratum the previous discovery of Herschel which, if crude, was at least suggestive.

It may be observed here, that a valuable suggestion to experimentalists in heliochromy is to be found in the fact that ordinary writing-paper that has been immersed in, or floated upon, a very weak solution of common salt in water, and when dry impregnated by being brushed over with ammonio-nitrate of silver, will show several colours with a degree of distinctness sufficient, at any rate, to admit of their being easily recognised. Ammonio-nitrate of silver is in reality a solution of oxide of silver in nitrate of ammonia, and is made by adding to a solution of nitrate of silver of any degree of strength—usually one of sixty grains of the argentic salt to the ounce of water—strong ammonia by one or two drops at a time. At first, a dense precipitate of oxide of silver is formed, but by continuing the addition of the ammonia it is re-dissolved, and the liquid becomes clear again. There are other methods of preparing paper on which the colours of the spectrum may be obtained; for example, by first washing the surface of the paper with nitrate of silver, and then with fluate of soda; or by means of chloride of barium and nitrate of silver, with a previous wash of a dilute solution of iodine. Experiments with these substances will prove suggestive, in addition to the interest that will attach to the experiments *per se.*

There are three definite methods of producing, by purely chemical means, photographs in natural colours, and these differ entirely from each other.

The writer of the present article had in his possession during the summer of 1863, a miniature case, containing a silver plate on which was impressed the spectrum in all the colours of nature. He obtained it from the late Sir David Brewster, whose property it was, for the purpose of exhibiting it to H.R.H. the Duke of Edinburgh (then Prince Alfred) and Lord Brougham. The colours were decided, although not brilliant; and a special point of interest lay in the incident of this picture having been exposed to solar light for nearly a fortnight, without any signs of fading being discovered. The photograph in question was produced by M. Becquerel, who, more than any other, has made heliochromy the subject of definite and protracted investigation. These investigations have resulted in the discovery of two processes, by which colours can be obtained. These we shall describe in detail.

Bichloride of copper and sulphate of soda are formed by double decomposition, arising from the mixture of one ounce of powdered sulphate of copper, two ounces of powdered chloride of sodium, and five ounces of water. These are mixed in a test-glass. Of this solution three ounces are mixed with a like quantity of a saturated solution of common salt, and eighteen ounces of water; and this forms the sensitising bath for the silver plates. A metallic plate faced with silver, bearing a high polish, and made very clean, is immersed in this solution, and is rapidly coated with a violet-coloured subchloride, when it is withdrawn, washed, and dried. This is all the preparation required for fitting it to receive the colours of the spectrum. It is not very sensitive, for although it bears comparison with the sensitiveness of ordinary photographic paper in the earliest days of the art science, it is very much slower than any of the preparations used at the present day for obtaining pictures in the camera. With a lens of large angular aperture, a brightly lighted landscape might require an exposure of an hour. Not so, however, if the light were allowed to act upon the sensitive surface through coloured or painted glass superimposed on the plate; or if the solar spectrum were allowed to fall upon it; under these circumstances, an exposure of a few minutes would suffice.

Another process of M. Becquerel, in which galvanic electricity is employed in the preparation of the plates, gives excellent results, and is the one by which was produced the spectrum in the miniature case referred to as having been presented by him to Sir David Brewster. This process was discovered in 1849, or about ten years after Sir J. Herschel's first experiment. He had observed that red rays which exercise almost no action upon sensitive paper prepared in the dark, acted much more rapidly upon the same paper after it had been exposed to light; and that while the paper in the first of these conditions gave only a brown or slightly violet colour in the actinic rays, paper in the second state, or after the exposure to light, gave variable colours, recalling those of the rays which produced them, and even developed those colours in the less refracted part of the spectrum. Pursuing the subject, he was led to a certain method for preparing plates, on which could be received coloured impressions, and which, divested of many reasons which M. Becquerel communicated to the writer for the adoption and discarding of certain lines of procedure, is substantially as follows:—An ordinary daguerreotype plate—that is, a plate of copper faced with silver—is polished, and the back having been protected by varnish, is attached by means of two hooks to the positive pole of a voltaic battery. To the negative pole is attached a piece of platinum foil, and both are then immersed in diluted hydrochloric acid, one part of pure acid to eight parts of water being a suitable degree of strength. In the course of about a minute the plate will have passed through several stages of colouration, including grey, yellow, blue, green, rose, violet and blue. When of a blackish violet colour the action is stopped and the plate is washed, then slightly rubbed with cotton wool, and dried. It is now ready for being impressed with the colours of the spectrum. If a plate so prepared be heated before exposure, it acquires new properties. It changes to a red colour, which is the state most suitable for receiving all the colours. Exposure to the sun under a sheet of paper impregnated with sulphate of quinine confers the same properties as the exposure to heat.

Photographs in natural colours, produced by the late M. Niepce de St. Victor, were recently exhibited at a meeting of the London Photographic Society. Having repeated M. Becquerel's experiments, already described, M. Niepce conceived that it was possible there might be some relation between the colour which a body communicated to a flame, and the colour which light developes on a plate of silver that had been chloridised with the body that coloured that flame. The bath in which he immersed the plate was formed of water saturated with chlorine, to which he added a chloride endowed with the property of imparting to flame the colour he wished reproduced on the plate. Strontium, for example, caused the red and purple tints to be vividly impressed; to excite the orange ray, calcium or uranium was used; sodium or copper sufficed for the yellows, and so forth. Pictures possessing a fair degree of brilliancy in their colours may be obtained in this way.

An important advance in heliochromy has been made by M. Poitevin, who, instead of using metallic plates, has employed paper as the groundwork of his experiments. Starting on the basis of Herschel's original discovery, he takes paper and prepares it with subchloride of silver. The method of doing this is known to every intelligent photographer, consisting in exposing to light ordinary chloride of silver, whether spread upon paper by the usual washes, or formed in collodion, as suggested by the late R. J. Fowler, the advantage of the latter being that it opens an avenue for reproductions—*à la* negative—from the picture first obtained. Confining our description to the production of heliochromes upon paper, with which we have conducted numerous successful experiments, we observe that the problem which presented itself was the production of a subchlorised paper and of an oxidising agent which, when exposed to the coloured rays, should reproduce those colours with the greatest possible degree of brilliance, and in the shortest possible period of time. How far these conditions have been fulfilled will presently be seen. A piece of subchlorised paper was floated for a minute on a saturated solution of bichromate of potash, a twenty-grain solution of chloride of potassium, and a saturated solution of sulphate of copper mixed together in equal volumes, and was then hung up to dry in a dark room; after this it was ready for exposure. The record of one trial will afford an opportunity of judging how far success was achieved. Some brilliantly-painted magic lantern slides were obtained, and under these were placed pieces of the sensitive paper, the whole being then exposed to the light of a cloudy sky. After an exposure of fifteen minutes, one of the pictures was taken into a darkened room and examined. The subject was an Arab with a red garment, the sky being blue, and the surrounding subjects green and yellow. The order of the action of the spectral rays was quite the reverse of what it is in photography of the usual kind, for the red cloak was strongly impressed in a good bright red, while the blue had scarcely begun to undergo a change; the green and yellow, too, were inferior to the red. After a continuation of the exposure for another quarter of an hour, a further examination showed a great increase in the detail of the refrangible rays, while the red had not become in the least degree deteriorated by the increased exposure. A more prolonged exposure brought out all the colours. The prints were fixed by immersion successively in water, acidulated with chromic acid, a weak solution of nitrate of lead and distilled water. At the time of these experiments we were not favourably impressed with the perfection of the fixing; nevertheless, the heliochromic prints thus produced remained in a tolerable state of preservation for about eight weeks, after which time they were presented to a friend.

By modifying Poitevin's formula, M. de S. Florent has greatly increased the sensitiveness of the paper as well as the perfection of the fixing. By immersing the subchlorised paper in a bath of a very weak solution of nitrate of mercury, and after partially drying with blotting paper, the sheets of paper are rendered so sensitive as to receive from a superimposed coloured *cliché* a vigorous impression in a quarter of a minute. By adding to the bath just mentioned a mixture composed of two parts of a saturated solution of bichromate of potash, two parts of sulphuric acid, and one part of chlorate of potash, the colours are more vivid without any diminution of the sensitiveness. Fixing is effected in ammonia diluted with alcohol in the proportion of one part of the former to twenty of the latter, followed by immersion in an alkaline chloride solution. Curiously enough, an increased degree of rapidity is found to be obtained if the subchloride of silver formed on the paper is prepared by exposure to light under a violet glass. The end of the year 1873 finds the problem of photography in natural colours far nearer a satisfactory solution than did the beginning of the year. It is to be regretted that the number of experimentalists in this field of research is exceedingly limited.

Pending the bringing to a practical issue the production of natural polychromatic photographs, attempts, some of them tolerably successful, have been made to combine colour and photography by means more or less mechanical. A few years ago, Mr. Robert Howe Ashton obtained a patent for a method of applying colour to photographs produced by the Woodbury process—a method which, although it has not yet been commercially developed to the extent its merits demand, is yet ingenious, simple, and capable of producing good results. In the Woodbury process, the lights and shadows are formed of transparent-coloured gelatine in various degrees of thickness, the deepest shadows being composed of the greatest thickness of the gelatinous ink. If a picture of this kind be printed upon a sheet of white paper, it will be in monochrome; but it occurred to Mr. Ashton to try the effect of producing flat tints by means of chromo-lithography on paper, and upon this printing the Woodbury picture in correct registration with those tints. This, in principle, is totally different from the colouring of a photograph by applying pigments upon it; for, in this latter, every touch of pigment hides some portion of the sun picture, whereas in the former the photograph is left quite intact; thus nature can be imitated with a wonderful degree of exactness, the most delicate details of the photograph being harmoniously wedded to colours possessing all the vigour of the artist's pigments. To produce by this process a thousand or more photographs, the services of a skilled artist are required only to arrange the colours on the various lithographic stones, and when this is done the production of the chromo-lithographic print which is to act as the substratum for the photograph, as well as the photograph itself, is a mere mechanical task, requiring technical care, but no artistic skill. It is anticipated that the application of colour in this way to Woodbury prints will be extensively carried out during the year now begun.

At the last meeting of the British Association much interest was manifested in a new process of photo-polychromy which was described, and specimens exhibited on behalf of M. Léon Vidal, of Marseilles, the inventor of the process. It is based upon the carbon or pigment printing process, but resembles in principle chromo-lithography. Every different tint that is shown in the picture implies a separate print, the colouring matter of which is set off or transferred to the sheet upon which the finished picture is finally impressed. As many negatives must be employed as there are colours to be printed. For example, suppose that the subject to be photographed were a web of tartan composed of three colours—red, blue, and yellow; three negatives would have to be taken, and from the first every part except that which is to be printed red would be stopped out with opaque varnish, and the same in respect of the other two. Three sheets of pigmented sensitive paper, one being of each of the colours required, are now printed by the ordinary process of photography, the red pigmented paper being printed by the corresponding negative, and the same with the yellow and blue colours. Each picture when finished has only a portion of the pattern impressed upon it. But the paper is thin and transparent, and the colouring matter readily sets off upon another sheet which forms its permanent abiding place. The first portion of the picture is laid down upon this final support, and by sponging the back of the transparent impressed paper with alcohol the image leaves the latter and adheres to the former sheet. The second picture is now laid down in a similar manner; and the temporary support being transparent, correct registration is secured by moving the second picture until each colour is seen to fall into its proper place. Finally, the third colour print is treated in the same way, and the completed picture contains every portion of the subject, although printed by three different operations. The mixture of colours may be practised to any desired extent; thus, if green is to be printed, the corresponding portion would be stopped out of the red-print negative only, and so with the other secondary colours purple and orange.

The foregoing was, in effect, the description which the writer of this article gave of M. Vidal's process of photo-polychromy at the British Association, and the specimens which he exhibited, showing the production of a picture in all its stages, were received with a marked degree of interest. The process certainly is ingenious, and the prints are very brilliant. They have been compared to chromo-lithographs, but although produced in a manner somewhat analogous, there is this important difference between them, viz., that while in the latter there is no real gradation of tone, the chromo-photograph possesses all the delicacy of shading and tint peculiar to a photographic print. But as the production of each tint is the result of an exposure of several minutes to the light of the sun in a printing frame, it follows that speed in the production of these prints can never be attained. If rapidity be desired, a method of printing similar to that of lithography must be employed, and fortunately in the now extensively adopted system of "heliotypy" or "collotypy" we have that very means at disposal for producing photo-heliochromes in a manner far more rapid and equally effective than that of M. Vidal. We shall present an outline of a modified process of this kind by which we have seen a number of coloured prints of great merit produced by an ordinary Albion printing-press, and it is very probable that this process will soon be extensively adopted for commercial purposes, for this reason, that while the proofs possess all the brilliance of chromo-lithographs, they further possess the accuracy and beautiful gradation of photographs.

The negatives are prepared in the same way as those of M. Vidal, viz., there must be as many negatives as there are colours to be produced, and from each negative must be stopped out every part that does not represent one special colour. Suppose that the subject is a rose tree, clad with foliage, and adorned with red flowers. Their negatives must be obtained by superposition from one original; all but the flowers, the trunk, and the leaves are then respectively stopped out in the separate negatives. Three printing "formes" or surfaces are now prepared—one for each of these negatives; the printing surface being, of course, gelatine, as adopted in the heliotype processes. The leaves are first printed, being inked with green. These will appear in all the beautiful delicacy, and with all the structural detail visible in the original tree. When the requisite number—hundreds or thousands—of impressions has been printed, the printing surface is removed (one press only is now supposed to be employed), and a second "forme," representing the flowers, is inserted in the bed of the press. This is inked with a transparent red lithographic ink the colour of the flower; and the partially-printed proofs are sent through the press a second time, care being taken to keep correct register. A third time the operation is repeated, so as to introduce the brown colour of the trunk and branches. If necessary, colours may be made to overlap.

What we have said about the printing of flowers applies equally to that of landscapes, portraits, and indeed to everything that can be represented by photography.

ON NAME AND RACE IN ENGLAND.

By Dr. RICHARDSON, F.R.S.

THE object of this paper is to suggest a theory that there exists a relationship between the names of the native inhabitants of this island and the races to which they belong. I mean between the *sur*names and the race. If the theory be true, it opens up a subject of physiological and even of political importance. It tends to establish the view, which I, for my part, believe to be correct, that distinct races, however closely they may be united together, by residence in the same country and by intermarriage, retain their original quality of race; that all change from admixture is but transitory in character; and that there is no end to any race except by its extinction.

Camden was the first to point out, in his "Remaines concerning Britain," a fact which even in this day is known to very few persons—viz. that the use of surnames or family names did not begin until about the time of the Norman Conquest. This, as a general fact, is now admitted, but there are certain exceptions of a rare character which indicate that the custom of adopting a surname existed a little before the Conquest. Thus Mr. Mark Antony Lower, in his learned work "The Patronymica Britannica," shows from a document in the Cottonian MSS., which must have existed earlier than 1066, that one Hwita Hatte, a keeper of bees in Hæthfelda, had a daughter Tate Hatte, who was the mother of Wulsige the shooter, and that Lulle Hatte, the sister of Wulsige, was wife of one Hehstan in Wealadene. Other members of the same family are also named; but so peculiar is the fact of such naming, that Mr. Ferguson maintains, says Mr. Lower, for the existing family of Hatt, that it is probably the oldest hereditary surname we have on record. The following quotation from the learned author of the "Patronymica Britannica" is definite: "I see no reason for departing from the year 1000 as the proximate date for the assumption of family names. The practice commenced in Normandy, and gradually extended itself into England, Scotland, and Ireland."

From the time named, the year 1000, the practice has continued, and has extended so greatly that at this time there are probably 35,000 surnames. In the sixteenth Annual Report of the Registrar-General (1856) thirty-two thousand eight hundred and eighteen different surnames were recorded, and the list has increased since that time.

I turn again to the "Patronymica Britannica" for a summary of facts, in the addendum of that work, bearing upon things, circumstances, or qualities from which surnames are derived. Camden had already, in his time, discovered a considerable number of origins, although his list does not approach the modern one to which I am about to refer. This modern list, extracted from the documents in the office of the Registrar-General, and compiled by a gentleman once in that office, is, Mr. Lower states, authentic in every particular. It places names, in relation to their origins, under the following heads:—

1. *Names from words relating to religion:* as Church, Font, Priest, Prophet, Dean, Parson, Minister, Tomb, Heaven, Hell, Sermon, Crucifix, Grave, Sexton, Clerk, Beadle, Verger, Crozier, Paternoster, Surplice. 2. *From words relating to the mineral kingdom:* such as Gem, Jewel, Stone, Gold, Silver, Steel, Diamond, Ruby, Glass, Flint, Chalk, Salt, Carbon, Clay, Slate, Ruddle. 3. *From words relating to the vegetable kingdom:* such as Tree, Ash, Birch, Cork, Date, Holly, Lemon, Oak, Pine, Root, Leaf, Bark, Clover, Hay, Cotton, Briars, Cane, Heath, Hazel, Garland, Poppy, Violet, Pink. 4. *From words relating to buildings, their parts, &c.*: such as Scaffold, Trussel, Smithy, Lodge, Barn, Mill, Castle, Barrack, Bastion, Mole, Temple, Pillar, Tunnel, Well, Pantry, Dairy, Kitchen, Chambers, Roof, Rafter, Larder, Window, Oven, Tile. 5. *From words relating to war and its concomitants:* such as Warrior, Sword, Dagger, Gun, Cannon, Guard, Staff, Corps, Rank, Shield, Banner, Archer, Bow, Arrow, Camp, Conquest. 6. *From words relating to moods and temperaments:* such as Eatwell, Cram, Nice, Savoury, Joy, Jest, Gay, Merry, Jolly, Witty, Reel, Mock, Hunt, Heat, Sport. 7. *From words relating to musical instruments:* such as Buglehorn, Fiddle, Fife, Horn, Pipe, Tabor, Drum, Timbrel, Harp. 8. *From words relating to epochs of life:* such as Birth, Marriage, Wedlock. 9. *From words relating to shapes:* such as Square, Round, Cone, Globe, Angle, Circus, and Circuit. 10. *From words relating to books:* such as Chart, Deed, Reams, Book, Page, Press, Print, Quire, Quill, Ledger, Annals, Charter, Letter, Card. 11. *From words relating to points of the compass:* such as East, West, North, South, Southern, Western, Bisouth. 12. *From words relating to implements, tools, or commodities:* such as Parcel, Bale, Pack, Box, Coop, Tub, Awl, Saw, Nail, Hone, Punch, Candle,

Coke, Couch, Bell, Scraps, Shell, Stirrup. 13. *From words relating to characteristics:* such as Pout, Loon, Late, Regular, Greedy, Dupe, Rant. 14. *From words relating to qualities:* such as Carnal, Anguish, Grief, Fear, Guile, Dudgeon, Proud, Sly, Vain, Lawless, Sawney, Quaint, Shallow, Vague. 15. *From words relating to clothing and ornaments:* such as Garment, Hat, Hood, Cap, Tippet, Shirt, Bonnet, Sash, Patten, Feather, Vest, Hose, Stocking, Gaiter, Brace, Tape, Diaper, Poplin, Silk, Pocket, Plush. 16. *From words relating to diseases and their concomitants:* such as Fever, Palsy, Gout, Fits, Boils, Rickets, Whitlow, Corns, Chap, Glanders, Spavin, Leper, Pill, Balsam, Bolus, Physic, Lancet, Mortar. 17. *From words relating to liquors:* such as Wine, Port, Sherry, Claret, Negus, Whisky, Stout, Eggbeer, Mead, Perry. 18. *From words relating to colours:* such as Blue, Green, Purple, Scarlet, Lake, Roan, Buff, Grey, Lavender, Cherry, Peach, Sable, Black, White. 19. *From words relating to titles:* such as King, Queen, Noble, Knight, Squire, Baron, Lord, Margrave, Templar, Rex. 20. *From words relating to money:* such as Coin, Cash, Guinea, Pound, Shilling, Sixpence, Penny, Farthing, Mark, Noble, Tester, Pottle, Gill, Mile, Cubit, Furlong, Yard, Inches, Road, Tod, Last, Pound, Barrel, Tons. 21. *From words relating to the weather:* such as Cloud, Dew, Fog, Sky, Mist, Thaw, Sleet, Hail, Rain, Wind, Tempest, Thunder, Lightning, Day, Noon, Star, Manyweathers. 22. *From words relating to figures:* such as Cipher, Unit, Two, Twelves, Score, Twentyman, Forty, Gross, Even, Double. 23. *From words relating to times and seasons:* such as Spring, Summer, March, May, Monday, Halfnight, Yearly, Feveryear, Christmas, Lent, Pentecost. 24. *From words relating to commerce:* such as Trader, Seller, Barter, Pay, Ransom, Bonus, Sale, Scrip, Loan, Borrow. 25. *From words as participles and verbs:* such as Boiling, Buzzing, Riding, Raving, Slaving, Weaving, See, Seek, Took, Gaze. 26. *From words used as adjectives:* such as Sturdy, Lusty, Doughty, Weakly, Dainty, Tidy, Ready, Pretty, Friendly, Bandy, Crisp, Humble, Slender, Weary, Neat, Dandy, Trollop. 27. *From words relating to singular occupations:* such as Pincher, Gamester, Smiter, Smoker, Bouncer, Bruiser, Snapper, Leader. 28. *From words meaning much ado:* such as Freak, Pother, Row, Rout, Mummery, Cant, Gossip. 29. *From words relating to the Voice:* such as Tone, Tune, Sing, Bass, Shout, Yell, Howl. 30. *From words relating to acts, motions, &c.:* such as Sleep, Strong, Gallant, Jump, Steady, Start, Trip, Step, Stride. 31. *From words relating to gambling:* such as Game, Swindle, Chance, Hazard, Raffle, Billiards, Skittles, Dice. 32. *From words relating to qualities:* such as Rich, Richman, Mean, Stern, Cross, Smart, Haste, Speed, Moist, Damp. 33. *From* *words designating rivers, mountains, nationalities, &c.*: such as Boyne, Derwent, Tyne, Humber, Severn, Nile, Jordan, Snowdon, Alps, People, Tribe, Kentish, Saxon, Norman, Jew, Kaffir, Pagan. 34. *From words signifying relationship and condition in life:* such as Parent, Stranger, Mother, Sire, Daddy, Husband, Bride, Orphan, Godson. 35. *From words indicating occupations, employments, and offices:* such as Mason, Tiler, Plumber, Builder, Fuller, Potter, Drover, Warder, Clerk, Poet, Mariner, Miller, Baker, Carman. 36. *From words signifying personal names:* such as Eve, Cain, Abel, Moses, Herod, Stephen, Prudence, Ajax, Hector, Fabian, Livy, Rufus, Tudor. 37. *From words referring to comestible and potable things:* such as Food, Feast, Fish, Fowl, Bacon, Hogsflesh, Peasoup, Marrow, Onion, Tiffin, Ginger, Grapes, Quince, Crumb, Lard. 38. *From topographical words*: such as Land, Meadows, Fields, Garden, Way, Highway, Rock, Cliff, Ferry. 39. *From words descriptive of parts of the body:* such as Eyes, Tooth, Sconce, Collarbone, Bowels, Blood, Withers, Pluck, Kneebone, Spittle, Gall, Beak. 40. *From names relating to ships and their associations:* such as Fleet, Hulk, Craft, Cutter, Tug, Keel, Cable, Oar. 41. *From names signifying birds, quadrupeds, reptiles, insects, and fishes:* such as Bird, Kite, Crane, Buzzard, Finch, Snipe, Swallow, Robin, Gull, Crow, Pigeon, Bantam, Gander, Gosling, Brute, Lion, Fox, Wolf, Stag, Leopard, Bear, Morse, Hart, Hind, Rabbit, Coney, Hare, Pig, Cur, Goat, Lamb, Mouse, Cricket, Flea, Bug, Mite, Fish, Shark, Salmon, Herring, Carp, Pike, Gudgeon, Trout, Sole, Pearl, Barnacle, Cockle, Cuttle. 42. *From words relating to countries and places:* such as Albion, Wales, Gaul, Congo, China, Sidney, Calvary, Gath, Dorset, Troy. 43. *From words relating to London and its suburbs:* such as London, Strand, Holborn, Harrow, Poplar, Hampstead, Kilburn, Richmond, Kew, Aldgate, Brixton.

It will be noticed in this list of names that several of a characteristic kind are not included. Thus the whole of the names having the prefix of *Mac*, *Ap*, and *Fitz* are omitted as well as those having the suffix of *son*. These, however, are compound names, and are not essential to the classification afforded by the return.

The compiler who has recorded the names, some of which we have given by way of illustration, has divided his list, as we have seen, into forty-three parts. This was a convenient division in extracting and tabulating the names from the mass that lay before him. But when we come to an analysis of the facts, we find, as a primary and important truth, that there are only a few origins of the names. We may divide these origins into the following parts:—

First and simplest, there are the names that may be called *personal*, that is to say, surnames derived from some name which would now be called a Christian name, and which, in countries where the Christian faith is predominant, would *legally* be so called, to whatsoever creed the possessor of it might belong. These names, which, as I say, we call *Christian*, were once the only names; and it is a strange fact, perhaps one of the strangest facts in history, that though now they seem only to be an addition to a surname, they still constitute the true legal name of the owner. The surname, in brief, commencing only as a fashion, remains as such. It may be changed at pleasure, but the Christian name attached to it, and which specially marks its owner, can only be changed by legal process. It was natural, therefore, in the change of fashion which took place when surnames were introduced, that the name by which the person had previously been known should either remain as a surname or should be modified by a prefix such as *Mac*, the son of, or a suffix, such as *son*.

In the second instance, there are the surnames derived from names of places, towns, counties, countries, villages, even houses in which the person lived. The man would be so-and-so of such a place, or so-and-so belonging to such a place: if the place were his possession, he would be *of* it; if he merely lived in the place, he would be belonging *to* it.

In a third division of names come those derived from some inanimate substantive things, such as a stone, a forest, a wood, a mill. The person would be called so-and-so of the mill or of the forest, upon which, by a very slight change, would follow, in many cases, names from occupations connected with the substantive things, such as Miller, Forester, and so forth.

In a fourth instance were surnames derived from the names of animals, which names, characteristic of the animal, were sometimes names of contempt.

Fifthly, there were names derived from qualities of the mind or of the body of the person. The man was sprightly, or strong, or frail, or hardy, or proud, or wild. Even peculiarities arising from disease would come under this head, such as Fits, Splayfoot, Leper, or Rickets.

In a sixth and last division were names signifying something more than qualities, that is to say, titles derived from estates, affairs or offices, such as King, Noble, Crown, Court, Judge, Sheriff, and the like.

I have reduced these derivations of names to a few simple and natural forms, in order to lead to the theory I would propound respecting name and race. When the fashion of surnames came into use, the surnames were taken, necessarily, either by accident in each case, or by some order or design which, though not apparent to, nor systematically intended by, those who assumed or conferred the names, was, nevertheless, systematic in result. If the names were taken or applied by accident, then it should follow that in all the races that made up the community the names were indiscriminately mixed. If a systematic plan were consciously or unconsciously followed, then particular names would be stamped upon and adhere to particular races. This latter position is what I believe to be the fact: I mean that at the time when surnames became the fashion in England, the fashion was varied according to the races which then existed.

To make this theory clear, it is essential to glance at the races which existed on the soil at the time when the surname became a part of the national history. At this period there existed three distinct populations at least. There was the Celtic population, which had been driven, by Saxon encroachments, from the centre of the island to the mountainous districts, to Wales, and to the Highlands of Scotland; there was the Saxon population, which held the most dominant sway, and which was enormously increased in number and power by the Norman Invasion; and there was a Jewish population, the extent of which is not known, but which could not have been inconsiderable. To these might be added, though I exclude them from the present argument, the remains, probably, of a Roman population, and a fragment of a nomadic or gipsy fraternity.

Among these three great races, then—Saxon, Celtic, Jewish—surnames were introduced, developed, and sent onwards. The origin of the names was limited in number, as we have seen, to a few heads, while the classification was devised, unconsciously perhaps, but, as I believe, methodically, by what may be called the peculiarities or idiosyncrasies of each race.

In respect to names that were *personal*, it is probable that all the races followed to some extent the same rule; but the Jewish race most distinctively followed it, and have held most persistently to it, although they have in course of time modified some such names, as when they have turned Abraham into Braham; Levi into Lewis; Jacob into Jacobson; Moses into Moss.

Names derived from *place* were, I think, divided mainly between the Celts and Saxons; the Celts especially taking names derived from localities. This fact becomes most noteworthy when a list of names is taken up, the Celtic nature of which is settled by the prefix of *Mac* or *Ap*.

Names derived from inanimate things and from occupations were assigned, I think, almost exclusively to the Saxons. This was in strict accord with their character as a race. The same rule applies also to names derived from occupations, trades, professions, and callings. There exists in such naming all the elements of Saxon straightforwardness and simplicity. The Saxons were the workers of substantive things; they felled the woods, hewed the stones, dug the ditches, ploughed the fields, sowed, thrashed, and ground the corn, made the bread, constructed the houses, delved for the metals, netted for the fish, braved the ocean, hunted the wild beast, shepherded the flocks, took charge of the castle or the prison, and in a word pursued all those rude but necessary arts by which a country, as yet devoid of refinement, and unacquainted with active commerce, is prepared to become, by the aid of other hands, refined and commercial. Thus it was natural that to them should be attached the names pertaining to the substantive things they were employing, or to the businesses upon which they were employed; nor can there be a better evidence of Saxon birth and race than a surname so derived.

It would hardly be fair, however, to say that the Saxon mind showed no sign of embellishment of name by adjective synonym. It sometimes added words to express the quality of a name as it might have done of a substance. Camden gives us some very good illustrations of this nature, speaking, however, of English Saxons rather than of Saxons proper. For example, the words *ael*, *eal*, and *al*, in compound names signified all, or altogether. So *Ælwin* is a complete conqueror; *Ælbert*, all illustrious, or bright; *Aldred*, altogether reverend; *Alfred*, altogether peaceful; *Ælf*, meaning help or assistance, is combined with other words, as *Ælfwin*, assistant strength; *Ælfwold*, an assistant governor; *Ælfgifa*, help-giver. *Ard*, belonging to or a natural disposition. So *Godard* means a divine temper; *Giffard*, a liberal temper or disposition; *Bernard*, a filial disposition; *Ricard*, belonging to riches or wealth; *Athel*, or *Æthel*, means noble; so *Æthelred* is noble counsellor; *Æthelward*, a noble ward or protector. *Bald* signifies bold; so *Winbald* is a noble conqueror. *Cen* or *Kin* means kinsfolk; so *Cinéhlem* is a protector of kindred. *Cuth* signifies knowledge or skill; so *Cuthwin* is a skilled conqueror; *Cuthred*, a skilled counsellor; *Cuthbert*, a skilled, famous, or illustrious man. *Fred* means peace; so *Frederic* is wealthy peace; *Winifred*, victorious peace. *Helm* means defence; so *Berthelm* is distinguished defence. *Here* and *Hare* mean an army; so *Harold* is general of an army; *Hareman*, a chief man in the army. *Hild* is lord or lady; so *Hildebert* is illustrious lord. *Mathilda*, noble lady. *Mund* means peace; so *Eadmund* is happy peace; *Ælmund*, all peace. *Ord* means edge or sharpness; so *Ordbright* is clear or bright edge. *Rad* means counsel; so *Conrad* is skilled in council; *Rad* or *Radbert*, illustrious in council. *Ric* means powerful, rich; so *Alfric* is all rich or strong; *Ricard* is belonging to the strong or rich. *Sig* means victory; so *Sigard* is victorious power or disposition. *Stan* means a superlative man or thing; so *Athelstan* is the most noble; *Wistan* the wisest; *Dunstan* the highest. *Wiht* means strong, nimble, lusty, forming first part of many names, as *Wihtman*. *Willi* signifies many, a multitude; so *Willielmus* is defender of many; *Wildred*, respected of many; *Wilfred*, peace to many. *Win* means war, strength, or love and esteem; thus *Winfred* is victorious peace, as before named. *Wold* and *Wald* mean a ruler or governor; whence *Bertwold*, a famous governor, and *Æthelwold*, a noble governor.

I have quoted these illustrations, with one or two modifications, from Camden, as showing a series of Saxon namings lying a little apart from the mere names of things. This arrangement may have occurred, as will be gathered from the sequel, from a temporary admixture of the Saxon and Celtic races. But it will be observed that in all the illustrations given, there is in each name the Saxon mark of a noun or a substantive thing.

Names indicating qualities, having connected with them no substantive thing, but standing alone, are peculiar, I believe, to the Celtic race. Thus such names as Merry, Jolly, Glorious, Small, Slender, Crouch, Dandy, Friendly, Fair, Flight, Tidy, are good illustrations, as are many names to which the prefix *Mac* is attached. MacDonald, the son of Donald, derived, according to Lower, from *Donhuil*, brown-eyed, is a typical illustration.

Names derived from titles belong, I think, both to the Saxon and to the Celtic races, but most names of this kind are Celtic. The names having relation to some office that has been common to Saxon and Celt alike, would account for their introduction into both races.

Surnames derived from names of animals are very distinctive of race. They belong, it seems to me, from all the evidence I can collect, exclusively to the Jewish race. To take a few illustrations: Lion, Cavallo, Wolf, Hart, Stag, Fox, Lizard, Mole, Rabbit, Hare, Coney, Leveret, Lamb, Cockle, Doe, Parrot, Dove, Pigeon, Hund, Seal, Roe, Deer, Hirsch, and its modification Herschell—these and a great number of similar names, if I had space to record them, would support this view. In many instances, the names have been variously modified; and again, in many instances, the more striking characteristics of Jewish type and expression have been lost by temporary admixture of race, while the name has been retained; but the alteration is never so perfect as to cover the original facts. In the same way, there are often various modifications of Jewish personal names, but the change rarely, if ever, conceals the truth.

Now and then it may be noted that one name, coming from two sources, may belong to two races, so that there may be distinct families of the same name but of different race. For instance, there is the Saxon *Hare*, derived from the same name as Harold, and there is the Jewish *Hare*, derived from the animal. There is the Saxon name *Ross*, derived from a heath or morass, and the Jewish *Ross*, derived from a horse: these illustrations might be greatly extended.

If it be asked how it is the names of animals are attached to representatives of the Jewish race, how, indeed, it can be that they should have names, many of which would be repugnant to their religious belief and ancient faith, this is the answer most consistent with probability: that at the time when the fashion of surnames became common, the Jews, an inoffensive, resistless, despised race, had the objectionable names thrust upon them by those who surrounded them. Some names, perhaps, such as Lion, they might themselves have assumed, while those who were strongest amongst them would fall into the fashion by retaining as surname their original Hebrew name. The rest, less powerful, we may say practically powerless, would have forced on them the names even of contempt their masters chose to bestow.

To sum up. The theory I would present respecting the origin of surnames is, that at the time when the fashion of surnaming came into vogue it developed itself in the three great English races as follows: (*A*) That personal names, with or without the prefix of *Mac*, *Ap*, *Fitz*, or the suffix *son*, were made common amongst all the races, but were not universally adopted. (*B*) That the Saxon race and the Celtic alike partly adopted other names derived from places or possessions. (*C*) That the Saxons assumed, or had assigned to them, other names derived either from substantive things or from occupations. (*D*) That the Celtic race assumed, or had assigned to them, other names expressive of qualities of mind or body. (*E*) That a portion of the Jews had assigned to them, or themselves assumed, the names of various animals.

The theory as to the origin of names in England which I have ventured to propose in the preceding pages I would extend further, by assuming that what occurred at the origin of surnames continues to this day; that admixture of race by marriage, importation of new families from other countries, modifications of old names, and introductions of new names by native families, have done no more than bring a few trivial exceptions to the general rule pertaining to name and race. I submit, moreover, that the characteristics of each of the three great races occupying England and Scotland at the present time are, as a rule, stamped in the names which the individuals of the races bear.

When now we look at the pure Saxon, he is as he ever was. He is strong of body, fair of complexion, truthful, industrious, orderly, slow, sure, retentive, courageous, firm, and by comparison with the other races stupid. He fights to the death, and does not much fear to die; he loves devotedly, but his love is confined to his own immediate circle; he hates as intensely as he loves, but he hesitates to hate and confines hatred to a very few objects, his slow perception preventing him from extending widely his bonds either of love or hate. He wanders the earth, liking a home, but caring little where he finds it so long as it is comfortable and isolated; he is social but reserved, and abhors being inspected or overlooked. He paints little, sings little, has very small admiration for other men, and as small a contempt. His belief is in himself, not from any kind of self-satisfaction or active sense of his own importance, but from a want of consciousness that anyone else is necessary for his support; to the last of his life he does not foresee, but rather tumbles into, death. Politically he would level the social earth of distinctions as he would the physical earth of trees or rocks or mountains, caring nothing for the natural or artistic forms he has brought to the dust. He is, in fact, the rude pioneer of all the other races of the earth; he serves them all, asking of them no homage, and offering none; the other races follow him in his course, improve his work, and hold him ever in watchful awe.

These are the typical characteristics of the Saxon man, and, varied only in regard to sex, of the Saxon woman.

The race forms the mass of the English people. Caring little for rule or ruling power, and entering sparsely into what is called the governing class of the country, it nevertheless rules all classes. It is the muscular industry of the country, seizing invention; it deals with the solid substantive materials of the earth, and it takes mainly, to itself individually, the name either of the thing or of the work with which it is occupied.

When now we look at the Celt we see him still as he ever was. He is a strong, brilliant man, more active than the Saxon, but wanting Saxon machinery of body and Saxon will for steady labour. A man of quick temper, he is easily led into passion of mirth, or of anger, or of grief, and easily led out of either; loving and hating by turns the same object; ingenious in work and perceptive; quick to learn, and analytical. At home he is disorderly and improvident. Ready at any time to fight, he cares little, comparatively, for the object of the contest. Not so truthful as the Saxon, he is much more ambitious of applause. Liking to wield power, he aims to rule by series of fixed principles, which he can invent but cannot control. Fond of art, he is busy ever in turning Saxon heaps into goodly palaces. This race forms the artistic part and active social part of English life. It is a race of qualities, and in its surnames, when they differ from the Saxon, it takes what it represents, qualities as distinct from things.

When now we look at the Jew in England, he is as he ever was, except, happily, that he is less oppressed. Small of stature, dark of complexion, and, by comparison, feeble of limb, he differs as much from Saxon as from Celt, although socially he combines more readily with the Saxon. The labour of the Saxon in the fields, on the waves, in the factory, in the mines, he despises, or at all events disowns, for his part. The battle field of the Celt gives him no idea of glory. Still he labours hard in his own line, which is that of utilising commercially the muscular labour of the Saxon, and the artistic labour of the Celt. He maintains a strong sympathy with all his own race, but towards other races, though he may be friendly disposed to them, he feels no bond. His domestic life is, as a rule, the most perfect; and his vitality far surpasses that of his compeers. Provident of his own and protective of his brother, his rival races know so little of his poor, that a professed Jew in an English workhouse is a wonder. He carries, as his name, either that of his fathers, or the name that was imposed upon him when he was the most oppressed; or now sometimes changes his name, taking one of Latin origin, as Lawrance. These are the Jews proper, if I may use the term, those who still retain their peculiarities as a people; they, however, represent but a comparatively small proportion of the Jewish element amongst us. If name and type of body may be taken for evidence of race, as the theory I offer suggests, there exists in our midst a population of feebly Christianised Jews, equal probably, in numbers, to either of the other races, which blends with the other races, yet remains racially distinct, and unconsciously acts, in politics, commerce, and education, sympathetically and practically with the professed Jews, from whom it seems to hold itself apart.

In politics, the Saxon is democrat; the Celt aristocrat and feudal; the Jew neutral, but leaning towards the Celtic side, especially in his desire to found a great house and exalt his own race.

In religion, the Saxon is Protestant in the fullest sense of that word, to the extent, indeed, of claiming the most perfect right of judging for himself, and of hating any and every functionary who shall dare to judge for him. The Celt is Catholic; he is lured by the sacred symbolism of the Church; he lets his every sense luxuriate in the mysteries of the altar, and gives his emotions full play in the varying services of the varying seasons. He prefers, in the mystical solemnities of religion, that he should be guided; and so he maintains zealously what the Saxon would as zealously destroy, the order of the priesthood and its proselytising influences. The Jew, though differing from the Celt in belief, leans towards the priestly order, and accepts ceremonials, fasts, and rituals, which to the Saxon would be intolerable; but he never proselytises, and, indeed, rarely speaks of the faith he holds nearest to his heart.

All these peculiarities belong to our English races, together with others of a physiological kind, which now I cannot include. They belong also through race to name; and the day I take it will come, when the Priest, the Politician, and the Physician will respectively learn many a useful lesson from a knowledge of this relationship.

REVIEWS.

THE ANTIQUITY OF MAN.*

HOW vast this subject seems to one who has suddenly wakened from the old Biblical faith in the world of six thousand years. How wonderful must appear to him the manner in which the geologist builds up from the most varied sources a chronology compared with which that of the Bible is but ephemeral. And yet how simple is the entire question to anyone who will impartially look first on the one picture and then on the other. Of course we do not mean to suppose that the subject is not surrounded by difficulties, for many of the questions which Sir Charles Lyell raises in this present volume are most vast and varied, and will probably take a generation at least for their final solution. But who, if he has any scientific power of mind, can even cursorily read this splendid treatise on man's age in the world without rising up with the solid conviction, that the tale of the Bible as told by most is as empty and as shallow, as incomprehensive and as unphilosophical, as it is possible to be. We trust that many will take up the book, and if so we vouch for it that few will lay it down fruitlessly: it is inevitable that it must rouse the mental apathy, which, alas! too many of us unhappily possess. For it is a book which any person of ordinary intelligence can read with profit, and a very little knowledge of geology is requisite for its complete digestion.

When we ask ourselves what has the author done in this edition that he has not already performed, we find an answer that completely justifies the production of a new edition, even so soon after the last one. We shall refer to but a few of the points of novelty in the book before us, as we imagine all geological readers will get the work if they do not already possess it. We see, for example, that Sir Charles Lyell has profited by the perusal of Evans' splendid work on ancient stone implements, which has come out since his last edition, and hence this part of his book is rich in facts relating to flint instruments and such like. Then, again, he has completely recast the chapter on Kent's Hole and the Brixham Cavern, and has added a considerable mass of novel evidence regarding the former. This part of the work is of considerable importance, for the vast researches of Mr. Pengelly

---

* "The Geological Evidence of the Antiquity of Man, with an Outline of Glacial and Post-Tertiary Geology, and Remarks on the Origin of Species in the special reference to Man's first Appearance on the Earth." By Sir Charles Lyell, Bart., M.A., F.R.S. 4th Edition, revised. London: John Murray, 1873.

and his fellow labourers, Mr. Falconer and Mr. Prestwich—assisted by grants from that wonderfully generous lady Baroness Burdett Coutts and the Royal Society, and carried on as they were for a considerable number of years —have only lately been brought to a conclusion. Further, the author has also given a description of a skeleton found by M. Rivière in a cave at Mentone, which, from the unpolished implements and extinct animals associated with it, he is inclined to consider as of the Palæolithic age. He tells us in his preface that since the work has been printed a second skeleton has been brought to light by M. Rivière, in a neighbouring cavern, under similar conditions. He has written to Sir Charles Lyell, telling him that he found with this second human fossil, a flint lance and hatchet both unpolished. Around the arms, wrists, and knees of this were bracelets of Mediterranean shells, and the skeleton and surrounding earth were stained red by oxide of iron, as was the case with the skeleton of 1872. Sir Charles Lyell is informed that extinct animals were also found at a higher level, but he thinks that evidence as to the position of these will have to be carefully examined before their "geological bearing on the age of human skeletons can be finally settled."

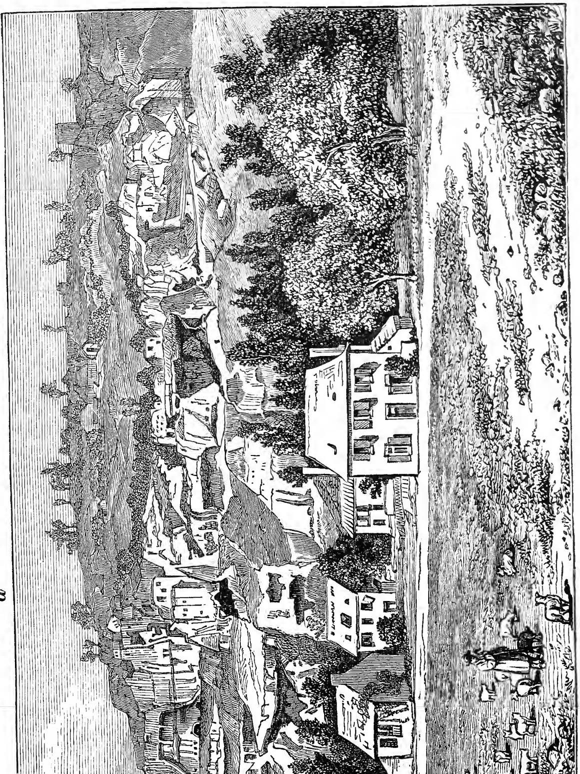
The author has found it necessary to entirely recast that portion of his book which dealt with the period immediately preceding that in which we have positive traces of man; and he has added a vast deal of quite novel matter. In regard to ice-action he is still somewhat undecided, and in the present volume he discusses both sides of this question, and adds a number of new facts of considerable interest from the researches of MM. Nordenskiöld and Brown. He has dwelt, too, on the remote dates of civilisation afforded us by the monuments of Egypt and oriental countries. Indeed on this important question he gives us a mass of information which is completely novel. Finally, among the novelties of the present edition may be cited the important facts taken from Huxley's paper* "On the Relationship of Reptiles and Birds," and some points as to the origin of races from Darwin's "Descent of Man," and Galton's "Hereditary Genius." These constitute most, but by no means all, of the novelties of this noble volume, and now we shall take a glance at one or two of them which alone our space allows.

We have abundance of cavern evidence—from Kent's Cavern in England to the splendid caves of Les Eyzies, Dordogne; of the latter of which the accompanying plate representing the several caves has been kindly lent us by the publisher, it being, we believe, originally produced in Messrs. Lartet and Christy's well-known work, "Reliquiæ Aquitanicæ."

The author shows that in one of the neighbouring caverns a carved tablet of ivory was found, which unquestionably represents the mammoth. The sketch which he gives of this leaves no doubt on the reader's mind as to its being the mammoth, for even the bulky hair which this animal is known to have possessed—from examination of ice-preserved Siberian specimens—is clearly represented on the specimen. Thus is conclusive evidence afforded of the fact that primeval man was co-existent with the mammoth family—a fact which was heretofore in considerable doubt.

* This paper appeared, with two pages of illustration, in the POPULAR SCIENCE REVIEW some years since. It was originally delivered as a lecture, but ours was the only illustrated form of it. The illustrations were done under Professor Huxley's superintendence.

a



a

VIEW OF THE CAVES OF LES EYZIES, DORDOGNE.

Two lines, the one vertical, the other horizontal, drawn from *a a* meet in the centre of the principal cave.

The facts regarding the Engis and Neanderthal skulls have been in the older editions, and there is not much additional matter to be added to them. We fancy, however, that had the author sought out in the proper directions he might have been able to give vastly fuller information in regard to the Irish lake-dwellings. Geologists are not numerous in Ireland, but archæologists are, and we fancy that had Sir Charles Lyell inquired of some of the many people connected with that very useful association, the "Kilkenny and South-east of Ireland Archæological Society," he would have been enabled to give more fully the facts of these structures, which now he appears to conclude are entirely different from their Swiss and New Guinea relatives.

There is a deal of important evidence cited in regard to the exploration of Kent's Cavern, and some important remarks are made on the subject of the teeth of Machairodus. Mr. Pengelly is opposed to Mr. Boyd Dawkins as to the position of this animal. The latter is inclined to place it in a lower deposit than Mr. Pengelly. That, however, will not much affect the importance of the discovery of its teeth. Besides, Mr. Pengelly's idea that the teeth are not mineralised as the bear's bones are, does not appear to us a very formidable objection. For he must remember that the layers of enamel do not normally contain more than two per cent of animal matter, and that the dentine contains vastly less than the ordinary bones of a bear. Still, however, his objection is worthy of attention. "If, as I believe," says Sir Charles Lyell, "it was contemporary of the mammoth and hyæna, it still lived on in England after the works of man had already been entombed in the red loam (No. 7 in the deposit of the cave) and sealed down with a floor of stalagmite. And if it was derivative from the Breccia, man was still equally its contemporary in that early period." With regard to evidence of the antiquity of man, which is derived not from geology but from history, as it is being now investigated, the author gives us a series of records which point to the fact of civilisation having been in existence in Egypt at a time when, according to our common notion, God was beginning to create the world. He says, "We cannot contemplate the average size and number of the pyramids now extant [upwards of forty, large and small], to say nothing of the inscriptions on them, without supposing them to have been the work of a long succession of generations. Yet the best authorities believe that these pyramids were built more than 3,000 years before the birth of Christ, and we find evidence that the Egyptians *had then attained a settled government, an advanced state of the arts, and extended commercial intercourse.*"

What Sir Charles Lyell says in regard to the difficulty of species lovers at the present day is strictly and rigidly true, but we had no idea that the view was as old as Lamarck. The author says, "What Lamarck then foretold has come to pass; the more new forms have been multiplied, the less are we able to decide what we mean by a variety, and what by a species." It is undoubtedly the case at present, that all naturalists of any note find the defining of a species a question which is absolutely impossible, if the definition is to be held exactly. In reference to the antiquity of man the question is not devoid of interest as to how man came into the world, and of course the author's views on this question are like those of most modern men of science, viz. that all vertebrate animals proceeded from a common stock. Professor Huxley, as we have already stated, described this view of the matter some years ago. But since that date a great deal of matter has been added to our knowledge of this point. There is, for instance, as the author points out, the remarkable form intermediate between birds and reptiles, which Professor Marsh, of Connecticut, U.S.A., pointed out in 1872. This form, which is fossil, is about the size of a pigeon. The whole of the general plan of the skeleton is truly bird-like, but the vertebræ are *biconcave*, and there are well-developed teeth in both its jaws. So completely did the jaw resemble a reptile's that Professor March, who first discovered only this bone, described it as that of a reptile before the other parts were subsequently found.

We had hoped to have touched on some of the last parts of the author's volume, those relating to Mr. Galton's idea of the mental superiority of ancient Greece, and also to transmutation and natural theology, two subjects of the highest interest; but we have already outrun our space as editorially allotted. Suffice it to say, that they are treated by the author with that calmness and consideration which are so characteristic of him and many other of our best science workers. We must therefore conclude, which we do unwillingly; but we trust, at all events, that we have said enough to prove to the reader of this Review, how vast, how true, how important, and intellectually how able, is this treatise upon the antiquity of man.

WHO ARE THE TODAS?*

THIS is a question which we fear there are not many of our readers who would be capable of answering with any degree of clearness. Yet it is not without its importance, as the present work discloses to us; and it has been very clearly answered by Lieut.-Col. W. E. Marshall, the author of the book upon our table. It is to be regretted that the author has selected as his title "A Phrenologist among the Todas," for that leads the reader to infer that it is a work dealing almost exclusively with the cranial estimate of the race, and in a fashion which the popular low-class lecturers of all our large towns so commonly adopt. He has thereby, we fear, excluded a good many who simply regard the work as phrenological and therefore absurd. This we confess was the light in which we ourselves were disposed to regard the volume at the outset. But on perusal it is seen how very little phrenology has to do with the book; and on the other hand, what a valuable record it is of the habits, tone of mind, and condition of education of a people who are very little known, and who inhabit the inland portion of the extreme southern division of our peninsula of Hindustan. The author of the work gives immense praise to the Rev. Friedrich Metz, who has spent more than twenty years among the inhabitants of the Nilgherry Hills, and whose knowledge of the various dialects of India and familiarity with

* "A Phrenologist among the Todas; or, the Study of a Primitive Tribe in South India." By William E. Marshall, Lieut.-Col. of Her Majesty's Bengal Staff Corps. London: Longmans, 1873.

the Todas enabled him to examine the people as to their customs, mode of marriage, polyandry, and slaughter of their female children. On all these subjects has he dealt at considerable length, and he has, at the end of the volume, given such a summary of the language as he was enabled to pick up in his intercourse with them. Further, he has given us a great number of illustrations; and these have a peculiar value, for they are not of the ordinary type, in which the reader is left to depend upon the love of truth which the artist possesses; on the contrary, they are almost all large photographs, done by the autotype process; and hence they convey, in a veritable style, the general character of the people, of their habits, and the mode of dress that is employed. There are two faults, one is the introduction of phrenology, a science which is absolutely without foundation, and the other is the somewhat loose style which the author follows. But when we remember how desultorily the book has been written, and that the author dates his preface from Faizebad, we have excuses amply sufficient for the second of these errors. We are confident that the book would have commanded a very large circulation had a better title been devised for it.

MAN AND APES.*

THE work now issued is little more than the reprint of certain papers which our readers will remember were published in these pages some time ago. In its complete form the book is an admirable essay on the anatomy of apes and half-apes, and it is so completely illustrated by nearly sixty well-executed figures of the various forms, besides a few representatives of the skeleton of man and the quadrumana, that it forms an excellent manual for the zoologist. But, besides this, it has an especial interest from the fact that it deals with that question which has been so long a matter of dispute between Mr. Mivart on the one side and Professor Huxley and Mr. Darwin on the other. We refer to the question, Was man developed by a gradual process from the apes, or was he not? It appears to us that Mr. Mivart takes the latter view, while admitting the possibility of evolution. He seems to regard man's brain as something completely apart from anything in the Quadrumana. In fact, it possesses a soul, and the monkey's does not. At least so we read Mr. Mivart's book. If it be so, if such be his view, then most assuredly we join issue with him. For strange as it may seem, we see no reason to deny a soul to the monkey or the gorilla, if we endow the bosjesman with such an immaterial presence. In any case, whatever view the reader may take, he is sure to be much pleased with Mr. Mivart's book, which is on the ante-Darwinian side, a book completely *sui generis*, and without a fault in point either of style or fact.

---

* "Man and Apes. An Exposition of the Structural Resemblances and Differences bearing upon Questions of Affinity and Origin." By St. George Mivart, F.R.S., V.P.Z.S., Lecturer on Zoology and Comparative Anatomy, St. Mary's Hospital. London: Robert Hardwicke, 1873.

BRITISH FISHES.*

AS the result of going through this work we earnestly express the hope of the author that it may soon reach a second and enlarged edition. For most assuredly it is the pleasantest (and withal good matter) and most readable little volume that we have ever met with on this subject. We truly hope that it may attain exceeding popularity, for it is, as Mr. F. Buckland wished it to be, an excellent popular account of British fish, with a certain amount of information on the subject of the various kinds of fisheries in the United Kingdom. On some fish, as for instance the *Lancelet*, the author's information is of the slenderest dimensions, although to the comparative anatomist few indeed of fish possess so extraordinary an interest. But we must remember that it is not for the zoologist that Mr. Buckland is writing, but for the ordinary individual; therefore, if we turn to an ordinary fish, we shall find our information fuller and more of a nature to interest such a reader. Take the many chapters on the salmon and trout for instance, or those on fish culture, and here we shall find our author perfectly at home. Not that it must be supposed for a moment that every British fish does not get a certain space allotted to it, and in most cases a cut illustrative of its structure; even *Amphioxus* has this latter, although we cannot say very much in its favour. But taking, as we said, the commoner fishes, we shall find our information popular, accurate, and in the most instances filled with an amount of facts which no one who was not perfectly at home on the subject could have given us. Leaving entirely aside the mass of useful knowledge which the author has put together on the subject of the trout and other fishes, let us take the salmon as an example of the book. Here we shall merely place before the reader the headings of the several chapters; if, when he has read them, he feels that the author has not done his work conscientiously, we think he will be difficult to please. First, then, is an account of the salmon; then follow "Salmon returning to their own River," "How to mark Fish," "Power of smell in Salmon," "Monster Salmon," "The Shannon Fish," "Ford of the Salmon," "Rhine Salmon Fisheries," "Anatomy of the Salmon," "Cathedrals and Salmon," "Salmon at Westminster Bridge," "Former abundance of Salmon in the Thames," "Causes of Extinction of Salmon in the Thames," "Salmon at Winchester," "How to kipper Salmon," "Unclean and Unseasonable Salmon," "Spring or early run Fish," "Early and Late Rivers," "Protection and Preservation of Salmon," "Darkness protects Salmon," "How to improve Spawning Beds," "Upper and Lower Proprietors in a Salmon River," "Value of British Salmon Fisheries," and last, though by no means least, the question of "Future Legislation." It will be seen from these headings that there is no subject connected with the question of salmon fishery into which he does not enter. Moreover, the whole is discussed in a pleasant style, without being too colloquial, as we once charged the author, now many years ago, with being. His illustrations are

---

* "Familiar History of British Fishes." By Frank Buckland, Inspector of Salmon Fisheries for England and Wales, &c. &c. London: Society for Promoting Christian Knowledge. 1873.

most numerous. We could have wished them done on a much larger scale; but that we must now hope will be the case in the third edition of this book, which has so well proceeded to its second.

MIND AND BODY.*

WE had expected, as his various writings had justified us in expecting, a better book than this one from Mr. Bain. Looking up to him as we have been accustomed for some considerable time to do, we had anticipated that in a work like the present one, which is especially intended for students at home and in America, he would, in this the second edition, have introduced us to something new. We heartily confess it is quite otherwise, however. Mr. Bain has employed a considerable space in words. We confess that it seems to us the work might with advantage be compressed into one-third of its present dimensions; and if this were accompanied by a little more simplicity of style, we think much might be gained. As it stands, we have not much hope of its success, for we fancy the author has not been at great pains in working up the more purely scientific portions of his subject. To be sure, the aim of the volume is a good one, and the author sets about proving many of the important points, which are so essentially the pith of the philosophy which has left Sir W. Hamilton ages behind us, with the usual force and clearness with which we were wont to associate Mr. Bain's name as a philosopher. But what we find fault with is the absence of any novelty in scientific fact tending to enforce the doctrines. Of course there are one or two scientific points of interest introduced, but we think the book on the whole is defective in regard to what has been done in the physiology of our nervous system, both at home and on the Continent.

WASTE PRODUCTS AND UNDEVELOPED SUBSTANCES.†

THE title of this book is not one which would attract a casual reader, yet assuredly, if he takes up the volume to glance through its pages, he will not readily lay it down again. Its author has been connected with the subject on which he now writes (for it is really a new book, and not a new edition) for many years, and has obtained the Society of Arts Medal for some of his papers on the matter of which the book consists. It is, therefore, not a mere compilation; though even if it were compiled we should look with wonder on the man who put it together; it is, on the contrary, a book which contains the fruit of many years' labour, and is in some measure

---

* "Mind and Body: the Theories of their Relation." By Alexander Bain, LL.D., Professor of Logic in the University of Aberdeen. 2nd Edition. London: Henry King, 1873.

† "Waste Products and Undeveloped Substances: a Synopsis of Progress made in their Economic Utilisation during the last Quarter of a Century at Home and Abroad." By P. L. Simmonds, Editor of the "Journal of Applied Sciences." London: Hardwicke, 1873.

a work which deserves to be placed along with Sir R. Kane's "Industrial Resources of Ireland"—a book which got its author a knighthood, and made his name familiar to everyone who was interested in such important questions. There is not a chapter of this work in which the reader will not find abundance of matter to interest him. Let us take at random a single passage, which will give the nature of the work:—"Margraf found that an unsightly weed, growing wild on the shores of the Mediterranean, contained a small quantity of sugar and a large proportion of soda. By transplanting and careful culture, a large part of the soda was eliminated, and potash substituted in its place, and the quantity of sugar considerably increased. The weed was transformed into the sugar-beet, and an industry established which has proved to be of great value to the European countries where the beet-root sugar is made." On every other mode of using up waste matters, and obtaining from them useful material, this book is abundantly fertile.

THE NATURALIST IN NICARAGUA.*

A BOOK that would have been excessively charming had it been thoroughly illustrated, and which is full of interest even as it is. We trust, however, that it may soon reach a second edition, and that when it does the author will scatter through his text a number of engravings taken from photographs. There is no book with which we can compare it, unless it be that admirable treatise by Mr. Bates, which we noticed about ten years ago, "The Naturalist on the Amazons." It is something of the same kind, but by no means the equal of Bates's volumes. And, curiously enough, it is dedicated to Mr. Bates, so that we may suppose the author took him as his type. It of course does contain illustrations—though not enough—and some of them, especially those of the "Hornet and the Mimetic Bug," "The Leaf Insect," and "Moss Insect," are greatly to be commended. The book extends over nearly 400 pages, and is brimful of interesting descriptions of a most instructive country, with useful records of the industry of the natives, of their habits, and of the antiquities and natural history, botanical and zoological, of the much-varied land which the author has successfully explored. Mr. Belt has a very charming style, and the ease and fluency with which he describes his various excursions add considerably to the charm of the work. All through he appears to be extremely severe upon the discovering Spaniards; indeed, in some passages, he indulges in the heaviest diatribes against them. And certainly there is much to justify this, as the reader of his book will satisfactorily see for himself. There is, however, one point which we ourselves must call attention to ere we close our notice of this interesting and instructive volume, and that refers to those marvellous instances of mimicry in which certain insects

* "The Naturalist in Nicaragua; a Narrative of a Residence at the Gold-mines of Chontales, Journeys in the Savannahs and Forests, with Observations on Animals and Plants in reference to the Theory of Evolutions of Living Forms." By Thomas Belt, F.G.S. London: John Murray, 1874.

indulge, and which has been so well dwelt on by Mr. Wallace in his "Malay Archipelago." We would refer more particularly to the author's account of the *moss* and the *leaf* insects. More than anything that he has said in the course of the work, these two insects illustrate the law of evolution which Mr. Belt has been endeavouring to illustrate. His description, and the cuts accompanying it, of the leaf and the moss insect, are simply marvellous. The leaf insect can with difficulty be distinguished from an ordinary leaf, and the moss insect, "of which I only found two specimens, had a wonderful resemblance to a piece of moss, amongst which it concealed itself in the day time, and was not to be distinguished except when accidentally shaken out." Altogether we have been much pleased with Mr. Belt's volume, and we do not hesitate to recommend it to our readers.

THE CONVOLUTIONS OF THE BRAIN.*

ONE of the most important advantages which has for many years been conferred on the student of philosophic anatomy has been given to him by the labours of Professor Ecker, in the book which has been translated into our language by Mr. J. C. Galton. Who has not wished, if he be a student of philosophy, for something like a definite work, in which he could follow out the various convolutions of the human brain. To the student of metaphysics, who has a certain knowledge of anatomy, this field has been completely a bare one; and when he has attempted to study any of the various memoirs which have appeared of late years on the subject of paralysis of speech, and injury to the "island of Reil," how destitute has he not felt of a work on the particular locality of any of the convolutions. Further, how impossible it has been to compare the convolutions of man with those of the apes. Of course a great deal of work on these subjects has been done by English anatomists; but if we ask where is there a book on the subject in English, we are compelled to say nowhere. Mr. Galton has therefore done wisely and well in introducing to us this admirable essay of Dr. Ecker's, which, with its numerous excellent woodcuts, its clearness of translation, and its general distinctness of type, renders its perusal less of a study than a pleasure. We shall hope that the introduction of this work among anatomists, together with a careful study of Dr. Ferrier's recent and valuable researches, will do much to render the study of the human brain by those who have the power—the superintendents of lunatic asylums—more popular and more productive than it has been of late.

---

* "On the Convolutions of the Brain." By Dr. Alexander Ecker, Professor of Anatomy in the University of Freiburg. Translated, by permission of the author, by J. C. Galton, M.A., Oxon, M.R.C.S., F.L.S. London: Smith, Elder & Co. 1873.

HOW TO MOUNT MICROSCOPIC OBJECTS.*

WHEN we say that a second edition of Davies' excellent little book is now before us, we shall doubtless gladden not a few of our histological readers; and when we add further that the second edition is brought out by the well-known inventor of Dr. Matthews's "Turn-Table," we shall satisfy all of the soundness of the best and cheapest work, which should be on the table of every student, we care not whether he is a tyro or is accomplished in the art of mounting. We would, with pleasure, if we were permitted, quote pages of this handy little volume, but we can point to a few of the many novelties which the present edition possesses over its predecessor. It is a wonderful book, from its being as useful to the purely medical man as to the naturalist; and as an illustration of this, we may mention that it contains, besides much other matter on the same subject, both Dr. Beale's and Dr. Klein's opinions on the subject of mounting nerve. Both use chloride of gold in staining the nerve, but the systems which the two adopt are somewhat opposite. However, the reader must see the difference in the book itself. The author's observations on the mounting of bone are of interest, and among other quotations is one from Dr. Beale, which directs the bone to be by no means ground, as that fills up the lacunæ and canaliculi. Mr. Parker's views on the question of the use of paraffin for embedding objects are also cited, from which we learn that it is useful to melt down ordinary paraffin candles, with a very small amount of paraffin oil, in order to make the mass more readily cut. We note, too, the introduction of some observations by Mr. Suffolk on the ether method of drying tissues, which are of considerable value. Mr. Davies' mode of obtaining the scrapings from various sounding-leads is, too, of importance, as likewise is Mr. McIntyre's—and no one could tell us better—mode of collecting the scales of certain insects. We think the author of the present edition might have given us a little more information on the subject of blood; and we observe that he has allowed the old references to the "Microscopical Journal" to remain unchanged, so that the reader of the book is absolutely unable to tell whether the "Quarterly" or "Monthly" is intended. But these are very slight complaints to have to make, and *tout entier* we are heartily pleased with Dr. Matthews's labours.

CONSERVATION OF ENERGY.†

TO those students of science—and we fancy that they are unfortunately few in number—who are capable of conceiving of the important questions anent the conservation of the mysterious and wondrous thing energy or force,

---

* "The Preparation and Mounting of Microscopic Objects." By Thomas Davies. 2nd Edition, greatly enlarged. By John Matthews, M.D., F.R.M.S. London: Hardwicke, 1873.

† "The Conservation of Energy: being an Elementary Treatise on Energy and its Laws." By Balfour Stewart, M.A., LL.D., F.R.S., Professor of Natural Philosophy in Owens College, Manchester. London: King & Co., 1873.

this book will prove an interesting one. We say interesting because, whether they agree with the author or not as to the possibility or the impossibility of the division of matter, there is no doubt that they will follow out many, indeed most, of his illustrations with the liveliest interest, and with a constantly enlarging view of the all-important question of the relation between force and matter. That is to say, they will considerably enlarge their knowledge of the serious and difficult question whether it is possible to conceive of force entirely and completely without matter. Indeed, they will find in it a good deal of what we call, for illustration sake, Berkeleyism. But apart from this, we cannot see the value of the work. We are willing to confess that we may be blind to the great practical importance of having a belief fixed and definite on a subject which, to our minds, must inevitably and for ever remain utterly indistinct. And if this is so, of course there may be a value in Mr. Stewart's book; but for ourselves we confess to our inability to see it. His style is marvellously good, but many of his passing illustrations, taken from other subjects than that to which he has been devoted, are manifestly unsound.

THE AMATEUR'S GREENHOUSE.*

THERE appears to be too much matter in the present work. We fancy that had the author left a good deal of his text for another volume, he would have done well. Still, the book is unquestionably a useful one, and its number of woodcuts on really important points renders its usefulness still greater. It has of course a number of coloured illustrations, which we assume that the publishers think of value. We, however, should prefer the volume without them completely; for we must say, that anything more horribly barbarous we have seldom, we might add never, beheld. But on all questions concerning the management of greenhouses, the various forms of building, and how you may put up a very cheap and also a very expensive structure, this book is certainly a valuable monitor. Then, again, as regards the contents of the greenhouse—their variety, form, care and management in culture, and their liability to pests or to disease—the remainder of the book gives ample information. It is unquestionably the sort of volume which a Londoner, who desires to "put up" a greenhouse, ought to possess.

PRACTICAL ASSAYING.†

THAT this book is in its fourth edition, and is considerably enlarged, is in itself sufficient proof of its great practical value, and of the high esteem in which it is held by the assaying world generally. It was a book

* "The Amateur's Greenhouse and Conservatory." By Shirley Hibberd. London: Groombridge & Co., 1873.

† "A Manual of Practical Assaying." By John Mitchell, F.C.S. 4th edition. Edited by William Crookes, F.R.S. London: Longmans, 1873.

excellent for its time in the days of its former editor, Mr. J. Mitchell, but ever since it has had the supervision of Mr. W. Crookes, F.R.S., it has been brought up to the time, and is in keeping with the immense progress which has been made in the art of assaying during the last five years. The general plan of the work remains the same as in the last edition, but a great deal has been added to each chapter, and all old processes of assaying which have been superseded by new methods are, of course, omitted from this edition. Chapter VII. is, for instance, much altered, and is of considerable value in a book of this kind, for it contains a full and perfect scheme for the "discrimination of minerals." Other chapters are likewise less or more altered, and thus the whole has been rendered a volume which all mineralogists must be acquainted with.

MEDICAL ELECTRICITY.*

DR. ALTHAUS' book is hardly one which we feel called upon to review. Still we may notice the fact of its appearance now, in its third edition and considerably enlarged. The author has done everything to bring the book up to the present state—a very advanced one—of electrical science, and the numerous illustrations he has given help much to render the subject intelligible to those who have not been familiar with the science of electricity. Dr. Althaus is, unquestionably, the first authority in this country on the subject of electricity in disease, and therefore all his assertions have a certain value. For ourselves, we may admit having a great faith in the effects of the three forms—electricity, galvanism, and faradism—in disease. But—and this is a matter of great importance—they occupy too much time to ever pay, in a pecuniary sense, the physician who employs them. Medical readers will do well to consult one part of this book, *i.e.* that relating to the detection of malingerers. This is certainly excellent.

ASTRONOMICAL SCIENCE POPULARISED.†

IN the two volumes that are now before us we find many of the difficult, and, to the general reader, uninteresting problems rendered at once simple and remarkably instructive and interesting. Mr. Proctor has the gift of making a complex subject very simple and intelligible, and this is no ordinary feat. Indeed, his books are among the very few of the popular class that are neither difficult nor inaccurate. He may, however, do too

* "A Treatise on Medical Electricity, and its Use in the Treatment of Paralysis, Neuralgia, and other Diseases." By Julius Althaus, M.D., M.R.C.P. 3rd edition. London: Longmans & Co., 1873.

† "The Expanse of Heaven: a Series of Essays on the Wonders of the Firmament." By R. A. Proctor, B.A. London: Henry King, 1873.

"The Borderland of Science; a Series of Familiar Dissertations on Stars and Planets, Sun and Moon, Earthquakes, Flying Machines, Coal, Ghosts, &c." By R. A. Proctor, B.A. London: Smith, Elder & Co., 1873.

much, and that we think he has been doing of late. We should infinitely prefer to see Mr. Proctor engaged in original researches than in the popularising of science. Good men cannot be spared, and he is too clever a man for the kind of labour which the production of such books as these involve. The chapters in these two books—which ought really to have been published under one title—are very interesting reading, but they are extremely unequal; some show great cleverness of style and thought, while it must be confessed that there are others which we should have wished to see removed from the volumes. In the second book, that entitled the "Borderland of Science," there is, besides astronomical papers, a few very interesting and sketchy papers on ghosts, flying machines, coincidences, &c. Further, it contains a capital portrait of the author, which has been excellently engraved by Mr. F. Holl, and which is an admirable likeness of Mr. Proctor.

STUDENTS' TEXT-BOOKS OF SCIENCE.*

LIKE all the others of this excellent series which Messrs. Longmans are bringing out is this one, the subject of Quantitative Chemical Analysis. It is clear, full, and to the point. If there is any objection to be raised, it is as to the illustrations, which we do not consider quite ample. But then of course a person who takes up a book of this kind is certain to have pursued chemistry already, and must have a familiarity with the subject. The discussion of the balance appears to us an excellent one, and it is too often either defective or absent from works of this kind.

Mr. Rigg's book is a very popular one, but its good type and the number of illustrations will render it useful to the very beginners at chemical science, a class for whom it must have been intended.

We cannot say anything in favour of either of the next two books—that on Animal Physiology by Mr. T. A. Bullock, or that on Astronomy by Mr. F. Bullock. We assuredly shall not recommend them to anyone. They are simply barbarous in style, illustration, and matter. They really appear as though some one who wrote books of the kind some thirty years ago fell asleep and suddenly awakened, and, like Rip Van Winkle, commenced his work anew.

There has been no book since Patterson's well-known "Zoology for Schools" that has so completely provided for the class to which it is addressed as the capital little volume by Dr. Alleyne Nicholson. We think, however, that the modesty of the author has been really excessive, and we think it a pity that for "obvious reasons the reproductive system

---

* "Quantitative Chemical Analysis." By T. E. Thorpe, Ph.D., F.R.S.E., Professor of Chemistry in the Andersonian University, Glasgow. London: Longmans, 1873. "An Easy Introduction to Chemistry." Edited by the Rev. A. Rigg, M.A. London: Rivingtons. "Students' Class-book of Animal Physiology." By T. A. Bullock, LL.D. London: Relfe. "Students' Class-book of Astronomy." By F. Bullock, LL.D. London: Relfe. "Outline of Natural History for Beginners; being descriptions of a progressive series of Zoological Types." By H. Alleyne Nicholson, M.D., D.Sc., Ph.D., Professor of Natural History in the University College, Toronto. London: Blackwood & Co., 1873.

and processes of the animals here described" have been ignored. At all events, we are thankful for what we have got, and we hope in his next edition the author will expand his scheme.

SHORT NOTICES.

An Introduction to Palæontological Botany. By J. Hutton Balfour, M.A., M.D., F.R.S. Edinburgh: A. and C. Black—is a short but very useful account of fossil plants. It is accompanied by some very good drawings on stone and wood; but it cannot be regarded as more than an Introduction. The list of works on the subject of fossil botany is good, but by no means complete.

The Parasitic Origin of Skin Diseases is an interesting and extended reprint from the "Quarterly Journal of Microscopical Science," by Mr. Jabez Hogg. It is published by Baillière and Co., London.

Principal Forbes and his Biographers. By John Tyndall. London: Longmans, 1873. This is a bold and clever defence of himself and those who are on his side in the Forbes controversy, by Professor Tyndall. We think it a sad thing that so unpleasant a discussion occurred subsequently to Professor Forbes' death, and we think that Principal Shairp is greatly to blame in the matter. It seems to us that it has resulted from a petty jealousy, which is contemptible when it is associated with a mind that one was wont to look up to. We think Professor Tyndall fully justifies his own and Professor Huxley's position.

The Darwinian Theory, and the Law of the Migration of Organisms. Translated from the German of Moriz Wagner, by James L. Laird. London: Stainton, 1873. Mr. Laird was quite right in translating this pamphlet. All who read it will see how very greatly it supports Mr. Darwin's views.

Solid Geometry. By W. T. Pierce. London: Longmans, 1873. The author has produced this work because he thinks there is an absence of books of the kind in this country, compared with France. It is elaborately illustrated.

The Marienbad Spa. By A. V. Jagielski, M.D. London: Trübner, 1873—is a long account of the Spa and its effects. Those who wish to try the waters should peruse this book in the first place.

The following books have not been reviewed in the present Number, but will be in the next:—Mr. Cash's "Where there's a Will there's a Way;" Dr. Winslow's "Manual of Lunacy;" Dr. Pettigrew's "Physiology of the Circulation in Plants, in the Lower Animals, and in Man;" M. Lartet and Mr. Christy's "Reliquiæ Aquitanicæ," Part XIII.; "The Daily Bulletin of Weather Reports, Signal Service, United States Army," for the month of September, 1872; "Darwinism and Design. or Creation by Evolution," by George St.Clair, F.G.S.; "Animal Locomotion, or Walking, Swimming, and Flying, with a Dissertation on Aeronautics," by J. Bell Pettigrew, M.D., F.R.S., &c.; "A Catalogue of Stars observed at the U.S. Naval Observatory during the Years 1845 to 1871," by Admiral Sands; and "Astronomical and Meteorological Observations made during the year 1870 at the U.S. Naval Observatory." Both published at Washington.

Scientific Societies Club

WE have had some difficulty as to the insertion of a notice as regards this institution, for we have not known where to put it. We have, as a last thought, put it here, as being at least more likely to be seen than if it were placed under any of the particular scientific departments. As to the progress of this club—for which, if it be successful, we shall be indebted to the extreme and laborious pertinacity of the Honorary Secretary, Mr. J. Logan Lobley—we have now a good tale to tell. At a meeting of the provincial committee, which was held in November last, a prospectus was drawn up, which, after stating the general character of the club, goes on to say that

"Original Members who shall have been Members of the Provisional Committee, or founders, to the number of 250—Entrance Fee, two guineas. Annual Subscription: Town Members, two guineas; Country Members, one guinea.

"Members entering after the Club is founded—Entrance Fee, four guineas. Annual Subscription: Town Members, three guineas; Country Members, one guinea and a half.

"Although these fees are small when compared with those of other Clubs, a large number of Members will furnish an ample income for an economically conducted and moderate establishment.

"When the requisite number of names have been received, a general meeting will be called to formally found the Club, but until then it is not proposed to incur any beyond very trifling preliminary expenses. Gentlemen sending in their names to be added to the Provisional Committee will consequently be under no pecuniary or other liability. It is, therefore, hoped that the proposal may meet with the general support of scientific men, and that thus a Scientific Societies Club may be successfully established."

There has already been, we believe, over a hundred members who have given in their names; and gentlemen desirous of joining will please to send in their names to the Editor of this Journal, who will then send them Prospectuses and answer any queries they may please to put.

SCIENTIFIC SUMMARY.

ASTRONOMY.

A *NEW Driving-clock for Equatorials.*—Lord Lindsay and Mr. David Gill have described to the Astronomical Society a form of clock which they have devised, and which bids fair to render the higher accuracy of rate which is now demanded by spectrum analysis and celestial photography readily attainable. The following conditions have been endeavoured to be realised :—

1. A driving power four or five times as great as that actually required, so as to avoid failure when the balancing of the instrument is defective.
2. A rate of motion equal in accuracy to that of a good sidereal clock.
3. Maintenance of the same uniformity of rate under change of work to be done, twice as great as that to which the clock will be likely to be submitted.
4. An easy and rapid means of adjusting that rate, and so avoiding delay in timing as much as possible.

The methods by which these several conditions have been arrived at, detailed in full, with various illustrations explaining the machinery employed, were given at the Proceedings R.A.S.

New Observations on Presence of Magnesium at the Edge of the Sun.—M. Tacchini gives observations, from June to end of August, of magnesium, line *b*, and the line 1474 *k*, the number of positions each day being noted. These vary from 26 to 60. August shows a maximum. The line 1474 *k* is always found where the line *b* is found; but the reverse does not always occur. Against M. Faye's theory the writer points out, among other things, that the penumbræ of spots are generally very broad, and many of their tongues, or currents, go to the very bottom in a way contrary to that which cyclones would show.—See "Comptes Rendus," Sept. 8.

On Auroræ Boreales.—A very important and valuable paper is that laid before the French Academy lately by M. Faye. Donati, he says, seeks the explanation of auroras in a meteorology which he terms cosmic. The phenomena are probably due to electro-magnetic currents going from the sun to the planets, their vehicle being ether. He himself hesitates to accept this view, and directs attention to the forces really acting in interplanetary space. In addition to attraction there is the force producing the phenomena of comets. Might not this, operating on our earth, give rise to auroras? The effects of this repulsive force are proportioned by surfaces and not by masses. Insensible in very dense bodies, they become enormous in matter of extreme tenuity. Hence the immense comets' tails of 30, 40, and 60 millions of leagues, having a direction away from the sun. These rarefied matters have a very high velocity, as if solicited by a force twelve or fifteen times greater than that of gravity. Spectral analysis shows that comets have two kinds of light—one from solar illumination, the other proper to them, and characterised by bright lines of a discontinuous spectrum, indicating incandescence of gaseous parts. The earth, too, viewed from afar, would present two spectra—that of solar light, and in the obscure part near the poles the discontinuous spectrum of its auroras, boreal and austral. The author does not think the feeble incandescence of cometary matter is caused by solar heat, for the same rays do not produce such effects with us. If a screen were placed across the tail, the particles striking it would likely become suddenly incandescent. Now the nucleus is just such a screen, against which the anterior molecules of the nebulosity strike, producing heat and light; while, on the other hand, molecules not thus arrested pass rapidly behind and from the tail. Thus there is a double effect. On our globe only the extreme and very rare layers of atmosphere have some analogy to these cosmic nebulosities, but they may give rise to some of these phenomena; not tails indeed, for the greater attraction of the globe holds in the matter about it. But they might produce some feeble light-effects similar to those of comets if the repulsive force communicated to them in certain regions a considerable velocity, transferring them to other regions of our globe. The limits of our atmosphere are unknown, but the true limit will be where our air, having become more rare than the vacuum in our best pneumatic machines, has been reduced to a medium doubtless comparable in density to the cometary nebulosities, on which the repulsive force of the sun acts. Consider this limit; it is not likely spherical. The lower layers of our atmosphere show by the barometer a well-marked minimum of pressure at the poles, and maxima which do not coincide with the equator. Temperature and radiations produce great irregularities in them; and it must be the same with the extreme layers. They probably experience on the side next the sun, the side on which they attain highest elevation, a repulsive force appearing in a slight pressure centrally, and movement at the edges. This limiting layer is thus conceived as having a less curvature, though a higher elevation, than that of the opposite side; and, as in the inferior layers (though in greater degree), presenting a depression near each pole on the right side, where the ground and inferior layers radiate least to the heavens. Then as to the edges of the hemisphere turned towards the sun. The superficial parts, reduced to extreme rarity, obey the repulsive force and are driven tangentially, acquiring considerable velocity in an hour or two. Reaching the depression near the poles, they enter the vacuum and rush across it. The earth's attraction produces a strong curvature in their trajectories, and they meet the limiting surface of the atmosphere beyond the depression; and if their velocity may have reached several hundred mètres per second, the incessant shock of these mobile particles against the fixed will give rise to light. The slight illumination which will be visible to us in a limited part of the heavens will have the characters of gaseous incandescence. This phenomenon will not occur equally all round the globe. In regions a little removed from the poles there is no vast depression to cross; the molecules in their passage encounter the resistance of a continuous layer, and cannot acquire the same velocity as at the poles. Hence the light will mostly be produced at the poles, and especially at the pole actually deprived of solar light.

What is the Cause of Solar Scoriæ?—This important question has been taken up by M. Faye, who answers M. Zöllner's views at length in the "Comptes Rendus" of the French Academy. He says that the spots are scoriæ produced by local cooling of the incandescent liquid which forms the solar globe. Their relatively low temperature gives rise to currents analogous to those about our coasts and islands; only in the sun they do not alternate. The lower currents flow perpendicularly to the sides of the islet from within outwards; the upper from without inwards. Hence a continual series of vertical movements, the horizontal axes of which are *tangential* to the contours of the scoriæ. Naturally the solar radiation is partly suppressed by such an islet; and if the temperature sinks to the point of condensation of vapours in the atmosphere, clouds are formed, whose figure depends on the upper currents flowing from all parts towards the vertical axis of the islet. They will especially be produced towards the central part, and the scoriæ will appear as a dark nucleus with its *enceinte* of penumbra. This local cooling, and the descending movement of the atmosphere towards the interior, will explain the depression observed at the dark nucleus of spots, and the effects of perspective as they approach the edge of the disc. If the ascending currents outside of the islet are strong they will leap up and give the appearance of ordinary protuberances. As to the eruptive protuberances, they are due to a local diminution of atmospheric pressure. The gases included and compressed, or simply dissolved in the liquid mass, escape like the gas-bubbles in seltzer water when the cork is drawn. The movements of spots are explained by "trade-winds" from the poles to the equator. The component of this action in the direction of the parallels diminishes the velocity of rotation, and retards the spots more in regions near the pole than at the equator, where the action is *nil.* Large scoriæ often break up and form several spots, the incandescent ocean appearing from below. Remarking on this theory, M. Faye points out that the spots on nearing the edge ought, according to it, to show a projection at the side of the sort of vase of which the scoriæ form the bottom; but there is nought of this, and the orifice becomes even with the edge of the disc. The depth of the spots has been measured, and found on an average three to four seconds. Everything indicates that the spots are cavities and not projections. On the other hand, the theory is more closely related to his (M. Faye's) own than P. Secchi's; both supposing a circulation and *down rush* of hydrogen; the axis, however, being, according to the author, vertical. And it is also preferable to the eruption theory in that it agrees with laws of movement of the spots, one of these being that each spot follows the movement of the parallel in which it is, and if it passes into another parallel takes the movement of that. But then one would suppose that the islets of scoriæ would be driven by the trade-winds supposed, towards the equator (like our ships). There is no such movement, and one even finds neighbouring spots which have limited movements in opposite directions, one towards the equator, the other towards the poles. The elliptic oscillation, too, has nothing in common with the displacement of floating bodies, and the phenomena of segmentation are hardly explained. The scoriæ (if such there were) would doubtless be formed in the hottest regions; but, on the contrary, they appear in the zones nearest the equator, never at the poles. The spots do not appear beyond the 35th degree of latitude, while the protuberances (supposed to be produced by them) appear as far as the 70th. Lastly, consider the long and constant solar radiation. The sun, being simply a liquefied mass, would have been long since encrusted. If the conductibility of liquids and solids is so small that these scoriæ resist the heat of the liquids for days, weeks, and even whole months, how are we to explain the enormous radiation of 1,200,000,000 calories per square mètre of surface daily? A state of fluidity nearly gaseous is necessary to allow the play of ascending and descending currents to bring heat up from the depths of the solar mass and supply the superficial radiation during millions of years, and to allow of the progressive contraction of the greater portion of the mass repairing, in calories, a part of the secular loss. M. Faye considers that the circulation of solar hydrogen arises from a more general phenomenon than that supposed by M. Zöllner, viz. the vortical movements (with vertical axes) produced in the photosphere by its own special mode of rotation; and then radiation is connected with a still more general phenomenon, the mode of alimentation of the photosphere.

A possible Lunar Atmosphere.—Mr. E. P. Neison has a paper in the last number of the "Monthly Notices," in which, after going into the subject with tolerable minuteness, he says that it remains to refer to the objections; and these we give, allowing the reader to examine the paper itself for the arguments in favour of this view. He states that the objections, with one or two exceptions, are all directed against an atmosphere usually as dense, or even denser than our own; they are valueless as directed against one only four-hundredths of this density. The phenomenon referred to by Mr. Proctor, in his work on the Moon, as preventing the occultation of a star, could only arise from a lunar atmosphere much greater than our own, even were it not prevented from the rays from the Moon after refraction being divergent and not convergent, as he assumes in his illustration. It will also be apparent that for the density of the supposed atmosphere, no distortion of a star before occultation could possibly occur; and the same applies to the occultation of a planet such as Jupiter or Saturn; the maximum effect would be to increase the size of the planet by about one-thousandth, but in no case distort it. Dr. Huggins' observation ("Monthly Notices," vol. xxv. p. 60) is evidently by no means delicate enough to detect the very slight effect capable of being exerted by an atmosphere of the density supposed. The effect of a lunar atmosphere upon an eclipse of the Sun would, if of the density assumed, be sensibly the same as a diminution of the semi-diameter by about one second, or would be lost in the effects of irradiation. Finally, it can hardly be seriously urged that it could materially interfere with the observation of the reversal of the dark lines in the solar spectrum, considering the smallness of the horizontal refraction, and the extremely minute amount of scattering of the solar rays the supposed atmosphere could effect. No known objection yet raised appears to limit a possible lunar atmosphere more than the difference between the occultation and telescopic semi-diameter. The real dimensions of the atmosphere shown to be possible upon the Moon's surface can be best shown by the fact that its total weight above one square mile is about four hundred thousand tons; and that it bears nearly one-eighth of the proportion of the Moon's mass, as the Earth's atmosphere does to the Earth's mass.

Variation of the Sun's Diameter.—Signor Respighi says, in a recent note to the Academy ("Comptes Rendus," Oct. 6) that Father Secchi has stated that the duration of the passage of the solar diameter, measured by mono-chromatic images obtained in the spectroscope telescope with the direct vision prism before the slit, was less than the duration given in the "Greenwich Nautical Almanack" by about 0·6 s., whence he infers that the diameter of the mono-chromatic image of the sun is about 8 s. less than the diameter of the image with compound light, obtained by the simple telescope with coloured glass. Signor Respighi is unwilling to admit this difference. He suggests possible causes of error; imperfect rectification and instability of instrument, influence of atmospheric refraction, but more especially the influence of variations of temperature of the prism. These sensibly displace the spectral lines. The results are constant, Father Secchi says; but it is replied, that during each passage of the solar image the temperature of the prism could not continue constant, and its variations would be reproduced periodically in successive passages (independently of the absolute temperature). It is precisely these periodic variations which might displace the solar image by a quantity nearly constant in all the successive passages. (This influence is much less sensible in the author's own instrument, in which the aperture is reduced by a diaphragm, and the prism has little absorption). Other possible causes of error are the undulation or agitation of the sun's border, and the personal error in observation of the two contacts. Some of the causes mentioned are avoided by using the objective prism. The author's observations by both methods gave results differing very little from the "Nautical Almanack" (not more than + or − 0·12).

BOTANY AND VEGETABLE PHYSIOLOGY.

The Gonidia of Lichens.—An important paper on this subject appears in "Grevillea" (September), and is worthy of being perused. M. Bornet, whose researches the paper deals with, is said to consider he has established the two following propositions:—1. Every gonidium of a lichen may be referred to a species of Alga. 2. The connection of the hypha with the gonidia is of such a nature as to exclude all possibility of one organism being produced from the other, and that the theory of parasitism can alone explain it satisfactorily. M. Bornet's paper, of which the one in "Grevillea" is an abstract, is illustrated with eleven coloured plates of microscopical dissections which assist very considerably the elucidation of his theory.

Eucalyptus Globulus as a Means of Draining Moist Lands.—It is asserted by those who seem to have practical acquaintance with the virtues of this plant, that it (which is a native of Australia) has the power when planted in marshy lands of improving their sanitary conditions, as has been proved in Algeria and Cuba. It drains the swamps, and at the same time gives off antiseptic vapours.

Lilium Washingtonianum described twice.—At a recent meeting of the Acad. of Nat. Sci. of Philadelphia, Mr. Thomas Meehan referred to a paper by Professor Alphonso Wood, entitled a "Sketch of the Natural Order of Liliaceæ" of the Pacific coast, published in the volume of the "Proceedings" for 1868, in which he describes a "new species" of *Lilium* as *L. Washingtonianum*, giving, as a reason for the name, that it was generally known as the "Lady Washington" by the miners. Professor Wood said, in his paper, that it was remarkable so fine a plant had been overlooked by other botanists. It so happens that it had not been overlooked, but had been described ten years previously by Dr. Kellogg, in the "Proceedings of the California Academy" for 1858. Through the unusual circumstance of two authors employing the same name, the confusion and trouble which loose and careless habits in describers bring on students, in the present case, will not be great; yet it is but just to Dr. Kellogg that this correction should go into the records of the Academy.

Are Double Flowers a Natural or an Artificial Result?—It has been stated at a meeting of the Academy of Sciences, Philadelphia, May 6 (only now, November, printed and circulated in England), by Mr. Thomas Meehan, who certainly appears to be one of the most industrious labourers in the botanical field in America, that on several occasions, during the few past years, it had been noticed among the *variations in nature* that the tendency to produce double flowers was by no means the special prerogative of the florist to produce. Many of our commonest wild flowers, which no one would think of cultivating, had double forms in cultivation which were no doubt originally found wild. Thus we had a double *Ranunculus acris*, *Ranunculus bulbosus*, *R. Ficaria*, *R. repens*, and some others. He had himself placed on record the discovery, wild on the Wissahickon, of a double *Saxifraga virginica*, and this season a fellow member, Dr. James Darrach, had found, in the same location, a double Trailing Arbutus, *Epigæa repens*. There were, in plants, two methods by which a double flower is produced. The axis of a flower was simply a branch very much retarded in its development, and generally there were, on this arrested branch, many nodes between the series forming the calyx or corolla, and the regular stamens and carpels, which were entirely suppressed. But when a double flower was produced, sometimes these usually suppressed nodes would become developed, in which case there was a great increase in the number of petals, without any disturbance in the staminal characters. But at other times there was no disturbance of the normal character of the axis. The stamens themselves merely became petaloid. This was the case in the *Epigæa*, now found by Dr. Darrach. He is not alone in calling attention to these facts in wild flowers, for Dr. Gray and others have recorded instances in the "American Naturalist."

A New Fungus Ceratostoma Helvellæ.—At one of the meetings in the autumn of the Eastbourne Natural History Society, Mr. C. J. Muller placed on record the discovery of a highly curious fungus, which he found growing on specimens of *Peziza hemispherica* on the wall of Compton Place in February last. The Peziza was growing amongst moss and on the rubbish of the wall, and externally presented the usual appearance of this fungus—that is to say, it was brownish, sessile, hemispherical, and clothed with dense fasciculate hairs, and varied in size from two lines to one inch in breadth. On examining, however, the hymenium, or fruit-bearing surface, he found it rough and papillate, an appearance altogether different from the usual hymenium of a Peziza; and on making a vertical section of the disc, discovered that the hymenium was thickly studded with pale brown perithecia or peridia, immersed among the asci belonging properly to the Peziza, and filled with very dark brown lemon-shaped spores, lying free in their receptacle. The perithecia were immersed at various depths in the hymenium of the Peziza, and were crowned with hyaline septate bristles. The asci and sporidia of the Peziza existed in the usual form, but seemed not so luxuriant as usual. As he could find no description of this fungus in the books on British Fungi, he referred the matter to Mr. M. C. Cooke, who, like himself, was disposed at first to regard it as coniomycetous, as no asci could be discovered; but further consideration of the subject led him to the opinion that it belonged to the genus *Melanospora* of Corda, which is ascomycetous, and this opinion was confirmed by Mr. Muller's being able to discover in the early stage of the fungus the existence of delicate asci, in which the lemon-shaped sporidia are produced. These asci are quickly dissolved away as the sporidia become mature, so that the latter appeared in the perithecia perfectly free, as spores appear sometimes in the peridium of a coniomycetous fungus. Finally Mr. Cooke has named the plant *Ceratostoma Helvellæ* "Peziza Ceratostoma." It is entirely new. Only two species of this genus have been found in Great Britain, and the character of the genus as given in Cooke's "Handbook" differs materially from that of the specimen under notice, so that it may be still questioned whether it has yet been correctly named. Messrs. Berkely and Broome regarded it as a species of *Melanospora*, no species of which genus has hitherto been recorded in Britain. We should like, however, to have Mr. Cooke's opinion on the matter.

The Curious Fertilisation of Pedicularis Canadensis.—This has been carefully observed by Mr. Gentry, who has described the process fully to the Academy of Natural Sciences of Philadelphia. The flower of this plant is composed of an erect tube, with a natural cleft running along its lateral walls from above, through one-third its entire length, presenting outwardly apparently a mere crease, from the manner in which the compressed margins of the upper lip fit into the rolled-in edges of the lateral lobes of the under lip. The upper lip is compressed, arched, and beaked, presenting an aperture at the apex, through which passes a curved pistil; the lower lip is reflexed, consisting of three lobes, one median and two lateral, assuming a platform arrangement. Enclosed within the upper lip are four stamens, didynamous, with their anthers turning backwards, facing each other ventrally. When ripe these anthers split upon the inner side, thus giving a fancied resemblance to an oval snuff-box thrown backwards upon its hinges. Each cell is filled with white pollen-grains. Now when the bee alights upon the tube, by means of its trunk it opens the natural cleft above alluded to, and, having thus gained a partial entrance, it would defeat its intention, did not the length of the flower's tube when contrasted with that of the bee's trunk, necessitate the admission of the entire head also. In this operation the lips of the flower are pressed apart, the margins of the upper lip are separated to receive the head, and the pollen-grains, already ripe, by the considerable motion to which they are subjected, become dislodged from their cells, and fall down in a dense shower upon the bee's back and head. Having obtained the coveted sweet, it flies to another flower upon a different stalk, as he observed in a score of cases during two days, but, before renewing the preceding operations, stations itself awhile upon the lower lip, its head coming in contact with the stigma of the pistil. Then, by means of the hairs that line the inner side of the tarsus of each anterior leg, and the constant rubbing together of the parts comprising its trophi or instrumenta cibaria, the attached pollen-grains are sent flying in every direction, sure to adhere to the stigma.

What is the Nature of the Hollyhock Disease?—It seems that during the months of June and July this disease was reported from different localities in England as having caused considerable damage to Hollyhocks. It is produced, says a note in "Grevillea," by *Puccinia malvacearum* (Mont.), a fungus not previously observed in this country, which was originally described by Montagne as occurring on the under surface of the leaves of *Althæa officinalis.* The Editor has also received specimens on *Malva sylvestris,* from J. Hussey, Esq., of Salisbury; Dr. Paxton, of Chichester; and Mr. E. Parfitt, of Exeter.

CHEMISTRY.

Estimation of Tannin in Tea-leaves.—This, which might at one time have been considered a purely physiological subject, has now in these days of "Chemical" Officers of Health, become a very serious and important problem. In the "Chemical News" for October 24, 1873, appears a paper on this subject. Its author, Mr. A. H. Allen, F.C.S., after giving a number of matters of interest, describes the details of the method he employed in the estimation of tannin, stating that he had found by the process in genuine black tea of rather more than average quality 12·5 per cent. of tannin, which presented a close agreement with those in the old analyses of Mulder, which he regarded as the most accurate and complete analyses of tea extant. The estimation of tannin was of the first importance; for if it reached the normal amount, all question of adulteration by exhausted leaves was at an end, and foreign leaves were very unlikely to be present. The only fallacy in such a conclusion would be caused by an admixture of catechu, or sloe-leaves. The next point of importance was the percentage of "woody fibre," as it was called by some analysts; and here, again, he was disposed to think that Mulder's analysis was the only accurate one. The percentage of gum, insoluble matter, and tannin in any sample of tea, considered carefully, would enable the analyst to form a very accurate opinion as to the presence or absence of exhausted leaves, &c. Analysed by the above-described methods, a sample of very superior black Congou tea gave the following results, which he had placed in juxtaposition with the numbers obtained after some of the same sample had been infused in the usual manner in the teapot (the exhaustion was not carried to excess, no second quantity of tea being used), and the leaves re-dried :—

	Original Tea.	Exhausted Tea.
Moisture	9·2	11·1
Insoluble matter	58·7	87·5
Gum	10·5	3·8
Tannin (by gelatin)	15·2	3·3

Purification of Drinking-water by simple Chemical Means.—Many of us suppose that the best method of dealing with water not sufficiently pure for drinking is to pass it through one of the Silicated Carbon Company's filters, or one of Spencer's *magnetic carbide* filters. It would seem, however, from Mr. Crookes' advice to the medical staff that is gone to the West of Africa (published in the "Chemical News," November 14), that a simpler and better means of dealing with extremely foul water is to be found in the A B C process. Under the name of A B C compound a mixture of sulphate of alumina, clay, and charcoal has been successfully used by the Native Guano Company for the purification of sewage. At their works he has repeatedly seen the sewage of such places as London, Paris, Hastings, and Leeds, converted in the course of a quarter of an hour, from an offensive-looking, vile-smelling liquid, into water, bright, clear, inodorous, and tasteless, non-putrescible, and so free from injurious matter as to allow delicate fish to live and thrive in it. With a little necessary modification he considers that a mixture capable of acting thus on town sewage is the most suitable for the purification of water for drinking purposes on the Gold Coast. He would suggest that the charcoal be omitted, and its place supplied by a permanganate. The ordinary potash salt will do, but if procurable in time he has reasons for believing that permanganate of lime would answer the purpose better. He has prepared a mixture of—

1 part of permanganate of lime,
10 parts sulphate of alumina,
30 parts fine clay,

and finds that when he adds this to London sewage, in the proportion of 20 to 10,000, the purification is very satisfactory, and the settlement rapid. With foul ditch water a less quantity will do. The mixture can be filtered, instantly yielding a bright filtrate, or it can be allowed to settle for a quarter of an hour, when the supernatant water can be poured off equally bright. He is not prepared to say what the price of this mixture would be, but it would probably not be many pence per hundred gallons of water.

We think this recommendation extremely valuable.

Preparation of Rabbit's Hair for Felt Hats.—In No. 40 of "Reimanns Färber Zeitung," it is said that rabbit's hairs were formerly treated with a solution of mercury in nitric acid, for the purpose of enhancing their felting properties. A mixture of nitric acid and treacle is proposed as a substitute.

The Syrups of Low-class Wines.—These syrups are special saccharine liquids, analogous to the juice of grapes, containing tannin, salts, &c., and colouring matters. It appears, says the "Revue de Chimie Scientifique," No. 41, that they are to be mixed with the residue of the grapes after the first pressure and pressed again, so as to yield a further quantity of a liquid which may pass for wine. M. Mène thinks that by this system "dangerous and gross falsifications" may be prevented, which are a calamity to the country and to society, because they interfere with, and injure, not merely health, but business and progress.

Variation of the Quantity of Carbonic Acid according to the Weight of Atmosphere.—M. Truchot has published, in the "Comptes Rendus" of Sept. 22, a series of observations which were made almost daily during July and August at Clermont Ferrand. The method consisted in passing the air into baryta-water previously titrated, then allowing deposition of the carbonate formed, then titrating anew the limpid supernatant liquor, a known quantity of which was separated with a pipette. The numbers obtained show—(1) That the proportion of CO_2 is a little greater during the night than during the day (as was previously observed by De Saussure and Boussingault). (2) That the proportion is not sensibly higher in the town than in the country. (3) That in the neighbourhood of plants with green leaves in full vegetation the proportion of CO_2 varies considerably according as these green parts are illuminated by the sun, or are in shade, or quite in darkness; this agrees with a well-known fact in vegetable physiology. (4) That, on a general average, the proportion is 4·09 vols. per 10,000 vols. of air, which closely agrees with the numbers obtained by De Saussure (4·15), Thénard and Boussingault (4), and Verver (4·2); but is much higher than those of German observers, Schulze (2·9), and Henneberg (3·2) at Rostock and Weende. To ascertain the influence of height, determinations were made simultaneously at Clermont Ferrand, 395 mètres above the sea; at the top of Puy de Dôme, 1,446 mètres; and at the top of Lancy, 1,884 mètres. The respective numbers were 3·13 vols. (per 10,000 of air), 2·03, and 1·72, showing a marked decrease with the height.

The Coefficient of Expansion of Carbon Disulphide.—This has been determined by Mr. J. B. Hannay, who lately (Nov. 20) read a paper on the subject before the Chemical Society. This paper contains the results of a numerous series of determinations of the specific gravity of carbon disulphide at small differences of temperature made in an apparatus of peculiar construction, so as to prevent evaporation, and to admit of the subsequent expansion of the liquid when adjusted at low temperatures. When the apparatus was carefully cleaned, the disulphide could be heated in it considerably above its boiling-point, thus affording a means of obtaining its specific gravity at comparatively high temperatures. The temperature of the specific-gravity bulbs was adjusted by immersing them in water kept at a constant temperature by a modified form of Carmichael's apparatus. Two tables of the calculated and observed variation from 0° to 62° C. in the volume and specific gravity of the disulphide, the latter referred to water at 4°, accompany the paper. From the results the author concludes that carbon disulphide expands equally for each equal increment of temperature, the number denoting the coefficient of expansion being 0·001129, and that denoting the decrease of specific gravity 0·001461 for each degree Centigrade.

Improvements in Dyeing. — Several improved methods are described in No. 40 of "Reimanns Färber Zeitung." This number contains receipts for a catechu brown on glazed calico; an iron buff on stout cotton goods; for a deep olive-green and a drap on wool; a coal-black, a blue-black, and a deep corinth on plush; also a black on cotton yarn, capable of bearing milling; and a blue on shoddy, the cotton in which has been first destroyed by the vapour of hydrochloric acid, and the residue neutralised with chalk. In dyeing, also, a preparation is used, known as "shoddy-carmide." It is made by dissolving, in two pails of hot water—12 lbs. of alum, 9 lbs. of indigo-carmine, and 3 lbs. of soluble aniline blue.

Death of Professor Dr. F. Crace-Calvert, F.R.S.—The "Chemical News" of October 31 records the death of this gentleman, whose contributions to chemistry have so frequently enriched its pages. Crace-Calvert spent the early years of his life in France. After his return to England he became Honorary Professor of Chemistry in the Royal Institution of Manchester, which position he continued to hold up to the time of his death. In 1850 he was also Lecturer on Chemistry in the Pine Street Medical School, Manchester, an appointment which he does not seem to have held later than 1855. In 1859 he was elected a Fellow of the Royal Society. He was also a Corresponding Member of the Academy of Sciences of Turin, Honorary Member of the Pharmaceutical Society of Paris, and Corresponding Member of the Industrial Society of Mulhouse. He died on the 24th of October, at the age of 54. "For many years," says the editor of the "Chemical News," "we enjoyed the pleasure of his friendship, and few can give more able testimony to his worth as a chemist, or his faithfulness as a friend."

Absorption and Spontaneous Liquefaction of Gases by Charcoal.—In "Les Mondes," Oct. 9, is an important contribution by M. Melsens, who states that he has obtained wood charcoal absolutely pure, and possessed of such high absorptive power that it can concentrate in its pores its own weight of gas. When charcoal thus saturated with any of the more readily liquefiable gases, such as cyanogen or chlorine, is placed in a glass tube fitted with a neck bent at right angles and closed at one end, and heated to 100° by a current of steam, the gas escapes, and, compressing itself in the closed end of the tube, passes at once to the liquid state.

How to print Aniline Colours on Calico.—First, a solution of gelatin is prepared, containing about 50 grms. to the litre of water. Solution of bichromate is added drop by drop till a straw-colour is produced; the aniline colour is then added, and the mixture thickened with dextrin or with roasted starch. After printing, the pieces are exposed for some hours to light, which renders gelatin insoluble in contact with chrome. The gelatin may be replaced by a solution of casein in a small quantity of ammonia.

An important Change in the Alkali Manufacture.—It seems, according to R. Wagner, that Leblanc's process is about to be superseded in England, Austria, and Germany. A solution of chloride of sodium is converted by bicarbonate of ammonia into chloride of ammonium and bicarbonate of soda; the latter crystallises out, while the chloride of ammonium remains in solution. This chloride of ammonium is distilled with limestone, and yields carbonate of ammonia, which is converted into bicarbonate by the excessive acid of the soda salt. No sulphur or sulphuric acid is needed. The soda obtained is very strong, and free from sulphur compounds. Plant, fuel, and labour are economised, and the escape of noxious gases is at an end. (As soon as we see this system at work, we will examine its probable consequences—scientific, sanitary, and commercial, says the "Chemical News.")

Cochineal Red on Cotton.—"Reimanns Färber Zeitung" says that this is done as follows:—To 10 lbs. of goods take 10 ozs. of tannin, and dissolve it completely in boiling water. Lay the goods overnight in the solution. Prepare some red liquor at 12° Baumé, and put into a small tub a sufficient quantity to work 2 lbs. of cotton yarn. Work the yarn for ten minutes. Fill up the red liquor to its original quantity, and work 2 lbs. more; and so on, till the whole of the yarn has been mordanted; it is then allowed to dry, with frequent turning. Piece-goods are winced for an hour in red liquor of the same strength. When dry, the goods are passed through a boiling bath of 1 lb. of prepared chalk to every 10 lbs. of cotton, and washed twice. 1 to $1\frac{1}{2}$ lb. of cochineal is boiled out in water; a colour-bath is made up at 40° Réaumur, with the addition of a little flavin, and the goods are worked, raising the temperature slowly to near the boiling-point. No. 35, 1873.

Modifications of the Cinchona Alkaloids and their Optical Qualities.—At the meeting of the Chemical Society, Nov. 6, Mr. D. Howard read a paper on this subject. After enumerating the various observations that had been already made, he drew attention to the approximate relation between the deviation caused by quinicin and cinchonicin and the alkaloids from which they are derived; thus the mean of the specific rotary power of quinine and quinidin in alcoholic solution is 47° to the right, and that of quincin, corrected for its combined water, 41°, whilst, in aqueous sulphuric acid, they are 20·5° and 19·4° respectively. A similar approximation is found to be the case with cinchonicin as compared with cinchonin and cinchonidin. The action of nascent hydrogen on the alkaloids in acid solution gives rise to compounds which Schützenberger regards as differing from the original compound in containing one atom more water; the author, however, is inclined to doubt this, as no evolution of hydrogen occurs when cinchonin or cinchonidin is treated with zinc and dilute sulphuric acid until a considerable excess of the acid has been added: the optical properties of the bodies formed are very similar to those of quinicin and cinchonicin. The author then proceeded to describe the method of preparation and optical properties of the various ethyl bases produced by the action of ethyl iodide or bromide on the cinchona alkaloids: the rotation produced by the salts of the ethyl bases is in most cases very nearly proportional to that which would be given by a salt of the original alkaloid, equal in amount to that contained in the new compound.

GEOLOGY AND PALÆONTOLOGY.

Darwinian Doctrines among Geologists.—Mr. Henry Woodward, F.R.S., F.G.S., has just published, in the "Geological Magazine" for December, the address which he delivered before the Geologists' Association at the opening of the present session, and we heartily wish we could publish it in full in these pages; for it is, without a question, the most able address that has been given to any geological society for many years. And this we say without any desire whatever to pay a compliment to the author. The whole tone of the address is calm, reasonable, and, above all, most philosophic as well. It is worthy of careful perusal, and we trust the Society has taken care to have it printed for private circulation among its members; for we know of nothing better calculated to arouse the dormant philosophic spirit than this intelligent address. There is but one part that we will quote, and that relates to the Darwinian doctrine of evolution, which we are delighted to see the author completely adopts. He says it "cannot be doubted that the majority of botanists and zoologists seem alike disposed to accept the doctrine of Evolution and Descent with Modification. There are still some scientific men, however, who find the derivative origin of species by descent repugnant to their ideas. For the opinions of such I have the greatest possible respect, feeling sure that, up to a certain point, controversial opposition to new theories has its beneficial aspect in ridding us of worthless notions. But Darwin's theory has already passed through the fire; like crude ore it has been washed, sifted, crushed, roasted, and smelted, and at the end the pure metal remains. The only question is one of terms and names. Professor Owen, who is himself a most advanced evolutionist, if we may be permitted to judge of his views by his published works, prefers to hold the conviction that all forms and grades of both vertebrate and invertebrate life are due to 'secondary cause or law,' not to 'natural selection.' Upon the nature of these very delicate and baffling distinctions I feel myself quite unable to enter on the present occasion. To the earnest seeker after truth it can never be an irreverent or idle object to investigate the process by which life has been gradually evolved on our earth; when, however, we have learned all that is in our power to discover, there is still the great problem of life itself unsolved, and we stand upon the threshold of the infinite."

A new Fossil Fish of unknown Age.—This is described by Dr. Traquair, of the College of Science, Dublin. Most interesting is his account of the structure of the upper surface of the bone near the posterior external angle. This is exactly similar to the structure of the scales and plates of many ganoid fishes. He says: "The bone is here very thin, measuring only about $\frac{1}{40}$ inch in vertical section. Immediately below the surface is an absolutely structureless layer of transparent ganoine about $\frac{1}{1000}$ inch thick. Through this, the punctures of the surface pass into a set of short vertical canals, each widening downwards so as to assume a rather conical figure. At their bases they are connected by horizontal tubes, and this system also communicates below with a close irregular network of ordinary Haversian canals, which ramify through the lower part of the section, and, becoming coarser below, cause the bone on its inferior aspect to assume almost a spongy appearance. The intervals between the set of short wide vertical canals, cup-shaped in the section, are seen to be each traversed by a vertical tube, which, coming up from the Haversian network below, soon divide in an arborescent manner into a great number of minute ramifying branches, which pass towards but not into the superficial layer of structureless ganoine. Adjacent trees of this kind also freely communicate with each other by means of arched branches, passing around and between the vertical canals between which their stems are situated. A beautiful kosmine-like layer is thus formed below the ganoine; it must be noted, however, that small lacunæ are occasionally seen among its minute tubules. In the true bone below, lacunæ of the ordinary type abound in the meshes of its Haversian network."

The Characters of the Irish Chalk.—A very good paper on this subject has been presented to the Irish Geological Society by Mr. E. T. Hardman, F.R.G.S.I. We shall give the result of chemical examination first. The following is the summary of the analysis. The specimens used were obtained from an old quarry in the townland of Legmurn, about a mile and a half north-east of Stewartstown. The chalk is so indurated as to be in reality a hard splintery limestone.

$CaCO_3$ . . .	97·320	ZnO .	traces.	Very perceptible even in small quantities of the chalk.
M_gCO_3 . . .	0·890			
SiO_2	0·537	BaO .	a trace.	
Al_2O_3 . . .	0·273	SrO .	a trace.	
Fe_2O_3 . . .	0·095	K_2O, Na_2O	amount not estimated.	
FeO	a trace.			

The author accounts for its hardness, in which respect it differs much from English chalk, by assuming an enormous pressure of the Irish basalt which was poured out over it. He says, when it is remembered that in the Hebrides the basalt reaches a thickness of between 3,000 and 4,000 feet, and that the Irish basalt, although now but from 500 to 1,200 feet thick, may have had similar proportions, there is no difficulty in referring the consolidation of the Irish chalk to pressure alone; for, taking the original thickness of the basalt at only 3,000 feet, the pressure on each square yard of underlying chalk would be about 2,000 tons. The analysis, which is extremely similar to one by Mr. Wonfor of the Chalk of Cushendall, Co. Antrim, shows that it is a limestone of very great purity, the percentage of silicious matter being so small as to be quite insignificant. It should, therefore, be of the highest value in many chemical manufactures, especially that of bleaching-powder. But it is remarkable that although in the North of Ireland an immense quantity of this material is used up, it is not made there, but is mostly imported from Glasgow and Lancashire. So far as he knows, there is not a single Chloride of Lime Works in Ulster.

British Fossil Botany in 1872.—The best *résumé* we have seen on this subject is that published by Mr. W. Carruthers, F.R.S., in the "Geological Magazine," October 1873. It is a most valuable list, and the author has added to the interest which such a list would have by briefly adverting to the nature of each paper. We trust that Mr. Carruthers will continue this record, which we should much like to see extended to other portions of palæontography and geology.

European Mammalia at the close of the Miocene Period.—Under the title of the "Fossil Animals of Mont Léberon," M. A. Gaudry has just published in Paris a paper which is of some interest, and which is very fully dealt with in a review in the "Geological Magazine," October. His conclusions are based upon the discovery of numerous intermediate forms between genera which have heretofore been considered as very distinct: for example, an ape intermediate between the *Semnopithecus* and *Macacus*; a carnivore between the *Hyæna* and Civet; a pachyderm between the *Anchitherium* and Horse; a ruminant between the Goat and Antelope. The comparison of fossils from other localities has afforded similar results. With the view of combating the opinions opposed to him, M. Gaudry has studied the fossil Miocene Fauna of Mont Léberon, and has arranged his observations under the following headings:—

1. The close of the Miocene Period was characterised by a great development of Herbivora.

2. The Miocene Mammalia prove that the types of the higher forms have been more variable than the lower.

3. An examination of the Mammalia proves that the Upper Miocene of Europe can be divided into two stages.

4. The study of the Miocene Mammalia supports the hypothesis that the separation of the Faunas has been only the result of the local displacement of the Faunas.

5. On the analogous forms of Mammalia which have preceded and followed those of the Upper Miocene.

6. On the distinction of races and species of some Mammalia at the close of the Miocene Period. These separate subjects are then discussed, and the reviewer deals with them in succession.

The Chemical Origin of Rocks.—This subject, on which of course much that is purely speculative is written, has been elaborately dealt with by M. Daubrée, and his book is briefly noticed in the "Geological Magazine." We merely call attention to the fact of its having been published, for the subject is much too vast to be described in a paragraph. However, we hope in an early number to deal with the whole question in an original article of some length. Till then we must leave it.

The Femur of Dinornis Struthioïdes in the British Museum.—We are indeed glad to learn that the bone of this huge extinct bird has come into the possession of the British Museum authorities. We are told so by Professor Owen, who described the bone in the "Proceedings of the Zoological Society," Nov. 12, 1839. In a letter to the editor of the "Geological Magazine," No. 112, he says: "The individual who, in October, 1839, brought this specimen to me, for sale, at the Royal College of Surgeons, asked ten guineas for it. When I had convinced myself that it was the shaft of the femur of a bird, and that the evidence supplied by the vendor made it at least probable that the specimen had been found in New Zealand, I reported the circumstances to the Board of Curators of the Royal College of Surgeons, and recommended the purchase of the specimen. This was declined. I had determined, on being entrusted with office in the Hunterian Museum, not to form a private collection, and my circumstances, in 1839, did not allow me to give ten guineas for a specimen; and this I stated to the vendor, in requesting permission to describe and figure it, which permission he liberally granted. The specimen was purchased by Benjamin Bright, Esq., of Bristol, to whom a copy of the abstract of my paper had been sent, and was placed in his private museum, which, on his decease, came into the possession of his son. On communicating to this gentleman the desirability of the original bone of the *Dinornis* being deposited in the British Museum, he most liberally permitted me to submit to the Trustees an offer, as a donation, of the entire collection made by his father and grandfather, including the original specimen which initiated the series of papers on the *Dinornis* that have since appeared in the 'Zoological Transactions.'"

A new Fossil Ape of the Lemur Group.—In the "Transactions of the Linnean Society of Bordeaux" for the present year M. Delfortrie describes the osteological characters of the cranium of a new species of fossil ape belonging to the family of the Lemurs, found by M. Bétille in the phosphate beds of Sainte-Néboule de Béduer, Lot, France; and the "Academy," Nov. 15, gives the following account of it:—"The skull, which is entire, is of an elongated conical form, and represents an adult individual. The occipital crest is slightly projecting, but wide in consequence of the development of the mastoïds. The parietals are very spreading, constituting nearly the whole of the cerebral arch. The temporals are flat, elongated, and exceed in height the half of that of the orbits. The frontal depressed, bearing a keel upon the median line. The orbital circle is closed, of nearly oblique oval form, strongly inclined towards the nose. The nasal bones are very elongated, slightly raised on the median line, and inclined on their exterior edge towards the junction with the maxillaries. The cranial characters are remarkably similar to those of the *Lori grèle*, but the dental system shows it to belong to an entirely new genus of the *Makis* family. With the exception of the principal and the two right hind molars, all the teeth were broken off by the workman's pick, but the roots of all of them, with the exception of the incisors, are adherent to the alveolæ, which are still intact, so that on allowing the normal number for the incisors, the series is found to be as follows: incisors two, canine one, premolars four, principal molar one, hind molars two. An insectivorous character is displayed in the sharp denticulation of the preserved teeth. To this fossil M. Delfortrie assigns the name of *Palæolemur Betillei*. While his paper was in the press M. Delfortrie received from the above locality a right mandible belonging to an individual of the same species. He has since forwarded both specimens to M. Albert Gaudry, who recognises in them many affinities with the Eocene or Miocene pachyderms, and traces a specific identity between the new fossil and *Aphelotherium Duvernoyi*, Gervais, and *Adapis Parisiensis*, Cuvier, both from the Paris gypsum; as well as with the *Adapis* from Barthélemy, near Apt.

An Examination of the Fossils from the Phosphate Beds of Quercy.—In the last volume of the "Comptes Rendus" of the French Academy M. P. Gervais gives the results of his examination of these deposits. Among the collections examined was that of M. Daudibertière, which was remarkable for the number and good preservation of its specimens, containing some fine examples of *Palæotherium* analogous to those occurring in the Paris gypsum beds, and some remains of *Rhinoceros minutus* and *Acerotherium*. There are also some teeth of a mammal related to *Rhinoceros*, but differing from that genus in some essential characters. M. Gervais, therefore, proposes a new genus for its reception, which he designates *Cadurcotherium*, and the species *Cad. Cayluxi*, from Caylux, whence it was obtained;

the animal was of greater height than *Rhinoceros* or *Acerotherium.* The teeth consist of the last upper molar and several lower molars; the upper, being larger and narrower than that of *Rhinoceros.* Its external face is slightly convex, and the hollowing of its crown is narrow and elongated. The lower molars are smaller than in *Rhinoceros*, with collines much more oblique and less projecting; their external face is undivided, and has but a slight curvature. The porcine remains from these beds consist of *Anthracotherium, Anoplotherium, Entelodon, Hyotherium, Cainotherium,* and a small animal allied to the latter, which possesses a well-marked bar between the first and second false upper molars. The ruminants consist of *Amphitragulus* and a species of *Cervus.* There are many species of *Hyenodon,* also *Cricetodon* and *Archæomys,* whilst *Peratherium, Aves,* terrestrial *Chelonians,* a species of *Crocodile, Lacertians*, and Serpents are all represented.

MECHANICS.

The Sand-blast Process.—One of the most interesting novelties in the mechanical world of late has been this process, which is of American origin, and which seems to pound the glass rather than to cut it. For curious polanscopic observations have been made, we fancy, by Mr. F. Wenham, V.P.R.M.S., on the subject. The main object of the inventor of this process was to engrave ornamental and other devices upon plain and coloured glass, upon stone, and upon metallic surfaces, in an expeditious and economical manner, and with a sharpness which is unattainable by any other means. The invention, which applies chiefly to intaglio and flat relief engraving, is based on the fact that when glass, stone, or metal is subjected to the impact of a blast of sand, or equivalent hard granular substance, the detrition of the surface exposed to its action will be rapidly effected. Although the sand-blast acts with energy upon hard or brittle surfaces, it has little or no effect upon elastic and soft substances, such as india-rubber, wax, paper, and lace, and it is upon such substances, therefore, that the patentee relies for protecting those parts of the glass, stone, wood, or metal which are intended to be untouched by the sand-blast. Thus, a piece of lace spread over and cemented to a sheet of glass will so effectually protect the glass when exposed to the sand-blast, that the threads of the network will, after a few seconds' exposure, be imitated on the glass by bright interlacing lines, while the rest of the glass surface will be reduced to a ground or frosted state. The efficacy of the blast depends upon its velocity. The sand may be propelled either by steam, water, or air, but steam is in general preferred where high velocities are required. When a large quantity of material is to be removed, as in the ornamenting of stone, a steam-jet of from 60 to 80 lbs. pressure is used. In this case the stencil is made of iron or rubber; but when a small quantity of material is to be worn away, or the surface merely depolished, as in ornamenting glass, a jet of air of from $\frac{1}{10}$th to 1 lb. pressure is preferred. With a low pressure, soft and delicate substances, such as paper designs, lace, leaves, &c., cemented on glass, may be used. With a steam-jet using two horse-power of steam at 70 lbs. pressure, and one pint of sand, two cubic inches of granite, four cubic inches of marble, or ten cubic inches of sandstone, may be cut away per minute. It will be obvious that flat or curved surfaces may be alike acted on by this process, the blast being in all cases directed at a right angle to the exposed surface. The blast process, besides executing ornaments in relief, or intaglio, may be applied for cutting grooves in quarries and tunnels, for dressing stone, for cutting stone in lathes, for cleaning scale from metals, for graining lithographic zinc, and for producing—by the aid of stencils or photographic gelatine—pictures, any variety of design, and even the most delicate line engravings.

MEDICAL SCIENCE.

Effect of Exercise on Temperature, and on the Circulation of the Blood.—One of the best papers that we have for a long time seen on this important subject is that which was some time since read by Dr. Handfield Jones, F.R.S., before the Royal Society. It is really one of the first instances in which practical use has been made of the sphygmograph in determining the action of the heart in excessive exercise. There are numerous sphygmographic records given in the paper which will well repay perusal. The principal conclusions at which the author arrives may be thus stated:—1. That the heart's force is, in most cases, more or less weakened by great exertion. 2. That the arterial contractility is probably always lessened, even when the exertion is moderate. 3. That after exhaustion the heart recovers sooner than the artery. 4. That the heart's action, in about one-third of the cases of severe but brief exertion, is increased in force. 5. That the acceleration of the pulse probably depends chiefly on exhaustion of the vagi. 6. That acceleration of pulse-rate has *per se* no effect in increasing intravascular pressure. 7. That the temperature is usually elevated by exercise from ·36° to 1°·8 F., but in rare instances, or after prolonged toil, may be lowered 1°·08 to 2°·16 F. 8. That the paresis of cardiac and heat-regulating centres, coinciding with consumption of nerve-force in motor centres, shows, that, in some way, one centre is capable of drawing upon another at a time of exigency. 9. That the dynamic test is indispensable to ascertain the lasting power of the heart, the tone of the vessels, and the validity of the nerve-centres regulating the temperature; in fact, to gauge the *radical* as distinguished from the *acting* forces. 10. That the capacity to endure fatigue well indicates, *cæteris paribus*, a like power to endure disease well.

Dr. Carpenter on Dr. Ferrier's Researches on the Brain.—At two of the Sunday afternoon lectures of the Sunday Lecture Society (November 1873) Dr. Carpenter gave discourses on the functions of the brain and what Dr. Ferrier has done towards a solution of this difficult subject. The best report of these lectures is unquestionably that given by the "Times," which is even too long for insertion in our columns. However, we may take a portion of it, more especially that part which relates to the inquiries made recently by Dr. Ferrier:—"The cerebrum of a bird may be entirely removed, as was long ago shown by Flourens, without destroying its powers of receiving sense-impressions and of performing respondent movements; and Dr. Ferrier has shown that the application of Faradic electricity to the *corpora striata* (which, though wrapped up in the hemispheres of the human brain, do not really form part of them) calls forth movement in nearly all the muscles of the body, the flexors predominating over the extensors; while a like stimulation of the *corpora quadrigemina* (another pair of the same fundamental series) calls forth violent action of the extensor muscles. Among Dr. Ferrier's most curious results is the control which the cerebellum is found to have over the movements of the eyeballs; and it seems probable that its special influence on the balancing movements of the body is related to this control over the visual direction. Proceeding, then, to the cerebrum, the lecturer pointed out that comparative anatomy and embryonic development agree in showing that the cerebrum of a reptile or bird is not the miniature of that of man, but represents only its anterior lobes; that in the lower mammalia the middle lobes sprout, as it were, from the back of the anterior; and that in the quadrumana and man the posterior lobes sprout from the back of the middle. Thus, as it is the anterior lobe which is common to all creatures possessing a cerebrum, while the posterior is peculiar to man and his nearest allies, it is in the former, not the latter, that (if there be any localisation of faculties) we should expect to find those which man shares with the lower animals. This conclusion, which had been distinctly drawn by the lecturer 25 years ago, is remarkably confirmed by the results of Dr. Ferrier's experiments. The surface of the hemispheres of the brain in the rabbit and other rodents is smooth, or destitute of convolutions; is formed by a thin layer of 'gray' or 'cortical' substance, composed of nerve cells lying in the interstices of a very close reticulation of capillary blood vessels; and the 'white' or medullary substance of the interior is composed of nerve fibres, which connect this cortical layer with the sensorium beneath, and which also pass between the different parts of the cortical layer itself. In ascending the mammalian series, however, we find the cortical layer folded into plaits, by which its surface is largely increased, and these plaits form what are known as the 'convolutions.' They were formerly supposed to be destitute of regularity, but the careful study of them by Leuret, Gratiolet, and others has shown that they are disposed on a regular plan, gradually increasing in complexity, which can be traced upwards through the quadrumana to man. It was, until lately, the current doctrine of physiology that no stimulation of the cerebrum would excite either sensation or motion, but it has been recently found that the application of a galvanic current to the cortical substance calls forth movements; and Dr. Ferrier has used with the best results the more intense Faradic current of an induction coil. The immediate effect of its application to the cortical substance was in all cases to produce an afflux of blood, shown by the visible distension of the vessels; and to the augmented activity of the reaction between the blood and the nerve substance, producing an excessive tension, like that of an overcharged Leyden jar, rather than to the direct stimulation of the nerve-substance itself, the lecturer attributed the discharges of nerve-force which produced movement, the evidence of this being furnished by the time that was required (especially when the two electrodes, or poles of the battery, were far apart) to call forth the action, as well as by the frequent continuance of action after the stimulation had ceased. Dr. Ferrier's experiments were in the first instance directed to the study of the phenomena of epilepsy; and he found that the application of the electrodes (the animal being stupefied by chloroform) to parts of the cortical layer at a distance from one another would excite general convulsions, exactly corresponding to those of some forms of epilepsy. But, on bringing the electrodes nearer and nearer to each other, so as to excite only a particular convolution or part of a convolution, he found himself able to call forth co-ordinated movements of particular groups of muscles, such as, in an animal in possession of its senses, we should regard as expressions of ideas and emotions. A rabbit munches, a cat moves its leg and foot as if clutching at a mouse or striking a ball, a dog wags its tail from side to side and then erects it, or even executes the movements of fawning. The precise localisation of these centres of motor activity is proved by the power of prediction which Dr. Ferrier has acquired as to the movement which would be caused by each particular application of his electrodes. The centres of all these movements are located in the anterior lobes, and in the anterior portions of the middle lobes; no movements being called forth by the application of the stimulus either to the posterior parts of the middle lobes of the cat or dog, or to the posterior lobes of the monkey; while the forward prolongation of their anterior lobes is equally irresponsive. Hence these negative results of experiment, *quantum valeant*, confirm the inference previously drawn by Dr. Carpenter, from comparative anatomy and from development, that the posterior lobes are the instruments of those higher operations resulting in ideas which do not prompt to motion."

Temperature of the Male as Compared with that of the Female.—A very excellent Memoir has appeared on this subject from the pen of Dr. J. Stockton Hough, in a new paper, the "Philadelphia Medical Times," Nov. 8th, 1873. It is worthy of perusal. We merely quote the author's conclusions. They are: 1. That males have, as a rule, from the beginning to the end of life, a higher temperature, and a less frequent pulsation of the heart, than females, varying, nevertheless, according to temperament, constitution, age, and condition of health. 2. That children have a higher temperature at birth, and for a short time subsequently, which, though slightly lower than that of an adult, nevertheless slowly and gradually declines to a certain point until about the sixth year of age is reached, after which it gradually increases until developmental maturity is reached, when it gradually and slowly declines again as old age (second childhood) advances. The pulsation of the heart follows just the opposite course, being most frequent when the temperature is lowest, and less frequent when it is highest. 3. That males appear to have a greater variation in temperature than females, thus agreeing with their greater variation in stature and many other peculiarities. 4. From all of which we conclude that the woman approaches more to her condition as a child than the man does, and is consequently less highly developed. The male is a secondary evolution from the female.

The Different Theories of Digestion are given by M. Claude Bernard in a recent lecture, which is published in the "Revue Scientifique de la France," October 18th, 1873. He gives an account of the various theories of digestion entertained by men of science from Hippocrates downwards.

The eminent Greek physician considered digestion as a cooking process. Galen maintained the existence of three kinds of digestion, the first performed in the stomach, the second in the duodenum, and the third in the liver; a view taken in later times by Servetus and Drake. Plistonicus, a disciple of Protagoras, identified digestion with putrefaction. Hilmon regarded digestion as fermentation, and divided it into six kinds, a view in which he was followed by Sylvius, Willis, Boyle, and others. The mechanical theory of Borelli, Bœrhaave, and Pitcairn considered digestion to be mere trituration, a view which, as far as animal food was concerned, was experimentally refuted by Réaumur. This naturalist introduced into the gizzards of birds portions of meat enclosed in perforated metal tubes. On killing the bird after some time the meat was found to have been dissolved. Seeds, however, enclosed in a similar manner, resisted the digestive process. Spallanzani succeeded in withdrawing gastric juice from animals, and by its means performed artificial digestion outside the living body. The author concludes with a notice of the observations made upon the celebrated Alexis St.-Martin, who, as is well known, had a permanent hole leading from without inward to his stomach, so that substances could be readily introduced into his stomach, and withdrawn from it.

Detection of Blood by finding its Crystals.—This is a much easier process than has been thought, and is also much more generally applicable. In blood decomposed, or that has been treated by acids or caustic alkalies, hæmoglobin is changed into a new substance; hæmatin is formed, which, combined with hydrochloric acid, gives characteristic crystals. In order to obtain them we must proceed thus: A small fragment of dried blood is placed on a glass slide; it is dissolved in a drop of water, and a minute portion of sea-salt is added. It is covered with a thin slide, and pure acetic acid made to pass between the two slides, and it is heated over a spirit-lamp to boiling-point. Acetic acid is again added, and it is heated afresh, and this is repeated till the crystals are obtained. They are rhomboidal, of a dirty brown colour, quite characteristic, and require to be seen with a magnifying power of three hundred or four hundred diameters. With the smallest quantity of blood this reaction can always be produced.

Effect of the Injection of Acid into the Brain.—A valuable paper on this question has been contributed to a late number of "Virchow's Archiv," from which the "Academy" makes a long extract, which we shall take in a shorter form. Nothnagel injected acid in the first place into the hemispheres of the brain of a rabbit. He found the animal remained quiet during the operation, and was subsequently lively and ate well. Its motility was apparently undisturbed, and no sensory disturbance could be detected. If the injection were made into the left hemisphere the right forefoot could be placed in any position, provided this were done slowly, without its being retracted, though similar attempts with the left forefoot were promptly resisted. The animal will sometimes remain in an abnormal position for several minutes. If the skin be slightly pinched the leg is, however, immediately retracted. When the animal lived over a week or so this peculiar condition became less and less marked. There was no corresponding condition in the hind limbs. A similar experiment on a dog gave the same results. The explanation which M. Nothnagel regards as the most probable is, that the animal has no ideal representation, or only an incomplete one, of the position of the extremity; in other words, there is partial paralysis of the muscular sense. He finds, however, from other experiments, that there is no very strict localisation of mental functions in definite centres situated in the cortex of the cerebrum. Nothnagel finds lesion of the lenticular nucleus is always followed by motor paralysis, but not necessarily by sensory. Injection into a definite point at the anterior and inner part of the corpus striatum (nucleus caudatus) gave the following results:—For the first two or three minutes, or even longer, the animal remained quiet, but gave the impression of being conscious. Then, without the slightest external irritation, it began to leap either straight forward or performed the *mouvement de manège*. It made from four to eight leaps, then sat still for a few seconds, then leaped again, and so on, the movements being always hasty, the pause shorter and shorter, till at length the movements became continuous, and after five to eight minutes it fell over on its side, the legs moving violently. In the course of a quarter or half an hour the animal was quite exhausted, and lay apathetic. The point of the brain which when irritated produced these effects Nothnagel calls the nodus cursorius.

Goître Caused in the French Army by Pressure on the Throat.—A long paper having been recently published, showing that goître was epidemic, Baron Larrey states (*Comptes Rendus*, September 29th) that he has frequently found young soldiers suffer from an enlargement of the thyroid gland and neighbouring tissues, owing to pressure of the shirt-button, coat-collar, and clasp of the chapote; a purely mechanical cause. On replacing the collar with a cravat the glandular swelling disappeared. He is unwilling to accept the term *epidemic goître* for an affection at once simple and easily remedied, and which he calls thyreöiditis; and he suggests the possibility of the mechanical cause acting in the St.-Etienne case.

Theory of the Pulse in Normal and Abnormal States.—The "Chemical News," in one of its October numbers, gives the following account of M. Bouilland's views, lately laid before the French Academy:—The author distinguishes four periods in each "arterial revolution," or the changes occurring from commencement of one pulsation to that of the next. Of the two *shocks* the first (known as the *pulse*) is produced by the ventricular systole of the heart; the second results from systole of the arteries (which are passive in the first, active in the second). These two alternating shocks constitute the normal *dicrotism*, of which the abnormal dicrotism is merely the intensifying, simple or double, that is, affecting either one shock or both. In opposition to Harvey and other physiologists, the author supposes in the arteries an *impulsive* force, without which the transport of the blood into all parts of the body could not be effected. The co-ordinated movements of arteries and heart are ruled by ganglionic innervation, but the precise situation of the co-ordinating nerve-centre has yet to be discovered.

METALLURGY, MINERALOGY, AND MINING.

Death of Gustav Rose.—This distinguished mineralogist has lately died at Berlin, in the 76th year of his age. In him Germany and the world have lost a wise and noble man,—conceded by all to be the first in science among the learned men of Germany. At first devoting himself to engineering, he subsequently gave all his time to scientific pursuits, and in 1823 took up his residence in Berlin. In 1826 he became Professor of Mineralogy in that University, and, after the death of Weiss, Director of the Royal Mineralogical Museum. He travelled extensively in Scandinavia, England and Scotland, Italy and Sicily, France and Austria. In 1829 he made with Humboldt and Ehrenberg the famous tour to the Ural and Altai Mountains and the Caspian Sea, and beyond to the borders of China, a journey which first made known the mineralogical resources of the extensive Russian Empire. His researches on his native soil were confined to the Silesian Mountains. He devoted himself to the study of meteorites, those wonderful bodies which reach the earth from stellar space. With his keen penetration he discovered the structure of iron meteorites, and the mineral components of the stony ones; and studied the striking differences between rock-making in a cosmic atom, and in the solid crust of the earth.—"Geological Magazine."

Death of Professor Breithaupt of Freiberg.—Not long after the death of Gustav Rose the science of mineralogy experienced, says the "Academy," Nov. 1st, another great loss by the death of the venerable Prof. Breithaupt of Freiberg, which took place on the 22nd of October. Johann August Friedrich Breithaupt was born at Probstzella, near Saalfeld, in May 1791, and so far back as 1813 already held an appointment in the institution, his connection with which has now, after a lapse of sixty years, been severed by his death. First he was appointed Edelstein-Inspector and Hülfslehrer in the Bergacademie, and in 1827 was created Professor of Mineralogy in that school. His first work was a "Kurze Charakteristik" of the mineral system, which appeared in 1820, followed by a "Vollständige Charakteristik" that passed through two editions. His chief production, however, was the "Handbook of Mineralogy," which appeared in three volumes, between the years 1836 and 1847. His memoirs on minerals, written from time to time during more than half a century, from the first, that appeared in 1855, on genuine crystals, to the one dictated with difficulty through failing sight and increasing infirmity, and published in the "Journal für praktische Chemie," at the commencement of the present year, contain vast stores of results of the highest value for the advancement of mineralogical science.

Metalliferous Veins in Cornwall.—This subject is a curious one for a French metallurgist to take up. M. Moissenet draws the following conclusions from his observations:—The parts of the vein whose incline approaches most nearly to a vertical direction are the most productive. The rich portions are commonly, in Cornwall, enclosed in a gangue of moderate hardness. Most frequently the metalliferous bands or columns incline in the same direction as the gangue. The rich portions are frequently disposed according to the direction of the stratigraphical system to which the initial fracture of the vein is related.

The Iron Mines of Michigan.—An article on these mines is in a late No. of the American "Polytechnic Bulletin" (a new journal). It says that in Michigan there are five varieties of ore, of which the most valuable so far developed is the Specular, which is a very pure sesquioxide, giving a red powder, and yielding in the blast-furnace from 60 to 70 per cent. of metallic iron. The ore occurs both slaty and granular or massive. It is often banded or interlaminated with a bright red quartz or jasper, and is then called "mixed ore." Probably the next in order of importance is the soft Hematite, which much resembles the brown Hematite of Pennsylvania. This ore is generally found associated with the harder ores, from which many suppose it is formed by partial disintegration. It contains some water, chemically combined, is porous in structure, yields about 55 per cent. of metallic iron in the furnace, is more easily reduced than any other ore of the district, and it forms an excellent mixture with the Specular. There are probably several varieties of this ore which have not been well made out. That found at the Jackson, Lake Superior, and New England mines is associated with the Specular, while the Foster bed is several miles removed from any known deposit of that ore. The magnetic ore of the district has thus far been found only to the west of the other ores. At the Washington, Edwards, and Champion mines, no variety but this is known to exist except the Specular, into which the Magnetic is thought sometimes to pass, the powder passing from black to purple, then red. This view is said to be strengthened by the fact that the Specular ore is found in octahedral crystals, which form is well known to belong to the magnetic oxide; hence, it is alleged, that the specular deposits were once magnetic, which by some metamorphic action have been robbed of one-ninth of their oxygen, which would make them chemically anhydrous Hematite. The Flag ore is a slaty or schistose silicious Hematite, containing rather less iron, and of more difficult reduction than either of the above named. It is often magnetic, and sometimes banded with a dull red or white quartz. This ore varies much in richness, and comparatively little has been shipped. It is probably the most abundant ore in the district. A Silicious ore, containing a variable amount of oxide of Manganese, is found at several points, accompanying the Flag ore. This ore is unquestionably of great value as a mixture, but as it has just been introduced, its importance has not been determined. The generally received geological theory of the origin of these ores is, that they were aqueous deposits, which have been highly metamorphosed. The masses are lens-shaped, varying much in thickness, on which the value of the mass chiefly depends. These masses are interstratified with a soft green slate, which always accompanies the Specular and Magnetic ores. Overlying these beds is usually found a quartz rock, which is probably one of the most recent of the district. Below the Specular is a green stone, often slaty, and beneath this are one or more horizons of the Flag ore, separated by crystalline schists. Next older than the Flag ores is another quartzite, which seems to be sometimes replaced by a silicious marble. Yet older are the granite rocks, which are supposed to belong to the Lawrentian system of Canada—the schists above named, including the ores, belonging to the Huronide system.

MICROSCOPY.

Browning's new Non-vibratory Microscope.—Mr. Browning has just brought out an instrument, the plan of which has been some time in use on the Continent, and which was in this country devised by Mr. Mayall, jun. Many plans have been tried for getting a revolving stage which should have its centering perfect, but they have almost always failed. In the present instance the stage and body are made in one piece, so that they revolve together, thus preventing any absence of complete centering, for of course the focus cannot be altered in even the slightest lateral direction. When first we saw the instrument we were under the impression that it could with difficulty be made in any binocular form, but this difficulty has been completely got rid of by Mr. Browning, who has already made several of this variety of instrument of the binocular kind.

Valuable Researches on the Monads.—Decidedly the best investigations of these creatures that have ever been conducted are those now being made and published in the "Monthly Microscopical Journal" for December. The authors of the paper describing them are the Rev.W. H. Dallinger, F.R.M.S., and Dr. Drysdale. Of one form which they have dealt with they say:—"We had thus gathered up the threads and completed the life-history. The usual method of multiplication is by fission, which goes on apparently to exhaustion. Amongst enormous numbers there are a few distinguished from the others by a slight increase in size and the power to swim freely. These become still—for a time amœboid—then round; a small cone of sarcode shoots out, dividing and increasing into another pair of flagella. The disk splits—each side becomes possessed of a nuclear body, and two well-formed monads are set free. These swim freely until they attach themselves to an ordinary form that has just completed fission, so that the nuclei are approximate. Sarcode and nuclei melt into each other; the form becomes free-swimming and triangular in shape—rests—loses its flagella; becomes clear and distended; then bursts at the angles, pouring out indescribably minute granules, from which myriads of new forms arise and repeat the cycle."

On the Use of Naphthalin in Section Cutting.—Mr. John Barrow read a paper on this subject before the Manchester Literary and Philosophical Society. He said:—"I wish to bring before the notice of the members, and those microscopists who are interested in cutting sections of soft or delicate tissues, the use of naphthalin as a support for such tissues in the section cutter. The advantages obtained by the use of naphthalin over wax and other bodies recommended for this purpose are—A low fusing-point, absence of contraction in the cutter, very little injury to the edge of the knife, and very ready solubility after cutting in benzol or spirit, so that the substance is removed at once from the section without injury. Naphthalin is a body not very generally known outside the works of the tar-distiller or colour-maker, so that possibly some of the members may not be able to obtain samples readily, but I shall have pleasure in supplying it to any of our own members."—"Chemical News," October 31st.

The Microscopical Aspects of Circular Solar Spectra.—These are given in a paper in the "Monthly Microscopical Journal" October, by Dr. Pigott, F.R.S., who thus tabulates them:—"1. As stated in the paper 'On a Searcher for Aplanatic Images,' regarding a convex lens as under-corrected, under-correction is shown by the appearance of the rings below or beyond the focal point and evanishment into mist above it. 2. Similarity in the rings on both sides (with change of colour also) denotes a balance more or less delicate of the aberrations. 3. An excentric position of the solar disk and a crowding of the rings more closely on one side than the other of the circular spectrum denotes parallelism, but non-coincidence of the axes of the convergent and divergent pencils. 4. Rare and beautiful forms resembling parachutes, vases, or comets, made up of ellipsoid, parabolic, or hyperbolic diffraction-lines, denote obliquity. 5. Their form depends on the nature of the aberrations present, and the mode of arranging the axis of the cone of rays forming the solar disk. 6. Inaccurate centering of the component lenses, either at the heliostat or in the observing or miniature-making objectives, is shown by 'excentric turning' patterns and the appearance of two or several central disks at the smallest focal spectrum. 7. The apparatus necessary to display these brilliant phenomena must be exceptionally heavy and steady, and the fine adjustment should have a screw 100 threads to the inch, as the ten-thousandth of an inch in the axis of observation completely changes the aspect of the phenomena."

List of Papers on Microscopic Anatomy published in the "Monthly Microscopical Journal" for October, November, December. There were:—

A Description of the Thread-worm, Filaria immitis, occasionally infesting the Vascular System of the Dog, and remarks on the same relative to Hæmatozoa in General, and the Filaria in the Human Blood. By Francis H. Welch, F.R.C.S.E., Assistant Professor of Pathology, Army Medical School, Netley, Southampton.—Researches in Circular Solar Spectra, applied to test Residuary Aberration in Microscopes and Telescopes; and the Construction of a Compensating Eye-piece, being a Sequel to the Paper on a Searcher for Aplanatic Images. By G. West Royston-Pigott, M.A., M.D., Cantab., Memb. Roy. Col. Phys., Fellow of the Camb. Phil. Soc., the Royal Ast. Society, &c., and late Fellow of St. Peter's Coll., Cambridge.—A New Freezing Microtome. By William Rutherford, M.D., Professor of Physiology, King's College, London.—On an Organism found in Fresh-pond Water. By R. L. Maddox, M.D., H.F.R.M.S.—A Description of some New Species of Diatomaceæ. By F. Kitton, Norwich.—Nematophycus or Prototaxites? By William Carruthers, F.R.S.—On Immersion Objectives of greater Aperture than corresponds to the Maximum possible for Dry Objectives. By Assistant-Surgeon J. J. Woodward, U.S. Army.—On Bog Mosses. By R. Braithwaite, M.D., F.L.S.—On the Investigation of Microscopic Forms by means of the Images which they furnish of External Objects, with some Practical Applications. By Prof. O. N. Rood, of Troy, N.Y.—Further Researches into the Life History of the Monads. By W. H. Dallinger, F.R.M.S., and J. Drysdale, M.D.—Some Remarks on the art of Photographing Microscopic Objects. By Alfred Sanders, M.R.C.S., F.L.S., and F.R.M.S., Lecturer on Comparative Anatomy at the London Hospital Medical College.—Immersed Apertures. (A Reply to Col. Dr. Woodward.) By F. H. Wenham, Vice-President R.M.S.—On the Crystals in the Testa and Pericarp of several Orders of Plants, and in other parts of the order Leguminosæ. By George Gulliver, F.R.S.

PHYSICS.

Observations on the Capillary Movements of Liquids. — M. Decharme's recent observations on this subject are of considerable value, and we are grateful to the writer who, under the initials of A. B. M., has given an account of them in the "Chemical News" of November 7th. After citing a number of experiments, the author says that, "*En resumé,* the little volatile, very soluble, very hygrometric liquids, which do not crystallise in the blotting-paper, are those which rise the highest, if not the most quickly. The order of capillary heights, that of velocity, and that of total duration, may therefore be, and in fact are, quite different for the same liquids experimented on successively in capillary tubes and in strips of blotting-paper. The laws governing the two phenomena are different. It appears, from experiments made on a great number of liquids, differing much both as to chemical composition and physical properties, that with capillary tubes the aqueous solution of chlorhydrate of ammonia and water are in the first rank for velocity and final height; while in strips of blotting-paper these two substances are surpassed in both respects; in the second, especially, by dilute acids, alkalies, and several potassic, sodic, calcic solutions, &c. Further, in strips of paper, chlorhydric acid has the greatest velocity, and generally reaches the maximum height. Silicate of potash, which, with capillary tubes, stood between glycerine and olive-oil, is here at the very bottom of the scale; its movement is almost *nil*; it only, indeed, reaches about 4 m.m. in the blotting-paper. From the preceding results relating to ascent of liquids in multiple strips, the following inferences are drawn:—1. They explain how liquids, water in particular, with matter held by it in solution, rises to such great height in porous building materials of a house, where the foundations rest in a damp soil. It is not an extraordinary thing that, after some time, the presence of moisture and saline matters should be detected, not only on the ground floor, but on the first, and even the second; the liquid ascending in capillary substances of great thickness, and not subject to much evaporation. 2. The vessels of plants, with their numerous anastomoses, and the permeability of the tissues composing them, are rather comparable to the superposed strips of blotting-paper than to capillary tubes. It is not surprising that the greater part of the saline solutions containing solids fit for their nutrition rise in the vessels to heights much greater than water would do, and also more quickly. It is not necessary, then, to have recourse to an extreme fineness of vessels, to explain the ascent of liquids in the tissues of plants. It is sufficient to remark that the saline solutions which naturally exist in the soil rise higher than pure water; sometimes to double the height, *e.g.*, the following salts:—Nitrate, sulphate, carbonate, bicarbonate of potash, nitrate of lime, carbonate of soda, chlorides of potassium, and of sodium.

The Chlorophyll Spectrum.—This spectrum, says M. Chautard (in the "Comptes Rendus," Sept. 8th), is characterised by a number of bands, one of which has the special properties of sensibility, sureness (in its division by alkalies, a character not found in the lines of any other organic liquid), and generality. Chlorophyll in plants exists in three different states, distinguishable through the spectrum—in leaves newly formed, in adult, and in dead leaves. In the first case, chlorhydric acid produces *accidental temporary* bands. In the second it produces quite another system of bands, which the author calls *accidental permanent.* In the third (and alcoholic solution), the accidental permanent bands appear immediately, without the intervention of hydrochloric acid. Chlorophyll is much less alterable than is generally supposed. It resists the action of iodine, acids, alkalies, digestive work; at least retaining characters by which it may be detected in mixtures the most complex and varied, and after lapse of considerable time.

How to Compare Different Samples of Gunpowder.—A very good paper on this important practical question appears in the "Moniteur Scientifique" for September, by M. de Tromenic, who describes his plan thus:—The apparatus proposed is a cylindrical vessel of cast steel of the capacity of half a litre. The sides are 3 to 4 centimes in thickness. The cylinder is closed with a screw stopper pierced by a central channel, provided with a tap and two lateral orifices, into which are cemented two wires from an electric apparatus to ignite the charge. It would be useful to fix a thermo-electric element in one of the sides of the cylinder to indicate the temperature of the gases in periods following the explosion. The cylinder is placed in a sheet-iron receiver full of water, which serves as a calorimeter, and which is again enclosed in a trough full of cotton, to avoid loss of heat. The cylinder is fixed immovably by a pressure-screw resting upon the stopper. A thermometer measures the temperature to about $\frac{1}{100}$th of a degree. The water is provided with an agitator.

Sugar from Beet-root, and the Use of Horsky's Diffusion Apparatus.—In one of the numbers of "Les Mondes" for September is an account of this method. It says that this apparatus does away with the rasping process in the manufacture of beet-root sugar, dispenses with three-fourths of the manual labour, and extracts the saccharine matter completely. The yield of sugar obtained by the use of this arrangement has this season amounted to 8·5 per cent., an amount greatly superior to that obtained in neighbouring establishments where other processes for extraction are in use.

The Stroboscopic Determination of the Pitch of Tones.—A very interesting paper on this curious subject is that by Herr Mach, which was read some time ago before the Academy of Sciences at Vienna, and has recently found its way in abstract into one of our English scientific magazines. In Herr Mach's apparatus there is a cylinder which makes three revolutions in a second, and is divided into five octaves. At one end of it begins 10 bands, which, however, become more numerous and dense towards the other end, being there 320. To the axis of a syren is fixed a disc having equidistant radial slits of the same number as the holes in the syren-disc. The surface of the rotating cylinder is looked at through this slitted disc, while the syren tone is gradually raised. According to the stroboscopic principle the bands look distinct and at rest where there pass before the eye an equal number of them and of slits in the disc. If a scale of numbers of vibration be attached to the cylinder, the number of vibrations of the syren can be at once ascertained by observing the part corresponding to the distinct and still ring of the cylinder. One sees, however, distinct and at rest, not only the part of the cylinder corresponding to the number of vibrations of the syren, but also all those parts which correspond to the harmonic over tones. Of all such parts it is, of course, that one which furnishes the smallest number of vibrations that corresponds to the vibration-number of the syren. (The author gives further details of the apparatus.) The determination may be varied in accuracy by varying the bands on the paper of the rotating cylinder. The apparatus may be applied to other sounding bodies. Thus, let a mono-chord string be stretched at right angles to the axis of the cylinder; then simple teeth (Zachen) appear where the sounding string is opposite that part of the cylinder indicating the same number of vibrations. Another application is to attach small mirrors to tuning-forks, and watch in them the image of the rotating cylinder. An organ-pipe may be also submitted to observation with aid of König's capsules and dancing jets.

A New Society in London for the Study of Physics.—A preliminary meeting was held on Saturday, November 29th, 1873, in the Physical Laboratory of the Science Schools, South Kensington, to consider the formation of a Physical Society. The chair was taken by Dr. J. H. Gladstone, F.R.S. Thirty-six gentlemen were present, including most of the Physicists of London. It was resolved that the following gentlemen be requested to serve as an organising Committee:—W. G. Adams, E. Atkinson, W. Crookes, A. Dupré, G. C. Foster, J. H. Gladstone, T. M. Goodeve, F. Guthrie, O. Henrici, B. Loewy, Dr. Mills, A. W. Reinold, and H. Sprengel. A letter was read from the Lords of the Committee of Council on Education, granting the use of the Physical Laboratory and Apparatus at the Science Schools, South Kensington, for the purposes of the Society.

ZOOLOGY AND COMPARATIVE ANATOMY.

Further Researches on the Spectrum of the Plantain-Eaters.—The researches of Mr. Church which were carried out some years since (and who was kind enough to send us some of the feathers at the time) have been recently confirmed by the discovery of Mr. J. Monteiro, who has found these birds, the Plantain Eaters, in great quantity in the neighbourhood of Sierra Leone. He says ("Chemical News," October 17) that "these lovely birds are common on the West Coast of Africa, and on that part of it that I am well acquainted with, viz., from Loango, in 5° S. lat., to Little Fish Bay, in 15° S. lat., their loud and prolonged cry is to be frequently heard in the thick forest, where they find their fruit food most plentifully. Over the whole of the country I have mentioned, and for a considerable distance inland, copper is found most abundantly distributed as malachite, or green carbonate; in fact, specks and indications of the green mineral are to be noticed almost everywhere. Whether such is also the case on the West Coast, at Sierra Leone, Senegal, &c.—where these birds are, I believe, still more usually found—I cannot say; but there is no doubt that in the large extent of country I have mentioned and explored for many years, and where these birds are common, copper is found very extensively disseminated. I am unable to say whether the copper enters their system as a constituent of their food, as suggested by Mr. Church, but I believe it most probable that these birds are attracted by the bright green of the malachite, and swallow small particles of it with the gravel, &c. that they, in common with all birds, consume with their food."

The Phosphorescent Organs of Elater Noctilucus have been investigated recently by MM. Robin and Laboulbène, who state ("Comptes Rendus," T. 77, No. 8), that the light first appears in the centre, then spreads throughout. A yellow linear zone of adipose tissue at the exterior, at length becoming luminous, is yet not photogenic; it only reflects the light produced by the central part. But it does so, not only from its internal face but throughout its thickness, the action being favoured by the transparency and high refringent power of the fatty globules. The phenomena of dispersion and interference thus produced are the cause of the remarkable brilliancy appearing when the light from the centre reaches as far as this zone. As to the changes of molecular state in the tissue proper of the organ, the authors think the phosphorescent tissue produces a substance which slowly accumulates in the cells independently of all nervous influence, and of the same order with other secretions; and that only the act by which it is discharged is voluntary. The principle rendering the cells luminous behaved like the *noctilucine* extracted by Phipson. The abundance of urates in the cells makes it probable that uric acid results from the photospheric decomposition of the preceding coagulable compound. The large number of tracheæ in the apparatus is doubtless connected with the consumption of oxygen accompanying the phenomena.

Multiplication of Acaridæ.—M. Mégnin recently read an important paper on this subject, which has been abstracted by the "Academy," Nov. 1st [which journal, we are sorry to learn, is less likely to give us similar abstracts in its future numbers.] He asks—"Whence come the legions of acaridæ which make their appearance with such rapidity in decomposing fluids, and what becomes of them when their work of destruction is accomplished, and the matter on which they feed is reduced to the condition of a dry powder? These organisms, he remarks, have no wings to bear them from spots desolated by famine; they have not the agility of ants to enable them to undertake long migrations; they have soft integuments which form but a very slight protection against external agents and their numerous enemies; their eggs, relatively large, are not found in the dust of the atmosphere in company with the germs of moulds and infusory animalcules; and they do not possess, like the anguillules, rotifers, and the tardigrades, the power of reviving after desiccation. Hence they are often referred to as illustrating the correctness of their views by those who hold the doctrine of spontaneous generation. But, according to Mégnin, what happens in a colony of tyroglyphs, when the privation of food seems to promise them speedy destruction, is that all adult and aged individuals as well as the young hexapod larvæ die, but the young and the octopodus nymphæ are preserved. These undergo a change of form and become clothed with a cuirass which completely disguises but at the same time protects them; moreover, they acquire a sucker by which they are enabled to adhere firmly to any passing object, such as flies, spiders, myriapods, and insects of all kinds, or even to quadrupeds, by which they are transported to places they could never reach by their own unaided efforts. If they find a suitable locality, as on a young mushroom or a mass of decomposing substance, the little acaridan quits its temporary host and its hypopial form and reassumes the original tyroglyphic one. Under the influence of abundant food it rapidly enlarges, becomes a sexual adult, and in forty-eight hours a new colony appears."

The Circulation in a species of Delphinus.—This is thus described by Dr. H. C. Chapman, in a recent paper in the "Proceedings of the Academy of Natural Science of Philadelphia." He says that the circulation of the blood offers us interesting peculiarities in the existence of vast plexuses, the breaking up of the brachial and other arteries into *rete mirabile.* Of the distribution of the arteries, the intercostals are the most remarkable. They are developed, twisted, interlaced to such an extent, as to give the appearance of a large thoracic gland, formerly in fact described as such. By this arrangement of the intercostal and other arteries there are formed large reservoirs of arterialized blood, enabling the animal no doubt to remain submerged for long periods of time. The dividing of the brachial artery into numerous branches has been explained by reference to the shortness of the pectoral fin or upper extremity, but this distribution has been observed in certain Lemurs and other animals, in which the upper extremity is well developed.

The Abdomen of the New Zealand May-fly.—This is certainly a curious organ. It is described by Mr. M'Lachlan, who, in a recent number of the "Entomologist's Monthly Magazine," in describing *Oniscigaster Wakefieldi,* a new genus and species of Ephemeridæ, from New Zealand, says that the extraordinary abdomen of this genus, if considered apart and without regard to the rest of the body, might almost be pardonably mistaken for that of some Myriapod, without the legs, or of a crustacean. Females only have been discovered, and till we gain some knowledge of the characters of the male, the affinities must remain somewhat uncertain. Mr. Eaton has pointed out in his Monograph on the Ephemeridæ that a tendency to lateral production of the terminal segments of the abdomen is shown in several genera, but the amount of expansion hitherto known is infinitesimal compared with that present in this new form. For actual affinity in this respect one must look to the aquatic stages of some forms; and if the assertion by MM. Joly that the so-called genus of branchiopod crustacea *Prosopistoma Latreille* is, as appears probable, in reality only the aquatic condition of an Ephemerid, we have in the "Binocle à queue en plumet" the nearest ally, so far as regards abdominal structure, to *O. Wakefieldi.*

Does a species of Bird always Build the same Nests?—An article in our present number shows that they do not. But we have additional evidence in support of this idea in the following remarks, which were recently made to the Natural Science Academy of Philadelphia (September 23rd), by Mr. Gentry, who has been examining the nests of *Vireo solitarius.* He states that Audubon, in describing the nest of *Vireo solitarius, Vieil.*, affirms it "is prettily constructed and fixed in a partially pensile manner between two twigs of a low bush, on a branch running horizontally from the main stem, and formed externally of gray lichens, slightly put together, and lined with hair, chiefly from the deer and raccoon." My experience has been quite different. Out of the many nests which I have seen and examined, I cannot recall a single specimen that will answer to the above description. I have five nests of this species, four of which are perfectly similar in structure; the remaining one, formed of the culms of a species of *Aira*, constituting an exceptional case, and the only one that has ever fallen under my notice. They are all shallow, loose in texture, scarcely surviving the season for which they were designed, and placed between two twigs of a cedar or a maple tree, at a considerable elevation from the ground, on a branch nearly horizontal to the main axis. They are built entirely of clusters of male flowers of *Quercus palustris*, which, having performed their allotted function, don their brownish hue at the very period when they can be utilized. Here is evidently a change within a moderately short period, rendered necessary by external causes. This necessity may have grown out of inability to procure the favourite materials, or a desire for self-preservation. In the case of the species under consideration, it cannot be denied that the utter inability, without unnecessary physical effort, to procure the hair of the afore-mentioned animals, particularly in sections where *they* have been compelled to retreat before the advance of man, may have been one of the causes which have induced the change. I am satisfied, however, that it has not been the leading one, but that self-preservation has operated in this case for individual and family good. The adaptation of the colours of the female bird to the tints of surrounding objects, during the trying period of incubation, and the establishment of certain resemblances to familiar external objects, are two of the ways in which it manifests itself.

A New Ganoid-fish from Turkistan.—The "Annals of Natural History," vol. xii. p. 269, contains a good translation of a Russian paper, by Professor Kessler, on a remarkable fish belonging to the family of the sturgeons, discovered by A. P. Fedchenko, in the River Suir-dar, in Turkistan. This fish differs greatly from all the known species of the genus Accipenser, in which Russia is so rich, and belongs to the genus *Scaphirhynchus*, established some time ago by Heckel as a North American sturgeon. Professor Kessler calls the Turkistan fish *S. Fedtschenkoi*; the native fishermen do not consider it to be a distinct species, but regard it as only the young of the sturgeon of the Aral Sea. They evidently do this in consequence of its normally small size, for the largest of twelve specimens examined by Professor Kessler was but 8½ inches long, several of them being perfectly mature. Dr. Günther, in a note appended to the translation, remarks that this discovery is an additional interesting item in the series of instances by which the close affinity of the North American, North Asiatic, aud European faunas is proved. He quotes as an analogous case the discovery of *Psephurus gladius* in the Yantsekiang, and adds: "After the discovery of this species, that of a Scaphirhynchus in Asia might have been foreseen, just as I anticipate with confidence the discovery of a Ganoid in Borneo."

A New American Pleuronectoid Fish, Glyptocephalus Acadianus, has been recently discovered by Dr. Theodore Gill. The only known specimen of the new type was obtained from a pond at Eastport, Maine, during the month of August, by an attaché (Dr. Edward Palmer) of the Commissioner of Fisheries (Professor Baird), and, notwithstanding the assiduous attentions of the commissioner and his staff, no other specimens were found; it must, therefore, be actually a very rare fish, or (what is more probable in such cases) peculiar in its habitat, and rarely coming within the range of operations of the fisherman. An attentive examination and comparison of this species with the *Glyptocephalus cynoglossus* of Europe, indicate that it is congeneric with that species and with the *Platessa elongata* of Yarrell, but more nearly allied to the latter. As the genus is now for the first time introduced into our fauna, a description of the characters common to all the species (generic), as well as distinctive of the new form (specific), is given, but would not prove of interest to the general reader.

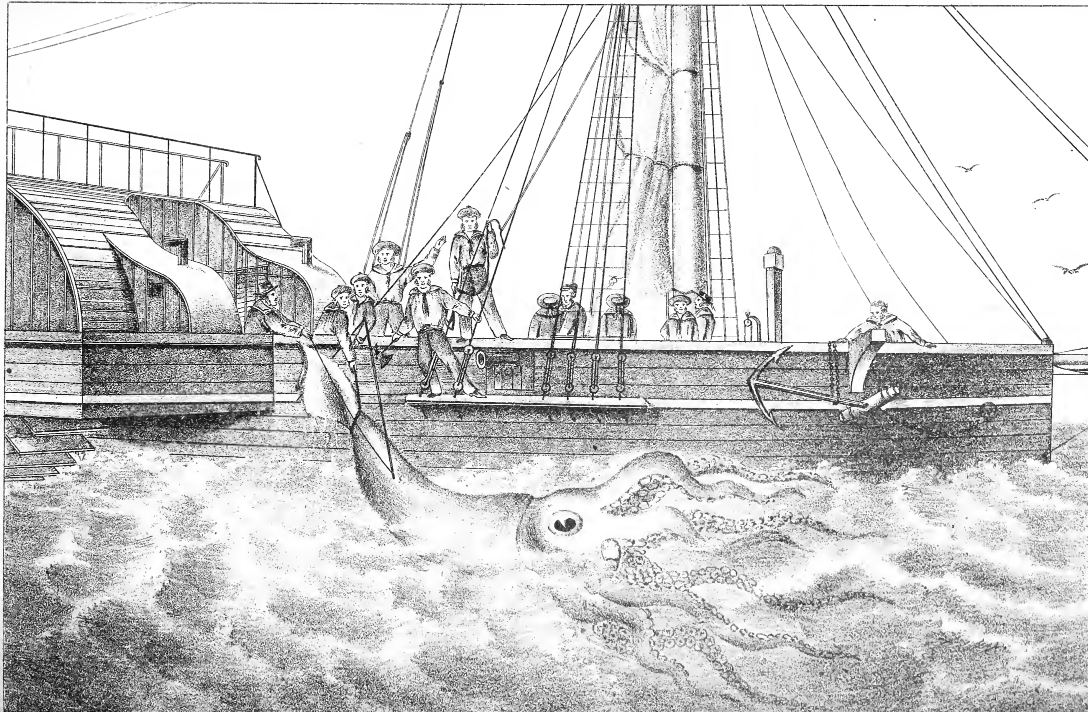
Claparède on the Sedentary Annelids.—An exquisitely touching narrative of the later labours of the eminent Swiss naturalist appears in "The Academy" for November 1st, 1873, which we quote in part, only because of its length, but we wish we could include it all. This memoir, says the writer, alluding to the work on Sedentary Annelids, "was written in the autumn of 1870, on the eve of Claparède's departure for Naples; on his way home again in the following spring he died. A sketch of his life by Henri de Saussure and a portrait by Hébert are contained in this memoir. The biographical notice originally appeared in the "Archives de la Bibliothèque Universelle de Genève," but by the desire of the family is republished here, with a few corrections. A very brief sketch of this memoir may not here be out of place. Filled with a determination to thoroughly investigate the structure of the Annelids, but living at the time at Geneva, Claparède was compelled to select an Oligochete worm for his researches; the results of his investigations appeared in a most exhaustive and beautifully illustrated monograph: "Histologische Untersuchungen über den Regenwurm." A sojourn by the sea-side, however enabled him to push his studies still further, and in the present memoir he gives us the minute anatomy of several species of Polychete worms, together with a discussion of the question of the reciprocal affinities of the larger groups. Many structures could only be investigated in the living Annelids, others again were better seen in alcoholic specimens. The process of cutting the sections and staining them is fully described, and more than two thousand sections were made and mounted, the immense majority of the illustrations being drawn from actual sections. The instances where a drawing has been made up from a comparison of several sections is quite rare. The original drawings were of a large size, and were afterwards reduced by the pantograph. The minute structure of the Annelids is described under the following heads:—The cuticle; the hypoderm or cutaneous connective tissue; the muscular layers; the Setæ; the perivesceral cavity; the circulatory system (one of the most interesting and critical chapters in this work); the digestive system; the respiratory system; the nervous system; and the segmentary organs. The explanation of the plates occupies forty-five pages, and forms a most important portion of the work. The species selected for illustration are—*Spirographis spallanzanii*, *Myxicola infundibulum*, *Protula intestinum*, *Owenia* *fusiformis*, *Terebella flexuosa*, *Stylarioides moniliferus*, *Audouinia filigera*, *Chaetopterus variopedatus*, *Aricia fœtida*, *Telepsavus costarum*, *Branchiomma vesiculosum*, and *Nerine cirratulus*.

Death of Albany Hancock, F.R.S.—There has hardly been a naturalist in these countries whose labours both in zoology and geology have been so vast and numerous, and withal have been carried out with such excessive modesty, as those of Mr. Albany Hancock, who passed away from among us on October 24th last, at the age of about sixty-eight years. To cite a list of his works would be merely vain. Everyone who has even glanced at zoology must be familiar with them, and the learned know well how accurate and painstaking they were. His life, says the "Geological Magazine," "was, in the ordinary sense of the term, singularly uneventful. Scarcely ever did he leave his birthplace, which, with the dales and fells in its neighbourhood, he loved as North-countrymen can—*never* did he forsake his pure naturalist's work. Each year of his manhood was marked by the discovery, accurate observation, and ever modest publication of new and important facts in biology. His work speaks for itself; but the spirit in which he worked, his intense love of Nature for her own sake, his unaffected shrinking from honours which were forced upon him, his readiness to impart his knowledge or to give all help to the humblest beginner who was willing to work, his life-long friendships—all these must not pass away unrecorded. They cannot pass away unremembered by any one who knew him."

Substances which Prevent the Development of Protoplasmic Life.—M. A. Béchamp has presented a paper on this subject to the French Academy ("Comptes Rendus," September 8th). He says that there are at least three distinct albumenoid matters in the white of eggs; in the yolk, besides the microzymas, insoluble in water, there are two bodies soluble in that liquid. He maintains that albumen, gelatin, infusion of yeast, with or without sugar, may be preserved easily in free contact with air. Urine and blood are easily preserved by creosote or phenol. Blood is one of the liquids where bacteria appear the least readily.

The Development of Polyps.—This has been investigated for some years, but it has never been completely made out till M. Lacaze-Duthiers has recently studied the subject most carefully in the course of a series of experimental researches which he carried out on board the "Narval," a French ship, which was sent by the Government for the purpose of making soundings, &c. along the African coast of the Mediterranean. These researches, which have been most minutely carried out, explain the whole matter without doubt, and have been published to the French Academy by the author on November 24th last. M. Lacaze-Duthiers seems surprised that English naturalists should have considered the Mediterranean a rather barren sea; for, according to his investigations, it is full of marine life. The memoir is worthy of perusal, for from it alone can be gathered the author's ideas. He considers that the observations hitherto made have failed because they were not carried out sufficiently early; and, indeed, he shows that it is alone by watching the embryo step by step that anything can be done. With regard to the tentacles he says: "It is thus that the tentacles of Actiniæ that one finds disposed so regularly, sometimes in successive cycles of the type 6: 6 of the first, 6 of the second, 12 of the third, 24 of the fourth, 48 of the fifth size, are far from being developed 6 at the first epoch, 6 at the second, 12 at the third, and so on. The number 12 has been produced at first by passing successively by the inferior numbers 2, 4, 6, 8, and 12." These and several other facts have been proved, says the author, with "an absolute certainty," for he has had the young polyp from the time when it was swimming about as an embryonic globe without any divisions whatever.

Death of Professor Louis John Rodolph Agassiz.—It is with the deepest regret that we have to announce the death of Professor Agassiz, the news of which has just reached us (December 15th). He was a Swiss by birth and education, and much of his great work was done in his European home. It was not till 1846 that he left Europe for America, where he has been almost ever since. He was assuredly one of the few leading men in the science of Zoology, although he was a believer in several distinct species of men, and a disbeliever in Darwin's grand theory of evolution. Of his various works it is unnecessary to speak, for, with the exception of his "Tour of Lake Superior" and "A Journey in Brazil," almost all his great efforts with the pen have been made in Europe. He was born May 28th, 1807, in the parish of Mottier, between the Lakes of Neufchâtel and Morat. In the autumn of 1871 he joined an exploring expedition to the South Atlantic and Pacific shores of the Continent, which was his last expedition. A careful exploration was made of the celebrated Sargasso-sea, and a nest-building fish was discovered in that vast bed of oceanic vegetation; and other important contributions were made to natural science. Agassiz received fewer distinctions from European Societies and Universities than many less distinguished men of science. The Academy of Sciences at Paris awarded him their prize, however, and offered him a scientific professorship, which he declined. He also received the Cross of the Legion of Honour. His natural simplicity of character made him very generally beloved; and one of Longfellow's poems describes him, amid all the knowledge he had gained of Nature, still at heart a child.



W. West & C° lith

Encounter with a Gigantic Cuttlefish (Loligo Bouyeri) by the French Corvette Alecton. Nov.r 30. 1861

GIGANTIC CUTTLE-FISH.

By W. SAVILLE KENT, F.L.S., F.Z.S.,

SOMETIME ASSISTANT IN THE NATURAL HISTORY DEPARTMENT OF THE BRITISH MUSEUM, AND LATE SUPERINTENDING NATURALIST TO THE BRIGHTON AQUARIUM.

[PLATE CVII.]

'Εξ ἁλὸς οἷά τε πολλὰ τρέφει κλυτὸς 'Αμφιτρίτη.

THAT the solid path of facts occasionally yields data for our consideration, equalling or even surpassing the most vivid creations of fiction, is scarcely anywhere so capable of demonstration as within the realms of Natural Science.

As an illustration of the above, we may with confidence assert, that no kingdom in fairyland was ever invested with such a wealth of form and beauty, as has of late years been made manifest to us through the revelations of the compound microscope. No gem-laden forest of the "Arabian Nights" ever grew such wondrous living crystal trees as flourish, unperceived by the unaided eye, in the smallest pool of brackish water. No fairy banquet-table was ever bedecked with ornaments of so chaste and rare design as might be borrowed from the exhaustless store of exquisitely carved vases, sheaths, or bucklers, which shelter from extinction the slender spark of mystic life as it exists in Nature's humblest forms.

Ascending rapidly from the microscopic to the prodigious, we find the same doctrine equally applicable, though nowhere, perhaps, with such accumulated force as among that group of beings which supplies the title of this contribution.

The position of the Cuttle-fish in the scale of "Nature," and its general structure as a type of the order to which it belongs, have already been so admirably treated upon in the pages of this serial,* that a recapitulation of such details would be superfluous. It is sufficient for our present purposes to state

* See article on "The Cuttle-fish." By St. George Mivart.—POPULAR SCIENCE REVIEW, vol. viii., 1869.

that the Cuttle-fish, in association with the Calamaries or Squids, are referred by naturalists to that division of the Cephalopodous (*head-footed*) Mollusca termed the Decapoda, all its representatives having ten arms wherewith to grapple for and seize its prey. Two out of these are specially modified, much longer than the remaining eight, and technically termed the tentacula, in contradistinction to the shorter and more numerous appendages which are recognised as the ordinary arms. The Calamaries or Squids, Teuthidæ, are further distinguished from the ordinary Cuttle-fish, Sepiadæ, by the greater length of their body, and more perfect adaptation, through the possession of larger fins, for an essentially pelagic life. Another important anatomical characteristic of this group is that the broad, internal, calcareous cuttle-bone, or *sepiostaire*, is here replaced by a narrow, horny, more or less transparent, "pen," or *gladius*, which, in combination with the largely developed ink-bag of these creatures, has secured for them the popular and not inappropriate title of "Pen-and-Ink" fish.

From time immemorial tradition has assigned to certain members of this Calamary tribe proportions so far exceeding those of any authenticated representative of the Invertebrata, or indeed, with the exception of the whales, of the whole animal kingdom, that little or no credence in their existence has been placed by modern men of science. Yet, notwithstanding the large extent to which fact and fiction have undoubtedly been blended together in the earlier accounts handed down to us, the events of the last few months have supplied some most important links to a hitherto very disconnected chain of evidence; these links establishing in a remarkable manner, and beyond question, the existence of Cephalopods belonging to this group of the most formidable dimensions. Before entering, however, upon the subject of this latest evidence, we propose to give a brief review of the position in which we previously stood in respect to our knowledge of these ocean monsters.

Aristotle, Pliny, Ælian, Strabo, and other ancient writers, are unanimous in assigning to the Mediterranean waters the presence of gigantic Cephalopods; and although in many instances their histories are shrouded in a tissue of fable and exaggeration, recent discovery compels us to admit that most, in all probability, had some foundation in the solid ground of facts. Pliny, in particular, relates a narrative of one of these creatures destroyed on the coast of Spain by Trebius Niger, a lieutenant acting in that country for L. Lucullus. The head of this monster, which was preserved and shown to Lucullus, was of the size of a cask, and one of the arms is stated to have measured thirty feet. Two of these arms, the last-named being doubtless one, are recorded as of much greater length than the others, and this incontestably indicates that the creature belonged to the division of the Calamaries. The suckers of this specimen are described by Pliny as resembling small basins, and no mention being made of claws or hooks in association with them, it may be further inferred that this terrible monster was more or less closely allied to *Loligo* and *Ommastrephes*. The manner in which it is said to have driven away the dogs by the terror of its breath, and to have used its formidable arms as clubs against its aggressors, before it was finally overpowered and despatched by spears and tridents, with other extraordinary attributes assigned to the animal, necessarily bear on their face the stamp of exaggeration.

From the Roman period until the commencement of the seventeenth century and the spreading of learning and civilisation to the northern and western states of Europe, no record of the existence of these mighty ocean monsters appears to have been preserved; but from this latter date up to the present time, we find much fragmentary and disconnected evidence for consideration. The deep fiords and rock-bound coast-line of Scandinavia appear to have been a more than ordinarily favoured resort of many varieties of large Cephalopoda, and hence it is that among the Scandinavian legends and traditions we find associated the wildest fabrications extant concerning the habits and proportions of these animals. In the year 1639 we have an authenticated record of a gigantic cephalopod captured on the coast of Zeeland, and of another of colossal size stranded on the rocks in the Gulf of Ulwangen, in 1680. This last example, which is authenticated by Früs, appears to have been a large Poulpe, or Octopus, rather than a ten-armed Calamary, and probably one similar to this supplied the material out of which that most terrible and prodigious of monsters, the "Kraken," was produced a century earlier by Olaus Magnus, Archbishop of Upsala, followed by Pontoppidan, Bishop of Bergen, in 1754. This stupendous production of human imagination was described as resembling an ordinary Octopus, but of such gigantic size that a whole regiment of soldiers was reported to have been able to execute manœuvres on its back. The title of the largest of living animals, "*das grosste Thier in der Welt*," was given to the Kraken by Pontoppidan, who further relates of this formidable monster, "The Norwegian fishermen sometimes find unexpected shallows when a short distance out at sea, the depth suddenly diminishing from one hundred fathoms to twenty or thirty. Then they know that the Kraken is rising, and immediately retreat. His back first appears, looking like a number of small islands; his arms rise above the surface like the masts of a vessel, and are said to have power to grasp the largest man-of-war and pull it to the bottom."

With similar unscrupulousness, Pontoppidan first propagated the story of the Sea-serpent and many other marine monstrosities, supporting his descriptions by illustrations certainly derived from his imagination. O. Magnus writes of the Kraken in corresponding terms, "*similiorem insulæ quam bestiæ.*" Later on the name of Denis de Montfort appears upon the scene, and he, by fully endorsing, with greater exaggerations, the fables of Pontoppidan and O. Magnus, only rendered previous confusion worse confounded, and hid behind a yet more impenetrable veil of mystery that line of demarkation between truth and fiction which doubtless existed at the outset. The adoption of these fables by De Montfort was the more pernicious in its effects, on account of the reputation he had acquired as a naturalist, and the measure of confidence with which the creations of his inventive imagination were consequently accepted. Even Linnæus was deceived by the descriptions of the Kraken, and gives it a place under the title of *Sepia microcosmos* in both his "Fauna of Sweden" and the first edition of his "Systema Naturæ." A fac-simile of Denis de Montfort's most sensational representation of this creature, in which the monster is portrayed as overpowering and dragging to destruction a three-masted vessel, will be found in Mr. J. Gwyn Jeffreys' excellent "Manual of British Conchology," vol. v., p. 148.

Emerging from the cloud of doubt and mystery which enshrouds the history of the Kraken, we now approach the firmer standing-ground of modern record and investigation.

Pernetty, in his "History of a Voyage to the Malouines Islands," made in the years 1763-4, speaks of gigantic cephalopods inhabiting the southern seas and known to the sailors in those parts under the title of the "Cornet." Molina, in his "Natural History of Chili," 1789, supports the statements of Pernetty, and remarks of one species inhabiting the Chilian Seas, *Sepia tunicata*, that it weighs at least 150 lbs.

Peron relates an account of a large Calamary observed floating in the neighbourhood of Van Diemen's Land (see his "Voyage to Southern Lands," 1824). The body of this monster is described as resembling a cask, and its large arms to have presented the appearance of enormous snakes writhing upon the surface of the water. The length of these arms is given as between 6 and 7 ft., with a diameter of 7 or 8 ins. This creature was probably a large Poulpe or Octopus.

Quoy and Gaimard furnish us, in their "Voyage of the Urania," in the same year, 1824, with additional valuable evidence relative to this subject; having encountered in the Atlantic, near the Equator, the débris of an enormous specimen, the greater portion of which, including the tentacles, had already been devoured by the sharks and sea-birds; that remaining was estimated to weigh 100 lbs., and a large portion was secured and deposited in the gallery of comparative anatomy of the Paris Museum.

A species of the uncinated Calamaries (tentacles provided with hooks as well as suckers), *Enoploteuthis unguiculata*, the mutilated body of which was found floating in the South Seas by Banks and Solander, during Cook's second voyage, was forwarded to the Museum of the Royal College of Surgeons, London, and there examined by Prof. Owen; it was estimated by him to have been fully 6 ft. long when perfect. The natives of the Polynesian Isles, who dive for shell-fish, are stated to have a well-founded dread of this formidable species.

The largest Calamary of which a complete figure and description has been published, occurs in De Ferussac and D'Orbigny's magnificent monograph of the Acetabulated Cephalopoda, 1834, and is named *Ommastrephes giganteus*. The total length of this animal is given as rather less than 4 ft.

In 1841 a Colonel Smith communicated to the second meeting of the British Association, held at Plymouth, a description of several fragments of a gigantic Cuttle-fish preserved in the Museum of Haarlem.

At the reunion of Scandinavian naturalists at Copenhagen, in the year 1847, Professor Steenstrup contributed a record of two gigantic cephalopods, captured in the years 1639 and 1790, on the coasts of Zeeland, the first of which has been already alluded to; and later on, in 1856, supplemented these observations with some remarks of high interest upon another specimen found at the Skag, Jutland, in the year 1854. This last example was cut to pieces by the fishermen to bait their lines, the dismembered body furnishing many barrow-loads, or in its entire condition, according to some writers, filling a large cart. The pharynx of this monster, which with its contained beak, equalled in size an infant's head, was unfortunately the only portion of the animal preserved. This, however, with the record of the two examples from the neighbouring island of Zeeland just mentioned, supplied Professor Steenstrup with material for the institution of his new genus *Architeuthis*; he, according to Crosse and Fischer, bestowing upon the species of which the pharynx was preserved the title of *Architeuthis dux*, and upon that represented by the two examples recorded as captured in the years 1639 and 1790, the provisional one of *Architeuthis monachus*.

M. P. Harting publishes, in the "Memoirs of the Royal Academy of Amersterdam for 1860," a description with figures of various portions of some very large cephalopods contained in the museum of that town; the beak of one of these, measuring nearly five inches in length, including the muscular socket, he considers to belong to a species identical with Professor Steenstrup's *Architeuthis dux.*

The mutilated carcass of a huge Cuttle-fish was stranded in 1860 or 1861, between Hillswick and Scalloway on the west coast of Scotland. The mantle-sac measured 7 ft. long, the tentacles 16 ft., and the ordinary arms half that length; one of the suckers examined by Professor Allman was three quarters of an inch in diameter.*

In December 1861, M. Bouyer, commandant of the French corvette *Alecton*, and M. Sabin Berthelot, French Consul at the Canary Islands, communicated to the Paris Academy of Sciences, through M. Vaillant, the description of a gigantic Calamary, encountered by that vessel between the islands of Madeira and Teneriffe. This monster was found floating at the surface of the water about mid-day, November 30, of the same year, and the vessel being stopped, immediate steps were taken to effect its capture. A volley of bullets which was first discharged into it caused the animal to plunge beneath the ship; appearing shortly after on the other side, it was attacked with both harpoons and fire-arms, neither of which, however, appeared to make much impression on its soft yielding flesh. After diving beneath the surface and reappearing several times, one ball struck it with marked effect, the creature immediately discharging a quantity of foam mixed with blood, and at the same time a strong musky odour made itself perceptible to all on board; the sailors were most anxious to lower the boats and carry on the attack at closer quarters, but Captain Bouyer forbade this, fearing the creature's powerful arms might seize and capsize them. At this point a noose was successfully cast over the animal's body, but owing to the smoothness of the latter, failed to tighten upon it until it arrived at the posterior extremity, just where the broad expanding fins took their origin. Efforts were now made to hoist the monster upon deck, and the greater portion of its body was already out of the water, when the enormous weight caused the rope to cut the animal completely through; the posterior part with the fins was brought on board, but the remainder, with the head and arms, disappeared beneath the waves, and was not seen again. Altogether the chase after this monster lasted more than three hours; the duration of the adventure giving time for one of the officers on board, M. Rodolphe, to make a hasty sketch of the scene, which was submitted to the members of the Academy with MM. Bouyer and Berthelot's account; a fac-simile of the same is reproduced to accompany this article. One circumstance of note in association with M. Rodolphe's drawing is the non-representation of

* Gwyn Jeffreys' "British Conchology," vol. v., p. 124.

the two longer tentacula. It is supposed that the animal may have previously lost these in a conflict with some other monster of the deep; it is equally probable, however, that they hung down perpendicularly in the water, and were thus lost to sight, a position which these organs invariably assume in the smaller members of the group when exhausted or approaching dissolution. The length of this gigantic Calamary was estimated by the numerous witnesses of the engagement to be about 30 ft., of which between 18 and 20 belonged to the body only. MM. Crosse and Fischer, the eminent French conchologists, were so satisfied with the accuracy of M. Bouyer's account and the drawing that accompanied it, that they have since republished the former in their "Journal de Conchyliologie," for 1862, bestowing upon the animal the name of *Loligo Bouyeri*, in honour of the spirited commander of the vessel. As unfortunately no portion of this specimen was brought home, it is impossible to ascertain whether it was identical with either of the species of *Architeuthis* previously instituted by Professor Steenstrup, or with the Newfoundland forms that yet await our notice. Notwithstanding this serious drawback, the contribution to our knowledge of these gigantic Cuttle-fish just cited must be regarded as one of the most complete we have yet enumerated. To the dimensions of the animal already given, it may be added that the colour of the creature was brick red, its long fusiform body was remarkably thick towards the centre, the eyes of enormous size, fixed, and ghastly to behold, and the fins at the posterior extremity rounded, fleshy, and very large. Louis Figuier, in his "Ocean World," rather brings discredit upon the above account by exaggerating the proportions of this Calamary in both his text and illustration.

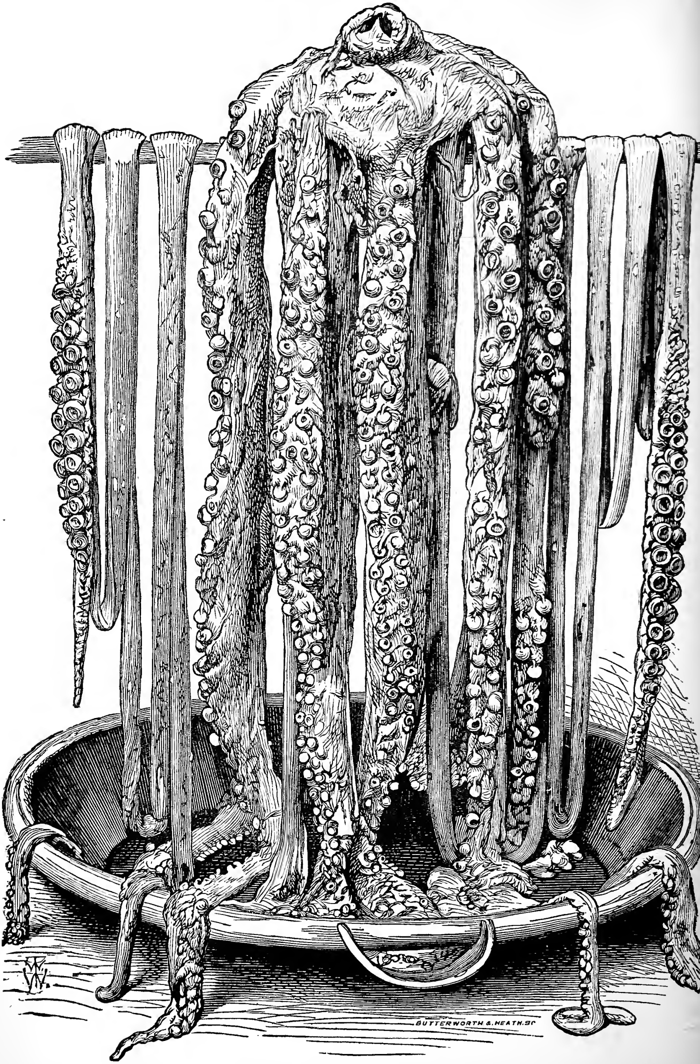
We now arrive at that portion of our evidence which may be regarded as of a more gratifying nature than any yet brought forward; since, by a record of the actual capture and preservation of the animals themselves, or the most important portions of them, it places us in a position to positively define the degree of affinity existing between certain of these monsters and the numerous smaller representatives of the group with which we were previously acquainted.

Within the last few months, as is, perhaps, already more or less generally known, a considerable sensation has been caused throughout the scientific world by the report from Newfoundland of a gigantic cephalopod, lately encountered by two fishermen in the neighbourhood of Conception Bay, a tentacle of the same 19 feet long being secured for the St. John's Museum. The story of this adventure, as told by the Rev. Mr. Harvey, to whom we are also indebted for the steps taken to preserve this remarkable tentacle, may be briefly stated as follows:—On October 26, 1873, while two fishermen from St. John's were plying their vocation off the eastern end of Great Belle Island, Conception Bay, they descried, at a short distance from them, a dark shapeless mass floating in the water. Concluding that it was a bale of goods, possibly a portion of the cargo of some wrecked vessel, the men rowed up to it, anticipating a valuable prize, and one of them struck the object with his boat-hook. In an instant the dark mass became animated, and opening out like a huge umbrella, displayed to view a pair of prominent ghastly green eyes of enormous size, which glared at them with apparent ferocity, its huge parrot-like beak at the same time opening in a savage and threatening manner. The men were so terrified by the terrible apparition that for a moment they were unable to stir, and before they could recover their presence of mind sufficiently to endeavour to make their escape, the monster, now but a few feet from the boat, shot out from around it several long arms of corpse-like fleshiness, and grappling for the boat, sought to envelop it in their livid folds. Two of these reached the craft, and in consequence of its greater length, one went completely over and beyond it. At this moment, one of the men, by name Theophilus Picot, fortunately recovered from his fright, and seizing a hatchet that happened to be on board, succeeded by a desperate effort in severing both these arms. On finding itself wounded, the animal moved off backwards, at the same time darkening the water with its inky emissions, and presently became lost to sight beneath the surface of the waves. The amputated arms which were left in the boat as trophies of the terrible encounter, were brought to St. John's, and, through the energy of Mr. Harvey, the longer one was secured for the museum, the shorter of the two having, unfortunately, been destroyed before its value was known. The same gentleman, who was the first to examine and describe this severed limb, found that it measured no less than 19 ft.; a large portion, some 6 ft., had been destroyed before Mr. Harvey rescued it, and the fishermen being of opinion that at least 10 more were left attached to the monster's body, the total length must have been very little short of 40 ft. The description of this limb by the Rev. Mr. Harvey entirely bears out his own opinion that it was one of the two longer arms or tentacula of a huge Calamary, and is thus given:—"It measured 19 ft., is of a pale pink colour, entirely cartilaginous, tough and pliant as leather, and very strong. It is but 3½ in. in circumference, except towards the extremity, where it broadens like an oar to 6 in., and then tapers to a pretty fine point; the under surface of the extremity is covered with suckers to the very point. At the extreme end there is a cluster of small suckers, with fine sharp teeth round their edges, and having a membrane stretched across each; of these there are about seventy. Then come two rows of very large suckers, the moveable disk of each 1¼ in. in diameter, the cartilaginous ring not being denticulated; these are twenty-four in number. After these there is another group of suckers with denticulated edges, similar to the first, and about fifty in number. Along the under surface about forty more small suckers are distributed at intervals, making in all about 180 suckers on the arm."

Respecting the dimensions of the body of this enormous creature the fishermen appear to have been too terrified at the time of the encounter to form a correct estimate. According to their statement it measured no less than 60 ft. in length; but, as lately observed by Professor Verrill, this was probably intended for the entire dimensions, including the arms, leaving about 20 ft. for the body proper. The portion of the ordinary arm that was severed at the same time, but afterwards destroyed, is said to have been 6 ft. long and 10 in. in diameter.

Scarcely had the news of this remarkable adventure and acquisition reached this country, than intelligence was received through the same source, from the Rev. Mr. Harvey, that a second gigantic Cuttle-fish, little inferior in size to the one just described, had been taken in a herring-net by three fishermen, in Logie Bay, some three miles from St. John's. The formidable arms having become entangled in the folds of the net, the creature's power of resistance was almost completely annihilated; it nevertheless required the efforts of all the men to finally overcome it, and it was not before they had succeeded in severing the monster's head from its body that they were able to take possession of their prize. The entire body of this Cuttle-fish was brought to St. John's, and photographs were made of it and steps taken to preserve the whole. An excellent reproduction of the best photograph taken, representing the anterior portion with the beak and all the arms, appeared with Mr. Harvey's account of the same in the "Field" for January 31.* The body of this specimen measures 8 ft. in length, with a girth of 5 ft., the two longest tentacles 24 ft., and the eight shorter arms each 6 ft. in length. The formidable horny, parrot-like beak is the size of a man's fist, and the membranous sockets of the prominent eyes gave a diameter of 4 inches. The description and figures published of this example leave no doubt that it is a smaller individual of the same species of which a single tentacle only was secured a

* Through the kind courtesy of the Proprietors of the "Field," we are enabled to present our readers with an engraving of the same on p. 122.



HEAD, WITH ARMS AND TENTACLES, OF GIGANTIC CALAMARY (*Megaloteuthis Harveyi*, S. Kent), from Logie Bay, Newfoundland.

few weeks previously; it, however, far surpasses the larger one in importance, since through it, we are for the first time placed in full possession of the entire structure of one of these ocean monsters.

Additional evidence is supplied by Mr. Harvey of other gigantic Cuttle-fish previously seen on the coast of Newfoundland: two in particular measuring respectively 40 ft. and 47 ft., having, as stated by the Rev. Mr. Gabriel, been cast ashore in the winter of 1870-71, near Lamaline, on the south coast of the island. Reliable evidence is also given of another monster stranded two years later, which in total length measured no less than 80 ft.

Professor A. E. Verrill, in a very interesting contribution to "Silliman's American Journal," reproduced in the "Annals and Magazine of Natural History" for March last, enumerates, with the foregoing, authenticated testimony of three other similar monsters from the Newfoundland coast. One of these was found floating (apparently dead) near the Grand Banks, in October 1871, by Captain Campbell, of the schooner *B. D. Haskins*, of Gloucester, Mass. It was taken on board, and part of it used for bait. The body of this specimen is stated to have been 15 feet long; and the portions of the arms remaining, which were much mutilated, were estimated at a length of from between 9 and 10 feet; the beak was forwarded to the Smithsonian Institution, and is now in the hands of Professor Verrill, for the purpose of description and illustration. Professor Steenstrup, who has seen a photograph of this beak, thinks it may possibly belong to the still very imperfectly known form which inhabits the northern coasts of Europe, and which, as stated at p. 117, he has proposed to distinguish as *Architeuthis monachus*.

Another specimen is quoted by Professor Verrill as having been found alive in shallow water at Coombes Cove. The body of this example is stated to have measured 10 ft. in length, the arms 6 ft., and one of the tentacles as much as 42 ft. In addition to this, he mentions a pair of jaws and two suckers, deposited in the Smithsonian Institution, received from the Rev. A. Munn, who states that they were taken from a specimen that came ashore in Bonavista Bay, and which measured in entire length 32 feet. This example, together with the one last named, and the species represented by the long tentacle and perfect specimen secured for the St. John's Museum, Professor Verrill considers may be identical with Steenstrup's *Architeuthis dux*.

Previous to the appearance in this country of Professor Verrill's interesting remarks on these Newfoundland specimens, a communication on the same subject was sent by the author of this article to the Zoological Society of London, and in which was adduced further evidence of the existence of these monsters, by the description of an arm no less than 9 ft. long, preserved in the vaults of the British Museum, but of which unfortunately no authenticated record has been preserved. Reference to this gigantic limb was made by the same author in the "Guide Book to the Brighton Aquarium," published in June 1873,* the true position among the Calamaries, and the probable total length of the perfect animal, being then anticipated. These latter dimensions were estimated at close upon 40 ft., and the events of the past few months show that this calculation was rather under than over-rated. In the perfect specimen captured in Logie Bay the comparative proportions of the arms to the two larger tentacula is just one to four, the former measuring 6 and the latter 24 ft.; the arm at the British Museum being clothed with suckers throughout its length proves it to be one of the eight shorter arms, so that the two tentacula in the same proportion would have reached to a length of 36 ft., and which, joined to the 15 or 20 additional feet, which were no doubt represented by the body of the animal, we arrive at a length of little less than 60 ft., or in fact the exact proportions given by the fishermen of the monster first encountered in Conception Bay. This remarkable arm in our national Museum some years since enjoyed a niche in the public galleries, and was on account of that circumstance alone remembered by the author, it having struck him, on his first visit to that overflowing storehouse of treasures, as one of the most extraordinary examples in the whole collection. As such it produced an indelible impression, and the instructive and interesting nature of this specimen will, it is to be hoped, once more secure its return from its present place of retirement to a position where it may now meet with wider appreciation. The suckers in this arm are much more thickly set than in either of the Newfoundland examples already described; there being a double row from one extremity to the other, and each row containing from 145 to 150, or a total of just 300 suckers to the whole arm. In the example from Logie Bay, one arm 6 ft. long is estimated to have only about 100 of these suckers; this supports an opinion already expressed that, if of the same species, the arm in the British Museum was considerably longer before it was subjected to the contracting influences of the spirit in which it is now immersed. At the same time this evidence of the British Museum specimen was submitted to the Zoological Society's notice, it was proposed, in the event of it not being possible to identify the St. John's specimens with

* Also in edition since issued. with the Author's name excised.

Professor Steenstrup's *Architeuthis*, to institute for them the generic title of *Megaloteuthis*, or Giant Calamary, and to further distinguish them, both being of the same species, as *Megaloteuthis Harveyi*, in recognition of the great service to science rendered through Mr. Harvey's steps taken to preserve these valuable specimens. To this gentleman, indeed, we are indebted for the means of solving and setting at rest for ever the long-vexed problem of the existence even of these formidable monsters of the deep.

The accounts published of these colossal Cuttle-fish from Newfoundland have had the effect of eliminating much additional information concerning these formidable molluscs and their allies. A correspondent, "J. M. M.," to "Appleton's American Journal of Science and Art" for January 31, 1874, relates that a monster of the eight-armed order, a Poulpe or Octopus, once seized hold of a submarine diver, while at work in the wreck of a sunken steamer, off the coast of Florida. The man, a powerful Irishman, was quite paralysed in its grasp, and, to use his own language, felt both his armour and himself "being cracked into a jelly." It seems he was just being brought to the surface, otherwise the creature would have killed him, for he was so suffering from the terrible embrace that he could move no part of himself; when dragged into the raft from which he had descended, and finally released, he fainted away. The men on the raft seized the animal by one of its arms and tried to pull it off, but were unable to overcome the adhesive power of a single sucker. They at last succeeded in removing the monster by dealing it a heavy blow across the sac which contains the stomach. This sac was further described as standing up stiffly above the eyes, which projected like lobster's eyes and gleamed like fire, altogether a frightful apparition to encounter. The writer adds, that the Italian fishermen of San Francisco who frequent the Farallone Islands, not unfrequently take these devil-fish, from 8 to 10 ft. across their extended tentacles.

Returning to the true Cuttles or Calamaries, a huge monster, evidently of this tribe, not long since seized a fishing-boat, near the village of Kononoti, Japan (see "Nature," June 5, 1873). The boatmen at length succeeded in despatching it by repeated blows. The creature's body, which was exhibited in a house near the temple at Asaka, measured 16 ft., and the arms nearly 5 ft.

Having satisfactorily established the existence of these monarchs of the ocean, it now remains for science to elucidate their habits and social economy. The facilities for this, however, are at present not very extensive. The fact of many individuals having made their appearance within a limited area, and at intervals following with marked closeness upon one another, would seem to indicate that they are gregarious, and in this respect participate in a habit dominant with the majority of their less pretentious congeners. In the common Squid, *Loligo*, the number of ova deposited by the female is something astounding, having been computed by Bohadsch at no less than 40,000! If anything approaching such a degree of prolificness obtains in these giants of their race, it is a matter of wonder that more has not been seen of them. Against this, however, it may be argued that, like the members of the whale tribe, they usually affect the open ocean, and only approach the land when driven by stress of wind or tide. Unlike the whales, a shoal of these might pass a ship unnoticed, having no occasion to raise the surface of their bodies above the level of the water to take in atmospheric air, as with the former. The great speed, moreover, with which all members of this group shoot through the medium they inhabit, aided by their powerful vision, would enable them to avoid with ease a threatened danger in the form of an approaching ship. Associated with this aspect, it is also worthy of note that in the majority of instances the examples encountered have been dead, or in a mutilated or exhausted state.

The question of the food upon which these monsters live has not yet been determined; though it in all probability consists principally of living fish, in common with most of their predacious race. The destruction they work among the finny tribes in this case must, on account of their enormous size, be very great; but, as throughout the scale of nature we find one tribe warring upon another and finally subservient for the support of one still more powerful, we shall find that these giant Calamaries are themselves an easy prey to other tenants of the deep. The whales, in fact, with which they have been compared in size, are their most formidable and implacable foes, and probably the only animals existing who could oppose these monsters with any prospect of success in their native element. Our remarks in this case are, of course, restricted to the toothed whales, or *Physeteridæ*, and with these we have abundant evidence to show that the colossal cephalopods constitute a favourite diet.

A mate of a whaling vessel stated to the writer of the communication to "Appleton's Journal," already quoted, that there were enormous Squids in the equatorial seas which furnished food to the sperm-whales, and that he had on one occasion seen an arm of one, 30 ft. long, depending from the mouth of a whale that seemed sick. Captain Francis Post, Captain E. E. Smith, and other witnesses, also supply testimony to the same effect; and one of these declared having seen a whale with an arm of a Cuttle-fish no less than 40 ft. in length. Among others, Mr.

Holdsworth has personally assured the author that he has himself witnessed this proclivity of the toothed whales, and additional testimony might be produced to the same purpose.

So large a mass of evidence has now been laid before our readers that to further augment it would be needless. Summing up the whole, we are forced to admit that this group of Cephalopodous molluscs contains representatives of enormous dimensions distributed in the seas throughout the globe, and embracing in all probability many distinct genera and species. Such is the formidable size of these giant Calamaries that they vie even with the Cetacea in magnitude, and in this respect yield to no other animals now existing. It further appears obvious that the numerous tales and traditions that have been current from the earliest times, concerning the existence of colossal species of this race, though in some instances unscrupulously exaggerated, had in all probability in the main a groundwork of fact, and can be no longer passed over as the mere fabrications of a disordered mind, as we have been hitherto inclined to accept them.

ON THE SPHYGMOGRAPH, OR PULSE-RECORDER.

By A. H. Garrod, B.A., Fellow of St. John's Coll., Camb.

It seems to be an impression with the public generally that the science of physiology is so intimately connected with medicine, that without a considerable amount of information on the latter subject, little or no progress can be made in the former. The same idea used to prevail with reference to chemistry, until, not many years ago, it gradually became apparent that it had an independent existence of its own, and might be studied by itself with much advantage. This current impression about physiology is no doubt a false one; and there is no more reason against any individual, with spare time at his disposal, devoting some of it to physiology, than that he should direct his attention to botany, ornithology, geology, or any of the subjects which are associated with them.

In the present paper it is my endeavour to explain the construction of an instrument, which has been known for a little over ten years, and which, from the comparatively little use that has been made of the facts which it discloses, has the advantage of leaving a large field for future work, open especially to those who are of a mathematical and mechanical turn of mind.

Until the time of the illustrious Harvey, the true action of the heart, and the use of the several large blood-vessels which are connected with it, were very incompletely understood. It was known that the heart pulsated, and sent blood out of its cavities into some of these vessels, and also that all the vessels were not alike; in fact, that there were two kinds of blood-vessels, those with thicker coats of a light yellow colour being called arteries, because they were supposed to contain air, and those with thinner and less yellow coats, called veins. It was also known that on cutting across some of these vessels, as in a deep wound, blood spirted in fountain-like jets; whilst in opening the veins of the surface of the arm the blood flowed in a slower and much more uniform stream. It was Harvey who showed, from the manner in which the valves of the heart were disposed, that the blood could flow through it only in one direction; and from the arrangement of the valves in the veins, proved that there was only one course for the blood in them also, from which he was led to enunciate the great problem of the existence of a circulation of the blood from the heart, through the arteries and into the veins, before it was proved that there were any direct means of communication between these two latter. When the microscope came into use, the truth of there being a circulation of the blood was verified by the discovery of the capillary system, a network of vessels, far too minute to be seen by the unaided eye, which placed the arterial in direct connection with the venous system of blood-vessels.

The blood being therefore pumped by the heart into the arteries, is sent into them in jerks or intermittent streams, just in the same way that water leaves the spout of an ordinary pump that is not being worked quickly. The arteries, however, being elastic, yield to the pressure of each stroke, part of the force of which is therefore spent in distending them, part in sending the blood onward. There being an interval between each two beats, the force stored up in the distended elastic arteries during the contraction of the heart is employed in aiding the onward blood-stream during the interval in which it rests; and as a consequence of this the flow of the blood becomes less and less jerky in the arteries further and further from the heart, until in the capillaries, and therefore in the veins also, the stream is quite uniform.

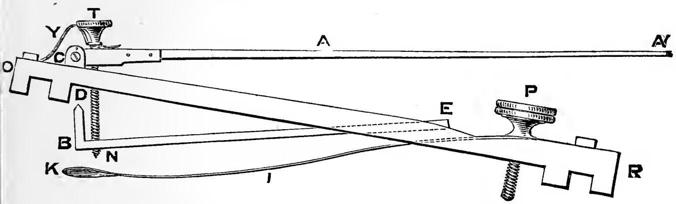
It is the above-mentioned jerkiness of the blood-flow in the arteries which produces the phenomenon known as the pulse; and as there are arteries of considerable size in most parts of the body, the pulse is to be felt wherever these vessels are sufficiently superficial. On either side of the wrist there is an artery which can be found with facility, that on the thumb side being the one most easily reached and most readily felt. Besides these, the carotid in the neck, the temporal not far above the outer end of the eyebrow, and the digital arteries along the sides of the fingers, are perhaps the most convenient for observation.

On placing the finger on one of these arteries, the sensation of an intermittent rise and fall is felt beneath it, which is termed the pulse, each rise closely following the pulsation against the chest-wall produced by the heart at the commencement of its contraction. In almost all cases nothing more is felt, but very attentive observation makes it evident that the impulse is abrupt, and that the following fall is slow. Some pulses appear to be much more ample than others, and differences in the rapidity of the heart's action are the most easy to be detected.

The smallness of the information that can thus be learnt from simply placing the finger on an artery has for some time past led physiologists to attempt to produce an instrument which will make the details of each pulse-beat manifest to some sense other than the touch. In elderly men with bald heads the anterior branch of the superficial temporal artery is seen to take a sinuous course towards the upper part of the forehead, and it is possible, without much difficulty, to *see* its pulsations. The beating of a small vessel which runs from the wrist to the ball of the thumb is often visible; and it is said that Galileo constructed the first sphygmoscope (σφυγμός, "the pulse," and σκοπέω, "I see"), by placing a small light mirror, with one edge resting on this artery and the other on the surface close by, in such a way that the sun's rays fell upon it, and threw the image of the moving beam on a wall some distance off.

There is, however, a great disadvantage in this method of observation, which is that the results arrived at are not permanent; so that experiments made at different times cannot be compared together as to their results. To obviate this imperfection Vierordt constructed a *sphygmograph* (σφυγμός, γράφω, "I write"), which by writing on a paper the movements that attended its application, rendered comparison easy. The instrument he employed consisted of a system of two parallel levers of the third order, linked together and made excessively sensitive by placing sand in counterpoise cups fixed on them. The tracings were obtained by making the tips of one of the levers scratch a smoked glass plate which moved past it slowly. They were found to be very unsatisfactory, as they formed nothing but simple up and down strokes of very similar length and general appearance, the reason being that the momentum developed by the weight of the levers, counterpoises, &c., entirely disguised the true movements of the artery, and made the trace completely valueless. M. Marey, of Paris, set to work to remedy this imperfection, making use of the important principle that a definite pressure can be applied on any given spot by a steel spring with the introduction of a much smaller weight of material than is necessary to produce the same pressure if no spring is employed. He made his lever extremely light; produced the compression of the artery, which is necessary before any movement can be imparted, by means of a strong spring, and employed a second small spring to counteract any tendency to oscillation that might occur.

Marey's sphygmograph, as originally constructed by Breguet, consists of a long brass frame, which supports both the lever together with its accessories, as well as the watchwork and paper on which the tracings are recorded. A strong steel spring, about four inches long, is attached to this framework near its middle, and is free at the other end, to which a small pad is attached for pressing on the artery. Attached to this pulse-spring is a piece of brass work which carries a knife-edge just over the pad, and this is easily raised or lowered by a long screw with a milled head. The knife-edge is in contact with the recording lever near its axis, having a small steel plate to



MECHANICAL ARRANGEMENT OF THE SPHYGMOGRAPH.

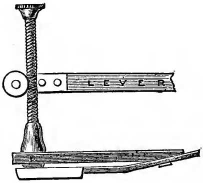
A A′, Recording Lever; B, Brass-work supporting the Knife-edge, D, and the Regulating Screw, T; K, Pulse-pad, at end of Pulse-spring, I; C, Arbor on which the Recording Lever rotates, close to which the Compressing-spring, Y, acts; P, Screw to regulate the strength of the Pulse-spring; G R, Main Framework of the Instrument.—(From "Handbook to the Physiological Laboratory.")

work upon. The lever is of the third kind, very light, and carrying a thin steel pen or scratcher at its tip. From this description it may be seen that any movement, such as that of the pulse, raising the pulse-pad, raises the lever also, and the reverse. There is a screw arrangement for varying the amount of pressure exerted by the spring on the artery.

The recording paper is fixed on a flat metal backing, which again is connected with a series of brass teeth, that catch in the wheel of the driving-wheel of the watch-work, running its whole length in ten seconds.

The whole instrument is bound on the arm by a silk cord, that is attached to pegs in the side-lappets, fixed on the body of the instrument.

Brequet now makes his sphygmograph on a slightly modified plan. Instead of the knife-edge and screw connecting the recording lever with the pulse-pad, he has a simple screw attached to the spring in the same way that the blade is fixed in a pocket-knife, so that it may be turned down flat or up at right angles. The worm of the screw bites in the teeth of a small wheel, which embraces the axis on which the recording lever works, and so makes the different parts of the instrument



BREQUET'S RACKWORK MODIFICATION OF THE SPHYGMOGRAPH.—(From "Handbook to the Physiological Laboratory.")

communicate with one another. Both this and the knife-edge instrument have advantages of their own; but it seems to me that in the long run the older one will be found the more generally applicable, and less likely to get out of order.

The recording paper should be highly glazed, like old-fashioned visiting-cards, to reduce the friction to the least possible amount; and further to do so, it is much better, instead of using a split pen (with two points necessarily) and ink, to smoke the paper and use a simple steel point as the pen. The paper is smoked by doubling the ends of the strip round the brass plate on which it is supported, and passing it over the flame of an ordinary composite candle until it is dark, but not black. After the tracing has been taken it should be thinly varnished, by pouring an ordinary photograph spirit varnish over it, in the same way that collodion is made to flow over a glass plate. The varnish darkens the smoke film considerably. If it is put on thick it is very apt to crack off when the paper is bent.

To take a trace, the screw which presses the pulse-spring should be raised so as to laxen it. Some slight mark should be made by beginners on the bare arm, where the pulse is felt to beat most strongly. The ivory pulse-pad should then be placed over the spot, care being taken that is not pressed to one side or the other; and the whole instrument strapped down. After this, the screw pressing the pulse-spring should be turned until the artery is considerably compressed, and the lever adjusted to beat in its proper place just opposite where the recording paper is to run. A fully doubled handkerchief, or something of about that thickness, should be placed between the arm and the part of the instrument on which the watchwork is fixed, to support it. The moving train, with the paper already smoked fixed on it, is then adjusted to its place, *after which*, the rate at which the pulse is beating is found by a watch in the ordinary way. All is then ready for the trace being taken.

As the watchwork drives the recording paper in ten seconds *when the instrument is level*, if such has been the case, the pulse-rate may be calculated indirectly; but to insure greater accuracy, it is best always to check the result by direct observation.

The next point for consideration is the value of the tracings taken with Marey's instrument; what more they teach than can be learnt by simply applying the finger on the pulse; and how the language of the instrument, as the interpretation of its minutiæ may be termed, is to be understood.

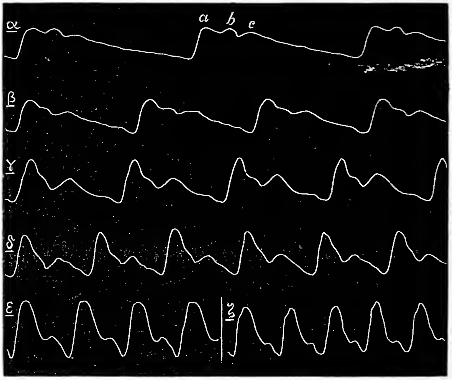
As the sphygmograph can only be applied to the arteries which supply the various tissues of the body, and not to those which run to the lungs—that is, to the systemic and not to the pulmonary arteries—it is not at all necessary to think of more than one side of the heart,—that is the left, when we attempt to explain the tracings obtained. For as the aorta springs from the left ventricle, and the left ventricle receives the blood which it pumps onwards, from the left auricle, it is the changes which occur during the beat in those two cavities which only have any appreciable effect on the pulse at the wrist.

The movements of the ventricles of the heart, it must also be remembered, consist of two separate series of events, one being the *systole*, which is the period during which it is in action, contracting and emptying itself of the blood it contains; the other being the *diastole*, which is the period during which it rests and becomes refilled with blood ready to pump out in the next systole. This alternation of systole and diastole may be compared with the down and up stroke that are made in the working of an ordinary pump; the up-stroke of the handle corresponding to the diastole, in which the cylinder above the piston is being filled with water; and the down-stroke to the systole, in which the water from the full cylinder is being raised, so as to flow from the spout.

Now to commence with the sphygmograph trace itself. The first rush of blood into the arteries at the onset of the systolic contraction produces a sudden increase in the diameter of the large arteries, which is propagated with considerable rapidity along the arterial system to the distant vessels. This produces the abrupt and nearly uniform rise (*a*) which almost always forms the most prominent and most considerable feature of the sphygmograph trace. This rise is, therefore, the indication of the commencement of the systole of the left ventricle, and it must be borne in mind that it terminates before the heart ceases to contract, especially in the larger arteries. The primary rise in all properly taken traces is continued on as a rounded apex, which is the highest point of the trace, and is followed by a fall of considerable rapidity as compared with the second descent to be described further on. This fall is also generally irregular, especially in slow pulses, being broken by a very small wave (*b*), termed the 'tidal wave,' by Mr. Mahomed, but perhaps better, the 'predicrotic wave,' as the latter name does not involve any theory, and will be better understood directly.

The dicrotic rise (*c*) is the feature in all tracings next most important to the primary ascent. It commences at the end of the fall just described; it is less abrupt and shorter than the main rise, and is more obtuse or rounded at its summit. It is followed by the second fall, which is much less vertical than the one which precedes it and more irregular, being frequently interrupted by two or three slight undulations. At the end of this fall the succeeding primary rise commences.

Different explanations have been given of the dicrotic rise in the sphygmograph trace, and some physiologists still think that its true cause has not yet been fully made out. The difficulty arises from the fact that at the same time that the action of the valves, especially the aortic, have to be considered, the many different direct and reflected undulations which are possible in distended elastic tubes must not be omitted from consideration. All evidence of any significance, however, goes to show that this dicrotic rise is simply the wave generated by the shock of closure of the aortic valve, transmitted along the arteries from the large to the smaller vessels, just in the same manner as in the primary undulation.



SPHYGMOGRAPH TRACINGS.

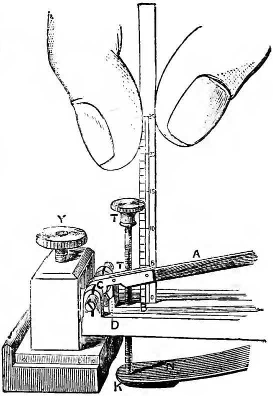
All taken from the same individual in health, under different conditions, to show the effect of difference in pulse-rate on the trace—

α 44 a minute	δ 103 a minute
β 63 "	ε 137 "
γ 72 "	ζ 172 "

In the top tracing *a* is the primary, *b* the predicrotic, and *c* the dicrotic rise.

The small predicrotic irregularity (*b*) above mentioned in the descent following the primary rise, originates from the same source as the dicrotic. My observations with an instrument which records simultaneously the movements of the heart and the pulse at the wrist (the cardio-sphygmograph) prove that it is simultaneous with the closure of the aortic valve at the heart; that it is a shock wave in fact, whilst the dicrotic wave is the more slowly travelling undulation of distension.

Now that we know some of the important features of the sphygmograph trace, the next point for consideration is how we are to make any use of our information. Dr. Burdon Sanderson has, by means of a simple apparatus attached to one end of the instrument, endeavoured to employ it as a hæmadynamometer or gauge, by means of which the pressure of the blood in the artery may be estimated. By adjusting the height of the pulse-pad, at the same time that the pressure exerted by the spring is estimated on a graduated scale, he finds the degree of pressure required to give indications in the trace of excessive compression of the artery against the bone, and so of the blood-pressure in the artery. There are several difficulties in the way, which make the results of comparatively little value, among which may be mentioned the differences in the elasticity of the skin of different individuals, and the uncertainty attending the situation in which the vessel is compressed.



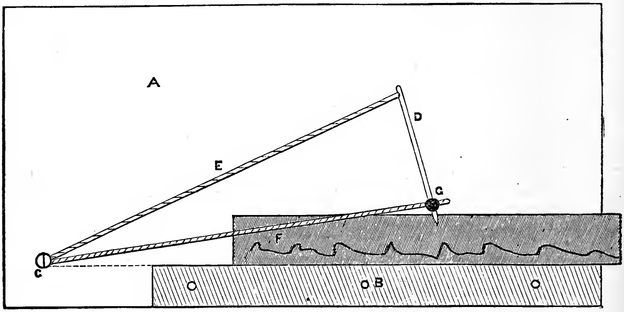
DR. SANDERSON'S METHOD FOR ESTIMATING THE BLOOD-PRESSURE BY THE SPHYGMOGRAPH.

Y, Screw for regulating the distance of the instrument from the arm.—(From "Handbook to the Physiological Laboratory.")

In looking at sphygmograph traces from the pulses of different rapidities, there is nothing more striking than that the length of the interval between the commencement of the primary and dicrotic rises varies very considerably in different tracings, being nearly constant in each. In most works on Physiology it is stated that there is a definite ratio between the length of systole and that of diastole. The subjoined table from "Carpenter's Principles of Human Physiology" will serve as an example:—

Auricles.		Ventricles.	
$\frac{7}{8}$ Dilatation	. .	Contraction $\frac{1}{2}$.	
$\frac{1}{8}$ Contraction .	.	First stage of Dilatation	$\frac{1}{2}$
		Second " "	

No hint is ever given to show that these relations of the several parts of the heart-beat may vary, because before the introduction of the sphygmograph the means of carefully estimating them were not at hand. But as in the pulse trace it is possible, and not in the least difficult, to observe both the length of the systole and the diastole, every opportunity is now afforded for their comparison. This must be done by accurate measurement, than which nothing can be easier. There is, however, one precaution that must be taken, and this is rendered necessary from the fact that the pen of the sphygmograph lever moves in part of a circle, a large one no doubt, and not in a straight line. Consequently, if the commencement of the primary and secondary rises in each beat are at a different level, which is generally the case, it is impossible to judge of the length of the systole, which, as far as it affects the pulse, is the interval between these points, without projecting them on the same horizontal line. If this were done by simply drawing vertical lines on the tracing and then making the measurements, imperfect results would be obtained; so it is necessary to make the lines curved, just the same as if the lever of the instrument had drawn them whilst the watchwork was not moving. To draw these lines with the tracing on the sphygmograph itself, with its own lever, is difficult, and wears up the watchwork unnecessarily. It is much better, after having varnished the trace, to scratch the lines on it with a needle, which is so fastened to strings of the same length as the lever that it can produce curves of exactly the same radius. The following sketch gives an idea of the apparatus I am



THE CURVE-CONSTRUCTING INSTRUMENT FOR SPHYGMOGRAPH TRACINGS.
(Half the described size.)

accustomed to employ. It is extremely simple, and requires no skill in manufacture. A thin slab of wood (A), 8 inches by 4, has another straight strip of the same material, 6 inches long (B), nailed along one of its longer sides to form a ridge against which the tracing may rest. A small screw (C) is then fixed in a line continuous with this piece of wood, in such a position that it holds exactly the same relation to the ridge formed between it and the bigger slab that the moving, trace-supporting, portion of the watchwork does to the axis of the recording lever. A stout needle (D) is then threaded with cotton or silk (E), of which the two ends are made to tie round the screw above mentioned, at $5\frac{1}{4}$ inches or so from its eye, according to the length of the recording lever. A small length of string (F) is also attached at one end to the same screw, and knotted (G) at $5\frac{1}{4}$ inches from it; the needle-point is then made to run into the knot a short distance, and project slightly beyond it. By this arrangement, if the thread and string are put on the stretch, by the finger placed between them against the needle, the point of the needle can be readily made to draw a circle similar to that formed by the recording lever. The tracing then being placed along the ridge on the board, the necessary curves are made on it from the points which it is desired to project.

By means of a small ordinary spring-bow, with a screw-nut to regulate the distances between its points, great precision in measuring can be arrived at with a little practice.

It is best to measure the length of each beat in the trace, and striking the average of them all, to consider that as the actual ratio desired. By this means the complication introduced by the effects of respiration is much reduced, and the inequalities in the movement of the watchwork are in great measure neutralised. For instance, in a pulse beating forty-four times a minute the following were the actual numbers obtained:—

3·8	3·8	
3·775	3·825 :	with the average of 3·8.

Again, in a pulse beating 137 times a minute the following were also the numbers, in which the effects of respiration are clearly marked :—

1·8	1·775	1·725	1·775	1·75	
1·725	1·725	1·775	1·675	1·728	
1·725	1·7	1·8	1·75		with an average of 1·7443.

It will not require a long experience in sphygmographic measurement to convince the student that it is in quick pulses that the systole is proportionately the longer, and in slow pulses the shorter, inspection alone putting that beyond a doubt. But when observations become more numerous, fresh facts become apparent. The following table gives the results obtained from a series of measurements in which the rapidity of the pulse was carefully observed, and the number of times that the systolic length was contained in the whole beat was recorded:—

Pulse Rate	Number of times that the systolic length occurs in the whole length of the beat
37 a minute	4·175
49 "	3·51
60 "	3·09
63 "	2·93
64 "	2·91
65 "	2·9
70 "	2·775
83 "	2·45
100 "	2·16
137 "	1·7443

From this table it is evident that the length of the systole must vary with the rapidity of the pulse, and with nothing else, it being constant for any given rate, or very nearly constant at rates which are very nearly the same. A large number of other measurements which I have made all help to verify the statement, that the length of the systole does not change in healthy persons when the pulses compared are of the same rapidity. But in pulses of different speeds the case is quite altered, as may be found from the above table; in which the actual length of the systole in parts of a minute can be obtained by multiplying the numbers given in the second column by those representing the corresponding pulse-rates in the first. The quantities thus obtained form the denominators of the fractions which represent the systolic length in parts of a minute, and they will be turned into decimals of a minute for facilitating comparison. As an instance, $1·7443 \times 137 = 238·9691$, and $\frac{1}{239} = 0·0041841$ of a minute. Again, $2·91 \times 64 = 186·24$, and $\frac{1}{186·24} = 0·005369$ of a minute. A little attention shows that the length of the systole does not vary as quickly as the beat, and that, by extracting the cube roots of the pulse-rates, numbers are obtained which do vary in a corresponding manner, within the limits of experimental error. Thus, the cube root of 64 is 4, and that of 137 is 5·155:

$$4 : 5·155 :: 1 : 1·288 \ldots$$

and

$$41841 : 5369 :: 1 : 1·283 \ldots$$

A similar arithmetical process applied to the other ratios in the same table will lead to the same result, namely, that *the length of the systole, as indicated in the sphygmograph trace from the radial artery does not vary when the pulse-rate is constant, but varies as the cube root of the rapidity of the cardiac action.*

This law, just stated, may be looked at in different lights as far as the manner in which the heart works and rests is concerned. Most of us always rest during the night and work in the day; and, supposing that each man has eight hours' sleep in the twenty-four, he may be said to rest during one-third of his existence. If the pulse were always to beat at a rate of 62 a minute, then, as the systole occupies one-third of the beat, the period of the heart's repose during the life of the individual would be two-thirds of its length.

Again, there are two ways in which the wager to walk a thousand miles in the same number of hours may be laid. Either the total number of miles must be completed in the time, with no restrictions as to the manner in which they are done; or it may be required that a mile should be walked in each of the hours. This last case is more strictly analogous to the action of the heart. The period of work is short, and that of repose is short also; but both are frequent. No time for prolonged rest is allowed, so all nutrition by the blood in the coronary vessels must be done intermittently, just as the pedestrian, in the second of the above-cited instances, is wise if he distributes his meals over his sitting intervals, instead of taking them in large quantities less frequently. What the above given law means is that, according to the differences in the length of the repose or diastole, so is that of the cardiac contraction modified; or again, referring to the analogy of the pedestrian: suppose that when walking a thousand miles, one in each hour, he could get over each mile in twenty minutes, then if he had to do the same distance in 1,500 hours, one in each hour and a half, he would get over each mile in a less time—say fifteen minutes—because the longer period of rest would make walking so much the less difficult. But it will be remarked that the length of the systole is greater and not less when the diastole is longer. This is perfectly true, but it does not alter the above-stated nature of the problem, because there is a peculiarity in the heart's action which is not found in that of the walking man.

The law which regulates the frequency of the pulse is not a simple one; but it partly depends on the influence exerted by the changes which occur in the calibre of the smaller arteries of the skin; the pulse being quick when these vessels, which have muscular walls, are very open, and slow, when they are contracted; so that when anyone blushes the pulse augments in rapidity, on account of the dilatation of the small vessels; and when the skin gets pale with cold the pulse is retarded. From these remarks it is evident that when the pulse is quick the blood escapes from the arterial system into the capillaries more quickly than when it is slow. Consequently, the difficulty which the heart must experience in emptying itself into the arteries varies according to the rapidity of the pulse; and as during the diastole which preceded any systole under consideration, it was supplied with blood at a certain definite pressure and for a certain definite time, the ventricles can likewise only develop a certain pressure and maintain it till a certain definite amount of work has been executed. Therefore the ventricles immediately they commence to contract exert their full systolic pressure, and maintain that pressure until they are empty. But it is evident that the time necessary to empty them of a definite amount of blood under these conditions, must depend on the rapidity of the flow of blood through the small arteries and capillaries; when the flow is halved the systolic time is doubled, *if no other force come into play;* in other words, the length of the systole is dependent on the arterial resistance. If therefore the length of the diastole did not affect the length of the systole, when the former is doubled the latter would be doubled also; but from the law that the systole varies as the cube root of the pulse-length, it may be inferred that such is not the case; and from a study of tracings from the movements of the heart, as they affect the chest-wall—termed cardiograph tracings—many reasons may be given in favour of the law that the nutrition of the heart's walls varies as the square root of the diastolic period,* during which only the blood can traverse the capillaries of the coronary system of vessels, which supply the ventricular walls.

There is yet more to be learnt from the sphygmograph trace. As previously stated, the length of the systole does not vary for any given pulse-rate; but the blood-pressure in the arterial system varies independently of the pulse-rate altogether; consequently, the length of the systole does not depend on the blood-pressure. This result, somewhat paradoxical at first sight, can only be explained on one assumption, which is that the variations in the arterial blood-pressure must affect the nutrition of the heart in such a way that the extra work to be performed, when it is raised, produces a corresponding extra power in the ventricle to overcome the added obstacle to its contraction. The coronary vessels being direct offshoots from the aorta, their blood must vary in pressure with that in their main stem; and this, in addition to the above-stated facts, all tends to prove that *the nutrition of the walls of the heart varies directly as the blood-pressure in* the arterial system.

These peculiarities in the mechanism of the cardiac action have a definiteness about them which can be obtained only through the employment of the sphygmograph; and I hope that the above remarks will lead others to repeat my measurements, in doing which I am satisfied that they will substantiate the accuracy of my results.

* See *The Law which regulates the Frequency of the Pulse*, "Journal of Anatomy and Physiology," May and November, 1873.

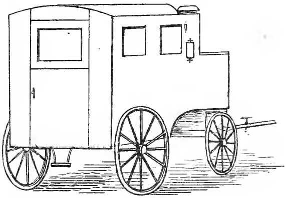


Fig 1.

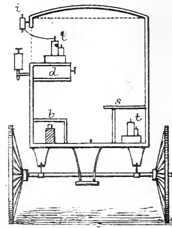


Fig. 2.

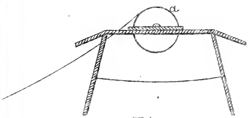


Fig 3.

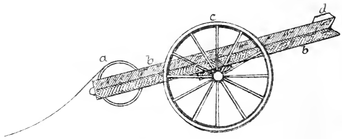


Fig 4.

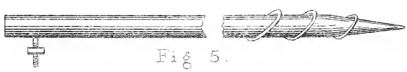


Fig 5.

Fig. 6

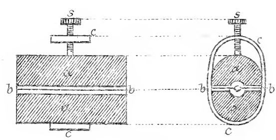


Fig. 7.

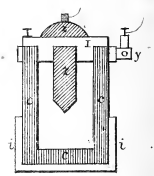


Fig. 8.

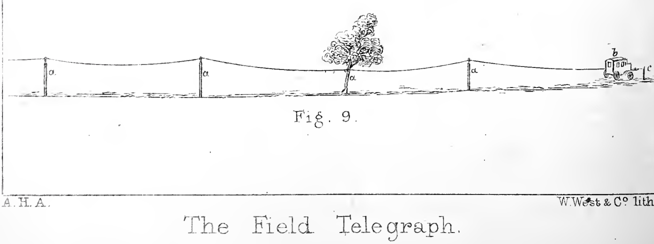


Fig. 9.

A. H. A. W. West & Co. lith.

The Field Telegraph.

THE FIELD TELEGRAPH.

By A. HILLIARD ATTERIDGE.

[PLATE CVIII.]

IN the year 1802, when Napoleon was First Consul, there arrived in Paris two artisans of Poitiers. One of these men, Jean Alexandre, had invented a rudimentary form of the electric telegraph, and, with his friend Beauvais, he had left the little country town full of high hopes to submit his discovery to the great soldier who was then guiding the destinies of France. He requested a personal interview with the First Consul, refusing to communicate his secret to anyone else. He was referred to the astronomer Delambre, whom he succeeded in convincing of the value of his invention; still, however, declining to reveal the way in which the electric signals were transmitted, unless to Napoleon himself. But the latter refused to grant the required interview, saying he had no time to trouble himself with such matters; and Alexandre and Beauvais went back to Poitiers in bitter disappointment.* Had Napoleon listened to the proposals of Alexandre, the course of history might have been changed; for had he been able to secure the exclusive possession of the electric telegraph, it is easy to imagine the effect it would have had upon his campaigns, and how difficult it would have been for even the allied armies of all Europe to contend against a great commander, who, by some secret means unknown to them, could obtain accurate and instantaneous information from every point of the theatre of war, and flash his orders to *corps-d'armée* divided from him and each other by miles of country, while his opponents had only to trust to horses and couriers to carry their orders and despatches.

A very little study of the wars of the French Revolution, in comparison with those of our own time, will be sufficient to show what an advantage the telegraph is to the modern com-

* Villefranche, *La Télégraphie Française, Étude Historique.*

mander. A striking instance of the extreme difficulty of combining the operations of separate corps or armies in the same theatre of war, without the aid of the telegraph, is afforded by the history of the campaign of 1796, in Germany, when Moreau and Jourdain were "acting in concert" against the Austrians. The Archduke Charles left a weak retarding force in front of Moreau, while he directed all his available strength against Jourdan; and the former general was actually advancing in triumph through Southern Germany, under the full conviction that his colleague had obtained a like success to the northward, while the latter had actually been defeated at Amberg, Wurtzburg, and Aschaffenburg, and driven back upon the Rhine, and Moreau only heard of his disaster in time to save his army from destruction by a hurried retreat through the defiles of the Black Forest. As a contrast to this, let us take the campaign of 1866, when the two Prussian armies advanced from separate bases into Bohemia, laying down the lines of the field telegraph as they moved forward, which, being connected by the permanent telegraphic system of Saxony, kept each army in constant communication with the other, and thus enabled them to combine their operations, and at length unite with decisive effect on the battlefield of Sadowa.

It is just twenty years since, for the first time, the electric telegraph was used in the field, and to our own army belongs the honour of having led the way in its adoption. The trenches and batteries before Sebastopol were traversed and connected by lines of telegraph, and the French soon followed our example, and constructed a similar system in their own lines; while later on a cable laid across the Black Sea put the armies in the field in direct communication with Paris and London.

Since that time a regular telegraph corps has been organised in every European army. And the field telegraph was used by the French in Italy in 1859, and in their campaigns against the Kabyles in Algeria; and in America both the Federals and Confederates made free use of permanent and temporary lines during the War of Secession, the Southern cavalry in particular displaying great daring and enterprise in riding round the flanks of the Federal armies, seizing their telegraph lines, sending false messages to the northern generals, and then cutting the line and retiring as rapidly and secretly as they came. It was, however, in the Prussian army, and in the great campaigns of 1864, 1866, and 1870–71 that military telegraphy attained its greatest development; and after the experience of these three wars, the Prussian telegraph corps is probably the most efficient in Europe. We have already seen how well it did its work in the campaign of 1866,* and in 1870 it established the network of wires over the north-east of France, that enabled Moltke, sitting in his bureau at Versailles, to move his armies as accurately and certainly as pieces on a chess-board; while round Paris itself a circle of telegraph wires—that in a moment flashed information of a sortie, and orders for a reinforcement of the threatened point, to every part of the long line of sixty miles, on which the besiegers lay—contributed almost as much towards the reduction of the vast fortress as the circle of steel and iron, of batteries, earthworks, and redoubts, which, without the connecting link of the telegraph wire, could not have been maintained for a single month. On their side the French displayed no less energy. The regular telegraph corps was shut up in Metz or lost at Sedan; but a fresh corps was organized for the armies of the Republic, and at Paris the telegraph lines linked together the enceinte, the forts and outworks, and the head-quarters of General Trochu. But it was in the second siege of the capital that the French telegraph corps obtained its greatest success. During the fighting in the streets of Paris, in May 1871, the moment a barricade was taken, a telegraph station was established in a neighbouring house, and when another post was carried, the telegraph corps would again move forward with the troops, and thus MacMahon was able to watch every turn of the fight, and provide for every contingency, in a way that otherwise would have been utterly impossible. For ourselves, we have had no European war since 1854; but our armies have carried the telegraph with them into India and China, and through the ravines and passes of Abyssinia; and now the "talking wire" stretches from Cape Coast Castle through the bush, across the Prah into the heart of Western Africa, hanging on the trees, with here and there a few poles, the whole having been erected by Fantee labourers, under the direction of a handful of Royal Engineers.

The object of the field telegraph is to keep the head-quarters of an army in communication with its several corps, and at the same time with the general telegraph system of the country. In the Prussian army, when the telegraph corps was re-organised after the war of 1866, it was formed into two divisions—the Field Telegraph Division and the Etappen Division—with a view to the more efficient performance of these two services.

* During the armistice which preceded the treaty of Prague in 1866, the Prussians displayed great carelessness about their telegraphic communications, and the troops often tore down a line to light their fires with the telegraph poles, and tie up their horses with the wire.—See Stoffel, *Rapports Militaires.*

Both divisions consist of several companies or sections, each of which contains about 150 men, including officers, telegraph operators, pioneers, workmen for the erection of the line, and drivers for the station-, store-, and baggage-waggons. In all armies the telegraph and *matériel* is, of course, very similar, and we shall therefore describe that of the Prussian army, adding a few notes on that of other countries.

The two essential portions of the field telegraph are the station and the line. In order that there may always be a sheltered place for erecting the instruments and transmitting messages, each detachment of the telegraph corps carries with it one or more waggons fitted up as stations; but wherever a halt of more than a few minutes is made, and there is a suitable building available for that purpose, a telegraph station is established in it by removing the batteries and instruments from the waggon. Fig. 1 (Plate CVIII.) is an outline sketch of a Prussian station-waggon; fig. 2 being a section of the same. The waggon is about 9 ft. long, with an interior height of 4 ft. 6 in., and a width of 4 ft. It is built as lightly as possible, and weighs when loaded only 14 cwt., and is easily drawn by two horses. On the outside are two insulated brass conductors (*i*, fig. 2), to which wires can be attached. Inside the windows is a shelf with a drawer (*d*), on which the instrument (*t*) can be placed when in use, and opposite to this is a seat or bench (*s*) for the operators, on which a man can sleep at night. Under the seat is a recess in which a spare instrument (*t*) is kept, while the batteries are arranged in a box (*b*) under the shelf. When a message is to be sent from this movable station the waggon is stopped, and the line-wire is attached to the insulated conductor (*i*). This is connected with the instrument and battery, and in order to complete the circuit the battery is placed in electrical communication with the second insulated conductor, to which another wire is attached which joins it to the earth-conductor or earth-stake (*piquet à terre*) (fig. 5). Thus the course of the current, when transmitting a message, is from the battery to the instrument, and by the first insulated conductor (*e*, fig. 2) to the line of wire, the earth-plate of the receiving station returning it to the earth-conductor, driven into the ground near the waggon, and thus back by the second insulated conductor to the battery.

The instruments are of the Morse pattern, constructed so as to fit in a very small space, and recording the signals with ink. The battery (of which there are two in each station-waggon) is a simple form of M. Marié Davy's sulphate of mercury battery. It consists of ten elements, one of which is shown in section in fig. 8; (*c*) is a charcoal vessel, containing sulphate of mercury moistened with water to the consistency of paste, and in this the zinc plate (z) is suspended by means of the india-rubber cover (I). The whole is placed in the india-rubber vessel (i), and a copper collar (y) is added, to which a connecting wire can be attached. This battery has the advantage of being very portable, while the india-rubber cover prevents the charge of sulphate of mercury from being spilt by the motion of the waggon.

The line may be either an aerial or a ground wire, or a combination of both, the former being stretched on poles, while the latter is insulated by being enclosed in a light cable, about half an inch thick, and laid along by the road sides or across the fields. The uninsulated wire and the cable are both issued to the telegraph corps coiled on small drums, several of which are carried by each store-waggon. In those companies which are to erect a wire stretched on poles, the waggon carries five English miles of uninsulated galvanised iron wire, one mile insulated in gutta percha, 1,000 yards of the cable, and 200 poles with insulators attached, all the wire being coiled on twelve drums. If it is intended to lay a ground line, the waggon carries eleven drums of the cable, one of wire covered with a light coat of gutta percha and tarred hemp, and a few poles and insulators, for carrying the line across small hollows, or raising it overhead in crossing roads. Beside these stores the waggon contains all the tools necessary for the work, and a light step-ladder is hung underneath it.

The wire is uncoiled from the drums by placing them successively on a hand-barrow, from which it is paid out as the barrow is carried along. The hand-barrow (fig. 3) consists of a light iron frame, with wooden legs and handles, which are made to fold up when not in use. On this frame the drum (a) is placed; one man carries it in front and two behind, the wire uncoiling and running out between them. A wheel-barrow (fig. 4) is sometimes employed, and is improvised for this purpose by attaching iron handles (d) to the step-ladder (bb) and placing it on a pair of light iron wheels (c), the drum (a) being hung in a socket near the top of the ladder.

The poles are made of well-seasoned and selected red pine, about 12 ft. long and a little more than an inch thick. At the bottom is an iron point for fixing them in the ground, and at the top a socket (s, fig. 6) of the same metal, with a hollow screw to receive the spindle or stem of the insulator. This consists of an iron spindle (b), with a male screw cut in it, which supports either a cap of gutta percha (g) or an earthenware cup surmounted by a metal bell. In both cases there is on the top of the insulator a metal hook (a), in which the line wire is hung. There are also insulators, the spindles of which terminate in spikes or sharp screws for driving into walls and trees, thus saving the trouble of erecting a post (fig. 9).

When all the wire of the first drum is laid down, the end of it is roughly spliced on to the wire of the next drum, and the joint secured by means of the connector (fig. 7, Plate CVIII.). This consists of two semi-cylindrical pieces of hard wood (a), their flat side being grooved to receive the wire, and covered with a layer of india-rubber (b) to act as packing, and insulate the joint, in the case of a ground line, and the whole is held tightly together by the brass collar and screw ($c\,c\,s$, fig. 8).

The line is very rapidly and easily constructed. In the case of a ground line it is simply paid out from the drums on the hand- or wheel-barrow, being buried in a shallow trench or elevated on poles, when it is necessary to cross a road, where the insulation of the cable might otherwise be injured by the wheels of passing vehicles. During the invasion of France the Prussians frequently avoided the roads in order to protect the line from the *franc-tireurs*, and made considerable *détours*, concealing it in woods, ravines and watercourses. Where the uninsulated wire is used, poles are erected about fifty paces apart, the hole to receive each pole being made by driving a sharp pointed iron bar into the ground with a heavy mallet. As soon as a pole is fixed the wire is run through the hook on the top of the insulator, and stretched tight by a man holding it over his shoulder, who keeps it in this position until the next pole is ready to receive it. Wherever there are trees or walls near the line, the work is still further lightened by dispensing with the poles, and merely attaching the wire to the insulators specially constructed for this purpose. In this way the line was erected for the Ashantee expedition, the negro labourers carrying only a light ladder to ascend the trees, a small axe to clear away the boughs, and a gimlet to make the hole for the spindle of the insulator. It never took, we are informed, more than five minutes to fix an insulator to a tree; but, in those few places where trees were not available, fully half an hour was occupied in erecting each pole, and even then it was often unsteady and had to be propped and guyed.

In Europe, where there is an extensive telegraph system in operation in every country, there is no need of the field telegraph lines extending from the front of the army to the base of operations. Far less than this is required. All that is necessary is to connect the head-quarters of the army with the nearest point on a permanent telegraph line, and in most European countries an army in the field would seldom, if ever, be more than ten miles from such a line. Ten miles of the field telegraph can easily be erected in half a day; indeed, the Austrian engineers assert that on favourable ground they could do the work in two hours. In most cases, of course, the advancing army would have to repair the permanent lines which would be partially destroyed by the retreating forces, and in this way twenty-five miles of wire were often erected by the Prussians in a single day. As soon as an army moves forward, the field telegraph line previously erected is taken down and re-coiled on the drums, while a fresh line is laid from the new head-quarters to the nearest permanent telegraph. This is done with a view to economising the material, an enormous amount of which would have to be carried with the army, if the lines it left behind it in its advance were not removed, and the poles, wire and insulators employed in their construction again utilised. The hand-barrows of the Austrian telegraph corps are designed to be used in re-coiling as well as uncoiling the wire; and for this purpose are fitted with a crank handle and ratchet-wheels, so as to enable a man to turn the drum and wind the cable upon it.

Besides the ordinary field telegraph companies, the French army includes a mountain telegraph corps, organised with a view to operations on the mountainous frontiers of the south, or to be ready to carry a line over a range of hills in an ordinary campaign, thus avoiding a long *détour* in the valleys, or securing lateral communication with troops divided from the main army by the hills. As the mountain line would have to be laid along narrow rocky paths, and through lofty passes, all carriages and waggons are dispensed with, and their place is taken by a train of mules. In a mountain telegraph company several of the mules are each laden with two drums of the insulated cable, the instruments and batteries are carried on pack-saddles on the backs of others, and others again transport the baggage, provisions, and forage of the company, and also a light tent to form a station whenever messages are to be sent along the line.

While the field telegraph affords a commander a rapid and certain medium of communication with his base of operations and the various corps of his army, it must be remembered that it is one which is continually liable to interruption by an enterprising enemy. Wherever a general has to contend with an army well provided with good cavalry, he will find it extremely difficult to protect his telegraph lines from being destroyed by daring raids of his opponents. There are several easy ways of making a telegraph line temporarily useless. The simplest and most obvious method is to pull down the poles and cut the wires into pieces; but when this is done the damage is easily detected, and the repairs at once commenced. The interruption will, therefore, be far more serious if it can be effected in a way which will not permit of its exact locality being so readily discovered. This can be done by cutting the wire, introducing a piece of gutta percha, or any other non-conducting substance into the course of the circuit, and connecting the ends of the wires with it, so as to give it the appearance of one of the ordinary joints or splices of the line. At the same time a few poles can be pulled down in another place, and the wires cut, and the probability is that the engineers who repair the line will not discover the hidden interruption of the circuit until after they have restored the gap, and found that the wire is still cut somewhere else, and even then the place where the non-conducting substance is introduced will not be discovered until some time has been employed in carefully testing the line with the galvanometer.

But there are other dangers to telegraphic communication in the field besides the mere damage to the line. If the enemy's cavalry get possession of a station, they can easily send messages containing false information or delusive orders to well-known officers of the opposing force, while the place from which they are sent, and the assumed name in which they are despatched, will give the messages an appearance of authenticity which, if it does not completely deceive the recipient, will at least be the cause of considerable doubt and perplexity to him, and perhaps make him hesitate to accept the accurate information or authentic orders received from other sources. Again, even without occupying a station, it is possible to read the messages which are passing along a telegraph line, and thus perhaps discover important secrets. All that is required for this purpose is a small portable receiving instrument, and a few yards of copper wire to connect it with the line. A single individual, thus equipped, can "tap" a telegraph line, in the day time, by receiving the message in the ordinary way; and at night (when, of course, it would be easier to approach the line) by listening to the clicking of the armature against the electro-magnet of the instrument. But all these dangers are only of a partial or temporary character. By carefully patrolling and testing the line, it cannot be interrupted for any length of time without the damage being observed and repaired. By adopting a secret arrangement that there shall be a certain number of letters in the two or three words at the beginning or end of every message, a despatch sent by an enemy can in most cases be detected; and again, by employing a cipher alphabet, it will be difficult for anyone who taps the line to obtain information from the messages which fall into his hands.

From this brief sketch of the structure and uses of the field telegraph the reader will understand what an important part it plays in modern war. On the march it directs the movements of advancing columns, on the battle-field it flashes orders and information with the speed of thought to right, centre, and left of the immense lines extended over mile after mile of country;

in beleaguered cities it places the whole defence from moment to moment under the eyes of those entrusted with its direction, and it is of no less value in the attack. It is not too much to say, that without this wondrous power it would be almost impossible to direct the movements of the thousands on thousands of men, and guns and horses, which form the vast armies of Continental Europe. It has effected a revolution in military science, none the less important because it is hidden from the general view, and seldom attracts the attention of even the ubiquitous special correspondent. Armed with all the weapons which inventive genius and mechanical skill can devise, the modern commander has the lightning also to do his work, and the electric current gliding on its secret path through the wide network of cable and wire tells him what is passing each hour in the remotest parts of the theatre of war, and transmits the mandates which decide the fate of nations.

EXPLANATION OF PLATE CVIII.

Fig. 1. Prussian station-waggon.
" 2. Section of the same.
" 3. Hand-barrow for uncoiling wire.
" 4. Wheel-barrow.
" 5. Earth-conductor.
" 6. Telegraph pole, socket, and insulator.
" 7. Wire-connector.
" 8. Marié Davy battery.
" 9. The field telegraph. *aaa*. Line-wire supported on posts and tree. *b*. Station-waggon. *c*. Earth-conductor.

SIDE LIGHTS ON THE POTATO DISEASE.

By WORTHINGTON G. SMITH, F.L.S.

SO much has been already written about the potato disease, that it might reasonably be doubted whether anything fresh could, at the present time, be placed on record. It is undeniably true that nearly all our knowledge of facts has been published over and over again, and it must be confessed that these facts have been added but little to since their original publication by Berkeley, Montagne, De Bary and others; but at the same time many side lights have been recently thrown on the subject of the potato disease by analogues found amongst other fungi, and by actual experiment. Observation and experiment have, without doubt, been greatly stimulated by the active measures set on foot by the Royal Agricultural Society in offering prizes for additional information, and disease-proof varieties; the effort to elicit a more complete knowledge of the potato disease ended in failure, and whether the more recent action in regard to "disease-proof" varieties will terminate in the same manner time can only show. Few subjects have been more written about by incompetent persons than the potato disease, and many hundreds of published essays (including, perhaps, the ninety submitted in competition for Lord Cathcart's prize) are of little value; the horticultural press for the last thirty years has teemed with communications of the most worthless character on this vexed subject in letters which have pointed to conclusions in every direction but the right one; the deductions and results of one writer being invariably flatly contradicted or nullified by the experiments and conclusions of another. The history of the first appearance of the potato disease has been ably written by Berkeley in the first volume of the "Journal of the Royal Horticultural Society," and this history has been reproduced in so many different forms and places, that we think it better to pass it over at once with the mere reference to its position. A very precise and clear account of the disease itself from a scientific point of view—and one embracing all our most reliable knowledge of the subject, ignoring everything but dry facts—has recently been written for the "Journal of the Royal Agricultural Society" (vol. ix. p. 1), in about 150 lines, by Mr. Carruthers, the Keeper of the Botanical Department of the British Museum: to any one desiring a *résumé* of all that is actually known, written in the fewest possible words, we commend this essay.

To men of science, and indeed to most intelligent readers (thanks to the popular scientific publications of the day), the fungus itself is now almost as well known as the commonest wayside plant; but for a complete comprehension of *Peronospora infestans*, and its habit of life, one not only requires to perfectly know this unit by itself, but all its immediate allies, and indeed most other fungi. Without this knowledge one is apt to give greater importance to certain facts in its life history than they deserve, and to ignore the greater importance of certain others because they may appear in themselves to be trivial.

We will pass by those writers, whose views must be worthless, who persistently confuse "zoospores" with "oospores" (and even sclerotia), and who write as if one name was synonymous with the other two. "Zoospores" are, of course, those bodies found on the fruiting threads of *Peronospora* (formed within certain privileged larger spores, by a differentiation of their contents), and which, when set free, move about like animalcules by means of cilia; whilst "oospores" are found on the mycelium (buried in the tissues of the supporting plant), and formed by the discharge of the contents of a body having anther-like qualities into another body, which bears an organism somewhat analogous with the ovule in flowering plants. Sclerotia proper, which *Peronospora infestans* may or may not have, are really knots of densely compacted mycelium, probably formed by the mycelial threads of the fungus winding themselves round and round each other in a knot-like fashion. Why the mycelium of certain fungi should possess this function it is difficult to say, but it is nevertheless a fact that the mycelium of many of the higher fungi invariably gets compacted in this way. The potato fungus produces spores and zoospores, by which it propagates itself; and most writers competent to form an opinion consider it highly probable that "oospores" are also produced on the mycelium, because, though never yet seen in connection with the potato-fungus itself, these "oospores" are produced on the threads of other and allied species of *Peronospora*. It is also uncertain whether the mycelium gets compacted into the small knotty bodies called sclerotia; but it seems reasonable to assume that during winter the fungus rests in the form of oospores or sclerotia; or where can the fungus come from the following year after a rest of nine months? Montagne and Berkeley have referred at some length to a body (dubious to us) found in spent potatoes, and described many years ago as a second form of fruit of *Peronospora infestans*, under the name of *Artotrogus*. This may eventually prove to be a second form of fruit of our plant; but though Montagne undoubtedly found *something* which he sent to Berkeley, he probably never met with it again, and we believe no observer has ever met with a trace of it since. At the time of its first observation and description great attention was directed to *Peronospora*, to the consequent neglect of all the other members of the vast fungoid army common enough on spent potatoes; and this is the reason, we imagine, why *Artotrogus* was referred to *Peronospora*; for it might, with equal reason, have been referred to a large number of other species belonging to very diverse genera, judging from the number of *Mucedines*, &c., common upon decaying potatoes.

We cannot help thinking that undue importance has been given to the non-discovery of oospores in *Peronospora infestans*, by some recent writers, who have said that their ultimate discovery might help us to doctor our potatoes in winter, and so get rid of the disease: it certainly seems strange that at present we know so little of the winter life of this mould after such unceasing search has been made by so many sharp students for such a number of years; but it is doubtful whether any amount of doctoring will kill the disease without destroying the potatoes, even if oospores or sclerotia are ultimately discovered. Many observers have noted the small number (sometimes entire absence) of mycelial threads in the cellular tissue of diseased potatoes, so that it recently became an interesting fact to observe a dense mycelium permeating the earth in the neighbourhood of diseased potatoes in some experiments conducted by us. Unfortunately, however, mycelial threads do not generally present marked distinctive characters, so that it would be impossible to refer these threads to *Peronospora* in preference to any of the other moulds, &c., common upon bulbs and tubers. The results of our experiments seemed, however, to point to the necessity of examining carefully the earth from which diseased potatoes are taken. The habits of mycelium whilst in a highly condensed state are better known amongst the higher fungi; and, as an interesting instance in point, we may mention a case which came under our own observation last autumn in regard to *Typhula phacorrhiza*, Fr., which plant is said generally to spring from a sclerotium, and of which Berkeley says, in his "Introduction to Cryptogamic Botany," that it grows indiscriminately on *Sclerotium complanatum*, and *S. scutellatum*; and that it is not improbable that both are contracted states of the *Typhula*, like the tuberiform base of *Agaricus tuberosus*, of which we shall have more to say anon. The brothers Tulasne have laughed at Berkeley's idea of one perfect plant springing from two diverse sclerotia, one of which is said to be peculiar to spring, and the other to autumn and winter; but when we bear in mind that "Sclerotium" is an altogether spurious genus founded on insufficient material, we believe that our eminent mycologist, Mr. Berkeley, will be found in the end to be quite right. Be this as it may, it appears that Mr. Renny found a batch of these sclerotia (said to be peculiar to dead leaves) on the gills of decaying Agarics—probably plants of *Agaricus nebularis*, Batsch.—and kept them in a dried state in his pocket-book for *three years* till last autumn, when he gave them to Dr. Bull, of Hereford, and requested him to plant them, which he accordingly did in a flower-pot. After a short interval of five or six weeks, and just at the time of the great Hereford "Fungus-foray," all these *Sclerotia* had grown and pushed up their heads through the earth, in the form of numerous perfect plants of *Typhula phacorrhiza*, a handsome and somewhat rare fungus in shape like a long yellow thread or club. It is very singular that these members of the order *Clavariei* should have been found in a sclerotioid condition upon the gills of a white spored agaric (*Agaricus nebularis*, Batsch.), which is notorious for producing another agaric (*A. Loveianus*, B.), with pink spores upon the top of its pileus. It is also worthy of note that in some instances two plants grew from one *sclerotium*, and, as a rule, the *sclerotia* were distinctly in pairs, a large specimen by the side of a smaller one: this habit, for which we can offer no proper explanation, is also common in the case of *Agaricus tuberosus* (Bull), and in both cases we have seen a single perfect plant supported on two tuberiform bases, caused by the shoots from the tubers coalescing at an early period of their growth. We are under the distinct impression, from actual experiment, that fungus-spores are generally very short-lived, and that they are almost immediately destroyed by dry air or too much moisture, and are only saved from destruction by their immediate germination after leaving the parent plant, and so forming dense masses of mycelium, which are able to more effectually resist wet and drought. This shortness of life in fungus spores need not cause us surprise when we remember how short-lived the true seeds of some flowering plants are, as in the *Umbelliferæ*. I am assured by a practical plant and seed-grower, that the seeds of some species of *Ferula* will not germinate if they have been gathered for more than two weeks, whilst among the long-lived seeds of the *Leguminosæ*, some species of Acacia have been known to rest for three years before germination took place (unless the outer coat of the seeds had been cut before planting). A rest clearly takes place with the fungus which produces the potato disease; but it does not follow that because we can light upon the fungus in a resting condition, we can therefore destroy it. It is not at all improbable that its winter life may reside in extremely fine compacted threads under ground and near the decaying tubers, but it would be still as impossible to exterminate these threads as it would be to put a stop to the harvest of our higher woodland fungi.

There are many difficulties yet to be cleared up as regards the winter life of fungi; one of which was very forcibly put by Dr. Bull at the last great Hereford Fungus-meeting in regard to *Agaricus tuberosus* (Bull) and *A. cirrhatus* (Schum.), both of which spring from a tuberiform base, which rests during the winter: the former is said to grow on dead *Russulæ* and on the ground, whilst we have found it imbedded at the base of the tubes of dead Polyporei; the latter (*A. cirrhatus*), more immediately referred to by Dr. Bull, which is generally found amongst dead leaves, he found imbedded in abundance some inch-and-half or two inches deep in the solid earth. How the compacted mycelium of one of the most tender of Agarics got so low down in the earth, and was afterwards able to push the tender perfect plant through the hard soil, was a difficulty he could not explain, and one which was by no means lessened by the fact of the plants being found precisely on the spot where some gigantic specimens of an Agaric (*A. aureus*) new to Britain had been previously found, and the reasonable surmise, that as in the case of *Agaricus nebularis* and *Typhula phacorrhiza*, they were in some way connected with each other.

As to any variety of potato being "disease-proof," De Bary has himself said that there is nothing in one potato plant more than in another to predispose it to the attack of the fungus; or in other words, no sort of potato, healthy or unhealthy, new or old, can withstand the attacks of the spores of the *Peronospora*. From the conflicting nature of the evidence on this point, principally from growers for the markets, we are bound to return a verdict of "not proven;" and from the fact of the Royal Agricultural Society now offering prizes for "disease-proof" sorts, it may be surmised that the Council of this Society consider it reasonable to suppose that some potatoes may really be able to throw off the attacks of the *Peronospora*. The forthcoming great trial of potatoes by the Society will probably put this question at rest; whilst the following record of some experiments of our own, very recently laid before the Royal Horticultural Society, will probably be read with interest at the present juncture.

We wrote in the autumn of 1872 to several nurserymen, asking them to forward us samples of different potatoes, accompanied by a brief opinion of their merits in resisting disease. Our request was at once acceded to, and one firm also kindly furnished us with a large number of potatoes in all stages of disease.

From the latter collection we selected specimens in which the disease appeared slight and superficial; and borrowing a suggestion from the known efficacy of sulphur in destroying the *Oïdium* of the vine, we dusted the potatoes externally and over all the cut surfaces with sulphur, and carefully planted them in the earth, so that they were entirely surrounded by sulphur. In the usual course the disease appeared on the foliage, and on digging up the plants in the autumn the crop was large; but all the potatoes were very much diseased, and their skins cracked and blistered.

We tried the effect of dipping potatoes in all stages of the disease in the following solution:—Pure carbolic acid, 2 drachms; glycerine, 3 drachms; and distilled water, 3 oz., with the following results: After resting half-an-hour in the solution, the potatoes changed to an intense crimson-brown colour, which, however, at length gradually, but not entirely, disappeared, and the fœtid odour peculiar to diseased potatoes passed quite away. The potatoes seemed at the time none the worse for dipping, so we soaked a large series, ranging in time from five minutes to twenty-four hours, and planted them in large pots; in the spring the potatoes were all dead and rotten, and many specimens were pervaded throughout by a fine deep malachite green colour; the tubers were, moreover, all thoroughly infested with worms, larvæ and insects. We were not at all surprised at these results, or at the ineffectual result of any attempt to cure the disease when the cellular tissue of the potato has once become corroded, as we consider the decomposition of the tissue of the potato analogous with mortification of the human body, and that when it has once set in it is impossible to arrest it.

A potato has been largely advertised of late under the name of "Red-skin Flourball" as "disease resisting," and we had some fine sound specimens sent on to us for experiment by Messrs. Sutton and Sons. Messrs. Wheeler and Sons, of Gloucester, also sent us a batch of "Red-skin Flourball," apparently identical with the first, excepting that they were very much diseased. We cut all the specimens of both collections (sound and diseased) in halves, and carefully tied with twine the diseased half of one potato to the sound half of another, to see if the naked corrosive plasma from the diseased portion would communicate itself to the sound portion. For one month Sutton's potatoes were thus exposed on a garden bed, and remained perfectly unaffected by the contact. We then planted them, one-half the collection in large pots, and the other half in a bed, with the following results: in the spring the tubers in the pots sprouted abortively, and on turning them out Sutton's specimens were thoroughly rotten, but germinating from the eyes, and infested throughout with a dense mycelium, from which sprung *Volutella ciliata* (Fr.). Messrs. Wheeler's plants were in a state of putrid cream, and not germinating at all. In the beds the effect was different; the first thing that struck us in the spring was the luxuriant but extremely pale foliage, which, as usual, became affected by the disease in the autumn. On digging up the tubers at a later date there was a good crop from Sutton's plants, so far unaffected by the disease that only one potato was bad.

We also planted by themselves a batch of "Red-skin Flourball" in a bad state of disease, as sent on to us by Messrs. Wheeler. Strange to say, these tubers produced with us a set of exceedingly healthy plants, which gave a large crop of potatoes, and of which, as in the last instance, only one tuber could be found affected. On taking some of these plants up in the spring, when the foliage was in vigorous health, the buried tubers were dotted all over externally with *Volutella ciliata* (Fr.). We know of an instance last autumn where a collection of "Red-skin Flourball" was grown without a single diseased specimen being found amongst the entire crop; but the produce of some seed of the same variety, growing in the next field, and sent out by Messrs. Sutton themselves, turned out badly touched with the disease. As far as we can ascertain, "Red-skin Flourball" was bad last season in certain directions, and quite free from the murrain in others.

From one nurseryman we received a batch of "Paterson's Victoria," a late potato, and one said by the sender to be very free from the disease with him. Now, being a late potato, this was somewhat remarkable, as one generally looks for freedom from disease in the earlier varieties; but as we had also received a number of the "King of Earlies" variety, nearly every tuber of which was more or less diseased, we thought we would try to infect the late potato with the diseased early variety; and so we tied badly diseased halves of "King of Earlies" to sound halves of "Paterson's Victoria," and planted them as before. The foliage came up dark green in the spring, and became affected with the disease in the autumn; on digging the plants up, "King of Earlies" only remained as a putrid creamy mass, whilst all the plants of "Paterson's Victoria" were perfectly healthy, and sent their stems right through the rotten masses of the "King of Earlies." We had a good crop of "Paterson's Victoria" without a single bad tuber, or indeed any trace of disease, except a few plants of *Volutella ciliata* (Fr.) on the external surface.

Healthy "Fluke" potatoes planted in contact with diseased "King of Earlies" took the disease badly; the foliage was very dark in the spring, showed the disease in the autumn, and at the time of digging up the "Flukes" were diseased right through, and the "King of Earlies" abortively sprouting. Both were covered with a dense mycelium, and the plants of *Volutella ciliata* (Fr.) were present in all stages of growth both inside and outside the tubers, and connected with mycelial threads.

We also tried some "Peach" potatoes planted with diseased "King of Earlies." The foliage again came up intensely dark, and showed the disease in autumn; later on "King of Earlies" was completely putrid and full of worms. "Peach," however, was almost free from disease, and what existed was of a very superficial character, two tubers only being slightly touched out of the entire crop, which was very good.

We tried healthy "Early Rose" tied to diseased "Kentish Early." The foliage was very late in appearing, but when it did appear it was perfectly healthy; it however soon showed signs of disease, and in the autumn "Kentish Early" was one putrid mass, having made no attempt to germinate, whilst "Early Rose" was half dead and putrid, and the earth surrounding both potatoes was densely traversed by white mycelial threads;—no crop.

In conclusion, we tied healthy "Kingsholm Kidney" to diseased "Kentish Early," sound "Gloucestershire Kidney" to affected "Kentish Early," and healthy "Milky White" to diseased "King of Earlies," with the effect that the healthy plants at once took the disease and were totally destroyed, making no apparent effort to germinate, the decomposed portions being found full of acari and worms.

We have found as a rule, in our experiments, that the potato-skins resisted decomposition to the last, and that the cut surfaces of sound potatoes were the readiest to be affected by the corroding influence of the naked plasma present in the diseased ones. That some potatoes are, however, able to resist this influence for some time is conclusively proved by "Red-skin Flourball" remaining unaffected for a month. In the autumn of 1872 we had a large quantity of diseased potatoes thrown into a corner in a heap. By the spring of 1873 the interiors had completely rotted away, leaving only the dried skins, apparently free from disease. These skins sprouted in a healthy manner, and gave promise of sound plants, but we had no opportunity of planting them. In examining thin slices of diseased potatoes under the microscope, we have constantly found the starch affected and discoloured, which has not generally been acknowledged as a fact; and we have also been very much struck by the abundance of mycelium sometimes produced underground, and not only traversing the interior of the potatoes, but whitening the earth and investing the broken brick used for draining such plants as were grown in flower-pots; this mycelium appearing to belong to *Volutella ciliata* (Fr.).

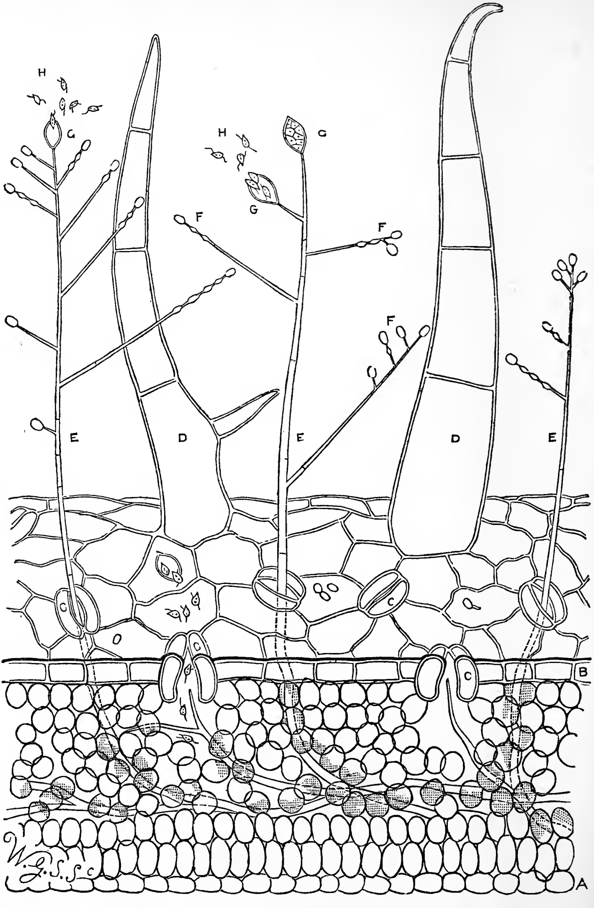
It may be said that these experiments all give contradictory results, and that what holds good under one set of circumstances totally fails under another. This we quite admit; but we consider that it is only by going over this uncertain ground, and noting the contradictory results of experiments, that any certain knowledge can be arrived at likely to prove at length useful. It is singular that certain varieties should be healthy in one place and diseased in another; but there can be no doubt that certain external circumstances affect the potato plant, and sometimes predispose it to take the disease; but we do not think over-cultivation could do this. Continued cultivation probably induces a delicate constitution, but all highly-bred animals (as well as plants) are delicate, and we constantly see this delicacy in children; but with extra care highly-bred animals acquire an equal lease of life with the coarser varieties, as a race-horse will probably live as long as a cart-horse. Exhaustion by too high breeding seems to us impossible. When a sound variety, such as "Sutton's Red-skin Flourball," falls in some district a ready prey to the disease, it appears to us that the plant has been previously thrown out of health, and then as a consequence succumbs with the weaker varieties; but what it is that deranges its health in certain districts has, we think, yet to be learned. That any plant or created thing could be murrain-proof or disease-proof is to us most unreasonable, and almost synonymous with death-proof. Something we should think might be allowed for idiosyncrasy, or an inherent peculiarity of constitution, which in itself predisposes certain potatoes to fall a prey to the disease, whilst others can throw it off; a similar state of things exists among men, for we know that whilst the bite of an adder or sting of a wasp will sometimes end fatally with certain individuals, yet others may be subjected to these inconveniences without any very serious results.

It has been suggested to us that in submitting potatoes to such a critical test as actual inoculation, that the experiments were unfair, and that the tests were analogous to inoculating a strong man for the small-pox, who would then be obliged to succumb, although under ordinary circumstances he might be well able to "resist" the disease; but we do not think this argument holds good, for if there is any meaning at all in the words "disease-resisting," these potatoes or men (as the case may be) should be able to throw off the pest under test conditions.

The study of the potato-fungus is by no means a simple matter, not only from its extreme minuteness, but from the number of other fungi which play a part in destroying the potato; amongst the foremost of these fungi we may mention *Acrostalagmus cinnabarinus* (Corda), *Torula herbarum* (Lk.), *Dactylium tenellum* (Fr.), *D. pyriferum* (Fr.), *Cladosporium herbarum* (Lk.), *Mucor mucedo* (L.), *M. ramosus* (Bull), *Ascophora mucedo* (Tode), *Penicillium crustaceum* (Fr.), *P. candidum* (Fr.), *Stysanus stemonitis* (Corda), *Fusisporium betæ.* (Desm.), *Volutella ciliata* (Fr.), *Papulaspora sepedonioides* (Preuss), and various species coming under *Sporidesmium*, *Vermicularia*, *Myrothecium*, &c., &c. The task to discriminate between one and the other of this set and many other allied species is no slight piece of work, and one not likely to be in the way of the competitors for Lord Cathcart's prize.

We do not propose here to enumerate the list of growers' varieties, early and late, new and old, British and foreign, which are said to be able more or less to resist the disease, as the subject is involved and interminable beyond reason; for years past the pages of the "Gardeners' Chronicle" alone have swarmed with experiments and results, counter-experiments and counter-results, without end. No doubt, if these communications were properly digested and tabulated, something valuable might be learned from the deductions they would show; but at present nothing of the sort has been done, and all the data founded on the experience of trade growers are in chaotic confusion.

So that our paper may not be like the play of "Hamlet" with the part of "Hamlet" omitted, we will in as few words as possible describe the fungus which is said to be the undoubted cause of the potato disease. The spores of *Peronospora infestans* which fall upon the foliage of the potato are ovoid and transparent, the longest diameter being about $\frac{1}{800}$th of an inch, the spores being borne upon slender transparent threads not half the above diameter. Having fallen upon a suitable nidus, with favourable circumstances of humidity, the outer coat of the spores becomes ruptured, and the corrosive plasma which they contain is set free. This mycelium speedily penetrates the epidermis of the leaf and permeates the cellular tissue in every direction, destroying the cells, and ultimately sending out fertile branches through the breathing spores of the leaf into the air. These branches, which are swollen in a moniliform manner (probably owing to repeated abortive attempts to produce fruit), terminate with spores at their extremities. Now whilst the mycelium is traversing the leaf-stalks and stems and hurrying on to the tuber itself, to blacken and consume the starch, the free fungus is all the while producing its spores, and amongst the spores bodies of a larger size which



Peronospora infestans, in situ × 225 dia. (camera lucida).

A B, thickness of lamina of leaf; C C, stomata, or breathing pores; D D, hairs on leaf; E E, threads of *Peronospora infestans*; F F, spores; G G, privileged spores, containing ciliated zoospores, which are seen free at H H.

may be termed privileged spores, which mature within themselves (by a differentiation of their contents) certain reproductive ciliated bodies called zoospores. When these zoospores are ripe and set free, they move about in moisture by means of two cilia with great rapidity, and either work their way into the breathing pores or germinate at once on the leaf surface, and penetrate the cuticle like the ordinary spores. In the wet dewy mornings of early autumn, when all the leaves are moist, it will be seen that a few infected plants may be the means of rapidly diffusing the disease (by means of these ciliated zoospores alone) over a very large expanse of cultivated ground. Now if we could imagine the whole of Britain free from the disease and a single infected field somewhere in France or Germany, a single puff of wind would send the spores over to us, and we should at once be as badly off as if we had suffered from the disease from the first.

As the murrain never comes upon us till a certain time in the early autumn, the most reasonable suggestion for exterminating the disease seems to be to cultivate those early varieties of potato which mature their fruit before the fungus makes its attack, and so *evade* the disease ; but it will be seen singularly enough, from our actual experiments, that it was a positively *late* potato, and not an *early one*, which entirely warded off the murrain, whilst the earlier varieties succumbed. And so we leave the subject. We can state facts, and call the facts by names; but as to what the disease is, we can say little more than that the potato disease is the potato disease, as cholera is cholera.

AIR BENEATH THE MICROSCOPE.

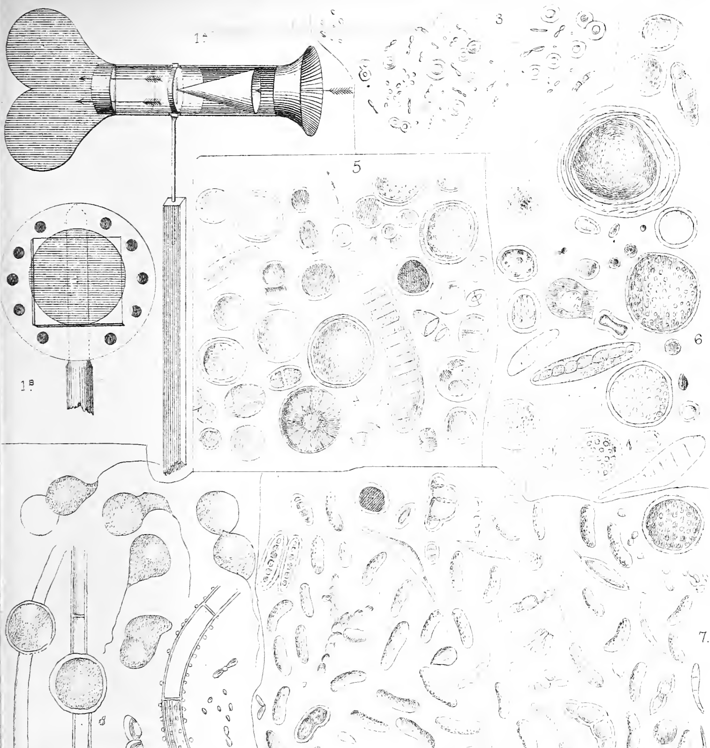
By the EDITOR.

PLATE CIX.

THE world may be said to consist—from one point of view at least—of an earthly soil and two great seas: one the aqueous ocean, and the other the atmosphere. Fish and a few other vertebrate, and many invertebrate, animals live in the watery ocean; but man, most mammalia, birds, and a very large number of invertebrate animals, inhabit the aerial one. And just as in the seas there are a certain number of animals that are compelled to inhabit the bottom alone, so in the aerial ocean we find the same state of things—one group of animals having to do with the land, while another set have the wide range of the atmosphere in which to carry on their lives. Of course this division is well enough known already; but the fact is by no means so familiar, that the very air we breathe is laden with inhabitants of both the animal and vegetable kingdoms, and in reality is very thickly populated and planted, although we have not ordinarily the power of recognising that this is so.

However, the microscope introduces us to quite a novel world; and now that its powers have reached almost the highest mark, now that practical art has nearly realised the all that is possible in the theoretical world of microscopy, now that microscopically the whole Bible might be written out—every verse in every chapter—about six or seven times within the space of a single square inch, in these days of objectives not only of $\frac{1}{25}$th or $\frac{1}{50}$th, but even of the $\frac{1}{75}$th* of an inch, we are by instrumental means introduced to an entire world of which a couple of hundred years ago we had not the faintest idea. The researches which we are about to describe, however, have not been conducted with the highest powers, and from that circumstance they are liable to serious error; inasmuch as it may

* It is worthy of note that not only have Powell and Lealand brought out a $\frac{1}{75}$ object-glass of really wonderful definition, but this has also been achieved by the American workers, Messrs. Tolles and Stodder.



The Aeroscope and the Air contents.

be very fairly contended, by those who are opposed to the views laid down by the men who have written upon the subject, that from the use of inferior powers a whole world of animal life was cut off from their gaze; but of this more hereafter.

We have now to examine the question, what does the air contain? And of course this is a difficult problem, from the very different nature of the questions that may be put. For instance, we must say in the first case, supposing that the reader understood that the air consists of 77 per cent. of nitrogen and 23 per cent. of oxygen, with a certain faint proportion of carbonic acid, ozone, &c., &c., that three distinct questions present themselves at the outset. 1st, Is there any difference in the results of examination of air as to the locality which is explored? 2nd, Is there any difference as to the time at which our exploration is conducted? and 3rd, Is there any distinction to be made in regard to the time at which the atmosphere is examined? And these three questions may be replied to generally in the negative—of course, making every allowance for trifling distinctions dependent on differences of temperature, and upon the effects of heavy falls of rain. The question naturally arises to one who is inexperienced with the microscope, how are you to examine the air? If you look through a microscope simply at the atmosphere you will certainly see nothing, no matter how high your power. Well, then, it is necessary, in a certain sense, to do what is equivalent to condensing the air; that is to say, we must have some means of examining at one glance several thousand cubic feet of the atmosphere. And how is this done? By means of a very simple instrument which has been termed the aeroscope (see fig. 1, plate CIX). Of course it must not be supposed that this is our only means, for we have the dust on leaves, and the specimens collected by means of moist-glass slides. But for all practical purposes the aeroscope furnishes us with a handy, easy, and most satisfactory apparatus for the investigation of almost any quantity of atmospheric air. And what is the *aeroscope?* It is an instrument whose use is to expose a glass slide in such a manner that a current of atmospheric air shall pass over it with considerable rapidity, and in such a fashion as to leave nearly all of its abnormal contents upon the slide. The instrument which is figured in our plate was originally devised by Dr. Maddox, hon. F.R.M.S., of Southampton, a gentleman well known for his investigations into the microscopic inhabitants of the atmosphere. But it has been very slightly modified by Dr. D. Cunningham, of Calcutta,* whose researches on the microscopic inhabitants of the air have been the fullest

* "Microscopic Examinations of Air." By D. Douglas Cunningham, M.B., Surgeon H.M. Indian Medical Service. Published by Government, 1874.

yet published. From his work—the latest, as it is unquestionably the fullest and most admirably illustrated volume on the subject—we perceive that his modification of the original aeroscope has been but very slight. It appears (A, fig. 1) to consist of three thin brass tubes, two of which slip over the third central one, and come into contact with the opposite sides of a projecting rim on its circumference. This rim is formed by the margin of a diaphragm which divides the central tube into two chambers. "It is of sufficient thickness to allow of a spindle passing up through it (fig. 1, B). The latter ends in a pointed extremity, which comes in contact with the upper end of the bearing, and provides for the free rotation of the system of tubes. Round the margin of the diaphragm there is a set of perforations to allow of the passage of air through it, and in the centre of its anterior surface there is a square plate of brass, with a slightly projecting rim on its lower margin. The anterior of the two lateral tubes is provided with an expanded orifice, and contains a small finely-pointed funnel in its interior; the pointed extremity opening immediately in front of the centre of the diaphragm plate. The posterior tube is quite simple, and has a good-sized fish-tail vane fitted into a slit in its extremity." Such is the construction of the aeroscope, and when used, as it was most extensively by Dr. Cunningham in his late inquiries, it is placed at a height of about 4½ ft. from the ground, and is allowed to revolve by means of a joint in such a manner that, owing to the vane, it is invariably exposed with its mouth to the wind. Hence if it is left, as it usually is, in one place for 24 hours, and the wind is blowing at even a rate of three or four miles an hour, the quantity of air that must traverse it is indeed most considerable.

And now, how is the capture of the wind's contents provided for? Very simply indeed. A microscopic covering-glass is carefully cleaned, and one of its surfaces smeared over with pure glycerine. A minute drop of the same is placed on the diaphragm-plate, and the dry surface of the cover-glass applied to it, leaving the smeared side exposed. (This did away with the necessity for springs.) Then the anterior tube being put on, it was ready for use. Now it is perfectly clear that with such an instrument it is easy to examine any amount of air, and its use has been often tried by Dr. Cunningham; with what results we shall speak of presently. But in the first instance it will be as well to describe some of the consequences that have followed on attempts in the same direction by other workers, and to divide them into those who connect the contents of the atmosphere at certain seasons with certain infectious or contagious diseases, and those who take quite an opposite view of the matter.

Among recent writers who have devoted much time and attention to the influences of atmospheric contagion, unquestionably he who takes the highest rank is Mr. Blackley.* It is to be regretted that he has confined his attention merely to one class of disease alone. But in compensation for this we have the very extended nature of his inquiries and the elaborate researches that he has made. Mr. Blackley considers that he has proved that hay-fever is decidedly caused by the patient's inhalation of air containing a considerable quantity of pollen; that this pollen adheres to the membranous lining of the larynx and air-passages and the nasal lachrymal membranes, and thus causes the excessive secretion which is so troublesome an effect of these diseased conditions. And whatever view we may take of this part of his book, he has by the most ingenious microscopical contrivances proved beyond the slightest question that a very large amount of pollen is inhaled from the air, at all events in certain seasons.

Dr. Beale, than whom there is no one of profounder experience in microscopic research, has given his testimony in favour of the view that the atmosphere contains the germs at least of certain diseases. He examined the infected air of the cattle-plague period, which had been obtained by Mr. Crookes, F.R.S., and he found that the fluid obtained from saturating the cotton-wool exposed to the air, in glycerine, gave undoubted evidence of fungus sporules. And he even goes so far as to throw out the suggestion that malarial poison may possibly be a degraded type of the bioplasm of lower animals and plants.

Another observer in this very wide field is Dr. Salisbury, of the United States of America. And his researches were very accurately made, and lead to conclusions most favourable to those who believe in the atmosphere's swarming with microscopic life, and in the possibility of disease being so carried from one place to another. Dr. Salisbury's† observations were made in the year 1862, when there was a great prevalence of intermittent and remittents fevers in the low valleys of the Ohio and Mississippi. These maladies appeared, says Dr. Cunningham, in the month of May, and were very prevalent in July and August. The season was a very wet one up to the end of June, but there was no rain in July, August or September, and with the cessation of the rain the increase in the number of cases of fever occurred. Dr. Salisbury in the first place examined saliva and mucus from the mouth and *nares* of the sufferers, and detected the presence of large numbers of zoo-

* "Experimental Researches on the Causes and Nature of Catarrhus Æstivus" (Hay-fever, or Hay-asthma). By C. H. Blackley, M.R.C.S., 1872.

† "American Journal of Medical Science," April 1866.

spores, animalcules, diatoms, desmids, cells and filaments of algæ, and spores of fungi. The only bodies which were of constant occurrence, and generally in great abundance, were small oblong cellules, which were either isolated or grouped in masses. They had a distinct nucleus contained within a smooth cellular envelope. He decided that they were algal cellules chiefly resembling palmellæ, and found that they only occurred in malarial districts.

He then proceeded to look for them in the air, his method of procedure being to suspend pieces of glass over marshy pools and swampy places. The glasses were set in the evening and removed before sunrise next morning. Drops of water were found adhering to their under surfaces and containing numerous cells of various kinds, but none resembling the peculiar palmelloid cellules previously alluded to. These were, however, constantly present in considerable numbers on the upper surfaces. His next step was to endeavour to ascertain from what source they were derived; and, after a considerable amount of fruitless search, he discovered it in a sort of greyish mould covering the recently exposed surfaces of cracks in rich prairie ground, which had been recently dried and was much broken up by the feet of cattle. On suspending glasses over places covered by this mould, he found numbers of the cellules in the fluid on their under surfaces.

In following out these experiments, Dr. Salisbury came to the conclusions that cryptogamic spores rise chiefly during the night and fall shortly after sunrise; that the height to which the cellules in question rose was 30 to 100 feet from the surface; that none of them were present during the day; that covering the soil to a depth of several inches with straw or quicklime prevented their rise; that a stay of fifteen minutes in places in which they abounded gave rise to dryness and febrile heat of the throat coincident with their presence in the pharyngeal mucus; and that persons exposed to their inhalation, even far from their original source, under entirely different circumstances, in non-malarial districts, suffered from attacks of fever as a consequence.

We hardly think that there could be more convincing evidence than this. But then it has to be borne in mind that the district under examination was somewhat an exceptional one.

The observations of Dr. Swayne and Mr. Brittan, though they were called in question by the subsequent Report of the Royal College of Physicians, similarly give evidence in favour of this view of the matter. They go to show that in cholera times there exist in the atmosphere peculiar cells, whose structure, as far as they could see, was identical with that of the well-known annular cells from choleraic fæcal discharges. They went so far as to publish figures of these cells, which they alleged were absent from localities which had no cholera cases in their neighbourhood. Dr. Ransome, again, gives his testimony in favour of the view that the air we breathe contains numerous organisms. In a paper read before the Philosophical Society of Manchester, in the year 1870 (and reported in these pages), he stated that an examination of the air showed that in cases of diphtheria numerous greenish conferval filaments appeared; and in the air collected from the neighbourhood of cases of measles, whooping-cough, and phthisis abundant examples of small round confervoid cells were found. At the same meeting at which the above statement was made, Dr. H. Browne alleged that he had obtained nearly the same results.

Beside these, there are several others on the same side. Signor Selmi, of Mantua, states that he has examined the moisture proceeding from the air of marshes, and found that it consisted in a considerable proportion of spores of algæ and active infusoria. Again, another observer, Dr. Balestra, has made important observations on the air in the Pontine marshes of Rome; and he states that there are in it the spores, of a greenish yellow colour and $\frac{1}{1000}$ mm. in diameter, of a minute species of algæ, and that these, he has no doubt, are the cause of those numerous cases of intermittent fever which are almost characteristic of the locality. M. Reveil also shows that the air in the wards of the St. Louis Hospital was, when collected by the microscope, abundantly charged with epithelial cells and various organic corpuscles. Evidence in favour of this view is also given by M. V. Poulet, who states ("Comptes Rendus," vol. lxv.) that he has found abundant quantities of *Bacterium termo*, *Monas termo*, and *B. bacillus*, in the air which had been breathed by persons who were suffering from common whooping-cough.

The annual Reports * from the Army Medical Department, too, give us a good deal of evidence; and it is the more valuable, not from the fact of its leaning to one side more than another, as from the circumstance that the investigations recorded have been invariably conducted with that strict regard for truth which renders them valuable as impartial accounts. The statements vary a little, according to the observer that makes them, but some of them point distinctly to the fact that the air is decidedly impure from the nature of its contents. For instance, Dr. de Chaumont states that 120 cubic feet of air were found to contain "epithelium in large quantity, hair and various fibres, sand, soot, crystalline sub-

* "Sanitary and Medical Reports of the Army Medical Department for 1867."

stances, and *sporangia of fungi and monads in considerable quantity.*" Mr. Dancer's * researches on the air of Manchester are also of great interest, more especially as he gives an estimate of the amount of spores present in the specimens of air examined. He says that there were *in* 2,495 *litres of the atmosphere* (an amount that would be breathed by any ordinary individual in about ten hours) *no less than* 37,500,000 *spores, exclusive of other materials.* And besides those whose researches we have mentioned, there are many others whose inquiries all point in the same direction.

But it must be confessed that the opposite side is, in regard to the absence of spores originating epidemic diseases, not devoid of evidence also. It can name no less authorities than Ehrenberg, the greatest infusorial investigator in the world; Pouchet, the celebrated supporter of the well-known doctrine of spontaneous generation; the Royal College of Physicians; and MM. Joly and Musset; besides various other less celebrated authorities. But the first can only be cited as opposed to the idea that there are the germs of infectious diseases in the atmosphere; for he shows most conclusively, by infinitely the most extensive of researches, that the air contains an amount of animal life that is something extraordinary. But, says Dr. Cunningham,† when speaking on this part of the subject, "no special forms of infusoria or spores were to be found in the atmospheric dust during the epidemic of cholera in 1848." So we may almost count Ehrenberg on the other side, i.e. among the anti-contagionists. In point of fact the only men of great reputation upon this side are MM. Pouchet, Joly, and Musset. Their researches are really most remarkable, for they seem to have observed none whatever of the hosts seen by numerous other workers. Of course they did find (more especially Pouchet) some corpuscles and a few other organisms, but nothing at all to account for the multitude that were developed in water exposed to the air.

Now we must remember that these three (Pouchet, Joly, and Musset) were the special advocates of spontaneous generation, and that therefore they could not be expected to find an ample supply of animal and vegetable life already existing in the atmosphere. But it may be said we have no right to accuse them of falsity, and therefore we declare that we do not do so; but what we do say is, that persons who were engaged in a desperate argument against the existence of these germs in the atmosphere, were not the most likely persons to find them in that position.

* They were published in the "Proceedings of the Literary and Philosophical Society of Manchester."

† "Microscopic Examinations of Air," 1874.

At all events, such is the evidence *pro* and *con.* We have a great deal of valuable testimony in favour of the existence of animal and vegetable life in the atmosphere, and a certain proportion of that testimony goes to show that contagious and infectious diseases may have their origin in atmospheric causes; whilst, on the other hand, we have hardly a tenth part of witnesses in favour of the view that the atmosphere is not a centre of animal and vegetable life, and that diseases do not spread through the assistance of atmospheric germs. We are now in a position to examine the latest evidence that has been offered on the subject, and to raise one or two objections that the whole method of investigation seems to us to be liable to.

We must now examine Dr. Cunningham's labours. These appear to us to be most valuable, and he has published them in a form which, for completeness of detail, for skilful arrangement, and for excellence and number of illustrations, has certainly no equal, at least in our language. Of course the reader must be referred to the work itself for the minute accounts. We can do little more than sketch in a most imperfect manner some of the results at which he has arrived. One of the first experiments he describes is of interest, as it shows how many different series of animal forms may arise in succession in the very same specimen of water which has been at first obtained from rain collected at a considerable height above the ground (having taken every precaution against uncleanliness), and which has been kept for several days perfectly free from the admission of the external air. In one specimen (No. II. p. 42 of Dr. Cunningham's work) the author found, seventy-two hours after it had been sealed, an ample amount of mycelial elements. Then two days subsequently (the specimens being regularly sealed after examination) he found an abundance of conidia. It was then sealed again, left for six days more, with the following results (see fig. 2, Plate CIX.):—"The filaments were then greatly disintegrated, but the cysts remained very distinct, and were present in abundance. They contained a mass of protoplasm marked with one or two granules and clear spaces, the latter of which could be seen to alter in number and position. In several instances these protoplasmic bodies were observed gradually to work their way out of their cysts, which were then left behind as extremely delicate rings, hardly visible save with careful examination. The process was comparatively slow, and the escaping zoospores, for such they seemed to be, *showed a well-developed flagellum in active motion for some time before they were entirely free.* Once detached in the fluid they moved actively about by means of the flagellum as well as by free amœboid extensions of their substance, and in many cases the flagellum temporally or permanently disappeared, so that, had the process not been actually observed to take place, the two conditions might have been regarded as belonging to distinct organisms. In other parts of the preparation, the mycelial threads had resolved themselves into innumerable conidial cells, while in others they were more or less completely decomposed into gelatinous masses containing granules and bacteroid staves."

Several other experiments of a similar kind were made, with pretty nearly the same results. The only question which here occurs to the author is that concerning the origin of bacteria; but that I think he gets over very fairly. The conclusions which he states are as follows :—1. Specimens of rain-water in Calcutta, collected with every precaution to ensure their freedom from contact contamination, sooner or later frequently show the presence of spores, mycelium, zoospores, monads, bacteroid bodies, and distinct bacteria. 2. They do not, as a rule, contain any of the higher forms of infusoria. 3. The zoospores are demonstrably derived from the mycelium arising from common atmospheric spores. 4. There is every probability that the monads and bacteria have a similar origin, but it remains quite uncertain whether their development is due to heterogenesis, or to the presence of their germs within their parent cells, or as the result of a process of normal development in the latter.

The next subject examined by Dr. Cunningham is that relating to the microscopic character of the air in sewers. He placed his aeroscope in such a position that it would have an opportunity of examining the air proceeding from the sewage of Calcutta, but the results he obtained were not very important (see fig. 3, Plate CIX.). He found as a rule nothing but bacteria and the spores, most probably, of aspergillus. And both these he thus accounts for. The bacteria, he says, are found here more readily than in the outer air, because the moisture renders them more readily seen; inasmuch as they change their form under the influence of drought, they are less observed in ordinary preparations. As to the presence of aspergillus, and that alone, he thinks it is due to the fact that of course other fungal spores could not exist in the sewer atmosphere, while aspergillus meets there the damp atmosphere and luxuriant organic matter which it requires.

The other specimens figured in our plate are copies of some few of those which Dr. Cunningham has collected by his aeroscope, which was exposed for 24 hours each day during the months of April, May, and June, 1872, at the Presidency Jail, and at a similar locality in Alipore. The results of these gatherings are not so strikingly remarkable as we should have expected, possibly for a reason that we shall refer to further on;

still, some of them are not devoid of interest, as the following remarks will show. Infinitely the greater number are apparently referable to the fungous orders, *Sphæronemei*, *Melanconei*, *Torulacei*, *Dematei*, *Mucedines*, *Pucciniæi*, and *Ceomacei*. Among these by far the most interesting was a specimen of the *Torulacei*, a species of the rare genus *Tetroploa* (see fig. 4, Plate CIX.). This was found only in two instances. The next curious specimen was one of a peculiar green algoid cell. This was obtained from Alipore. The specimen shows a number of green cells (see fig. 5), circular in outline, of various sizes, and showing a division of their contents into from two to four separate piles. The presence of these Dr. Cunningham apparently attributes to a tank in the neighbourhood, so that if this view be correct they have no real bearing on the case. However, we may suppose as at least possible that they were not due to the tank at all. Next in order is another peculiar specimen, which is supposed to be the pollen of some species of lily (see fig. 6). Of course its peculiarity in this instance is simply its rarity of occurrence.

Finally we come to the specimens which more than any other have an especial importance, and these are the ones represented in figs. 7 and 8 (Plate CIX.). They are of importance because they are peculiar, and because they are from two different localities, and from both of these places at the time when cholera was present largely. It is of interest to note the number of the peculiar curved spores that appear in both these cases, and to observe that these spores did not appear before when cholera was not present. They would, indeed, lead one to believe in them as the parents of cholera. But unhappily Dr. Cunningham's inquiries, which were conducted for a long time subsequently to this observation, show that these peculiar spore-like bodies did not appear in many other cases in which the mortality from cholera was even greater than it was during their presence; thus showing, as he thinks—with very good reason we imagine—that it was only because their period of growth was at the time of the first experiment, that they were present, and that their absence was due not to the absence of cholera which raged violently at the time, but to the fact that their peculiar season had passed by. And, as he very properly observes, it shows the necessity for the carrying out of researches all the year round; for unquestionably if his inquiries had stopped at the period of finding these specimens, a very different conclusion would inevitably have been framed.

Although we have been unable to give even the briefest account of Dr. Cunningham's inquiries upon this subject in many of the channels he has adopted, we may nevertheless give the following conclusions which he has laid down, as they enable the observer to perceive the different lines of research he has adopted:—1. The aeroscope affords a very convenient method for obtaining specimens really representing the nature of the true atmospheric dust. 2. Specimens of dust washed from exposed surfaces cannot be regarded as fair indices of the constituents of atmospheric dust, since they are liable to contain bodies which may have reached the surface otherwise than by means of the air, as well as others which are the result of local development. 3. Specimens collected by gravitation also fail to indicate the nature and amount of organic cells contained in the atmosphere, as the heavier amorphous and inorganic constituents of the dust are deposited in relative excess due to the method of collection. 4. Dew also fails to afford a good means of investigating the subject, as it is impossible to secure that all the bodies really present in a specimen of it should be collected into a sufficiently small space; and, moreover, because it is liable to accidental contaminations, and also affords a medium in which rapid growth and development are likely to take place. 5. Distinct infusorial animalcules, their germs or ova are almost entirely absent from atmospheric dust and even from many specimens of dust collected from exposed surfaces. 6. The cercomonads and amœbæ appearing in certain specimens of pure rain-water appear to be zoospores developed from the mycelial filaments arising from common atmospheric spores. 7. Distinct bacteria can hardly ever be detected among the constituents of atmospheric dust, but fine molecules of uncertain nature are almost always present in abundance; they frequently appear in specimens of rain-water collected with all precautions to secure purity, and appear in many cases to arise from the mycelium developed from atmospheric spores. 8. Distinct bacteria are frequently to be found amongst the particles deposited from the moist air of sewers, though almost entirely absent as constituents of common atmospheric dust. 9. The addition of dry dust (which has been exposed to tropical heat) to putrescible fluids is followed by a rapid development of fungi and bacteria, although recognisable specimens of the latter are very rarely to be found in it while dry. 10. Spores and other vegetable cells are constantly present in atmospheric dust, and usually occur in considerable numbers; the majority of them are living and capable of growth and development: the amount of them present in the air appears to be independent of conditions of velocity and direction of wind; and their numbers are not diminished by moisture. 11. No connection can be traced between the numbers of bacteria, spores, &c., present in the air and the occurrence of diarrhœa, dysentery, cholera, ague or dengue; nor between the presence or abundance of any special form or forms of cells, and the prevalence of any of these diseases. 12. The amount of inorganic and amorphous particles and other débris suspended in the atmosphere is directly dependent on conditions of moisture and of velocity of wind.

Dr. Cunningham concludes his observations with some valuable suggestions as to the reasons why his inquiries differ from those of Ehrenberg. For example, he shows that the methods employed by the two observers were totally distinct. The German worker obtained nearly all his specimens, not *directly* from the air, but from leaves, pieces of bark, bits of moss, &c. But of course this is a most unreliable mode of operating; for when surfaces have been wetted with rain, there is no difficulty for rotifers, tardigrades, and so forth, to make their way over them to a considerable extent; and it is this circumstance, doubtless, which gave the foreign observer such a wide zoological range to his collection. Dr. Cunningham's mode could alone gather what was actually in the air; hence his series of specimens have not been so wonderfully strange.

The author of the last work on the subject suggests in his final remarks that these collections might, if kept and then studied, have formed a different tale for the observing student. And here, doubtless, he has hit upon the right cord. We greatly regret that the idea did not occur to him before, as then it might have enabled him to give us something more worthy of a genuine microscopical student. It is, then, in absence of this and of another fact that, it appears to us, the author has, through omission, committed a serious error.

Two questions arise from the series of researches which he and others have conducted, and which have been described in the earlier pages; and these are—1. Are there always in the atmosphere a quantity of animal and vegetable germs ready, at any moment when offered the favourable conditions of development, to come into existence? 2. Do a certain number of these (animal or vegetable) promote those various epidemics and other diseases, so terrible in their manifestations, and which come and go in many instances just as a cloud of minute particles might be driven by the wind? The first of these questions is, it seems to us, partly answered in the affirmative; and it only requires the employment of the highest powers and the necessity of most careful watchmanship on the part of the student in order to have a complete and decisive reply. The Rev. Mr. Dallinger, F.R.M.S., has shown us by his recent inquiries ("Monthly Microscopical Journal," January, February, and March, 1874) how a whole host of organisms may completely escape observation by the use of powers even so high as those Dr. Cunningham has employed. It is only, then, by the employment of objectives of the $\frac{1}{25}$-inch and

$\frac{1}{50}$-inch and the highest eye-pieces that we can hope for any useful result. And, furthermore, as Mr. Dallinger has pointed out, most careful and continuous observation must be carried on. Assuredly if he (Mr. Dallinger) had not adopted the continuous method of study, he would have described as three or more distinct individuals what were merely the successive stages of development of one and the same being. It is almost absurd to see, as we do, in the controversy which took place some few years ago between M. Pasteur and M. Pouchet, reference to the employment of a power actually of 150 *diameters!!!* Why, a power of 150 diameters would be altogether inadequate to such labours as those that are demanded; it would be as much, and even more, out of place than Malpighi's lenses would be if compared with the powers of the microscopic anatomist of to-day.

As to the second point of inquiry, that has been much less fully worked out. Although Mr. Blackley and a few others have attempted to show the immediate connection between disease and microscopic organisms, much less has been done than the subject demands. And there can be no doubt that it only awaits some one who, like Mr. Dallinger, will bring intense patience and the highest powers of the microscope to bear on it, to enable the subject to be completely cleared up.

At all events, we can congratulate the workers on the useful labour that they have given to the subject, and we must only hope that in a few years more, we shall be able to see more clearly that immense surface which is now as it were a barren plain with an immensity of exquisite *mirages* spread above it.

ON THE TRANSMISSION OF SOUND BY THE ATMOSPHERE.

By JOHN TYNDALL, D.C.L., LL.D., F.R.S.

THIS notice embraces the scientific results of an inquiry on fog-signals, undertaken at the instance of the Elder Brethren of the Trinity House, and communicated, with their friendly concurrence, to the Royal Society.

The investigation was begun on May 19, 1873, and continued till July 4. It was resumed on October 8, and continued to the end of November. It also includes observations made during the dense fog which enveloped London on December 9 and the succeeding days.

Gongs and bells were excluded from this investigation, in consequence of their proved inferiority to other instruments of signalling. The experiments were made with trumpets blown by powerfully compressed air, with steam-whistles, guns, and a steam-syren, associated with a trumpet 16 feet long.

Daboll's horn, or trumpet, had been highly spoken of by writers on fog-signals. A third-order apparatus of the kind had been reported as sending its sound to a distance of from 7 to 9 miles against the wind, and to a distance of 12 to 14 miles with the wind. Holmes had improved upon Daboll; and with two instruments of Holmes—not of the third, but of the first order—our experiments were made. On May 19, at 3 miles distance, they became useless as a fog-signal; at a distance of 4 miles, with paddles stopped and all on board quiet, they were wholly unheard. At a distance of 2 miles from the Foreland the whistles tested on May 19 became useless. The twelve o'clock gun, fired with a 1 lb. charge at Drop Fort in Dover, was well heard on May 19, when the horns and whistles were inaudible. On May 20 the permeability of the atmosphere had somewhat increased, but the steam-whistle failed to pierce it to a depth of 3 miles. At 4 miles the horns, though aided by quietness on board, were barely heard. By careful nursing, if I may use the expression, the horn-sounds were carried to a distance of 6 miles. The superiority of the 18-pounder gun, already employed by the Trinity House as a fog-signal, over horns and whistles, was on this day so decided as almost to warrant its recommendation to the exclusion of all the other signals.

Nothing occurred on June 2 to exalt our hopes of the trumpets and whistles. The horns were scarcely heard at a distance of 3 miles; sometimes, indeed, they failed to be heard at 2 miles. By keeping everything quiet on board, they were afterwards heard to a distance of 6 miles—a result, however, mainly due to the improved condition of the atmosphere. Considering the demands as to sound-range made by writers on this subject, the demonstrated incompetence of horns and whistles of great reputed power to meet these demands was not encouraging. On June 3 the atmosphere had changed surprisingly. It was loaded overhead with dark and threatening clouds; the sounds, nevertheless, were well heard beyond 9 miles. On June 10 the acoustic transparency of the air was also very fair, the distance penetrated being upwards of $8\frac{3}{4}$ miles. The subsidence of the sound near the boundary of the acoustic shadow on the Dover side of the Foreland, and before entering the shadow, was to-day sudden and extraordinary, affecting equally both horns and guns. We were warned on June 3 that the supremacy of the gun on one occasion by no means implied its supremacy on all occasions, the self-same guns which on May 20 had so far transcended the horns being on this day their equals and nothing more. June 11 was employed in mastering still further the facts relating to the subsidence of the sound east and west of the Foreland; the cause of this subsidence being in part due to the weakening of the sonorous waves by their divergence into the sound shadow, and in part to interference.

The atmosphere on June 25 was again very defective acoustically. The sounds reached a maximum distance of $6\frac{1}{2}$ miles; but at 4 miles, on returning from the maximum distance, they were very faint. The day had, as it were, darkened acoustically. On this day the guns lost still further their pre-eminence, and at $5\frac{1}{2}$ miles were inferior to the horn. No sounds whatever reached Dover Pier on the 11th; and it was only towards the close of the day that they succeeded in reaching it on the 25th. Thus by slow degrees the caprices of the atmosphere made themselves known to us, showing us subsequently that within the limits of a single day, even within a single minute, the air, as a vehicle of sound, underwent most serious variations. June 26 was a far better day than its predecessor, the acoustic range being over $9\frac{1}{4}$ miles. The direction of the wind was less favourable to the sound on this day than on the preceding one, plainly proving that something else than the wind must play an important part in shortening the sound-range.

On July 1 we experimented upon a rotating horn, and heard its direct or axial blast, which proved to be the strongest, at a distance of $10\frac{1}{2}$ miles. The sounds were also heard at the Varne light-ship, which is $12\frac{3}{4}$ miles from the Foreland. The atmosphere had become decidedly clearer acoustically, but not so optically; for on this day thick haze obscured the white cliffs of the Foreland. In fact, on days of far greater optical purity, the sound had failed to reach one-third of the distance attained to-day. In the light of such a fact, any attempt to make optical transparency a measure of acoustic transparency, must be seen to be delusive. On July 1 a 12-inch American whistle, of which we had heard a highly favourable account, was tried in place of the 12-inch English whistle; but, like its predecessor, the performance of the new instrument fell behind that of the horns. An interval of twelve hours sufficed to convert the acoustically clear atmosphere of July 1 into an opaque one; for on July 2 even the horn-sounds, with paddles stopped and all noiseless on board, could not penetrate further than 4 miles.

Thus each succeeding day provided us with a virtually new atmosphere, clearly showing that conclusions founded upon one day's observations might utterly break down in the presence of the phenomena of another day. This was most impressively demonstrated on the day now to be referred to. The acoustic imperviousness of July 3 was found to be still greater than that of the 2nd, while the optical purity of the day was sensibly perfect. The cliffs of the Foreland could be seen to-day at ten times the distance at which they ceased to be visible on the 1st, while the sounds were cut off at one-sixth of the distance. At two P.M. neither guns nor trumpets were able to pierce the transparent air to a depth of 3, hardly to a depth of 2 miles. This extraordinary opacity we consider to arise from the irregular admixture with the air of the aqueous vapour raised by a powerful sun. This vapour, though perfectly invisible, produced *an acoustic cloud* impervious to the sound, and from which the sound-waves were thrown back as the waves of light are from an ordinary cloud. The waves thus refused transmission produced by their reflection echoes of extraordinary strength and duration. This we may remark is the first time that audible echoes have been proved to be reflected from an optically transparent atmosphere. By the lowering of the sun the production of the vapour was checked, and the transmissive power of the atmosphere restored to such an extent that, at a distance of 2 miles from the Foreland, at seven P.M., the intensity of the sound was at least thirty-six times its intensity at two P.M.

On October 8 the observations were resumed, a steam syren and a Canadian whistle of great power being added to the list of instruments. A boiler had its steam raised to a pressure of 70 lbs. to the square inch; on opening a valve this steam would issue forcibly in a continuous stream, and the sole function of the syren was to convert this stream into a series of separate strong puffs. This was done by causing a disk with 12 radial slits to rotate behind a fixed disk with the same number of slits. When the slits coincided a puff escaped; when they did not coincide the outflow of steam was interrupted. Each puff of steam at this high pressure generated a sonorous wave of great intensity; the successive waves linking themselves together to a musical sound so intense as to be best described as a continuous explosion.

During the earlier part of October 8 the optical transparency of the air was very great; its acoustic transparency, on the other hand, was very defective. Clouds blackened and broke into a rain and hail-shower of tropical violence. The sounds, instead of being deadened, were improved by this furious squall; and, after it had lightened, thus lessening the local noises, the sounds were heard at a distance of 7½ miles distinctly louder than they had been heard through the preceding rainless atmosphere at a distance of 5 miles. At 5 miles distance, therefore, the intensity of the sound had been at least doubled by the rain —a result entirely opposed to all previous assertions, but an obvious consequence of the removal by condensation and precipitation of that vapour the mixture of which with the air had been proved so prejudicial to the sound. On this day a dependence was established between the pitch of a note and its penetrative power—the syren generating 480 waves, being slightly inferior to the horns, while generating 400 waves a second it was distinctly superior. The maximum range on October 8 was 9 miles. On October 9 the transmissive power had diminished, the maximum range being 7½ miles. On both these days the syren proved to be superior to the horns, and on some occasions superior to the gun.

On the 10th and 11th, a gale having caused our steamer to seek safety in the Downs, we made land-observations. The duration of the aërial echoes was for the syren and the gun 9 seconds, for the horns 6 seconds. The duration varies from day to day. We sought to estimate the influence of the violent wind, and found that the sound of the gun failed to reach us in two cases at a distance of 550 yards to windward, the sound of the syren at the same time rising to a piercing intensity. To leeward the gun was heard at five times, and certainly might have been heard at fifteen times the distance attained to windward. The momentary character of the gun-sound renders it liable to be quenched by a single puff of wind; but sounds of low pitch generally, whether momentary or not, suffer more from an opposing wind than high ones. We had on the 13th another example of the powerlessness of heavy rain to deaden sound.

On the 14th the maximum range was 10 miles, but the atmosphere did not maintain this power of transmission. It was a day of extreme optical clearness; but its acoustic clearness diminished as the day advanced. In fact the sun was in action. We proved to-day that by lowering the pitch of the Canadian whistle, its sound, which had previously been inaudible, became suddenly audible. The day at first was favourable to the transmission of the longer sound-waves. After a lapse of three hours the case was reversed, the high-pitched syren being then heard when both gun and horns were inaudible. But even this state of things did not continue, so rapid and surprising are the caprices of the atmosphere. At a distance of 5 miles, at 3.30 P.M., the change in transmissive power reduced the intensity of the sound to at least one half of what it possessed at 11.30 A.M., the wind throughout maintaining the same strength and direction. Through all this complexity the knowledge obtained on July 3 sheds the light of a principle which reduces to order the apparent confusion.

October 15 was spent at Dungeness in examining the performance of Daboll's horn. It is a fine instrument, and its application was ably worked out by its inventor; still it would require very favourable atmospheric conditions to enable it to warn a steamer before she had come dangerously close to the shore. The direction in which the aërial echoes return was finely illustrated to-day, that direction being always the one in which the axis of the horn is pointed.

The 16th was a day of exceeding optical transparency, but of great acoustic opacity. The maximum range was only 5 miles. On this day the howitzer and all the whistles were clearly overmastered by the syren. It was, moreover, heard at 3½ miles with the paddles going, while the gun was unheard at 2½ miles. With no visible object that could possibly yield an echo in sight, the pure aërial echoes, coming from the more distant southern air, were distinct and long-continued at a distance of 2 miles from the shore. Near the base of the Foreland cliff we determined their duration and found it to be 11 seconds, while that of the best whistle echoes was 6 seconds. On this day three whistles, sounded simultaneously, were pitted against the syren, and found clearly inferior to it.

On the 17th four horns were compared with the syren and found inferior to it. This was our day of greatest acoustic transparency, the sound reaching a maximum of fifteen miles for the syren, and of more than sixteen for the gun. The echoes on this day were continued longer than on any other occasion. They continued for fifteen seconds, their duration indicating the atmospheric *depth* from which they came.

On October 18, though the experiments were not directed to determine the transmissive power of the air, we were not without proof that it continued to be high. From 10 to 10.30 A.M., while waiting for the blasts of the syren at a distance of three miles from the Foreland, the continued reports of what we supposed to be the musketry of skirmishing parties on land were distinctly heard by us all. We afterwards learned that the sounds arose from the rifle practice on Kingsdown beach, $5\frac{1}{2}$ miles away. On July 3, which, optically considered, was a far more perfect day, the 18-pounder, howitzer, and mortar failed to make themselves heard at half this distance. The 18th was mainly occupied in determining the influence of pitch and pressure on the syren-sound. Taking the fluctuations of the atmosphere into account, I am of opinion that the syren, performing from 2,000 to 2,400 revolutions a second, or, in other words, generating from 400 to 480 waves per second, best meets the atmospheric conditions. We varied the pressure from 40 to 80 lbs. on the square inch; and though the intensity did not appear to rise in proportion to the pressure, the higher pressure yielded the hardest and most penetrating sound.

The 20th was a rainy day with strong wind. Up to a distance of $5\frac{1}{2}$ miles the syren continued to be heard through the sea and paddle-noises. In rough weather, indeed, when local noises interfere, the syren-sound far transcends all other sounds. On various occasions to-day it proved its mastery over both gun and horns. On the 21st the wind was strong and the sea high. The horn-sounds, with paddles going, were lost at four miles, whilst the syren continued serviceable up to $6\frac{1}{2}$ miles. The gun to-day was completely overmastered. Its puffs were seen at the Foreland; but its sound was unheard when the syren was distinctly heard. Heavy rain failed to damp the power of the syren. The whistles were also tried to-day, but were found far inferior to the syren. On the 22nd it blew a gale, and the *Galatea* quitted us. We made observations on land on the influence of the wind and of local noises. The shelter of the Coastguard Station at Cornhill enabled us to hear gun-sounds which were quite inaudible to an observer out of shelter; in the shelter also both horn and syren rose distinctly in power; but they were also heard outside when the gun was quite unheard. As usual, the sound to leeward was far more powerful than those at equal distances to windward. The echoes from the cloudless air were to-day very fine. On the 23rd, in the absence of the steamer, the observations on the influence of the wind were continued. The quenching of the gun-sounds, in particular to windward, was well illustrated. All the sounds, however, gun included, were carried much further to leeward than to windward. The effect of a violent thunderstorm and downpour of rain in exalting the sound was noticed by observers both to windward and to leeward of the Foreland. In the rear of the syren its range to-day was about a mile. At right angles to the axis, and to windward, it was about the same. To leeward it reached a distance of $7\frac{1}{3}$ miles.

On the 24th, when observations were made afloat in the steam-tug *Palmerston*, the syren exhibited a clear mastery over gun and horns. The maximum range was $7\frac{3}{4}$ miles. The wind had changed from W.S.W. to S.E., then to E. As a consequence of this, the syren was heard loudly in the streets of Dover. On the 27th the wind was E.N.E.; and the syren-sound penetrated everywhere through Dover, rising over the moaning of the wind and all other noises. It was heard at a distance of six miles from the Foreland on the road to Folkestone, and would probably have been heard all the way to Folkestone had not the experiments ceased. Afloat and in the axis, with a high wind and sea, the syren, and it only, reached to a distance of six miles; at five miles it was heard through the paddle noises. On the 28th further experiments were made on the influence of pitch, the syren when generating 480 waves a second being found more effective than when generating 300 waves a second. The maximum range in the axis on this day was $7\frac{1}{2}$ miles.

The 29th of October was a day of extraordinary optical transparency, but by no means transparent acoustically. The gun was the greatest sufferer. At first it was barely heard at 5 miles; but afterwards it was tried at $5\frac{1}{2}$, $4\frac{1}{2}$, and $2\frac{1}{2}$ miles, and was heard at none of these distances. The syren at the same time was distinctly heard. The sun was shining strongly; and to its augmenting power the enfeeblement of the gun-sound was doubtless due. At $3\frac{1}{2}$ miles, subsequently, dead to windward, the syren was faintly heard; the gun was unheard at $2\frac{3}{4}$ miles. On land the syren and horn-sounds were heard to windward at 2 to $2\frac{1}{2}$ miles, to leeward at 7 miles; while in the rear of the instruments they were heard at a distance of 5 miles, or five times as far as they had been heard on October 23.

The 30th of October furnished another illustration of the fallacy of the notion which considers optical and acoustic transparency to go hand in hand. The day was very hazy, the white cliffs of the Foreland at the greater distances being quite hidden; still the gun and syren-sounds reached on the bearing of the Varne light-vessel to a distance of 11½ miles. The syren was heard through the paddle-noises at 9¼ miles, while at 8½ miles it became efficient as a signal with the paddles going. The horns were heard at 6¼ miles. This was during calm. Subsequently, with a wind from the N.N.W., no sounds were heard at 6½ miles. On land, the wind being across the direction of the sound, the syren was heard only to a distance of 3 miles N.E. of the Foreland; in the other direction it was heard plainly on Folkestone Pier, 8 miles distant. Both gun and horns failed to reach Folkestone.

Wind, rain, a rough sea, and great acoustic opacity, characterized October 31. Both gun and horns were unheard 3 miles away, the syren at the same time being clearly heard. It afterwards forced its sound with great power through a violent rain-squall. Wishing the same individual judgment to be brought to bear upon the sounds on both sides of the Foreland, in the absence of our steamer, which had quitted us for safety, I committed the observations to Mr. Douglass. He heard them at 2 miles on the Dover side, and on the Sandwich side, with the same intensity, at 6 miles.

A gap (employed by the engineers in making arrangements for pointing the syren in any required direction) here occurred in our observations. They were resumed, however, on November 21, when comparative experiments were made upon the gun and syren. Both sources of sound, when employed as fog-signals, will not unfrequently have to cover an arc of 180°; and it was desirable to know with greater precision how the sound is affected by the direction in which the gun or syren is pointed.

The gun, therefore, was in the first instance pointed on us and fired, then turned and fired along a line perpendicular to that joining us and it. There was a sensible, though small, difference between the sounds which reached us in the two cases. A similar experiment was made with the syren; and here the falling off when the instrument was pointed perpendicular to the line joining us and it was very considerable. This is what is to be expected; for the trumpet associated with the syren is expressly intended to gather up the sound and project it in a certain direction, while no such object is in view in the construction of the gun. The experiments here referred to were amply corroborated by others made on November 22 and 23.

On both of these days the *Galatea's* guns were fired to windward and to leeward. The aërial echoes in the latter case were distinctly louder and longer than in the former. The experiment has been repeated many times, and always with the same result.

In front of the Cornhill Coastguard Station, and only 1¼ mile from the Foreland, the syren, on the 21st, though pointed towards us, fell suddenly and considerably in power. Before reaching Dover Pier it had ceased to be heard. The wind was here against the sound; but this, though it contributed to the effect, could not account for it, nor could the proximity of the shadow account for it. To these two causes must have been added an acoustically flocculent though optically transparent atmosphere. The experiment demonstrates conclusively that there are atmospheric and local conditions which, when combined, prevent our most powerful instruments from making more than a distant approach to the performance which writers on fog-signals have demanded of them.

On November 24 the sound of the syren pointed to windward was compared at equal distances in front of and behind the instrument. It was louder to leeward in the rear, than at equal distances to windward in front. Hence, in a wind, the desirability of pointing the instrument to windward. The whistles were tested this day in comparison with the syren deprived of its trumpet. The Canadian and the 8-inch whistles proved the most effective; but the naked syren was as well heard as either of them. As regards opacity, the 25th of November almost rivalled the 3rd of July. The gun failed to be heard at a distance of 2·8 miles, and it yielded only a faint crack at $2\frac{1}{2}$ miles.

Meanwhile this investigation has given us a knowledge of the atmosphere in its relation to sound, of which no notion had been previously entertained. While the *velocity* of sound has been the subject of refined and repeated experiments, I am not aware that since the publication of a celebrated paper by Dr. Derham, in the "Philosophical Transactions" for 1708, any systematic inquiry has been made into the causes which affect the *intensity* of sound in the atmosphere. Derham's results, though obtained at a time when the means of investigation were very defective, have apparently been accepted with unquestioning trust by all subsequent writers—a fact which is, I think, in some part to be ascribed to the *à priori* probability of his conclusions.

Thus Dr. Robinson, relying apparently upon Derham, says, "Fog is a powerful damper of sound," and he gives us physical reasons why it must be so. "It is a mixture of air and globules of water, and at each of the innumerable surfaces where these two touch, a portion of the vibration is reflected and lost." And he adds further on, "The remarkable power of fogs to deaden the report of guns has been often noticed."

Assuming it, moreover, as probable that the measure of "a fog's power in stopping sound" bears some simple relation to its opacity for light, Dr. Robinson, adopting a suggestion of Mr. Alexander Cunningham, states that "the distance at which a given object, say a flag or pole, disappears, may be taken as a measure of the fog's power" to obstruct the sound. This is quite in accordance with prevalent notions; and granting that the sound is dissipated, as assumed, by reflection from the particles of fog, the conclusion follows that the greater the number of the reflecting particles, the greater will be the waste of sound. But the number of particles, or, in other words, the density of the fog, is declared by its action upon light; hence the optical opacity will be a measure of the acoustic opacity.

This, I say, expresses the opinion generally entertained, "clear still air" being regarded as the best vehicle for sound. We have not, as stated above, experimented in really dense fogs; but the experiments actually made entirely destroy the notion that clear weather is necessarily better for the transmission of sound than thick weather. Some of our days of densest acoustic opacity have been marvellously clear optically, while some of our days of thick haze have shown themselves highly favourable to the transmission of sound. Were the physical cause of the sound-waste that above assigned, did that waste arise in any material degree from reflection at the limiting surfaces of the particles of haze, this result would be inexplicable.

Again, Derham, as quoted by Sir John Herschel, says that "falling rain tends powerfully to obstruct sound." We have had repeated reversals of this conclusion. Some of our observations have been made on days when rain and hail descended with a perfectly tropical fury; and in no single case did the rain deaden the sound; in every case, indeed, it had precisely the opposite effect.

But falling snow, according to Derham, offers a more serious obstacle than any other meteorological agent to the transmission of sound. We have not extended our observations at the South Foreland into snowy weather; but an observation of my own made on December 29, in the Alps, during a heavy snow-storm, distinctly negatives the statement of Derham.

Reverting to the case of fog, I am unable in modern observations to discover anything conclusive as to its alleged power of deadening sound. I had the pleasure of listening to a very interesting lecture on fog-signals, delivered by Mr. Beazeley before the United-Service Institution; and I have carefully perused the printed report of that lecture, and of a paper previously communicated by Mr. Beazeley to the Institution of Civil Engineers. But in neither of these painstaking compilations can I find any adequate evidence of the alleged power of fogs to deaden sound.

Indeed, during the discussion which followed the reading of Mr. Beazeley's paper, an important observation in an opposite sense was mentioned by Mr. Douglass, to whose ability and accuracy as an observer I am able to bear the strongest testimony. Mr. Douglass stated that he had found in his experience but little difference in the travelling of sound in foggy or in clear weather. He had distinctly heard in a fog, at the Smalls rock in the Bristol Channel, guns fired at Milford Haven, twenty-five miles away. Mr. Beazeley, moreover, has heard the Lundy-Island gun "at Hartland Point," a distance of ten miles, during dense fog. Mr. Beazeley's conclusion, indeed, accurately expresses the state of our knowledge when he wrote. In winding up his paper, he admitted "that the subject appeared to be very little known, and that the more it was looked into the more apparent became the fact that the evidence as to the effect of fog upon sound is extremely conflicting." When, therefore, it is alleged, as it is so often alleged, that the power of fogs to deaden sound is well known, the disjunctive *not* is to be inserted before the predicate.

The real enemy to the transmission of sound through the atmosphere has, I think, been clearly revealed by the foregoing inquiry. That enemy has been proved to be not rain, nor hail, nor haze, nor fog, nor snow—not water, in fact, in either a liquid or a solid form—but water in a vaporous form mingled with air so as to render it acoustically turbid and flocculent. This acoustic turbidity often occurs on days of surprising optical transparency. Any system of measures, therefore, founded on the assumption that the optical and acoustic transparency of the atmosphere go hand in hand must prove delusive.

There is but one solution to this difficulty; it is to make the source of sound so powerful as to be able to endure loss by partial reflection, and still retain a sufficient residue for transmission. Of all the instruments hitherto examined by us, the syren comes nearest to the fulfilment of this condition; and its establishment upon our coasts will, in my opinion, prove an incalculable boon to the mariner.

An account of the observations made during the recent fog will be included in the paper shortly to be presented to the Royal Society. These observations add the force of demonstration to others recorded in the paper, that fogs possess no such power of stifling sound as that hitherto ascribed to them. Indeed, the melting away of fog on December 13 was accompanied by an acoustic darkening of the atmosphere so great that, at a point midway between the eastern end of the Serpentine, where a whistle was sounded, and the bridge, the sound possessed less than one-fourth of the intensity which it possessed on the day of densest fog.

Thus, I think, has been removed the last of a congeries of errors which for more than a century and a half have been associated with the transmission of sound by the atmosphere. (This Paper was read before the Royal Society, Jan. 15, 1874.)

REVIEWS.

EVENINGS AT THE MICROSCOPE.*

A NEW edition of an old and excellent work has made its appearance. What shall we say of it? That it is a most admirable book cannot for a moment be denied, and that it is written by one who is no mere compiler, but is himself a thorough and complete master of his subject, are unquestionable facts. But still we have fault to find. We do not think that the author gave himself very much pains in his production of the new edition. And we say this because we do not find that much new matter has been added to the old one. We do not say that the old matter is not full of interest and valuable from a histological point of view, but we certainly should have expected a greater proportion of absolutely new matter in an issue of a work which first made its appearance fifteen years ago. We think that Mr. Gosse would have done infinitely better had he given the book to some experienced microscopist to bring out the new edition. However, as it is, the volume is an excellent one; in point of style it is wondrously clear, even to a reader who has no knowledge of science; for the author, having a thorough acquaintance with the subject, has been careful to avoid the introduction of technicalities, and has withal avoided the employment of that "free-and-easy" method which is too often used by the would-be popular author. With regard to illustration, we think Mr. Gosse is somewhat to blame for not introducing a greater number of cuts. The entire number is about 114 in all; there are no plates, and moreover the cuts, so far as we have seen, are badly worked, and reflect very little credit on the printer. It is when we come to compare the book for a moment, merely as to illustrations, with Dr. Carpenter's that we see how sadly deficient it is in this respect. Carpenter's has over 400 woodcuts interspersed with the text, and admirably "worked;" besides, it possesses no less than twenty-five exquisite plates. If we leave aside these objectionable features in the new edition of Mr. Gosse's book, we have much indeed to please us. For assuredly there are few more interesting studies than the microscopic structure of the various animals and their processes in the Invertebrate sub-kingdom; and there are few subjects of general interest in the wide range that extends from the sponge and other protozoa to the insect and the crab, that have not found a place in this

* "Evenings at the Microscope; or, Researches among the Minute Organs and Forms of Animal Life." By Philip H. Gosse, F.R.S. A New Edition. London: Society for Promoting Christian Knowledge, 1874.

volume. It has furthermore a particular interest arising from the author's wide acquaintance with the subject, and from his introduction of accounts of curious structures that he himself has described before the Royal and other scientific societies. One of the facts recently introduced relates to the structure of hair; and on this point the author quotes from an article which appeared as a reprint in the "Monthly Microscopical Journal" for March or April 1873. Mr. Gosse, however, quotes it from the "English Mechanic" of May 1873—a journal which, of course, copied part of the paper from the "Microscopical Journal," evidently without acknowledging the source. This, of course, is only of importance to the reader; for in this instance he is referred by Mr. Gosse to the "English Mechanic" or the "New York Medical Journal;" and as the English journal did not most probably quote it in full, the reader is compelled to refer to a foreign journal, not knowing that it is to be found fully reprinted in an English one.

In Mr. Gosse's chapter on Blood an account is given of the peculiarity of the circulation in *Perophora,* one of the Tunicates, which is an animal that he exhibits to his imaginary audience. This creature has certainly a curious mode of propelling its blood. In almost all animals the blood travels invariably in the same direction, but in this one it is very different. In the author's words:—"After we have watched this course followed, with regularity for perhaps a hundred pulsations or so, all of a sudden the heart ceases to beat, and all the globules rest on their circling course, that we had supposed incessant. Strange to behold! after a pause of two or three seconds the pulsation begins again, but at the opposite end of the heart, and proceeds with perfect regularity, just as before, but in the opposite direction." In fact, there has been a complete reversal of the current of the circulation. His remarks on the tongues of mollusca are also of considerable interest, though, of course, not novel to the comparative anatomist. We quite agree with him when he says that "it sounds almost like a fable to be told that the great spotted slug, which we sometimes find crawling in damp cellars, carries a tongue armed with 26,800 teeth." Yet there is, as he afterwards observes, no doubt whatever of the fact.

We may observe that the author makes a slight mistake in referring to Professor Greene's excellent "Manual of the Animal Kingdom." Indeed, if the reader were to make enquiry, we doubt whether any ordinary bookseller could tell what he wanted. Professor Greene has written but two volumes; these are respectively entitled the Manuals of the Sub-Kingdom Protozoa and Cœlenterata. Undoubtedly they do possess—and that first in order—the title of "Manual of the Animal Kingdom;" but that heading has fallen altogether into disuse, because the original intention to complete the series likewise became non-performed. There is but one other point to which we will allude ere we close our observations, and that is with regard to Mr. Gosse's remarkable discovery of the curious ring of curious animals surrounding the pure chitonous material of the mouth of the tube of the *Sabella.* This ring is figured in the book, and it is certainly a most startling resemblance to a series of human naked figures standing together in a ring, and thrusting their arms above and below their heads in a state of intense gesticulation. It is really a most singularly life-like representation of a group of human beings. The author, who could not tell to which group to refer this creation, and thinking at first that it belonged to *Coryne*, says:—"While I was observing the individual in question I saw it suddenly open the head-lobe and unfold it into the form of a broad shovel-shaped expanded disk, not, however, flat, but with the two halves inclining toward each other, like two leaves of a half-opened book. This immediately reminded me of the great sucking disk which I had seen evolved from the obtuse summit of *Stauridia producta*, and confirmed my suggestion of the natural affinities of the form." From the idea which they originated of the *Lares* of Roman mythology, and from their being on *Sabella*, the author has given them the title of *Lar Sabellarum*. This strange animal has remained unrecognised for more than seventeen years, till the Rev. T. Hincks found it again in the summer of 1872.

It will be seen from what we have said that Mr. Gosse's book is full of interesting material, very graphically set before the reader; and though it is by no means what it might have been, still we must do it the justice to say it is a treatise that no microscopist should be without.

CIRCULATION OF THE BLOOD IN ANIMALS AND PLANTS.*

THERE are, no doubt, a few of our readers who will exclaim, when they have read the heading of this notice, "What nonsense! Surely this must be a mistake? Imagine the circulation of the blood in a head of cabbage!" If anyone feels tempted to make a remark of this kind, we can only say to him, "You are very wrong." Most unquestionably plants possess a distinct circulatory system, though it is conducted differently from that of the higher animals. In the book which is now before us the author has given an account of the various systems of the circulatory apparatus in animals and plants, dwelling, of course, at length on that of man and mammalia; and he has illustrated his remarks by more than 150 woodcuts, many of them being from his own drawings. Of course the lectures of which the book consists were originally delivered in the College of Surgeons, Edinburgh; and from this fact, as well as from the very high physiological reputation of the author, we have nothing in the shape of absolute criticism to display. Our duty, therefore, will be merely to dwell on some of the points of interest which strike us as being especially worthy of remark. In tracing the account of the circulation in plants, Dr. Pettigrew points to the difference of opinion which exists between Dr. Balfour on the one hand, and Mr. Herbert Spencer (the distinguished Sociologist) on the other. He thus lays down the two opinions:—He shows first that "Mr. Herbert Spencer has demonstrated by his experiments that the passage of fluid though the spiral and other vessels is much more rapid than through the mere cellular tissue. On the other hand, Mr. Spencer's view, that there is no direct connection between the age of a vessel and its porosity, has been called in question, for Professor Balfour

* "The Physiology of the Circulation in Plants, in the Lower Animals, and in Man: being a Course of Lectures Delivered at Surgeons' Hall, Edinburgh, in 1872." By J. Bell Pettigrew, M.D., F.R.S., F.R.S.E. Illustrated by 150 engravings on wood. Edinburgh: Oliver and Boyd, 1873.

says "that the tubes forming the wood are pervious to fluids in their young state, but that their walls soon become thickened by deposits of lignine, and in the heart-wood of trees their cavities are obliterated." Another objection, too, has been raised against Mr. Spencer's idea, viz. that the spiral vessels frequently contain air within them. But, as Dr. Pettigrew very justly observes, this is an objection taken purely from the known condition in animal life; and there is really no reason why the vessels of vegetable tissue might not contain air as well as their circulatory fluid. Besides, there are many other analogies between the vessels in animals and plants which are dwelt on at some length by the author. On the subject of the rythmic action of the heart, which is so peculiar, the author refers to the rythmic action that is seen in certain plant-cells. The observations of Herr Cohn show that in *Gonium pectorale* and other plants the contractions of the vacuoles, like the contractions of our own hearts, take place at regular intervals. "The contractions (says Sir James Paget, in his Croonian Lecture) and the dilatations occupy equal periods, as do those of our own heart ventricles, and when two vacuoles exist in one cell their rythms are alike and exactly alternate, each contracting once in about forty seconds, and the contraction of each occurring exactly mid-distance between the two successive contractions of the other." Thus we see the exact counterpart in plant-life of a function which we used to consider at one time essentially an animal one. And this leads on the author to speak of the various forces of *osmose*, and to explain by very simple experiments the manner in which they act on the living plant; and thus he passes to consider Spencer's theory of the action of the wind in bending plants, and by this carrying on the circulation; next to the question of the action of syphons, and finally to the so-called circulation in metals in Seebeck's discovery; and with his remarks on this subject he terminates the history of the circulation in plants. Indeed, this part of his book will well repay the reader, for it is a lucid and full account of what has been done in the matter.

It is on the next portion of the work that the author displays his powers to best advantage, for it is here that he is most at home with his subject. And indeed he has collected together a number of facts, many of them from his own original labours, such as to render the merely popular reader somewhat aghast at the immense range of the subject. He deals with the lowest type of circulatory system, and shows the mode of its operation; and then he passes on to consider the higher forms, until at last, after over one hundred pages engaged in the discussion of those subjects, he comes to consider the forces of the human circulation, and the organs in, or through, which these forces are exerted. All through he has splendidly illustrated his subject; and we find him, in all that relates to the structure of the human heart and its valves, giving us an amount of information, and putting it in so easily-digested a form, that by this fact alone, if we were ignorant of the valuable papers he has published on the subject, we could see that he has not approached the question for the first time. But he has gone over and over it, so that at last, when he is speaking, there is no uncertainty about what he says; but, on the contrary, it is all clear and distinct, either the known or unknowable. Of course our space will not permit us to give even a sketch of the method he pursues, but it will allow of our recommending certain portions of his volume as specially deserving perusal. And such, we should say, are all those parts relating to the structure of the heart and the arrangement of the fibres in such a peculiar manner as to produce those motions which to some people are so inexplicable. The part of the work that deals with the structure of the heart, and the peculiar arrangement of the valvular provisions, is one that we must strongly advise our medical readers to peruse. In it they will find the correct explanation of much of the movement of the heart given, and that, too, by the most distinguished modern student of this wonderful construction. We can only express in conclusion our deep sense of gratitude to Dr. Pettigrew for the very able manner in which he has discussed, so as to be familiar to any well-educated person, some of the most complex problems in the whole range of physiological science.

TEXT-BOOKS ON CHEMISTRY.*

IT is strange that the idea has not occurred to any English writer that a book written in explanation of the immense and novel strides that chemistry has made within the past fifteen years would prove an extremely interesting book to the man of scientific mind. But it often happens that he who is outside the range of workers, no matter what their subject may be, often sees more clearly the entire relations of the labour than he who is engaged in it. "The best hurler," says the Irish proverb, "is always by the ditch." And so it has happened in the present instance, for the only book that has been published on the subject of the changes that have occurred in chemistry—at least the only one that is especially a survey of the science as it is, compared with what it was—is a book of American authorship. Indeed, so far credit must be given to our transatlantic brethren, but we fancy not very much further. Throughout the book, although in many cases the author sees perfectly plainly the picture that is before him, and in some cases appears to put it more clearly before his audience than it is before himself, yet there are points on which, we confess, he seems to us to be decidedly in the wrong. It is of course not for us to deal with such questions here as the law that was laid down in 1811 by Avogadro with regard to the constitution of substances. Nor is it to be expected that we shall deal with the questions of *quantivalence* and *metathesis* in these pages. But it seems to us as if the author, gifted as he is with a remarkably clear power of explanation, has gone farther than mere fact will allow him. In any case, whatever view may be ultimately taken of the theories he has put forward, all must allow that he has written ably and clearly on a subject on which, while we have many men among us who are practically versed in it, we have, nevertheless, but very few who are

---

* "The New Chemistry." By Josiah P. Cooke, Erving Professor of Chemistry and Mineralogy in Harvard University, U.S.A. King & Co., 1874.

"Introduction to the Study of Organic Chemistry. The Chemistry of Carbon and its Compounds." By H. E. Armstrong, Ph.D., F.C.S., Professor of Chemistry in the London Institution. London: Longmans, 1874.

able to come before the public with so difficult a subject so well dealt with as the present one has been by Mr, Josiah P. Cooke.

The other volume is one by a very distinguished operator and teacher of chemical science, and it is likely, in our opinion, to be approved of. We think in the outset that the author has been wise in omitting a vast number of substances whose relations to other well-established bodies has not been completely made out. By doing this he has, in our opinion, given compactness and completeness to a volume which would otherwise have been a great disjointed mass. Besides, it must be borne in mind that the works of which it forms one are addressed especially to the student, and it is hardly required to bring under his notice an entangled mass of facts. We do not agree with the author in regard to his non-recognition of the division of the carbon compounds. It seems to us that the division into the two groups—fatty and aromatic—is one which has been more and more justified by recent work, and we therefore think it is to be regretted that Professor Armstrong has adopted a different tone of thought. Still, we think his decision a good one so far as it goes. He divides the substances into—first, *carbons*; then follow in this order the others, as, for example, *hydrocarbons*, *alcohols*, *ethers*, *aldehydes*, *acids*, *ketones*, and *amines*. We note, too, with pleasure, that the author's style is clear and to the point, even if it be a little Germanic; and his explanations are in most cases sufficiently ample.

TWO OF FARADAY'S WORKS.*

IT seems to us that Mr. Crookes has done very wisely in editing these splendid works of Professor Faraday's, and once more bringing them before the public. For there is a power in them that is unequalled in most modern essays on the subject; and further more, as they are addressed to children or young people, they will form admirable class-books for those who attend the Christmas lectures of the Royal Institution. Besides, the editor has introduced a capital feature—he has brought in between parentheses, wherever it was necessary, a description of what Faraday actually did in the endeavour to make an experiment succeed. The lectures are in two books, one of which is devoted to the subject of force in general; and the other relates the "Chemical History of a Candle." We may remark *en passant* that they are both remarkably well got up—paper, printing, illustrations, binding, being all of them excellent. Of course, there is nothing new in the books, but there is a vast amount of extremely useful information, put in a form that very few indeed of those who have followed Farady have been able to imitate. We wish we had space to give one of the lectures, but we have not. We have gone over several of the different chapters on the subject of "the Candle," with a view to see whether we could not find some of them from which we could take about

---

* "The Chemical History of a Candle;" by Michael Faraday, D.C.L., F.R.S. Edited by W. Crookes, F.C.S. London: Chatto & Windus.

"The Various Forces of Nature;" a Course of Lectures at the Royal Institution, by Michael Faraday, D.C.L., F.R.S. Edited by W. Crookes, F.C.S. London: Chatto & Windus. 1874.

twenty lines which would be complete in themselves, but in vain. The writer has such a habit of saying in one part that a fact which occurs shall be explained further on, that it is impossible to find any part of the lecture complete, and which yet does not amount to more than a few sentences.

The other book is not of so fascinating a character as that upon the candle, yet it is, if anything, more philosophic. It is an excellent popular attempt to put before the reader what Mr. Grove (now, we believe, Judge Grove) has given the more scientific public, viz. the idea that all forces are but modifications one of the other, and are reproducible. Thus, gravitation, or mechanical force, gives rise to heat, heat to light, electricity, magnetism, and chemical action; and all may be converted into one form, which they came from originally. And this is what is demonstrated so wonderfully popularly in the clever little book before us. There is but one portion which we will quote, as it shows the author's peculiar style better than anything else. He is endeavouring to explain the method by which he himself can become a conductor of electricity, and he shows it thus: "And if I were to show you a stool like this, and were to explain to you its construction, you would easily understand that we use glass legs because they are capable of preventing the electricity from going away to the earth. If, therefore, I were to stand on this stool and receive the electricity through this conductor, I could give it to anything that I touched (the lecturer stood upon the insulating-stool, and placed himself in connection with the conductor of the machine). Now I am electrified I can feel my hair rising up. Let us see whether I can succeed in lighting gas by touching the jet with my finger (the lecturer brought his finger near a jet from which gas was issuing, when, after one or two attempts, the spark which came from his finger to the jet set fire to the gas). You now see how it is that this power of electricity can be transferred from the matter in which it is generated and conducted along wires and other bodies, and thus be made to serve new purposes utterly unattainable by the powers we have spoken of on previous days." And all through the book is written in this simple, easy, familiar style—a style which was as characteristic of the man as any other circumstance of his remarkably simple mode of life.

WHERE THERE'S A WILL THERE'S A WAY.*

THOSE who are strongly democratic in their tastes will doubtless like this little book, for it is a very well-written history of a series of men who—almost all poor men—under the greatest difficulties, devoted themselves intensely to the study of natural history. Of course, the author of such a book is an extreme admirer of such men as he has taken it upon himself to describe, and is likely enough, in writing of their lives, to exaggerate the actions of his heroes, so as to make them seem to calmer eyes a little too heroic. If we say this, it is merely to guard our readers against

* "Where there's a Will there's a Way; or, Science in the Cottage. An Account of the Labours of Naturalists in Humble Life." By James Cash. London: Hardwicke, 1873.

certain stories of men whose devotion to natural history was so thorough that they were capable of doing excessive ordinary work from 6 A.M. till 9 P.M., and then, without resting for even half-an-hour at home, going out again, naturalising till it was past daylight, and then lying down under a tree till morning's sun enabled them once more to set about their excursion. Of course, accounts of this kind are utterly valueless, and wherever they occur the reader will do well to bear this fact in mind. Still, there is a good deal of honest truth about the book; and, inasmuch as it rescues from oblivion a number of men who, however humble their qualities, are infinitely too noble to be allowed to be absolutely forgotten, it has a very excellent purpose. The author is clearly himself one of the number of men whom he describes, and therefore his calling attention to one of the subjects of his work, as one of the most remarkable scientific men of modern times, must be taken for what it is worth; we confess, ourselves, to never having heard the name of the gentleman before. However, there is no doubt that the biographer has done his work, if not wisely, at least too well, and we owe him our thanks for the volume.

THE MICRO-SPECTROSCOPE AND ITS WORK.*

MR. W. T. SUFFOLK has by these lectures on the spectroscope supplied a want, and has given the opportunity to microscopic workers to do a little work—and perhaps original work—at the subject of spectrum analysis. These lectures were originally delivered orally before the South London Microscopical Club; and Mr. T. G. Ackland, the honorary registrar to that society, has very carefully transcribed them. They are, of course, purely elementary, but will nevertheless be exceedingly valuable to those who cannot purchase works like Roscoe's. They are well printed, are excellently illustrated with a series of woodcuts and seven lithographic plates of spectra, one of which gives the spectral bands in colours of fourteen objects; some of them being elements, some compounds, and some the peculiar spectra given by certain heavenly bodies. The author gives credit to Mr. Browning for having supplied certain of the substances from which bands are taken, and this, we think, he is justified in doing, for Mr. Browning's collection is by no means an ordinary one, as we know from personal experience.

AN ILLUSTRATED HISTORY OF THE MOON.†

WITHOUT the slightest doubt this book must have an extensive sale, if it is properly advertised; and our reason for saying so is that the authors have hit upon a new feature in giving so many grandly-executed views of

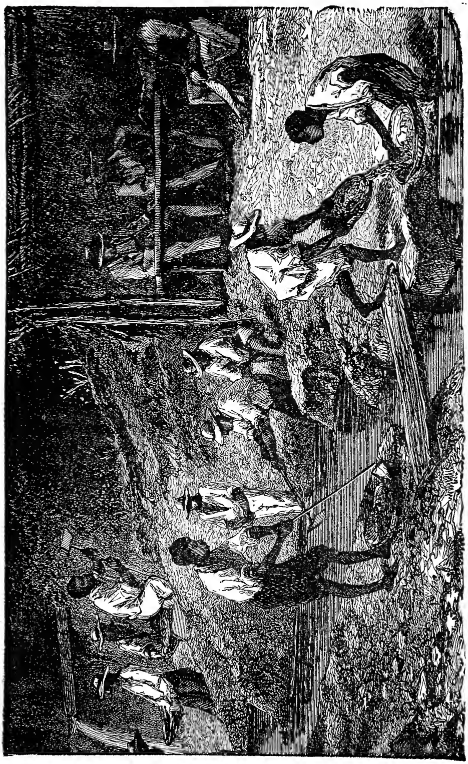
* "Spectrum Analysis, as applied to Microscopical Observation, &c." By W. T. Suffolk, F.R.M.S. London: John Browning.

† "The Moon: Considered as a Planet, a World, and a Satellite." By James Nasmyth, C.E.; and James Carpenter, F.R.A.S., late of the Royal Observatory. London: John Murray, 1874.

the lunar planet. We confess that even if the book were to consist of these enlarged views alone it would still be a work admirable in its way, and strikingly attractive from the fact that it presents features that are altogether unknown to the English world at large. It is true that of late years we have, through Mr. Proctor, been presented with enlarged photographs of the moon's structure; but then though they may be more truthful in every minute detail, than the wondrous drawings in the book before us, they could not, unless they were fifty times enlarged, convey so good an idea as the plates which Messrs. Carpenter and Nasmyth have given to the world. The authors to a certain extent apologise for the absence from the book of the customary astronomical facts, stating that they have been rather at pains to give the moon's physiography, *i.e.* "the causative phenomena of the features, broad and detailed, that the surface of our satellite presents for study." But they need offer us no apology, for whilst such matter is to be found amply given in the hundred and one treatises on astronomy, there is not one which gives us the excellent graphic account that they have furnished. They have, in the first place, done very wisely; that is, they have given us bird's-eye views of the scenery of Vesuvius and its neighbourhood, and side by side a view on the same scale apparently of a portion of the moon's surface. We think this idea is a very happy one; for there is nothing that could more distinctly convince anyone that the moon's surface is chiefly made up of volcanic masses than the view from above downwards of portions of our own plutonic region. Anything more distinctly alike than the two surfaces it is absolutely impossible to conceive. There are twenty-five plates in the volume, besides numerous woodcuts, and it is impossible to say which of the former is the best. To be sure the photograph of the moon is immensely inferior to the splendid picture that was published by Mr. Proctor some time since; but then the views, almost stereoscopic, of the lunar scenery are certainly wondrous, and they give us quite a different notion of lunar scenery from that which is generally adopted. It seems to us that the authors are not unfrequently mistaken in some of their original ideas—more especially is this the case in the attempted explanation of the peculiar cracks and streaks which are so well known in lunar features. They have tried several experiments on a glass globe, on which they have caused considerable internal pressure to be exerted, and they fancy that this when cracked in a peculiar manner illustrates the mode in which the phenomenon of cracks appears on the lunar surface. But we think they are wrong; for the globe shows its cracks all in one direction, and from the north pole (let us say) towards the equator, whereas the lunar cracks are arranged round each volcano; and we cannot imagine that in such a planet the force which produced volcanoes was in a separate series of circles. However, this is a very trifling objection to raise. *Tout entier* we are immensely pleased with our authors' labours, and we wish them every success in the sale of their excellent book.

DIAMONDS AND PRECIOUS STONES.*

THOUGH not a very profound treatise, this is an excellent popular account of the subject on which it treats. Further, it is very profusely illustrated, which lends a charm to what might otherwise be considered

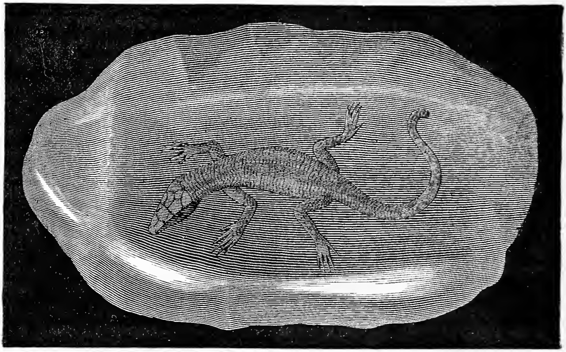


FIRST WASH OF THE DIAMOND-YIELDING SOIL IN BRAZIL.

rather dull reading. The only chapter which we do not see the reason of in a treatise expressly on mineralogy is the first one, which deals more with

* "Diamonds and Precious Stones: a Popular Account of Gems, from the French of Louis Dienlafait." Illustrated by 126 engravings on wood. London: Blackie and Son, 1874.

sketchy palæontology than anything else. The chapter on the diamond's history (from which the preceding cut is figured) is a good one. It relates, besides the history of the time of the diamond discoveries in Brazil, the manner in which diamonds were found at South Africa, and it states that no less than 1,500,000*l.* worth were sent from the Cape of Good Hope in the year 1871. The figures of the different minerals are all given, as well as general sketches, so that the scientific reader will not be disappointed. Among the different illustrations there is one which the Publishers have lent us, and which is of interest. It is a figure of a piece of amber large



enough to enclose a lizard, which is distinctly seen buried in the mass. This specimen belonged originally to the late well-known Duke of Brunswick, by whom it was given to the Kirckers collection. There are many other points of interest in the book, but we have given enough to show that the volume is an excellent popular account of what some think an uninteresting subject.

PALÆONTOLOGY OF PÉRIGORD.*

THIS noble work, which was so well and so long ago begun by M. Lartet and Mr. Christy, and which is now very near its completion, is solely under the editorship of Professor Rupert Jones, the two authors having both died since the book was begun. We have now before us Parts XIII. and XIV., and we are nearly as well pleased with them as we were with those that have gone before. We say nearly, for we are not quite so satisfied with Part XIII., which has two of its plates executed, not by the well-known

---

* "Reliquiæ Aquitanicæ: being Contributions to the Archæology and Palæontology of Périgord, &c." By Edward Lartet and Henry Christy. Edited by T. Rupert Jones, F.R.S. Parts XIII. and XIV. London: Williams and Norgate.

French artist and lithographer, Mr. Louveau, but by Taylor and Francis. And assuredly if we wanted an example of the two methods, the English and the French, we could not be better satisfied. The French has all that elegance of design and rigid truthfulness of character which in so remarkable a degree characterises their work, and which has been so excellent a feature in the earlier series of this work. On the other hand, the English work displays carelessness and a want of finish which make it contrast sadly with the foreign plates. We are glad to see that the editor has not continued this unfortunate feature, for in Part XIV. we have again the exquisite French lithography. Part XIII. is not very remarkable in its figures, but Part XIV. contains an exquisite plate representing that well-known picture of a mammoth's tusk on which is carved a sketch of the mammoth. Mr. Woodward, F.R.S., suggests that there can be little doubt that the sketch, such as it is, was the result of a life study of the animal. This is a fact of great import, for it would show that the animal was really alive in France when the caves of Périgord were inhabited by man. The other matter in the two numbers of the work is of great interest. We hope soon to see it completed.

WALKING, SWIMMING, AND FLYING OF ANIMALS.*

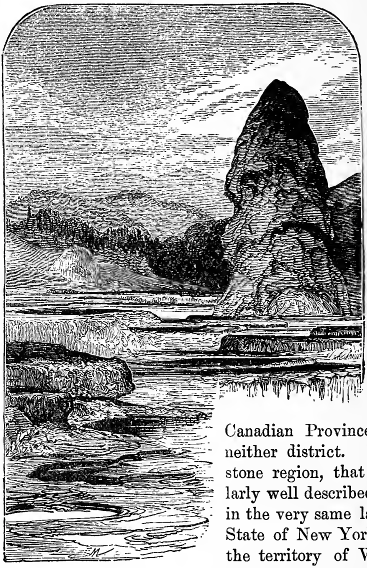
A VERY interesting work is that which Dr. Pettigrew has given us, and it is—save to those few who form members of the Society which terms itself Aëronautical—on an entirely novel subject. Of course, on this account there may be many points on which the author will be said to have pushed his views to too dogmatic an extent. However this may be, the book is undoubtedly one which possesses an interest far beyond the common. It is but a popular account at best; those who desire to go into the subject fully are referred to the author's well-known memoirs on the subject (two of which he has been good enough to send us, and which will be found in the Transactions of the Linnæn Society and in those of the Royal Society of Edinburgh). But those who care to go through the pages of the volume before us will find in it much to entertain and to instruct them. In the first part of the work will be found an account of the different modes of walking, running, swimming, and flying adopted by mammals, birds, and insects; amply illustrated. And in the second—to our mind as interesting a subject, though one dealt with at some length by the Aëronautical Society—will be found, firstly, a clearly-tabulated statement of the scientific conditions on which flight is to be attempted, and then an able résumé of what has been done in this country and on the Continent to favour the progress of the different modes of flight. All through the book is full of interesting matter, and we offer our best thanks to Dr. Pettigrew for thus addressing the general public on a point in which it is not less interested than the more specially scientific man.

---

* "Animal Locomotion; or, Walking, Swimming, and Flying; with a Dissertation on Aëronautics." By J. Bell Pettigrew, M.D., F.R.S., Pathologist to the Royal Infirmary of Edinburgh. Illustrated by 130 engravings on wood. Henry S. King & Co., 1873.

WONDERS OF THE YELLOWSTONE REGION.*

WE fancy that there are not a few, even among educated people, who have not the remotest idea where the district so graphically described in this volume is to be found. One of our friends, not a distinguished Fellow of the Geographical Society, told us it was somewhere in Brazil; and another said, without doubt it was a part of the



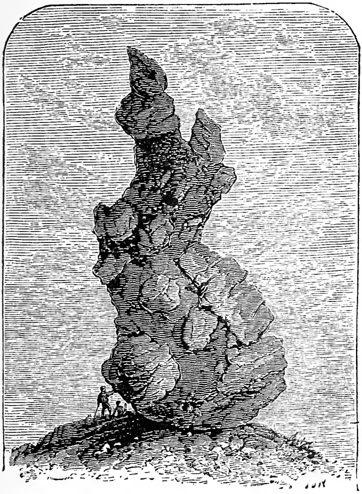
THE LIBERTY CAP.

Canadian Provinces. It is in neither district. The Yellowstone region, that is so singularly well described, is a district in the very same latitude as the State of New York, but it is in the territory of Wyoming, *i.e.* about midway between the Pacific Ocean and the Mississippi. It has only been lately described at all, even to the scientific world, and now it has all its beauties unfolded to the general reader. The author of the book gives the most enthralling account of the scenery, and of his own marvellous escapades and thrilling adventures over the mountains, through the snow and hot springs, with frequently no fodder of any kind but a few roots cooked in a neighbouring hot spring. His descriptions are some of them of great

---

* "Wonders of the Yellowstone Region, in the Rocky Mountains. Being a Description of its Geysers, Hot Springs, Grand Cañon, Waterfalls, Lake, and surrounding Scenery, explored in 1870–71." Edited by James Richardson. London: Blackie & Son. 1874.

interest, especially when they are accompanied by woodcuts, as they are in the following case.* Here is his account of the Liberty Cap, a view on the White Mountain:—"On this sub-terrace is a remarkable cone, about 50 feet in height and 20 feet in diameter at the base. Its form has suggested the name of Liberty Cap. It is undoubtedly the remains of an extinct geyser. The water was forced up with considerable power, and probably without intermission, building up its own crater until the pressure beneath was exhausted, and the spring gradually closed itself over at the summit. No water flows from it at the present time."

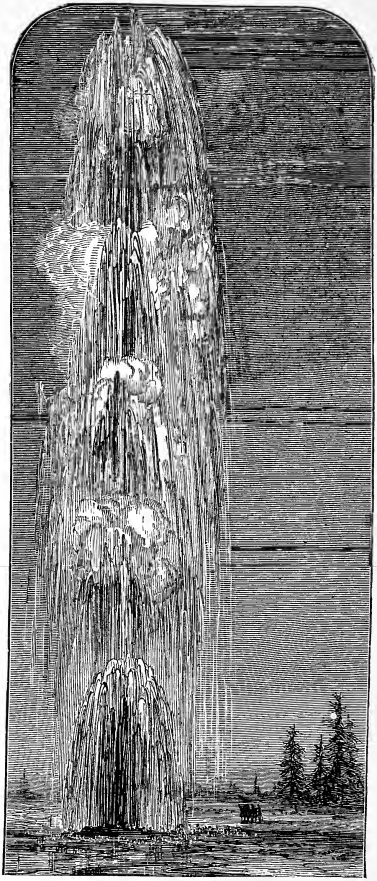


THE DEVIL'S HOOF.

Again, in describing the scenery of Gardiner's River, on the way to the Grand Cañon, the author quotes Mr. Langford, who says of the falls of the river into the Yellowstone, which are surrounded by columns of breccia, that "the position attained on one of these narrow summits, at a height of 250 feet above the boiling chasm, requires a steady head and strong nerves. . . . . Many of the capricious formations wrought from the shale excite merriment as well as wonder. Of this kind especially was a huge mass sixty feet in height (see figure above), which, from its supposed resemblance to the proverbial foot of his Satanic Majesty, we called the Devil's Hoof." The work concludes by giving a quotation from the Government papers relative

* Cuts kindly lent by the Publisher.

to the people's great park which it is proposed to make at the Yellowstone River. And in this part there is barely a couple of lines describing one of



THE GIANTESS, FIREHOLE BASIN.

the finest hot springs, we suppose, in the whole world. We have no record of its size, but from the statement that it is more majestic than the great geyser of Iceland, some idea may be formed of it. The opposite cut exhibits this geyser, and from the relative size of the trees it would seem of a wondrous height. Meanwhile, we must express our surprise that so little has been said about it, for even in another chapter, which relates especially to this subject, the chief information is that taken from English accounts of Iceland. We fancy that the book has been written by different persons, under the control of the editor. It is, at all events, full of interesting matter, even if occasionally some of the writer's (who he is we know not) tales seem exceedingly marvellous.

SHORT NOTICES.

The Student's Guide to Zoology. By A. Wilson. London: Churchill, 1874. This appears to be a good book. The author is clear in his statements, without being at all dogmatic in his teaching. We think the position he has taken up in regard to Mr. Mivart and Mr. Darwin's views is most creditable. We should recommend his manual to the junior student unquestionably.

Veritas. By H. Melville. London: Hall, 1874. This is a book which, to the uninitiated, appears wonderful; but to those who really understand the question relative to Freemasonry, which it pretends to discuss, it is really worthless. It is well printed and illustrated.

The Unity of Natural Phenomena. From the French of M. Emile Saigey, with an introduction and notes by T. F. Moses, A.M., M.D., Professor of Natural Science in Urbana University. Boston: Estes and Lauriat, 1873. This is a well "got-up" volume, printed in an excellent type. Of its views there need not be anything said, for they are, of course, not new; still they are unknown to a considerable mass of even educated people, and we suppose it is these that the present editor addresses. It is a clever book, and to one who has any general knowledge of science an extremely fascinating one.

The Treasury of Botany. Edited by J. Lindley, M.D., F.R.S., and Thomas Moore, F.L.S. New edition, 2 vols. London: Longmans, 1874. This being a stereotyped work, of course anything like important modification of the old edition is quite impossible. Therefore an addition becomes a necessity. This has been given in the present issue. It is of about 100 pages, and is written by a class of botanists who are known to be the best men in the entire scientific world. The book on the whole is a good new edition.

A Manual of Lunacy. By F. S. Winslow, M.B.A. London: Smith and Elder, 1874—is not a book on which we can be expected to pass any opinion. We therefore do not.

The Ocean: its Tides, Currents, and their Causes. By W. L. Jordan. London: Longmans, 1874. We really must decline to consider Mr. Jordan's theories. He puts them forth ingeniously, and spends much money on his book; but we cannot agree with his opinions.

Darwinism and Design, or Creation by Evolution. By George St. Clair, F.G.S., M.A.I. London: Hodder and Stoughton, 1873. It seems hardly fair to notice a book of some size in a few lines, especially when the author's aim has been a good one and is cleverly carried out. However, we cannot avoid it. For we do not think Mr. St. Clair's book entitled to a long notice in the presence of other more important ones. However, we may say that he has considered the whole question with a great deal of calmness, and has done justice to most of those who are concerned in the matter. We do not agree in his conclusions, however, and we assert that if he used reason alone to examine the questions considered, he must come to a quite different result.

The Vapour and Turkish Bath: their Value in Diseases of the Skin. By J. L. Milton, M.R.C.S. London: Hardwicke, 1874. This pamphlet shows us how we can construct a portable Turkish bath in our own house; and it is therefore a useful little book.

Treatise on Solid Geometry. By W. T. Pierce, Architect. London: Longmans, 1874—is a large 4to. volume, illustrated by more than 36 plates. In this the author appears to point out clearly the more important problems in the practical division of solid geometry. It is to be followed by another treatise.

Half-hours with Insects. Part I. By A. S. Packard, Jun. Boston: Estes and Lauriat, 1874. This is the first part of a serial popular work. It promises well. The author's effort is a capital one, and the publishers have secured the very best men in the country for the work.

We have also received: "University of London Questions," by C. J. Woodward, B.Sc.; "The Psychology of Scepticism and Phenomenalism," by James Andrews; and "The Geology of Suffolk," by J. E. Taylor, F.L.S., F.G.S.

SCIENTIFIC SUMMARY.

ASTRONOMY.

The Astronomical Society's Medal to Professor Simon Newcomb.—The President of the Society, in giving the medal to Mr. Huggins for Professor Newcomb, dealt at length with the latter's high claims for it. Of Professor Newcomb's writings, he said that they exhibit all of them a combination, on the one hand, of mathematical skill and power, and, on the other hand, of good hard work—devoted to the furtherance of astronomical science. The "Memoir on the Lunar Theory" contains the successful development of a highly original idea, and cannot but be regarded as a great step in advance in the method of the variation of the elements and in theoretical dynamics generally; the two sets of planetary tables are works of immense labour, embodying results only attainable by the exercise of such labour under the guidance of profound mathematical skill, and which are needs in the present state of astronomy.

The Astronomical Society's New Rooms.—The Council had expected that the Society would have been in possession of their new rooms in Burlington House in time for this anniversary meeting. Many unforeseen causes for delay, however, have occurred; but the Council have now been assured by Mr. Barry, the architect, that in April next the decorations and fittings of the rooms will be completely finished. As a question of prudence, as well as of convenience, it will be probably thought necessary to defer the migration of the Society from Somerset House till the conclusion of the present session. This will give time for the walls to become perfectly dry, especially in the basement, and thus the rooms will be made more habitable for the assistant-secretary. It may be as well to record that the entrance to the rooms is the first door on the left hand on entering the quadrangle from Piccadilly.

The Last Volume of the "Monthly Notices."—It is a fact that speaks well for the progress of the Astronomical Society that the last volume of the "Monthly Notices" is the largest that has ever appeared. It clearly exhibits the general activity of the Society during the past year, as well as the progress of astronomical science. Several of the papers are of more than usual interest, and more than the ordinary number of plates illustrate some of the more important. The valuable charts of the transits of Venus of 1874 and 1882, drawn by Mr. Proctor, may be specially referred to. These show the lines of equal acceleration and retardation of ingress and egress, and also the lines of equal duration for internal contact in 1874, and for external contact in 1882. An interesting series of drawings of Mars and Jupiter, by Mr. Knobel, is inserted in the June number. Some beautiful drawings of Jupiter, exhibited at one of the evening meetings by the Earl of Rosse, have since been chromo-lithographed at his expense, and the use of the stone has been kindly offered by him to the Society. The Council therefore hope to insert a copy of this valuable plate in the March number of the "Monthly Notices." This number has not yet reached us, so we are unable to say whether the idea has been carried out.

Deaths of Astronomers during the Year.—The Astronomical Society gives the following list of Fellows and Associates whom it has lost during the past year. Fellows:—Rev. Temple Chevallier, B.D.; F. H. Elliott, Esq.; Rev. George Fisher, F.R.S.; D. A. Freeman, Esq.; R. W. S. Lutwidge, Esq.; William Mann, Esq.; J. R. M'Clean, Esq., M.P., F.R.S.; Rev. Jacob Morton; Frank Robertson, Esq., late Royal Madras Engineers; Sir David Salomons, Bart., M.P.; J. Stanistreet, Esq. Associates:—Dr. Giovan B. Donati, Comm. M. F. Maury.

Professor Schmidt's Map of the Moon.—The President of the Royal Astronomical Society said, in his address, published in the "Proceedings" for February, 1874, that this remarkable representation of the lunar surface, which has occupied the attention of Professor Schmidt during thirty-four years, is now completed. Those who have seen this magnificent work speak of it in the highest terms, not only of its general appearance, but also of the wonderful manner in which the details of the lunar features are delineated. The map is two metres in diameter, and the drawing is made with the most extraordinary care and precision, the minuteness of the work being almost beyond conception. A specimen of the map has been exhibited at one of the meetings of the Society, when it was much admired for the extreme delicacy with which all the details of the lunar surface are given, and a hope was generally expressed that the map in its entirety would eventually be published. Unfortunately, the funds of the Athens Observatory are too limited to permit the publication of this great work at the expense of that establishment, and Professor Schmidt fears that there is no chance of publishing it either in Greece, France, or Germany, owing to the expense, which must necessarily be great. It is hoped, however, that some means will be provided for engraving this beautiful production, and thus make it available to astronomers. Meanwhile, Professor Schmidt has been requested to forward an estimate of the probable expense of transferring the map upon stone for lithographic engraving. If the estimate be not excessive, probably some means may be found to preserve to science the valuable results of Professor Schmidt's thirty-four years' labour.

Comets of the last Twelve Months.—Seven comets have appeared during the last twelve months; three, and probably four, of them being periodical comets which have returned to perihelion, and were therefore expected.

The Diameter of Venus.—A careful series of measures of Venus has been made by Mr. Plummer, at the Durham Observatory, using the double-image micrometer. Observations were made on twenty-six days near the inferior conjunction of the planet in 1873, and the resulting value of the diameter at mean distance is $17''\cdot321$, with a probable error of $\pm\ 0''\cdot046$, the augmentation of the diameter from the effects of irradiation being—0″·546. It follows, therefore, that each contact is affected by one-half this quantity. Mr. Plummer has substituted these values in the equations, and on the whole the agreement is generally very good, and it is believed that in the cases where the difference is sensibly above the average, that it arises from a variation in the amount of irradiation, depending upon the transparency of the atmosphere. The observations were all made in full daylight, when the planet was not far from the meridian.

BOTANY AND VEGETABLE PHYSIOLOGY.

The Composition of Fungi.—The "Academy," in a recent number, says that the Russian Agricultural Commission for the Vienna Exhibition has published some analyses of edible fungi made under the superintendence of Professor Nicolas Socoloff. They may be compared with that given by Professor Church for *Lycoperdon giganteum*, which is also edible when young (see the "Academy," September 15, 1873). The following are the results for three species of *Boletus*:—

	B. edulis.	*B. edulis.*	*B. annulatus.*	*B. scaber.*
Water	11·52	11·50	12·34	13·49
Ash	7·36	6·52	7·56	7·90

The composition of the ash was:

	B. edulis.	*B. edulis.*	*B. annulatus.*	*B. aurantiacus.*
Phosphoric acid . . .	25·06	26·08	21·74	20·27
Sulphuric acid . . .	12·97	8·42	—	—
Iron sesquioxide . . .	1·63	·98	·53	1·1
Manganese proto-sesquioxide	2·22	2·41	—	—
Lime	1·00	5·95	—	—
Soda	3·60	·87	3·99	1·65
Potash	50·37	57·76	58·10	56·09
Sodium chloride . . .	3·11	3·55	—	—

The proportion of nitrogen varied in dried specimens from 6·63 to 7·56 per cent.; the average percentage of phosphoric acid was 1·7 per cent.

Insect Labour in the Fertilization of Lathyrus.—Mr. Francis Darwin, who appears to have made a very special study of this subject—which, by the way, is calculated to throw so much light upon his father's views—has recently contributed an important paper on the above subject to the pages of a contemporary. He says that in many Papilionaceæ, Lathyrus for instance, the insect visiting the flower rests on a platform which is formed of the carina and the expanded alæ, but in the scarlet runner this platform is made up by the alæ alone, the carina being tightly coiled into a spiral close up to the entrance to the tube to the corolla. The alæ are attached, one on each side, to the proximal part of the carina; so that when an insect rests on them, its weight bears on the carina, and causes the pistil—which is contained in it as in a sheath—to be forced out. The direction of movement of the pistil is downward and to the left, so that a bee resting on the expanded alæ and pushing in its head to the left of the coiled-up carina, would come in contact with the pistil as it darted out of its sheath; but if the insect went to the right of the coil it would escape the pistil altogether. The end of the pistil is covered with hairs, and performs the same function as the brush in Lathyrus in smearing the bee with pollen. It is, therefore, of great importance for the cross-fertilization of the plant that the bees should go to the left of the coil. As a matter of fact, they all but invariably do go to the left; the very few bees that I have seen going to the right appear dissatisfied and unable to find their way into the corolla. Now, to reach the nectar-holes, the insect's proboscis has to pass down a tunnel formed above by the tube of the vexillum, below by the upper surface of the tenth stamen; the entrance into this tunnel is a narrow archway leaning towards the left, i.e. having its highest point to the left of the middle point of its base. As before stated, the flap almost blocks up the tunnel, so that to get to the nectar-holes the proboscis must pass over the top of the flap, and must therefore travel along the highest part of the tunnel; but since at the entrance arch the highest point is to the left, the bee finds it necessary to go to the left of the coiled-up carina, to reach the nectar-holes in the easiest way. If this view of the function of the flap, when considered in relation with the disposition of the pistil, carina, &c., be correct, it adds another instance to the long list of mechanisms for insuring the cross-fertilisation of flowers by means of the visits of insects.

A Hollyhock Fungus.—At a meeting of the Royal Horticultural Society (Jan. 21), Professor Thiselton Dyer made some remarks on a parasitic fungus, which was proving exceedingly destructive to hollyhocks. It has been identified by Berkeley in this country, and subsequently by Durieu de Maisonneuve in France, as *Puccinea Malvacearum* of Montagne; it was first described from specimens collected in Chili by Bertero.

Dr. Regel on Vitis.—"Silliman's Journal" for February (1874) contains a notice on this subject by Professor Asa Gray, which is of interest, as it expresses rather a different opinion to that put forward by our own "Gardeners' Chronicle." Professor Gray says, the "Gardeners' Chronicle" calls attention to Dr. Regel's bringing forward, as an "objection to the Darwinian theory, the circumstance that the cultivation of the American vines has resulted, in the course of a few score of years, in the production of as great an amount of variation as has been obtained in Europe and Asia during tens of centuries." Upon which it may be remarked, 1, that there are in North America several species to work with, against the single one cultivated in Europe and western Asia; and, 2, that the American varieties in question, for the most part, have not been made, but rather selected and improved within the last two or three score years. As most youngsters here very well know, all our vines vary greatly as to their fruit in the wild state. So that nature had long ago begun the work which the cultivator in this case only accelerates and directs, and gets the credit for.

American Plants in France.—Dr. Asa Gray states, in "Silliman's Journal" for February, that *Ilysanthes gratioloides,* a rather insignificant plant of the American flora, has recently been found in abundance in France, in the neighbourhood of Nantes. It is thought to have appeared there between the years 1853 and 1858, and to have been in some way received from the United States, but the manner of its coming eludes enquiry.

The Fruiting of an Alga, Mastigonema.—In "Grevillea" for February Mr. Cook, the editor, says that Dr. Wood has described, in his new and interesting work on the "Fresh Water Algæ of the United States," a new species of *Mastigonema,* which he calls *M. fertile.* His remarks upon this species are of interest to Algologists. He says—"I found this plant in a stagnant pool in 'Bear Meadows,' forming a filamentous, felty mass, with *Œdogonium echinatum* and other algæ. The variously curved and interlaced flexible filaments are always simple, and of uniform, or nearly uniform, diameter through their whole length; excepting that, in some instances, there are small, local, bulbous enlargements of the sheath. Though the ends of the filaments, in all the specimens I have seen, are abruptly truncate, it is very possible that in the young trichoma the apex is prolonged into a long hair, as in most of the *Mastigonema.* The inner filament is sometimes very distinctly articulated; often, however, it is not at all so. The sheaths are firm, not at all lamellated, and generally project beyond the inner trichoma. The spores are cylindrical, yellowish, with a pretty distinct, although very close coat. They are always enclosed in distinct cells, and are mostly several in a filament, placed at intervals in its length.

"This is the first instance, at least," says Mr. Cooke, "that I know of, in which a species of this genus has been found in fruit, and it is interesting to note the resemblance of the spores to those of the more commonly fruiting *Rivularias.*"

The Source of Nitrogen in the Food of Plants.—A somewhat strange series of opinions are those that have been started by M. Dehérain in his recent paper in the "Annales des Sciences Naturelles." While adopting the conclusions of Lawes and Gilbert, Ville and Boussingault, that plants have no power of absorbing nitrogen directly from the air, he still holds that the atmospheric nitrogen is the source of that which enters into the composition of the tissues of the plant. The results of a series of investigations which M. Dehérain has carried out tend to show that atmospheric nitrogen is fixed and retained in the soil through the medium of the hydrocarbons, such as humus, in conjunction with alkalies, and that this fixation is favoured by the absence of oxygen. In other words, the fixation of atmospheric nitrogen occurs when organic materials are in process of decomposition in an atmosphere either deprived of oxygen or in which that element is deficient. Under these circumstances carbonic acid and hydrogen are both given off, the latter uniting with nitrogen to form ammonia. According to the earlier researches of Thenard there are in soil two strata exposed to the action of the atmosphere—an upper oxidising and a lower deoxidising stratum. In the first stratum the nitrogen is obtained from the atmosphere, and impregnates the subjacent soil around the roots; in the second the nitrogenous compounds are converted into insoluble humates. The air of the soil is therefore at a certain depth deprived of oxygen; hydrogen is produced as the result of the decomposition of organic substances; and this hydrogen unites with the nitrogen to form ammonia. If these views are correct, they will have a considerable practical importance in agriculture, the value of a manure depending not so much on the actual amount of nitrogen present in it as on the quantity of carbonaceous substances which possess the power of taking up nitrogen from the atmosphere.

Trichomanes radicans in Kentucky.—The discovery of the Killarney Fern n Kentucky, U.S.A., is of interest, because it was not known before to have so northerly a range. It is now known to extend to latitude 38°.

CHEMISTRY.

Black Powder found in Snow; what is it?—In a letter from M. Nordenskjöld on Carbonaceous Dust, with Metallic Iron, observed in Snow (dated from Mossel Bay, lat. 79° 53′ N., received at Tromsöe July 24), the writer remarks that in December 1871 he found in some snow collected towards the end of a five or six days' continuous fall in Stockholm a large quantity of dark powder like soot, and consisting of an organic substance rich in carbon. It was like the meteoric dust which fell with meteorites at Hessle near Upsal in January 1869. It contained also small particles of metallic iron. Suspecting the railways and houses of Stockholm might have furnished these matters, he got his brother, who lived in a desert district in Finland, to make similar experiments; which he did, and obtained a similar powder. In his Arctic voyage the writer has met with like phenomena. The snow from floating ice has furnished on fusion a greyish residue, consisting mostly of diatoms (whole or injured); but the black specks, a quarter of a millimètre in size, contained metallic iron covered with oxide of iron, and probably also carbon. He thinks, therefore, that snow and rain convey cosmic dust to the earth, and invites further observation on the subject. M. Daubree, in presenting the letter, recalled a case of meteoric dust having fallen at Orgueil in 1864. He expressed the hope that M. Nordenskjöld has obtained sufficient quantities of pulverulent matter to be able to determine a characteristic fact—the presence or absence of nickel.

The Freezing of Alcoholic Liquids.—M. Melsens has made some experiments ("Naturforscher," 1873, No. 39) on the effect of low temperatures on brandy and wine, and his results accord completely with those of Horrath, who noticed an unexpectedly slight degree of sensation of cold in alcohol which had been exposed to a low temperature. Melsens finds that when brandy is cooled to 20° and even 30° or 35° below zero, it can be swallowed without any discomfort, provided only it be taken from wooden vessels. At 30° it is viscid and opalescent, and contains about 50 per cent. of alcohol. At −40° or −50° the strong alcoholic liquid becomes a solid, and if placed in the mouth in this state the pasty mass as it melts on the tongue appears less cold than ordinary ice. It has to be cooled to −60° to produce any impression of cold, and then is but rarely accounted very cold. The coldest portion prepared by Melsens had a temperature of −71°, and this produced in the mouth a sensation resembling that experienced on taking a spoonful of hot soup. He also describes the effect of great cold on effervescing wines.

Mr. Wanklyn and Dr. Redwood on Alum in Bread.—An important discussion on this point has been carried on in a paper called the "Circle" between the two eminent chemists above named. It resulted from the fact that an analysis of Dr. Redwood, which gave forty grains of alum in a four-pound loaf was disputed by the bakers, who asserted that they put no alum into the bread, and demanded an independent analysis, which was made by Mr. Wanklyn, with the result quite different from that of Professor Redwood, viz., that there was no alum in 1,500 grains of the bread. The magistrates before whom this case came dismissed it. There is thus a very important point to be discussed, viz., whether Professor Wanklyn or Professor Redwood is correct. We give no decided opinion on the point, but we certainly incline to the belief that Professor Wanklyn is by no means in the wrong.

Where is Thallium Found?—Mr. W. Crookes, F.R.S., has published in several numbers of the "Chemical News" a most valuable paper on the estimation of thallium, in which he gives an exhaustive list of the localities in which it is found. Thallium is, he says, a very widely distributed constituent of iron and copper pyrites. Upon examining a large collection of pyrites from different parts of the world, it was found present in more than one-eighth. It is not confined to any particular locality. Amongst those ores in which it occurs most abundantly (although in these cases it does not constitute more than from the 100,000th to the 4,000th of the bulk of the ore), may be mentioned iron pyrites from Theux, near Spa in Belgium, from Namur, Philipville, Alais, the south of Spain, France, Ireland, Cornwall, Cumberland, and different parts of North and South America; in copper pyrites from Spain, as well as in crude sulphur prepared from this ore; in blende and calamine from Theux; in blende, calamine, metallic zinc, sulphide of cadmium, metallic cadmium, and cake sulphur from Nouvelle-Montagne; in native sulphur from Lipari and Spain; in bismuth, mercury, and antimony ores, as well as in the manufactured products from these minerals (frequently in so-called pure medicinal preparations of these metals); in commercial selenium and tellurium (probably as selenide and telluride); and various other substances. Vide "Chemical News," Feb. 13, 1874.

A Phosphide of Antimony.—In a late number of the "Proceedings" of the Chemical Society of Berlin, Mr. W. Ramsay, in a preliminary notice on this subject, states that Mr. R. W. E. Macivor, of the Andersonian University, Glasgow, has succeeded in forming phosphide of antimony, PSb, as a red powder, insoluble in benzol, ether, and bisulphide of carbon, and containing—

Antimony	79·48
Phosphorus	20·21
	99·69

See also "Chemical News," February.

Reduction of Carbonic Acid to Carbonic Oxide by Phosphate of Iron.—Mr. S. H. Horsford has found that when carbonic acid and a mixture of phosphate of soda and green vitriol with a little water are introduced into a tube, the carbonic acid is gradually reduced to carbonic oxide. The author considers this phenomenon as very important for vegetation. Vide "Bull. de la Soc. Chim. de Paris," Vol. xx. No. 10.

What are the Adulterations of Tea?—This subject, which all—whether chemists or not—are interested in, has been very exhaustively dealt with in a paper read before the Chemical Society of London, at a recent meeting, by Mr. J. Bell, of the Laboratory at Somerset House. He says that tea is adulterated to a very large extent, not only with leaves of various kinds, including exhausted tea-leaves, but also with inorganic substances, such as quartz, sand, and magnetic oxide of iron; these latter substances are rolled up inside the leaf, and one sample of green tea examined was found to contain no less than 20 per cent. of quartz and 8·6 of the magnetic oxide. The latter may readily be separated by grinding up the tea, and removing the magnetic oxide with a magnet. The facing employed for green tea usually consists of French chalk and Prussian blue. In the preparation of exhausted tea-leaves, they are rolled up with gum-water, and then dried, catechu being added in some cases to restore the astringency. The article known as the "maloo mixture" consists essentially of exhausted tea-leaves. In searching for the presence of other leaves than those of the tea-plant the best method is to heat a small quantity of the suspected tea with water until the leaves are sufficiently softened to admit of being unfolded. They should then be spread out on a piece of glass, and carefully examined as to the nature of the serratures and the character of the venation, also the form of the cells of the epidermis and the stomata, and the peculiarities of the hairs as shown by the microscope. The essential differences which the tea-leaf presents when compared with other leaves were minutely described. The chemical composition of tea was next discussed, the amount of lignin and of tannin being very important.

Absence of Italian Chemists.—At a congress of Italian *savants*, which held a recent sitting at Rome, a meeting of the Chemical Section, under the presidency of Professor Cannizzaro, undertook a discussion on the rarity of original chemical research in Italy, and on its causes. The Section was of opinion that to awaken activity in this department it is desirable that the profession of chemistry should offer to students a career analogous to that presented by engineering or by medicine. To this the "Chemical News" adds: "A similar complaint and a similar suggestion might be made in England, with the additional complaint that engineers and medical men are continually encroaching upon the sphere of the professional chemist."

M. Paul's Improvement in Photo-Lithography.—According to the "Chemical News" of February 13, 1874, M. Paul produces a positive image on paper covered with a layer of albumen mixed with a concentrated solution of bichromate of potash. After a sufficient insulation under the negative, the paper is covered with lithographic ink, and then immersed in cold water to dissolve the unaltered albumen.

GEOLOGY AND PALÆONTOLOGY.

New Facts on the Relations between Reptiles and Birds.—Our readers will remember that some years ago we gave the only illustrated paper which had appeared on this subject, by Professor Huxley. Now Mr. H. Woodward, F.R.S., has come before the Geological Society, to offer additional remarks on this important question. The author, after giving a brief sketch of the Sauropsida, and referring especially to those points in which the Pterosaurians approach and differ from birds, spoke of the fossil birds and land reptiles which he considered to link together more closely the Sauropsida as a class. The most remarkable recent discoveries of fossil birds are: —

I. *Archæopteryx macrura* (Owen), a Mesozoic type, which has a peculiar reptilian-like tail, composed of twenty free and apparently unanchylosed cylindrical vertebræ, each supporting a pair of quill-feathers, the last fifteen vertebræ having no transverse processes, and tapering gradually to the end. —II. *Ichthyornis dispar* (Marsh), discovered by Professor O. C. Marsh in 1872 in the Upper Cretaceous beds of Kansas, U.S. It possessed well-developed teeth in both jaws. The teeth are set in distinct sockets, and are more or less inclined backwards.—III. *Odontopteryx toliapica* (Owen), an Eocene bird from the London Clay of Sheppey, the skull of which alone has been discovered, has very prominent denticulations of the alveolar margins of the jaws.

The author then referred to the Dinosauria, some of which he considered to present points of structure tending towards the so-called wingless birds.

I. *Compsognathus longipes* (A. Wagner), from the Oolite of Solenhofen, is about two feet in length, having a small head, with toothed jaws, supported on a long and slender neck. The iliac bones are prolonged in front of and behind the acetabulum; the pubes are long and slender. The bones of the fore limbs are small, and were probably furnished with two clawed digits. The hind limb is very large, and disposed as in birds, the femur being shorter than the tibia. The proximal division of the tarsus is anchylosed with the tibia, as in birds.—II. The huge carnivorous *Megalosaurus*, ranging from the Lias to the Wealden, had strong but not massive hind limbs, and short reduced fore limbs; it moved with free steps, chiefly if not solely on its hind limbs, which is true also of the vegetable-feeding lizards of the Mesozoic rocks.

The author next drew attention to the Frilled Lizard of Australia (*Chlamydosaurus Kingii*, Gray), which has its fore limbs very much smaller than the hind limbs, and has been observed not only to sit up occasionally, but to run habitually upon the ground on its hind legs, its fore paws not touching the earth, which upright carriage necessitates special modifications of the sacrum and pelvic bones.

The Solenhofen Limestone, in which Pterosauria are frequent, and which has yielded the remains of *Archæopteryx* and of *Compsognathus*, has also furnished a slab bearing a bipedal track, resembling what might be produced by *Chlamydosaurus* or *Compsognathus*. It shows a median track, formed by the tail in being drawn along the ground; on each side of this the hind feet with outspread toes leave their mark, while the forefeet just touch the ground, leaving dot-like impressions nearer the median line. Hence the author thought that while some of the bipedal tracks which are met with from the Trias upwards may be the "spoor" of Struthious birds, most of them are due to the bipedal progression of the Secondary Reptiles. [This abstract was unavoidably "crushed out" in our last number.]

Further Evidence of the Anatomy of Hypsilophodon Foxii has been brought before the Geological Society (Dec.) by Mr. J. W. Hulke, F.R.S., in a recent note. The material for this note was a slab from Cowleaze Chine, containing portions of two individuals of *Hypsilophodon Foxii*, one consisting of a skull with a great part of the vertebral column, the other of a portion of the vertebral column. The author described some details of the structure of the skull, and especially the palatal apparatus. The pterygoids, which are not mesially joined, have a stout body, the posterior border of which bears a very large basi-syphenoidal process, and the left pterygoid retains the root of a strong quadratic process, in front of which the hollow outer border runs out into an ectopterygoid. In front of the pterygoids the palatines are partially visible, also separated by a fissure. Of the eight vertebræ, the three last are firmly anchylosed, and the seventh and eighth form part of the sacrum. They are constricted in the middle, and their transverse processes, which spring from the junction of two vertebræ, are bent backwards, joining the dilated outer end of the transverse processes of the next vertebra, including a large sub-circular loop. The second fragment of a vertebral column, which belonged to a smaller individual, includes the sacrum and several vertebræ. Near the skull the slab contains several very thin bony plates of irregularly polygonal form, regarded by the author as dermal scutes. In connection with the question of the generic rank of *Hypsilophodon*, the author stated that in *Hypsilophodon* the centre of the sacral vertebræ are cylindroid, and rounded below, whilst in *Iguanodon* they are compressed laterally and angulated below.

Death of Professor Reuss.—Geologists will be sorry to learn of the death of this eminent worker. He was a Fellow of the Geological Society of London, and he was the author of various papers on the Crustacea and Foraminifera.

A New Lemur from the Phosphate Beds of Quercy.—M. H. Filhol points out, in a note to the French Academy (says "The Academy"), the more important peculiarities of the cranium of another new genus of the family Lemuridæ, lately found in the phosphate beds of Quercy. The interorbital space is wide, and differs much from that of the *Lori*. The orbits are large, and indicate an animal of nocturnal habits; the temporal crests are united posteriorly to the frontal, while in the *Nycticèbes* they are directed backwards without being united. The teeth are not so sharp as those of the *Lori*, and the first premolar of the upper jaw is far less developed. The molars bear a great resemblance to those of the *Galogo*, but in that genus there is an interval between the first and second upper premolars, and the first upper premolar is very stout and presents the aspect of a canine, neither of which peculiarities is noticed in this fossil. The form of the lower maxillary is the same as that of *Galogo*, and the tympanic bone is also similarly developed. This fossil, to which M. Filhol assigns the name of *Necrolemur antiquus*, is closely allied to the *Galogo*, though it also presents some affinities with the *Lori*.

Does the word "Sarsen" mean Rocks?—A writer, who signs himself "R. F.", thus writes on this subject in the "Geological Magazine" (February). He says it was supposed by Dr. Stukeley that the word Sarsen came to us from the Phœnicians, and other writers since his time have adopted it as such; but the greatest authority of the present day confutes the opinion, and states "that no word in the English language is of Phœnician origin." If so, we must inquire what other language will assist us. In the Anglo-Saxon, *ses* is a rock or stone; *sessi*, a settle or seat; *sesan* or *sesen*, rocks. The *e* in *sesan* is without accent, and sounded like *e* in *there*, *ai* in *fair*, or as *après* (Fr). The word Sarsen is pronounced by the country people *sāsen*, omitting the *r*; so that, perhaps, the word Sarsen is no other than the Anglo-Saxon *Sesan*, "rocks," and correctly sounded by the Wiltshire descendants of our Saxon forefathers, who still retain many other words of the same origin in great purity; for, as Dean Trench has observed, "they have not gone from us, but we from them."

The Nature and Formation of Flint.—At a meeting of the Geologists' Association, held January 2, Mr. M. H. Johnson, F.G.S., read a paper on this subject. The object of the paper was to show the nature of several members of a large group of bodies occurring in sedimentary deposits of different ages, and which are generally known as nodules, and described as concretionary. Those specially alluded to were the Septaria from the London and Kimmeridge Clays, the Flints from the Chalk, Iron Pyrites from the Chalk, the Phosphatic nodules of the Gault, the Clay Ironstone nodules of the Carboniferous series, and the Ironstone from the Woolwich Beds. By the gentle action of solvents the structure of these bodies is revealed so as to be easily examined by the microscope. They are then found all to agree in possessing a silicified organic structure, which may be described as a network of fibres, or a mass permeated in every direction by anastomosing canals. This structure was subsequently filled in with other material, such as carbonate of lime, silica, bisulphide of iron, phosphate of lime, carbonate of iron, &c., the particular substance thus filled in depending upon the relative abundance of the substances dissolved in the interstitial water of the surrounding matrix.

Mode of Occurrence of the South African Diamonds.—A paper was read on the mode of occurrence of the South African diamonds, before the Geological Society of London, by Mr. E. J. Dunn, and is published in the "Geological Magazine" (January, 1874). In this the author stated that the diamonds of South Africa occur in peculiar circular areas, which he regards as "pipes," which formerly constituted the connection between molten matter below and surface volcanoes. The surrounding country consists of horizontal shales, through which these pipes ascend nearly vertically, bending *upwards* the edges of the shales at the contact. The rock occupying these pipes was regarded by the author as probably gabbro, although in a very altered condition. Intercalated between the shale-beds there are sheets of dolerite, &c., and dykes of the same rocks also intersect the shales at frequent intervals. Within the pipes there are unaltered nodules of the same dolerite. With regard to the relation of the diamonds to the rock of the pipes in which they are found, the author stated that he thought it probable that the latter was only the agent in bringing them to the surface, a large proportion of the diamonds found consisting of fragments. At the same time he remarked that each pipe furnished diamonds of a different character from those found in other pipes.

Discovery of a New Species of Fish of the genus Acrolepis in the Millstone Grit of Yorkshire.—At the monthly meeting of the Manchester Geological Society, held on the 27th of January last, Mr. John Aitken, F.G.S., of Bacup, exhibited and described a number of very fine specimens of fish of the genus Acrolepis, new to science, which he had obtained from the *débris* brought out in excavating a tunnel through Wadsworth Moor, in the neighbourhood of Hebden Bridge, near Halifax, in a bed of fine black shale, separating the third from the fourth, or Rinderscout Grit, the principal specimens having been obtained from nodular concretions which abound in the shale at this horizon. The specimens comprise two nearly perfect heads (in the jaw of one a tooth is exhibited *in situ*), and several parts of the body, illustrating the form and structure of the fish, amongst which are the remains of two or three fin-rays. The scales and head-plates are beautifully ornamented and covered with a fine enamel, the former being rhomboidal in form and profusely sculptured, having a number of distinct waving ridges and furrows traversing them in the direction of their longest axis, varying in number according to the position they occupied on the body of the fish, the ridges having a strong tendency to bifurcate, and not unfrequently to further subdivide into two or three branches, these often again converging and becoming re-united. The genus Acrolepis has been figured by Professor Agassiz, MM. Coy and King, the specimens having been obtained from the magnesian limestone of this country and Germany. The specimens, however, under consideration have much larger scales, and differ in the style of ornamentation, as well as in many other essential particulars, from any of the figures referred to, leaving no doubt as to their constituting a new species of this rare fish. The discovery also carries this genus into a new horizon, which has previously been considered as almost barren ground. It is now known to be co-extensive with the carboniferous system, ranging from the mountain limestone through the millstone grit and coal measures into the Permian formation, where it appears to have attained its maximum development, its remains having been found there more plentifully, both in our own country and on the Continent, than in any of the older members of the system. It is intended shortly to have the specimens described and figured, so that the palæontologist may have an opportunity of judging of the value of the discovery. In addition to the specimens above referred to, Mr. Aitken also exhibited remains of four or five other genera of fish, viz. Acanthodes sp. Cladodus, Rhizodus, Ctenocanthus and Palaconiscus, together with a considerable variety of other fossils, all from the same locality.—[We may mention that these statements have been made to us by a correspondent who has given no name or address.]

MECHANICS.

An Ingenious Form of Pyrometer has been devised by Mr. Bailey. It is used for indicating heat, saving coal, and promoting uniformity of production in malt-kilns, ovens, and in other places where a certain degree of heat is requisite. The pyrometer for malt-kilns is 4 ft. long, and has an enamel dial 4 in. in diameter; the dial is indicated to 300°—black figures on a white ground. One of these pyrometers has been tried at the Valley Mill, near Holyhead, and the proprietor has tested it and found it very sensitive at any change of temperature, enabling the man to keep his kiln at the proper heat, which is very important in malt-kilns. These pyrometers are used by the Government departments for baking bread; they are also used for indicating the waste heat in flues of works and locomotives, for indicating the temperature of blast-furnaces, gas retorts, and other useful purposes where high temperatures are used.

Winstanley's Coal-cutting Machine.—Certainly mechanical invention seems to rapidly do away with the necessity for multitudes in the shape of hand labour. This machine is designed for holing in mines which are worked on the wide work or long wall system. It is driven by compressed air, the pressure required being from 20 to 30 lbs. per square inch, according to the nature of the coal to be cut. The height of the machine is 22 in., and the gauge of the wheels can be made to suit any ordinary colliery tramway. The cutter holes its own way into the coal, cutting from nothing up to 3 ft. or more in depth, the thickness of the groove being 3 inches. The small coal made by holing represents only from 25 to 35 per cent. of the quantity of small coal produced by hand holing. The average rate of holing in hard coal, with a pressure of 30 lbs. per square inch, is 25 yards per hour, including stoppages, and this may be considered to equal the work which would be done by at least thirty men in the same time. See "Report of the Manchester Society for the Promotion of Scientific Industry."

MEDICAL SCIENCE.

The way in which the Body uses up its Food.—A paper by Herren Pettenkofer and Voit, which originally appeared in the "Zeitschrift für Biologie," Band ix., Heft I., is of some interest. The present paper deals only with the processes of disintegration which occur in the body when varying proportions of meat and fat are given as food. In some of these 1,500 grammes of meat were given with 30, 60, 100, and 150 grammes of fat; in others 500 grammes of meat with 200 of fat; and so on. Their experiments showed that fat is absorbed in large quantities from the intestine, and that within certain limits the larger the quantity of fat in the food the more is absorbed. But when a certain proportion has been stored up in the body, less is taken up from the intestine. The most important conclusion at which they have arrived is that albumen is, under ordinary circumstances, more easily split up in the body into simple products than fat, so that so far from fat retarding the disintegration of albumen, albumen, if taken in sufficient quantity by a carnivorous animal, delays the oxidation of the fat, by splitting up into some form of oleaginous compound and other secondary products, the former of which is more easily oxidizable than ordinary fat. The fat derived from the albumen must of course be estimated as food fat, and viewing it in this light, it may be said that the consumption of fat in the body increases with the amount of albumen present in the body, or in other words, the better the general state of the nutrition of the body, the more fat is disintegrated. Lastly, they show that the disintegration of fat increases notably with physical exertion.

Why does Albumen (white of egg) Coagulate?—MM. Mathieu and Urbain give the following explanation of this. They say that when the gases dissolved in serum of blood are completely extracted, there is obtained an albuminous liquid, which does not coagulate, even at a temperature of 100°. This experiment, performed with egg albumen, formed the starting-point of the present research. The mercury pneumatic machine extracts not only the gases from albumen, but the volatile salts. The two cases are considered separately. The authors show—1. That carbonic acid is the agent of coagulation of albumen by heat; and 2. That albumen, deprived of its volatile salts, is transformed into globulin.

Connection of Bright's Disease of the Kidney with Changes in the Blood-vessels.—Dr. A. L. Galabin, M.A., has been good enough to forward us a copy of his excellent treatise on this question. It is elaborate as an essay, profusely illustrated, and deals cleverly with a very important question. We fancy that the time will come when all the modern ideas of the connection between Bright's Disease and any condition of the capillaries will be completely overthrown. Till then we must accept doctrines like those of the author, which are certainly [as will be seen by his essay, published by Smith & Elder,] most ingenious.

Pathological Specimens by Post.—It is extremely important that some means may be devised of sending pathological specimens by post, so that they may preserve all their characters. We have heard of various plans that have been proposed, but none of them seems so practical as that Dr. Richardson, an American microscopist, describes in a recent number of the "Philadelphia Medical Times." He says:—Place a small fragment of any tumour or pathological structure, say a quarter to half an inch square and one-tenth of an inch thick, in a couple of drachms of saturated solution of acetate of potash, and allow it to fully imbibe the fluid by soaking therein for forty-eight hours. The solution is best made by simply pouring half an ounce of rain water upon an ounce of dry granular acetate of potash in a clean bottle. When the tissue is fully saturated with this saline liquid, remove it with a pair of forceps without much pressure, and insert it in a short piece of india-rubber tubing, or wrap it up carefully in a number of sheets of thin sheet rubber or oiled silk, tying the whole firmly at the ends with stout thread. When thus prepared, specimens can be inclosed with a letter in an ordinary envelope and sent long distances, doubtless thousands of miles, by mail, without danger.

Is Carbolic Acid really Useless as a Disinfectant?—This is certainly a question that should be answered, and that early, for American physiologists appear to think it is useless. In the last number of the "Lens," a Pensylvanian journal, it is stated that most questions have two sides, and it is wise to look at both. While we have been disinfecting with carbolic acid, chlorine and coal gas, and fumigating with burned tar and sulphur, Jerome Cochran, M.D., professor of hygiene and medical jurisprudence in the Medical College of Alabama, and censor of the State Medical Association, seriously questions if there be any disinfectant virtue in those crude materials of our sanitary regulation. Professor Cochran writes nearly four columns in the "Mobile Register," of a late date, on this subject, and fortifies himself behind some stubborn facts. He is evidently well read in the subject whereof he treats. After reviewing the action of the Mobile Board of Health, which contended that the comparative exemption from yellow fever in that city was due to this disinfecting agency of carbolic acid, Professor Cochran says there is not a particle of reliable evidence to show that they have derived any benefit at all from all the carbolic acid scattered in their streets and yards. He contends that if carbolic acid has any power to destroy the infectious germs of yellow fever, it ought to exhibit that power most clearly where it has been most freely used. He shows that the City Hospital of Mobile has been more thoroughly disinfected than any other part of the city; that the whole atmosphere in the vicinity has been saturated with it for weeks, and yet the protective virtues of disinfection have failed to check the progress of yellow fever in the hospital and vicinity, and have also failed to modify its type, while at other places in the city where disinfectants were not used there was no fever. He claims that in the experience of Mobile, time and money have been thrown away in the use of disinfecting agents.

Examining the Remains of Petrarch, the Great Italian Poet.—A contemporary states that the Italian Scientific Commission appointed to examine, from an anthropological point of view, the remains of the Italian poet Petrarch, and to publish the result of its observations at the centenary of the great poet, proceeded, we learn from "La Nature," in the beginning of December, to open the urn of red granite, amid a large gathering of people. The bones, instead of being contained in a coffin of wood or metal, were spread upon a simple plank, and were of an amber colour, moist, and partly mouldered. The cranium, of medium size, was intact, the frontal bone much developed. The jaws still contain many teeth, among which were a number of molars and incisors very well preserved. The orbits were very large. Nearly all the vertebræ and ribs were found. The bones of the pelvis were in good condition, as also the scapula, the humerus, and the other bones of the arms; the apophyses of the femurs were very prominent. There was discovered also a quantity of small bones which probably composed the hands and the feet. The vestments were reduced to a dark powder. From the size and length of the bones, we may conclude that Petrarch was a man of middle height and robust constitution.

The Hygienic Value of the American Liquor Laws.—It is stated in the "Boston Medical and Surgical Journal," that the liquor law at present in force in Massachusetts has "produced nothing but opium-eating, secret drinking, hypocrisy, black-mail, and State constables." The law will probably be modified or repealed by the new legislature.

METALLURGY, MINERALOGY, AND MINING.

An Ingot of Palladium.—An ingot of this exceedingly rare metal was exhibited at Vienna last summer, in the English Department. It was valued at 2,500*l.*, having been extracted from about 1,520,000*l.* (!) worth of platiniferous gold.

Tempering Steel for Tools.—The "Chemical News," quoting from the "Bulletin de la Soc. Chim. de Paris" (Vol. xx. No. 10), says that the following procedure, due to M. Kulicke, is in use at the works at Saarbruck. It serves to restore the nature of steel altered or burnt, and consists in plunging the article—previously brought to a cherry-red heat and forged for an instant—into the following mixture, and then into cold water:—Tartaric acid, 12 parts; fish-oil, 60 parts; powdered charcoal, 4 parts; bone-black, 16 parts; yellow prussiate, 20 parts; tallow, 20 parts; burnt stag's horn, 6 parts.

Experiments on the Colours of the Diamond.—At the meeting of naturalists which took place recently at Wiesbaden, and of which an account is given in the "Chemical News" (February 26, 1874), and in the Mineralogical Section, Herr Flight described his experiments on the colours of the diamond. A rose-coloured diamond of twenty-nine carats, exhibited at Paris in 1867 by Coster, of Amsterdam, was bleached in four minutes on exposure to diffused light, but resumed its colour when heated in asbestos, and retained it if preserved from daylight. Two dull yellow diamonds from the Vaal river were selected, one of which was preserved for comparison, while the other was subjected to modifying experiments. On being heated to redness in a current of hydrogen, it was found colourless when cold, but gradually assumed its colour on exposure to daylight. If heated in a current of chlorine the result was the same.

Crystallographic and Chemical Features of Caledonite and Lanarkite.—A paper was contributed to the Chemical Society in December by N. S. Maskelyne and W. Flight on the crystallographic and chemical characters of caledonite and lanarkite. Some specimens from Leadhills, recently procured for the British Museum, having been found on analysis to have a composition differing from that of caledonite, a specimen of true caledonite was also examined, and, while neither analysis accorded with that of Brooke, both agreed in showing this mineral to be lead sulphate in combination with lead hydrate and copper hydrate. Lanarkite was found to contain no water, and to be a compound of lead sulphate and lead oxide, as Pisani has shown.

A Year's Amount of Sulphur.—It seems that in 1871 the Italian (Sicilian) mines of sulphur produced . . . . . 6,860,000 cwt.

mines of sulphur produced	6,860,000 cwt.
The rest of the world in the same year produced .	152,500 „
	7,012,500
Equal in gross tons to	351,625

The quantity of sulphur produced from the roasting of pyrites is insignificant, this industry being occupied almost exclusively in the production of sulphuric acid.

MICROSCOPY.

The Structure of Filaria Immitis.—This is very fully described in a valuable paper published in the "Monthly Microscopical Journal," No. LVIII., by Mr. F. H. Welch, F.R.C.S. He says that by the aid of glycerine and high microscopic powers the following details were brought out:—The parietes were composed of cutaneous and muscular strata. The former consisted of a corium externally covered by imbricated longitudinal layers of a beautifully delicate minute epidermis, much resembling that on ophidians; the latter was made up of three layers in the following relation from the corium inwards: two oblique intersecting each other at an acute angle, a longitudinal, a circular, all varying in the degree of development in different situations. The combined muscular and cutaneous tunics constituted about one-eighth part of the total body diameter, and formed a "tube charnu" to the loosely-lying contained viscera. The mouth was a small circular aperture in the centre of a papilla, occupying the most prominent portion of the rounded anterior extremity of the worm, and the longitudinal muscular layer diverging from it was very strongly pronounced. Continuous from this was the œsophagus, about $\frac{2}{10}$ inch in length, and $\frac{1}{200}$ inch in thickness, with strongly pronounced walls made up of a longitudinal and circular muscular layer. At the junction of the gullet with the stomach was a clearly-defined sphincter. The stomach, or rather alimentary tube, into which the œsophagus opened, expanded from the sphincter into a delicate membraneous canal nearly $\frac{1}{100}$ inch in thickness, pursued a straight course along the body length of the worm, diminishing at the centre to $\frac{1}{130}$ inch, and could be traced to within half an inch of the tail end, where it terminated in a cæcal extremity about $\frac{1}{400}$ of an inch in diameter, lying between the convoluted ovarian tubes, or, in the case of the male worm, either above or below the sperm duct. Its delicate wall was made up of very fine longitudinal and circular fibres, and retained within it fat globules and granules, and not uncommonly red colouring matter, doubtless derived from the blood of the host. The alimentary canal was encircled throughout its entire length by the reproductive organs. It will thus be seen that there was no anal aperture, a circumstance possibly connected with the life history of the worm passed within the vascular canals of the host, and with the nutriment obtained from a vital fluid comparatively free from effete products; the alimentary excreta of the parasite being thus reduced to a minimum, if not an actual nullity.

On Microscopic Cements.—Mr. F. Kitton makes some remarks on this subject in a letter to the editor of the "Monthly Microscopical Journal" (No. 61), which are worthy of being recorded. He says:—"For dry mounting, where only very shallow cells are required, I have found nothing better than asphalte dissolved in benzole, with a small quantity of gold size added. The cells should be made by the addition of successive layers of varnish, each layer to be hardened before the next is put on. When thick enough, the slides should be placed in a cool oven and allowed to remain all night. In order to attach the cover, I put a fresh layer of asphalte (without gold size) on the surface of the cell, and allow it to remain exposed 5 or 6 minutes. The cover may now be placed upon it, and pressed upon the cell by a slide, previously heated to ensure perfect contact, and it may now be finished off with an exterior ring of the asphalte (No. 2); or if it is wished to put a coloured ring or rings round it, a layer of ordinary shell-lac varnish should be run round it before using them. I have found the dammar cement, made by Mr. W. White, of Litcham, the best medium for mixing the colours with; a few drops of gold size may be added with advantage. I would advise the use of zinc white, in preference to white lead, as the latter turns yellow in the course of a month or two. I give the preference to vermilion or purple lake for the exterior ring, finishing with an interior ring of zinc white."

Mr. Wenham's Position as a Controversialist is admirably, and, we need hardly say, most tersely defined by himself in a contribution to the March number of the "Monthly Microscopical Journal." For ourselves, we must certainly say he appears to have kept his temper throughout in the most admirable manner, and he has formed a very excellent comparison to some other gentlemen who shall be nameless for the present. Mr. Wenham says:—"The controversy was begun by myself three years ago; I then disputed what I considered to be an erroneous theory, appearing in an essay illustrated by large diagrams, to prove an increase of aperture alleged to be obtained by immersion lenses,* and pointed out how rays were taken from wrong positions in impracticable constructions. The author of the essay referred to had merely carried the rays into imaginary front lenses, and there deserted them, regardless of their ultimate destination to a posterior conjugate focus at the eye-piece. The subject has been kept up at intervals by correspondents, with whom I have still to deal. It is difficult to do this without exciting some degree of irritation, because the nature of such strictures implies ignorance of the laws of optics—a science which has long been so exactly defined that there should be no error of the passage of a ray through refracting surfaces. According to disposition, so do modes of controversy differ: perhaps my own is an obnoxious one. Let it be compared. Some always write in the first person singular, and reiterate their views as if to command belief, conceding no credit, and ignoring all replies except to those who favour their assumptions, with an air that says—

'I am Sir Oracle, and when I ope my mouth let no dog bark!'

This scarcely brings forth fruit. I am always glad to exchange notions with practical working men, and to give my own in ordinary phraseology. Then there is discussion with a snarl, of which this aperture questions affords some examples. This is the least productive of any, for its main strength consists merely in picking out contradictions and anomalies of phrase; science is tossed aside, and its cold reasoning avoided because it is not understood, and abuse is mistaken for keen argument. Satire may appear in discussion, arising not from ill-temper, like the last, but it is apt to offend. I find it difficult to restrain the propensity at times, though quite aware how few can accept it. Again, there is the meek and amiable style, that can neither make or answer a strong objection, saying, 'I am sorry that I have disturbed you, gentlemen; I will drop the question rather than disagree!' This I cannot take credit for, but judging from the attitude of some of my opponents, it would seem as if I am a wolf amongst the lambs."

Two New Nobert's Test-Plates have been received by Dr. Woodward, of America; and although the fact was announced in the "Quekett Club Journal," we have not seen it till we observed Dr. Woodward's notice of the circumstance in the "Lens." This is because the "Quekett Club Journal"

* "Monthly Microscopical Journal," vol. iii. p. 16.

is not sent to us among the other journals, British and foreign, which we receive. Dr. Woodward says:—"I may mention here, as a matter of interest, that I have recently examined two new test-plates by Nobert—the first ruled for Professor Barnard, of Columbia College; the second for the Army Medical Museum—in which the maker has attempted to rule lines twice as fine as those of the nineteenth band. These plates have twenty bands. The first ten correspond respectively to the 1st, 3rd, 5th, 7th, 9th, 11th, 13th, 17th, and 19th of the old plate. The lines in the second group of ten bands purport to be ruled at the following distances apart:—The 11th band $\frac{1}{11000}$ of a Paris line, the 12th band $\frac{1}{12000}$, and so on up to the 20th band, lines of which are said to be $\frac{1}{20000}$ of a Paris line apart. As I have not yet been able to resolve any of these new bands, I will not at present express an opinion as to whether Nobert has actually succeeded in ruling them as attempted."

Papers of the Quarter on Microscopical Subjects.—The following have been published in the "Monthly Microscopical Journal" for January, February, and March, and some, especially those of Mr. Dallinger, are of great importance:—

Notes on so-called Acarellus. By S. J. McIntire, F.R.M.S.—Further Researches into the Life History of the Monads. By W. H. Dallinger, F.R.M.S., and J. Drysdale, M.D.—On the Microscopic Structure of a Granitoid Quartz-porphyry from Galway. By Professor Edward Hull, M.A., F.R.S.—The Structure of the Scales of Lepisma Saccharina. By G. W. Morehouse.—On the Origin and Development of the Coloured Blood Corpuscles in Man. By Dr. H. D. Schmidt, New Orleans.—Further Researches into the Life History of the Monads. By W. H. Dallinger, F.R.M.S., and J. Drysdale, M.D.—On a Simple Method of Preparing Lecture-Illustrations of Microscopic Objects. By Rev. W. H. Dallinger, F.R.M.S.—A Method of Dissecting Podura Scales. By F. H. Wenham, V.P.R.M.S.—The President's Address.—Further Researches into the Life History of the Monads. By W. H. Dallinger, F.R.M.S., and J. Drysdale, M.D.—Further Notes on the Zoosperms of Crustacea and other Invertebrata. By Alfred Sanders, Lecturer on Comparative Anatomy at the London Hospital Medical College.—Angular Aperture of Object-glasses. By F. H. Wenham, Vice-President R.M.S.—Further Remarks on Immersion Apertures. By J. J. Woodward, Assistant-Surgeon U.S. Navy.

PHYSICS.

A New Form of Maximum and Minimum Thermometer was some time since described in a French scientific journal. It is formed of a compound spiral, consisting of two differently expansible metals electroplated. By increase of temperature it tends to unroll, by decrease to twist up; in so doing it moves one or the other of a pair of indices over a graduated scale, the one of which registers the highest temperature attained, the other the lowest. If required, a third needle may be added, by means of which the actual temperature at any moment may be read off without disturbing either of the others. The instrument is used at several meteorological stations in Switzerland and Russia, and gives great satisfaction.

The Evaporation of Volatile Liquids.—The "Philosophical Magazine," No. 46, states that M. Stefan has lately conducted a series of researches on this subject. In the following experiments, to avoid the great lowering of the temperature at the surface, narrow tubes were chosen for evaporating vessels, instead of the wide vessels hitherto used; with them he arrived at the following results:—(1.) The velocity of the evaporation of a liquid from a tube is inversely proportional to the distance of the level of the liquid from the open end of the tube. This law holds with rigorous exactness when the distance of the level a little exceeds 10^{mm}. (2.) The velocity of the evaporation is independent of the diameter of the tube. This result was obtained from experiments with tubes the diameter of which varied from $0{\cdot}3^{mm}$ to 8^{mm}. (3.) The velocity of the evaporation increases with the temperature, so far as with this the vapour-pressure of the liquid rises. If p be the maximum of elasticity of the vapour corresponding to the temperature of the observation, P the atmospheric pressure under which the liquid evaporates, the velocity of the evaporation is proportional to the logarithm of a fraction of which P is the numerator and $P-p$ the denominator. If the pressure of the vapour becomes equal to that of the air, this logarithm becomes infinitely great, and signifies that under this condition the liquid boils.

Interruptions to the Atmosphere's Power of Conducting Sound.—One of the most interesting and important papers that have appeared in that Journal for some years is that by Professor Tyndall on his experiments on the conveyance of fog-signals (P.R.S. 149). It will be found in full at p. 175.

Incandescent Substances Transmitting Electricity.—M. Donlot contributes a paper on this subject to the "Comptes Rendus" (December 22, 1874). He says that carbon and platinum act oppositely. Thus, a cylinder of glowing charcoal being substituted for the ball of an electroscope, if one bring near a positively charged body, the leaves will quickly diverge till they are discharged by the metallic balls on either side; then diverge again, and be again discharged; and so on, as long as the electrified body is held near. If it be removed before the leaves reach the balls, they remain apart, with positive electricity; thus the carbon has allowed the negative electricity to flow off. The results are different where a negatively electrified body is used. The electroscope is affected only at a very small distance; the leaves separate less quickly, and they come together promptly, whenever the source of electricity is withdrawn. These phenomena are quite opposite to those observed by M. Erman with the uphlogistic lamp—that is, with an incandescent platinum wire—which indicate that incandescent platinum allows positive electricity to flow away more readily than negative.

A Physical Society was much wanted in London, and now it is established. A numerously-attended meeting was held on Saturday, Feb. 14, 1874, in the Physical Laboratory at the Science Schools, South Kensington, for the purpose of establishing a Physical Society in London. The chair was taken by Dr. J. H. Gladstone. The bye-laws prepared by the Organising Committee appointed on Nov. 29 were received and amended. The following were chosen officers for the first session:— *President*—Dr. J. H. Gladstone, F.R.S. *Vice-Presidents*—Prof. W. G. Adams, F.R.S., and Prof. G. C. Foster, F.R.S. *Secretaries*—Prof. E. Atkinson and Prof. A. W. Reinold. *Treasurer*—Prof. E. Atkinson. *Demonstrator*—Prof. F. Guthrie. *Other Members of the Council*—W. Crookes, F.R.S., Prof. A. Dupré, Prof. T. M. Goodeve, M.A., Prof. O. Henrici, B. Lœwy, Dr. E. Mills, and H. Sprengel.

ZOOLOGY AND COMPARATIVE ANATOMY.

Large Cuttlefish.—Our first article (see p. 113) shows that it is no longer a mere idea that huge cuttle-fish are in existence. But besides this we may refer to a paper which Mr. Saville Kent, F.L.S., F.Z.S., read before the Zoological Society of London on March 3, on a huge Cephalopod or cuttle-fish, announced by the Rev. M. Harvey as lately encountered in Conception Bay, Newfoundland, and of which a tentacle sixteen feet long has been secured for the St. John's Museum. Mr. Saville Kent contributed the additional evidence of an arm nine feet long preserved in the British Museum, in proof of the gigantic dimensions occasionally attained by certain members of this order of the Mollusca, and proposed to institute the new generic title of *Megaloteuthis* for their especial reception; he further suggested distinguishing the Newfoundland example as *Megaloteuthis Harveyi*, in recognition of the service to science rendered by Mr. Harvey, in his record of, and steps taken to preserve, so valuable a trophy.

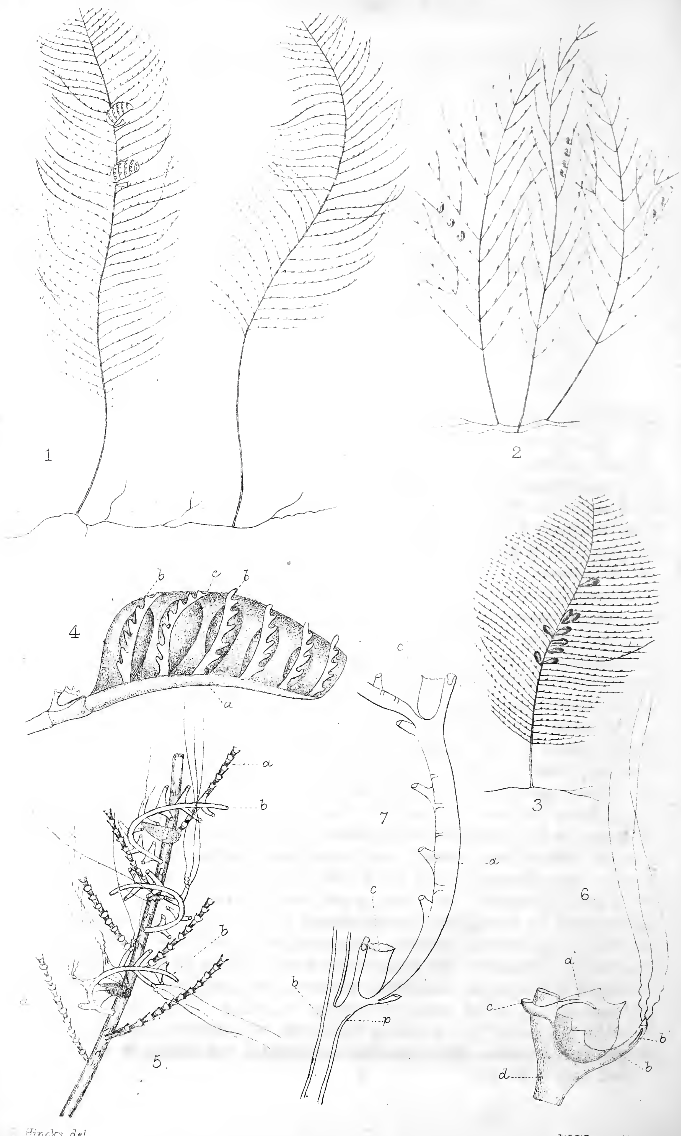
The Museum of Comparative Anatomy, Cambridge, Massachusetts, U.S.A., has, we learn, been placed under the direction of Mr. Alexander Agassiz, son of Professor Agassiz, and Mr. Carey, both of whom are thoroughly conversant with Professor Agassiz's plans with regard to the Museum, and familiar with the collections. Who will take the Agassiz lectureship we cannot tell, though we hear it has been offered to a most distinguished English candidate—one who would do it infinite credit.

A new Classification of Birds.—Another of the contributors to the present number of the "Popular Science Review," Mr. Garrod, B.A., F.L.S., lately (Feb. 3) read a paper before the Zoological Society of London, in which he proposed a new classification of birds, founded mainly on the disposition of their muscles and other soft parts. The five muscles which he had observed to vary most were the ambiens, the femoro-caudal, the accessory femoro-caudal, the semi-tendinosus, and the accessory semi-tendinosus. After stating which of these are present or absent in the different families of birds, he showed that the presence or absence of the ambiens muscle is so intimately correlated with other characters that a division of the whole class into *Homalogonati* and *Anomalogonati*, depending on that peculiarity, would stand the test of much criticism. The Homalogonatous birds were divided into the Galliformes, the Anseriformes, the Ciconiiformes, and the Charadriiformes; the Anomalogonatous into the Passeriformes, the Piciformes, and the Cypseliformes. Among the most important changes proposed or substantiated were the placing *Serpentarius* and *Cariama* with the Otididæ, the Cypselidæ with the Trochilidæ, and the Musophagidæ among the Galliformes.

The Anatomy and Varieties vf Myxine.—At a recent meeting of the Boston Society of Natural History (U.S.A.), Mr. F. W. Putnam read a paper on *Myxine,* a low genus of fishes, known to fishermen as *hags,* giving an account of its anatomy, which was illustrated by a series of specimens exhibited. The several species described by various authors must be reduced to one, having a wide geographical distribution, being found on both sides of the Northern Atlantic, and also on the southern coast of South America. Mr. Putnam showed that the variations in the number of lingual teeth, which are from eight to eleven in each row in specimens from the North Atlantic and from the Straits of Magellan, could not be considered as of specific importance. The different varieties of this species he considered as follows:—*Var. septentrionalis,* the short and thick form, from the North Atlantic, *var. limosa,* the long and slender variety, also from the North Atlantic; while the southern variety may be called *australis,* the name under which Jennyns described it as a true species.

The New York Aquarium.—We are informed that this splendid institution is progressing as rapidly as can be expected. It is to be erected in the Central Park, New York, where the magnificent Free Museum and Menagerie have already a place. We believe that the credit of starting this enterprise is due to the Messrs. Appleton, the proprietors of the well-known "Appleton's Journal." These gentlemen have communicated with Mr. W. S. Kent, F.L.S., who has left the Brighton Museum, and we believe that with his assistance as scientific adviser they cannot fail to establish an institution which will be in every way creditable to zoological science. We trust they may soon have secured ample funds to realize the undertaking; and we have no doubt that, with Mr. Kent's assistance, experienced as he is not only as a naturalist, but also, and especially, as an aquarian naturalist, they will eventually attain most complete success.

A Swarm of Ephemerids.—At a meeting of the Philadelphia Academy of Science late last year, Mr. Gentry communicated a notice of a great swarm of ephemerids which passed through the town of Lewisburg, on the Susquehanna river one afternoon. The swarm was estimated to be about a mile in length by nearly a half mile in width, and was so dense as even to obscure passers-by on the opposite side of the street.



Hincks del.

W. West & C^o. sc.

Plumularians.

PLUMULARIANS.

By the Rev. THOMAS HINCKS, B.A., F.R.S.

[PLATES CX., CXI.]

THE *Plumularians* constitute a natural and strongly marked group of hydroid zoophytes. To the systematist they present salient structural characteristics; to the biologist obscure and fascinating problems; while they delight the eye of the least scientific observer by the gracefulness and delicacy of their feathery forms. Not only are the features of the family sharply cut and of very definite type, not only are its habit and expression singularly distinctive, but it also offers to us some morphological peculiarities, which, as it were, isolate it amongst its recent kindred. A sketch of this most exquisite and original group, as free as may be from the technicalities of the mere specialist, may possibly have some interest even for those who are not themselves students of the Hydroida, and fittingly find a place in the pages of the Popular Science Review.

The most obvious character of the family, suggested at once by the name, is the plumous habit, which gives it its peculiar beauty. Each colony consists of a number of elegant plume-like shoots, delicately wrought in chitine, which either spring singly from the trailing fibre that binds them to weed or stone, or, borne on erect and branching stems, form large and composite growths which are almost like miniature palms in general aspect. Our British species are for the most part of the simpler habit and of humble size; but in the warmer seas the family is represented by lovely arborescent forms which sometimes attain gigantic proportions. Another salient character of the Plumularian family is found in the disposition of the calycles, or dwellings of the polypites, on one side of the ramules only, instead of on two sides, as in the allied family of the Sertularians. Each of the delicate pinnæ of the plumule bears on its anterior aspect a single line of these little cup-like receptacles, exhibiting in the various species many quaint and elegant forms, hyaline or amber-tinted, and often adorned with curiously crenated or spinous margins. In one section the calycles are set close together in a continuous series (Pl. CX., fig. 1); in another they are more or less remote (Pl. CXI., fig. 1); and this apparently trivial distinction is associated with other more important differences which divide the family into two well-marked groups. Of these I shall have more to say hereafter.

A dry account of the structural features and the arrangement of parts, however accurate, can give no idea of the living beauty of these exquisite organisms. The grace of their curves, the hyaline delicacy of their texture, the nameless charm of their entire figure cannot be expressed in a diagnosis. In the attempt to render them the pencil is at fault—much more the pen. Yet I must endeavour to give a general notion of a characteristic form in the living state, and to suggest the principal elements of the beauty that is common to the entire family before proceeding to the special points of structure to which this paper will be chiefly devoted.

I shall select as an illustration one of the smaller British species (Pl. CXI., fig. 2), which is neither better nor worse than its kindred, but a fair average specimen of its race. Imagine, then, a piece of rock, the surface of which and of the sinuous tubes of the *Serpula* incrusting it, is netted over by the finest of fibres, from which rises a whole forest of exquisite plumes. The tallest of them is less than an inch in height; they are all but colourless, so delicate is the horny material composing them, and their slender, shadowy forms are only distinguishable in a strong light. The trailing tubular thread from which they spring may remind us of the *rhizome* that gives origin to the fronds of the fern; it is the common base of the Plumularian commonwealth, holding its many families in organic union, and binding them to the surface over which they spread. Each plume * was pushed forth as a bud from the prolific pulp pervading it, and is itself a branching tube inclosing an offset from the common flesh.

The plume is a colony, sharing the life of a great commonwealth and helping to nourish and enlarge it. The delicate pinnæ, set for the most part alternately along its main stem, bear at intervals minute transparent cups (Pl. CXI., fig. 3 *c*), the homes of as many hydræ which expand above them their wreaths of slender, beaded arms, all of them bound together as one organism by the ramified thread of flesh pervading the structure. In this section of the Plumularian family, the polypites are not wholly retractile within the calycle; only the base of

* Except the first, which is developed immediately from the embryo.

the body is protected by it, the rest is always exposed. In the present species a conspicuous band of opaque-white encircles the body, like a girdle, a little below the tentacles, and adds much to the beauty of a colony in full life and activity, when its many polypites are in eager pursuit of prey, stretching themselves forward, and casting forth their flower-like wreaths, now suddenly clasping their arms together, and then as suddenly flinging them back; now holding them motionless, the tips elegantly recurved, and then on some alarm shrinking into half their size, and folding them together like flowers closing their petals when the sun is gone. Distributed over the stem and branches are a number of smaller cups (*calicetti*, the Italians call them) of peculiar form and structure, which contain, not hydræ, but zooids of another kind, of which I shall have much to say (Pl. CXI., fig. 3 *s*). They are very characteristic of the Plumularian family, and, as we shall see, suggest some curious speculations; and, it may be, afford a clue to the genealogy of the tribe.

And if in the polypites, with their wreathed arms, which stud the surface of these vegetative animals, we may fancy a resemblance to the flowers of the plant, we may also find the counterpart of the seed-vessel in the elegant reproductive capsules scattered throughout the colony. In the species now under consideration (*Plumularia cornucopiæ*) they assume the shape of an inverted horn, and are formed of material translucent as the finest glass. Each of them, in fact, is a little crystal cornucopia, in which is lodged one of the reproductive members of the commonwealth, a class totally distinct from that which is charged with the function of alimentation. These graceful receptacles are several times larger than the calycles, from the base of which they spring, singly or in pairs, and within them the ova are produced and the embryos matured which are to give rise to new colonies. (Pl. CXI., fig. 3 *g*.)

It must be noted in passing that there is nothing stiff or angular in these beautiful organisms; no awkward attitude or graceless line. They are flexile and wavy; the mid-rib of the plumes is bent into the prettiest curves, and the pinnæ are elegantly arched. They are like the Birch amongst trees, in lightness, delicacy, and grace.

Plumularia cornucopiæ belongs, as I have said, to the section of the family in which the calycles are more or less remote from one another, and the polypites only partially retractile. In this division the "calicetti" * are distributed at

* I do not know whether my readers will thank me for informing them that these "little cups" have been named "Nematophores" and "Sarcothecæ" by writers on the Hydroida.

intervals along the stem and branches, and the reproductive capsules are unprotected. In the other principal section, the calycles are ranged close together in a continuous series, and the polypites can withdraw themselves wholly into their little dwellings; the "calicetti" are disposed in definite and constant order round the calycles, and the reproductive capsules are either collected in groups and inclosed in a pod-like case, or all but universally associated with some kind of protective appendage. In Plate CX., fig. 1, a member of this section is represented, and a glance at it will show that, however it may differ in minute structure from the form which I have just described, it presents the same general habit, and is as conspicuous for grace of figure and delicacy of detail. Two of the ribbed and crested cases which shelter the reproductive capsules are shown standing out from the mid-rib amongst the pinnæ, and will at once attract attention from the singularity of their form.

A familiar member of this section of the Plumularian group is the common "Podded Coralline," which overspreads some of the larger Fuci, covering them with its network of fibres, and hanging on every spray a multitude of its amber plumes. What masses I have seen of the "podded weed" (*Halidrys siliquosa*) invested throughout by this elegant hydroid, overgrown by the ramifying thread, which had pushed its way over stem, branch, and branchlet, and sent up at all points hundreds of the plumous shoots, each of which bore aloft a company of polypites, a vast parasitic population!

Such a commonwealth is a wonderfully complex unity; a single germ its origin; a single life pervading it; thousands of *quasi* independent elements included in its manifold individuality. A single plume of *A. pennatula* (Pl. CX., fig. 3) of moderate size may bear some fifteen hundred hydræ; and a plume is but one state of a great federation. Dana reckons the number of polypites on a single specimen of a large foreign species, which reaches a height of three feet, at not less than eight millions; and this is but a pigmy compared with the gigantic form from the Pelew Islands, described by Semper, which attains a height of five or six feet and spreads as a veritable forest over the bed of the sea.

We are not acquainted with many variations upon the Plumularian type; the family has hitherto included few genera, but the number will probably increase considerably as we become more familiar with the modifications of the reproductive system. Two of the sub-groups I have already briefly characterised; in the "Sea-beard" (*Antennularia*), one of the commonest waifs on our sandy shores, the pinnæ are ranged round the stem in whorls, and the plumose appearance is lost. The divergence from the normal arrangement in this case is apparently great, but the gap between this and the common pattern is bridged over by intermediate forms. In yet another section, the pinnæ are shortened, and carry only a single calycle and hydra, and here again the feathery character is less distinct; but the loss in this respect is compensated for by other elements of beauty. A single species of this section (*Plumularia obliqua*) is found on our shores, a fairy-like form of exquisite delicacy and grace; and, strange to say, it is equally at home at the Antipodes. I have masses of Australian algæ over which it spreads in extraordinary luxuriance, as though the habitat were thoroughly congenial to it. From the same region I have obtained another and very remarkable form (still undescribed) which is referable to this section. Other sub-divisions of the Plumularian family are founded, as I have said, on differences in the reproductive system, with a single exception, to which I shall refer more particularly hereafter. The principal varieties in external aspect have now been noted, and it will be seen that the main features of form and habit are strongly marked, and very constant throughout the tribe.

I proceed to notice some very interesting points of structure, which are peculiar to this family and have no parallel amongst recent hydroid zoophytes. Allusion has frequently been made to the "calicetti," which occur on the Plumularian colonies, in addition to the calycles in which the hydræ are lodged. They exhibit various forms, being sometimes tubular (Pl. CXI., fig. 4) and sometimes cup-shaped; they sometimes consist of a single chamber and sometimes of two chambers, placed one above the other. In the latter case the lower portion is tubular (Pl. CXI., fig. 5), while the upper expands into a hemispherical bowl; the whole may be compared to a goblet on a stand. These curious appendages are, like the calycles, an extension of the horny covering which invests the whole of the common flesh of the hydroid. In one section of the family their disposition is invariably the same; two stand out laterally, one on each side of the calycle, and one, which is often of considerable length, is adherent to the front of it, and has the appearance of supporting it like a bracket (Pl. CX., fig. 6). In the other section there is no such uniformity of arrangement; the "calicetti," which are here of the cup-shaped type (Pl. CXI., fig. 3), are distributed, often profusely, over the colony, in the neighbourhood of the calycles, along the course of the pinnæ, on the central stem, and even at times on the creeping fibre. They are very useful to the systematist, supplying him with good criteria for the discrimination of species. But their chief interest lies in the nature of their contents.

When examined in the living state, they are found to inclose an offshoot from the common flesh, or rather from the outermost layer of the common flesh (Pl. CXI., fig. 4 *d*). This offshoot is composed of a soft granular substance, devoid of definite structure—the "sarcode" of physiologists; the kind of material of which the humble rhizopod is made; a pulpy, semifluid mass, without consistency or stability, capable of the strangest behaviour, and actually "behaving" like a creature of much lower rank than the organism with which it is associated. In the compound sarcotheca (I must abandon the elegant Italian for the more cumbrous Greek designation) a column of sarcode pervades the lower and tubular chamber (Pl. CXI., fig. 5 *y*), and on passing into the terminal cup divides, in most cases, into two lobes, which are charged with different functions. From the inferior lobes (Pl. CXI., fig. 5 *b*), when the Plumularian is in healthy and vigorous condition, long filamentary processes are emitted, extensions of the sarcode mass, bearing the closest resemblance to those which an *Amœba* pushes forth as it moves sluggishly along in search of food. From the top of every cup these threads of sarcode stream forth, slowly lengthening as they follow the course of the stem or branch, until the plume is invested by the living gossamer. The extremity of the extensile process is usually closely appressed to the surface of the stem, over which it glides almost imperceptibly. In some cases the sarcotheca is furnished with two apertures, one terminal and the other opening into the calycle, and through the latter these strange protoplasmic offshoots are discharged. I think we must regard these curious structures as distinct zooids in the hydroid commonwealth; a band of stealthy workers, presenting a striking contrast to the vivacious hydræ, stealing along on their mysterious errands, and almost unperceived enveloping the colony with their slender threads. There seems to be a pretty constant efflux and reflux of these streams of sarcode. After a time the outflow is stayed, and as slowly as they had advanced, the processes are withdrawn, until they disappear altogether in the lobes which originated them. This may truly be called a marvellous structure, or assemblage of structures. It is as though a complex Rhizopodal and a complex Cœlenterate organism were bound up together. We have here amœboids and hydræ fraternizing in one and the same commonwealth!

The upper lobe of the column of sarcode pervading the tubular case (Pl. CXI., figs. 4 *a* and 5 *a*) plays a very different part. It emits no amœboid processes wrought out of its substance, but it bears a special apparatus of its own. It incloses a number of long, slender, somewhat bean-shaped thread cells, disposed in a cluster within the sarcode a little below the summit. From these, when the apparatus is in action, long, delicate threads are discharged, that stream upwards from the extremity of the sarcotheca. These fine extensile lines are cast out to enormous distances, intertwining and waving about in the water. At times a tuft may be seen slowly contracting, and I have observed one dragging down with it a mass of stuff which it had collected.* A specimen which I possess of an Australian Plumularian preserved in fluid, exhibits a really wonderful display of these thread-like organs, sheaves of which surmount a large proportion of the multitudinous "calicetti," and quite change the aspect of the hydroid (Pl. CX., fig. 5). The Italian naturalist Meneghini, who first figured these organs (so far as I know), represents them in this condition. From the aperture of every cup rises (in his figure) a tuft of very numerous tentaculoid appendages. The whole structure is not unlike a very delicate polypite, and as such he seems to have regarded it.

No description can give an adequate idea of this wonderful apparatus as it appears when in full action. The Plumularian colony, indeed, offers us a remarkable combination of vital movements in the play of the polypites as they seek their food, the stealthy outgoings of the amœboid processes as they slowly traverse the stems, and the rapid emission and extension of the waving threads.

I may mention in passing that the thread-cell, which is eminently characteristic of Cœlenterate organisms, has also been detected in some of the Protozoa.

When we come to inquire into the function of the curious structures which I have just described, we are very much at fault. As the thread-cells are stinging organs, the superior lobes of the sarcode mass (or sarcostyle) may naturally be regarded as so many forts with heavy armature placed for the defence of the colony. They may help to keep off carnivorous enemies, such as the naked molluscs (*Nudibranchs*), which are fond of browsing on the polypites of the Hydroida. But what of the amœboid processes emitted from the lower lobe? It seems not improbable that they may be subservient in some way to the work of nutrition; or if not, they may be instrumental in keeping the surface of the plume free from impurities and foreign growths which might be detrimental to the well-being of the colony. Considering the immense number of the sarcothecæ and the various structures and activities connected with them, it is impossible to doubt that they must bear some important relation to the life of the hydroid.

We may regard the amœboid processes, I think—and this is their most interesting aspect—as a clue to the genealogy of the Hydroida, as evidence of a time when there was a much closer

* *Vide* a paper by the author, entitled *Contributions to the History of the Hydroida,* "Annals of Nat. Hist." for Nov. 1872.

relationship between them and the Rhizopods than now exists; as 'survivals,' indeed, from a remote and very early stage in the evolution of the Order. They indicate the line of development along which the Hydroida have passed, and throw a light on the cradle of the race. If we may accept Professor Allman's ingenious interpretation of the Graptolites, a tribe of Silurian fossils, which has been bandied about from one division of the animal kingdom to another, but has now settled down amongst the Hydroida, we shall have still more direct testimony to the same effect. The Graptolite consists of a tubular stem, carrying a series of tubular offsets on one side of it, or on two opposite sides, and inclosing a solid rod or axis. It bears a sufficiently close resemblance to the Hydroida in general aspect and arrangement of parts to justify its association with them, notwithstanding certain points of divergence. The tubular offsets have been usually regarded as equivalent to the calycles in the recent species; but Allman, from an examination of their structure, has come to the conclusion that they represent sarcothecæ rather than the dwellings of the polypites. To him the Graptolite is a Plumularian *minus* calycles and (probably) polypites; a Plumularian hardly distinguishable from a composite Rhizopod, reduced to a level with this humble type in its ways of life, and dependent for its nutriment on the movements of its protoplasm. The Graptolite, we may say, according to this view of it, indicates the transition from the Protozoan to the Cœlenterate structure. It can hardly be admitted that there is as yet an adequate foundation for this ingenious theory,* but whether we accept or not the conjecture of the accomplished biologist, I should be inclined to hold that the mere presence of the amœboid bodies in organic connection with the Plumularian, side by side with the polypites, as associated zooids in one and the same colony, is in itself no obscure indication of a genealogical relationship between Rhizopod and Cœlenterate and of their primitive affinity.

* Allman grounds his view of the morphology of the Graptolites, mainly on the peculiar structure of their supposed calycles, which differ essentially, he thinks, from those of all recent hydroids. Their cavity is uninterruptedly continuous with that of the main stem, and there is no constriction or partition of any kind at the base; and in this they agree perfectly with the sarcothecæ, whereas, in the living hydroids, the calycle is marked off, more or less distinctly, as a proper chamber, by a constriction or imperfect diaphragm. But it may be remarked on this, that the latter character is by no means universal. In the genus *Salacia*, for instance, the calycles are not separated from the stem by any constriction, and *the polypites when contracted can withdraw themselves wholly from them into the tube of the stem.* In the genus *Cuspidella* there is also an absence of all constriction at the base of the calycles; so that this characteristic of the Graptolite is not so significant as is assumed.

The sarcothecæ, *so far as their amœboid element is concerned*, are peculiar to the Plumularian family. But beyond its limits we meet with some curious structures, which remind us of the apparatus of thread-cells, with which they are also furnished. Thus in a most interesting form (*Lafœina*) obtained from great depths by the distinguished Norwegian biologist, Sars, the creeping stem gives origin to a multitude of slender tubular cases, each of which incloses a thread of sarcode, terminating above in an enlarged capitulum, in which a number of thread-cells are immersed. A small orifice at the summit of the horny tube, protecting this curious piece of structure, gives a passage to the thread-like darts, which are emitted from the cells. These defensive zooids, if such be their nature, are thickly distributed amongst the polypites, which are completely environed by their strange protectors. They remind us forcibly of "calicetti" deprived of their extensile lobes. Another appendage of still more extraordinary character occurs in the genus *Ophiodes*, and marks an advance on the primitive structures I have been describing. And it is a fact of no little interest that an appendage strictly identical with this is met with on a Plumularian lately obtained from great depths off the coast of Norway,* occupying, as it seems, the place of the ordinary sarcothecæ, which are wanting.

This appendage (Pl. CXI., fig. 6) is a tentaculoid organ of very snake-like appearance, attached below to the common flesh of the zoophyte, and terminating above in an enlarged head, bearing numerous thread-cells, from which long, barbed threads are emitted. It is very extensile, and capable of the most vigorous movements. The base is protected by a small chitinous cup, but the main trunk is naked. One or two of these strange organs are always stationed like watchful sentinels near each calycle, around which they execute the most energetic movements, twisting themselves in all directions, and casting forth their lasso-like threads. Their appearance in a Plumularian, as a substitute for the usual "calicetti," is significant; they are probably an advanced modification of the simpler structure. Their occurrence *in identical form* on two species belonging to different families is certainly remarkable, and points probably to a common ancestor from which both are descended.

I cannot pursue the history of these curious structures further, but merely remark that they seem to replace the more primitive forms, with which the protoplasmic movements are associated, and which, so far as we know, are absolutely confined to the Plumularian family.

* This is the exceptional Plumularian (*Ophionema* mihi) to which I hav referred before.

I pass on to notice briefly another structural feature, which is peculiar to the family, and, indeed, to one section of it. Reference has already been made to the elegant vase-like receptacles within which the ova are matured in one of the principal divisions of the Hydroida. They are usually distributed singly over the colony, or massed together on certain portions of the stem. In some of the Plumularians, however, they are collected in small groups, which are inclosed in a curious pod-like case (Pl. CX., fig. 4). These *corbulæ* (baskets), as they are called, are intercalated amongst the pinnæ, and in fact take the place of a pinna (Pl. CX., fig. 1). They consist of a mid-rib, from which a number of curved and serrated appendages are given off on opposite sides, the extremities of which meet above, while the spaces between them are filled in by a delicate chitinous expansion, and the whole forms a closed receptacle, giving shelter to the reproductive capsules. The latter are borne on the mid-rib, near the base of the lateral appendages. This well-developed pod-like structure occurs only on a limited number of species; but less highly differentiated protective contrivances are met with on other members of the family, which clearly represent earlier stages in the process of its evolution. By collating these we are able to trace the whole course of its history from the first slight modification of the normal elements to the perfect *corbula*. The morphological record is complete, and we have the organ before us both in its infancy and in its maturity. I may state, to begin with, that the *corbula* is a metamorphosed plumule; a secondary plume modified to form a protective covering for the reproductive capsules. The mid-rib (Pl. CX., fig. 4 *a*) is homologous with the main stem of the plume; the lateral appendages (Pl. CX., fig. 4 *b b*) constituting the framework of the case are homologous with the pinnæ, which, however, have undergone a change adapting them to their new office. The calycles are suppressed, and only the "calicetti," sometimes the two lateral and sometimes the anterior, remain. These often attain an abnormally large size, and give to the structure its crested and serrated appearance. In this way the pinna is transformed into a support for the chitinous wall of the *corbula*.

The course of development that has resulted in the formation of this curious receptacle can be clearly traced. I shall give a few of the principal modifications which have led up to it. One species of Plumularian at least, belonging to the section of the family in which the *corbula* or an equivalent is usually present, is altogether destitute of any trace of this organ. Its reproductive capsules are unprotected as amongst the Hydroida generally, and are borne on the main stem or towards the base of the pinnæ (Pl. CX., fig. 3). In a beautiful Australian form, however, we find a deviation from this primitive arrangement. In this species the primary plume gives off at intervals a number of secondary plumes or plumules, which occupy the place of ordinary pinnæ, and in this way a ramified composite structure is formed. Some of these plumules exhibit a curious peculiarity; every here and there a pinna occurs, which is destitute of calycles, but carries a line of sarcothecæ on each side (Pl. CX., fig. 5 *b b*), and near the base a single reproductive capsule. Such abnormal pinnæ are always curved inwards, so as to bend over and in some measure shield the capsule. They occur at intervals, and alternately on each side of the plume, two of the ordinary pinnæ intervening between each pair. In other respects the plumule is perfectly normal.

In another species (African *) these modified pinnæ alternate on both sides of the plumule with the ordinary pinnæ. In yet another form (Australian) † *all* the pinnæ on the plumules producing the reproductive bodies are thus modified; the calycles with their polypites have altogether disappeared, and only the lateral "calicetti," much elongated, remain. All the pinnæ are now reduced to the condition of serrated appendages, which bend forward to meet one another, and overarch the capsules at their base. In this case the plumule is a *corbula*, *minus* the membrane which unites the lateral appendages. In our own "Podded Coralline" (*Aglaophenia pluma*), this element is supplied; a horny outgrowth from the modified pinnæ binds them all together, and completes the closed casket in which the reproductive members of the colony are lodged (Pl. CX., fig. 4).

The various stages in the evolutionary process are here clearly traceable. We may note the plumule changing its character little by little through a series of intermediary forms, until at length it passes into the *corbula*, and so follow the organ step by step from its rudiments to its perfect state. We have first an occasional pinna modified as a support and shelter for a reproductive capsule; then the number of such pinnæ increases until the entire plumule is involved in the change and becomes altogether subservient to the protective function; then the pinnæ, hitherto disjunct and independent, unite, and the plumule, as it were, disappears in the pod-like *corbula*. There is no serious gap in the morphological record.

One or two variations upon the ordinary structure occur. Thus in the finest of our British Plumularians the "Sea-Palm" (*Aglaophenia myriophyllum*), the lateral appendages of the beautiful open *corbula* with which it is furnished differ in

* *Aglaophenia patula*, Kirchenpauer.

† *Aglaophenia ramosa*, Busk.

some degree from those which have been described. Each of them consists of a short stem bearing a single calycle, clearly the equivalent of the ordinary pinna (Pl. CX., fig. 7 *p*), and *between* the front of this calycle and the sarcotheca which usually adheres closely to it, rises a tall recurved appendage, bearing along one edge a line of "calicetti," which give it the appearance of being serrated like a saw (Pl. CX., fig. 7 *a*). These secondary appendages bend inwards, their extremities crossing above, and constitute the protective portions of the *corbula*. The exact significance of these structures I cannot determine; they do not *take the place* of the anterior sarcotheca, which is present as usual, but spring, as I have already stated, from the stem supporting the calycle between the latter and the sarcotheca; and as in some cases after reaching a certain height they give origin to a second calycle (Pl. CX., fig. 7 *c′*), bearing a similar appendage similarly placed, they must, I think, be regarded as of the nature of *ramules*, though abnormally developed. But into such detail I do not propose to enter here. I merely wish to direct attention to the developmental history of the *corbula* as fully unfolded to us in the series of existing specific forms.

I have thus endeavoured, with as little use of technical phraseology as possible, to indicate the chief characteristics of the Plumularian family, the most significant points in its history, and the leading traits of its remarkable beauty. I have only to suggest in closing that those who seek rest and health at this season by the seaside may readily find some members of the tribe that I have attempted to sketch, amongst the waifs on the beach or in the charming nooks and corners of the tidal pools, and may study them at first-hand with much delight and profit to themselves.

DESCRIPTION OF THE PLATES.

PLATE CX.

FIG. 1. *Aglaophenia tubulifera*, Hincks, of the natural size.

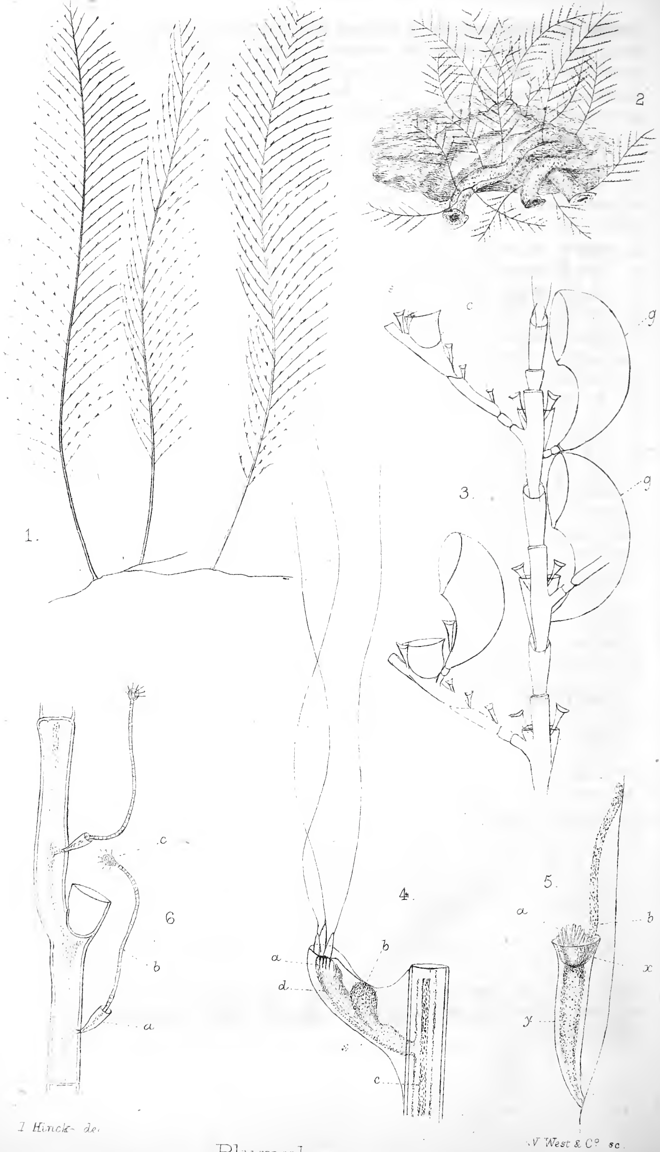
„ 2. *Plumularia Catharina*, Johnston, of the natural size.

„ 3. *Aglaophenia pennatula*, Ellis and Solander, natural size.

„ 4. The *corbula* of *Aglaophenia pluma*, Linnæus, magnified. *a.* The mid-rib. *b, b.* Lateral appendages. *c.* One of the reproductive capsules.

„ 5. Portion of a plumule of an Australian species, bearing modified pinnæ. *a, a.* Normal pinnæ, with calycles. *b, b.* Modified pinnæ, with sarcothecæ, but without calycles, supporting reproductive capsules.

Pl. CXI



J. Hincks del.

W. West & Co. sc.

Plumularians.

FIG. 6. A single calycle of the same species, highly magnified. *a.* The cup in which the polypite is lodged. *b.* The anterior sarcotheca. *b′.* One of the bean-shaped thread-cells. *c.* One of the lateral sarcothecæ. *d.* The internode of the stem.

„ 7. One of the lateral appendages of the *corbula* of *Aglaophenia myriophyllum,* Linnæus, magnified. *b.* Portion of the mid-rib. *p.* Short stem, bearing a single calycle (*c*). *a.* Curved appendage bearing sarcothecæ. *c′.* Second calycle.

PLATE CXI.

FIG. 1. *Plumularia pinnata,* Linnæus, natural size.

„ 2. *Plumularia cornucopiæ,* Hincks, natural size.

„ 3. A portion of one of the plumes magnified. *c.* The calycle. *s.* The sarcotheca. *g, g.* Reproductive capsules.

„ 4. One of the lateral sarcothecæ of *Aglaophenia p'uma,* Linn., showing the thread-cells in action, magnified. *a.* The upper lobe of the contained sarcode-mass (the sarcostyle, *s*) bearing the thread-cells. *b.* The lower lobe, from which the a mœboid processes arise. *c.* The common flesh, from the outer layer of which the sarcostyle takes its origin. *d.* The chitinous case of the sarcotheca.

„ 5. The two-chambered sarcotheca of *Plumularia setacea,* Ellis, showing one of the amœboid processes. *a.* The upper lobe with thread-cells. *b.* The extensile process. *x.* The terminal cup-shaped chamber in which the lobes are lodged. *y.* The inferior tubular chamber.

„ 6. Portion of a pinna of *Ophionema parasiticum,* G. O. Sars, magnified, showing two of the snakelike organs, one above and one below the calycle. *a.* The small chitinous cup. *b.* The flexible and extensile trunk. *c.* The enlarged capitulum, containing thread-cells.

This figure is after G. O. Sars.

BRITISH AND FOREIGN PREPARATIONS FOR THE TRANSIT OF VENUS.

By RICHARD A. PROCTOR, B.A. (Cambridge),

honorary fellow of king's college, london; author of "the sun," "the moon," "saturn," &c.

ON December 9 of this year the most important astronomical event of the present century will take place. Venus will pass in transit across the face of the sun, affording thus the means of determining her own distance and thence the distance of the sun, the fundamental element of dimensional astronomy. It is true that another transit will occur eight years later, yet the circumstances not being then so favourable as they will be next December, the transit of the present year retains its position as the most noteworthy astronomical event of the nineteenth century. Most of the readers of the Popular Science Review are doubtless aware that the circumstances of the two transits have been the occasion of considerable controversy (in which I have unfortunately felt it my duty to take a large share); and some may fear that the circumstances of the controversy are to form the subject of the present paper. That, however, is not my purpose. Controversy can only be justified when some useful purpose may be fulfilled by means of it, as the establishing of some new truth or the recognition of some new method of research. When there is no such reason for discussion or argument the true lover of science is bound to avoid controversy, as necessarily leading to mischievous results, uncompensated (in the case supposed) by any gain either to knowledge or to our means of acquiring knowledge. I do not know, indeed, that controversy is justified even where it tends to establish some new truth, if that truth is one which must in the long run make good its ground; in such a case (as, for example, in the case of my views respecting the constitution of the heavens) I prefer, for my own part, to let the matter bide its time. But in the controversy about the transit of this year the element of time was concerned in such a way that it was impossible to wait beyond a certain point, for the controversy related to the choice of methods for observing an event which could not be postponed. I had, therefore, good reason, after waiting from 1869, when the matter was first mooted, to the spring of 1873, some twenty months before the transit was to take place, for urging the views which seemed to me correct, and which have since been admitted by all to be so, as earnestly and as publicly as possible. But so soon as the time came when the final changes had been made, and the plan of operations definitively decided upon, the discussion of the original scheme or of the modified one, and the consideration of disadvantages which would have resulted from one, or of advantages which might have been obtained from yet more extensive modifications, was henceforth improper. Certainly they would be improper on my part; nor do I think that the history of this matter requires that any stress should be laid by others on mistakes which have been in the main corrected, or on the threatened loss of opportunities which will not now be allowed to pass away unutilised.

For fortunately a far pleasanter task than any on which I have hitherto been engaged in connection with the transit of 1874 lies now before me. I propose to describe how the various nations which take part in scientific research propose to take advantage of the opportunities presented by the approaching transit; and the task is a pleasant one, because there is scarcely a single point of vantage in the whole field of operations which will not be adequately occupied.

Let us in the first place consider the methods available for observing the approaching transit, then the regions where these various methods can be most suitably applied, and lastly the dispositions made to occupy in suitable force those favoured regions.

It is hardly necessary to enter here at any length into the principles on which the determination of the sun's distance by means of the observation of Venus in transit, depends; because the whole subject has been already very fully dealt with elsewhere, and is sufficiently simple. The following mere outline of the matter may, however, be permitted. If two observers, separated by a sufficient distance on the earth, see Venus in transit at the same moment, they will see her disc projected on the different parts of the sun's face; and it is manifest that if the distance between the two observers on the earth be known, the displacement of Venus on the sun's face, if observed with sufficient accuracy, will give the means of defining the distance of Venus, and thence (by Kepler's third law) the distance of the sun.

This is the method which is sometimes known as the *direct* method. It indicates the circumstance which lies at the basis of all the methods of observing the transit. The difficulties involved in this method are sufficiently manifest. Two observations, in order to be brought into comparison, must be made either at the same instant of absolute time, or nearly so, the exact difference of absolute time being known; and as the observers are at opposite parts of the earth this condition involves the necessity of the accurate determination of the longitude of each station, and of the local time of each observation. Moreover, the determination of the exact position of Venus on the sun's disc is not only a task of considerable difficulty in itself, but, inasmuch as it requires time, while Venus is all the while travelling onwards, the position of Venus at any given instant has to be inferred from observations not all made at that instant, and is thus not in reality a direct determination.

These considerations led Halley to devise his now famous method, in which an observation of duration is substituted for the determination of position at a particular instant. If an observer at a northern station times Venus as she travels along a certain chord, it becomes possible to determine the position of the chord; so also the position of the chord along which a southern observer notes the transit, can be inferred from the observed duration.* Thence the distance between the two chords can be deduced; and this is precisely what is required and would be obtained (as we have seen) for the direct solution of the problem.

The difficulties in Halley's method are easily recognised. First, the whole transit requires to be seen, since the beginning and end have to be observed. As a transit may last several hours, this circumstance introduces a double difficulty; for the selection of stations is limited by the fact that, instead of half the earth being available, the portion of the earth where the whole transit is visible is only *the part common to the two hemispheres* which are in day-light at the beginning and end of the transit; and, moreover, a station requires to be *well placed* (so as to produce suitable parallactic displacement of Venus) both at the beginning and end, and there may be very few stations so situated. To this may be added the circumstance that it is antecedently less likely that favourable meteorological conditions will prevail both at the beginning and at the end of the transit, than at one or other epoch. But this, perhaps, is counterbalanced by the advantage that an observa-

* I have here taken no account of the earth's rotation, and have spoken only of the motion of Venus's centre, not wishing to deal with any but the rough elements of the problem.

tion, either at the beginning or end, would in most cases be useful for the method next to be considered, called Delisle's, because a good Halleyan station is commonly a fair Delislean station, and may be (in fact, very commonly is) a very good station for Delisle's method.

Delisle devised his method to meet the two objections mentioned at the beginning of the preceding paragraph, which chanced to apply with particular force to the transit of 1761. In fact, he found, when studying the conditions of that transit, that Halley's method would fail totally for it. He suggested that instead of the duration of the transit the absolute time at which the transit began *or* ended should be determined. It is clear that a northern observer so placed that Venus would, as seen by him, travel along a certain chord, or a portion of such, would see the transit begin at a different time than would a southern observer, from whose station she would appear to traverse a different chord, either longer or shorter, according as the transit took place across the lower or upper portion of the sun's disc. The difference of time so caused would be less or greater according as the chords were nearer or farther apart. So that the determination of this time-difference involves the determination of the distance between the chords in this, precisely as in Halley's method. And a similar remark applies to the observation of egress by two observers, one at a suitable northern, and the other at a suitable southern station.*

The chief difficulty presented in Delisle's method consists in the fact that it requires the longitude of each station to be most accurately determined, and also the exact moment of local time when the transit begins or ends, as the case may be. This difficulty is analogous to one of those already considered in dealing with the direct method.

Lastly, there remains a method which modern progress has rendered available, viz., the photographic method. This method, viewed astronomically, requires no explanation, since it amounts merely to the substitution of photography for observation in applying the "direct method." It removes the second difficulty considered in dealing with that method, leaving the first untouched, though the possibility of taking many photographs during the progress of the transit, and of thus determining the *chord* of transit, is a manifest advantage possessed by the photographic method.

* Here, as in considering Halley's method, I have overlooked for the moment the dimensions of the disc of Venus. Of course the observer cannot note the passage of Venus's centre over the edge of the sun's disc, but must observe the contact of her disc and the sun's, either on the inside or on the outside.

I turn next to the consideration of the plans of the various nations taking part in the observation of the transit of next December.

To America, I conceive, the pride of place must in all fairness be conceded. She might reasonably have contented herself with but slight efforts on the occasion of this transit (because the transit of 1882 will fall pre-eminently to her share). But the American Government has voted a sum (30,000*l.*) twice as great as that which has been granted by the British Government* for the purpose. Then, as I have already mentioned, America has undertaken the most difficult of all the tasks which the proper observation of the transit rendered necessary,—I mean the occupation of the Crozets. Moreover, the preliminary investigation of the conditions of the transit by American astronomers is altogether excellent, contrasting strongly with the pretentious but blundering manifesto published by the official astronomers of a great nation, which shall be nameless.

I have before me as I write the series of charts published by the Commission appointed to investigate the circumstances of the transit. These consist of four finely-executed stereographic charts showing that hemisphere (and a fringe beyond) on the earth which is turned sunwards at the time of (1) ingress exterior contact, (2) ingress interior contact, (3) egress interior contact, and (4) egress exterior contact. On these charts are marked two series of curves, one carried through points where the contact occurs at the same instant, and the other carried through points where the contact occurs at the same part of the sun's limb. After a careful study of each chart (a study as careful as that which I gave to the Astronomer-Royal charts in 1869), I am able to pronounce them singularly accurate for the degree of approximation which the authors claim.†

The American astronomers are disposed to rely chiefly on

* It must be noted, however, that our Government unhesitatingly granted all that the Astronomer-Royal asked, so that it would be altogether unfair to accuse the British Government of stinginess in the matter.

† There is a very elaborate investigation of the error actually arising from the use of circles for the time-curves in the projection, to represent curves which are not in reality circles of the terrestrial sphere. The maximum error is found not to be more than 12′, which, says the author of the paper, "having regard to the scale on which the charts have been constructed, may be considered as within the unavoidable errors produced by imperfection of drawing." The maximum error is four times as great when the curvature of Venus's shadow-cone is altogether neglected in the usual way, according to which the resulting time-error is always of one sign. In the American charts the error is so distributed as to be positive or negative according to circumstances.

the photographic method, *applied at stations where the whole transit can be seen.* The condition italicised is of some importance as indicating a distinction between two possible photographic methods, one corresponding to Delisle's, the other to Halley's. The English arrangements, for example, include the application of photography at the Sandwich Islands, where only the beginning of the transit can be seen, and at Alexandria, where only the end can be seen. Such photographs can, of course, only have value when the absolute time at which each is taken is accurately known. On the other hand, the American stations are so selected that photographs can be taken throughout the whole continuance of the transit, and these, by indicating the chords of transit, will have a value independent of the exact determination of absolute time; so that, in fact, the original error of the Astronomer-Royal (that Halley's method fails totally in 1874) would, if not corrected, have affected the application of the photographic as well as other methods. Fortunately, the American astronomers have not been misled by it,* and at all the stations they propose to occupy, the whole transit will be visible, as will presently be more particularly noted.

The plan they adopt for photographing the sun differs essentially from that which European astronomers propose to employ. "For the purpose of obtaining an enlarged image on the photographic plate," writes Professor Hilgard, of Washington (describing the ordinary method), "the image of the sun, after being formed in the focus of the telescope, is enlarged

* At the same time I must remark, that it seems to me altogether proper that the Delislean stations in question should be occupied by photographers. What I have all along insisted upon has been, the necessity of employing every available method, and occupying every available station; and it would have been a matter to be regretted, had any one of the regions originally suggested by Sir G. Airy been neglected. I note this because I heard, during my stay in America, some rather too severe comments on the Astronomer-Royal's urgently-expressed desire that the Americans should establish two observing parties in the Delislean region, of which the Sandwich Islands give the leading stations. It was doubtless a mistake on Airy's part to desire that this region, already amply provided for, should be occupied instead of Halleyan stations altogether more advantageous; and it was well that the American astronomers declined to change their plans. Yet the occupation of this Delislean region, *as well as* the other Halleyan stations, was equally proper. An eminent American humorously compared Sir G. Airy's request to the well-known appeal of the fox who had lost his tail: "Your Astronomer-Royal," said he, "who has lost his head over Delisle's method, seems to wish Newcomb to lose his also." But I must confess I should have been very sorry to have seen the Delislean stations insufficiently provided for.

by a lens or camera to the desired size, the photoheliographs, as they are called, being thus enlarged to a diameter of about four inches. This plan has been adopted for the photographic apparatus to be used by the British, German, and Russian parties commissioned to observe the transit of Venus. A different plan has, however, been adopted for the American parties, with the view of avoiding some difficulties to which the former method may be thought subject. These are conceived to reside in the fact that not only all imperfections in the focal image are thus enlarged, but that the optical imperfections of the camera are superadded. To avoid this objection it was deemed best to make the telescope so long that the image formed in its principal focus would need no further enlargement. Here another difficulty presented itself. The telescope must be forty feet in length in order to give an image four inches in diameter. Such a telescope, pointed at the sun, would scarcely be manageable. Hence the plan was devised, which Professor Winlock was the first to put into practical operation. It consists in fixing the long telescope in a horizontal position, and reflecting the sun's rays into the object-glass by means of a plane glass mirror, moved by clockwork, so as to throw the image of the sun continually into the telescope. This need not be done with great precision, since, as has already been said, the time of exposure is exceedingly small, and the mirror can at any time be adjusted. It is obvious that, in this arrangement, as much depends upon the perfect figure of the mirror, as in the other upon that of the enlarging lens; but it is, doubtless, an advantage that different methods should be employed, so long as a sufficient number of stations are occupied to give an independent result for the sun's distance from observations by each method alone, since such only can be considered as strictly comparable. This condition is amply fulfilled by the abundant provision made by the American Government for the observation of the important event in prospect."

I may remark, however, that Professor Newcomb, with whom I had the pleasure of a conversation relative to the subject, attaches very great importance to the advantages of the American method. He remarked that by employing this method the astronomer is enabled to measure the distance of Venus from the sun's centre with an exact knowledge of the value of the deduced distance, because, the focal length of the telescope being known, the value of any distance indicated in the focal image is at once determined. All that is necessary, then, is to determine the centre of the solar image, which can be safely done by measurements made from the limb. Manifestly no photographic effects affecting the position of the limb in the photograph could appreciably affect the determination of the centre, even though such effects were not absolutely uniform all round. But in the ordinary method of photographing the determination of the arc-distance of Venus from the centre is not reliable (in a problem of such extreme delicacy), because the *estimated* dimensions of the solar image could not be *accurately* determined, while the observed dimensions, being determined from the photographic limb of the sun, would be affected more or less by photographic irradiation. No apparent sharpness of the limb can render certain the fact that the limb in the photographic image corresponds to the true solar limb. I must confess that Professor Newcomb's reasoning seems to me irrefutable. It will be observed that it does not depend on practical or technical knowledge of photography, since the photographic irradiation demonstrably exists, and is demonstrably variable in amount. In a conversation with Dr. H. Draper, of New York, whose experience in those matters is well known to be unsurpassed, I found Professor Newcomb's doubts fully confirmed. It is true that Dr. Rutherfurd, whose great practical experience in solar photography is unquestioned, agrees with his eminent British rival in such work, Dr. De la Rue. But then it is to be remembered that both Rutherfurd and De la Rue view the matter as photographers, while Newcomb and Draper view it from an astronomical standpoint, and in this case the astronomical, not the photographic relations, are chiefly in question. We do not want handsome solar pictures, but pictures which can be confidently measured; and certainly the plan adopted by American astronomers is that which best meets this requirement. I may add that a very eminent American astronomer, speaking to me on this subject, made this strong remark, "I regard the photographic method adopted by the British astronomers as involving a mistake as fatal as Airy's original mistake would have been if uncorrected." *

The programme of the American expeditions is as follows:—Eight parties will take the field, three in the northern hemisphere, "where the meteorological conditions are supposed to be somewhat more favourable than at corresponding stations in the southern hemisphere," where there will be five. The three northern stations will be (1) at Vladivostok, in Siberia; (2) at Tien-tsin, in China; and (3) at some as yet undetermined place in Japan. Originally, in response to the strongly expressed wish of the Astronomer-Royal that two Delislean

* Lord Lindsay, it is to be noted, will employ the same method as the American astronomers, after carefully testing, in a series of photographic experiments, the reliability of the two methods.

stations should be occupied, one on the Sandwich Islands group, and another at Tahiti, the Americans thought of occupying Owhyhee, rejecting as disadvantageous the suggested Tahitan station. But since then the idea of having any Delislean stations has been abandoned, and, as just mentioned, the five remaining stations are all to be in the southern Halleyan region, the adequate manning of which I urged so warmly last year. "The *Swatara*," says an American paper, "the vessel which is to convey the various southern observing parties to their stations," sailed from New York during the first week of June, and is "to lay in provisions at Cape Town, as well as a supply of hens for the sake of their eggs, wherewith to albumenise the photograph plates. Then a party will, weather permitting, be left at the Crozet and Kerguelen Islands. As in frequent conditions of the wind access to the Crozet Islands is impossible, enough provisions will be left with the observers, and possible prisoners, to last them a whole year. From Kerguelen the vessel will sail to Hobart Town, thence to Bluff Harbour, in New Zealand, and thence to Chatham Island, the last southern point of observation, which is either uninhabited or else inhabited by cannibals. Here the *Swatara* will remain till the transit is over, and will then, the possible cannibals allowing, revisit the various stations to take up the different parties, supposing them to be found." "Each station will be provided with four principal instruments: The photographic telescope just described, with a 5-inch object-glass corrected for the actinic rays, and forty feet focal length; a telescope of five inches aperture and eight feet focal length, equatorially mounted for the observation of contacts; a transit instrument for the determination of time and geographical position; and an astronomical clock. The telescopes, both visual and photographic, have been ordered from the well-known firm of Alvan, Clark, and Sons, who have just completed and mounted at Washington the greatest refracting telescope in the world. Although the photographic method is mainly relied on, the eye-observations of ingress and egress are not to be neglected, and it is proposed to supplement them by measuring the distances of the cusps while the planet is entering the sun's disc and leaving it." This last point I regard as one of extreme importance, as will be gathered from my remarks on the subject in "The Sun," and in the Monthly Notices of the Astronomical Society, vol. xxx. p. 46 *et seq.*

While all the American stations, as well northern as southern, are Halleyan, the English stations were for the most part selected originally as Delislean. In fact it is a rather startling circumstance that the Astronomer-Royal, in his original description (Monthly Notices, vol. xxix. pp. 36, 37) of those among them which really are Halleyan as well as Delislean, failed to notice that both ingress and egress can be observed, the strange charts illustrating his paper not suggesting in any way that such is the case. Eight stations have been provided for by the Home Government, while one is now provided for by the Indian Government. Originally five stations were to have been occupied, and solely for the application of Delisle's method; and the region in North India, for which the Indian Government will provide, had been overlooked altogether (Monthly Notices, vol. xxx. p. 37), being strangely omitted from the charts illustrating Sir G. Airy's paper.

It is well to note, first, that ample provision has been made for the application of Delisle's method. No less than three stations will be occupied in the group of the Sandwich Islands, where Captain Tupman (the head of the entire enterprise) will be stationed. Here photography will be applied specially to the determination of the moment of ingress, by a contrivance of Janssen's (improved by De la Rue) enabling the photographer to take sixty successive pictures of the ingress. Under Captain Tupman's command will be Lieutenants Ramsden and Noble, and Messrs. Johnson, Forbes, and Barnacle. The observation of accelerated ingress has been well provided for, especially as some of the Halleyan stations in Japan and the north-east of Asia are excellent for this phase also.

Retarded ingress will be observed at Kerguelen's Land and Rodriguez. According to the published statements there will be two stations on Kerguelen's Land, but Fr. Perry, who is chief in this region, has power to assign one party to Heard Island if a landing shall be found to be practicable. The three stations here are all Halleyan as well as Delislean, the whole transit being most favourably visible. It is well, therefore, to note that ample provision has been made for applying Halley's method, as well as for photographing the whole progress of the transit. The observers under Fr. Perry will be Fr. Sidgreaves, Lieutenants Corbet, Goodridge, and Coke, and Mr. J. B. Smith. At Rodriguez, Lieutenants Neate and Hoggan, and Mr. C. E. Burton, will be the observers.

Accelerated egress will be observed at Christchurch, New Zealand, by Major Palmer and Lieutenants Darwin and Crawford. This station, like the stations for observing retarded ingress, is Halleyan also, and is now well provided for as a station for observing the whole transit.

Retarded egress will be observed at Alexandria by Captains Browne and Abney, and Mr. S. Hunter.

The names of the observers at Peshawur, in North India, have not been published, but it is known that the whole transit will be observed at that station, and photography employed. I may be permitted to note, in passing, the slow approaches made towards the course now actually adopted. When I pointed to Peshawur as a station which ought undoubtedly to be occupied, at first for some months nothing was said or done ; then a photographic station at Delhi was suggested, Peshawur being scouted; then the locale was changed to Peshawur; lastly (and quite recently) it was announced that contact observations had been amply provided for, so that in the long run, or about March 1873, what I had advocated in March 1869 was adopted to the letter. Although at Peshawur the whole transit will be visible, this station is specially suited for the observation of the retarded egress, being for this purpose superior to Alexandria.

The total cost of the British expeditions, exclusive of the Indian station, will be about 15,000*l.*

Lord Lindsay's station at Mauritius must be mentioned in this connection. The work done there will probably be at least as reliable as that done at any other station, and the photographic preparations are, on the whole, more complete than those adopted anywhere else.

It may be mentioned, also, that Colonel Campbell will proceed to Thebes on a private expedition, working with the Egyptian party as a volunteer.

Russia is distinguished by the largeness of the number of stations she will provide for. She will have no less than twenty-six stations, ranging from the Black Sea to the region occupied by American astronomers in North China. Eleven of these stations (the more easterly section) will be Halleyan, the remainder covering a large part of the region whence the retarded egress will be favourably observable.

It has recently been announced that the German astronomers will occupy five southern Halleyan stations, one of these being the desolate Heard Island. Their original purpose was to occupy one station in the North, viz. at Chefoo, in China, one in the Auckland Islands, and Macdonald Island, besides a photographic station in Persia. They will rely considerably on the "direct method" of observation.

France will occupy five stations, all Halleyan, having declined to occupy (as invited by our Astronomer Royal) the three Delislean stations, Marquesas, Bourbon, and Suez. The selected stations are, in the North, China (two) and Japan; in the South, Campbell Island and St. Paul's Island.

On the whole, it may be said that ample provision has been made for the observation of the most important astronomical event of the century. Every region whence useful observations can be made, will be occupied by observing parties, and use will be made of every available method.* Let us hope that the success of the expeditions will remove any unpleasant recollections of former controversies. In the words of a leading weekly journal, "If weather and other conditions only favour the observers at the various stations, we believe that results will be obtained so satisfactory as to leave no thought or inclination in any quarter for a return to less pleasing considerations."

* I should have been glad, however, to hear that photographic observations were to be made at Cape Town, Port Natal, and in South Madagascar, where, though the whole transit will be visible, the planet will be projected farther from the sun's centre during the middle of the transit than as seen from any other accessible station.

ON THE NATURAL HISTORY AND GEOGRAPHICAL DISTRIBUTION OF LIVING AND EXTINCT BEARS.

By A. LEITH ADAMS, M.B., F.R.S.

THE contracting of the range and feeding grounds and diversities of food and climate, from far back geological epochs up to the present day, have unquestionably influenced not only the bulk and outward aspect, but also modified the bony skeleton of many animals.

With reference to the Bear Tribe, which is only one of many examples, we find that the largest specimens of fossilized individuals discovered in European caverns, surface soils, and in bogs are relatively much larger than any instance among living species, only very bulky examples of the grizzly bear (*U. ferox*) being comparable, and they fall short as regards dimensions. A comparison between the smaller fossil cave bear (*U. priscus*) and the brown bear (*U. arctos*), shows that if not identical they were closely allied; indeed, taking into consideration the various modes by which animals have been expelled from their ancient haunts, there seems good cause to suppose that these two bears claim a common ancestry. According, therefore, to the above view it may be fairly advanced that the grizzly bear was at one time common to Europe and North America. Again, considering the relative degrees of ferocity of living species—and in these respects they differ specifically to some extent—it is well known that the grizzly bear is the only one which will attack man unchallenged; indeed, the Arctic, brown, black, and sun bears, &c., rarely assail him, unless when pressed, as in case of wounds, or in guarding their young. We may believe, therefore, that primeval man would have waged a deadly warfare against so conspicuous and powerful an enemy, and would have exterminated the more ferocious bears, thus leaving the brown bear (*Ursus arctos*) to pursue its ways and frequent its ancient haunts, until advancing civilization in Europe finally repelled it to a few mountainous and secluded regions. The alliance between the brown and grizzly bears is close, but not sufficiently intimate to lead naturalists to consider them one and the same species. In size of course the latter is superior, but now and then individuals of the brown species are met with in Asia, if anything, only slightly less bulky. These, however, are exceptions, whereas the remains of the great extinct cave bear (*U. spelæus*) show that the average dimensions of the animal exceeded considerably that of any recent species. Now to return to the geographical range of the brown bear (*U. arctos*). In Asia it is spread over Siberia and the Himalaya. On the latter chains, probably from a long sojourn in the snowy regions, its fur has become more fulvous; hence the appellation of Isabella* and white bears bestowed on the denizens of the Cashmere and more eastern ranges. This aberrant form of a well-known animal, the fur of which generally varies from a dark brown to even black, such as obtains in the bears of Northern Europe and Asia, is intensely instructive to naturalists, who, for lack of better information, are often compelled to bestow specific names on slender foundations. A still lighter coloured variety (*U. syriacus*) is met with on the mountains of Eastern Turkey and the Caucasus. In America, in the Aleutian Islands, there are "brown and red bears,"† which, unfortunately for our wants, are not yet described with greater accuracy; it is, however, recorded by Sir John Richardson, that "the barren lands lying to the northward and eastward of Great Slave Lake, and extending to the Arctic Sea, are frequented by a species of bear which differs from the American black bear in its greater size, profile, physiognomy, longer soles and tail, and from the grizzly bear also in colour, and the comparative smallness of its claws. Its greater affinity is with the brown bear of Norway, but its identity with that species has not been established by actual comparison. It frequents the sea coast in the autumn in considerable numbers for the purpose of feeding on fish."‡

* This shows how cautious naturalists should be in giving specific names to objects from imperfect materials. Dr. Horsefield, in the "Linnæan Transactions," vol. xv., p. 334, from a mutilated Nepaul specimen sent to the Museum of the India House, enumerates, among other characters, that this so-called *U. Isabellinus* has its "claws small and straight." Now I have shot or examined, I may confidently state, upwards of one hundred specimens, and can assert that the claws on the fore feet are fully curved, and on the hind feet that they are small but curved. The question contemplated by this distinguished traveller and naturalist at the time was, whether or not the above bear was a tree-climber. Now, although it does not often ascend trees, the curved claws are of great utility in preserving its footing on glaciers and soft or yielding soil, and on rocky declivities.

† Langsdorff's "Voyages and Travels," vol. ii., p. 74.

‡ "Fauna Boreali Americana," p. 21.

The point, however, still unsettled is, whether or not this "barren-ground bear" is identical with the brown bear of Europe, and whether or not, in conjunction with the grizzly bear, the two represent the same species which were spread over Europe in prehistoric times. Referring to species which have ceased to exist; although much remains to be done, a great deal of valuable information has been gained in relation to the natural history of extinct bears from a study of the characters and habits of the living. Thus, among the most puzzling features in connection with the remains of extinct fossil bears, met with in caverns, are discrepancies in size between the teeth and bones of adult individuals. So marked is this that palæontologists cannot believe that they represent large and small varieties of one species; considering, however, the advantages enjoyed by the progenitors of the present tribe of the members and the contracted range and food of the latter, there seems good reason to suppose as regards dimensions that the bears, like deer and several other animals, have absolutely degenerated, and are decreasing in size. Indeed, everyone who has examined the remains of the associated quadrupeds found along with the exuviæ of fossil bears, lions, and so forth, must believe that all fed sumptuously in those days, and also attained to the maximum dimensions of their species.

I found that the Himalayan brown bear was subject to much diversity in dimensions, so much so that certain old males presented remarkable contrasts to smaller-sized adult individuals of both sexes, as much, in fact, in the bony skeleton and outline of the cranial ridges as in coloration; moreover, so marked are these discrepancies, that supposing their skeletons had been found in a fossil state, the comparative anatomist could scarcely be blamed who pronounced them to belong to different species. Again, I found that the largest or patriarch bears are more addicted to passing their latter days in caverns than are the younger and more active. This was demonstrated by the appearances of their retreats, which are met with in secluded mountain forests, where the den is situated either under a shelving precipice or in the rock, from whence the owner descends daily to the sward below, where, after browsing until mid-day, it is a habit of the individual to repair to the neighbouring spring, usually shaded by trees, and wallow in the muddy water. In consequence the sides of these pools are beaten and plastered like a beaver-dam, whilst from the margin leading towards the den are deep impressions in the turf, caused by the animal constantly treading in the same footprints.* Thus it pursues the even tenor of its ways, hybernating in the den

* Author, "Wanderings of a Naturalist in India," p. 241.

for nearly half the year, and dividing the remainder between a circumscribed feeding ground and the pool, until, dying in its lair, the body either crumbles into decay, or is partly or entirely covered over by calcareous drippings from the roof of the cavern, or by *débris* washed in from the decomposing surfaces above or around the external opening. Upon the top of this deposit other generations of bears might appear and disappear, and so on for ages, the retreat becoming either the abode of the parturient female, or the hybernating den of one or many individuals. Let us see how such data apply to bygone epochs, as displayed in the ancient bone caverns of England.

In the report on the exploration of Brixham Cave, near Torquay, Devonshire,* Mr. Busk has determined, from deviations in dental characters and size, what he believes to be no less than three species of bears, viz. the great cave bear (*U. spelæus*), the grizzly bear (*U. ferox fossilis*), and the common brown bear (*U. arctos* vel *U. priscus*). One circumstance connected with the ursine remains struck him as remarkable, and that was "the number of instances in which bones obviously belonging to the skeleton of the same animal were found collected together in one spot," thus indicating that the carcases had been either conveyed into the cavern by other carnivores, or that the bears had died there. Again, with reference to this ancient British emporium of extinct animals, we find him adding, "There can be little doubt that amongst the bears' relics, as with those of the hyena, some at least must have belonged to animals which habitually used the cavern as a place of refuge, and especially, perhaps, at the time of parturition, and when they were nursing their young."

In further support of the latter statement it may be observed that the young are invariably born before the she-bear leaves her winter retreat, and that they often accompany the parent for two years. Of course, a den or cavern may at any time become the retreat of divers carnivorous quadrupeds; much depending on how the various sorts predominate. Thus, for instance, in a country where lions, tigers, hyenas, bears, and so forth are common, it might just happen that one or other will retain alternate possession of the retreat and drag its prey thereunto, so that the exuviæ might get intermingled. In Great Britain, in the days when such caves as Kirkdale,† Brixham, Settle,‡ the Gower Caves § of South Wales, and Kent's Hole,||

* "Philosophical Transactions," vol. clxiii., p. 471.

† Buckland, "Reliquiæ Diluvianæ."

‡ Tiddeman, "Geological Magazine," vol. x.

§ Falconer, "Palæontological Memoirs," vol. ii., p. 525.

|| Vivian's "Cavern Researches."

were tenanted by wild quadrupeds, many of which are either now extinct or have been repelled to distant lands, it appears, if we are to judge not only from the variety but also the dimensions of many of the remains, that the British area then, whether insular or connected with the Continent, was overrun by the larger mammalia, to wit, the lion, hyena, bear, deer, hippopotamus, rhinoceros, elephant, ox, and bison, not to speak of hosts of smaller mammals. Now, on the Himalayas the chief predatory quadrupeds have more or less a hard struggle for existence, owing to the comparative paucity of, and difficulty in procuring subsistence as compared with herds of deer and the like in less alpine situations. The plantigrade bear is especially at a disadvantage in this respect, and we need not therefore be astonished to find that it subsists chiefly on vegetable food. Hence modifications in the characters and position of the teeth are likely to occur under the changed habits of life; indeed, considered as an exponent of discrepancies in the dental construction of extinct mammals, it is of the utmost importance to fully realise similar contingencies. Thus, in relation to the food of recent species, we find the grizzly bear still clinging to the haunts of the buffalo on the prairies and plains of the West, but destined at no distant period to be swept off the Continent, whilst the American black bear, essentially a vegetable feeder, will linger on just as may have happened in Europe with the cave bears and the *Ursus arctos*. Indeed, what is now going on in the New World in relation to the extinction of many of the wild quadrupeds, to wit, the bear, beaver, elk, &c., was accomplished in Europe before the historical period. But the statement is not quite correct that the black bear of North America is partial to vegetable food, inasmuch as both it and the barren-ground bear, when compelled by dearth of vegetable food, repair to the sea-shore and feed on marine animals; moreover I have seen the brown bear on the Himalayas, soon after coming forth from the long winter siesta, make attacks on cattle and horses, and when hard pressed for early plants which had not had time to spring up, even devour the carcase of one of its own species. In fact, bears will eat almost any description of food.

With reference to their constitutional peculiarities. The hybernating species seem to possess very sensitive nervous centres both as regards extremes of heat and cold; even the Polar bear is said to occasionally fall into a lethargic condition in midwinter. No doubt, therefore, from the abundant remains met with in caves, that the extinct forms also hybernated; not from scarcity of food, but on account of climate and their particular organization. One of the most trying ordeals in the Canadian forest during midsummer is the annoyance occasioned by mosquitoes and the still more venomous black fly. Neither man nor wild beast enjoys any particular exemption from these persistent tormentors. The amateur fisherman is often compelled to give up his pastime at the most tempting moment, and the woodcutter is driven into the clearings, and the bear and elk into the lakes. In Asia at the same season it is also a common occurrence to see the Himalayan brown bear basking or sound asleep on the melting surface of a glacier, as much to escape the torments of insects as for coolness; in fact, this species does not display a sufficient pliability of constitution to enable it to withstand extremes of heat and cold. Now, whatever may have been the character in these respects of such of its compeers as the cave lion and the hyena in pre-historic times, their present descendants have become restricted to warm regions, although the tiger, so closely allied to the former, is a native of northern as well as middle and southern Asia. Indeed, in attempting to speculate on the nature of the climates during the cave periods, from a knowledge of the present characters and haunts of living species, we must always bear in mind the examples of the hairy mammoth and rhinoceros, but for the discovery of which it would still be a wonder how, if like their naked representatives, they could have withstood the rigours of northern winters. Probably the hippopotamus of those days was also covered with thick fur, and the shaggy mane now restricted to the lower and fore parts of the lion may have been continued, more or less, over the entire body. Again, naturalists are apt to associate the reindeer with Arctic climates, and argue that similar conditions must have prevailed at one time in the South of France, where the fossil remains of this animal have been discovered. But although the climate was, no doubt, more rigorous then than at present, there is no need that it should have equalled that of Lapland of the present day, inasmuch as the caribou or woodland reindeer was common in the New England States of North America within the last two hundred years, and I found it plentiful in the forests of New Brunswick, latitude 45° N. Indeed, if we were to suppose western Europe covered with forest trees, whereby the mean temperature would be lowered, there is nothing to have prevented the animal from migrating in the colder portion of the year from Norway to the shores of the Mediterranean, just as Richardson * found the barren-ground reindeer traversing similar distances in northern Canada.

It has just been stated that the fossil bears met with in caverns of Germany are demonstrably much larger than either fossil or recent specimens of the grizzly bear. It is the case, also, that skulls and bones dug out of bogs in Great Britain far

* "Fauna Boreali Americana."

exceed the dimensions of any living representatives of the brown bear, but approach in this respect to the former; so that, considering the comparatively modern histories of many of the deposits of these fens and turbaries, it has been surmised by Mr. Busk, in his admirable report already referred to, that the grizzly bear lingered on in England and Ireland to comparatively speaking recent times, probably up to the pre-historic epoch, when the deer and bovine tribes were plentifully distributed over the country. Now, considering that all these fossil bears were co-existent, and taking into consideration that they were placed, more or less, on the same footing as regards food, it can scarcely be that the small were degenerate descendants of the large, the differences in size being too great for such a supposition, unless we are to believe that a far greater variability existed then than now, in which case degeneracy would have been a marked character in many species. We might believe, however, from the great tendency to variation in dimensions and colouring already pointed out in the case of the brown bear, and the cavern-haunting propensities of the larger or aged individuals, that the great fossil cave bear (*U. spelæus*) stood in much the same relation to the *U. ferox fossilis*, and was only a large variety of the latter, just as, in all probability, the so-called "gigantic urus" stood to the "great wild bull" (*Bos primigenius*). In fact, abundance of food and unrestricted freedom are as necessary conditions of the prosperity of an animal as the contrary produces a stunted and deteriorated race. What long ages have passed away since the beaver built its dam on the banks of the Thames, or the hippopotamus, elephant, and rhinoceros fed on its banks; when herds of enormous oxen, deer, and the like pastured freely over the country, before man had invented any more deadly implement than a flint arrow or a stone hatchet? Finally, we come to the mutations in the physical aspect of the continent, together with the subsequent struggles for existence and gradual disappearance of the species until only the deer-wolf and brown bear remain of all the large animals which then frequented Europe. Indeed, it is only necessary to survey the remains found in England alone, to become satisfied that the large assemblages of carnivorous and herbivorous quadrupeds were denizens of the area, at a period when our island was not only a portion of the continent of Europe, but when its climatic and topographic conditions must have been different from what obtain at the present day.

None of the following species of bears have hitherto been discovered in fossil states. This circumstance, however, may be owing more or less to the fact that the soils of the countries they frequent have not been subjected to the same searching scrutiny as those of Europe in general and Great Britain in particular; at the same time, it must be observed that although caverns are common in North America, no traces have yet turned up of the grizzly bear, indicating either that its progenitors were not cavern-haunting, as in Europe, or else that the animal is a far more modern occupant of the Continent. The same may be said of the American black bear, which is restricted to the temperate latitudes; and although disappearing with the forests, was, within the historical period, very plentiful from Mexico northwards to the confines of the Arctic Circle. This species, like the brown and grizzly bears, presents the same variability with reference to colouring and texture of the fur; the Polar bear (*U. maritimus*) being the only one of the family that preserves regularity in these respects. Its food is also more uniform, and being restricted to the Arctic regions there are not the same influences affecting it as with the preceding, spread over vast continents which differ much in climate, aspect, and natural productions. Although many bears vary very much in outward appearance and osteological characters, we find, as in equine and feline species, a general disposition to particular markings on certain parts of the body. For example, the spinal and shoulder stripes so distinctly defined in the zebra, repeated in the ass, and now and then appearing in the horse, are represented by the light-coloured shoulder and brisket markings in the bear tribe so well seen in the black and sun bears, whilst in the brown and grizzly the collar is faint and scarcely discernible, unless when the winter is being replaced by the summer fur.

The well-known long-nosed bear (*U. labiatus*) of Hindostan, so distinct in osteological characters from any other member of the genus, retains also the white mark on the front of the chest. Whatever may have been the distribution of this species in pre-historic epochs, it is now restricted to the torrid regions of the above country. Reverting to the North American black bear, we find the white spot is only occasionally present on its brisket; and better defined on the black or spectacled bear of the Cordilleras (*U. ornatus*), a species distinct in several respects from the last, and more closely allied to the black bear of Asia, the nearest habitat of which is in the East Indian Islands, where it is known as the *bruang*. From thence it extends northwards to Eastern Siberia, over 45 degrees of latitude, and throughout countries differing very much in physical and climatal conditions; moreover, excepting that its fur is longer and thicker in the temperate than torrid regions, there is little difference either in the coloration of the pile or in osteological characters. However, like varieties of the brown bear met with in regions wide apart, it has received various names, which are now classed under the one common appellation of Malayan bear. The animal is also plentiful along the southern slopes of the Himalayas, from whence specimens sent to Cuvier were described by him as a new species, and named "Thibet Bear," whereas the species is not met with in that elevated region. The white mark on the brisket of the South American black bear (*U. ornatus*) and the above is very much alike. It assumes the shape of a crescent, and to the hunter is an excellent point at which to direct his rifle.

Along the base of the chains which encircle the Valley of Cashmere there is an intercommunication between the black and the afore-mentioned isabel-coloured bear; in fact, here is a border line where the two meet and dispute their footing; so that in autumn, when the jungle fruits are plentiful and the Indian corn and other grains are ripening, the latter, descending from his alpine retreats, pushes southwards into the valleys frequented by the former for the purpose of feeding on walnuts, mulberries, and wild apples. Now, considering the specific differences between the two, and that the brown is the larger, it is a fact of which I had ocular demonstration that the black bear no sooner sees his antagonist than he boldly attacks him, and compels a retreat. Indeed, it is a common occurrence in secluded valleys to observe the Malayan bear in an apple or walnut-tree greedily devouring fruit, whilst his brown compeer is feeding on whatever happens to be knocked down, but no sooner does the former descend than the latter decamps into the jungle. A similar competition between allied forms of the same genus takes place at higher elevations on these ranges. There the ibex and great horned wild goat establish themselves on certain feeding grounds and dispute each other's footing, so that the two are rarely seen on the same mountain; in fact, the rule is more or less universal, and the competition is always most severe between allied forms; but, strange to say, it is not invariably the most powerful animal that is victorious; nor does it appear why or wherefore. At all events, this enmity has among other effects that of both contracting and extending the range of species, and when applied to the study of the geographical distribution of living and extinct forms it enables us to understand how an animal may be checked in its advance, driven back, or even exterminated by one of its own genus.

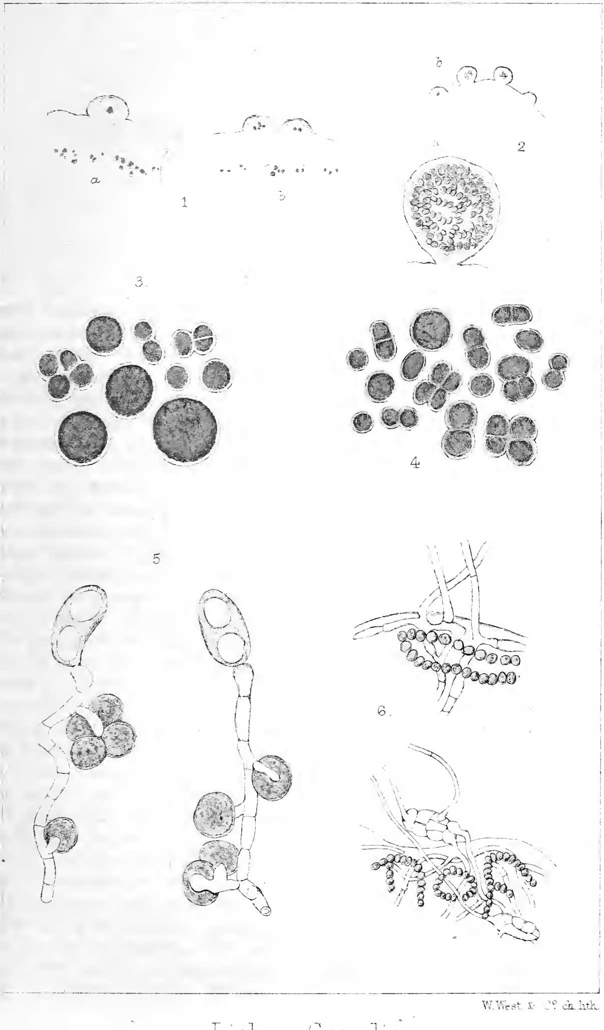
In tracing the geographical distributions of living and extinct bears we naturally wonder how the grizzly found its way to America, and how the black bear of Asia gained admittance into the East Indian Islands. The only reply is that the Aleutian Islands are remnants of a sunken area which united the New and Old Worlds, and that the Malayan peninsula extended further southwards; moreover, as just observed, the same is applicable to Great Britain and Ireland. Thus through mutations in the relative conditions of sea and land we find that many species accustomed to roam over vast tracts of country became restricted to small islands; and to the bear tribe, of all others, this would be trying to their constitutional habit of wandering from place to place; add thereto new climatic conditions, consequent on the geological changes, and we may fairly believe that modifications not only in the habits, but also the outward appearance and internal anatomy would ultimately result, so that the cabinet naturalist, trusting to bodily appearances as represented by specimens from various regions, is apt to assign distinctive characters to what are merely varieties of one species.

ON THE LICHEN-GONIDIA QUESTION.

By the Rev. J. M. Crombie, F.L.S., &c.

[PLATE CXII.]

SINCE the time when lichens were definitely separated from mosses, algæ, and fungi, under one or other of which they were indiscriminately included by the earlier writers, they have until quite recently been regarded as constituting a distinct class of plants. Not only lichenists, but cryptogamists in general, have in all modern systems of classification viewed them as entitled to as definite a position in the vegetable kingdom as the algæ and the fungi. Between these two classes they have universally been held to occupy an intermediate place, though their connection with the one or the other is, in most of the proposed arrangements of their families and tribes, not sufficiently manifest. In Nylander's arrangement, however, which, in its main features at least, is the most natural, and consequently the most scientific yet propounded, lichens are connected with the algæ on the one hand by means of the inferior genera of the *Collemacei*, and with the fungi on the other hand by means of the inferior genera of the *Pyrenocarpei*. At the same time it has always been difficult to draw very definitely the boundary-lines between the three classes; though of late years, with the consent both of algologists and mycologists, the lichen territory has gradually been enlarged at the expense of its neighbours. Alike from the algæ and the fungi it has made various important acquisitions, which of right belonged to it all along, though its title-deeds to them were written in characters so minute or obscure that it required both microscopical aid and keen research rightly to interpret them. The result was that lichenists supposed that they had good grounds for believing that their much-loved, if in some respects difficult science, was at length in a fair way of having its limits well defined, with respect to the other two neighbouring classes of cryptogamics. Very recently, however, a theory has been started, which, should it be



W. West & Co. ch. lith.

Lichen-Gonidia.

accepted, would virtually deprive lichens of the position which had hitherto been assigned them in the vegetable kingdom. For though the name might still be retained for the sake of convenience, or of old associations, it would serve no longer to convey to our minds the notion of a distinct class of plants, but only to remind us of its degradation from a position once as high and apparently more stable than that held either by the algæ or the fungi. "Lichenes fuerunt; algo-fungi modò sunt."

The theory referred to, and now well known as the "Schwendenerian hypothesis," has been favourably received by various continental cryptogamists, and has given rise to a very considerable amount of discussion. No lichenist, however, has as yet given it any support; nor, judging from present appearances, is at all likely to do so. It may very succinctly be stated thus:—Lichens are not autonomous plants, but are composed of an algal and a parasitic fungus. This singular hypothesis, which if well founded would be entirely subversive of all previous conceptions of the nature of lichens, was first made public by Professor Schwendener in 1868, towards the conclusion of a paper entitled "Untersuchungen über den Flechtenthallus." According to the view here propounded, each individual lichen is to be considered as an algal-type which has become the host of a parasitic fungus-growth; whence it follows that these algæ, under the name of "gonidia," have hitherto been erroneously regarded by cryptogamists as special organs of lichens. In a subsequent paper, published in 1869, "Die Algentypen der Flechten gonidien," he expresses his views at still greater length, and endeavours to strengthen and amplify his theory by various arguments founded upon personal observation. To render the theory more intelligible, and to prepare the way for entering into details, it may be well to give the general conclusion which he arrives at in his own, and, in this instance, somewhat pictorial language. "As the result of my researches," he says, "all these growths are not simple plants, not individuals in the usual sense of the term; they are rather colonies, which consist of hundreds and thousands of individuals, of which, however, only one acts as master, while the others, in perpetual captivity, provide nourishment for themselves and their master. This master is a fungus of the order *Ascomycetes*, a parasite which is accustomed to live upon the work of others; its slaves are green algæ, which it has sought out, or indeed caught hold of, and forced into its service. It surrounds them, as a spider does its prey, with a fibrous net of narrow meshes, which is gradually converted into an impenetrable covering. While, however, the spider sucks its prey and leaves it lying dead, the fungus incites the algæ taken in its net to more rapid activity; nay, to more vigorous increase."

Now this hypothesis seems to have been suggested to Schwendener from two different sources. Of these the one was an observation made (of course not seriously) by Nylander in "Lapp. Or." p. 117, to the effect that if the gonimia in the various cephalodia of lichens were parasitic algæ, then it would be necessary to regard *all* lichen-gonidia as such parasites, seeing that in an anatomical point of view they all agreed. And the other and more immediate source was an alternative more seriously put forward by Professor De Bary in "Hoffmeist. Handb. der Phys. Bot." ii. p. 291, where, in speaking of some genera of the *Collemacei*, he says: "These are either perfectly developed states of plants, whose imperfect forms have hitherto stood amongst the algæ, as *Nostocaceæ* and *Chroococcaceæ*; or these latter are typical algæ, which assume the form of *Collema*, *Ephebe*, &c., through certain parasitic *Ascomycetes* penetrating into them, spreading their mycelium into the continuously growing thallus, and frequently attached to their phycochrome-bearing cells." Taking the alternative thus proposed as his starting-point, Schwendener, commencing with the *Collemacei*, made various observations and experiments, which were afterwards extended to other tribes and genera, the result of which was to lead him to the conclusion that lichens consist of the two primarily distinct elements already mentioned—that the lichen-gonidia are algæ, and the lichen-thallus a parasitic fungus. This position he endeavours to support by the following considerations:—1. That as yet the genetic relationship of the gonidia to the hyphæ (that is the colourless filamentose tissue which in conjunction with the coloured cellular portion, or the gonidia, constitutes the lichen-thallus) has in no case been directly proved, but only assumed for anatomical reasons. 2. That chemically the gonidial membranes differ from the hyphal membranes, the former having an algal and the latter a fungal reaction. 3. That the various gonidia-forms, alike in structure and mode of increase, correspond to parallel types of unicellular and filamentose algæ. 4. That as the germination of the spore has not been observed further than the commencement of a hypothallus, this is probably owing to the want of the co-operation of the requisite algal. 5. That between lichens and *Pyrenomycetes* there is a very striking correspondence in the development of the fructification. In the second of the two papers above mentioned, he enters into further details, and enumerates the various algal-types, which he regards as constituting the gonidia. These types, which are eight in number, he includes under two groups, viz. "Phycochromaceæ," and "Chlorophyllaceæ," so called from the colour of their respective cell-contents. To the former group, that with bluish-green cellules, he assigns five types, viz. 1. *Sirosiphoneæ*; 2. *Rivulariæ*; 3. *Scytonemeæ*; 4. *Nostochaceæ*; 5. *Chroococcaceæ*: and to the latter group, that with chlorophyll-green cellules, he assigns the remaining three types, viz. 6. *Confervaceæ*; 7. *Chroolepideæ*; 8. *Palmellaceæ*.

Such genera and species of these as live in water are of course excluded from the question, as it is evident that the fungus can have no access to them. The author also describes the occurrence of these so-called algal-types in various genera of lichens, and makes lengthened observations upon the character they present and the modifications they undergo in several individual species. Amongst other instances adduced by him, he finds the hyphæ entering the fronds of different algæ, e.g. *Sirosiphon*, *Nostoc*, *Glœocapsa*, and encompassing the gonidia with a network; while in a certain number of genera, e.g. *Roccella*, *Arnoldia*, *Lempholemma*, *Pannaria*, he found them uniting themselves to the gonidia by an actual junction. In the conclusion of his paper, he observes that the algal-nature of the lichen-gonidia, which he maintains has been established in the cases he has reviewed, is extremely probable also in every other case, and that consequently the gonidium, as hitherto supposed, is not a self-developed organ of the lichen. But though not thus to be regarded, gonidia would still, in a physiological point of view, remain as instruments of assimilation and of a sexual increase. Although they have not the power in themselves to form a thallus, they are an essential constituent of it, and are undeniably the most important, though not the only ministers of nutriment for the composite plant called a "lichen," inasmuch as this is also partly furnished by means of the substratum. From this general outline of the hypothesis of Schwendener, it will at once be perceived that its adoption would entirely subvert all our previous notions as to the mutual relations between the filamentose tissue and the gonidia of lichens. Nay, more, it would necessitate the degradation of lichens from the position they have so long held as an independent class, and in any system of classification would render it extremely difficult rightly to dispose of the half algal, half fungal monstrosity, called a "lichen."

Passing over several other writers who have, more or less directly, made contributions of greater or less importance to the above theory, we come to a more recent, and in some respects the most plausible advocate of the hypothesis. This is Dr. E. Bornet, whose extensive, and from a Schwendenerian point of view, exhaustive memoir upon the subject, entitled "Recherches sur les Gonidies des Lichens," appeared in "Ann. des Sc. Nat.," 5 ser. t. xvii. (1873). In this he expands and further illustrates the views put forth by him in a previous communication to the Academy of Sciences at Paris, accepting in its full extent Schwendener's theory as the only one capable of explaining satisfactorily many obscure phenomena in the physiology of lichens. He accounts for its rejection by lichenists from the circumstance of Schwendener not having sufficiently demonstrated the relations of the hyphæ and the gonidia, and the manner in which their connection is established. For, as he observes, it is of essential moment to show that these relations are such as necessarily involve the idea of parasitism, and that apart from this they cannot otherwise be rightly interpreted. Noticing the great similarity presented by the gonidia of lichens to certain algal types, he argues that this is not a merely accidental circumstance, inasmuch as these gonidia multiply and reproduce in the same way as the corresponding algæ, and in entire independence of the hyphæ. At considerable length he passes in review an extensive series of lichens, and, as the result of numerous observations, identifies their gonidia with certain algal genera and species.

Adopting the two groups of Schwendener already referred to, he examines first the *Chlorophyllaceæ*, viz.: *Trentepohlia*, Mart.; *Phyllactidium*, Kütz.; *Protococcus*, Ag. Of these he has traced the resemblance between *Trentepohlia* and lichen-gonidia in thirteen genera, viz. 1. *Roccella*, as already pointed out by Schwendener; 2. *Lecanora*, in numerous species; 3. *Dirinia repanda*, Fr.; 4. *Cœnogonium*, *Linkii*, Ehr. and *C. confervoides*, Nyl.; 5. *Byssocaulon niveum*, Mnt.; 6. *Lecidea lutea* (Dcks), and *L. microsperma*, Nyl.; 7. *Graphis elegans*, Ach.; *G. contexta*, Pers., and *G. heterospora*, Nyl.; 8. *Opegrapha varia*, Pers., and *O. herbarum*, Mnt.; 9. *Stigmatidium crassum*, Duby.; 10. *Arthonia cinnabarina*, Wallr.; 11. *Melaspilea arthonioides*; 12. *Chiodecton myrticola*, Fée, and *C. nigro-cinctum*, Mnt.; 13. *Verrucaria nitida*, Schrad., in which, as well as in several *Graphidei*, it had formerly been recognized by De Bary. The genus *Phyllactidium* he regards as furnishing the gonidia to *Opegrapha filicina*, Mnt.; while a species of *Protococcus*, viz. *P. viridis*, Ag., is similar to the gonidia of *Physcia parietina*, as proved by sowing the spores of the latter upon the former. Similarly he reviews Schwendener's other group, the *Phycochromaceæ*, and finds the following genera belonging to it furnishing gonidia to various genera and species of lichens:—1. *Calothrix*, Ag., to *Lichina*; 2. *Scytonema*, Ag., to *Ephebella Hegetschweileri*, Itz., to the cephalodia of *Stereocaulon ramulosum*, Sw, and to various *Pannariæ*; 3. *Lyngbya*, Ag., to *Stereocaulon ramulosum*; 4. *Nostoc* to *Collema*, *Arnoldia*, *Physma*, *Leptogium*, *Obryzum*,* the cephalodia of *Stereocaulon*, *Nephromium*, *Stictina*, and certain *Pannariæ*; 5. *Stigonema*, Ag., to *Lichenosphæria Lenormandi*, Born., *Spilonema paradoxum*, *Ephebe pubescens*, and the cephalodia of *Stereocaulon furcatum*; 6. *Glœcapsa*, Kutz, to *Synalissa*, *Omphalaria*, *Phylliscum* (as formerly shown by Schwendener), *Cora*, and certain cephalodia of *Stereocaulon*. As may readily be inferred from the above, there is not a different alga corresponding to each species or even genus of lichen; for, as Bornet observes, a small number of the former furnish the gonidia for a great many different lichens. In the higher lichens, he acknowledges that it is difficult to see well the attachment of the hyphæ, though in certain of the species above enumerated he has been able to detect the same kind of adhesion between the two as in the other cases.

With reference to the vital subject of the relations of the hyphæ to the gonidia, the author asserts that there is not the least evidence that the gonidia originate from the hyphæ any more than the hyphæ from the gonidia; but that, on the contrary, the two are originally quite independent, and that their union is a subsequent event. This he illustrates and endeavours to prove from observations directly made upon various of the lichens which he thus reviews. In doing so he shows that while in the great majority of lichens the hypha envelopes the alga, forming a more or less embracing network around it, yet in certain cases the union is something more than mere contact, being a penetration by the hypha into the interior of the algal-cell. In such instances, *e.g.* in *Arnoldia minutula*, Born., the cell becomes enlarged, the wall thickens, the contents become colourless; at length the wall shrivels up, and the gonidium finally becomes a dead membrane. In those cases where the algæ are composed of independent cells, the modification which they undergo as the result of this union is not very marked, but in those where they are filamentous the alteration is much more visible, while in some other instances the individual cells are altered without the general appearance of the algal itself being very much affected. Various other points in connection with the relations between the hyphæ and the gonidia are touched upon by Bornet in the course of his researches, which extend in all over some 60 different genera of lichens. Without, however, entering upon these minor details, suffice it to say that as the result of his investigations, he considers that he is fully warranted in establishing the two following propositions:—1. Every gonidium of a

* Bornet does not seem to be aware that this genus is entirely parasitic, and belongs to the Pyrenocarpei.

lichen may be referred to a species of algal. 2. The connection of the hypha with the gonidia is of such a nature as to exclude all possibility of the one organ being produced by the other, and the theory of parasitism can alone explain it satisfactorily. This alone, he urges, can account for the gonidia of very diverse lichens being almost identical, as well as for the marked differences between the gonidia of certain other lichens of which the thallus and fructification are identical. In the same way, too, he thinks can only be rightly explained the origin of dead gonidia found in the interior of lichens, as also the occurrence in the same thallus of dissimilarly coloured gonidia.

As had been suggested by De Bary, various experiments have been made in the way of lichen- and alga-culture, in order to ascertain with certainty whether the germinating lichen-spore developes gonidia or not, and whether these latter in a free state form from themselves hyphæ or not. Accordingly Professor Reess, in order to test the applicability of the theory with respect to the *Collemacei*, made certain experiments, which were recorded in October, 1871, in "Monatsb. der k. Akad. der Wissensch. zu Berlin." These consisted in sowing the spores of *Collema glaucescens*, Hffm., upon *Nostoc lichenoides*, Vauch., which readily germinating sent forth numerous filaments that penetrated into and spread within the substratum (see our Plate CXII., fig. 6). After undergoing various phases of development, they at length surrounded the moniliform gonidia, and formed the "collema-mycelium," or hypha-mass, so that in this way, in process of time, the Nostoc becomes converted into a Collema, although no fructification could be obtained. Hence Reess inferred that every Collema is in free nature a "nostoc," which should no hypha come to it remains so, but which, should it be thus inoculated, undergoes the above transformation. We may here observe that while Reess' experiment appears to confirm Schwendener's theory as to Collema being a compound organisation (and it has constantly been adduced as one of its main supports), he at the same time strongly suspects, nay, even plainly asserts, that the fungus-hyphæ represented by the latter (Schwend. t. x. figs. 19–21) as attacking a nostoc, are in reality those of a true mould. But whether this be so or not, both writers agree in regarding Nostoc as an independent algal, which may either normally continue so, or become the host of a parasitical fungus, and by it be converted into Collema. Strongly impressed by the arguments put forward by De Bary, Schwendener, and Reess, as to the nature of the *Collemacei*, some, amongst whom is Professor Cohn, while regarding the theory as untenable with respect to the other families, would exclude the *Collemacei* altogether from the class of lichens.

A few years previous to this, however, instigated no doubt by the researches of Cohn, a microscopist of the first order, who in 1852 indicated the production of zoospores in the gonidia, or at least in *Protococcus*, which he supposed might come from lichen thalli, certain collateral investigations had been made by Famintzin and Baranetsky. The results of these were recorded in several continental journals, e. g. " Mem. de l'Acad. imp. Sc. St. Petersbourg," 1867, and Melang. " Biol. Bull. de l'Acad. de St. Petersbourg," 1868. Having obtained chlorophyllaceous gonidia from the genera *Physcia*, *Evernia*, and *Cladonia*, they succeeded in cultivating them independently of the lichens themselves. According to their report some of these, as those of *Physcia parietina*, produced zoospores, while others continued to increase by vegetative growth. These free gonidia they concluded were identical with the genus *Cystococcus*, Naeg., which is thus not an independent alga, but only a phase of the gonidia of the lichens under review. So also with respect to the phycochromaceous gonidia obtained from *Collema* and *Peltigera* similarly cultivated, they found that these were capable of maintaining an independent life, and in some cases were identical with so-called algals. Hence they concluded that these free-living gonidia ought to be omitted from the list of algæ. Somewhat similar experiments were subsequently made by Woronin (*vid.* " Ann. des. Sc. Nat. " ser. v. t. xvi. p. 317) on *Physcia parietina* and *Ph. pulverulenta*, but in neither case was any lichen-thallus produced. Bornet also attempted the cultivation of spores and algæ together, and in his memoir gives details of the experiment in which he was most successful, where some spores of *Physcia parietina* were sown upon a layer of *Protococcus viridis*. Germination of the spores took place some days subsequently, and they put forth radicle filaments, which speedily elongating and ramifying, wherever they came in contact with the cells of the Protococcus, adhered either directly or by a lateral branch, the filament in either case being applied closely to the gonidium and surrounding it [see our Plate, fig. 5]. He adds that if the presence of the spores still adherent did not show the true nature of these radicles, it would be impossible to distinguish the gonidiiferous filaments from the adult lichen. All other experiments, however, of this kind, are certainly, in so far, at least, as the amount of labour expended, and the variety of methods adopted, and the apparent care taken to ensure success, completely cast into the shade by those of Dr. Treub. A short notice by him in " Lichenencultur," in " Bot. Zeit.," Nov. 1873, was followed immediately afterwards by a lengthened memoir, entitled " Onderzoekingen over de Natuur der Lichenen," Leiden, Nov. 1873. In the second section of this he gives a detailed account of the researches and experiments on the subject which he made in 1872–3. Unfortunately for himself, in the view of the time occupied and the pains taken, most of these ended in an abortive manner; and with respect even to the most favourable of the results obtained, he is forced to acknowledge, that while they would lead him to infer the truth of Schwendener's theory, they would not be wholly decisive.

Such, then, may be regarded as a very brief outline of the main features of the notorious "Schwendenerian hypothesis," and of the physiological arguments and the culture experiments by which it has been thought to be substantiated. We therefore proceed similarly to give an equally brief sketch of the reasons which have been adduced on the other side to show that it is quite untenable, having no solid foundation to rest upon, and no certain proofs to sustain it. As already intimated, it has up to the present time been universally rejected by lichenists. And this not because it was something novel, startling, and subversive, but upon other grounds which have appeared to them to be quite good and sufficient. Amongst others Nylander, Krempelhuber, Th. Fries, Müller, have given forth their verdict upon the subject in very decided terms, regarding the theory as impossible, nay, even absurd. Nor, indeed, was it to be expected that they would stand by *æquo animo* and see their favourite lichens ruthlessly deprived of their autonomous existence, and be converted, as if by the stroke of a magician's wand, into a spider-like master fungus, and an imprisoned algal slave. Surely it must have been the very novelty of such a strange theory much more than the apparently plausible arguments by which it was supported that has recommended it to the acceptance of any botanist of the so-called "new school." All the lichenists just named have made various contributions of greater or lesser length in opposition to the hypothesis, while Köerber and Tuckerman have also most decidedly refused to accept it, and expressed themselves as still continuing firmly to hold that lichens are independent plants. Some of the contributions referred to are scattered over various botanical journals, while others are contained in separate lichenological treatises. Those of Nylander are contained chiefly in "Lapp. Or." (1866), p. 117, "Notula de Cephalodiis;" in "Flora," 1868 p. 185, "Circa evolutionem gonimicam Collemaceorum Notula;" *ibid.* 1870, "Animadversio de Theoria Gonidiorum algologica;" "Obs. Lich. Pyr. Or." (1873) p. 45, "Contra hypothesim parasitosam Schwendenerianam;" and more especially in "Flora," 1874, p. 56, reprinted with additions in "Grevillea," II. p. 145, "On the algo-lichen hypothesis, and the nutrition of lichens." The observations of Krempelhuber, who enters at considerable length into the subject, and discusses *seriatim* the considerations adduced by Schwendener in favour of his hypothesis, are contained in his well-known "Geschichte und Litteratur der Lichenologie," iii. Bd. 1872. Those of Dr. Th. Fries are recorded in the introduction to his "Lichenographia Scandinavica," 1871, pp. 4–8, though he does not enter very minutely into details. Dr. Müller has also made a short communication upon the subject in the "Flora," 1872, p. 90, in which he declares the new theory to be impossible, and at the same time puts forth a hypothesis of his own, which he thinks would give a new and more natural interpretation to the researches of Schwendener. And not only have these lichenists entered the field to maintain the old and well-established theory, but other botanists, such as Professor Caspary, have also lent their valuable assistance. This latter distinguished botanist has made some valuable observations upon, and adduced very strong arguments against, the hypothesis in a paper "Ueber die neuren Ansichten in Betreff der Flechten, wonach diese Schmarotzer seien," in Schriften der Physik Oekon. Gesellschaft in Kœnigsberg, 1872, Abth. ii. p. 18.

To give, however, anything like a fair abstract of the several arguments used by these writers in the various memoirs and works now quoted, would of itself far exceed the limits of the present article. All, therefore, that we can attempt to do is simply to point out their bearing upon the leading principles of the Schwendenerian hypothesis, as originally propounded by the author, and subsequently further illustrated by Bornet and others. And, indeed, if these leading principles can be shown to be untenable, and to have no foundation in fact, then all the subsidiary points connected with them must necessarily lose all their force and meaning. The best method, probably, as it certainly is the most concise, of showing the invalidity of the theory under discussion, will be to review the two main considerations upon which the Schwendenerian theory is evidently founded, and which the celebrated author, in a more recent communication entitled "Die Flechten als Parasiten der Algen" in "Verhandl. der Naturf. Gesellschaft in Basel," 1873, again puts prominently forward. Afterwards we shall notice any objections of a different nature, which do not so directly come under either of these heads. The two main considerations referred to, which thus fall to be discussed are—1st, the agreement of lichen-gonidia with algæ; and 2nd, the relation between the gonidia and the hyphæ.

With respect to the former point, Schwendener, as already quoted, asserts "That the different gonidia-forms, as to structure and manner of increase, correspond to parallel types of unicellular and filamentose algæ Now there can be no question that the hypothesis has its origin, and its sole origin, in the resemblance of the gonidia to certain algæ or pretended algæ, and but for this it would never have been heard of. Such a resemblance had previously been noticed by authors, and so far back as 1849 Thwaites, in "Ann. Nat. Hist.," ser. ii. vol. iii. p. 219, had distinctly drawn attention to the circumstance. In this, however, he rightly saw but as it were a parallelism between the two; for "similarity," be it observed, is neither logically nor scientifically synonymous with "identity." Itzigsohn also, in "Bot. Zeit.," 1854, p. 521, pointed out the similarity of certain lichen-gonidia to free algal forms; though Schwendener regards this as evidence of identity. Subsequently Nylander, in "Flora," 1870, p. 92, when briefly noticing the algological theory of gonidia, very pertinently observes that such an unnatural existence as they would thus pass, enclosed in a prison and deprived of all autonomous liberty, is not at all consonant with the manner of existence of the other algæ, and that it has no parallel in nature, for nothing physiologically analogous occurs anywhere else. He also asks what prevents the gonidia of lichens presenting forms and a structure similar to algæ (or the gonidia of algæ?) as in certain specified instances, and yet both remain distinct classes of vegetation, noticing as a case in point that although chlorophyll is nearly similar in mosses, ferns and phænogamics, no one has ever on that account united them in one and the same class. The more accurate view to be entertained of those algæ which have the appearance of gonidia is, he observes, to regard them at least partly as being in reality not algæ, but erratic lichen-gonidia vegetating abnormally. This is the view taken also by Krempelhuber, who holds that there are no conclusive reasons against the assumption that the lichen-gonidia may be self-developed organs of the lichen proper rather than algæ, and that these gonidia can continue to vegetate separately, and so be mistaken for unicellular algæ. So also Th. Fries, l. c. says that it is likely that various modern families of "algæ" consist of lichen-gonidia growing free, which ought therefore to be excluded from the system of algæ. Again, in "Obs. Pyr. Or." l. c. Nylander argues that were the hypothesis true, "Lichens would grow best and occur most abundantly in places where those algæ which are regarded as the gonidia of lichens abound, and would there also be observed to be crammed with these elements." On the contrary, however, he affirms (and the experience of all field-lichenists will verify the assertion) that "lichens avoid these stations, which are not inhabited except sparingly by *Collemacei* and a few others, which are not always well developed, and which do not contain any such algoid-gonidia in their texture." Moreover, he adds (p. 47) that "so far are what are called algæ, according to the turbid hypothesis of Schwendener, from constituting true algæ, that on the contrary it may be affirmed that they have a lichenose nature, whence it follows that these pseudo-algæ are in a systematic arrangement to be referred rather to the lichens, and that the class of algæ hitherto so vaguely limited should be circumscribed by new and truer limits." In this last observation there no doubt lies a complete answer to the above consideration adduced by Schwendener in support of his theory. Hence such so-called algal genera as *Cora*, Fr., *Dichonema*, N. ab Esnb., *Scytonema*, Ag. (= *Gonionema*, Nyl.), *Sirosiphon*, Kutz (= *Spilonema*, Born.), and probably some others are as yet known only in a very imperfect condition. This, as will at once be perceived, would very considerably reduce the lists given by Schwendener and Bornet; and if it be conceded (nor can it reasonably be denied) that some other algal genera are but free living lichen-gonidia, these lists would clearly dwindle down to nothing. Such a solution of the, in some cases *real*, and in others only *fancied*, identity of certain supposed algæ with lichen-gonidia, is certainly much more natural and intelligible, even as it rests upon surer grounds, than the forced and *prima facie* most improbable Schwendenerian theory. As a very striking example of this *fancied* identity, which after all is only "similarity," let us take one of Bornet's strong points, viz. the identity of *Protococcus viridis* with the gonidia of *Physcia parietina*, for this is evidently what he wishes to be inferred from his culture experiment. The two are no doubt very *similar*, but a reference to our Plate, figs. 3 and 4, will suffice to show that they are not *identical*; for, as will be seen, the gonidia of *Physcia parietina* are larger, and multiply themselves in a lesser degree, while the *Protococcus* multiplies itself with the utmost readiness and celerity, much more quickly indeed than do the gonidia. *Ex uno disce omnes*. So much, then, for the first of the two main considerations by which Schwendener seeks to support his hypothesis.

We turn, therefore, to the second of these, viz. the relation between the gonidia and the hyphæ. Here he affirms, "That as yet the genetic relationship of the gonidia to the hyphæ has nowhere been directly proved, but only assumed for anatomical reasons," whilst, as he adds, "the anatomical connection itself may possibly depend on 'copulation.'" Now, it is worthy of notice that this assertion is directly at variance with what Schwendener himself originally believed, and which he supported by figs. in Nægeli, "Beiträge zur wiss. Botanik," Heft. ii., p. 125, t. i. f. 18, t. v. f. 6. This view, he thought, was sufficiently established by the previous observations of Bayerhoffer and Speerschneider, though he maintained that the gonidia were produced from the intermediate joints of the hypha and not from the terminal cells. If, then, the figures by which he illustrated his original views upon this point were correct, as they presumably were, it is a legitimate inference that the change in his opinion more immediately originated in the desire to obtain additional confirmation of the fundamental principle of his theory already noticed by pressing the connection, real or assumed, of the hypha to the "alga" (gonidia) into his service. It is, indeed, very difficult, as every lichenist who has made the experiment well knows, to trace this connection except in the earliest stages of lichen development, and even there only under exceptionally favourable circumstances. Hence the various experiments already noticed in lichen-culture were instituted chiefly for the purpose of ascertaining *a primo initio* what was the origin and the character of this relationship. And here we may observe that from those recorded, which are in various respects confessedly unsatisfactory and inconclusive, the hypothesis in reality derives little or no support, since in great part, at least, the phenomena witnessed, or said to have been witnessed, are capable of another and more probable interpretation. Thus, with respect to Reess' famous experiment with Nostoc and Collema spores, there can be little doubt that Nylander's hypothesis, as expressed in "Flora," 1868, p. 353, *et alibi*, is correct, viz. that "Nostoc" is in part, if not altogether, a rudimentary or undeveloped state of "Collema," analogous, as it were, to the "leprariæ" amongst the Lichenacei. He has observed that nostocine thalli are frequently met with in sandy and gravelly places, and in the same spots shortly afterwards are found *Collema pulposum*, which entirely agrees with our own observations on chalky detritus at Shiere in Surrey, and in the case of various other species of *Collemacei* on gravelly soil in the West Highlands. This certainly seems a much more natural solution than the "dimorphic" theory of Dr. Müller in "Flora," 1872, p. 90. But even were this not the relation between Nostoc and Collema, the experiments of Reess would, after all, prove absolutely nothing in favour of his theory in this particular instance; for had he sown the spores of a *Parmelia* or *Opegrapha* upon the nostoc, he would have witnessed a similar phenomenon. So, also, with respect to the filament germs seen by Bornet and Treub entering into *Protococcus*. This, again, would not prove the position taken up by them, for M. Norman, an equally practical and distinguished observer, has seen hyphæ surrounding the chlorophyll of *Jungermannia* and its grains of pollen, *vid.* "Allelositismus," 1872, pp. 249 and 252. But though from the nature of the case thus difficult to be correctly ascertained, and though the results of cultivation are thus inconclusive, what we believe to be the true connection between the filamentose tissue and the gonidia had been sufficiently established by the observations of Bayrhoffer, Speerschneider, Tulasne, Thwaites, Gibelli and others. Nay, De Bary himself, in his treatise "Morphologie und Physiologie der Pilze Flechten, und Myxomyceten," p. 258, &c., had distinctly shown that the green gonidium originates in the expansion of a short lateral branch of the hypha, which becomes shut-off as a globular-cell, and acquires a green colour. This green matter Nylander in "Flora," 1874, p. 76, asserts, as the result of observation, arises originally within the primary chlorophyll- or phycochrom-bearing cellule, and is not intruded from any external quarter. And that the gonidia themselves are not derived from any foreign source, will be sufficiently evident from a reference to Figs. 5 and 6 of our Plate, where in the case alike of a lichenaceous and a collemaceous species, it is apparent that they originate in the *isidia*; for it is in the young isidia that they may be most conveniently studied and that their origin is most clearly seen.

Once formed, as De Bary proceeds to show, the gonidium increases independently by division; but though usually seen placed on the branchlets of the hyphæ, yet this is not universally the case, for a number of them eventually lie without stipites scattered between the hyphæ. This has still more recently been confirmed by Th. Fries, "Lich. Scand." p. 7, who, from direct observations, adopts and defends the opinion. For, as he states, the hyphæ are not only elongated into filaments, but also send out short branches, of which the terminal cell gradually dilates, becomes subglobose, at length is filled with chlorophyll, is subsequently changed into a gonidium, which finally is variously divided and gives origin to other gonidia. With respect also to a number of these gonidia being found free and scattered amongst the hyphæ, the same author rightly argues that this circumstance is by no means antagonistic to the opinion that there is normally present a connection between the gonidia and the hyphæ. As bearing also upon the point in hand, Krempelhuber, l. c., observes that Schwendener does not attempt to explain from what source the fungal hyphæ, which involve the green algæ, are derived. On the assumption, however, that he would explain it by asserting that the filaments produced from the germinating lichen-spore are the hyphæ of the first rudimentary thallus, he urges that the idea of such hyphæ going in search of an algal host, and giving rise to a lichen-thallus with fructification, is inconceivable. Moreover, there are species of lichens which in many countries never fructify, and whose propagation can consequently be carried on only by means of the soredia, and the hyphæ of such could in themselves alone no more serve for propagation than the hyphæ from the pileus or stalk of an Agaric, while it is highly improbable that they could acquire this faculty by interposition of a foreign algal. On the other hand, he argues it is much more conformable to nature that the gonidia, as self-developed organs of the lichens, should, like the spores, enable the hyphæ proceeding from them to propagate the individual. This we believe to be the true doctrine concerning the relation of the gonidia to the hyphæ; and when the new theory has had its brief, if brilliant day, will no doubt be generally accepted. But apart altogether from these considerations, there remains another argument, which of itself is sufficient to show, whatever the connection in other respects between these two elements may be, it is certainly not that of a "fungus" with algal-colonies or lichen-gonidia. For Schwendener's master parasite turns out to be no *fungus* after all, as has clearly been shown by Nylander, in "Grevillea," ii. p. 147, note: "The anatomical filamentose elements of lichens are distinguished by various characters from the hyphæ of fungi. They are firmer, elastic, and at once present themselves in the texture of lichens. On the other hand, the hyphæ of fungi are very soft; they possess a thin wall, and are not at all gelatinous, while they are immediately dissolved by the application of hydrate of potash, &c." This well-marked and essential distinction between the two, which observation amply verifies, at once effectually demolishes the fungal part of the Schwendenerian hypothesis

So much, then, for the two leading principles upon which the new theory evidently rests. The three other minor considerations adduced by the author in its support, as quoted by us above, are disposed of by Von Krempelhuber, l. c., in a very few words. To the second, he replies that this difference of reaction is of no importance, since the membranes of the thecæ originating from the hyphæ give the same reaction as that of the gonidia-membrane. To the fourth, he replies, that though the observations of Tulasne, &c., did not entirely succeed in directly establishing the development of the gonidia from the hyphæ, still, from their regular appearance upon the latter, the probability that this was their origin cannot reasonably be denied. And to the last consideration he replies that the presence of gonidia is not the only distinguishing feature between even the lowest lichens and such fungi (*Pyrenomycetæ*) as show an agreement in their fructification. But besides these particular objections to the hypothesis, there are various other more general ones, of equal validity, which have been adduced. Of these the two principal have reference to the nutrition and the distribution of lichens. With respect to their nutrition, Nylander, "Obs. Pyr. Or.," l. c., has stated that "it is around the gonidia (or gonimia) that we perceive the vegetative life to be chiefly promoted and active, as for example (putting forth young parts and) creating colorific matter; while, on the contrary, those portions of the thallus remote from the gonidia or advanced in age—as best appears in incrassate crustaceous lichens—having lost their life, become entirely tartareous, forming as it were but 'thickened deposits.' Now as the gonidia of all lichens are normally covered by the filamentose tissue, while in many a continuous cortical stratum entirely surrounds the other portions of the thallus, it is evident that, thus isolated from the outer world, they can derive nutriment for their growth and increase only from the thallus itself, to whose nutrition, according to Schwendener, as already intimated, they should themselves subserve. But, as is very aptly observed by Th. Fries, l. c., p. 5, "It is very well known that other plants, from which parasites draw their nourishment, become in consequence languid, sicken and die, while here we see plants (algal colonies) on all sides infested by parasites (fungi), which not only do not suffer any injury, but are even so incited and stimulated that they grow, increase and multiply all the more." Well may he exclaim, "a useful and invigorating parasitism—who ever before heard of such a thing?" Add to this, that, as shown by Nylander at considerable length in "Flora," 1874, pp. 59–61, lichens derive their nourishment directly from the atmosphere (receiving nothing from the substratum unless as if mechanically, e.g. iron and lime), and that this penetrates chiefly through the surface (the cortical stratum) of the thallus to the gonidial stratum where the active life chiefly has its seat, and it is clearly demonstrated that Schwendener's idea is not only *à priori* most improbable, but also *à posteriori* entirely erroneous.

But another strong objection has been made on the score of the distribution of lichens. On this point Krempelhuber argues that as many lichens are cosmopolitan, and as their gonidia are everywhere alike, it must, on the Schwendenerian hypothesis that these are algæ, be assumed that such algæ have as wide and general a distribution as the lichens, and that this being entirely contrary to our present knowledge of algal distribution, is in the highest degree improbable. Moreover, and as bearing more directly upon this argument, it may quite pertinently be asked, how is it that lichens, if the composite plants represented, are met with in situations where neither algæ nor fungi are seen? This has been very distinctly put by Dr. Müller, l. c., where he observes that in the high Alps, amongst huge expanses of rocks, far removed from woods, where no *Ascomycetes* occur, and where algæ are but rare, lichens are often met with in great abundance. This is amply confirmed by our own observations amongst the higher Grampians in Braemar, where towards their summits, amidst extensive wastes of detritus, granitic and quartzose boulders, where not a single fungus is seen, and where algæ are unknown (for the *aquatics* in the nearest springs and rills are, on Schwendener's own showing, excluded), the *Lichenaceæ* are sufficiently plentiful and varied. Other arguments, of greater or less force, have been made by Krempelhuber in the work we have quoted, but our limits will not permit us to enter upon these. We shall therefore content ourselves by simply stating in conclusion the three general, and as we take it unanswerable objections, made to the hypothesis by Professor Caspary in the paper above noticed. There he urges:—1st. That if the theory were true, the fungus-parasite would in size and number of cells exceed many hundred times the nourishing plant, though nothing such elsewhere occurs in nature. 2nd. The theory is impossible, for the "algæ" in which the fungus is parasitic would display the greatest health and vigour, and at the same time would be multiplied, which is absurd. 3rd. That the theory is impossible because the nourishing "alga," being entirely enclosed in the fungus, evidently can supply to it no nourishment.

Those of our readers who wish to know further details of the hypothesis and the objections than we have been able to give in the space now at our disposal, will find such in the various memoirs, papers and works which we have quoted, as also to a certain extent in Mr. Archer's excellent *résumé* of the subject, "Quart. Journ. Micr. Sc." vol. xiii. p. 217, and vol. xiv. p. 115. A translation by the same gentleman of Schwendener's most recent contribution to the hypothesis, in which he replies to various objections that have been urged against it, entitled "On the Nature of the Gonidia of Lichens," will also be found in the same journal, vol. xiii. p. 235. In this, as was only to be expected, Schwendener still endeavours to maintain his theory in all its integrity, and farther illustrates, by other similar observations, this sensational "Romance of Lichenology," or the unnatural union between a captive Algal damsel and a tyrant Fungal master.

EXPLANATION OF PLATE CXII.

Fig. 1. *a* and *b*. Sections of the thallus of *Pertusaria Westringii*, magnified forty diameters, whence it is evident that the gonidia are produced from the internal cellules of the isidiose globule, and are not derived from any other quarter.

FIG. 2. *a* and *b*. Isidose globules of *Collema furvum* in different conditions, whence it is seen that the gonidia originate from the very beginning in the isidia themselves.

" 3. Gonidia of *Physcia parietina.*

" 4. *Protococcus viridis*, both equally magnified, and both from the same piece of bark, showing that, though sufficiently similar, they are yet not identical.

" 5. Copied from Bornet (Pl. x.), and showing the spores of *Physcia parietina* sown together with *Protococcus viridis*, sending forth filament-germs which insert themselves into or fix themselves upon the globules of the *Protococcus.*

" 6. Copied from Reess, and showing the filament-germs of the spores of a *Collema* entering into *Nostoc lichenoides*, Vauch.

THE LOGOGRAPH, OR WRITING BY THE VOICE.

By W. H. BARLOW, F.R.S., V.P. Inst. C.E.

All articulated sounds made by the human voice are accompanied by the expulsion of air from the mouth; and in a series of articulated sounds the air is ejected in impulses which vary in quantity and pressure, and in the degree of suddenness with which they commence and terminate.

It appeared to me that it would be interesting and probably useful, as tending to elucidate the process and effects of articulation, to construct an instrument which should record these pneumatic actions by diagrams, in a manner analogous to that in which the indicator-diagram of a steam-engine records the action of the engine.

In considering a suitable form of recording instrument, the conditions to be met were: first, that the pressures and quantities were very variable, some of them being extremely small; and, secondly, that the impulses and changes of pressure follow each other occasionally with great rapidity.

It was therefore necessary that the moving parts should be very light, and that the movement and marking should be accomplished with as little friction as possible.

The instrument I have constructed consists of a small speaking-trumpet about 4 inches long, having an ordinary mouthpiece connected to a tube ½ an inch in diameter, the other end of which is widened out so as to form an aperture of 2¼ inches diameter.

This aperture is covered with a membrane of goldbeater's skin or thin guttapercha.

A spring which carries the marker is made to press against the membrane with a slight initial pressure, to prevent as far as practicable the effects of jar and consequent vibratory action.

A very light arm of aluminium is connected with the spring, and holds the marker; and a continuous strip of paper is made to pass under the marker in the same manner as that employed in telegraphy.

The marker consists of a small fine sable brush, placed in a light tube of glass $\frac{1}{10}$ of an inch in diameter. The tube is rounded at the lower end, and pierced with a hole about $\frac{1}{20}$ of an inch in diameter. Through this hole the tip of the brush is made to project, and it is fed by colour put into the glass tube in which it is held. To provide for the escape of the air passing through the instrument, a small orifice is made in the side of the tube of the speaking-trumpet, so that the pressure exerted upon the membrane and its spring is that due to the difference arising from the quantity of air forced into the trumpet and that which can be delivered through the orifice in a given time.

There being an initial pressure upon the membrane, to prevent vibratory action as before described, the strength of the spring and the size of the orifice had to be adjusted, so that while the lightest pressures arising under articulation could be recorded, the greatest pressures should not produce a movement exceeding the limit of the width of the paper.

It will be seen that in this construction of the instrument the sudden application of pressure is as suddenly recorded, subject only to the modifications occasioned by the inertia, momentum, and friction of the parts moved. But the record of the sudden cessation of pressure is further affected by the time required to discharge the air through the escape orifice.

Inasmuch, however, as these several effects are similar under similar circumstances, the same diagram should always be obtained from the same pneumatic action when the instrument is in proper adjustment; and this result is fairly borne out by the experiments.

We are thus enabled to trace to what extent the pneumatic action varies with different articulations; and it will be seen that, although there are instances in which considerable differences in sound do not make much variation in the diagram, yet, as a rule, every change of sound or articulation produces a change in the diagram, and that there are pneumatic actions revealed by this instrument which are imperceptible to ordinary observation.

Before referring to the peculiarities of the diagrams, it may be desirable to say a few words on the quantities of air used in articulation.

On reference to medical authorities, it appears that the average quantity of air expelled in one respiration is estimated at 40 cubic inches, and that the total air-space of the lungs is estimated to average 110 cubic inches.

I have ascertained by experiment that a balloon made of gold-beater's skin, whose cubic content when full was 523 cubic inches, was filled with twelve ordinary respirations, or at the rate of about 44 cubic inches for each respiration.

Also that by filling and emptying the lungs as completely as practicable, the 523 cubic inches could be filled with six respirations, or about 88 cubic inches for each respiration.

I also made the following experiment to ascertain the average quantity of air used in pronouncing syllables.

Using the same balloon and speaking into an elastic tube communicating with it, I read from a book until the balloon was filled, taking care to close the elastic tube when it was necessary to take breath.

The results were as follows:—

	Time required.	No. of syllables.	Cubic inches.
	84 seconds	353	523
	84 "	353	523
From another part of the book	90 "	364	523
" "	95 "	364	523
Mean . .	86 "	359	523

Showing an average of about $1\frac{1}{2}$ cubic inch of air for each syllable, and rather more than four syllables per second, including stops.

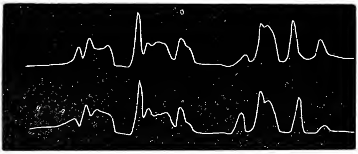
Without stops, from five to six syllables can be pronounced in a second.

The lungs appear to be capable of exerting considerable pressure in the expulsion of air; but distinct articulation becomes difficult against a pressure of 2 inches of water, and I could not pronounce any words against a pressure of 4 inches without considerable exertion.

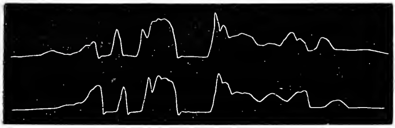
The following diagrams made by the instrument show the degree of accordance obtained when the same words are repeated by the same speaker :—

Experimental Tests.

Recording Instrument.



Repetition Trials.



One of the first features manifested in using the instrument is the action produced by the silent discharge of air from the mouth, after a syllable or word or a sentence is pronounced. This silent discharge appears to depend on the force required in the last syllable, if more than one are consecutively uttered, and is most developed in those syllables terminating with the consonants termed "Explodents," whether with or without the silent vowel E after them. This effect is exhibited in fig. 1., page 282.

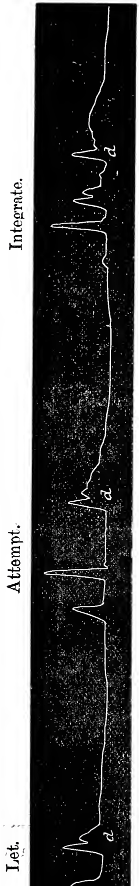


Fig. 1.

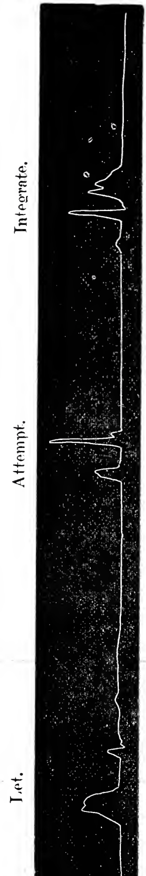


Fig. 2.

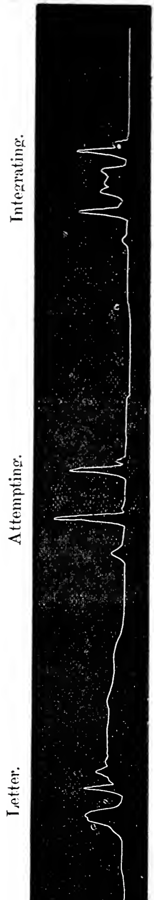


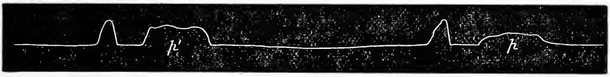
Fig. 3.

In these diagrams the part marked *d* is the silent discharge, and its appearance in the diagram is under the control of the will; for, by holding the breath immediately after pronouncing the word, this part of the diagram can be altered and the discharge of air postponed or let off gradually, as exhibited in fig. 2.

If, instead of terminating with the "Explodents," another syllable be added to each word, making them terminate with consonants of softer sound, the air which would have been silently discharged is used to form the syllable added, and the subsequent silent discharge is very much diminished (see fig. 3).

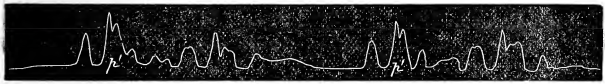
There are other silent or, rather, insensible actions which occur *within* certain words, as is exhibited in the differences between the word "*Excommunicate*" and the syllable "*Ex*" and the word "*Communicate*," pronounced separately.

Ex. Ex.



Communicate. Communicate.

Excommunicate. Excommunicate.



Here it is seen that the part *p*, which is the secondary sound of the syllable "*Ex*," becomes compressed, its length being shortened and its height increased; so that although nearly insensible as regards sound, it becomes developed into the form *p′*, and constitutes the most prominent feature of the diagram when the whole word is pronounced.

Some words are shortened when a syllable is added. This effect is strongly exhibited in the word "*Strengthen*" as compared with "*Strength*." "*Strength*" is, I believe, the only word of one syllable in the English language which contains seven consonants, all of which are pronounced.

The diagrams are as follows :—

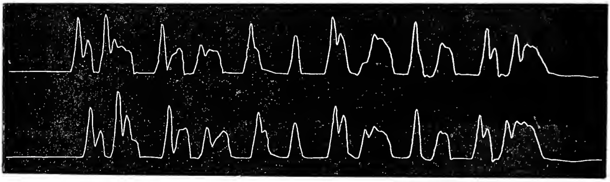
Strength. Strength.

Strengthen. Strengthen.

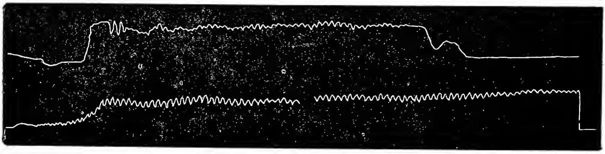
As a test of the rapidity of action of the instrument, I have used the old nursery words, "*Peter Piper picked a peck of pickled pepper.*"

This is said at the rate of six syllables per second; and it will be observed that there are two principal upward and two principal downward movements to many of the syllables, besides other subsidiary actions.

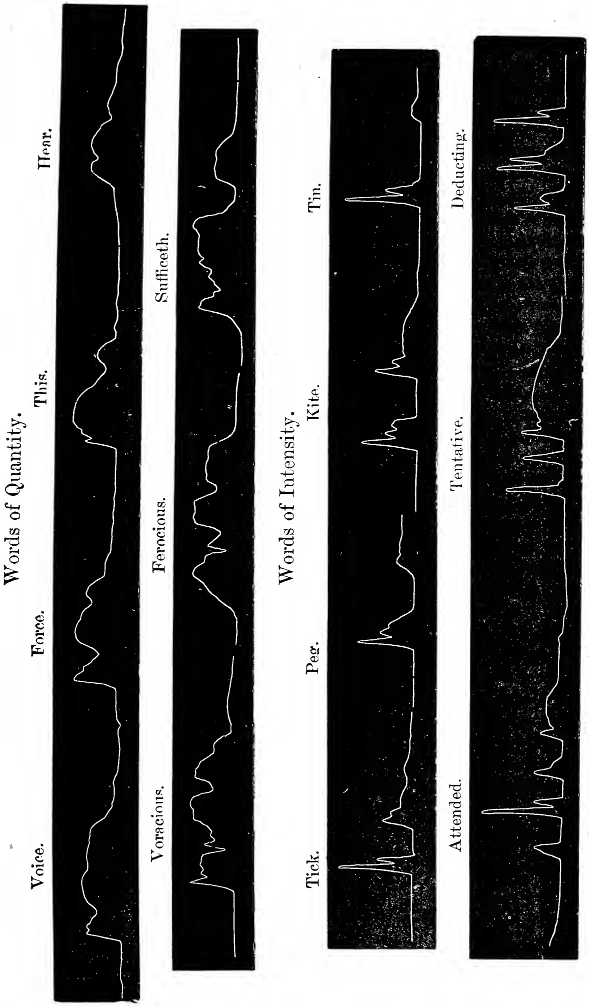
Peter Piper picked a peck of pickled pepper.



Another curious test is the continuous sound of the rough R. The upper diagram is pronounced by myself, the lower one by my son.



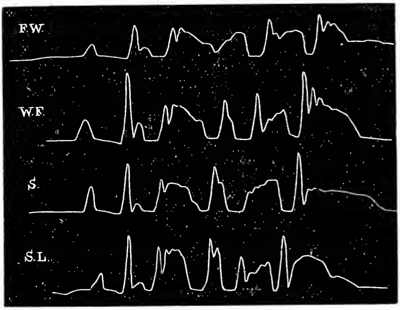
There is a very marked difference in the quantity of air used and the degree of compression in different words and syllables.



The difference in the action between whispered sounds and those spoken loud is not so great as might have been expected.

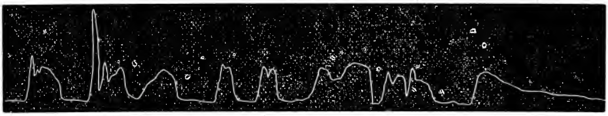
The word used in the four following trials is "*Incomprehensibility.*"

The first is whispered faintly,
The second is whispered forcibly,
The third is spoken at the ordinary tone of the voice,
And the fourth is spoken loudly.

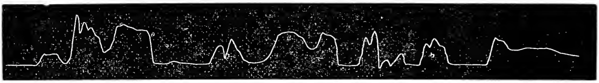


In order to show the manner in which the diagrams of words are affected when spoken together, I give four lines from Hohenlinden," and the words separately.

By torch and trumpet fast arrayed,

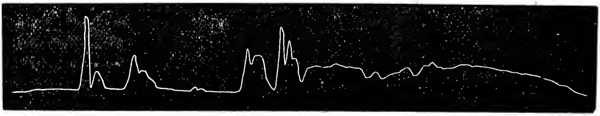


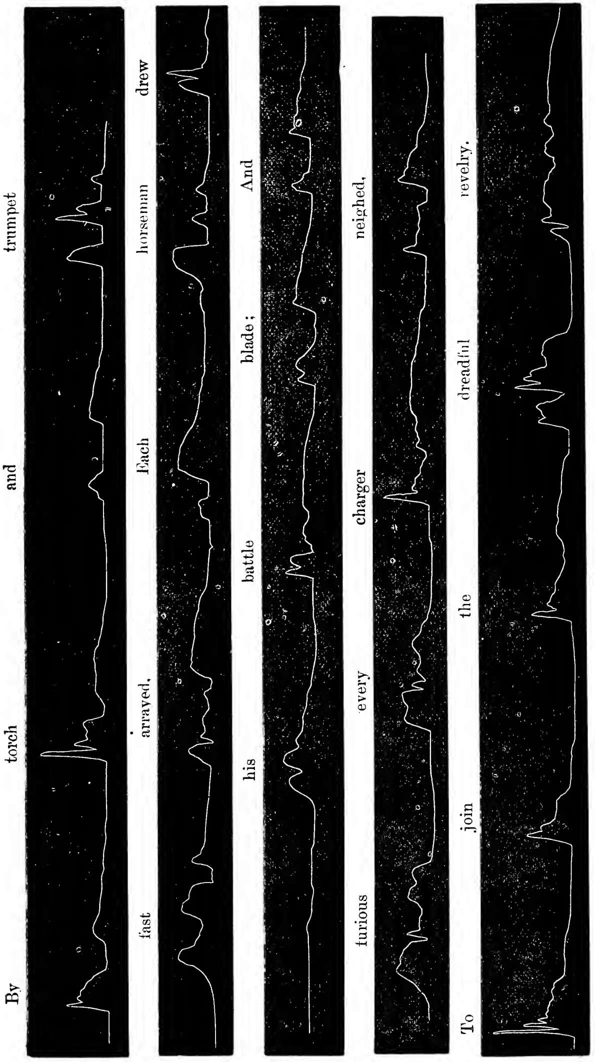
Each horseman drew his battle blade:



And furious every charger neighed,

To join the dreadful revelry.




By torch and trumpet
fast arrayed, Each horseman drew
his battle blade; And
furious every charger neighed,
To join the dreadful revelry.

It will be observed that the diagrams of the separate words, although they become modified when grouped together, are more or less discernible in the lines continuously spoken; and the similarity of sound at the termination of the first three lines, which constitutes the rhyme of the verse, is represented in the similarity of form, or in the character of the form, of the terminations of the diagrams of these three lines.

The subject might be pursued much further by showing the diagrams of the same words spoken by different individuals, the outlines produced by the words and sentences of other languages, the effect produced by change of accent, &c.

My object, however, has not been to pursue the subject into minute detail, but to show that the articulation of the human voice is accompanied by definite pneumatic actions, and that those actions, many of which are insensible to ordinary observation, are capable of being recorded.—*A Paper read before the Royal Society, April* 16, 1874.

REVIEWS.

MENTAL PHYSIOLOGY.*

IT cannot be doubted that the study of the mind, or, in other words, the pursuit of mental physiology, is at once the most difficult and attractive study within the wide range of scientific subjects. Its difficulty results from the very nature of the subject, for it has within its grasp the important questions—Is the mind exclusively the result of the merely physical operations of brain-substance, or is there another and more mysterious agent engaged in the elaboration of thought? These are grave questions, and ones which unhappily cannot be decided positively at present, nor in all probability *in futuro*. Still it is possible to arrive, from a mass of evidence, at some sort of conclusion; and we are aware that certain men of considerable mental powers have arrived at the decision that mind is simply brain-power, *i. e.* the result of physical or chemical changes in the substance of the tissue, and that there is nothing whatever present in the shape of a soul or spirit. That, however, is not the view propounded by Dr. Carpenter, who, so far as we can perceive, believes unquestionably in the existence of a soul, but endeavours to explain most mental phenomena by ordinary physiological laws. The difficulty of doing for mental questions what is a comparatively easy matter in the case of the ordinary organs of the body, is admitted on all hands; and indeed this purely physiological mode of treatment is a method which the great metaphysician, Sir W. Hamilton, never for a moment attempted, and which we may say has not been tried on any extensive scale till Dr. Carpenter took the matter in hand. And indeed it would have been difficult for any ordinary student of mental operations to attempt, for he who would endeavour to solve the various questions it involves must possess an intimate acquaintance, not only with the laws laid down by the metaphysical schools, but must also have given serious attention to the physiology of the mind of the healthy and the insane. We cannot say that Dr. Carpenter has satisfied his readers on the question of the existence of a soul, for we ourselves fail to be convinced by any physical facts adduced by him in this part of the testimony. But we cannot help expressing our admiration of the cogency of his arguments—their number, power, and immense variety—in proving that the several psychical

* "Principles of Mental Physiology, with their Application to the Training and Discipline of the Mind, and the Study of its Morbid Phenomena." By W. B. Carpenter, M.D., F.R.S. London: Henry S. King & Co., 1874.

endowments of man are purely and simply physiological phenomena. Indeed, he has made his book one full of interest to even an ordinary reader, for the number of instances he has recorded of mental peculiarity in every chapter of his volume is simply wonderful. He has given such a myriad of cases in point on every question raised in the work, that in some two or three instances we are compelled to pause ere we place implicit reliance on the testimony adduced.

This treatise, which includes over seven hundred pages, is divided into two sections, the one including the facts of general philosophy, the other devoted to more special branches of the subject. The chapters in the first division, which are in some cases brim-full of reliable anecdotes, are on the general relations between mind and body; on the nervous system and its functions; on attention, sensation, perception, and instinct; ideation and ideo-motor action; the emotions, habit, and the will. In each of these several chapters, some of which are enlarged from his treatise on "Human Philosophy," Dr. Carpenter gives ample details which go to show the operation of the particular faculty with which he is dealing, and he proves that it is exclusively the result of certain operations of the mind. One wishes that he had space to give the author's arguments and his mode of drawing some of his conclusions, but it is impossible to quote. We must, then, suffice ourselves with the statement that in many cases the reasoning is almost syllogistic, while the abundance of instances brought forward, and Dr. Carpenter's well-known excellent style of composition, unite to make the reading of the book a pleasure instead of a task.

In the second part, that which is devoted to special physiology, the following subjects are dealt with:—Memory, common sense; imagination, unconscious cerebration; reverie, abstraction, electro-biology; sleep, dreaming, and somnambulism; mesmerism and spiritualism; intoxication and delirium; insanity; influence of mental states on the organic functions; mind and will in nature; and, lastly, an appendix containing an account of Dr. Ferrier's experimental researches on the brain. Now, all these subjects are treated in the same exhaustive and absorbing style as those we have referred to already. And of all we think the chapter which deals with the question of spiritualism and mesmerism at once the most interesting and important. Dr. Carpenter takes the middle course in these matters; that is, he neither scoffs at both nor believes in either. He shows by the most ample cases that mesmerism is an unquestionable influence which one mind is able to exert over another, and he argues that it is capable of being beneficially exerted in certain cases, as we know ourselves that it has been; while he as distinctly opposes the so-called spiritualism as one of the most glaring humbugs in existence. He gives his own opinion of various *séances*, and enumerates abundant examples both from his own experience and that of others, to show the abominable tendency of so-called spirituality. He mentions Mr. Wallace as a remarkable instance of a clever man being so completely led away in his belief on this subject. And, indeed, one cannot but regret that so able and genial a man as Mr. Wallace undoubtedly is should be brought among the crowd of simple people who are unquestionably deceived by those charlatans the so-called spiritualists.

The author's observations on table-turning are also of intense interest, for he does not simply give his own opinions on the subject, but he quotes a number of passages—many pages of them—from the several writers on the subject, and then he shows the utter absurdity of the propositions laid down. Those who are interested in this subject cannot do anything better than read Dr. Carpenter's treatise, for therein they will find abundant testimony from both English and American writers on the subject of table-turning, while they will also find the assertions of the spiritualists examined in a perfectly fair manner, and shown to be all of them perfectly worthless as scientific statements. Indeed, we have never known a case of belief in table-turning which was not satisfactorily exposed and done away with when Professor Faraday's apparatus was employed, and the supposed inaction demonstrated to be positive and active movement.

In every other of the chapters of this book the author is not less lucid in his mode of exposing error, nor less exact in his display of facts and arguments tending to the elucidation of complex mental phenomena. Indeed, if there is an error anywhere, it is in the abundant examples which his writing affords of views which few scientific men would take upon themselves to refute. But in these lies a large amount of interest, and by taking them in hand Dr. Carpenter has, one would think, satisfied even the most bitter and least learned of his opponents. In giving Dr. Ferrier's views in an appendix at the end of the volume, we do not know that he has done wisely, for after all Dr. Carpenter's book is one addressed to the general world, and Dr. Ferrier's experiments are only those which, in the present phase of the subject, are adapted to such a place as the "Philosophical Transactions." And we say this not from any want of admiration for the originality of Dr. Ferrier's most important investigations, but from the view that his results are as yet but incompletely founded, and that they must be repeated many times before they are sufficiently admitted to allow of the conclusions which have been drawn from them being firmly established. However, this is a matter of very slight importance, and it can in no way affect our criticism of Dr. Carpenter's essay, which we consider in every respect worthy of the reputation of its author.

DARWIN'S CORAL REEFS.*

WE regret very much being unable to offer a full notice of Mr. Darwin's last work, as it did not reach us till towards the last week in June. We must therefore content ourselves with a very brief account of this, the second edition of a most remarkable essay which first made its appearance—before Mr. Darwin was known to the general public—in the year 1842; reserving till our next number a more full account of this,

---

* "The Structure and Distribution of Coral Reefs." By Charles Darwin, M.A., F.R.S. With three Plates. 2nd edition, revised. London: Smith, Elder, & Co., 1874.

the first in more senses than one, English account of the interesting geological facts concerning the growth of coral-reefs. There is, however, one point on which we may observe that the book has especial importance, and that is with reference to the valuable work which has been done within the last few years on the American continent by Professor Dana. It is the more worthy of note because Professor Dana, strange to say, accepts views which are in point of fact diametrically opposed to those advanced by Mr. Darwin. In this book, however, Mr. Darwin endeavours to prove, and we think most satisfactorily so—though we shall dwell on this subject in our next review—that his own views are in the main correct. And he cites the evidence of the late Professor J. Beete Jukes in his favour, for the latter says, in his account of the voyage of H.M.S. *Fly*:—"After seeing much of the Great Barrier Reefs, and reflecting much upon them, and trying if it were possible by any means to evade the conclusions to which Mr. Darwin has come, I cannot help adding that his hypothesis is perfectly satisfactory to my mind, and rises beyond a mere hypothesis into the true theory of coral reefs." But then, on the other hand, there is the evidence of Chamisso, which is distinctly opposite. However, we shall see in our next notice towards whom the mass of testimony predominates. It will suffice here to point out that Mr. Darwin gives an abundance of evidence, much of it quite new, bearing on this important question; and that it is in his usually happy style, which for terseness and clearness are almost unparalleled in the English language. There is, too, another quality which it would be unjust not to indicate, and that is the manner in which the author deals with his opponents — it is invariably with kindness and consideration. Every one of the points is pushed even more than they would be by the writer himself; and wherever they appear to outweigh Mr. Darwin's evidence, they are constantly and fully admitted. We have, then, said enough to show the quality of the book before us; we regret to have to put off our criticism of it for so long a period.

LARDNER'S HYDROSTATICS.*

ONE had almost thought that Lardner's books, though they were excellent at the date of their publication, could hardly have been produced at this date with much chance of success. However, we fancy that in issuing a new edition the publishers of the present volume have not been unwise in their selection of an editor, and therefore, from this fact alone, we have reason to imagine that the book has been brought up to date. And this is really the case; so that we may say that the editor's labours have not been thrown away, but, on the contrary, that they have produced a

* "Handbook of Natural Philosophy." By Dionysius Lardner, D.C.L. "Hydrostatics and Pneumatics," new edition, the greater part re-written by Benjamin Loewy, F.R.A.S., Lecturer on Physics in University College. Lockwood & Co., 1874.

volume which is in every way far superior to what we had imagined would have been the case. We confess that we have been agreeably disappointed, and that the book is not by any means so backward as we had imagined. We think the editor has been very wise in omitting the subject of heat, which originally occupied part of this volume. We think, too, that he has not been unwise in his introduction of a number of examples which can be readily worked out by the student who has even an elementary knowledge of algebra and geometry. This, of course, makes the book more valuable to the student who means to go in for such an examination as that of matriculation at the London University. Indeed the work was absolutely useless to such a man before, for its examples were of the simplest possible kind. Still, in many parts it is defective; that, for example, on flying machines, contains little that is not to be found in the old edition, while the amount of work done both in this country and abroad on this particular subject is indeed both vast and interesting. In type and illustrations this work is, as is usual with the so-called Lardner's books, excellent, and the amount of new matter, including the excellent examples at the end, is very considerable. We do not think, however, that a single illustration is to be found throughout its pages which did not exist in the older edition.

A BOOK ON ROSES.*

MR. SHIRLEY HIBBERD is now so well known as a writer on the subjects of the garden and the greenhouse, that the mere announcement of a new work or a new edition from his pen is sufficient to bring ample readers. The present work is a newly edited account of those various roses which, with little difficulty, may be brought up by all who are in possession of the merest shadow of a greenhouse. But all of the book is not devoted to these, for a very considerable portion of it is occupied by a description of the various modes of grafting, bedding and propagating roses: such, for example, as the dwarf rose, the climbing rose, the pillar rose, the fairy rose, the yellow rose, and the wilderness rose. Then several of the author's chapters have to do with such questions as stocks for roses, roses under glass, roses in town gardens, roses for decorations, the enemies of the roses, and on buying new roses, or sending roses by rail or post, the curiosities of rose growing, the rose show; lastly, reminders of monthly work. Indeed there is no form of information which the amateur rose lover requires that will not be found in Mr. Hibberd's work; furthermore the author possesses a lively style, so that the reader is prevented from falling asleep over his pages. The illustrations to the volume are partly woodcuts and partly coloured lithographs; these latter are double-page, and represent a single flower natural size. In some cases they are only fair; but in others—the yellow roses, for example—they are really exquisitely rendered.

---

* "The Amateur's Rose Book; comprising the Cultivation of the Rose in the Open Ground and under Glass," &c., &c. By Shirley Hibberd. A New Edition. London: Groombridge, 1874.

LONGMAN'S TEXT-BOOKS.*

THESE text-books continue to be published on every conceivable subject; and so far as we have yet seen they are entrusted to writers who are thoroughly qualified to undertake the task, *i.e.* they are men who thoroughly understand the subject they have taken in hand, and, still more important, are capable of teaching the elements of the branch of knowledge on which they write, with clearness and terseness of style. We therefore think that considerable credit is due to Professor Goodeve for the skill he has shown in merely choosing his writers. In the present instance we have a book which he himself has written, and we may express our extreme satisfaction with it, for the author has endeavoured not merely to convey fact to his readers, but to convey it in such a manner that principle is involved in every description. We note also that Mr. Goodeve has endeavoured as far as possible to point out to the student the relation borne in each case by heat to mere mechanical action. This is a point of very great importance. It is of interest to know that the book contains an outline of one part of Mr. Goodeve's lectures at the School of Mines, for it shows us that the teaching of at least this portion of science is thorough and complete. We cannot do more than give the heads of the different subjects which the writer has taken it on himself to touch upon, though we should have wished to make one or two remarks *apropos* of his observations on Sir J. Whitworth's experiments on the small-bore rifle. The subjects treated of are, in the widest terms, as follows, each heading being resolved into an immense number of separate branches, viz. first, introduction on the relation of force and matter, and the different laws of force; then the following chapters:—The parallelogram of force, and its nature on work and friction; on the centre of gravity; on some of the mechanical powers; on the equilibrium and pressure of fluids; on the equilibrium and pressure of gases; on pumps; on the hydraulic press and hydraulic cranes; on motion in one plane; on circular motion; on girder beams and bridges; on strength of tubes and the catenary; and lastly on some mechanical inventions. On all these questions the author speaks, and speaks to the point.

The other volume is a manual of qualitative chemical analysis, which is a book excellent in design, but hardly so thorough in its conception as the mechanical work. Still, it is an admirable introduction to the subject on which it treats, and the authors seem to have taken care to point out every possible accident to which the student is liable. The only fault we have to find is as to its length. It appears as though the authors fancied a student perfectly ignorant of chemistry commencing the study of the practical division. If so, they have made a mistake, for we do not consider it at all probable that anyone would be likely to attempt gaining a knowledge of practical chemistry before he had been initiated in the elements of at least the

* "Principles of Mechanics." By T. M. Goodeve, M.A., Barrister-at-Law, Lecturer on Applied Mechanics at the School of Mines. London: Longmans, 1874.

"Qualitative Chemical Analysis and Laboratory Practice." By T. E. Thorpe, Ph.D., F.R.S.E., Professor of Chemistry in Andersonian University, Glasgow, and M. M. P. Muir, F.R.S.E. London: Longmans, 1874.

inorganic branch. If so, he would find the subject difficult in the extreme, and we do not see that, in such a case, the present volume would prove an easy introduction to the field of practical research. While, on the other hand, if the reader is already informed, so far as a six months' course is likely to achieve, we fancy that he will consider much of Messrs. Thorpe and Muir's book an idle task. Still, in all that relates to the ordinary laboratory work, the student will be fully recompensed by the perusal of this volume. For we find in it many of those little facts relative to the bending of tubes, the preparation of three-necked Florence flasks, and many other of those practical difficulties which the student eventually overcomes only through the aid of a good-natured chemical assistant. When to this we add an excellent system of qualitative analysis, we can say that the authors have done their work creditably. There is only one question we should like to ask, and that is whether the Fresenius mode of conducting the method of analysis be not more simple than the author's? It certainly is shorter by dividing the elements of analysis into three groups—the chlorohydric acid, sulphuretted hydrogen, and the sulphide of ammonium classes. In any case we are well pleased with these two works of Messrs. Longmans.

LITTLE DINNERS.*

ALTHOUGH a volume with the title of the present one hardly deserves a place in a Science Review, yet a cookery book, when well done, is really a work in which all men, whether *savans* or not, must feel an interest. And so it is, most unquestionably, in the present case. The book now before us is not one of the ordinary kind, whose instructions about the simplest dinner would be simply extravagant for anyone whose income was less than a thousand a-year. On the contrary, it is addressed to those who have been compelled to give up the cod and oyster-sauce, and the time-honoured saddle of mutton, as entirely beyond their income. The authoress has endeavoured to lay before her readers a very valuable variety of dishes of all kinds; but it is of importance to bear in mind distinctly her remarks on the subject of dealing with the butcher and fishmonger, for these are of the utmost importance, more especially as we are aware how very universally they are neglected, and that by those who most need them. She says, "so far from there being any real difficulty in procuring the *morceaux* required for her little dishes in the following *menus*, they are precisely those which may be most easily and cheaply obtained. But it will not do, if economy is an object, to order them from the butcher or fishmonger. Ten to one if they will send the weight or cut asked for, and ten to one also if any other will answer. The Spanish proverb, 'He who wants a thing goes for it, he who would miss a thing sends for it,' should be borne in mind by all housewives and cooks. True artists are always most careful about the kind and quality of the material they use, and *it is only by going to market and choosing for yourself that you will get the right thing.* The system of sending for

---

* "Little Dinners; how to Serve them with Elegance and Economy." By Mary Hooper, Professor of Domestic Economy in the Crystal Palace School of Art. London: Henry S. King & Co., 1874.

orders is unknown in France. Everybody goes to market there, and here lies another secret of the national success in cookery." If, as we hope, our author's advice may be universally followed, we doubt not that a great deal will be effected towards the much-desired end. But it is her method of selecting dishes that we have especially to commend. For we venture to assert that the reader who has any experience will travel through her volume without finding half-a-dozen dishes which may not be readily prepared for anyone whose income is even 200*l.* a-year. And again, it is necessary to give the authoress credit for her selection of dishes. Indeed, in this respect her book is a peculiar exception; it would almost seem to have been prepared by a Frenchwoman. Throughout her *menus* are arranged with a skill and a regard for good taste which is not often to be found in this country among writers for the kitchen. In her general remarks on the subject, the author shows, by a few telling examples, how sadly our system of cooking fails. We need only mention two or three. For instance, there is the mode of cooking vegetable-marrow by cutting it into slices, a mode which absolutely destroys its flavour; the same thing may be said about onions. Again, in cooking fish, soles for example, there is complete destruction of flavour by removing the skin, instead of simply scraping them; and again, much fish is injured by an absence of vinegar. In the matter of roasting, the authoress advises the employment of a Dutch-oven, made by Burton, of Oxford Street. She says it is infinitely less expensive than a fire, and that "any of the joints given in the bills of fare may be roasted in this oven with a mere handful of fire, and will be found quite as nice as if done on the spit." With regard to this question we know some of our readers will express a doubt. But then the author says that ventilation must be attended to, and she says that, in this respect, "the plan of the first patentee, Flavel, of Leamington, has never been surpassed." She also recommends the use of gas-stoves, which, we believe, the Crystal Palace Company are now largely introducing among the surrounding inhabitants. We wish we had space to give some of the writer's observations on the use of the stew-pan and the frying-pan, for they are eminently practical, and above all are to the point. So is it with regard to her remarks on the general management of the table, the use of wines, and so forth. Indeed, we are well pleased with the volume, and we trust our readers will excuse the length to which our remarks have extended *apropos* of such a subject.

THE STRUCTURE OF THE EYE.*

AN interesting little book is this, and we can quite imagine that it formed, when delivered, a very capital though sketchy lecture. Yet we do not see what claim it has to publication. Still, if it pleases the author, we do not know whom we are to blame. It is, of course, a most outlinear account—indeed in many cases its sketches are too slight—of the various description of eyes to be met with throughout the animal kingdom; and it has, in addition, a few remarks on allied subjects, as, for instance, M. Pouchet's observations on the changes of colour in fish and crustacea. It is amply

* "A Popular Description of the Human Eye, with Remarks on the Eyes of Inferior Animals." By W. Whalley, M.R.C.S.E. London: Churchill, 1874.

illustrated; but unfortunately the author, evidently unacquainted with the subject, has not made use of those engravings which are most uncommon. Indeed, on the contrary, the greatest number of the cuts have been abundantly seen by anyone who has a knowledge of physiology or comparative anatomy. Still the subject is most interesting, and the author's observations are accurate in most cases, and are eloquently put forward, so that altogether the little volume is not an unwelcome one.

A ZOOLOGICAL DICTIONARY.*

A NEW edition of "Maunder's Treasury of Natural History" is now before us, and demands a few observations at our hands. And in the first place we should have condemned the work if it had been in its first edition; but as it now comes forth under the charge of a different writer from the author, our observations are of course of a different nature. And this is for the following reason: the book being published, must be *sold*, in order to defray its expenses; and therefore we think the publishers are not so much to blame in giving the work to a responsible editor and asking him to bring out a new edition. But if we had to do with Mr. Maunder himself, we certainly should indulge in a little anger at his expense. For what, we would ask him, have you produced such a volume? It has been expensive to print, difficult to illustrate, and, above all, it cannot be well done by a single writer, while at the same time we may complain that its type is so small that even a brief reference to it is painful to the eyes; while finally, its figures are ridiculously small. Just, for instance, imagine an elephant which barely measures an inch and a-half in length, and you get some conception of the plan of illustration. Still, with these disadvantages, we have something on the other side. For example, the illustrations are abundant, the matter extends over more than 800 pages, and the articles, though many of them are brief, are withal very good in point of matter. The present editor has done his work creditably, especially when we consider that he had to deal with a stereotyped volume. He has introduced a considerable quantity of entirely novel matter into the pages.

THE TRANSIT OF VENUS.†

THE work which Mr. Proctor has just issued to the public is infinitely the most important of his numerous treatises, save and except his very earlier essays. We say this more especially of the second part of the present

---

* "The Treasury of Natural History; a Popular Dictionary of Zoology." By S. Maunder. Revised and corrected, with an extra Supplement, by E. W. H. Holdsworth, F.L.S., F.Z.S. With 900 Woodcuts. London: Longmans, 1874.

† "The Universe and the Coming Transits." By Richard A. Proctor, B.A., formerly Secretary to the Royal Astronomical Society. London: Longmans, 1874.

volume, that particularly relating to the Transit of Venus. Unquestionably the various essays which occupy the first part of the volume are of a most worthy type, and are, as are most of the author's articles, written in the clearest style, and with an amount of knowledge of his subject which unfortunately is very seldom at hand when information for the public is required. They relate to the so-called star-streams, and they deal at length with the whole subject. These essays are reprints from various papers which Mr. Proctor has communicated within the past twelve months or so to the several literary and scientific journals, and they contain an able expression, and a full one, of the author's different views on the subject of sidereal space. The chapters of the greatest interest—at least from a purely speculative standpoint—appear to us to be those which he has entitled "A New Theory of the Universe." We cannot, in the very limited space before us, attempt to give a survey of his views on this point, or to indicate what appears to us to be erroneous in some of his ideas, but we also cannot let it pass without a faint expression of our admiration. Not without interest, too, are the observations on his remarkable chart, which we sometime since noticed in these pages; and several other chapters might likewise with perfect fairness be noticed. One thing we should commend to Mr. Proctor's notice, and that is the propriety of making a summary of each chapter, so that the student would have a ready means of seeing what he had been aiming at throughout the text. Such a scheme would be of advantage to the earnest reader, and it would be of infinite service to the reviewer, who would thus, in many cases where he has read the same matter before, be saved the trouble of going once more over the same field.

The part of Mr. Proctor's volume which possesses most interest to the more purely scientific man is that which relates to the transit which is rapidly approaching. And this is because in style it is entirely different to the remainder of the work, being in point of fact so many pages reprinted from the "Proceedings of the Royal Astronomical Society." On this account they do not possess that stream of continuity which, if it could have been present, would be undoubtedly desirable. But they possess a solid mass of fact, which the real student will read with pleasure. And we think this all the more because, among the papers that have been reproduced, is one from the Astronomer Royal criticising some of Mr. Proctor's work, and likewise the reply which the author felt called upon to make. This difference of opinion is of very great importance, and to the reader of Mr. Proctor's remarks it certainly seems that he alone is right. But there is, doubtless, so much to be urged upon the other side, that anyone who went fully into the matter would experience a greater difficulty about decision than a mere reader of the present volume. One thing is perfectly certain—that no fitter person could be chosen to command at some station from which the coming transit could be observed than Mr. R. A. Proctor; and we trust that if he does not undertake the observation of Venus in this case, it will be alone from some motive relative to health. Assuredly he has shown himself deeply interested in the work before the astronomers of this country and America, and we shall certainly be excessively surprised if he does not go out on some of the expeditions.

A TALE OF AGES.*

POETRY is surely not Science, yet there are some who expect that *in futuro* a race of pure poetical *savans* will arise who will work veritable wonders in the Poet-world. Perhaps Mr. Richardson is, as it were, the troubadour of this future race. His lines are fairly written; and though in metre they are sadly deficient, in the statement of fact they are as accurate as alone a geologist could make them. Their nature is briefly this: First there is an introduction, and then follow a series of geological sketches in verse, viz. the respective ages of chaos, water, fire, forests, ice, and man. In the Appendix Mr. Richardson explains the technicalities which he employs, so that his verse may be intelligible, not merely to the student of natural science, but to the generally educated reader. It is, after all, to Edinburgh men chiefly that the poem will be of interest, for the geological observations extend exclusively to the country surrounding. We think that the author was not unwise in his selection of the spot; but he cannot complain if more Southern readers, unless they are well-read geologists, fail in many cases to see his points. In many instances, too, his rhyme is somewhat questionable. Surely no one ever pronounced *eozoon* in such a manner as one is compelled to do in the following line:—

> "What first breathed life with God?—embalmed in stone
> Most time-worn, the Laurentian eozoon."

We have said enough to explain what we think of the effort made by the author, and the reader must now take up the book and judge of it for himself.

PHYSIOLOGY FOR PRACTICAL USE.†

WE don't know how it is, but unfortunately people generally do not much like books of this kind. To be sure a certain number read them with avidity until, like the old gentleman of former days, they timidly rise from their chairs lest they might fall to pieces from the shock. But we do not believe that the mass of people—we mean people who read scientific books—care to peruse a physiological essay. We do not think, therefore, that Mr. Hinton's labours will meet with the success they unquestionably deserve; for though he has not brought out his science as one who had gone into the subject might have done, and though he has left such a number of important physiological questions untouched that we almost wonder at the title he has chosen, still what he has done he has done well for the public, for the style of composition is really out of the common. Indeed, had not the editor stated the contrary in the preface, we should have imagined the several articles were from the same pen. It is our intention,

---

* "A Tale of Ages; being a Description of some of the Geological and Historical Changes which have occurred in the Neighbourhood of Edinburgh." By R. Richardson, Secretary to the Geological Society of Edinburgh. Edinburgh: Edmonston & Douglas, 1874.

† "Physiology for Practical Use." By various Writers. Edited by James Hinton. Two vols. London: Henry S. King & Co., 1874.

in noticing the book, to confine our attention to one chapter alone, as it is really of most importance, and as the editor puts it forward as entirely new, the other sections of the work being merely reprints. This chapter is that upon the Physiology of Alcohol, and we may say of it at once that it is a fair and tolerably impartial, if a little superficial, account of the bearings of the question, whether alcohol is a poison or a food. In his account of the various species of alcoholic drink the author is perfectly correct in his statement of the nature of the three classes of alcohol—that for the scent manufacturer, that for the apothecary, and that for ordinary consumption; that form which is sent out to be consumed among savage tribes we know nothing of. But in his statement as to the effect which alcohol produces in experiments upon the frog's foot, &c., we differ from him totally. We are quite aware that such statements have been made by scientific men: that, for example, weak spirits cause an increase in the circulation of frogs' feet, and strong spirits absolutely abolish it, and totally destroy the texture of the part. Such experiments are totally valueless as practical tests. We know, from the result of many experiments on the subject, that the effects of alcohol on the frog's foot are most uncertain, and that often it requires strong spirits of wine to produce such an effect as that described. Besides, when it is removed the part quickly recovers. And again, why should we calculate that the same thing would occur in the circulation of a warm-blooded animal as happens in a cold-blooded one? We do not object to such experiments; but what we do object to is the wholesale fashion in which some too quick-witted individual proceeds to draw most mathematical conclusions from these results. Again, the author can clearly not have been a medical man, or else he must have been wonderfully ignorant when he wrote such a sentence as the following one in connection with the absorption of alcohol. He says, "*It is not absorbed through the same channels whereby the food gains access to the blood; the villi do not take it up, the lacteals contain no appreciable trace of it.*" Surely no medical man would have fallen into so dire a mistake as to imagine that all absorption save that of alcohol takes place through the lacteals. Why, the thing is absurd on the very face of it. Allow us to state that an exceptional quantity of matter, merely the fatty materials, are taken up through the lacteals; and all really nitrogenous matter, your flesh meat, sugar, bread and potatoes, pass not through the lacteals, but by the very same channels through which the alcohol passes —by the blood-vessels that surround the stomach.

Having pointed out so great an error, it is of course plain that the author has not been medically educated, and therefore we must receive many other of his statements with caution. But we entirely agree with him in his general result, that alcohol should be taken in moderation, and that less should be indulged in by plethoric and excitable persons than by lymphatic temperaments, as they are called. In all that relates to the general effects of alcoholic stimulants, we are entirely in unison with the author of this chapter; and it is only when he becomes physiological that we are compelled to urge disagreement. For instance, he says that part of the alcohol is carried off; to this we of course agree; but when he tells us it is by the alimentary canal, we are at a loss to understand him; if he said by the kidneys and skin, we could at once have admitted his assertion. But these are very small faults to have to find with an article which on the whole is really very good indeed. We may, in conclusion, give the headings of the several other chapters, which are, we believe, reprints from different journals. These are as follows:—The brain and its servants; the faculty of hearing; the eye and sight; the sense of smell; the sense of taste; digestion; the skin; corpulence; the bath; the sense of touch; notes on pain; respiration; taking cold; influenza; headache; sleep; sleeplessness; ventilation; the liver and its diseases; muscular motion; occupation and health; and lastly, training and gymnastics.

FOOD AND DIETETICS.*

ONE would imagine that no book would be more popular than a work which treated fully and fairly of the different varieties of food, and of their action on the human body. Yet it is strange that, as a rule, books on food, for some reason or other, are by no means popular, and are read by very few, even of those medical men whom they especially concern. However, we trust that in the present instance this rule will not hold good, for the author has been at pains to introduce into this volume everything in the faintest degree of scientific value which touches on his subject; and he has not only done so, but he has taken care that only the most recent views on the physiology of digestion have found a place in his pages. The book is a very vast one, of course, from the extended nature of the treatment adopted in it, for it covers nearly 550 pages of large octavo, and of comparatively small type. Yet we do not think the author has wasted space; for throughout he appears to have adopted a habit of condensation, and his style is remarkably good, and, above all things, clear and decisive. The part which seems to us of greatest interest is that in which he discusses the physiological question regarding the exact nature of the waste of the human body, and hence the nature of the food which we should take in compensation for the loss caused by vital action; in other words, to repair our bodies. Of course many other points are discussed in the work, as for instance the following: —The classification of foods; animal and vegetable foods, and beverages; the preservation of food; principles of dietetics; practical dietetics; therapeutic dietetics; and hospital dietaries. But it is to the physiologico-chemical part we would alone refer. In discussing this part of the subject the author has been, of course, from his medical and chemical knowledge, quite at home, and he has done his work well. He has brought before the reader an amount of information which has till now lain in the proceedings and journals of English and foreign societies. So that, in point of fact, his views, or those he adopts, will strike the great majority of his readers as being absolutely novel. We would especially refer to those remarkably accurate observations and careful experiments of Messrs. Fick and Wislicenus, in Switzerland; Flint, of New York; Parkes, Lawes, and Gilbert, of England. In dealing with the results arrived at by each of these autho-

* "A Treatise on Food and Dietetics, physiologically and therapeutically considered." By F. W. Parry, M.D., F.R.S. London: Churchill, 1874.

rities, the author quotes amply from their works cases which support their views, and records experiments which have been made on men for the purpose of ascertaining the truth of the theories which they set forth. The great point which all alike go to prove is the comparatively small value of nitrogenous food in ordinary diet. We say comparative, for it will be remembered by those who have studied the subject that, till of late years, nitrogen was considered to be the chief element that was used up in muscular exertion. But now results show a different face. It seems, from the experiments recorded in the present volume, that the muscles do not become wasted during action to the extent that was imagined, and hence that nitrogenous food is not at all so much required for the employer of muscle—the common labourer. Of course it would be out of our power to give the facts on which Dr. Parry founds these conclusions; those we must leave for the reader to discover himself; but we promise him, if he have physiological taste, an ample return for the money expended in the purchase of this volume.

THE PROPER MOTION OF THE FIXED STARS.*

It is not very long since we had a book from this author on the subject of the Glacial Period, and we fancy that the writer to whom it was entrusted decided to say nothing in answer to it, considering it a work founded on erroneous ideas. Unfortunately, we are compelled to come to a conclusion of a like kind; and although the author is excessively severe on one of his former critics, who compared the motion of the earth to that of a top, we certainly think the critic's argument a sound one, and Colonel Drayson's attack on it weak in the extreme. However, though we do not believe in the correctness of the author's conclusions, we may give them here as follows, and with them conclude our notice of the book:—(1.) The earth rotates on its axis in the same manner as at present taught in astronomy. (2.) It revolves round the sun along a plane making an angle of about 66° 32′ with the axis of diurnal rotation. (3.) The semi-axis of diurnal rotation traces out a circle on the heavens round the pole of the ecliptic, *but not round this pole as a centre*, the centre being 6° from the pole of the ecliptic. (4.) The earth's axis has a small elliptical movement round its mean position in about $18\frac{2}{3}$ years.

SMITHSONIAN REPORT.†

It is to be regretted that so much delay is made in regard to the publication of this report; the volume which is before us, being that which was issued late in the year 1873, comes to us in 1874, and yet really records

* "The Cause of the Supposed Proper Motion of the Fixed Stars, and an Explanation of the Apparent Acceleration of the Moon's Mean Motion; with other Geometrical Problems in Astronomy hitherto unsolved." By Lieut.-Col. Drayson, R.A., F.R.A.S. London: Chapman & Hall, 1874.

† "Annual Report of the Board of Regents of the Smithsonian Institution, for the Year 1871." Washington, U.S.A.: 1873.

the proceedings of the Institution for the year 1871. However, as our readers are especially interested only in the Appendix to the Report, we suppose they are uninterested in the period of publication. The papers in the present volume are attractive, and some of them have a high scientific value. The first is a memoir of Sir J. F. W. Herschel, by Mr. N. S. Dodge. This memoir runs to about twenty-four pages, but it is really one of the best and happiest essays we have ever read. The life of the great Herschel is most tellingly written, and we think that few who have gone through the first page will place the book down until they have completed it. The next paper is an address to the French Academy by M. Arago on the subject of Joseph Fourier. Dr. Odling's address to the Royal Institution on Professor Graham's scientific work is also an able article; but of course it is familiar to many of our readers. A capital paper is that by Professor Helmholtz on the "Relations of the Physical Sciences to Science in General." This was delivered before the University of Heidelberg, and is translated by Professor Kroch. Dr. Kornhuber, too, gives a good paper, which was originally read before the Vienna Society for the Diffusion of Useful Knowledge, on the subject of "Alternate Generation and Parthenogenesis in the Animal Kingdom." It is to be regretted that the author did not deal more fully with his subject. On the other hand, a writer who has given us, if anything, too long a paper, is Mr. E. De Forest, M.A., who gives us no less than 65 pages "On some Methods of Interpolation applicable to the Graduation of Irregular Series, such as Tables of Mortality, &c. &c." This paper is, however, of the utmost value and importance to actuaries, and to such we specially recommend it. Among other papers of interest in the volume we may refer to the following:—"The Language of the Dakota, or Sioux Indians;" "Indian Mounds near Fort Wadsworth, Dakota Territory;" "Instructions to Captain Hall, who was Commander of an Expedition to the North Pole," contains some very valuable advice to the naturalist, more especially in that part devoted to geology, which is by the late Professor Agassiz.

SHORT NOTICES.

Annual Record of Science and Industry for the Year 1873. Edited by S. F. Baird, with the assistance of Eminent Men of Science. New York: Harper, 1874. We are pleased to see that this work has decidedly improved. Indeed it now forms a very valuable record, which, of course, will not be of assistance to the specialist, but will prove an immense boon to the amateur in scientific matters. The notices of work done are almost invariably judiciously executed, and, so far as we can see, they include almost all the acts of general interest. The author has associated with him a number of eminent workers in the fields of science, and he has thus ensured the accuracy of his reports.

Divine Revelation and Pseudo-Science. An Essay. By R. G. Suckling Browne, B.D. London: Longmans, 1874. This is a book which we only notice in courtesy to the publishers. It is written by an aged person, who, with little knowledge of his subject, and infinitely less reasoning powers, attempts to do battle against the doctrine of evolution. Mr. Darwin's admirers can afford to smile at the author's efforts to demolish him.

Manuals of Elementary Science: Physiology, by F. Le Gros Clark, F.R.S.; and *Geology*, by T. G. Bonney, M.A., F.G.S. London: Society for Promoting Useful Knowledge. These are two little books intended, of course, for mere beginners, and are both very good. Mr. Bonney's is, in point of style, much better adapted to the class of readers it is intended for than Mr. Le Gros Clark's. Both books are, we think, singularly defective from their absence of illustration. We point this out because we think that, of all books, those intended for the young should be full of woodcuts.

Report on a Topographical Survey of the Adirondach Wilderness of New York. By V. Colvin. Albany: The Argus Company, U.S.A. This is an interesting account of a plan of engineering observations carried out by the author. Otherwise it is of little interest to English readers.

Annual Report of the Chief Signal Officer to the Secretary of War for the Year 1872. Washington: Government Printing Office, 1873. Here we have an extensive report on the condition in which telegraphy is in America. Those who are at all interested in meteorology will obtain much information relative to the subject, the machines employed, &c., &c., in this work.

The Year-book of Facts in Science and Art. By John Timbs. London: Lockwood & Co., 1874. This is by no means a book like the American work noticed above. It is full of glaring errors, and is most injudiciously edited. Still it is not without interesting matter, and it has a capital steel engraving of Professor Tyndall.

We have also received:—"The Annual Report of the Board of Public Education of the First School District of Pennsylvania," for the year ending 1872; "The Lives of Sir James Young Simpson and Michael Faraday," by the Religious Tract Society; "Botanical Tables for the Use of Students,' by E. B. Aveling, B.Sc. (Hamilton Adams, 1874), which are not bad, but are only fit for students who have learnt botany; "The Vacation," a Poem, by J. S. Nairne (Glasgow, 1874); "Spiritualism, and Why I Object to It," by the Rev. T. Ashcroft (London, Tweedie, 1874), a very able onslaught on spiritualism; "Hints for Health," by J. S. Stocker, M.D. (London, Churchill, 1874), consists of two useful lectures addressed to the people on the influence of air, water, food and wine on the system.

The following books will be noticed in our next number; they arrived too late for review in the pressent issue:—"A Manual of Botany," by Robert Brown, M.A., Ph.D. (Blackwood & Co., 1874); "Geological Survey of Missouri Iron Ores and Coal Fields," by Raphael Pumpelly (New York, Bien, 1873); "Reports on the Geological Survey of the State of Missouri, 1855 to 1871," by J. C. Broadhead, F. B. Meek, and B. F. Shumard (Jefferson City, Regun, 1873); "Geological Survey of Victoria, Report of Progress," by R. B. Smith, F.G.S. (Melbourne, Freres, 1875); "Reports of the United States Geological Survey of the Territories," for the years 1867–9, under the Department of the Interior (Washington Government Printing Office, 1873).

SCIENTIFIC SUMMARY.

ASTRONOMY.

THE *Approaching Transit of Venus.*—We describe elsewhere the plans which have now been adopted for observing the approaching transit of Venus. It is satisfactory to note that the part which England is to take is far worthier of her position in the scientific world than that originally proposed for her. Instead of five stations, she will now occupy no less than nine. Instead of applying only Delisle's method, she will apply not only both Halley's and Delisle's methods, but the photographic and the direct methods. The concessions which have been made, in fact, by the officials at our National Observatory are ample in themselves, and honourable to those who had acquired, by long tradition, what might almost be termed a vested interest in inflexibility of demeanour. It is a point of small importance that the change of plan has been effected silently, and as it were surreptitiously. So long as Halley's method is to be applied we can readily overlook the vague manner in which its advantages have been spoken of. Now that the value of the once overlooked North Indian region is fully recognised, we need not count the steps by which a plan for applying photography only at Delhi gradually became metamorphosed into a decision to apply all the available methods at Peshawur, the very course pointed out by Mr. Proctor in 1869. One modification only of this kind requires to be here noticed. It is commonly stated that there are to be two English stations at Kerguelen Land, and it is in this manner that the Greenwich statement just published presents the matter. It should be known, however, that the head of the Kerguelen party has instructions to put one of the parties on Macdonald, or Heard Island, if possible; in other words, another concession has been made by Greenwich in secret. Sir George Airy "does good by stealth."

The American and European Methods of Photographing the Transit.—Some attention has recently been directed to the question of the relative advantages presented by the two methods of photographing the transit of Venus adopted respectively by the American and by the European observers. The managers of the American scheme of observations consider that the method which has so long been adopted at Kew, however excellent for securing beautiful sun-pictures, is not trustworthy enough for recording so delicate a phenomenon as the transit of Venus. In the Kew method the focal image is optically enlarged, and although the amount of enlargement—that is, the scale of the sun-pictures—is theoretically calculable, practical difficulties are involved which render the scale so determined not strictly reliable. Accordingly, the best estimate of scale, when this method is employed, may be regarded as derived from the picture itself, that is, from the measurement of the photographic disc. Inasmuch, however, as this disc is enlarged by photographic irradiation, it is manifest that an element of uncertainty is introduced, the amount of irradiation being variable under varying conditions. In the American method the focal image is used to give the photographic picture, and thus the scale of the picture is known at once, since it depends merely on the focal length of the object-glass. The centre of the photographic solar disc is determinable with great accuracy, no matter how great or how small the extent of photographic irradiation may be; so, also, the centre of the disc of Venus is accurately determinable, and hence in this method the distance of Venus from the sun's centre can be determined independently of the photographic peculiarities of the picture. The American astronomers maintain that their method is very much more trustworthy than the other, and their opinion would appear to be confirmed by the experiments on photographic irradiation which led Lord Lindsay to adopt Professor Winlock's method in preference to Dr. De la Rue's.

Observations of the Transit at Cape Town.—Mr. Proctor has pointed out that observations of the middle of the transit at Cape Town—directly, and by the two photographic methods just described—would have great value, and might—if weather proved bad at the few other southern stations—become of paramount importance. Venus will be thrown farther from the sun's centre, as seen at Cape Town, than at any other station. It is to be hoped that no opportunity really available for the effective observation of the transit will be thrown away; and though we do not hear of any measures at Greenwich for employing this method, it is still possible that the authorities there are preparing to avail themselves of the suggestion.

Continued Observations of the Companion of Procyon.—Otto Struve, at the May meeting of the Astronomical Society, described the observations made at Poulkowa upon the small companion of Procyon; not the old and comparatively well-known companion, as some appear to suppose. After presenting the details, he sums up the result as follows:—

"If we take the mean values from my observations for the two years, we have

1873,	March 28	$d = 12''\cdot49$	$P = 87^\circ\cdot65$
1874,	April 10	$11\cdot67$	$96\cdot65$

or, after applying the systematic corrections:

1873,	March 28	$d = 12''\cdot49$	$P = 90^\circ\cdot24$
1874,	April 10	$11\cdot67$	$99\cdot60$

"The distance in the interval would, therefore, seem to have diminished by about $0''\cdot8$. But owing to the difficulty of the measures, the mean values themselves must be subject to such uncertainties that the reality of the apparent diminution must be considered doubtful. But in regard to the increase of the position-angle, there can scarcely be any doubt. Not only does the observed increase of $9^\circ\cdot5$ correspond to a considerable linear change of place, amounting to $2''\cdot0$, but the measures of direction are in themselves much easier, and more certain than those of distance.

"It is well known that Professor Auwers, as soon as he had received my observations of last year, repeated his investigations into the variable proper motion of Procyon, availing himself also of the observations of this star which have been made since 1862. From this he concluded that it was doubtful whether the object observed by me was really the sole body disturbing the proper motion of Procyon, but that the doubt would be removed if it appeared this spring that the position-angle had undergone an increase of from 9° to 10°. And this increase has really shown itself above in the most remarkable manner. I consider it, therefore, to be decisively established that the object I have observed is actually the companion whose existence has been theoretically proved by the calculations of Auwers, and hope that the astronomical world will rejoice with me in the triumph thus obtained for the labours of my honoured friend, and through them for our common science. In order to remove any exception that might be taken that the wished-for result had in any degree been itself the cause of the recognition, and affected the measurement of the place of so difficult an object, I will just remark that I had not looked again at the paper of Auwers in question since its first receipt last summer, and had totally forgotten the data of its criterion, and the mutual relation of the two stars. I did not again take it up until after I had succeeded in making the first observation, and the results of that paper were even less present to the mind of my assistant, Herr Lindemann, whose younger eye appears generally to have seen the companion even better than mine."

Determining the Solar Parallax by Observations of Juno at Opposition.—Lord Lindsay and Mr. Gill, in a recent number of the "Monthly Notices of the Astronomical Society," describes their plans for observing Juno in order to determine the sun's distance. "The method recommends itself," they remark, "not by a favourable factor of parallax such as is afforded by a transit of Venus or a favourable opposition of Mars, but by the extreme precision with which a minute point of light can be bisected as compared with that with which a web can be brought into contact with a disc. We are not aware that any results have been published of the application of the method, though there is no doubt that it is capable of great accuracy. It has appeared to us that if we select suitable stars, and observe the parallactic displacement of a minor planet relative to these stars by the Earth's rotation, we have a method of determining the solar parallax, which is free from the difficulties and disappointments attending observations in which co-operation is necessary, and where the most complete arrangements and perfect observation may be upset by unfavourable weather at the opposite station.

"The accuracy of the proposed method will depend —

"1. On the amount of displacement that can be measured.

"2. The accuracy with which the measures can be made.

"3. The number of nights on which the measures can be repeated, which will partly depend on the nights on which suitably situated stars of comparison can be found.

"The planet Juno at the opposition of 1874 appears to be very favourably situated for a trial of this method. Mauritius (the station where we observe the transit of Venus) is a suitable position, and the Repsold heliometre, with which the transit is to be observed, a suitable instrument. Juno, in opposition on November 5, 1874, has a horizontal parallax of 8″·7, and south declination of 3°·50′. If we suppose that we may begin to measure when the planet has an altitude of 20°, and continue till the altitude is 40°, and at setting begin to measure at an altitude of 40°, and observe till the planet's altitude is 20°: then for opposition we have—

Altitude.	Parallax in A.R.	Hour Angle.
°	″	h m
20	7·70	4 41
30	7·05	
40	6·14	3 15
	Time available for observation	1 26

"In other words, we shall be able to observe a displacement of about 14″, and have 86 minutes both at rising and setting during which we may make observations.

"The observations can be made with advantage for a month preceding and a month following opposition.

"It will not be possible to observe the planet at so large an hour angle in the early mornings a month before opposition, as the sun rises when the hour angle of the planet is about three hours and a half; nor similarly a month after opposition shall we be able to commence observation so early as is desirable, and moreover the horizontal parallax will be a little less. Notwithstanding this, for fifty nights an average parallactic displacement in A.R. of about 12″ can be obtained."

Variability of Red Stars.—Mr. Birmingham has communicated to the Astronomical Society some observations which appear to show that several of the stars in Schjellerup's well-known Catalogue of red stars are variable in brightness and in colour. He had already discussed the probable variability of No. 252 (which is now invisible). The others which he deals with are Nos. 98, 101, 74, 77, 63, 152, and 280. We quote the following remarks, as calculated to be of interest to observers :—

"I first searched for No. 90 on February 3, 1873, and failed to find a red star in, or near its position. I was equally unsuccessful on February 6 and 8, on September 23, and December 26; and on December 28 I wrote to Messrs. Dunkin and Lynn, of the Royal Observatory, requesting that it might be looked for. This was accordingly done, and Mr. Downing, who made the observation with the transit-circle, found, on January 3, that the exact position of 90 was occupied by a 7-magnitude colourless star, without even a tinge of red. It was afterwards identified as one of Flamsteed's stars, 44 *Camelopardali.* This star, which I knew, appeared to me rather less than the 7th magnitude and of a bluish-white colour. Schjellerup notes it as one of the stars in the Dorpat Catalogue, where it is rated at the 7th magnitude and marked *rubra*; so that it seems to be variable at least in colour, if not in magnitude.

"No. 101 is taken by Schjellerup from a note by Piazzi, who describes it as red, without stating the magnitude, and rather roughly gives its position as preceding by about 2′ another of his stars (No. 187) on the same parallel. I failed to find the red star on February 6, 1873, and was informed by Mr. Knott, to whom I communicated my failure, that he was equally unsuccessful on two previous occasions—on June 25 and August 3, 1866. However, on January 13 of the present year, I found a star in the position of 101, as well as I could identify it in a hurried observation. The passage of clouds did not permit me to examine it very closely, but it appeared about the 8·5 magnitude, and slightly tinged with red. I have not observed it since then; but I would suggest that it should be closely watched in its present excellent position for observation, when its very probable variability might be determined.

"In No. 74 I have remarked differences within the limit of one magnitude, with considerable changes of colour: Schjellerup thus notes it—*Bessel-roth, Cape Obs., vivid red*, 8.

"My observations of it were as follows:—

"1871, March 8. Pale red, 8.
"1872, Feb. 10. Orange, 8.
"1873, Feb. 2. Orange, 7·5 to 8. A *comes* 9·5; intense blue; 285°.
"1873, Dec. 20. Fine orange red, 7.
"1874, March 6. Fine orange red, 7.

"No. 77, described by Schjellerup as *roth*, 7·7, seems less than 8, with the palest possible red tinge.

"It may not be out of place here to refer to a very remarkable orange star which I found when looking for 77. On February 2, 1873, it appeared about the 7th magnitude, with a comes of the 11th; position 95°. On December 26 the large star seemed of the 6th magnitude. On February 3 it looked nearer the 5th, and on March 8 it appeared of the 6th magnitude again. Its roughly approximate place by equatoreal is R.A., $6^h\ 14^m\ 40^s$, Decl. $-2°\ 54'\ 30''$. Its small apparent changes may, indeed, be due to errors of estimation; but I consider it worth the attention of observers, as it is strikingly superior in depth of colour to very many of the stars in Schjellerup's Catalogue; and the fact that it was missed by previous observers of red and orange stars is strongly suggestive of variability. From recent measures, I find that the angular distance of the comes is 100″, and the position 99°.

"No. 63, unless I mistake its identity, presents an instance of complete change of colour. It appears as one of Schjellerup's own stars in the list, where it is marked '*roth*,' but to me it always shows a fine blue tint; while 51 *Orionis*, near it, is deeply orange.

"No. 152, which is described in the Catalogue as a red star of 8·5 magnitude, has been observed by me on several occasions as a fine orange star of the 6th magnitude.

"No. 280 furnishes an undoubted instance of change. It is catalogued as one of Argelander's stars of the 6th magnitude, and followed by a blue star on the same parallel. On April 18, 1873, I noted it as a good red, but no more than 7·5, and estimated the blue star at the 9th magnitude. On January 15, 1874, the red seemed only of the 8·5 magnitude, and it was considered even smaller by the Rev. Mr. Webb, on the 12th. On February 17, I thought the star had risen to the 7th magnitude, with a high colour."

New Comet.—The new comet discovered by Coggia has not presented any special telescopic features of interest up to the present time. Secchi has found that the nucleus presents a spectrum resembling that of carbon—a fact already observed in the case of Encke's comet, and Brorsen's.

Structure of the Solar Photosphere.—Mr. S. P. Langley communicates the results of his study of the sun's surface with a fine 13-inch refractor at the Allegheny Observatory, Pennsylvania. He has been successful in resolving the so-called rice-grains seen at Greenwich, by Huggins and by others, into minute components. "By taking advantage," he says, "of the brief and rare intervals of definition, which admit the use of high powers of such a telescope, I found the rice-grains resolved into an order of minute components, scarcely hitherto observed. These components then form *granules* (a word used by others as a synonym for 'rice-grains'). They are very minute bodies, present over the whole solar surface, faintly discernible in the faculæ, and in the penumbræ of the spots are expended into long filaments, whose aggregation forms the thatch-straws of Mr. Dawes, as the aggregation of granules forms the rice-grain. The latter term should be employed hereafter, I think, only so far as it may be necessary to recognise a tendency of these granules to unite in clusters of approximately uniform size. The granules are occasionally seen singly, more frequently united in clusters of from two or three, to ten or more, and by their degree of juxtaposition, and perhaps by their actual superposition, form the inequalities of brilliancy of the rice-grain noted by Mr. Huggins, and account for the irregular outline of the latter, which he has already remarked upon. With the largest apertures and powers, not only then do these brilliant bodies appear smaller, but from their apparent area is to be taken the minute dark spaces which it now appears intervene between their component parts." He infers, after careful consideration of the probable real magnitude of these objects, that the sun's light (which comes from them almost wholly) really proceeds from less than one-fifth of the sun's whole surface.

Contentions in the Astronomical Society.—The general meetings of this Society have lately been characterised by unpleasant contentions. In 1873 Colonel Strange, Professor Pritchard, and Mr. Lockyer, combined in an unsuccessful attempt to remove from the Council the Secretary (Mr. Proctor), Sir Edmund Beckett, Captains Noble and Tupman, and Mr. Browning (Mr. Proctor resigned the Secretaryship last November, being then in America, and we believe that Sir E. Beckett and Mr. Browning declined to be put in nomination again). This year there appears to have been a falling-out among the dissentients of 1873, Colonel Strange objecting to Professor Pritchard as a Vice-president of the Society. The President, Professor Cayley, with his usual skill and judgment, brought the attack to a close by pointing out that Professor Pritchard was absent. We are glad to say that no attempt has been made to bring again before the Council the rejected scheme for obtaining a new government observatory for predicting the weather by studying the sun-spots.

BOTANY.

Tortula Sinuosa, in Warwickshire.—It may interest Bryologists to know that Mr. J. Bagnall has found this moss in Warwickshire, on the mortar of a brick bridge, near Wootton-Waven; in this habitat small, apparently starved: also near Fenny-Compton, on an old tree-stump, the plants in this locality being robust; in both habitats, however, barren. *Pottia cavifolia* is not confined to oolitic soils, as he found it abundantly on mud-capped walls, at Harbury, Fenny-Compton, Kineton, Wilnecote, and on marly banks at Bearley; all these localities are on the lias, and are all in South Warwickshire. The forms he finds at Wilnecote seem to be typical, whilst at Harbury and the other localities the piliferous form appears to prevail. (See also "Grevillea," May.)

British Hepaticæ.—"Grevillea" says that the announcement that the first part of Dr. Carrington's long-promised work is ready will be gratifying to some of our readers. It is uniform with the last edition of Sowerby's "English Botany," and is issued by the same publisher (Mr. Hardwicke, of Piccadilly).

Measuring the Growth-rate of Plants.—Mr. E. Askenasy measures with a micrometer the advance of the growing-point of a root or branch in a glass tube in the field of the microscope. The stem is fixed by cork or other means at one end of the tube, and the conditions of light, temperature and moisture are easily regulated.

The Fertilization of Gentians by Humble Bees.—The closed gentian (*Gentiana Andrewsii*) has flowers an inch and a quarter or more in length. These inflated, bright blue flowers of late autumn, appear to be always in the bud, as they never open. The corolla is twisted up so as to leave no opening at the top. The flowers are all nearly erect, with two stigmas considerably above the five anthers. The writer says he sees but one way in which it can be fertilized, that is by insects; but who the writer is we are not told. "Several of my students, as well as myself, more than two years ago, have often seen humble bees entering these flowers. They pry or untwist the opening with their mouth organs and legs, and then pop into the barrel-shaped cavity, which they just fill."

The Structure of Fibre-cells.—Dr. R. Braithwaite is delivering before the Quekett Club a series of lectures on Vegetable Histology. The following are his observations on so-called fibre-cells:—"These are almost entirely confined to the vascular bundles, and hence are found in wood and bast tissue, but very rarely in pith or bark. They are distinguished by their great length, and touch each other on all sides, with more or less oblique faces, so that in outline they are fusiform, pyramidal or conical, and have very rarely any transverse partitions, but we find such in the branched bast-cells of Euphorbia. Fibre-cells are usually simple, and always more or less thickened, with the secondary and tertiary laminæ distinct. The thickening is greatest in bast tissue, and in it also the secondary layers are most distinct, those of wood-cells being more homogeneous, and pores are also usually present. A small wide-turning spiral band is seen near the bordered pores in wood-cells of yew, vine, mezereon, *Viburnum Lantana*, &c., and with narrower turns in the wood of *Pinus picea.* Broader spiral bands are observable in the outer wood-cells of the annual ring of conifers, and the striation is more sharply defined by application of nitric acid. When unlignified, as in *Apocynaceæ* and *Asclepiadaceæ*, the iodine tests produce on the laminæ the usual blue reaction."

Cryptogamic Vegetation in the Egg of an Ostrich.—At a meeting of the Scientific Committee of the Royal Horticultural Society (March 4, 1874), the Rev. M. J. Berkeley called attention to the following communication made by Professor Panceri to the Institut Egyptien, at its meeting on December 13, on Cryptogamic vegetation found within the egg of an ostrich, which was interesting in connection with what he had himself brought before the committee on March 5 and 19, 1873. The egg had been given Professor Panceri at Cairo, and was still fresh, the air space having not even been formed. He soon, however, noticed the appearance of dark blotches within the shell, and having broken it open to ascertain the cause, he found that they were produced by the growth of minute fungi. Instances of a similar kind had already been studied by him, and he had communicated the results to the Botanical Congress held at Lugano in 1859. The Rev. M. J. Berkeley had found *Cladosporium herbarum* in the interior of a fowl's egg.

Passage of Gases through Colloid Membranes of Plants.—The experiments which have been made on this subject fully bear out those well-known experiments of Graham on the passage of gases through caoutchouc. M. Barthelemy says that the white spots on the *Begoniaceæ* are merely elevations of the epiderm on a layer of nitrogen. The leaves of certain varieties, very thin on the living plant, are reduced, on fading during the winter in darkness, to the condition of a pellicle endued with elasticity, and representing almost nothing but the cuticular layers. It was these colloid membranes which served to repeat Graham's experiment. Except in a few modifications of detail, he followed strictly the course of that illustrious physicist. In four experiments made with different membranes, he obtained the following results :—

	1st.	2nd.	3rd.	4th.
CO^2	1^h	1^h	1^h	1^h
N	15^h	$13^h\ 40^m$	$15^h\ 30^m$	14^h
O	6^h	$6^h\ 20^m$	7^h	$5^h\ 40^m$

The experiments made under conditions of pressure, temperature, and moisture, which doubtless were not identical, yet agree sufficiently well with those of Graham, and allow us to conclude that the natural colloid surfaces of vegetables have for carbonic acid an *admissive power* which is thirteen to fifteen times as great as that of nitrogen, and six or seven times that of oxygen. Some days after he repeated the experiment with carbonic acid, perfectly dry, and found for its velocity compared with nitrogen numbers varying from nine to eleven; it seems, then, that dry carbonic acid passes less rapidly than when moist. Replacing the vegetable film by caoutchouc, he obtained a similar result. The difference in the case of dry oxygen and nitrogen is less marked. He remarks, in conclusion, that these experiments prove the dialysis of carbonic acid through the cuticle of leaves, just as much as those of Dutrochet on membranes and aqueous solutions prove endosmose by cellules.

CHEMISTRY.

The Elections at the Royal Society.—At a late meeting of the Royal Society, the following gentlemen were elected Fellows of the Society:—Isaac Lowthian Bell, F.C.S.; W. T. Blanford, F.G.S.; Henry Bowman Brady, F.L.S.; Thomas Lauder Brunton, M.D., Sc.D.; Professor W. Kingdon Clifford, M.A.; Augustus Wollaston Franks, M.A.; Professor Olaus Henrici, Ph.D.; Prescott G. Hewett, F.R.C.S.; John Eliot Howard, F.L.S.; Sir Henry Sumner Maine, LL.D.; Edmund James Mills, D.Sc.; Rev. Stephen Joseph Perry, F.R.A S.; Henry Wyldbore Rumsey, M.D.; Alfred R. C. Selwyn, F.G.S.; Charles William Wilson, Major R.E.

Volatile Acids of Wines.—According to E. Duclaux, who has a paper in the "Comptes Rendus," April 20, wines when sound contain acetic acids in slight amount, mixed with about $\frac{1}{12}$th or $\frac{1}{15}$th part of butyric acid. Valerianic acid is found in quantities not exceeding 10 milligrams per litre, and a higher fatty acid in almost infinitesimal proportions. When wine is affected with the disease known as "*tourné*," or "*pousse*," acetic and metacetic acids are developed in about equal proportions, and to the amount of 2·5 or 2·6 grms. per litre. In the disease of bitterness, acetic acid is developed, along with butyric acid and traces of higher fatty acids. The amount of butyric acid formed is larger than in alcoholic fermentation. The author intends to investigate the disease of "acescence" on a future occasion.

Alcoholic Fermentation.—Herr Oscar Brefeld, in the "Journal of the Chemical Society of Berlin," abstracted by the "Chemical News," June 12, concludes that—(1) Alcohol yeast, like all other plants, requires for its development and increase the co-operation of free oxygen. (2) If air and free oxygen are excluded, the yeast cannot grow. These two facts overturn the theories of Pasteur on fermentation. (3) Yeast, in contradistinction to all other plants, when in solutions where its growth is possible, has a great affinity for free oxygen. It can completely extract free oxygen mixed with 6,000 volumes of carbonic acid. (4) *Mucor racemosus* has the same property, and excites alcoholic fermentation in a saccharine solution. (5) Growth and increase of yeast may take place without fermentation, and, again, fermentation without growth of yeast. The carbonic acid thrown off in fermentation is remarkable for its purity. (6) In suitable solutions, exposed to the air, growth of the yeast plant and fermentation occur simultaneously in different parts of the liquid. (7) Fermentation is the expression of an abnormal incomplete vital process, in which all the bodies required for the nutrition of the yeast do not simultaneously and harmoniously co-operate. These results will prove of great technological interest.

Gum in Fruit-trees considered as a Pathological Phenomenon.—M. Prilleux says, in "Comptes Rendus," April 27, that the flow of gum is a real disease, which he names *gommose.* The alimentary substances, placed in reserve in the interior tissues, instead of promoting the plant's growth, are diverted to production of gum, and a portion accumulate, awaiting the instant of their transformation about gummy centres, which seem to act as centres of irritation. The case is analogous to what occurs when an insect deposits one of its eggs in the tissues of a plant, leading to production of a gall, which consists of new cells holding a mass of nutritive matter (particularly fecula) destined, not for the wants of the plants itself, but for the development of the small parasite which appears. The production of gum at expense of the nutritive matter has no other limit than the complete exhaustion of the plant. Scarification of the bark is the best remedy. Mr. Prillieux's explanation is this :—To cure the disease the materials misappropriated to formation of gum must be brought back to their normal destination. Hence a more powerful attraction for them must be introduced than that of the gummy centres. Now the wounds of the bark necessitate the production of new tissues, and under this strong excitation the reserve matters are employed in formation of new cells, and cease to be attracted in the wrong direction. (See also *Chemical News, June* 19.)

Prepared Paper a Test for Urea.—An article on this subject has appeared in the "Comptes Rendus," by M. Musculus. The following is a brief abstract of the method of using the paper as a qualitative test. Urine, when it has arrived at full alkaline fermentation, is thrown upon a filter. The pores of the paper are soon filled up with globules of a certain ferment, and the filtration slackens. The paper is then washed with distilled water till the alkaline reaction has disappeared, and dried at a temperature of 35° to 40° C. In this state the paper is an efficient test for urea, which, if immersed even in a very dilute solution for ten or fifteen minutes, it converts into carbonate of ammonia. The test-paper should be coloured yellow with turmeric. It is then dried afresh, and preserved in a stoppered bottle. If a slip of the paper is soaked in a solution of urea containing only $\frac{1}{10000}$th part, it soon becomes covered with brown spots. If it is required to detect urea in a liquid, it must first be neutralised. If alkaline carbonates are present, acid enough must be added to decompose the bicarbonates which may be formed. These salts might induce an error. They do not immediately colour turmeric paper brown, but after the lapse of a little time the brown tint appears, especially on exposure to the air. Neutral alkaline salts do not interfere. The fermentation proceeds as well in the presence of phenic acid as in its absence. The quantitative determination of urea may be also performed with this paper.

Is Turacin from the Touraco?—We are glad to see that Professor Church's well-known researches on this peculiar colouring matter have been again completely confirmed. M. J. J. Monteiro, in the "Comptes Rendus," fully confirms the results of Church as to the presence of copper in the feather of the Touraco. Copper exists in the regions inhabited by the bird, in the state of green carbonate (malachite).

How Soluble in Water is Arsenious Acid?—This question is answered in a late number of the "Bull. de la Soc. Chim.," by M. L. A. Bücher. He says that 1 part of crystalline arsenious acid dissolves, after 24 hours' digestion, in 355 parts of water at 15° C. The amorphous acid, in the same conditions, requires 108 of water. If the crystalline acid has been dissolved at a boiling heat, and the solution left to stand for twenty-four hours, it contains to 1 part of acid 46 of water. The amorphous acid, in the same circumstances, remains dissolved in 30 parts of water.

How to form Octahedral Crystals of Borax.—M. Gernez says it is known that borax forms two hydrates—the one containing 5 equivalents of water, and crystallizing in regular octahedrons; the other containing 10 equivalents, and forming oblique rhombic prisms. The octahedral crystals are commonly considered stable only at relatively high temperatures. He, however, finds that both the prismatic and the octahedral form can be produced at low temperatures. The temperature of 56°, which has been indicated as the inferior limit for the production of prismatic borax, is in reality only a temperature near the higher limit at which the production of prismatic borax has been observed, since this salt loses a part of its water at this temperature.

The Amount of Carbonic Acid Gas in the Atmosphere.—It seems that a series of determinations have been recently made by M. Truchot, who has published the results in papers in the "Comptes Rendus" (lxxvii. p. 675), and the "Bull. Soc. Chimique." His method of analysis consisted in passing a known volume of air through a graduated solution of barium hydrate, allowing the barium carbonate to deposit, and retitring the solution. He finds: 1st, that at Clermont-Ferrand, where the experiments were made, the proportion of carbonic gas is a little greater during the night than in the daytime—a fact confirmatory of the observations of Saussure and Boussingault. 2nd, that the proportion is not sensibly greater in the city than in the open country. 3rd, that in the vicinity of green-leaved plants the quantity of carbonic gas varies notably, according as the green parts are exposed to full sunlight, to diffused light, or are in the shade; the amounts being 3·54, 4·15 and 6·49 parts in 10,000 of air. 4th, that the general mean is 0·814 milligram of carbonic gas to the litre of air, or 4·09 parts in 10,000; a number very near that usually received. 5th, that the proportion of carbonic gas diminishes with the altitude, thus:

Station.	Altitude.	Wt. CO_2 to the litre.	Vols. of CO_2 in 10,000 of air.
Clermont-Ferrand . . .	395^m	$0·623^{mg}$	3·13
Puy-de-Dôme	1446	0·405	2·03
Pic-de-Sancy	1881	0·342	1·72

How to Detect False Colouring Matter in Wine.—M. De Cherville gives the following test. He says:—"Pour into a glass a small quantity of the wine under examination, and dissolve in it a morsel of potassa. If there is no deposit, and if the wine takes a greenish tint, it has not been artificially coloured. If a violet deposit has been formed, the wine has been coloured with elderberries or mulberries. If the deposit is red, beet-root or peach-wood has been used; and if violet-red, logwood. If the sediment is violet-blue, privet berries have been employed, and if a bright violet, litmus."

The Dangerous Nature of certain Coloured Tapers.—At a recent meeting of the Chemical Section of the "Glasgow Philosophical Society," an important paper on this subject was read by Mr. James McFarland, Assistant to the Professor of Chemistry, St. Andrew's. In this the author detailed a series of experiments which he had prosecuted for the purpose of determining the nature of the colouring matter in the green and red wax tapers. He distinctly ascertained that the former owed their colour to the presence of Scheele's green (arsenite of copper). Their average weight was 2 grams, and the average time occupied in burning was seventeen minutes. Guided by the colour and by the alliaceous odour evolved during combustion, he had no difficulty in pronouncing that arsenic was present; its presence was experimentally determined, and its quantity estimated to be 0·60 per cent. of the taper, equal to 0·35 grm., or 5·43 grs. of arsenious acid—quite enough to poison two people if taken directly in the solid form. The red tapers weighed, on an average, about 8 94 grams, and burned seventeen minutes, leaving 3 milligrams of ash totally devoid of metallic appearance. Mercury, existing as vermilion, was found by Reinsch's process, and its quantity was afterwards carefully determined. The amount of mercuric sulphide ultimately collected, washed, and dried, was 1·66 per cent. In one series of experiments, the following results were arrived at, white, yellow, blue, red, and green tapers being experimented upon:—

White.—Perfectly harmless; little ash.

Yellow.—Harmless; coloured with chromate of lead; ash, metallic.

Blue.—Harmless; coloured with ultramarine.

Red.—Highly poisonous, containing 1·93 per cent. of vermilion; the tapers very highly coloured; slight ash.

Green.—Poisonous; colour due to arsenic; metallic ash; quantity of arsenic not determined, but probably about 1 per cent.

GEOLOGY.

The Carboniferous Plants of Canada have been explored by Dr. J. W. Dawson, F.R.S., who has published a series of reports upon the subject, which have been reprinted from the "Transactions of the Geological Society of Canada." The work is said to extend greatly our knowledge of the Lower Carboniferous Flora. It also contains a list of the species of the Middle and Upper Coal Measures, and discusses the character of Sigillarioid and Lepidodendroid stems.

The British Seas, from a Geological Aspect, have been very carefully described by M. Delesse in his recent work on the subject. With regard to the orography of the "Ocean Britannique," as M. Delesse calls our seas (excepting the Channel and the North Sea), he shows how Britain and Ireland stand on a sort of terrace, the boundary of which on the Atlantic side coincides sufficiently exactly with the 600 feet contour line (*below* sea-level, of course), in such a manner that were the British Islands elevated 600 feet they would be joined to France and to Denmark, but the Faroë Islands, Iceland, and Rockall would still be islands. As is natural in the neighbourhood of a mountainous centre, the surface of this terrace is very irregular around Scotland; especially there are to be noted two valleys extending under the sea to the West of Scotland, separating it on the one hand from the Hebrides, and on the other from Ireland. The littoral deposits of the South of England are naturally very similar to those of the North of France, and are equally referable to the geological formations along the coast-line. Among the submarine deposits sand stands preeminent. Next in point of frequency comes gravel, which is found in oddlyshaped masses at various points, especially to the west of the British Isles, south of Cork in the Bristol Channel, between Land's End and the Sorlingues Islands, and also in the Channel. In the eastern part of the Channel gravel occupies a large surface, and seems to unite the Greensand of Upper Normandy to that of England. In the Bristol Channel and to the South of Ireland, M. Delesse suggests that the origin of the gravel is probably a submarine outcrop of Old Red Sandstone; south of Exmouth and of Star Point it may be referred to the prolongation under the sea of the arenaceous Triassic beds. The distribution of mud is next investigated. The patches of mud-bottom in the Channel are due doubtless to the Palæozoic schists. the Triassic clays, the Liassic marls, and the Eocene clays. They can be traced from Dover to the Lizard.

The Sub-Wealden Exploration in Sussex.—The boring to prove the Palæozoic rocks of Sussex, which was commenced in 1872, is—says the "Geological Magazine," June, 1874—now being carried on with great vigour. The adoption of the Diamond method of boring has proved a great success. Mr. Henry Willet, in his Sixth Quarterly Report, dated March 28th, states that a total depth of 671 feet has been reached. The drill, called the "Crown," is a ring of soft steel 3½ inches in diameter, and has 15 diamonds set in it round its lower edge. It revolves at a speed varying from 150 turns a minute in soft strata, to 300 in hard rock. Water is pumped down the centre, and rising at the sides, conveys the *débris* in suspension to the surface. The diamonds are not brilliant, and have no cleavage planes; they come from Brazil, and are called "Carbonado." The cores brought up are sometimes six or seven feet long in one piece, and form a beautiful section of all the strata passed through. The fact that delicate shells are found lying at right angles to the axis of the bore, is an indisputable proof that the beds are horizontal.

The following strata have been penetrated:—

	feet	in.		feet	in.
Shales	17	0	Dark gypsum, impure	13	0
Blue limestone	2	0	Blue shale	3	0
Shale	5	0	Gypsum in nodules and veins	13	3
Blue limestone	2	0	Gypsum marl	8	0
Shale	4	0	Black sulphurous marl	1	0
Limestone	1	0	Greenish sand, with nodules of chert	21	0
Shale	4	0	Sandy shale, with nodules of chert	38	0
Limestone	3	0	Carbonate of lime veins	2	0
Shale	4	0	Hard sulphurous black shale	12	0
Limestone	4	0	Soft sulphurous black shale	7	0
Blue shale	16	0	Hard shale, with chert	12	0
Grey shale	3	0	Black shale	2	0
Hard shale	14	0	Very sulphurous black shale	12	0
Shale, with crystals of carbonate of lime	9	6	Paler shade, with gypsum veins	4	0
Grey shale	1	0	Dark shale	2	0
Greenish shales, with vein of gypsum	20	0	Grey shale "Kimmeridge Clay," very fossiliferous	378	0
Impure gypsum	9	0			
Pure gypsum	4	0			
Impure gypsum	8	0			
Pure gypsum	3	0			

Mr. H. Peyton considers that the first 180 feet represent the Purbeck beds, and the next 110 feet the Portland beds.

The recent accident owing to the boring-rods slipping down the hole presented no serious difficulties. The whole of the tackle has now (June 17) been recovered, and the boring is resumed. It is anticipated that a depth of 1,000 feet from the surface will be reached this week, which will terminate the first contract with the Diamond Rock Boring Company. It is to be hoped that sufficient funds will be forthcoming to enable the boring to be continued, as the interest attaching to the exploration is intensifying as the depth increases, and the marvellous speed with which the Boring Company have lately been prosecuting the work would make it probable that a month or two more would solve the question of what underlies the Wealden formation.

The Coal-fields of the United States have been fully dealt with by Mr. James Macfarlane, M.A., in his work on this subject, which has been published by Messrs. Trübner, of Paternoster Row. With regard to the enormous area of the coal-fields of the United States, containing in all 192,000 square miles, besides the lignites of the Far West, the vast quantity, great variety, accessibility, and wide distribution of their stores of coal—which, up to the present time, have hardly been developed to any really great extent—it is, as Mr. Macfarlane justly observes, the manifest destiny of America soon to become the greatest coal-producing country in the world. The following table shows the relation between area and production of the various coal-producing countries of the world (given on p. 674 of Mr. Macfarlane's admirable work).

Coal-producing Countries.	Area in Sq. M. of Coal-fields.	Date.	Tons.	Per Cent. Production.
The United States . . .	192,000	1872	41,000,000	18·66
Nova Scotia	18,000	1871	673,242	0·31
Great Britain	11,900	1871	117,352,028	53·41
France	1,800	1867	12,148,223	5·54
Belgium	900	1871	13,671,470	6·23
Prussia	1,800	1869	26,774,368	12·19
Austria	1,800	1862	4,525,783	2·02
Spain	3,000	1869	593,033	·27
Chili, Australia, China, &c.	28,800	1872	3,000,000	1·37
Totals	260,000		219,738,147	100·

The Physical History of the Rhine Valley.—Professor A. C. Ramsay, V.P.R.S., has gone into this subject in a paper read before the Geological Society, at one of its meetings this session. The author first described the general physical characters of the valley of the Rhine, and discussed some of the hypotheses which have been put forward to explain them. His own opinion was that during portions of the Miocene epoch the drainage through the great valley between the Schwarzwald and the Vosges ran from the Devonian hills north of Mainz into the area now occupied by the Miocene rocks of Switzerland. Then, after the physical disturbances which closed the Miocene epoch in these regions, the direction of the drainage was reversed, so that after passing through the hill-country between the Lake of Constance and Basel, the river flowed along an elevated plain formed of Miocene deposits, the remains of which still exist at the sides of the valley between Basel and Mainz. At the same time the Rhine flowed in a minor valley through the upland country formed of Devonian rocks, which now constitute the Taunus, the Hundsruck, and the highland lying towards Bonn, and by the ordinary erosive action of the great river the gorge was gradually formed and deepened to its present level. In proportion as the gorge deepened, the marly flat Miocene strata of the area between Mainz and Basel were also in great part worn away, leaving the existing plain, which presents a deceptive appearance of having once been occupied by a great lake. The paper gave rise to an important discussion.

MECHANICS.

The Transmission of Detonation by Tubes.—A valuable paper on various detonating substances was read by Mr. F. A. Abel, F.R.S., at an early meeting of the Royal Society. The author says that a great difference appeared, at first, to be established in the power possessed by tubes of different materials of favouring the transmission of detonation, the glass tubes being far in advance of the others in this respect. It was eventually established very clearly, by a series of experiments, that this difference was not due, to any decisive extent, to the physical peculiarities (in regard to sonorosity, elasticity, &c.) of the materials composing the tubes, but chiefly to differences in the degree of roughness of their inner surfaces, and in the consequent variation of the resistance opposed by those surfaces to the gas-wave. Thus the power of a glass tube to favour the transmission of detonation was reduced, by about two-thirds, by coating the inner surface with a film of French chalk, while the facility of transmission, through a brass tube, was nearly doubled by polishing its interior, and was increased threefold with a paper tube by coating the interior with glazed paper. The following are some of the points established by these experiments on the transmission of detonation by tubes:—1. The distance to which detonation may be transmitted through the agency of a tube to a distinct mass of explosive substance is regulated by the following conditions:—(*a*) By the nature and the quantity of the substance employed as the initiative detonator, and by the nature of the substance to be detonated, but not by the quantity of the latter, nor by *the mechanical condition* in which it is exposed by the action of the detonation. (*b*) By the relation which the *diameter* of the "detonator," and of the charge to be detonated, bear to that of the tube employed. (*c*) By the strength of the material composing the tube, and the consequent resistance which it offers to the lateral transmission of the force developed at the instant of detonation. (*d*) By the amount of force expended in overcoming the friction between the gas and the sides of the tubes, or other impediments introduced into the latter. (*e*) By the degree of completeness of the channel, and by the positions assigned to the detonator and the charge to be detonated. 2. The nature (apart from strength or power to resist opening up, or disintegration) of the material composing the tube through which detonation is transmitted, generally appears to exert no important influence upon the result obtained. At any rate, the differences with respect to smoothness of the interior of the tubes far outweigh those which may prove traceable to differences in the nature of the materials composing them.

A Patent Feeder and Grate for Consumption of Fuel, which has been recently exhibited by Messrs. Frisbu, appears to be a most ingenious arrangement for feeding a fire with coal from the bottom. This feeder and grate provides a simple method of feeding fuel up, from underneath the fire, into all descriptions of furnaces, fuel-boxes and fire-grates. By this principle of feeding from below the fire there is no fresh consumption of the fuel, the igniting of the fresh coal is a gradual process, while at the same time a very intense heat is obtained. The hottest portion of the fire being constantly at the top utilises the heat, and preserves the fire-bars from being burnt out; the heat of the surface of the fire is not abated by the supply of fresh fuel, and no cold air is admitted to the furnace while feeding, thereby preserving a perfectly uniform heat. By feeding from beneath, the coal is pushed up and outwards equally from the centre of the grate, and is evenly consumed, with scarcely any refuse except fine ashes, which drop down through the grate-bars without raking. From various testimonials which the inventor has received, it seems that there is a great saving in the use of the coal; thus one firm says their coal bill averaged 160*l.* a month, but on introducing one of these burners they only used that quantity in four months.

A Useful Saving Form of Fire-grate.—The invention of the Rev. J. Wolstencroft is described as follows in the "Chemical News." The inventor says the great difficulty is solved, viz. "how to get a healthy, cheerful fire, imparting a genial heat, with half the amount of fuel commonly used." We (says the Editor) saw the grate in use, and we must candidly admit it was the most cheerful and the brightest fire in the place, but as to the amount of coal it daily consumed we are unable to say. According to some experiments which have been performed with it by James D. Curtis, Commander Royal Navy, there is truth in the inventor's statement that there is a great economy in the consumption of fuel. Captain Curtis, of Brimpsfield, Gloucester, experimented with the grate in his harness-room from August 18, 1873, to September 1, 1873, using no other fire, burning slack coal delivered for 24*s.* per ton, employing this fire daily for cooking small things, such as boiling potatoes for the fowls, &c., and after the daily use the fire was left to burn itself out during the night; the cost of coal per day was 3½*d.* The front of the grate is continued down to the floor, cutting off the supply of air from within the room; by this means an air chamber is formed under the grate, to which the air is communicated from within or without the building, bringing the draught under and directly through the fire-bars. In a fire-grate which has been fitted up in Manchester, at the office of one of the Local Boards, the air-chamber communicates with the main sewer, and draws its supply from thence—thus, as it is supposed, ventilating the sewer, at the same time consuming the noxious sewer gases. Any kind of fuel can be used, and very small coal can be burnt as easily and with as good results as lumps; coke and cinders may be burnt over and over again, until they become as fine as sand. The ashes from the fire all drop through the bottom of the grate into or through the air-chamber, consequently dust from the fire is greatly diminished in the room; the draught may be regulated at pleasure with a valve. The invention may be easily applied to many existing grates at the cost of a few shillings.

MEDICAL SCIENCE.

The Localization of Function in the Brain.—Dr. David Ferrier has given a valuable abstract of a paper read before the Royal Society. It is published in abstract in the "Proceedings of the Royal Society," vol. xxii. No. 151. He says that among the experiments now related are some in further confirmation and extension of those already made on cats, dogs, and rabbits, as well as a new series of experiments on other vertebrates. In particular, numerous experiments on monkeys are described, for the purpose of which the author received a grant of money from the Council of the Royal Society. In addition, the results of experiments on jackals, guinea-pigs, rats, pigeons, frogs, toads, and fishes are narrated. The method of investigation consists in the application of the stimulus of an induced current of electricity directly to the surface of the brain in animals rendered only partially insensible during the process of exploration, complete anæsthesia annihilating all reaction. It is supplemented by the method of localized destructive lesions of the hemispheres. Special attention is called to the precision with which a given result follows stimulation of a definite area—so much so, that when once the brain has been accurately mapped out, the experimenter can predict with certainty the result of stimulation of a given region or centre. The theory that the phenomena are due not to excitation of cortical centres, but to conduction of the electric currents to basal ganglia and motor tracts, is considered to be disposed of by the fact of the precision and predictable characters of the results, and by the marked differences in the phenomena which are observed when regions in close local relation to each other are excited. Other facts are pointed out bearing in the same direction; among others, the harmony and homology subsisting between the results of experiment in all the different animals. The experiments on monkeys are first described. Reference is continually made in the description to figures of the brain, on which are delineated the position and extent of the regions, stimulation of which is followed by constant and definite results. A complete statement of these results in the present abstract is impossible. Generally, it may be stated that the centres for the movements of the limbs are situated in the convolutions bounding the fissure of Rolando, viz. the ascending parietal convolution with its postero-parietal termination as far back as the parieto-occipital fissure, the ascending frontal, and posterior termination of the superior frontal convolution. Centres for individual movements of the limbs, hands, and feet are differentiated in these convolutions. Further, in the ascending frontal convolution, on a level with the posterior termination of the middle frontal, are centres for certain facial muscles, *e.g.* the zygomatics, &c. At the posterior termination of the inferior frontal convolution and corresponding part of the ascending frontal are the centres for various movements of the mouth and tongue. This is the homologue of "Broca's convolution." At the inferior angle of the intraparietal sulcus is the centre for the platysma. In the superior frontal convolution, in advance of the centre for certain forward movements of the arm, as well as in the corresponding part of the middle frontal convolution, are areas, stimulation of which causes lateral (crossed) movements of the head and eyes, and dilatation of the pupils. The antero-frontal region, with the inferior frontal and orbital convolutions, give no definite results on irritation. Extirpation of these parts causes a condition resembling dementia. No results could be ascertained as regards the function of the central lobe or island of Reil. Irritation of the angular (*pil courbe*) causes certain movements of the eyeballs and pupils. Destruction of this convolution gives data for regarding it as the cerebral expansion of the optic nerve, and, as such, the seat of visual perception. The phenomena resulting from irritation of the superior temporo-sphenoidal convolution (pricking of the ear, &c.) are indications of excitation of ideas of sound. It is regarded as the cerebral termination of the auditory nerve. The sense of smell is localized in the uncinate convolution. The situation of the regions connected with sensations of taste and touch is not accurately defined, but some facts are given indicating their probable locality. Of course this valuable paper will appear in full, with illustrations, in the "Philosophical Transactions."

Secondary Action of Snake-poisons on the Muscles.—One of the longest essays that we have almost ever seen—with the exception of Dr. Carpenter's in the "Proceedings of the Royal Society"—is that on the effects of snake-poisons, published Dr. Fayrer and Dr. Brunton (No. 149). In this, among other facts of the greatest interest and importance, they state that the muscles of the part into which the poison has been introduced are very apt to undergo rapid decomposition. "We have already shown that their irritability is either lessened, or completely destroyed, by the action of the venom; and it seems very probable that the mere contact of any other foreign body, containing *Bacteria* or their germs (as the water in which the cobra-poison was dissolved in our experiments certainly did) would suffice to explain the decomposition of the muscle without assuming any special putrefactive action on the part of the poison; for the muscle, which has been at least temporarily killed by the poison, is placed in the body in the most favourable conditions of temperature and moisture for the occurrence of decomposition whenever any germs are brought into contact with it. However, Weir Mitchell found that the venom of the rattlesnake had a curious influence upon muscle, which could hardly be explained without the supposition that the poison had a peculiar disorganizing action upon the muscular tissue. In every instance the venom softened the muscle in proportion to the length of time it remained in contact with it; so that, even after a few hours, in warm-blooded animals, and after a rather longer time in the frog, the wounded muscle became almost diffluent, and assumed a dark colour and somewhat jelly-like appearance. The structure remained entire until it was pressed upon or stretched, when it lost all regularity, and offered, under the microscope, the appearance of a minute granular mass.";

How the Actual Cautery should be used.—We fear a good many professional men are completely ignorant of this. We ourselves have known men who fancied that the iron was not to be brought even to a red-heat; whereas when rightly done, it is done with a *white-hot iron,* pain being then completely avoided. Dr. J. S. Camden makes the following remarks on this subject in the "Medical Times and Gazette," and, as they are worthy of being recorded, we give them a place. He says:—"I see in Dr. Fayrer's work on the 'Thanatophidia of India' that the actual cautery was used unsuccessfully (which in another place he calls a *red-hot* iron). This is not what I was always taught and seen as actual cautery. I consulted 'Cooper's Surgical Dictionary,' edited by Lane. There it is called an iron in a state of incandescence, which is, according to Maunder, incipient white heat. Prof. Symes, in his lecture, calls it a red-hot iron. I also made many inquiries of medical friends, and all spoke of it as a red-hot iron. Having twice assisted in using, and once used, actual cautery, I hope I know something on the subject. When actual cautery is to be used, the iron must be heated till it is really of a white heat, and looks almost as white as white paper. If then applied, it destroys the part instantaneously, giving no pain; but it must be removed quickly on the heat decreasing, and then another iron applied. Several irons are required for use, and a fierce fire kept up by bellows, till your object is attained; but if a red-hot iron only is used, the agony is intense, as we all know who have touched it. The first time I saw it used, on a girl of fourteen years, no pain was given, to my great astonishment; the second time, on an elderly person (both for fungus in the upper maxillary bone), her screeching was fearful, till I told the operator his irons were not half hot enough. He then requested me to heat them properly, which being done, not a murmur was heard. The irons were being used only red-hot. The last time was opening four or five sinuses in a favourite horse's shoulder. He never flinched, and scarcely seemed aware of what was being done. The only thing he noticed—for he never moved—was the hissing made by the destruction of the skin. Actual cautery is painless. I would suggest using—to obtain the white heat for actual cautery—a large spirit blow-pipe."

Physiology of the Flight of Birds.—In one of the numbers of the "Comptes Rendus," M. Marey, who is the Dr. Pettigrew of France, has a paper on the above subject. He used various artificial birds for experiment. Comparing their wing-stroke with that of corresponding real birds, he perceived that the former must be three or four times more rapid than the latter in order to raise the weight. Some condition, then, increasing the resistance of the air under the wing must be wanting in his apparatus. This is, he showed, the translation of the bird. Air shows inertia; that is, submitted to a constant repulsive force, it resists strongly at first, then acquires velocity, which it tends to retain after the force has ceased to act. Move a light disc uniformly in a direction perpendicular to its plane. It may be shown with a registering dynamometer that there is—(1) a considerable resistance at the beginning, from inertia of the air column; (2) a weaker pressure maintained throughout the movement; (3) a tendency to impulsion of the disc when it has stopped, from the acquired velocity of the air column. Thus the resistance of the air to movements of bodies consists of a regular *régime,* preceded and followed by two variable states. The former is that which various experimenters have sought to measure. The resistance of the air attaining its maximum during the initial variable state, it is clear that the wing of a bird would find in the air a more solid fulcrum if, throughout its descent, it were placed in these initial conditions. Now, through translation, the wing at each instant of its descent comes to act on a new column of air, which it tends to depress. But, from the short duration of the pressure, each of these columns has not time to acquire the velocity of the wing; it is thus compressed, and presents the maximum resistance of the initial variable state. To test this theory M. Marey gave his artificial birds a horizontal movement of translation: attaching one, *e.g.*, to the end of a long arm which was driven round while the wings were made to beat by means of a steam-driven air-pump. When the arm was stationary, the wing described between its extreme positions an angle of about 60°. On driving the arm 10 metres per second the amplitude was reduced to 30° and even 20°, showing the effect of resistance of the air on the velocity of the strokes.

METALLURGY, MINERALOGY, AND MINING.

The Fibrous Quartz of the Cape.—This, which is a pseudomorph after Krokydolite, is described by Herr Dr. F. Wibel, whose paper on this subject is noticed in the "Geological Magazine" for March 1874. It says that it is a curious fact that whilst quartz so commonly occurs crystallized, it has rarely been observed in distinctly fibrous forms. The best-known example is that of the so-called fibrous quartz of South Africa. The object, however, of the paper of Dr. Wibel is to show that this substance is not an original form of quartz, but is merely a product of pseudomorphism, in which the fibrous structure of a pre-existing mineral has been retained. Dr. Wibel has examined two varieties of this African mineral—the one brown and the other blue. The brown variety occurs in the form of bands in a highly siliceous brown ironstone. Analysis of the fibrous mineral showed that it contains—silica, 57·46; ferric oxide, 37·56; water, 5·15. Treated with hydrochloric acid, the iron is removed, and a white fibrous siliceous material is obtained. Hence he concludes that the brown mineral consists of a mixture of white quartz and ferric hydrate in the form of Göthite ($Fe_2O_3 \cdot H_2O$).

What is Brochantite?—This question is fully answered by Dr. A. Schrauf, in a paper in the "Proceedings of the Vienna Academy of Science," and is noticed at length in the March number of the "Geological Magazine." It seems that several minerals, differing from one another both chemically and morphologically, have hitherto been grouped together under this specific name; and, although our knowledge of many of these varieties is still imperfect, yet the author feels justified in referring them to four distinct types, namely:—(1) The Brochantite of Rézbánya in Hungary, of which two varieties (*a* and *b*) are recognised; and some of the Cornish and Russian Brochantites. (2) The Waringtonite of Cornwall, and a third variety (*c*) from Rézbánya. (3) The Brochantite of Nischne-Tagilsk, in

Siberia. (4) The Königin of Russia, and a fourth variety (*d*) from Rézbánya. Dr. Schrauf points out that the relation between the crystalline forms of Brochantite and those of Malachite. Just as Malachite was originally described as prismatic and subsequently determined to be monoclinic, so it appears that careful measurements of Brochantite tend to remove it from the prismatic system. The author believes that some varieties of Brochantite are monoclinic and others triclinic. In addition to the crystallographic details, the paper includes a comparative review of the paragenetic and chemical relations of the Brochantite group of minerals.

A new Mineral: Kjerulfine.—Herr von Kobell has given this name to a new fluophosphate from Bamle in Norway. It is described as compact, with an imperfect cleavage in two directions nearly at a right angle. The fracture is uneven and splintery; lustre oily; colour faint red to yellow, in thin fragments translucent. G. = 3·15. H. = 4—5. When heated phosphoresces with a white light. B.B. fuses easily at 3 with intumescence to a blebby enamel. Soluble in hydrochloric acid, but with greater readiness in nitric acid. With sulphuric acid, hydrofluoric acid is evolved and sulphate of lime separates from the solution. Analysis gave

$\overset{\cdot\cdot}{\dddot{P}}$	$\dot{M}g.$	$\dot{C}a$	$\dot{N}a$	$\dot{K}$	Fl	$\dddot{S}i$	$\dddot{\text{A}}l\ \dddot{\text{F}}e$	$\dddot{S}$
42·22	37·00	7·56	1·56	*tr.*	4·78	1·50	5·40	*tr* = 100·02

Excluding the silica and alumina and the oxygen of the lime and soda, and averaging the analysis up to 100, von Kobell considers pure kjerulfine to consist of $\overset{\cdot\cdot}{\dddot{P}}$ 46·62, $\dot{M}g$ 40·86, Ca 5·96, Na 1·28, Fl 5·28, from which he calculates the formula to be $2\dot{M}g^3\ \overset{\cdot\cdot}{\dddot{P}}$ + Ca Fl, in which a small portion of the calcium is replaced by sodium.

The Recent Minerals of Vesuvius.—Of course in such a district as that of Vesuvius there must be new minerals appearing from time to time after eruptions have taken place. Professor J. Vom Rath, of Bonn, has published an important paper on this subject. The fourth part of this essay has just been published, and it consists of geographical, geological, and mineralogical observations on the vicinity of Massa Maritima; on Calabria; and on Vesuvius. Under the last head, Vom Rath has many important observations on the minerals formed at the eruption of 1872. He shows that the composition of *Microsommite* of Scacchi is related to that of the sodalite group, and makes the important observation that nephelite, sodalite, and microsommite, which are alike in occurring in the pores of lavas, have resulted alike from the action of sea water (rich in chloride of sodium) on the silicate of the lava. Microsommite occurs in hexagonal prisms, with pyramidal planes on the basal edges, and has 43° 40′ as the basal angle of the pyramid. Vom Rath concludes also that leucite was one of the results of sublimation at the eruption of 1872, as well as augite, hornblende, cavolinite, biotite, hematite, and magnetite.

MICROSCOPY.

An American $\frac{1}{75}$-inch Object-glass.—The "Monthly Microscopical Journal" says that, according to a statement in an American journal, Mr. Tolles has alone made an object-glass of this power; but the American journal is evidently unfamiliar with the fact that Messrs. Powell and Lealand have long since achieved a similar result. The paper in question says, "Boston stands pre-eminent in the production of exquisite and wonderful optical instruments. Mr. Tolles has just achieved the great result of producing a $\frac{1}{75}$ objective for microscopic uses—a glass of such difficult construction, that we believe no optician has ever attempted it before. The power of this objective is such that a single white blood corpuscle covers the entire field of vision. Mr. Tolles has produced two of the finest $\frac{1}{50}$ objectives ever constructed, one of which is in this city, the other in the hands of a Western gentleman. The angular aperture of one is 120°; that of the other, and the last constructed, is 165°. The objectives are of great excellence, *and, in the opinion of competent microscopists, far surpasses in defining power and clearness of field those of European make." ! !*

A New Mode of Cataloguing Microscopic Objects.—At a recent meeting of the Medical Microscopical Society, a paper was read from Mr. Groves, "On Cataloguing and Arranging Microscopic Specimens." After describing the difficulties generally experienced, he said that he had adopted a method at once simple and of universal application. For small collections he advocated a total absence of classification in the cabinet, though for large collections he considered it necessary. The catalogue he used, and which he would rely upon in all cases rather than the classification of specimens, was this:—He took an ordinary alphabeted note-book, and in that noted under the proper alphabetical heading every portion of every preparation. Thus, for a specimen of small intestine:—Under (I) was entered Intestine, small, No. —; then under (G), Glands, Brunner's, No. —; Peyer's, No. —. Under (V), Villi, No. —; Villi, lacteals of, No. —; Villi, invol. muscle of, No. —, and so on. This method he found very handy for specimens required for demonstration purposes.

The Structure of the Ovum in Loligo.—Mr. E. Ray Lankester, M.A., has read a very valuable essay before the Royal Society, which will be published fully, with illustrations, in the "Transactions," upon the above and several kindred subjects. The following is a short account of the chief points of interest in the section devoted to *Loligo*:—(1) The explanation of the basketwork structure of the surface of the ovarian egg by the plication of the inner egg-capsule. (2) The increase of the yelk by the inception of cells proliferated from the inner egg-capsule. (3) The homogeneous condition of the egg at fertilisation. (4) The limitation of yelk-cleavage to the cleavage-patch. (5) The occurrence of independently-formed corpuscles (the autoplasts) which take part in the formation of the blastoderm. (6) The primitive eye-chamber, formed by the rising up of an oval wall and its growing together so as to form a roof to the chamber. (7) The origin of the otocysts by invagination. (8) The rhythmic contractility of a part of the wall of the yelk-sac. (9) The disappearance of the primitive mouth, and the development of a secondary mouth. (10) The development of a pair of large nerve-ganglia by invagination of the epiblast immediately below the primitive eye-chambers.

Progress of Microscopy during the Quarter.—The following is a list of the papers that have appeared in the "Monthly Microscopical Journal" during the past three months:—

Contributions towards a Knowledge of the Appendicularia. By Alfred Sanders, F.L.S., F.R.M.S., Lecturer on Comparative Anatomy at the London Hospital Medical College.—Note on the Verification of Structure by the Movements of Compressed Fluids. By Dr. Royston-Piggott, F.R.S., &c.—Note on the President's Remarks on the Searcher for Aplanatic Images, as to the Principles upon which it acts. By Dr. Royston-Piggott, F.R.S.—On Bog Moss. By R. Brathwaite, M.D., F.L.S.—The Fungus of the Hawthorn (Aœstelia Lacerta, Tulasne; Æcidum Lacertum, Grev.). By Thomas Taylor, Superintendent of the Microscopical Department of the Commission of Agriculture, U.S.A.—Points in the Histology of the Human Kidney. By R. Branwell, Brighton.—The Scales of Lepism as seen with reflected and Transmitted Light. By John Anthony, M.D., F.R.M.S.—Note on a curious Proboscis of an unknown Moth. By S. J. McIntyre, F.R.M.S.—An Instrument for excluding Extraneous Rays, in measuring Apertures of Microscope Object-glasses. By F. H. Wenham, Vice-President R.M.S.—On the Construction of the Dark or Double-bordered Nerve Fibre. By Dr. H. D. Schmidt, of New Orleans, U.S.A.—The Theory of Immersion. By Rev. S. Leslie Brakey, M.A.—On certain Beaded Silica Films Artificially Formed. By Henry J. Slack, F.G.S., Sec. R.M.S.—The Suctorial Organs of the Blow-fly.—By John Anthony, M.D., F.R.M.S.—On the Use of Black Shadow Markings and on a Black Shadow Illuminator. By Dr. Royston-Piggott, M.A., F.R.S., &c.—The Theory of Immersion. By Rev. S. Leslie Brakey, M.A.—On Bog Mosses. By R. Brathwaite, M.D., F.L.S.

Microscopic Structure of Irish Granites.—A paper has been published on this subject in the "Geological Magazine" by Professor Edward Hull, F.R.S. Two granites are described by the author, those of Co. Mayo and Co. Wicklow. He says of the first that the three slices which were prepared for him "show the general structure of the granite extremely well. With the 2-inch object-glass, and under polarised light, its constituents are brought out in their relative proportions; the most abundant mineral being orthoclase, next silica, then the triclinic felspar, then mica, and lastly magnetite." With regard to the silica, he says that, as usual in granites, it forms the basis in which the other minerals (felspar and mica) are imbedded. It is itself without crystalline form, receiving only the forms given to it by the sides of the felspar crystals; and with polarised light the boundary edges are often seen lined by narrow parallel bands of different prismatic colours. The interior portions exhibit, on rotating the analyser, the usual gorgeous shades of colouring, one colour sometimes imperceptibly shading off into another over the fields of view, like the blending of the colours in a rainbow. With the $\frac{1}{4}$-inch object-glass the silica is seen to be highly cellular, and fluid bubbles in some of the cells come into view. With the No. 2 eye-piece, magnifying 350 diameters, the bubbles are well developed, and appear to occur in most of the cells. Sometimes the cells are exceedingly irregular in form, sending out angular projections in various directions. The bubbles seem generally to occupy a large proportion of the cells, about one-third or one-fourth of the entire space, from which it might be inferred that the vapour from the condensation of which the bubble has been formed was not originally highly rarefied. Sometimes the cells occur in long slightly curved lines. Tubes and trichites are rare in the silica of this slice. With regard to the granites of Co. Wicklow, he says the investigations of Dr. Haughton leave little to be added in the way of analysis. Nevertheless, the microscope reveals the presence of a second variety of felspar besides the orthoclase, which he and Professor Galbraith were able alone to detect from chemical examination. This second mineral, indeed, might probably have remained unobserved, except for the introduction of this new process of investigation. The rock from which the thin slices are taken is finely crystalline granular, and consists of a siliceous paste, enclosing white felspar, silvery-grey and black mica. With polarised light the felspar is seen to consist of orthoclase and a triclinic felspar, less abundant but well defined. He says that the silica occurs in an amorphous state, enclosing the other minerals, as is usual in true granites; but is itself composed of numerous individual patches, each refracting the light differently so as to represent, on rotating the analyser, a great variety of rich colours. The individual patches are generally separated by a fringe of prismatic hues, probably due to varying thicknesses of the section at the edges. With the $\frac{1}{4}$-inch object-glass numerous cells of varying shapes and sizes come into view, together with some wonderfully long "trichites" ("trichiten," Zirkel), some straight, others bent or curved, and stretching through the mass in various directions. They are quite distinct from the tubes, and I have no good idea regarding their nature. The cells are often disposed in lines, or along planes, perhaps concealed cleavage-planes. The cells for the most part contain fluid bubbles, but they are only visible with a very high power; some, however, are "stone-cavities." Along with the cells and stone-cavities are also to be observed, with a high power, very remarkable straight tubes terminated by rounded ends, and evidently hollow or filled with gas. One of these, remarkable for its length, seems to contain a very minute bubble near the centre, and another at the end of the tube itself.

How to Examine the Growth of Plants with the Microscope.—A method for this purpose has been lately described in an American journal. The inventor employs a glass tube of convenient size, to be placed in the field of a microscope, and allows the root, or other part of the plant, to grow in this. Of course the part must be fixed at some point, either with cork or with damp bibulous paper. The free end of the root has now room for growth, either in water or in moist air—preferably the latter. The tube can be subjected to a known degree of heat by the use of Sach's hot-air chamber. The tube, having been fixed on the stage, can be accurately observed every few minutes, or after a longer time, a micrometer being all that is needed for determining distances. The errors which may result from these observations are frankly alluded to. This simple method is particularly adapted to the investigation of the effect of light on growth, as the whole apparatus is completely under control of the observer.

The Hairs of the Lower Mammals.—Dr. Hoffmann, in an essay on this subject, says that in the hair of the lower mammals we find generally the same three layers as in human hair, but differing to such a degree that, as a rule, a hair can be easily recognized as belonging to an animal. The cuticula in most animals has absolutely and relatively larger cells, which give the hair a characteristic appearance, as is seen especially well in the wool from sheep. A toothed or saw-like appearance of the contour of certain animal hairs depends upon the larger development and peculiar relations of the cuticular cells, whose points stand out so far from the hair that the latter has a feathered appearance, as in a field-mouse. Among animals the greater bulk of the hair is formed by the medullary substance, the cortical substance being only a thin layer; often, indeed, is reduced to a hem-like streak. This predominance of the medullary substance is seen best in the shaft of the hair; towards the end the cortical substance predominates, the medullary becoming thinner. Generally the cortical substance has the same structure as in human hair, and the same variety of pigmentation; in some animals, as the cat, rat, and mouse, the cortical substance is more translucent and of finer structure, resembling, under the microscope, a hyaline envelope of the medullary substance. The medullary substance in animals is an interesting study, differing greatly from the same layer in human hair. The cellular structure is generally very evident, without the employment of any reagent. The cells vary greatly in size and form.

PHYSICS.

Frost Striations in Mud.—The Rev. F. R. Goulding, in a communication to "Silliman's American Journal" for March 1874, asks the privilege of calling attention to a natural phenomenon which has long enlisted his interest and that of a few others, but which remains, so far as known, without explanation. It is that of slight but plainly marked striations of the soil after a freezing, looking as if a very light harrow had been drawn over it from north-west to south-east, leaving irregular furrows, varying from half an inch to an inch and a half in depth, and from centre to centre. His attention was first drawn to it in 1854, in Upper Georgia, during and after the thawing of an extensive and severe "black frost." After residing for ten years in a region where there were no *black frosts*, and of course no striations, he returned to the mountains of Georgia, and was soon reminded of former observations by seeing the soil of his garden very deeply marked the same way as before, during, or rather toward the close of a hard freezing. This place is about forty miles distant from the other, and the soil wholly different, that being limestone and this granite. These striæ invariably run from north-west to south-east, and this is so in shaded as in sunny places, and whether the wind at the time blew, or whether the air was still. They begin to appear before the frozen surface has thawed. Further, the direction is at right angles to the stratification of the country, the out-croppings of the rocks being here in a line from north-east to south-west. If this coincidence be connected with the cause of the striations, it is in some way beyond the ken of the observers. The writer would be glad to receive any facts that will prove whether any uniformity of direction is observable elsewhere, and that will, if so, lead to its explanation.

A Horizontal Pendulum.—In "Poggendorff's Annalen," C.L., p. 134, is described by Herr Zöllner a series of experiments with a form of horizontal pendulum of such surprising delicacy that it seems to open a wide and fruitful field for investigation. This instrument consists of a short horizontal rod suspended by a vertical piece of fine watch-spring, and carrying at one end a heavy leaden weight and mirror. To prevent the other end from rising, a second watch-spring is attached, and fastened below. The two points of support lie therefore nearly in the same vertical, and are equidistant, one above and the other below the pendulum. They are connected with the top and bottom of a vertical rod, which rests on a tripod, with levelling screws. If the two points lie in the same vertical, the weight will remain in any position; but if one of the levelling screws is slightly moved, the pendulum will assume a position of equilibrium around which it will vibrate if disturbed. It will act, in fact, precisely like a common pendulum, except that the effect of gravity has been greatly diminished, so that the time of vibration is increased. Its sensibility is of course readily varied by shifting the levelling screw. In the instrument actually employed, the pendulum weighed about 6 lbs., and when removed from its supports and vibrated vertically like a common pendulum, its time of oscillation was about ·25 of a second. The springs were about eight inches long, and the delicacy of the instrument was such that its vibrations were easily observed when the time was increased to thirty seconds, corresponding to a diminution of the force of gravity of 14,000 times.

The Effect on Induction Currents of the Condenser.—This is a paper which does not well bear abstraction. It appears in full in the "Comptes Rendus." While physicists generally admit that the spectral modifications, produced by introduction of a Leyden jar into the induced circuit, are due to variations of the temperature, and not to any particular alteration in the physical nature of the discharge, he yet offers some remarks in support of this view:—1. One may observe the thermal superiority of the condensed spark over the ordinary spark on comparing together the spectra obtained, under different conditions, by means of the same substance. 2. The effects of the condenser being due to increase of temperature, there is gradual passage from the spectra obtained with the aureole of the ordinary spark to those when a powerful Leyden jar is employed. 3. The action of the condenser does not appear the same in different spectra. 4. The different lines of the same spectrum are not always equally affected by the condenser. 5. The lines intensified by the condenser become nebulous and enlarged. 6. The broadening of narrow lines at high temperature is explained by the perturbations undergone by the molecular movements, when the forces applied are considerable. 7. The lines of emission of solid or liquid substances are nebulous. 8. It seems necessary to distinguish two kinds of continuous spectra proceeding from those of the second order, viz. (*a*), spectra, the lines of which are enlarged by increase of temperature; (*b*), those the lines of which owe their enlargement to the little freedom of the molecules. If, as it seems, there is sometimes a gradual transformation of the shaded bands of a spectrum of the first order into the narrow ones of one of the second order, it is in consequence of an increase of temperature. 9. The sparks of different induction-coils present spectral differences to the inequality of the temperatures developed.

Is a Circular or an Ordinary Compass the Best?—This is an important point, and it is decided in favour of the round one by M. E. Duchemin, who says that it has the following advantages over the ordinary form (see "Comptes Rendus," vol. lxxvii. p. 890):—1st. A magnetic power, for a given diameter, double that of a needle whose length is equal to this diameter. 2nd. The existence of two neutral points instead of one, which has the effect of maintaining the position of the two poles constant; the magnetism seems to be so energetically preserved, that even the strongest sparks of a Holtz machine do not cause any displacement of the poles of the magnet. 3rd. A more satisfactory means of suspending the magnet when it is well mounted and balanced by a plate of agate; it seems then to move as if placed in a liquid. 4th. An increase in sensibility of the magnet proportional to its diameter. 5th. The possibility of neutralizing the magnetism of the vessel by means of a second magnetic circle, changing the position by an amount calculated beforehand, and thus permitting the compensation of the compass before the sailing of the vessel. This idea was suggested by Captain D. Venie.

A Vitroscope turned into a Tonometer.—M. Terquem, says the "Chemical News," uses graduated tuning-forks with slides and a small lens (Lissajous). By moving the slides the rates of vibration can be varied throughout an octave. He has other tuning-forks with lenses, but ungraduated. One of the former (or standard forks) and one of the latter (or auxiliary forks) are fixed at right angles in the same horizontal plane, and may thus make horizontal and vertical vibrations respectively. Some powdered antimony is gummed to a branch of the auxiliary fork, and this, illuminated with a lamp, gives bright luminous points. The forks are first brought into unison, and then the variations of the elliptical curve (got from co-existence of the vibrations) when the slides are moved are noted, along with the number of beats in a given time. By a few further steps one is enabled to ascertain the absolute number of vibrations for each note.

Influence of the Magnet over Polarisation.—Professor Villari has a contribution *apropos* of this question in "Poggendorff's Annalen," which is thus briefly abstracted in "Silliman's American Journal." A beam of polarised light was passed through the poles of a powerful electro-magnet, and a glass cylinder interposed, which acted like a cylindrical lens. This cylinder could then be turned end over end with any desired velocity. When not magnetised, it of course produced no effect on the plane of polarisation, whether in motion or not. But when caused to revolve rapidly, the angle through which it turned the plane of polarization was considerably diminished, since in each revolution it remained in the axial direction too short a time to acquire its full magnetic polarity. To impart sufficient diamagnetic intensity to be perceptible by the change of plane, at least ·0012″ was required; while, to produce the complete effect, 0024″ was necessary.

How Physically to detect Thallium easily.—Mr. Crookes, in one of the series of papers on thallium which he is publishing in the "Chemical News," gives the following as a simple method:—A few grains of the ore are crushed to a fine powder in an agate mortar, and a portion taken up on a moistened loop of platinum wire. Upon gradually introducing this into the outer edge of the flame of a Bunsen gas-burner, and examining the light by means of a spectroscope, the characteristic green line will appear as a continuous glow, lasting from a few seconds to half a minute or more, according to the richness of the specimen. By employing an opaque screen in the eyepiece of the spectroscope, to protect the eye from the glare of the sodium line, thallium may be detected in half a grain of mineral, when it is present only in the proportion of 1 to 500,000. The sensitiveness of this spectrum reaction is so great that no estimate can be arrived at respecting the probable amount of thallium present.

New Arrangement of M. Becquerel's Sulphate of Copper Pile.—M. Trouve says, in the "Comptes Rendus," quoted in the "Chemical News," that the salts of copper and zinc are simply maintained in contact with the metals (of the same name) by the capillary action in rolls of paper. The pile is very portable, has the same electromotive force as an ordinary sulphate of copper pile of the same number of couples, and may act continuously for a long time if placed in a closed vessel to obviate desiccation.

What is the Opeioscope?—A late number of the "Lens" says that this is a new and simple instrument, suggested by Professor A. E. Dolbear, for the purpose of demonstrating the pulsations of sound. Take a tube of any material, from one to two inches in diameter, and anywhere from two inches to a foot or more in length. Over one end paste a piece of tissue paper, or a thin piece of rubber or goldbeater's skin—either will do. In the centre of the membrane, with a drop of mucilage, fasten a bit of looking-glass not more than an eighth of an inch square, with the reflecting side outward, of course. When dry take it to the sunshine, and with the open end of the tube at the mouth, hold the other end so that the beam of reflected light will fall upon the white wall, or a sheet of paper held in the hand; now speak, or sing, or toot in it. The regular movement of the beam of light with the persistence of vision, presents very beautiful and regular patterns, that differ for each different pitch and intensity, but are quite uniform for given conditions. If a tune like "Auld Lang Syne" is tooted slowly in it, care being taken to give the sounds the same intensity, a series of curves will appear, one for each sound and alike for a given sound, whether reached by ascension or descension, so that it would be possible to indicate the tune by the curves; in other words, it is a true phonautograph. By trial one can find some tone which causes the membrane to vibrate in a single plane, and of course a straight line will appear upon the screen. If, while the sound is continued, the tube be swung back and forth at right angles to the line, the sinuous line will appear, which may be either simple, representing a pure and simple sound, or it may be compound-sinous, showing over-tones, precisely as in König's manometric flames. With the lecture-room darkened, and using the beam of light from a *port lumière* or from a lantern, these may be projected of an immense size. There is no trouble in the world in making them eight or ten feet amplitude, or more if needed.

At a distance of but three or four feet, the curves will spread out to two or three feet in length when a tone is made to which the tube can reasonably respond.

ZOOLOGY.

Examination of Teeth of Cestracions.—Professor Agassiz says, in the last paper he wrote, which was one only recently printed by the "American Naturalist," "Let me here say that from single specimens of Cestracions, obtained in different parts of the world, have been indicated three supposed genera based on the conditions of the teeth at different periods of age. To show that this should not be accepted as an unquestionable result, let me say that I have examined the young of the three supposed genera. They are all provided with keeled molar teeth, while the adults have the flat grinders supposed to be characteristic of the Cestracion type alone. I am therefore satisfied that it is worth while to collect largely and preserve a number of specimens, even if they be sharks and skates and occupy a great deal of room, in order to learn their history, which has just shown of what importance has been the identification of teeth among fossils. Thus sharks drop their teeth, and scatter them along the bed of the ocean in great numbers, probably ten or twenty times as many as they have at one time while living, so that it is not to be wondered at that we so frequently find in collections of fossils loose teeth of sharks, and that we so rarely find the jaws of sharks with teeth in their places. Of course in those species in which the teeth are isolated and do not support one another, we should hardly ever expect to find them fossil in position; while those which are pressed upon one another may be found in the fossil state, and that occurs again and again, and among the fossil fishes there are a number of sharks in which jaws with teeth arranged in rows are represented."

Relation between the Colour of certain Birds and their Geographical Distribution.—A communication has lately been made to the Academy of Sciences of Paris, by Mr. Alph. Milne-Edwards, upon the relations existing between the colour of certain birds and their geographical distribution, having special reference to the fauna of Polynesia. His inquiries have embraced not only researches into the absolute fact of melanism in the way of black plumage, but also the degree to which this influence has modified the true colours. Referring to the fact that birds with black plumage are found in all parts of the globe, in certain families of wide geographical extent, he states that melanism is exhibited decidedly only in the southern hemisphere, and especially in the portion embracing New Zealand, Papouasia, Madagascar, and intermediate regions. Thus, in the swans, all the species of the northern hemisphere are white; in New Holland, however, there is a species that is entirely black; while in Chili and elsewhere in South America we have the Coscoroba swan, entirely white, with some of the quills black, differing in this respect alone from the allied species in China. Again, in speaking of the black parrots, Professor Milne-Edwards remarks that none of these are to be found black in America or

Asia, or in Africa excepting along the borders of the Mozambique Channel, though they are not rare in the more southern regions included in the limits mentioned, some of them being entirely black, and others with a gloss of this colour, such as to obscure the other tints.—*Silliman's Journal*, Vol. vi. No. 40.

The Honey-making Ant of Texas and New Mexico (*Myrmecocystus Mexicanus of Westwood*).—Mr. Henry Edwards gives the following account of these curious insects. He says the natural history of this species is so little known, that the preservation of every fact connected with its economy becomes a matter of considerable scientific importance, and the following observations, gleaned from Capt. W. B. Fleeson, of this city, who has recently had an opportunity of studying the ants in their native haunts, may, it is hoped, be not without interest. The community appears to consist of three distinct kinds of ants, probably of two separate genera, whose offices in the general order of the nest would seem to be entirely apart from each other, and who perform the labour allotted to them without the least encroachment upon the duties of their fellows. The larger number of individuals consists of yellow working ants of two kinds, one of which, of a pale golden yellow colour, about one-third of an inch in length, acts as nurses and feeders of the honey-making kind, who do not quit the interior of the nest, " their sole purpose being, apparently, to elaborate a kind of honey, which they are said to discharge into prepared receptacles, and which constitutes the food of the entire population. In these honey-secreting workers the abdomen is distended into a large, globose, bladder-like form, about the size of a pea." The third variety of ant is much larger, black in colour, and with very formidable mandibles. For the purpose of better understanding the doings of this strange community, we will designate them as follows:—(1) Yellow workers; nurses and feeders. (2) Yellow workers; honey makers. (3) Black workers; guards and purveyors. The author then proceeds at length to describe them in the "Proceedings of the California Academy of Sciences," Vol. v., Part 1. The site chosen for the nest is usually some sandy soil in the neighbourhood of shrubs and flowers, and the space occupied is about from four to five feet square.

Rissa Tridactyla, an Aleutian Bird.—The description of this bird is of interest. It is by Mr. W. H. Dall, and appears in the "Transactions of the California Academy of Sciences." He says that the nest, eggs, and young in down, were all obtained about July 11, 1872, at Round Island, Coal Harbor, Unga Island, Shumagins. They were also common at Delaroff Harbor, Unga, and seen at Kadiak, but not at Unalashka, or to the west of Unimak Pass. On entering Coal Harbor, Unga, we were at once struck with a peculiar white line which wound around the precipitous cliffs of Round Island, and was seen to be caused by the presence of birds; and as soon as opportunity was afforded, I took a boat and went to the locality to examine it. The nests, in their position, were unlike anything I had ever seen before. At first it appeared as if they were fastened to the perpendicular face of the rock, but on a close examination it appeared that two parallel strata of the metamorphic sandstone of the cliffs, being harder than the rest, had weathered out, standing out from the face of the cliff from one to four inches, more or less irregularly. The nests were built where these broken ledges afforded a partial support, though extending over more than half their width. The lines of nests exactly followed the winding projections of these ledges everywhere, giving a very singular appearance to the cliff, especially when the white birds were sitting on them. The nests were built with dry grass, agglutinated together and to the rock in some unexplained manner; perhaps by a mucus secreted by the bird for the purpose. The nests had a very shallow depression at the top, in which lay two eggs. The whole establishment had an intolerable odour of guano, and the nests were very filthy. The birds hardly moved at our approach, only those within a few yards leaving their posts. I reached up and took down two nests, one containing two young birds, and the other empty. Wind coming up, we were obliged to pull away, and the bird, which came back, lighted on the rock where her nest and young had been, with evident astonishment at the mysterious disappearance. After flying about a little, she again settled on the spot, and suddenly making up her mind that foul play on the part of some other bird had taken place, she commenced a furious assault on her nearest neighbour. As we pulled away, the little fellows began to be affected yb the motion of the boat, and with the most ludicrous expression of nausea, imitating as closely as a bird could do the motions and expressions of a sea-sick person, they very soon deposited their dinner on the edge of the nest. It was composed of small fishes or minnows, too much disorganised to be identified. Eggs, in a moderately fresh condition, were obtained about the same time, but most of them were far advanced toward hatching. These birds are very curious, and scouts are always seen coming from a flock of them when a boat or other unusual object approaches. These scouts very soon return to the flock if not molested, aud the whole flock then proceeds to investigate the phenomenon.

Cuttle-fish.—The following few memoranda may be acceptable in reference to the "Cuttle-fish" paper of April last. They are sent to us by Major-General Nelson, of Stoke, Devonport:—"On my way to Bermuda, 1827, the crew caught a shark of some size; in its stomach we found a portion of the arm of a creature of this description; but whether Poulpe, &c. &c., I was not then able to determine. The said 'portion' was about 4 feet long, 4 inches in diameter at one end, and 3 inches at the other. Both ends were sharply cut off; and there was no appearance of decomposition by digestion, which might lead to the supposition that the locket-shaped appendages were suckers deformed by that process. I have a small carefully-sketched Sepioteuthis, done some time afterwards, the suckers of which agree very well in form with those shown in the Plate, p. 122.

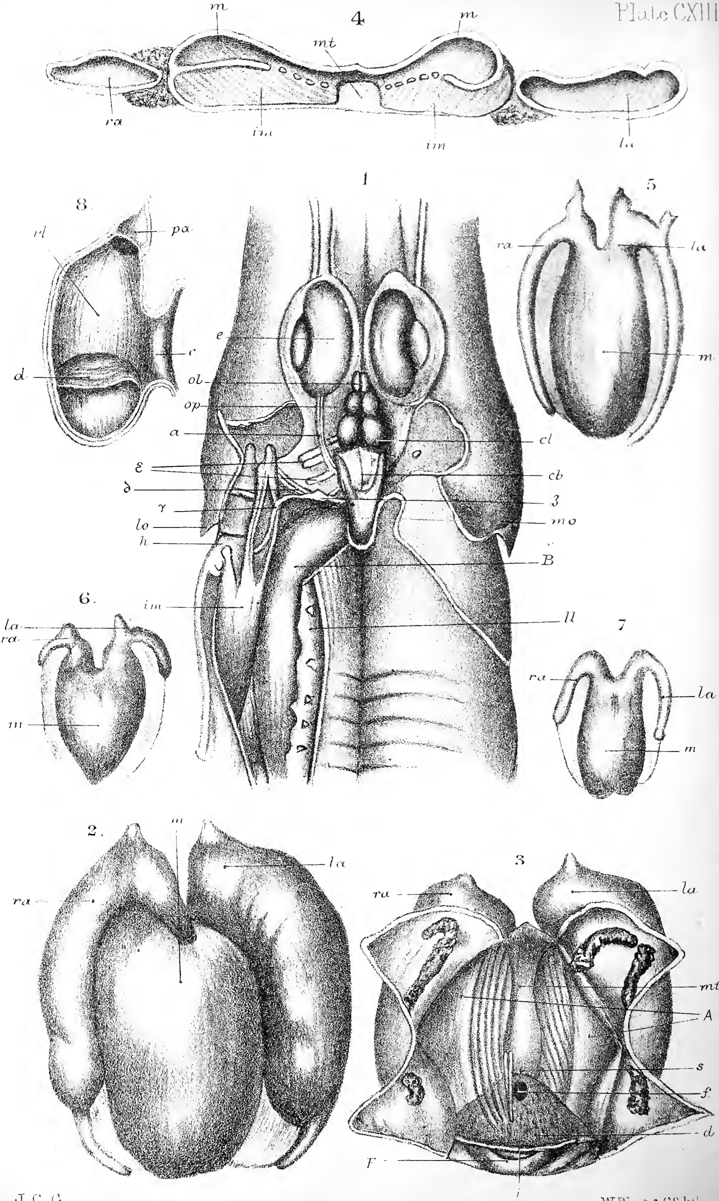
"As well as I can now remember, these locket- or heart-shaped processes were about $1\frac{1}{2}''$ long, and hung by means of a short neck, some 3 or 4 inches from the arm; the substance of this last being of the usual greyish-white matter, reminding one of the *hard*-boiled albumen of a duck's egg, as far as colour is concerned.

"The movements of the above-mentioned Sepioteuthis are wonderfully fleet, as it flits among the coral-reefs, just touching the white sand with the end of an arm; and then, on alarm, discharging a cloud of sepia, and, under cover thereof dodging back in quite an opposite direction, with the quickness of lightning.

"I had followed one of these animals—or a congener thereof—to a small hole in a rock. In vain I solicited him to come out by gentle pulls with a boat-hook; at last, when they were harder than he liked, he discharged the contents of his sepia-bag all over me, and spoilt my waistcoat and trowsers completely—that dye requiring no mordant, and being, as far as I know, indelible."

The Anatomy of the genus Phronima.—In an interesting paper of the Royal Society, published in the "Proceedings" for March, Dr. J. D. Macdonald enters into a minute account of this singular crustacean, which is very excellently illustrated in the plate which, singular enough for the Proceedings of the Royal Society, accompanies the paper. The case which accompanies the animal seems most strange. Dr. Macdonald says on this point that a specimen was taken in the towing-net, but with the addition of a numerous progeny of young in a large gelatinous but tough nidamental case. This interesting nest was shaped like a barrel, but with both ends open, and the external surface was somewhat tuberculated and uneven. The wall of the tube presented numerous round and puckered openings, observing no very definite arrangement, but through which entering currents were observed to pass. These openings generally, though not invariably, pierced the tuberculations. An external membrane, with an internal lining, were distinctly visible, both seeming to be continuous at the rims of the tube. The space between these layers was filled up with a pulpy substance, in which scattered nucleiform bodies were detected with a higher power of the microscope. I have been particular in the description of the case, as some far-fetched guesses were made as to its real nature. The cutting, piercing, and tearing implements of *Phronima* would very soon alter and reduce a bell-shaped Medusa, a Salpian, or a Pyrosoma tube to the required pattern; for there is usually a great uniformity in the character and appearance of this case.

A Revision of the Echini has been conducted by Mr. Alexander Agassiz. Part III. of his work, quarto, which has forty-five plates, has quite recently been issued. Professor S. E. Verril thus notices its appearance in a late number of "Silliman's American Journal." He says that this excellent work is profusely illustrated by unique plates, a large part of which have been made by different photographic printing processes, directly from photographs of the specimens, and are of unrivalled excellence. The Woodbury-type process, the Albert-type, and the Heliotype, have all been successfully employed, while superior lithographs have also been used to some extent. Part III. contains detailed descriptions of all the known species, except those of the east coast of North America, which were described in Part II. Such species are, however, referred to in their proper systematic places. Twenty-eight plates illustrate Part III.; the remaining seventeen relate to structure, and belong to Part IV., but are issued in advance of the text, owing to the loss of the MSS., drawings, and some of the plates, by the great Boston fire in November, 1872.



J.C.G.

W. West & Co lith.

Musical Organs of Fishes.

THE SONG OF FISHES.

By JOHN C. GALTON, M.A., F.L.S.

[PLATE CXIII.]

"Un vrai vagabondage musical qui saisit par sa nouveauté, et est tellement attrayant qu'on l'entend avec plus de plaisir, ou du moins avec plus d'étonnement, que ces excentricités musicales que l'Allemagne a cherché dans ces derniers temps à importer chez nous."—DUFOSSÉ, 1874.

M. DUFOSSÉ, though in the above sentence unmistakably declaring himself no disciple of the composer of *Tannhäuser* and *Lohengrin*, and showing himself a far from promising proselyte as far as "the music of the future" is concerned, and though thus sadly assimilating his musical taste to our British standard—such as it is—is, nevertheless, entitled to be heard with respect, at all events so long as fishes are under consideration, seeing that he has for a long period had several hundred of these cold-blooded musicians under constant observation.

That certain fishes produce at certain seasons sounds—nay, more, that many such sounds can be brought under the category of musical notes—is known but to few even in these our days, though the fact did not escape the notice of that most observant of all natural historians, Aristotle;* and that which he thought and wrote in Greek on this subject has, of course, as in other things, been echoed some centuries later by Pliny in Latin.†

More recently recorded observations upon the sounds produced by fishes are but few and far between. One of the best perhaps of all accounts is that given by Sir J. Emerson Tennent, late Governor of Ceylon.‡ When at Batticaloa

* "Ψόφους δέ τινας ἀφιᾶσι καὶ τριγμοὺς οὓς λέγουσι φωνεῖν, οἷον λύρα καὶ χρόμις· οὗτοι γὰρ ἀφιᾶσιν ὥσπερ γρυλλισμόν· καὶ ὁ κάπρος ὁ ἐν τῷ Ἀχελώῳ ἔτι δὲ χαλκεὺς καὶ κόκκυξ." "Hist. Anim." iv. 9, 3.

† "Nat. Hist." lib. xi. Ælian, too (Περὶ Ζωῶν Ἰδιότητος, lib. x. cap. ii.), quotes Aristotle's statement, but adds nothing new to it.

‡ "Sketches of the Natural History of Ceylon," pp. 380–85 and 401. London: 1861.

—a place half way down the east coast of this island—he made some inquiries about certain sounds "resembling the faint sweet notes of an Æolian harp," which were alleged to proceed from the bottom of a neighbouring lake. The fishermen said that both they and their fathers knew of these sounds, which were declared to be audible during the dry season, but to cease when the lake had been swollen after the rains. These, they said, proceeded not from a fish, but from two species of mollusc (a *Littorina* and a *Cerithium*), known by the Tamil name of *oorie cooleero cradoo*, or the "crying shell." * Sir E. Tennent took a boat and visited the lake by moonlight, and thus describes the sounds which he heard:—"They came up from the water like the gentle thrills of a musical chord, or the faint vibrations of a wine-glass when its rim is rubbed by a moistened finger. It was not one sustained note, but a multitude of tiny sounds, each clear and distinct in itself: the sweetest treble mingling with the lowest bass. On applying the ear to the woodwork of the boat the vibration was greatly increased in volume." The sounds varied considerably at different points, and could be localised, as it was possible to row away out of their influence. This fact, thought Sir E. Tennent, lends support to the view of the fishermen, that the sounds were produced by molluscs and not by fish. Similar sounds have been heard in the harbour of Bombay; described as "like the protracted booming of a distant bell, the dying cadence of an Æolian harp, the note of a pitch-pipe or pitch-fork, or any other long-drawn-out musical note." These sounds came from all directions, almost in equal strength, and arose from the surface † of the water all round the vessel. The fish which was alleged to produce these sounds closely resembled in size and shape the fresh-water perch of the north of Europe. These phenomena were carefully observed and noted by a party of five intelligent persons.

The "Magoora"—a fish found in the lake at Colombo—is stated by the fishermen to make a grunt when disturbed under water; and a certain flat-fish in Siam, according to Pallegoix, "fait entendre un bruit très sonore et même harmonieux."

At Caldera, in Chili, at the mouth of the Pascagoula, in the Mississippi State, and of the "Bayon coq del Inde" river on

* It is known, from the observations of the late Prof. Grant, that one at least of the gasteropodous molluscs (*Tritonia arborescens*) has the power of producing sounds—apparently by the mouth, which is armed by two horny plates—so that it is possible that the sounds in question were really produced by molluscs; a point on which Sir E. Tennent was not able to satisfy himself.

† It should be noted that the sounds heard by Sir E. Tennent "came evidently and sensibly from the depth of the lake."

the north shore of the Gulf of Mexico, similar submariue sounds have been remarked, but by what animal produced is at present unknown. Darwin, moreover, mentioned as occurring in the Rio Parana, in South America, a kind of *Silurus*, called "armado," which is remarkable for a harsh grating noise, which it makes when caught by hook and line, and which can be distinctly heard when the fish is beneath the water.*

The most graphic and analytic description, however, of such music is that given by M. Dufossé, who thus describes his sensations when traversing in a fishing-lugger off the coast of France a shoal of Maigres (*Sciœna aquila*), so closely packed together as to be literally "côte-à-côte"—

"Tout à coup et tandis qu'une multitude de sons mystérieux, baroques, d'un charivari inouï, frapperont l'oreille du naturaliste, il se sentira saisi d'une sorte d'enivrement passager durant les courts instants duquel il aura bien de la peine à se défendre de quelques hallucinations auditives; toutefois, redevenu observateur impassible, il ne tardera pas à constater que les parois du bâtiment qui le porte sont animées de mouvements vibratoires, et dès lors il distinguera nettement, que c'est le tremblement physique qu'il ressent qui produisait le trouble nerveux auquel il a été un moment en proie, et par suite il trouvera le secret du léger degré d'enivrement qu'il a éprouvé dans la triple nouveauté des sensations qui sont venues inopinément et simultanément envahir tout son être: nouveauté de la surexcitation nerveuse résultant des mouvements de trépidation du chasse-marée; nouveauté encore de la nature même des sons étranges qui fascinaient ses organes auditifs; nouveauté enfin du mode de transmission des vibrations sonores qu'il percevait à travers un milieu liquide." Further on the noises are thus described:—"Ces assemblages de sons extraordinaires, bourdonnant comme le feraient un grand nombre de jeux d'orgues" (shade of Charles Babbage!) "qui seraient complètement désaccordés, cacophonie d'une bizarrerie indescriptible, auxquels tous les Sciénoïdes du groupe auront pris part," &c.

M. Dufossé has further been informed by some pilots, whose testimony he considered reliable, that a sea captain who was going up the Gironde, on hearing for the first time the sounds produced by numerous maigres in the neighbourhood of the ship, was thrown into a state of great alarm, supposing that he had sprung a leak in the hold!

Though phenomena such as those just described have been from time to time observed, wondered at, and noted by more or less competent witnesses, it was not until within the last fifteen years that any attempt had been made to inquire into their nature, and to investigate by patient and closer observation, and by carefully conducted experiments, the organ or organs by which they are produced. Until quite recently, then, all had been merest conjecture. It is to two French observers,

* "Naturalist's Voyage Round the World," p. 136. Lond.: 1860.

M. Moreau, in some slight degree, but more especially to the oft-repeated and most laborious observations and experiments of M. Dufossé, carried out upon several hundreds of fishes, mostly inhabitants of the Mediterranean, that we owe all our present information—which, it must be admitted, is wonderfully full and exact—on this hitherto obscure and totally neglected subject.

It has long been known that many members of the Gurnard family possess and exercise the faculty of emitting sounds when still under water, from which circumstance they have been collectively termed "Organo" in Italy, and in France "Grondin." * M. Moreau describes the air-bladder of the "Tub-fish" or Sapphirine Gurnard (*Trigla hirundo*) as possessing thick and strong muscles, the fibres of which are of the *striped* variety, and are thus presumably voluntary. These are supplied by two large nerves which take origin from the upper part of the spinal cord, below the pneumogastric nerve, and close to the first pair of dorsal nerves. The mucous membrane lining the air-bladder is thrown into a fold or diaphragm (comp. *d*, fig. 2), which subdivides the main cavity into two secondary chambers, which communicate by an aperture (*f*, fig. 2) in this partition, having some functional analogy to the pupil of the eye; for under the microscope this structure is seen to be provided with sphincter-like muscular fibres, disposed concentrically to the opening, while other fibres, radially arranged, run at a tangent to these. Both sets of fibres are of the *smooth*, presumably involuntary, variety. In August 1863 M. Moreau "sacrificed," as he terms it, a grondin by section of its spinal cord above the dorsal region, and, after opening the abdomen of the fish, he applied a feeble galvanic current to the nerves proceeding to the air-bladder. Immediately there were produced sounds, audible to persons at some distance, having the same character as those emitted by the fish during life. A current was next applied directly to the muscle of the air-bladder, but without result. M. Moreau then cut a window in the lower portion of the bladder, so as to expose the diaphragm to view, and upon galvanisation being again repeated, this membrane was seen to be thrown into a state of vibration, but no sounds were produced. M. Moreau, who does not seem to have been satisfied with these results, then proposes to continue his experiments at some future time.† The absence of sound in the last experiment seems to me to be easily accounted for, seeing that the membranous cavity, more

* The λύρα of Aristotle probably embraced fishes of this family. Yarrell thinks that the most probable derivation of the word gurnard is from the Dutch *guurheid*, roughness, in allusion to the peculiarity of the head of this fish. ("Hist. Brit. Fishes," 3rd edition, vol. ii. p. 106.)

† Sur la voix des Poissons. "Comptes Rendus," tome lix. p. 436. 1864.

or less distended with a gas to which impulse would have been transmitted by the vibration of the diaphragm, had now been opened.

So much for M. Moreau. The rest of this article must perforce be devoted to the admirable researches of M. Dufossé, whose observations and experiments have been so numerous, so carefully conducted, and so productive of valuable results, that this *savant* is at length enabled to reduce to system and classify—an all-important step in any branch of science—the various acoustic phenomena which he has observed among fishes.* Such phenomena may be divided into two primordial groups or "categories." Under the first of these may be placed the various sounds which fishes produce when taken off the hook and line, and are pitched into a basket or some other receptacle. Such sounds are accidental, temporary, for the most part evidently involuntary; often convulsive, being produced sometimes by one part of the organism, at another time by another part. Such sounds are subservient to the exercise of a function which cannot be expressed, and cannot be brought into relation with any intention on the part of the animal. Among such noises are those produced by unusual movements of the bony elements of the jaw or gill-coverings ("opercula"), e.g. in the barbel, loach, carp, gurnard, and others. In the short-snouted variety of the sea-horse (*Hippocampus*) a peculiar sharp sound is made by a little chevron-shaped bone, loosely articulated with two of the bony (*preopercular*) elements of the gill-covering, resembling that produced by the sudden return of a displaced foot tendon into its bony groove. The tench, carp, loach, and other thick-lipped fish, make a peculiar noise if they be compelled suddenly to open the mouth. This in the tench is so often repeated as to be in a degree comparable with the croaking of a frog. To such sounds M. Dufossé gives the name of "phénomènes accoustiques irréguliers."

With regard to the sounds of the second category, which "better merit the attention of the physiologist," these are voluntary, constant, and are always produced by the same organ. They are, moreover, always reproduced under analogous circumstances, are evidently intentional, and can even serve to characterise a species. Such are the "phénomènes acoustiques réguliers." The phenomena of this category are further divided by M. Dufossé into groups or sections. The first of these comprises "expressive noises, or incommensurable expressive sounds." As the noises are not all engendered by the same

* Recherches sur les bruits et les sons expressifs que font entendre les poissons d'Europe. "Annales des Sciences Naturelles," 5ième serie, Zoologie, tome xx. 1874.

mechanism, it is necessary to subdivide them yet further into two secondary groups or "divisions." The first division includes all the expressive sounds of a harsh nature, and comprises, as far as the fishes of Europe are concerned, only one subdivision —that of stridulation, having for its cause the friction of the dental organs. Of souch sounds—"bruits de stridulation"— there are two modes of causation.

a. By friction of the pharyngeal bones.* These noises are characterised by being composed of sonorous emissions, clear, short, rough and piercing, without flexibility or softness, and by commencing and ending abruptly ("brusquement"). The best example of this has been found in a species of mackerel, namely the "Saurel" (*Scomber brachyurus*, Linn.), known in the fish-markets of Paris under the name of "Maquereau bâtard," and by that of "Séveran" on the coasts of old Provence. Both the males and females are equally sonorous, and especially so in the hottest part of summer; and, moreover, present this advantage to the physiologist, that they will live for more than ten minutes—on rainy days for even sixteen or seventeen—after removal from the water, without seeming to suffer. M. Dufossé made several experiments with the saurel, and found that puncturing the air-bladder or other viscera had no influence on the sounds emitted, while, on the other hand, they entirely ceased when various substances, such as bits of kid glove, had been stuffed between the pharyngeal teeth—those *dents-en-velours*, as Cuvier termed them, which, like a gin, jealously guard the approaches of the gullet. It must be noted that the branchial arches differ from those of most of the mackerel family in having their mucous lining not clothed with a softish cartilaginous cushion, but encrusted inside with calcareous plates, and carrying tooth-like organs of the hardness of enamel. The various muscles, too, of the hyoidean apparatus (that which mainly influences the movements of the bones carrying the lower pharyngeal teeth) are relatively largely developed. It was further found that when the fish was examined in a vessel filled with sea-water, the sounds emitted were not accompanied by the liberation of a single bubble of gas from any of the natural openings of the body, nor did the fish come to the surface to swallow the least mouthful of air.

b. By the friction of densely hard prominences from the jaws, playing the part of intermaxillary teeth, noises being thus produced which resemble the grinding of the teeth of pigs, or of certain ruminants. Only one fish is as yet known to employ such mechanism, namely the Sun-fish (*Orthragoriscus mola*), which

* These bones are described and figured in an article, "How Fishes Breathe," POPULAR SCIENCE REVIEW, Oct. 1871.

has two hard prominences, one on each jaw, fulfilling the function of intermaxillary teeth.

We now come to the second division, which comprises all kinds of blowing sounds, "bruits de souffle." Many fishes produce such sounds, among them being the carp tribe and the *Silurus glanis;* but the most remarkable effects have been noticed in the loach (*Cobitis*), the barbel, and the carp. All these fishes have an air-bladder provided with a duct, which communicates with the gullet, and which is, moreover, divided, in the carp tribe, into two chambers, which, however, communicate, by a transverse constriction. In the loach this organ lies in front of and out of the abdomen, in a box formed for it by bony plates derived from the sides of the second and third vertebræ. According to the researches of Weber, which have been confirmed by Bréchet and others, the air-bladder in the barbel and loach is brought into relation with the organs of hearing through the medium of a chain of bones, so that the slightest vibrations of the wall of the bladder can be transmitted to them. Weber, in consequence, then, regarded the air-bladder as an organ for the reinforcement of the sounds transmitted to the body of the fish by the surrounding medium. From experiments made upon the barbel and "meunier" (*Cyprinus dobula*), M. Dufossé concludes—

α. That the sounds emitted by these fish are voluntary, because the animal can open or close at pleasure little valves in the duct of the air-bladder, which control the escape of gas from this receptacle—an act essential to the production of sound.

β. That the function of the air-bladder and duct, in addition to any other which they may discharge in common with these organs in other fishes, is "to produce a certain quantity of gas, and to expel the same with the speed necessary for the formation of sounds of expression;" and that the principal agent in the propulsion of this gas is, through its anatomical relations, the posterior lobe of the air-bladder. The sounds emitted by the loach have a greater intensity, and present greater varieties.

We now come to the second and most important section of the second category. This includes sounds having the following character. Their *timbre* is more or less sweet and soft, and never excites such sensations as are produced by the grinding of teeth. It is, moreover, subject to an extraordinary degree of change, varying frequently, and even changing during the extent of a sound. Such sounds can be appreciated musically; are, in other words, "commensurable."

Let the reader place a finger in each ear, and then "set his teeth" hard. After hearing a dull low murmur, like the rumbling of a distant chariot, he may possibly exclaim, in the language of Catullus—

> "—— sonitu suopte
> Tintinant aures."*

Not so. Such sound is of a totally different kind. The sound in question is due to a vibration caused by the contraction of his temporals and masseters—those "aldermanic" muscles, as we believe they have been termed—and has been investigated by many observers, among them the celebrated Wollaston, and has in consequence received many names, e.g. *Wollastonian vibration*, *Agitatio spiritum* (Grimaldi), *Bruit de rotation* (Lænnec), *Trémulation musculaire* (Dugès), &c. Wollaston essayed to count the vibrations of these sounds, and found only from 14 to 36 in a second, so that they can hardly be regarded as "commensurable," i.e. musical sounds, if Dupré's recent conclusions be correct, that a sound composed of less than 32 vibrations per second cannot be appreciated musically. Now M. Dufossé has discovered that in many fishes the sounds produced by them are essentially of an analagous nature, and that the vibrations into which these may be analysed can be measured by appropriate instruments. Further than this, he has shown that there are two methods of the causation of such sounds—1, by the contraction of muscles lying in close contiguity to the air-bladder, so that the latter fulfils the office of an instrument of reinforcement of sound, in other words, a kind of sounding-board; 2, by the contraction of muscles which are part and parcel of the air-bladder itself. So then this latter may be regarded *in toto* as an instrument of music, and not merely as playing a secondary *rôle*. The Mailed Gurnard, "Marlamat" (*Trigla cataphracta*, Linn.), offers a good instance of the first of these methods. In the abdomen of this fish, arched over by the ribs and lying within the so-called "lateral" muscles of Cuvier, may be seen on either side a muscle (*i m*, fig. 1) which runs along the whole length of this cavity. This muscle is attached posteriorly to certain fibrous internal aponeueroses, and, after increasing in size and becoming cylindrical anteriorly, splits into two slips, the shorter of which is attached by a tendon to the so-called "humeral" element of the pectoral fin (*h*, fig. 1), while the other terminates at the back of the skull. These muscles are further conspicuous by their red colour, have moreover the characters of voluntary muscles, in that their ultimate fibrils are transversely striped, and are supplied by special branches from the third pair of cervical nerves (β, γ, fig. 1)—nerves which in other gurnards pass to the "intrinsic" muscles (*i*, *m*,

* "Upon my ear a noisy nothing rings.' —Keats (*Endymion*).
βομβεῦσιν δ' ἀκοαί μοι.—Sappho.

fig. 4) of the air-bladder.* M. Dufossé has established the curious fact that, in the majority of cases, it is not the totality of the fleshy bundles of the intracostals which contract to produce sound, but only that portion of the muscular surface which is in immediate contact with the air-bladder; and that, under these circumstances, whatever organs, whether bony or otherwise, are acted upon by these muscles, come only into play as accessories to the production and propagation of sound.

Let us now briefly consider the second of the two methods of the production of "commensurable" sounds. Here the air-bladder is itself "a *generator* of sounds, as completely independent of the rest of the organism of the fish as any other apparatus of 'psophosis,'† or even of phonation with which the animal may be endowed." After placing a gurnard on its back, making a long incision in the abdominal walls, and carefully drawing aside any viscera which may obstruct the view, if the tip of a finger be held in contact with the air-bladder, vibration will be felt exactly synchronous with, and having the same intensity as the sounds produced by the fish. This can be further proved by means of a stethoscope applied to this organ. Further than this the air-bladder will be seen, during the emission of such sounds, to be affected by movements which may either throw the organ into folds or subject it to a greater tension in various parts; and this even to such a degree as somewhat to alter its general shape. Having isolated the organ as much as possible by delicate yet rapid manipulation from the rest of the body, with the exception of the vessels and nerves which pass to it, let a stethoscope, provided at its mouth with a diaphragm of gold-beater's skin, be applied to the anterior part of the organ; then let the nerves which pass to the latter be severed, first on one side and then on the other, when it will be found that the sound first decreases in intensity, and finally ceases altogether. From this and other experiments M. Dufossé concludes that the air-bladder, in the majority of the gurnard family—

a. Is a physiological organ, which, whatever may be its other functions, is a *generator* of sounds.

β. That its "intrinsic" muscles, by their vibration, aided and intensified by the rest of the organs, are the agents of such sounds.

* The anatomist Stannius mentions, among other branches of the pneumogastric nerve, certain which run "inter membranas vesicæ natatoriæ. Inde ab œsophago in ductu ad vesicam decurrentes hanc ipsam adsequuntur. *Fibræ hæ nerveæ omnes colore niveo ceteris excellunt.* (*Symbolæ ad Anatomiam piscium.* Rostochii: 1839.)

† This is a word coined by Dugès. It appears to be derived from ψόφος (Lat. *strepitus*), any articulate sound, as opposed to φωνή.

γ. That other muscles, by their contraction, can alter the shape of the organ, and thus modify the quality of the sounds emitted.

What part the internal partition, or "diaphragm" (see figs. 3, 8, *d*), takes in modification of sounds does not seem to be clearly established, except that, in the maigres, at any rate, where it is fairly developed, it does no more than play a very secondary part—"un effet bien accessoire, bien peu important dans l'emission de ces phénomènes acoustiques." Space unfortunately will not permit us to consider the interesting modifications of the air-bladder, and the concomitant variations in vocal phenomena deducible therefrom, which are met with in the maigre, umbrina, the dorées and the dactylopterus; but mention must not be entirely omitted of the fact that in one of the sea-horses (*Hippocampus brevirostris*) the mechanism of the production of sounds is reduced to its simplest expression, being merely the vibration of voluntary muscles reinforced by an air-bladder having neither duct nor diaphragm, nor "intrinsic" muscles, both sets of organs being no better developed than in fishes which do not produce any sound whatever.

As space further fails us for a proper review of the gamut of the piscine orchestra, we must content ourselves with one example. We will take the maigres, a description of whose musical performance has been already quoted at the beginning of this article. The sounds emitted by these fishes are notable principally for their length, having a mean of 25 seconds, and for their uniformity, "qui va jusqu' à la monotonie la plus fatigante." The *timbre* varies very much, the most common being that of a common reed-organ or the reed of a flageolet. Another pretty fequent *timbre* resembles that of the largest string of a violoncello, sometimes passing to that of the *bourdon* of a contre-basse. Some sounds are, however, less sweet, and may have some likeness to the tone of a hurdy-gurdy or rattle, while others are clear and pure, resembling in their *timbre* those produced by a hautboy, harmonica, or accordéon. M. Dufossé would limit the range of sounds produced by the maigres, from the most acute to the deepest, to three or four tones. They have generally a great tendency "to degenerate into a humming sound," either from an excess or from a want of intensity.

It would have been interesting, had the limits of this article permitted it, to have considered more fully the phenomena of sound just described, from a musical and physical point of view; but as the subject has in these pages been regarded rather from a biological stand-point, I would fain leave the more mechanical part of it—one fraught with great interest, and most fully and ably discussed by M. Dufossé—to the consideration of the physicist and scientific musician, for "la vibration musculaire," as this writer well observes, "attend encore son historien ; le savant qui, au moyen de recherches expérimentales assez multipliées, pour faire une étude bien approfondie, bien complète de ce fait naturel, l'élevera au rang des phénomènes les plus intéressants de la biologie."

It appears that out of more than 3,000 species of fishes no more than 52 are at present known to produce sound. This contrasts most singularly with that which happens among the other four vertebrate classes, containing at least 12,000 species ; for here every individual possesses a larynx—in other words, an organ of voice—and out of these those that are incapable of exercising the functions of this organ are in a very small minority.

Not only is there every reason to believe that the majority of sounds produced by fishes are not casual utterances, but are truly voluntary ; but there is among such as give vent to them a most remarkable development of the organs of hearing in all essential particulars—e.g. in the semicircular canals, otoliths, and nerves*—correlative with the degree of perfection of the instrument. Further than this, as the sounds generally excel in frequency and intensity at the breeding season, it will not be unreasonable to regard them—granting, as we do, that the chirp of the cricket and the croak of the frog is each in its way an alluring serenade—as nuptial hymns, or, to use language ascribed to Plutarch, as "deafening epithalamia."† More than this ; seeing that the carp, and others of the same family, have given unmistakeable proofs of their aptitude to receive some rudiments of education, and in particular to perceive certain sounds, it can yet be possible that the moral admonitions of a St. Anthony of Padua—by many still regarded as a work of supererogation—may, no less than the amorous twang of the vesical zither, after all not have fallen upon totally deaf ears.

* See Retzius' "*Anatomische Untersuchungen.* 1*ste Lief.* 1*ste Abth:* Das Gehörlabyrinth der Knochenfische" (Stockholm, 1872); and the beautiful preparations, made, we believe, by Mr. Charles Stewart, the Curator, in the Museum of St. Thomas' Hospital, London.

† M. Dufossé suggests that the song of the fabled Sirens had its origin in the utterances of shoals of maigres. It is probable that the *latus*—that "marvellous morsel," as Athenæus termed it, caught in the "fretum Siculum" to garnish the tables of Roman epicures, was, as Rondelet and Cuvier suggested, none other than the maigre.

DESCRIPTION OF PLATE CXIII

All the figures have been selected and drawn by the author from plates illustrating M. Du Fossé's article in the "Annales des Sciences Naturelles."

FIG. 1. View of the upper surface of the front part of the body of the Mailed Gurnard, or "Malarmat" (*Trigla cataphracta*). A large portion of the skull and of the front part of the spine has been removed; also all the "great lateral" muscles, both superficial and deep, at the interior end of the body, and, on the left side, all the layers of muscles, and the ribs as far as the dorsal arch of the abdomen. A portion of the membrane lining the abdomen is retained, to which are attached the "intracostal" muscle. The whole of the posterior half of this muscle has been separated from its attachments and reflected, in order to demonstrate more readily the course which the entire muscle describes. *l o.* Left gill-cover, or "operculum." *o l.* Olfactory lobe of brain. *o p.* Optic lobe. *c l.* Cerebral lobe. *c b.* Cerebellum. *m o.* Medulla oblongata, or first part of spinal cord. *α.* Two cerebral nerves belonging to the fifth pair. *β.* The three roots (of which only two are represented in the plate) of the nerve destined for the "intracostal" muscle, and the ganglion developed upon them outside the vertebral column. On dissecting this ganglion, it is found that the nerve despatched to the muscle is a continuation of the largest — the most anterior, of the three roots. *γ.* The nerve of the "intracostal" muscle after its emergence from the ganglion. *δ.* Two nerves arising from the ganglion, on their way to the "operculum" and neighbouring parts. *ε.* Three rootlets of the cerebral nerve of the eighth pair. *h.* A portion of the "humeral" bone (Cuvier)—probably one of the "clavicular" bones of more modern nomenclature, a little separated from the "operculum." *i m.* Left "intracostal" muscle, the posterior flattened portion of which has been reflected. Its extremity, which is pointed, could not be represented. *l l.* The "great lateral" muscles, superficial and deep, of the left side, seen in section, amongst which the ribs are visible, also in section. *e, e.* Eyes. B. Air-bladder.

FIG. 2. The air-bladder of a Sapphirine Gurnard, "Perlon," (*Triglahirundo*). It is seen from its lower aspect, and is distended by a greater quantity of gas than it would normally contain. *m.* Principal lobe. *ra, la.* Right and left appendicular lobes respectively.

FIG. 3. The same—also from the inferior aspect—opened by a crucial incision; the four triangular flaps resulting from which are reflected over the appendicular lobes (*ra, la*). A, P. Cavity of the principal lobe separated by the diaphragm (*d*) into two divisions—the anterior (A) and posterior (P) chambers. *d.* The "diaphragm" (Duvernoy) lying obliquely from before backwards and from above downwards, *s* indicating its anterior

superior, and *i* its posterior inferior border. It is pierced by an elliptical foramen, *f*. *m t*. Median tendon, representing a kind of "rachis" or mid-rib of the entire apparatus.

FIG. 4. Transverse section of the air-bladder of the same fish. *ra*, *la*. Right and left appendicular lobes. *m m*. Cavity of the principal lobe. *m t*. Median tendon. *i m*. The two "intrinsic" muscles of the air-bladder. The oblique direction of their fleshy bundles of fibres can be seen.

FIGS. 5, 6, 7. Various phases of development of the air-bladder of the same fish. The lettering as before.

FIG. 8. Right lobe of the air-bladder of the Dactylopterus (*D. volitans*), represented in section to show the incomplete diaphragm (*d*), which projects at the level of the tubular isthmus (*c*) of communication with the left lobe. *rl*, *pa*. Internal surface of main cavity, and "pyramidal appendix" of right lobe respectively.

CLASSIFICATION OF COMETS.

By RICHARD A. PROCTOR, B.A., Cambridge,

AUTHOR OF "SATRUN," "THE SUN," ETC.

SOME of the facts of science are stranger than any fictions which even the liveliest imagination could devise. So strange are they that even the student of science who has been engaged in the work of mastering them is scarely willing to admit them in their full significance, or to accept all the inferences which are directly or indirectly deducible from them. This, true in all departments of science, is especially noteworthy in astronomy; and perhaps there is no branch of astronomy in which it is more strikingly seen than in that which relates to comets. During the last quarter of a century discoveries of the most surprising nature have been made respecting these mysterious bodies; relations have been revealed which bring them into association with other objects once regarded as of a totally different nature, and the path seems opened towards results yet more amazing, by which, more than by any others which even astronomy has disclosed, we seem brought into the presence of infinite space and infinite time The earth on which we live—nay, our solar system itself—seems reduced to utter insignificance compared with the tremendous dimensions of comet-traversed space; while all the eras of history, and even those which measure our earth's existence, seem as mere seconds compared with the awful time-intervals to which we are introduced by the study of cometic phenomena.

One of the most interesting points suggested by the recent cometic discoveries is the question, how comets are to be classified. That they are not all of the same order is manifest, whether we consider their size, or the shape and extent of their orbits. But precisely as in zoological classification mere size or development is considered a much less important point than some really characteristic difference of structure, or even than a difference of distribution, so in classifying comets it would be unsatisfactory in the extreme could we have no more characteristic difference to deal with than that of dimensions. Supposing, for instance, that we could separate comets into those with or without a nucleus, or those with or without a tail; such a classification, if it was found to correspond with a real difference of nature, would be much more satisfactory than the arrangement of comets into various orders differing only in size. One of the most interesting questions, then, in the cometic astronomy of a few years ago was this—Are the peculiarities just referred to—the absence or presence of a nucleus, or of a tail—really characteristic, or do they correspond to mere differences of development? I say that this question belonged to cometic astronomy of a few years ago, though even then there were reasons for regarding the various forms of structure observed in comets as depending only on development. Of course comets which, during the whole time of their visibility, showed neither tail nor well-defined nucleus, could afford no means of answering the question. But a comet like Donati's—the glorious plumed comet of 1858—which appeared as a mere globular haze of light, and gradually during its approach to the sun assumed one form after another of cometic adornment—the nucleus, the fan-shaped expansion, the long curved tail, striations within the tail and envelopes outside the fan, while finally even subsidiary tails made their appearance—teaches us unmistakably that these features depend merely on development. We might as reasonably place the chicken in another class than the full-grown fowl because it has neither comb nor coloured tail-feathers, as set a small comet in another order than that to which Donati's belongs because the small one shows neither tail nor coma. The gradual loss of these appendages by Donati's comet, during its retreat into outer space, of course strengthens this view. But perhaps the most remarkable proof ever afforded of the variety of appearance which the same comet may present, was that given by Halley's comet at its return in 1835–36; for on that occasion, after showing a fine coma and tail during its approach towards the sun, it was seen in the southern hemisphere by Herschel and Maclear, not only without tail, but even without coma, appearing in fact precisely like a star of the second magnitude. After this—that is to say, during its retreat—it gradually resumed its coma, and even seemed to be throwing out a new tail, but no complete tail was formed while the comet remained visible.

Indeed the difference between the appearance presented by the same comet before and after its nearest approach to the sun is not only remarkable in itself, but subject to remarkable variations. "What is very remarkable," says Sir John Herschel on the first point, "the shape and size are usually totally different after the comet's reappearance (on the other side of the sun) from what they were before its disappearance. Some," he remarks on the second point, "like those which appeared in 1858 and 1861, without altogether disappearing as if swallowed up by the sun, after attaining a certain maximum or climax of splendour and size, die away, and at the same time move southward, and are seen in the southern hemisphere, the faded remnants of a brighter and more glorious existence of which we here witnessed the grandest display; and on the other hand we here receive as it were many comets from the southern sky, whose greatest display the inhabitants of the southern parts of the earth only have witnessed. It also very often happens that a comet, which before its disappearance in the sun's rays was but a feeble and insignificant object, reappears magnified and glorified, throwing out an immense tail, and exhibiting every symptom of violent excitement, as if set on fire by a near approach to the source of light and heat. Such was the case with the great comet of 1680, and that of 1843, both of which, as I shall presently take occasion to explain, really did approach extremely near to the body of the sun, and must have undergone a very violent heat. Other comets, furnished with beautiful and conspicuous tails before their immersion in the sun's rays, at their reappearance are seen stripped of that appendage, and altogether so very different, that but for a knowledge of their courses it would be quite impossible to identify them as the same bodies. Some, on the other hand, which have escaped notice altogether in their approach to the sun, burst upon us at once in the plenitude of their splendour, quite unexpectedly, as did that of the year 1861."

It was clear, then, long since, that comets cannot be classified either according to their size or their development. But this has been even more conclusively shown by the spectroscopic analysis of large and small comets. For certain bright bands seen in the spectra of the small comets which had been examined before the present year, are found also to characterize the spectrum of the comet which adorned our northern skies last June and July, and to be shown not only by the coma, but also by the tail. I do not here enter into any special consideration of the results of spectroscopic analysis as applied to this comet, because to say truth our spectroscopists have not met with any noteworthy success; and we must wait till the spectroscopists of the southern hemisphere have sent in their statements before we can determine whether any special accession has been made to our knowledge. It may, however, be assumed from what has been observed here, that the characteristic spectrum of comets, large and small, is that three-band spectrum which was first recognised during the spectroscopic investigation of Tempel's small comet in the year 1866.

Comets, then, must be classified in some other way. It is not difficult to select the proper method of classification—a method not only satisfactory as respects the distinctions on which it depends, but exceedingly suggestive (as, in fact, every just mode of classification may be expected to be).

I would divide comets into three classes, according to the nature of their paths.

First, there are the comets which have paths so moderate in extent that their periods of revolution belong to the same order as the periods in which the planets revolve around the sun. This class includes all the comets which have been described as Jupiter's comet-family, and all those similarly related to Saturn, to Uranus, and to Neptune. Other comets of somewhat greater period than Neptune's comet-family may perhaps be regarded as associated with as yet undiscovered planets revolving outside the path of Neptune, and therefore as belonging to the same family. I would not, however, attempt to define very narrowly the boundary of the various classes into which comets may be divided, and in what follows I shall limit my remarks to comets which are clearly members of one or other class, leaving out of consideration those respecting which (for want, perhaps, of more complete information than we at present possess) we may feel doubtful.

Secondly, there are comets of long periods, but which yet show unmistakably, by their motions, that they are in reality members of the solar system—such, for instance, as Donati's comet, which may be expected to return to the sun's neighbourhood in the course of about two thousand years.

Lastly, there are the comets whose motions indicate a path not re-entering into itself. These are of two orders: those which retreat from the sun on a path tending with continual increase of distance to become more and more nearly parallel to the path by which they had approached him; and those whose retreating path carries them divergingly away so that they retreat towards a different part of the heavens than that from which they arrived. Technically, the two orders are those of comets pursuing (i.) parabolic and (ii.) hyperbolic paths. In reality, however, we may dismiss the parabolic path as never actually followed by any comet, any more than a truly circular path is ever actually followed by any planet. We may take it for granted that any comet which seems to follow a parabolic path really follows either an enormously elongated oval path, and so belongs to our second class; or a path carrying it for ever away into outer space, and *nearly* in the direction from which it had arrived, but not *exactly*. A comet's path could only have the true parabolic form by a perfect marvel of coincidence; and in point of fact if a comet could by some amazing chance approach our sun on such a path, the very least of the multitudinous disturbing attractions to which the comet would be exposed would suffice to change the path either to the elliptic or the hyperbolic form.

And here we may pause to inquire how far the second of the three classes into which comets have thus been divided can be regarded as a class apart. Does the mere fact that a comet has a re-entering path—so that, unless perturbations affect it, the comet will travel in continual dependence on our sun—afford a sufficient reason for distinguishing the comet from others which travel on a hyperbolic path? It appears to me that this question admits of being answered in two ways. When we remember that a comet approaching our system on a slightly hyperbolic path might have that path changed into an elliptic figure by the perturbations to which the comet would be subjected during its visit, we may reasonably decide that the mere fact of a comet pursuing an elliptic path ought not to be considered a valid reason for distinguishing it from one of the hyperbolic comets. But when we consider, on the other hand, that there are comets like those of Jupiter's family, which are quite distinctly separated by the nature of their paths from the hyperbolic comets, we may not unreasonably infer that some at least of those which travel on elliptic paths of great eccentrity are in reality to be classified apart from the hyperbolic comets, as having had a different origin and a different history. We might, indeed, reverse the argument just adduced, and reason that the hyperbolic comets ought not to be classified apart from the comets of long period, because perturbations excited within the solar system might change an elongated elliptic orbit into a hyperbolic one. The point at issue is thus seen to resolve itself into the question whether we can assert that there are comets which from the earliest times (the youth of the solar system) have belonged to it (i.) with short periods and (ii.) with long periods, while (iii.) other comets have visited it from other systems. We find in fact that the attempt to classify leads in this case, as it has led in so many others (as perhaps it inevitably must lead, if properly conducted), to the question of origin.

And here perhaps the question will arise, may we not cut the Gordian knot by denying that even the comets of short period can be separated from the hyperbolic comets which visit our system from interstellar space? I am aware that the theory of comets and meteors which Schiaparelli has advanced, and which many in this country have viewed with considerable favour, points to this conclusion. For according to that theory meteor-systems are groups of discrete bodies which have been drawn towards our solar system, gradually lengthening out as the process of indraught continued, and have then been compelled by the perturbations to which they have been subjected within our system, to become members of it; and as comets and meteor-systems have been found to be associated together in some mysterious way, this theory of the introduction of meteor-systems is in reality a theory of comets. Now since some certainly among the meteor-systems have periods of moderate length, this theory of Schiaparelli's would regard the short-period comets as drawn out of the interstellar depths, while manifestly it would be absurd not to extend Schiaparelli's theory to hyperbolic comets. In fact, we know that he himself regards his theory as requiring the occasional appearance of meteors of hyperbolic path, and therefore as not merely consistent with the phenomena of hyperbolic comets, but accounting for them. Adopting his theory, then, to its fullest extent, we should regard all comets and meteors as bodies coming from the interstellar depth; for it is not easy to see how any comet or meteor-system could be so far distinguished from its fellows as to be regarded as originally a member of the solar system.

But for reasons which appear to me incontrovertible, I find it impossible to give in my adhesion to Schiaparelli's views, in the form in which he presented them. A line ought to be carefully drawn between what has been proved and what has not been proved respecting the opinions which Schiaparelli has advanced. His most happy conception, that meteors would be found to travel in the paths of comets, has been realised, and no possible question can be raised as to the completeness of the demonstration; but it is quite otherwise with his supposition respecting the manner in which meteoric systems or comets have been introduced into the solar system. It not only has not been proved that comets have been compelled by the perturbations of the planets to become permanent members of the solar system, but grave doubts rest on the bare possibility of such an event occurring.

Let it be remembered that the conditions of the problem are purely dynamical. We know that a comet's head obeys the laws of gravity, and whatever peculiarities may affect the motions of the matter of comets' tails are not by any means such as would help to render easier the captures conceived by Schiaparelli. Confining ourselves then to gravity, we can determine readily in what way a comet might be captured. Take the case of a particle travelling towards our solar system from out the interstellar depths under the influence of the sun's attraction. Such a particle may be regarded as practically approaching the sun from an infinite distance,* and we know

* The point considered is the velocity of the particle at given distances from the sun; and the estimated velocity is appreciably the same whether

its velocity at given distances from the sun. Thus, when at the distance of Neptune its velocity would be 4·7 miles per second; at the distance of Uranus, 5·9 miles per second; of Saturn, 8·3 miles; of Jupiter, 11·3 miles; of the asteroids, from 15 to 16 miles per second; and the velocity in crossing the distances of Mars, the Earth, Venus, and Mercury, would be 20·8 miles, 25·9, 30·3, and 41·4 miles per second repectively. Now we know that the greatest velocity which any given planet can communicate to a body approaching it under its sole influence from interstellar space is very much less than the velocity which such planet can communicate to a body approaching it under the sun's influence in addition to its own, for the communication of velocity to a moving body is a process requiring time, and in the latter of the two cases just considered the body is for a much smaller time under the influence of the planet.* And the velocity which a planet can

the particle be supposed to come from the distance of the nearest star or from an infinite distance. This is easily seen from the formula

$$V^2 = v^2\left(2 - \frac{r}{a}\right),$$

where r represents the radius of a circular orbit described with velocity v, and V is the velocity at distance r, of a body travelling in an orbit having mean distance a. For regarding the earth's orbit as unity, put

$$r = \text{earth's distance} = \text{unity},$$
$$v = \text{earth's velocity} = 18·3,$$

taking a mile as the unit of length, and a second as the unit of time; for though we have put r = unity, this does not force us to take r as our unit of length, because we only require to consider the ratio in what follows. Then we have—

$$V = 18·3\sqrt{2}\sqrt{1 - \frac{1}{2a}} = 25·9\left\{1 - \frac{1}{4a} - \frac{1}{32a^2} - \&c.\right\} = 25·9,$$

if a is made infinite. But if a be taken equal to half the distance of Alpha Centauri, say = 100,000, we have

$$V = 25·9 - 0·00006475 - 0·000000000809375 - \text{smaller terms},$$

all the terms after the first being together manifestly less than 0·00007, or about 4½ inches. In other words, whereas a body approaching the sun from infinity would have a velocity of about 25·9 miles per second, a body approaching the sun from the distance of Alpha Centauri, so that its mean distance may be regarded as half the distance of that star, would have a velocity less by 4½ inches per second, a difference so small that it may be regarded as evanescent. It is a curious consideration, however, that minute though such differences are when we are merely comparing velocities, yet distances due to such differences in the enormous time-intervals which the study of comets introduces to our consideration, are to be measured by thousands of miles.

* The comparison is easily made in any given case. Take, for instance, the planet Jupiter, supposing it at rest, and a particle drawn towards it

communicate under any circumstances represents the velocity which, under similar circumstances, the planet can withdraw from a moving body. So that Jupiter, Saturn, Uranus, and Neptune, are severally unable to deprive a particle which, drawn in by the sun's attraction, passes near to them, of more than a portion of the velocity which these planets are respectively able to communicate to a body approaching them from infinite space. Taking, for example, the case of Jupiter, we may regard 40 miles per second as a sort of negative fund from which Jupiter would have the power of drawing, to reduce the velocity of bodies moving from him, if Jupiter were the sole attracting influence under which such bodies had acquired their velocity; *but* in the case of bodies which have been drawn inwards by the sun's attraction, the fund is reduced, as shown in the note below, to about 30·3 miles per second. Now this might seem ample when we remember that the velocity of a body crossing the path of Jupiter under the sun's influence alone would be but 11·3 miles per second. But it is to be observed that the estimate only applies to bodies moving all but directly from Jupiter, and coming all but into contact with his surface. The power of Jupiter in this respect diminishes rapidly with distance from the surface. At a distance from Jupiter's centre equal to four times his radius, his power is already diminished one half, and this distance is far within that of even his nearast satellite. Moreover, it is to be noticed that a body which moves in such

from an infinite distance under the combined influence of the sun and planet (the particle lying originally on the side away from the sun). We readily obtain for the velocity V of the particle just as it is reaching the surface of Jupiter the equation

$$V^2 = \frac{2\,M}{J + j} + \frac{2\,m}{j};$$

where M represents the sun's attractive influence at a unit of distance, and m Jupiter's, while J represents Jupiter's distance from the sun, and j the radius of Jupiter. For the velocity v of a particle under Jupiter's sole influence we obtain the equation $v^2 = \frac{2\,m}{j}$. Now it is easily calculated that $\frac{2\,M}{J + j} = (11{\cdot}3)^2$, while $\frac{2\,m}{j} = (40)^2$ nearly. Hence the velocity $V = \sqrt{(11{\cdot}3)^2 + (40)^2}$ = less than 41·6; while $v = 40$; so that a body approaching the sun under his sole influence would have, at Jupiter's distance, a velocity of 11·3 miles per second; one approaching Jupiter under the combined influence of the sun and planet would reach Jupiter's surface with a velocity of 41·6 miles per second; and a body approaching Jupiter under his influence alone would reach his surface with a velocity of 40 miles per second. So that Jupiter helping the sun adds a velocity of 30·3 miles per second as compared with the velocity of 40 miles per second, which he can communicate to a body approaching him from infinity.

sort that Jupiter exerts his most powerful retardative influence, must have moved for some time previously in such a way that Jupiter exerted nearly his most powerful accelerative influence.* It may be readily shown to be impossible for Jupiter to withdraw much more velocity than he had already communicated; and similar remarks apply, of course, to Saturn, Uranus, and Neptune.

The application of these considerations to Schiaparelli's theory is easily perceived. In order that a particle attracted from outer space may be compelled to travel in a closed orbit around the sun, its velocity must be diminished. And this can very readily happen. But for the particle to travel in an orbit of a particular extent or mean distance, its velocity where it crosses the distance of the disturbing planet must be diminished by a certain amount; and in dealing with Schiaparelli's theory, it is a cardinal consideration whether the observed orbits of periodic comets are such that we can admit the possibility of their resulting from any diminution of velocity which the disturbing planet could have produced. Taking, for instance, the November meteors, which pass near the orbits of Uranus and the earth, and do not approach any other orbit near enough for any such effects upon the orbital motions of these bodies as we are now dealing with.† We may dismiss the earth from consideration at once, because our planet is far too small to modify the motions of bodies rushing past her with the velocity, nearly 26 miles per second, which the sun communicates to bodies approaching him from interstellar space, by the time they reach the earth's distance from him. Uranus then alone remains. Now the present velocity of the November meteors when crossing the orbit of Uranus amounts to about 1½ miles per second. The velocity of a particle approaching the sun from interstellar space would be nearly 6 miles per second when at the distance of Uranus. It may be seriously questioned whether, under any

* It is manifest that a particle in approaching from without must be, in the first instance, accelerated by any planet to which it draws near, no matter what the direction may be in which the particle arrives. It may begin to be retarded, however, before it has reached the distance from the sun at which the disturbing planet is travelling. In any discussion of the change of path as to position, we should need to inquire very carefully into the manner of approach; but in the above discussion we are only inquiring into the change of velocity.

† Both Jupiter and Saturn can perturb the November meteors, and thus modify the shape and position of the meteoric orbits; but such changes, though by no means inappreciable, are utterly insignificant compared with those required to change the motion of a body approaching the sun from interstellar space into motion in an orbit like that of the November meteors.

circumstances whatever, a particle crossing the track of Uranus without encountering the planet could be deprived of $4\frac{1}{2}$ miles per second of its velocity. For though Uranus can deprive a body directly receding from him (and starting from his surface) of a velocity of about 13 miles per second, yet the considerations above adduced show that only a fraction of this velocity could be abstracted from a body moving past Uranus; and it is certain that if so large a reduction as $4\frac{1}{2}$ miles per second could be effected at all, it would only be by a singularly close approach of the particle to the surface of Uranus.

But setting apart the improbability that a body arising from interstellar space could be in this way compelled to travel in the orbit of the November meteors, the possibility of such a capture would not prove the possibility of the capture of a flight of bodies large enough to form that meteor system and its accompanying comet. If the whole material of the system and its comet had arrived in a compact body, the material attractions of the parts of that body would be sufficient to keep them together; whereas, in point of fact, the November meteor system and its comet occupy at present a large range of space, even if the meteors be not scattered all round the orbit (however thinly along portions thereof). If, on the other hand, the material of the body were not in a compact form, the body would be necessarily large, and a portion of it only would be captured by Uranus. Nay, it is not even necessary that this should be conceded. For though we admitted that the whole of a large and tenuous body not kept together by the mutual attraction of its parts or by cohesion, might be captured, it is manifest that different parts would be captured in different ways, and would thenceforth travel on widely different orbits. That a system of bodies already drawn out into an extended column, and in respect of length already resembling the meteor systems we are acquainted with, could be captured, as Schiaparelli's theory requires, and all sent along one and the same closed orbit, is altogether impossible.

It is to be noticed also that we gain nothing, as respects the interpretation of comets, by adopting Schiaparelli's hypothesis. To assume that cometic matter has been wandering about through interstellar space, until the sun's attractive influence drew such matter towards the solar system, is to explain a difficulty away by advancing another still greater; moreover, we have not a particle of evidence in support of the supposition. To suppose, on the other hand, that comets have *crossed* the interstellar spaces, coming to us from the domain of another sun, is to remove the difficulty only one step. We know that comets pass away from the domain of our sun to visit some other sun after an interstellar journey of tremendous duration;

and to suppose that comets, whether of hyperbolic or elliptic orbit, came to us originally from the domain of another sun, is merely to suppose that that happened to such comets millions of years ago which we know to be happening to other comets at this present day, but not by any means to explain the nature of comets or their origin. We know that many comets leaving our system to visit others had not their origin within our system; and we cannot assume as possible or even probable that any comet had its origin within the domain of another sun than ours, unless we assume as possible or probable that some among the comets leaving our own sun had their origin within our sun's domain.

Thus, then, we have been led to the conclusion that whether we adopt, with Schiaparelli and others, the theory that comets with meteoric systems can be drawn into the solar domain, or regard such an event as of very infrequent occurrence, we still find that the origin of comets must be looked for within solar systems; or rather, since we cannot claim to trace back comets any more than planets or suns, to their actual origin, we may say that at an early period of their existence comets belonged to the solar system. The system has had no more occasion, so to speak, to borrow comets from other systems—that is, from other suns—than these have had to borrow comets from it and from each other.

We decide, then, that comets may certainly be classified into those which belong to our solar system from the earliest period of their history, those which visit it from without, and pass away to other suns, and an intermediate class consisting of those which having visited it from without have been constrained, by perturbations affecting them within it, to become attached permanently to its domain. We may note also that as there are comets now belonging to our solar system which originally belonged to other solar systems, so probably many comets originally belonging to our solar system are now either attending on other suns or wandering through the star-depths from sun to sun.

It has been from viewing the matter in this way, recognising the almost decisive evidence that comets have from earliest times been members of our solar system, that I have been led to inquire into the possibility that some comets may have been expelled from the sun, and that others—those, namely, which seem attached to the orbits of the giant planets—may have been expelled from those planets when in their former sun-like condition. The evidence to show that there is an adequate expulsive power in the sun is striking, and we may reasonably infer that the small suns formerly dependent upon him had a similar power. The motions of the members of the comet families of Jupiter, Saturn, Uranus, and Neptune, accord far better, too, with this theory than with Schiaparelli's.

It is to be noticed, however, in conclusion, that we may also not unreasonably admit the possibility that comets may be, as it were, the shreds and fragments left from the making of our solar system and of others, since the sun and planets in their former nebulous condition and expanded forms would have had a power of capturing these wandering shreds which at present they no longer possess.

FIRST PRINCIPLES OF AERIAL TRANSIT.

By F. H. WENHAM, C.E.

UNIMPEDED by all terrestrial obstacles—to man impassable without the aid of science—birds of passage traverse with ease their aerial roadway with level track, making every chosen spot of earth alike their home, instinctively directed by change of season to more congenial climates.

The question is frequently asked, Is man ever destined to accomplish this sublime mode of locomotion, or is it always to remain the sole privilege of unthinking animal creation? The answer is generally in the term "impossible," so far that it is a common proverb uttered to express the height of impossibility.

It is not an easy task for objectors to explain the conditions on which such a very positive assertion is based. It is generally summed up in the statement, "Not power enough; the pectoral muscles of birds are enormously strong in proportion to their weight, far exceeding those of any terrestrial animal." And thus the argument is abruptly dismissed. But this reasoning is both unphilosophic and untrustworthy, and forms no criterion for the determination of a mechanical condition of actual work performed; for a large bird must of necessity have powerful pectoral muscles, merely for the purpose of sustaining the weight of the body on those wings, even supposing that they rested on solid supports or props. The wings are hinged to the body like levers, and these huge muscles are needful merely to supply the place of rigidity when no mechanical force is expended in the way of motion. Rejecting, therefore, the size of muscles as an uncertain proof of acting force, as far as it relates to rapid motion, and avoiding all abstruse calculations and complex formulæ, let us consider a few of the acting laws involved in the question of flight.

There is no principle in mere rapid horizontal movement alone that would cause a heavy body to maintain its level, for during this motion it is still answerable to the laws of gravity. If a leaden ball is set free in vacuo, it will fall sixteen feet in the first second of time. If that same ball is fired horizontally from a rifle, and propelled a mile distance in one second, it will still descend sixteen feet during its passage, falling as before in the same time.

If the ball is fired on the level of perfectly smooth water, it will not sink till the force is nearly expended. It would remain above water without sinking as long as the velocity was maintained. This arises from the fact that the ball, in its swift passage over the surface, meets with so many particles in a brief period, that there is no time to give them motion, and, in consequence, the water stratum is not deflected or does not yield under such a speed; therefore the ball continues to traverse as if on a solid plane. This being illustrative of a main principle of flight, will be referred to again under a modified condition.

Assuming, from our knowledge of elementary laws, that there is no principle in any form of motion that can be given to a weight within a body in free space that will create a persistent force in one direction only, so as to counteract the action of gravity due to the earth's attraction, and that weight is an absolute condition of all tangible matter, it follows that in flight the air alone must be considered as the sole medium of support. Rapid transit at the will of the aeronaut is the main condition worth consideration, as a subject of general utility; aerostation, implying the use of large volumes lighter than an equal bulk of air—exemplified by the various forms of balloons—may be excluded from the argument, as the inconveniences attendant upon their use, and their enormous size, renders anything like speed of propulsion impracticable—precluding all hopes of improvement or discovery in this direction as a means of locomotion to be generally employed by man.

The laws of flight, in the true sense of the term, must be considered entirely upon the resistance of the atmosphere, and based upon its principles of action and reaction, implying the impulse of a very light body—such as air—affording support to one of far greater density—such as that of a bird—with the least possible expenditure of power, the aerial stratum forming a roadway that levels all terrestrial obstacles, and one that will not yield during the passage in flight to a degree causing an undue expenditure of force.

According to the laws of action and reaction in two bodies in motion towards each other, if they are components of different weights or densities, and as time is always an element in the determination of the value of mechanical power, it follows that, to maintain equilibrium, the impulse or opposing force must be obtained from the reaction of a larger body of the lighter element during a given period; and as in the case of flight the abutment has to be secured upon the yielding air, and the reaction effected by the resistance of an extensive surface, we have to decide what that area must be in proportion to the weight to be sustained.

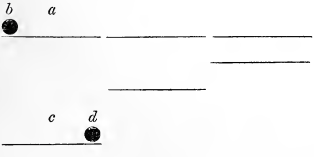
It may be assumed that the limit can be determined by the condition, that supposing the muscular force of the man or bird should fail, that the wing surface or plane will afford sufficient resistance in a perpendicular descent, while the animal is passive, to prevent him from being injured on reaching the earth. If one square foot of extension be taken for each pound weight, the resistance of the air will limit the rate of descent to twenty-two feet in one second of time; this will be sufficient for safety, as it is the velocity acquired by a body at the end of a fall of eight feet—a height from which an active man may leap down with impunity. It has been ascertained that this area of one foot for each pound is the average wing surface of most flying animals. It must, however, be borne in mind that large wing surface does not indicate great facility of flight, as some of the swiftest and most enduring flyers have only half that area; let the argument, therefore, be based upon the proportion of one pound per square foot of surface. If this is arranged in the form of a parachute, and the total weight of the man and apparatus be taken at 200 lbs., we then have this weight overcoming the resistance of the atmosphere at a falling speed of twenty-two feet per second, or 1,320 feet per minute. This gives us a definite measure of units of force, generally estimated at what is termed "foot-pounds," that is the total force, weight, or resistance in pounds, multiplied by the rate or velocity in feet per minute. By so multiplying these given quantities we have the enormous force exerted of 264,000 foot-pounds, and dividing this by the Watt standard of horse-power, viz. 33,000 lbs. raised one foot high per minute, there results eight horse-power, or about the strength of forty men. Now the whole of this force is represented by the action and reaction of the opposing bodies—the resistance of the plane to the air, or conversely the air to the plane, for precisely the same amount of power would be consumed if the weighted plane were itself stationary and the air rushing against it at the rate of twenty-two feet per second. This eight horse-power, therefore, represents the force expended, and that would be required to keep the body weighing 200 lbs. sustained at a uniform height in air, on an area of the same number of square feet.

If the estimate is taken with another area, the result will differ. With half a square foot per pound the descent will be about 1,800 feet per minute, and the power expended nearly equal to that of eleven horses. Let us advance into the other extreme, and allow for the 200 lbs. a surface of 43,560 square feet —equal to *one acre* in extent. The descent would now be very slow—about one mile per hour, or eighty-eight feet per minute, and the force expended nearly equal to half a horse power. We thus see that not by any practicable extension of surface is it possible for man to raise himself perpendicularly in air by his own muscular force, the data having been taken under the most favourable estimate, without deducting for loss arising from the friction of a motive machine.

This may be conformable to the opinion of the large class of antivolants, and is consistent with the assertion of some who are professed scientists; but the argument does not end here, for the conditions named do not truly represent any form of flight, which strictly means one of horizontal progress. Many of the gallinaceous birds located in forest tracts, by aid of wings of the largest area and a great temporary exertion of strength, are able to raise themselves nearly perpendicularly into the branches above, but they are so incapable of taking long flight that when pursued in an open country they prefer running to flying as a means of escape.

Numerous experiments have been made by man to raise himself by various machines perpendicularly in air, but they have all failed; and this about represents the summary of all that has yet been done in the science of aeronautics by mechanical means only.

Another effect remains to be considered. Instead of allowing the weighted plane to fall perpendicularly, let it be moved rapidly in a horizontal direction while still free to descend. The rate of descent under these circumstances would be immensely retarded, according to the speed given. A fresh, unmoved stratum of air quickly acted upon every instant with its inertia undisturbed offers great resistance to deflection, and from this cause, at a very high speed, affords a nearly solid support. This condition serves to explain the mystery, and involves the true principle of flight, which depends not upon large area, but on the width of the stratum, and consequently *the weight of air passed over in a brief time*. All the data for exact calculations are yet wanting. If the air were non-compressible, like water, an accurate result could be foretold of the lifting force of inclined surfaces; but air being elastic, a formula is required differing from that applicable to a dead weight, and therefore the theory can only be put forth in demonstrative forms. Let *a* be a plane surface in the form of a *square*, and while constrained to a horizontal position during descent, let a weight or ball (*b*) be set at one extremity. The fall through air in a given period to be from *a* to *c*.



But during the descent let the ball have a horizontal course at the top of the plane from *b* to *d*; of course, in this case, the rate of descent will not be altered whether the ball is stationary or in motion—it is simply a question of area and relative weight. But suppose the velocity of the ball is doubled, so as to carry it over a space equal to the end of a second similar plane, the ball will then descend only half the distance in the same time. If the velocity is trebled, so as to carry it on as far as the end of plane No. 3, the ball will fall only one-third the distance; and so on with a reduction of fall proportionate to the rate of traverse. This is illustrative of the influence of a high horizontal speed on the rate of fall, but it may also be demonstrated how this rate of fall at the same speed may be greatly reduced by the form and disposition of the surface, relative to the direction of motion.

The foregoing surface is assumed as a *square*, but suppose this to be cut in half and the two lengths added end to end transversely to the line of motion, the area remains just the same as before, and the fall will still be from *a* to *c*, while the ball passes only half the distance; but let the ball travel at the same speed as on the first plane, it will then pass over another similar elongated element of only half the width, and take double the time to descend to *c*; or, in other words, by this disposition of area the supporting power of the surface will be increased nearly two-fold.

Again, if cut into four and extended the same way, its value will be quadrupled.

Having as yet no flying contrivance of our own construction that can be independently propelled through free air at a high speed in order to exemplify this theory, we must refer to the formation and action of a bird's wing for a working example. The most remarkable one is the wandering albatross. This extraordinary bird is found thousands of miles away from land, and may be said to live in the air, for in stormy weather it never rests on the ocean, but gathers up the floating substances that serve it for food during flight. The wings extend fifteen feet from end to end, and are only eight and a half inches in width at the broadest part. The bearing afforded by such a wide stratum of air may be considered as almost an unyielding one at the usual rate of speed, and in consequence it is able to swoop down, and rise again apparently to the same height, by the mere momentum of its own body, with no aid by the impulses of the wing. The bird could not perform this feat if its aerial support was a rapidly yielding one, but would require to flap diligently in order to regain its former level.

The albatross also affords the best visible indication of the power consumed for maintaining its flight. Many observers have declared that in straight flight it apparently never moves its wings at all—at least, in the rapid flapping manner of smaller birds—and this seems to crown the mystery; for if there is mechanical force exerted, it should be indicated by some form of motion. But the term "stationary wings" must be taken in a modified sense in this case, for the strokes may be too slow to be perceptible. It cannot be supposed that a few deliberate strokes, a foot or so in extent each minute, will propel the bird with a force not much exceeding that exerted by a lady's fan in still air; but the conditions are in reality widely different. If the bird, as a fixture in still air, were to wave its wings slowly in a stroke of twelve inches, the power and also the effect would be inappreciable, not greater than the fan; but when the bird is traversing the air, say at the rate of thirty miles per hour, instead of exerting a trifling force on one foot perpendicular of air, during a rate of ten waves per minute, each stroke passes over a stratum of *two hundred and sixty-four feet* of air, and this, having its momentum undisturbed from the same law of reaction that applies to the support, must also give nearly a solid abutment for propulsion.

Taking the albatross as a model for the utmost perfection in the principle of animal flight, the question of the possibility of imitation has to be considered. The mechanical action of the wing is not complicated. This bird does not find support upon the air by a series of downward impulses by means of any wing action or mechanism that gives a bearing only during the down stroke, and without resistance in the upward one, for the weight of the bird is equally sustained during the time of both the rise and fall—the wing, in both cases, being inclined upwards so that the rush of air against the inclined plane represented by the under surface causes a continuous and equable support. This is regulated by the sense of feeling of the bird, which, conscious of its own weight and the proper movement required for support, is able instantly to adapt the position of its wings, under all conditions of flight, so as to carry it securely. The faculty of propulsion requirés no particular muscles for its performance, as it is an inherent property in the formation of the wing itself—the fore edge being in all cases rigid, and the backward part consisting of the elastic ends of a row of feathers, which, in slightly yielding, acts upon the air as a propeller, like the waving tail of a fish in water. This action may be seen, and the effect felt, by waving the dried extended wing of a large bird.

The foregoing theory fully accounts for the necessity of lateral extension of wing in all birds of prolonged flight; and in all attempts at imitation, where economy of motive force is a chief desideratum, the principle must be borne in mind. If the antivolants say dogmatically that flight is impossible, on an assumed insufficiency of power compared with the weight of the machine, far more reasonably may they use the term on any attempt to imitate the flying mechanism of the albatross, for in this respect they may be quite right.

If near one foot in length of wing is required for every two pounds that the bird weighs (it has more than this), the comparison will be, that if a man and machine weigh together only 300 lbs., he will require an extent of wing of 150 feet from end to end. Very little consideration will show this to be utterly impracticable. Let the machine be constructed of the lightest trussed work that can be contrived, with the smallest margin of strength, it will necessarily be exceedingly heavy, and present so much resistance to the air that excessive power would be required to propel it, and by reason of its rigidity it would probably be destroyed by collision with the ground at the very first experiment. It is the very elastic jointing of the wing of an albatross that constitutes its safety; were it one long, taper, thin, tubular bone, the least violence would cause fracture; but the wing, by the feeling of the animal, is caused to yield to circumstances, and can instantly be drawn away from risk of accident.

These wings having been considered in their action as mere inclined planes, whose purpose and intention is to obtain a bearing upon a very wide stratum of air, then it follows that this stratum need not extend out in one line, but may be taken in different planes in superposed positions. It can readily be imagined that a dozen of these birds might fly at the same speed, at a certain distance one above the other, as if linked together, the weight of whose united bodies would not exceed that of a man and machine. This would be no violation of the principle herein described, and affords some chance for the construction of a very light and strong machine. A man might thus be sustained on a series of twelve wings or planes, not exceeding in length those of the albatross. These aeroplanes could be stretched by very light laths, merely for the purpose of keeping their surfaces flat, and connected with each other by a system of cords only, as the pressure of the air beneath them would cause them to rise free of each other. Nothing in the shape of a long heavy spar would be needed, as the cords of suspension for the whole system of aeroplanes could be brought down or converge at an angle to near the body of the machine. These surfaces are merely for support, and receive no motion. The propellers would be a detached and separate affair—either as two long rods vibrating vertically, with elastic blades yielding backwards from the line of motion, like a bird's wing, or the arrangement might rotate like two vanes of a windmill or screw propeller.

The experiment of the great lifting power of superposed planes has been tested repeatedly, by stringing a number of paper toy kites one above the other—the string then assumes a less inclined position; the total weight is small. They are, of course, *strong enough*, and, if in sufficient numbers, will readily lift a man; but, in a rough arrangement of this description, the conditions of resistance are too great to enable him to propel himself, when raised from the ground, with a force equal to the pull of the string, which, if very long, in itself exposes a large area to the wind.

The bodies of all swift flyers are formed of such a figure as to cause but little impediment against the air in the line of motion, and the greatest possible amount to vertical descent. These conditions would have to be considered in all artificial flying arrangements by disposing the material in a position that will offer the least forward resistance, like in the flight of an arrow, with all the front edges of the various parts made conical or wedge-shaped.

THE THERMOMETER AS A COMPANION IN DAILY LIFE.

By A. H. GARROD, B.A.,

PROFESSOR OF ZOOLOGY IN KING'S COLLEGE, LONDON.

THE value of the thermometer as an aid to diagnosis in disease has been so considerable that its importance in a physiological, in contradistinction to a pathological point of view, has been somewhat overlooked. What we do know with regard to its teaching in health is mainly due to the researches of Dr. John Davy, who has been followed by Senator, Jürgensen, and a few others.

At first sight the subject may seem unattractive. A single observation affords but little information, and a great many have to be completed and recorded before any points of interest can be extracted from fluctuations which are almost exclusively limited to two degrees of the Fahrenheit scale.* The same argument would, however, apply to the barometer—an instrument which has nevertheless attracted the attention of a large number of amateurs, whose voluntary assistance has greatly conduced to the establishment of those laws affecting the winds which have justified the foundation of a predicting meteorological committee of scientific men. It is to be hoped that by the time the reader has arrived at the last page of the present article he will be persuaded that there is more to be learnt, and profitably learnt, by the employment of the thermometer in its physiological aspect, than he previously anticipated.

The body, examined some time after death, exhibits no peculiarities which distinguish it from inorganic matter as far as the development of heat is concerned; its temperature is that of the external air, and varies with it. The case is different wit' the living body, as we all know. It cannot be correctly said that during life the temperature is above that of the atmosphere, because in tropical climates, and under certain artificial conditions, such is not always the case; but what we can say is, that the temperature of the deep parts is always greater than

* In this article all temperatures are referred to the Fahrenheit scale only.

that of the surface. This is easily proved by placing a thermometer under the tongue, where it will always record not more than a degree or so above or below 99°, and another in contact with the skin of any uncovered part of the body, where it will be certainly found to rise to a less extent.

Before going further it will be necessary to describe the most suitable form of instrument to be employed in taking the temperature of the body. The thermometer which best answers the requirements of these experiments is that termed the clinical thermometer, which is employed by medical men. It can be obtained of any surgical instrument-maker, and often from chemists. The whole instrument is about five inches long. The bulb is small, but it should be of the same diameter as the stem, though it is frequently made smaller. The stem is uniformly cylindrical; graduated on its surface, and with a strip of porcelain running along its length, internally, to show up the mercury column, which is very fine in calibre, and flattened to render it more distinct. The instrument is self-registering on Phillips' principle; that is, a portion of the mercury column is detached from the rest, and prevented from rejoining it by the presence of a minute quantity of air between them. The graduated scale is a very open one, the fifteen degrees between 95° and 110° occupying nearly four inches. Consequently, when the mercury cools down after an observation, it leaves the bulb partially empty, which makes it necessary that particular care should be taken, in carrying the instrument and shaking down the index, that the latter does not descend into the bulb, and in so doing mix with the body of the mercury, thus destroying the self-registering power. To prevent this inconvenience a simple and fairly effectual method has been recently adopted; the tube between the scale and the bulb is constricted by being heated and slightly drawn out, so that the mercury has some impediment made to its return, which is not sufficient to overcome the attraction of the particles of the continuous fluid, but is enough to prevent the index passing it, unless an extraordinary amount of force is employed. The figures and lines of the graduation being on the surface, are apt to become indistinct from the black wearing off. This can be replaced with facility by, after having first carefully freed it from all greasy matter, heating the stem—not the bulb—and rubbing a piece of heel-ball up and down it. The superfluous blacking which is left after the operation should be scraped off with a knife, which, held obliquely to the tube, is run up and down it several times.

Next as to the method of employing the instrument. Before commencing an observation it is first necessary to see that the self-registering mercurial index has been sent down the tube since the last experiment. This is done by holding the thermometer firmly in the right hand, bulb downwards, and gently tapping the one hand against the other. After four or five taps the index will be seen to have descended a certain number of degrees, and when its upper end has descended below 96° F., it has been sent down sufficiently far. Great care must be taken not to shake the thermometer so violently as to make the index go into the bulb.

After the register has been properly adjusted at about 96°, it is better to hold the bulb in the hand for a few minutes before first using it, so as to raise the temperature of the whole mass of the mercury. And in doing this, it does not matter in the least if the index is by this means pushed up above 97°, for it is a certain fact that the temperature in the closed hand never exceeds that in the mouth.

The floor of the mouth is the only suitable place in which to determine the temperature of the body; for though it may not indicate quite as high temperature as deeper parts, nevertheless it very nearly does so, and gives indications of the least change in the body temperature with great accuracy. It is necessary for this that the mouth should be kept closed for a short time before an observation is made. The thermometer, warmed as above directed, is then placed in the mouth, as far back as possible below and to the side of the tongue, and retained there for two and a half or three minutes. Then, without opening the mouth, or withdrawing the instrument from it, the bulb is transferred by the stem to the other side of the tongue, and retained there for the same time. The thermometer may, at the end of the five minutes, be withdrawn, and the height of the index recorded, the time at which it was taken out being indicated at the same time.

In observations like some to be recorded further on, when the temperature is taken every five minutes, the instrument, immediately after having been read off, is shaken down and replaced in the mouth, the record for one time being kept whilst the temperature for the next is being obtained.

The source of the internal body-heat must now engage our attention, and the argument from analogy will assist in explaining it. A fire is warm because of the chemical decomposition, or degradation, which is occurring in its fuel. Under the influence of the oxygen of the atmosphere the elaborate hydrocarbons are being reduced to the condition of carbonic anhydride and water. A boiler on the fire may drive a steam-engine. The heat imparted to the water in the boiler is the source of the power of the engine, which latter depends for its working properties on the difference between the temperature of the steam and that of the condenser in the low-pressure, or the external air in the high-pressure engine. The machine is a heat-engine.

To take another example. A galvanic battery, when in action with a short thick copper wire completing the circuit, develops a considerable amount of heat in its cells. If it be made to work an electro-magnetic engine, part of the heat disappears from the battery to appear as work done and friction overcome. The machine depends for its power of doing work, not on the heat developed, but on the disruption of chemical affinities, produced by the contact of dissimilar metals. This cannot, therefore, be termed a heat-engine.

It is very important, for a proper understanding of the subject under consideration, that we should have some idea as to what is the mechanism of the living locomotive system. Are the muscles of the body heat-engines, or do they convert the energy of chemical affinity directly into work? This question can only be answered very incompletely in the present state of our knowledge. All facts and arguments at our disposal are, however, totally opposed to the assumption that muscular fibre works on the principle of a heat-engine. Many cold-blooded animals—that is, animals with a temperature but slightly above that of their surrounding medium—possess a very effective muscular mechanism; witness the grasshopper and the frog; in them locomotion is prompt and powerful; yet they are scarcely warmer than the atmosphere. There can be little doubt, as remarked by the illustrious Joule, that an animal more closely resembles an electro-magnetic than a heat-engine; and such being the case, it is not to the direct action of the muscles that we must look for the source of animal heat.

A valuable simile, suggested by Fick and Wislicenus (but here somewhat modified), will assist in making this somewhat difficult subject more clear. According to them a bundle of muscular fibre is a kind of machine, consisting of albuminous material, just as an electro-magnetic engine is made of iron, brass, &c. Now in the battery of this engine zinc is consumed in order to produce force; so in the muscular machine fats are consumed for the same purpose. And, in the same manner as the constructive material of the engine (iron, &c.) is worn away and oxidised by wear and tear, so the constructive material of the muscle is worn away by the exercise of its function. And, as above shown, the reduction of the carbon and the hydrogen of our food to the used up state of carbonic anhydride and water, is accomplished otherwise than by the direct development of heat.

As in the electro-magnetic machine, when the metals of the battery are not immersed in the fluids of the cells, the engine does not act, and no heat is generated; so, whilst the muscular fibre is at rest it develops no heat, except that which may be evolved in the repair of its framework by the albuminous constituents of the blood traversing its capillaries. Immediately, however, the nervous impulse for action arrives at each fibre, chemical change is started; as when the metals of the battery-cell come in contact with the acid, heat is developed, and work is performed. This heat adds to the warmth of the body, as may be seen by the rise in temperature attending a walk in the accompanying instance. In this case the thermometer for some time before the commencement of the walk—which was at the rate of four miles an hour on level ground—had registered 98°, my time being employed in standing about:—

Time.	Temperature.	
12·45	98·4	
1· P.M.	99·1	Walking for 5′.
1·15	99·5	Walking.
1·30	99·6	"
1·45	99·675	"
2· P.M.	99·65	Ceased walking 5′.
2·15	99·2	Standing.
2·30	98·95	"

But during rest the voluntary muscles do not act; it is not therefore to them that the ordinary heat of the body can be ascribed. There are, however, muscles which are continually acting, such as the heart and respiratory muscles. These do a large amount of work, the former driving the blood through the whole circulatory system at an enormous rapidity, under great pressure; the latter expanding the chest nearly twenty times a minute. The work they perform being so great, the necessary chemical decomposition appears mostly as such, instead of as heat in the organs themselves. Nevertheless, it not manifesting itself externally, shows that it must all be ultimately converted into heat in the system, in the resisting capillaries, and in the chest-wall.

Here, therefore, is a considerable source of heat, to which must be added the vermicular movement of the intestines, together with that of the sphincters. Statistics for the calculation of whether these forces are sufficient to account for the total heat of the body are not forthcoming. The amount of blood which traverses the large arteries in a given time is not yet known, and the value of the minor forces is far from easy to estimate. There is no doubt, however, that the heat developed by these processes is considerable, and that rise in temperature is correlated with activity of function. I have elsewhere* given reasons to show that the pressure of the blood in the arteries is

* Journal of Anat. and Phys., vol. viii. pp. 54, 189.

independent of the pulse-rate, therefore the quicker the pulse the greater the amount of work which the heart has to perform. This throws some light on the high temperature in pyrexia, consumption, and other diseases in which the heart's action is conspicuously rapid; and explains how it is that the pulse and the temperature so frequently tell the same tale, rising and falling simultaneously.

In the animal kingdom there are two different ways in which the internal body-heat is distributed. In the so-called cold-blooded animals—including all the invertebrata, the fishes, amphibia, and reptiles—the temperature of the deep parts is but slightly higher than that of the surface, and there is no arrangement for maintaining a uniform internal heat. In birds and mammalia, the warm-blooded animals, the internal temperature is very constant, and much higher than that of the cold-blooded animals of this country.

As the result of a large series of observations, the average human temperature is known to be 98°·6, and it is the same in the negro that it is in the Greenlander; the same in the Englishman, whether he is here or in Borneo. Different hot-blooded animals, nevertheless, have differences in their temperature. Dr. John Davy's observations show that in the sheep and goat the average is nearly 104°, in a squirrel and a rat it was 102°, and in a hog 105°. Birds possess a temperature higher than that of any mammalia, it frequently reaching from 109° to 110°. In them the activity of the circulating and respiratory processes is very great.

The human frame being thus evidently, as far as we are at present concerned, nothing more than a mass of matter, with a constantly developing internal source of heat, the next point that has to be considered is the mechanism by which the wonderful uniformity that is found to exist in the living body is maintained, especially when it is remembered that the variations in the heat of the external air and coverings are so considerable.

On myself, as the result of nearly five hundred observations, the limits of my temperature have been 97°·5 and 100°·3, a range of scarcely 3°, some of the results having been obtained in the hottest rooms of the Turkish-bath, and others whilst in an atmosphere of 45°. The arrangement by which so great a uniformity in the temperature can be obtained must be an exquisite one, as anybody who has attempted to hatch birds' eggs in an artificial incubator, or who has at any time found it necessary to keep a calorimeter at a constant temperature for a considerable time, will fully appreciate.

The surface of the body being constant in extent, this uniformity cannot be the result of changes in that. It must, therefore, depend on variations in the radiating and conducting power of the skin itself. The radiation may be left out of the question, because, though the differences of colour in the surface which depend on its congestion or its anæmic condition will have some influence, that must be quite insignificant in comparison with other forces which come into play. A box, whether of metal or porcelain, whether black or white, whether dull or burnished, no doubt radiates differently, but none of the possible changes of the healthy skin can in any way compare in importance with any of these differences. We must therefore look to modifications of the conducting power of the surface for the mechanism by which the uniformity is arrived at, and in doing so it will be necessary first to take a glance at the structure of the skin itself.

The skin is composed of two main layers, a superficial non-vascular covering—the epidermis—which protects the parts beneath, in the same way that lacquer does the brass-work of a microscope. This epidermis is nearly white, and translucent to a certain extent; it is the layer most of which is raised from the surface when a blister "draws." The second deeper stratum is the true skin, or corium. It consists of connective tissue and fat, which support innumerable minute branches of vessels and capillaries. Nerves and sweat-glands abound, varying in number in different parts. The small vessels, the arteries and the veins, unlike the capillaries which connect them, are protected by walls which contain muscular fibres; in fact they—especially the former—are little more than muscular tubes, which are capable of varying in diameter and length according to the extent of contraction of their coats. The muscular arteries are supplies with nerves, which are in direct communication with ganglia, or miniature centres of nervous action, and are not capable of being influenced by the will. When the circular or transverse muscular fibres of the small arteries come into action, by reducing the calibre of the vessels they lessen the flow of blood to the capillaries; the skin becomes less red; and quite pale when the contraction is extreme, as in fright. When the small arteries relax, blood freely enters the skin, the crimson of the blush being the result. The calibre of these small cutaneous vessels is entirely regulated by the currents traversing the nerves which communicate with them, and it is by modifications in it that the conducting power of the skin is capable of varying to the extent required to maintain a uniform body temperature.

To take an example of the manner in which this is proved to be the case:—The air being close upon 70°, the clothing being ordinary summer dress, the temperature in the mouth is observed to be 99°·15 at 10·15. The following consecutive series of observations will prove the effect of rapidly removing the clothes at 10.30.

Time	Temperature	Time	Temperature
10·15	99·15	10·40	99·125
10·20	99·15	10·45	99·175
10·25	99·15	10·50	99·175
10·30	99·125	10·55	99·175
10·35	99·025	11 P.M.	99·175

From this Table it will be seen that the effect of stripping is, in this case, only to produce a rise in the temperature of 0·05°, which is scarcely worthy of note. Such being the case, and it being evident that the removal of the clothes has altered the heat relations of the subject of experiment to external objects, some physiological influence must have come into play to reduce the superficial conducting power of the body, so that the loss of the clothing shall be made up for. Cold to the surface contracts the vessels of the corium, as can be readily proved by placing a piece of ice on the warm hand, when the place on which it has rested becomes pale. The cooling effect of the air at 70° must, therefore, have produced just sufficient contraction of the cutaneous vessels to compensate for the removal of the badly conducting clothing.

I chose an atmospheric temperature of 70° for this illustration purposely, in order to show this negative result, as it may be termed. At higher external temperatures the complicating influences of perspiration have to be taken into account; and they are slightly perceptible in the above instance, the fall of temperature at 10·35 being caused by the evaporation of the moisture from the skin, which was previously retained there by the clothing.

Time	Temperature	Temp. of Air	Time	Temperature	Temp. of Air
11·15	98·95	47°	11·15	98·8	52°
11·20			11·20		
11·25	98·975		11·25	98·8	
11.30	98·975		11·30	98·8	
11·35			11·35		
11·40	99·3		11·40	99·	
11·45	99·35		11·45		
11·50	99·575		11·50	99·35	
11·55	99·625		11·55		
12 night	99·7		12 night	99·375	
12·5	99·675				
Stripped at 11.30 Rise 0°·725			Stripped at 11.30 Rise 0°·575		

When the air is below 70° there is always a well marked rise in the body-temperature on stripping, greater as the intensity of the cold is greater, as might be expected from the previous explanation. The preceding and following Tables will illustrate this at a glance :—

Time	Temperature	Temp. of Air	Time	Temperature	Temp. of Air
10·45	98·1	59°	11·5	99·	67°
10·50	98·		11·10	99·	
10·55	97·925		11·15	99·	
11 P.M.	98·		11·20	99·	
11·5	98·19		11·25	99·05	
11·10	98·35		11·30	99·19	
11·15	98·4		11·35	99·21	
11·20	98·425		11·40	99·19	
Stripped at 10.55 Rise 0°·5			Stripped at 11.15 Rise 0°·2		

The cutaneous contraction at temperatures below 70° is therefore more than sufficient to make up for the loss of the clothing if the experimenter remains quiet, which was always the case in the above instances; and it is quite surprising for how long a time it is possible, on this account, to stand nude in an atmosphere even as cold as 40°, without any inconvenience or unpleasant sensations in the extremities. In fact, as has been remarked by others, it is only a way of taking a bath—an air-bath—milder, it is true, than a sponge-bath, but almost equally beneficial in a sanitary point of view.

After having remained some time in this cold air-bath suppose the experimenter to reclothe, or to get into bed. By so doing it is evident that all the previous compensating arrangement has to be much modified if the uniformity of the temperature is to be maintained. And such is the case. The primary effect of the contact of the clothing is to prevent any further conducting away from the surface of the heat, not great in amount, which the previously chilled, and if very cold, "goosey" skin, has been losing. Within a short time the clothes get warmed, and before long become sufficiently heated to cause a universal relaxation of the cutaneous muscular arteries, which allows of a full flow of blood into the capillaries, in such cases known as the glow, at the moment of production of which the temperature of the body commences to fall rapidly, and continues to do so for some half-hour or more, because the blood, entering the skin in abundance, imparts its heat freely to the still only partially-warmed clothing, and receives no extra supply to compensate for its loss. The glow after a cold-bath has an exactly similar origin. The following example will serve to illustrate these facts :—

Time	Temperature	Temp. of Air
12 night	99·4	51°
12.5		
12.10	98·8	
12.15	98·6	
12.20	98·4	
12.25	98·2	
12.30	98·	
12.35	98·	
12.40	97·8	
12.45	97·8	
12.50	97·75	
12.55	97·65	

In this case, after having remained nude for half an hour, during which time the temperature rose three-fifths of a degree, getting into bed at twelve o'clock resulted in a fall of a degree and four-fifths within an hour—an amount which is very considerable when the shortness of the time is taken into account. On several other occasions I have been able to observe a similar fall under similar circumstances. The glow in this case was felt at 12.7.

By another method the effect of heat in dilating the vessels of the skin, and so reducing the body-temperature, can be simply proved. If, after remaining some time nude in a cold air as above described, when the thermometer under the tongue has ceased to rise, if the feet or hands are placed in *hot* water, the temperature immediately commences to fall, the more rapidly the hotter the water. This apparently paradoxical result depends on the fact that the localized heat dilates the cutaneous vessels generally, and so causes more to be lost from the exposed parts than is gained from the hot water. Directly the contact is made with the water a shiver is felt, which is a certain indication of the general relaxation of the cutaneous vessels. The following is a case in point :—

Time	Temperature	Temp. of Air
11.35	99·85	53°
11.40	99·85	
11.45	99·5	
11.50	99·05	
11.55	99·	

In this instance the temperature while nude had remained some time at 99°·85, when immersing the feet, at 11.40, in water at 112°, caused the prompt fall which is here recorded.

On a cold day anyone sitting in a large room with one side or the back to a warm fire for some time is fairly certain to feel the cold to a more than ordinary extent in the distant arm, or in the hands, and this is because the fire heating the parts exposed to its influence increases the circulation through the skin of both sides, and therefore allows of excessive cooling in the unwarmed limbs. Rheumatism of the shoulder and knee, and tic-doloureux, are often so caused.

It is in the Turkish-bath that, in this country at least, the effects of an atmosphere hotter than 70° can be best investigated. There is, however, a precaution with regard to the thermometer itself which must always be taken in employing it in an air exceeding 110°. This being about the highest point to which the clinical instrument is generally graduated, and to which its tube extends, any much greater heat will split it. A fair-sized mercury reservoir at the end of the tube will obviate this difficulty; but then the thermometer cannot be a self-registering one, as it would be impossible to keep the small detached index from joining the main column at high temperatures. In the Turkish-bath it is best to employ a thermometer at least a foot long, not graduated near the bulb, and with a dilatation at its other end. This can be read off by a companion, or, better still, by the experimenter himself, with the assistance of a movable reflector fastened to the stem of the instrument two inches above it, at an angle of 45°, in such a manner that it can be made to slide from end to end.

Perspiration commences very quickly when the hot room of the Turkish-bath is entered; and though on going into the hotter chambers, one after the other, the amount of perceptible moisture does not appear to increase, this is because at each stage the evaporation becomes more rapid. This is proved by the excess of the cutaneous moisture, always found on first returning to the cooler rooms, when also the temperature, from being nearly stationary commences to rise more evidently.

In none of my previous remarks have I included the effects of the water-bath, because directly the body is immersed in fluid, the regulating action of the skin can come but slightly into play. The morning tub, short in duration, and not accompanied with complete immersion, raises the temperature, as does exposure to cold air; and a similar glow is subsequently felt. The bath has a very different effect if at all prolonged, even in tepid water, and before breakfast. Five minutes after a swim of four minutes duration in the sea at 74° my mouth-temperature was 96°, which gradually rose two degrees by the end of an hour, and another degree in the next hour, during which I had breakfasted.

On another occasion, with a temperature of 98°·9, remaining in a hot-bath recording 111°, my temperature rose to 103°·75 in less than twenty-five minutes, when faintness and great giddiness came on. In twenty minutes after leaving the bath my temperature had fallen between three and four degrees.

These show how little the body is capable of withstanding changes in the temperature on occasions when the skin cannot be called into action, a difference in temperature of 37° only, brought into action for a very short time in each case, causing a variation in body-temperature of seven-and-a-half degrees.

In the above slight sketch of some of the most interesting points connected with this instructive branch of physiological science, my desire has been, by a few well-marked instances, to illustrate how much there is to be learnt from the employment of the thermometer in the study of some of the most commonplace phenomena of every-day life; and so to stimulate others to prosecute similar enquiries in this rich field of biological research, which is, in many respects, but a blank to us as yet. Dr. William Marcet and Dr. Lortet have recorded the rapid fall in temperature which occurs in the ascent of mountains; the former able author has promised further observations on the effects of descending from great heights; as yet, however, he has not published any results in that direction. Any amateur tourist who, in a trip to Mount Blanc or Mount Etna, could supply this information, would do work which is certain to be fully appreciated by physiologists generally. Dr. Jürgensen and Dr. Finlayson have given curves, representing the daily fluctuations in the human temperature, which are particularly constant. These curves can be nothing else than the sum of the various forces which I have above endeavoured to show are continually coming into play during our daily life; nevertheless, there are still many gaps to be filled up before the complete proof that they are so is arrived at. The whole subject is fraught with interest in all directions.

THE VEGETABLE CELL.

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

LECTURER ON BOTANY, ST. THOMAS'S HOSPITAL.

IN few departments has science-teaching undergone so great a change in recent years as in the mode in which the botanical student is instructed in the rudiments of his science. It is not many years since botany was thought to consist in a knowledge of the names of plants, and a facility in distinguishing and naming closely-allied species differing from one another in the most minute characters. The real history of the structure of the plant, the mode in which its various tissues are formed, the function which each part is destined to fulfil, were not thought to form any part of the programme. Teachers of botany now recognise that their science has a much wider scope and a far nobler aim. He may be a profound botanist who has no knowledge of "critical" species, who would hesitate in assigning the most recent Latin name to half the flowers he might gather in wood or by wayside. To make himself acquainted, as far as Nature will reveal her secrets, with the internal economy of the subjects of his study, is the main object of his observations and of his labours. And for this purpose the microscope must be freely used. Indeed, it is only since our opticians have produced instruments of such power and comparative perfection that we have been able to gain much insight into the internal structure of plants.

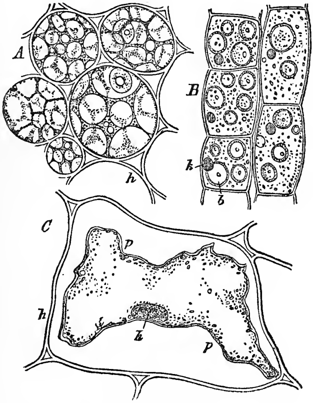
At the foundation of all vegetable anatomy lies a knowledge of the structure of the Vegetable Cell. Though a few earlier writers obtained some insight into the part played by it in the structure of plants, Schleiden was the first, in his "Principles of Scientific Botany,"* to present the theory of the cell in a connected and complete form; Schleiden's account being confirmed, carried into more minute details, and in some points corrected,

* Translated by Edwin Lankester, M.D., and now published by R. Hardwicke, 192 Piccadilly.

by subsequent observers, chiefly belonging to Germany, von Mohl, Nägeli, Hofmeister, and others.

It cannot be too clearly understood by the beginner that all vegetable tissues, of whatever kind, are formed originally from cells of the structure we are about to describe, and that all growth is the result of the multiplication, in some way or other, of these cells. There are plants of so simple a structure—Unicellular Algæ and Fungi—as to be formed of but a single cell; others, also Algæ and Fungi, consist of but a single filament of cells; but all the higher plants are aggregates of cells, infinite in number, which, in the more complicated forms of tissue, have undergone modifications in a great variety of ways.

Fig. 1.



Forms of cells; *A* and *B*, from the maize; *C*, from tuber of artichoke, after action of iodine and dilute sulphuric acid; *h*, cell-wall; *p*, protoplasm; *k*, nucleus.

The vegetable cell is a sac or vesicle completely closed on all sides; the form of the cells may be easily recognised by placing under a comparatively low power of the microscope thin sections of potato, elder-pith, or a similar tissue, or in the semi-transparent leaves of *Anacharis* or *Vallisneria*, or those of *Sphagnum*, which consist of only a single layer. The cell represented in fig. 1 C.* is from the tuber of the artichoke.

* All the woodcuts in the present article are borrowed, by permission of the English publishers, from the English edition of Sachs's "Text Book of Botany," the most complete and trustworthy work on Vegetable Morphology, about to be published by the Clarendon Press, Oxford.

The coating of the sac or cell-wall (*h*) consists of a very fine membrane of cellulose; within which are a variety of substances known as cell-contents. Two of these are invariably present in all young growing cells—water, and a mucilaginous, more or less granular substance known as protoplasm (*p*); situated within which, but varying in position, generally almost close to one side, is a nearly transparent body consisting of denser, almost solid protoplasm, the nucleus (*k*). The structure and composition of each of these parts must now be examined somewhat more in detail.

The *Cell-wall* is always of uniform chemical composition, its constitution being $C_6H_{10}O_5$, identical with starch and dextrine. Though perfectly continuous and destitute of any pores or orifices that can be detected even by the highest powers of the microscope, the cell-wall is, nevertheless, permeable to fluids, the cell-sap passing into the cell by the process of osmose; that is, the passage through a permeable diaphragm of the less dense of two fluids of different specific gravity, which are separated by the diaphragm. The substance of the cell-wall is secreted from the protoplasm which it contains; but, according to Nägeli, Sachs, and other competent authorities, its growth in thickness does not depend, as was at one time supposed, on the formation of new consecutive layers, each within those already in existence, but on the "intussusception" or interposition of fresh particles or molecules of cellulose, which penetrate into every part of the cell-wall from the protoplasm. The cell-wall is perfectly colourless, and when thin transparent, so as to reveal the internal structure of the cell.

The *Protoplasm* is the essential life-giving portion of the cell. It is not homogeneous in its consistency; the layer nearest the cell-wall is somewhat denser, and forms a kind of skin or bladder enveloping the interior portion, and was termed by Mohl the "Primordial Utricle" (Primordial-schlauch), a term which is still generally applied to it. The *Nucleus** already described is present in the early stage of the cells of all higher organisms without exception; but that it is not an essential ingredient is shown by its absence from Unicellular Algæ and the cells of some other lowly organisms. Very commonly the nucleus contains other smaller bodies of a similar character within it, the Nucleoli. The protoplasm is often absent from one or more cavities in the interior of the cell, termed vacuoles, which are occupied by the cell-sap. The protoplasm may be contracted and separated from the cell-wall by the application of iodine and dilute sulphuric acid; a drop of iodized solution of chloride of zinc causes the cell-wall to assume a beautiful

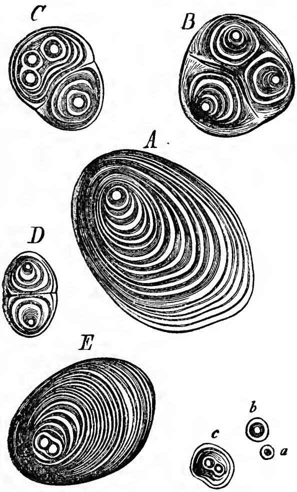
* Called by Schleiden and the older writers, the Cytoblast.

blue colour, the nucleus and the rest of the protoplasm retaining a brownish yellow tint. The exact part played by the nucleus in the functions and in the multiplication of cells is a point at present involved in much obscurity. Protoplasm differs in its chemical composition from cellulose in containing nitrogen; but the experiments hitherto made have failed in assigning to it any definite and constant formula.

In addition to water and protoplasm, which are contained in every cell when in a growing condition, other substances are frequently or generally present, of which some of the most important may here be mentioned.

Starch is the first product of assimilation, that is, of the

Fig. 2.



Starch-grains from a potato-tuber (× 800). *A*, an older simple grain; *B*, a partially compound grain; *C*, *D*, perfectly compound grains; *E*, an older grain, the nucleus of which has divided; *a*, a very young grain; *b*, an older grain; *c*, a still older grain with divided nucleus.

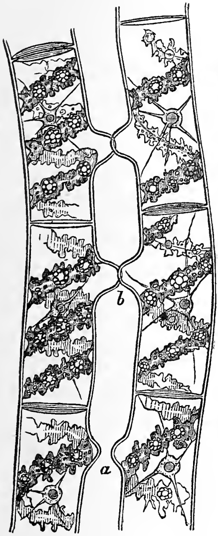
union of the elements of water absorbed by the root with the carbon removed by the leaves from the carbonic acid of the atmosphere. Its chemical composition is identical with that of cellulose, but its properties are different. It never enters, moreover, into the constitution of the cell-wall, but is always imbedded in the protoplasm in the form of distinct grains, the form of which is nearly uniform in the same species of plant. If a drop of dilute solution of iodine is placed on a thin section of a potato, the grains of starch are beautifully brought out under the microscope by the bright violet colour they assume, the cell-wall remaining colourless. The pith of the elder or the bulb-scales of the hyacinth form equally good preparations, starch being invariably found in those parts of the plant—as bulbs, tubers, rhizomes, albuminous seeds, &c.—where the food-material is stored up for the nutrition of the young plant, whether it be developed from an embryo or a leaf-bud. Starch-grains generally contain a body which may be defined as the nucleus, and the growth of the grain invariably takes place by intussusception, new particles of the formative material becoming intercalated, according to Sachs, between those already in existence; the new matter assuming the form of concentric layers of greater and less density, that is, containing a smaller and larger admixture of water. Grains sometimes become compound by the production of several nuclei in their interior.

Chlorophyll is the substance which gives the green colour to leaves and branches, and may be considered as a peculiar form of protoplasm, the green colour being probably due to the admixture of a very small quantity of iron. The constitution of this substance has been very carefully investigated by Mr. H. C. Sorby, who finds it to be composed of different bodies, especially a blue and a yellowish-green one, the mixture of which gives the familiar leaf-green colour. The chlorophyll always occurs in the form of minute granules interspersed through the protoplasm, which vary their position in the cell according to the intensity and direction of the light. They are formed only in the presence of sunlight, either direct or diffused; and it is only those parts of the plant which possess them that have the power of decomposing the carbonic acid of the atmosphere. Independently of this alteration in position of the chlorophyll-grains, the protoplasm itself has often — perhaps always—a rotating or circulating motion within the cell; this can be easily perceived in the leaves of *Vallisneria*, the stems of *Chara*, the hairs on the filaments of *Tradescantia*, the stinging-hairs of the nettle, &c.

Raphides are minute crystals, generally of oxalate of lime, contained within the cell; their purpose is not known; possibly the fixation of oxalic acid, which might otherwise be injurious to the plant. They abound in many plants, as the stem of the *Cactus*, the leaf-stalk of rhubarb, &c., and may very easily be made out in a section of the leaf of the hyacinth.

Having described the structure of the cell and its contents, we may now follow the various modes in which cells multiply, or by which the different kinds of tissue are formed from the original cell. The various modes of the multiplication of cells may be classed under four heads:—(1.) Free cell-formation; (2.) Cell-formation by Conjugation; (3.) The Renewal or Rejuvenescence of cells; and (4.) Cell-division. Of these the last is by far the most common; but the three first are of the utmost importance, as throwing great light on what it is that constitutes the vital principle of the cell.

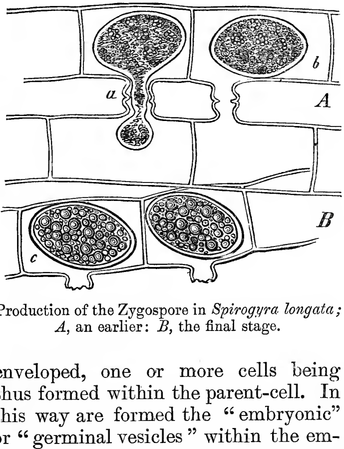
Fig. 3.



Two filaments of *Spirogyra longata*, showing the commencement of the process of conjugation (× 550).

1. *Free Cell-formation* is of comparatively rare occurrence. It consists in a portion of the protoplasm within the primordial utricle of a cell becoming separated, and secreting a membrane of cellulose in which it becomes

Fig. 4.



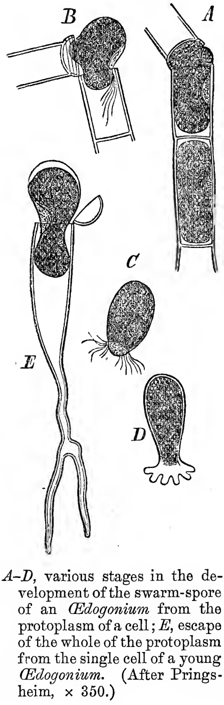
Production of the Zygospore in *Spirogyra longata*; *A*, an earlier: *B*, the final stage.

enveloped, one or more cells being thus formed within the parent-cell. In this way are formed the "embryonic" or "germinal vesicles" within the embryo-sac, as well as the first cells of the endosperm or "albumen" in the ovules of Angiosperms; and the spores within the asci of some species of Ascomycetous Fungi, as *Peziza*.

2. *Cell-formation by Conjugation* is also an exceptional phenomenon, being exhibited only in the reproduction of some of the lower classes of Algæ—the Palmellaceæ, Desmidiaceæ, Diatomaceæ, and Zygnemaceæ (sometimes collected together into the single group of "Conjugatæ"), and in a few genera of Fungi, as *Syzygites* and *Mucor*. A very good instance of

the phenomenon is presented by an abundant and well-known fresh-water Alga, *Spirogyra longata*, which forms a most beautiful object under the microscope, from its chlorophyll-grains being combined in each cell into a spiral band visible through the transparent cell-wall. Figs. 3, 4 represent the mode in which this process takes place. The filaments of this Alga consist of a row of cylindrical cells, each of which contains a mass of protoplasm, and the conjugation takes place between the adjacent cells of two more or less parallel filaments. Protuberances (fig. 3 *a*) first of all make their appearance at opposite points of these adjacent cells, which at length meet (*b*). The cell-wall then gives way between them (fig. 4 *a*), and the two masses of protoplasm coalesce, the whole finally passing over into one of the cells (*b*), and the resulting mass at length becomes coated with a cell-wall of cellulose (*c*), forming the reproductive body known as a "Zygospore." In the unicellular Desmids and Diatoms, and in the Mucorini, the process is similar.

Fig. 5.



A–D, various stages in the development of the swarm-spore of an *Œdogonium* from the protoplasm of a cell; *E*, escape of the whole of the protoplasm from the single cell of a young *Œdogonium*. (After Pringsheim, × 350.)

3. *Renewal or Rejuvenescence of Cells.*—This process takes place in the formation of the "Swarmspores" or "Zoospores" of Algæ. The whole contents of a cell of a filament contract, and the mass of protoplasm assumes an ovoid form, with a broad green and a narrower hyaline end, provided with cilia or fine threads of protoplasm either at this latter end or surrounding the whole spore. The cell-wall gives way, and, as the swarmspore escapes from it, it moves rapidly forward with its narrower end in front, propelled by the vibratile motion of its cilia, till it at length comes to rest, when its cilia disappear, and it attaches itself by means of rhizoids or root-hairs; and then only the naked ball of protoplasm becomes coated or encysted with a cell-wall secreted out of its own substance, and finally developes into a new filamentous Alga. Fig. 5 shows the various stages of this process in the case of an *Œdogonium*. This

process, as well as those already described, is relied on as demonstrating a point of cardinal importance in vegetable physiology, viz. that the vital portion of the cell is its protoplasmic contents, independent of the cell-wall.

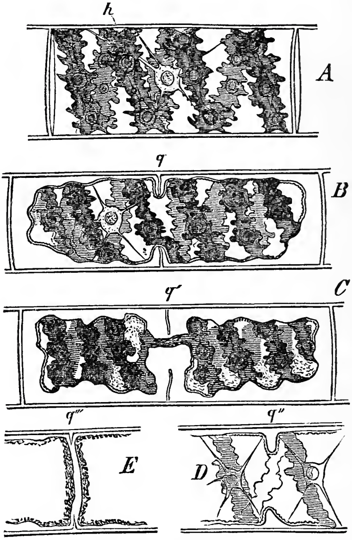
4. *Cell-division* is the process which occurs in all reproduction of cells connected with vegetative growth; i.e., it is the sole means by which tissues are produced. The following modifications of the process are presented, viz.:—

a. Where the whole of the protoplasm contained within the primordial utricle divides into derivative, or daughter-cells; but these daughter-cells do not secrete cell-walls until they have become completely isolated. This is the mode of formation of the zoospores of *Achlya* and other less highly developed Algæ, as well as the spores of *Equisetum*. If the whole protoplasmic body, in its contraction, were to form only one ball, the process would become one of renewal or rejuvenescence. Two varieties occur of this modification, according as the protoplasmic body contracts during its division, owing to the expulsion of water, or no contraction takes place. The pollen-grains of some Endogens are formed on the latter principle.

b. Where the daughter-cells formed by the division of the protoplasm of the mother-cell become coated with a cell-wall of cellulose while the division is taking place. This is again subject to two varieties, depending on the contraction or not of the protoplasmic body during division. The ordinary mode of the formation of the pollen of Exogens within the anther belongs to the first of these categories; the second illustrates the mode in which ordinary cellular tissue is formed by the multiplication of cells. Fig. 6 represents this most common form of cell-division in the case of the filamentous Alga *Spirogyra longata*. The protoplasm-sac first begins to fold in at two opposite points, the line connecting which would often pass through the nucleus. The nucleus divides into two, the two halves gradually separating from one another and forming the distinct nuclei of the two new cells which are produced by the folding in of the protoplasm and the simultaneous secretion from the protoplasm of a new dividing-wall of cellulose.

Several apparently anomalous modes of the production of cells are known, to which special terms have sometimes been given; but they may all be referred to one or other of the modes now described. The formation of the "spores" of the Mould-Fungi, and of the "basidiospores" of the higher Fungi, is sometimes described as if it consisted in the "basidium" or basal cell forming several spores in succession, which become detached one after another from the parent-cell. What really takes place is in all cases a bipartition of the cell of the hymenium, but into two very unequal portions; the smaller portion or "basidiospore" becomes detached; the larger portion remains behind, rapidly grows to its original size, and again and again repeats the same process. The process described in some text-books as the "Gemmation" or "Budding" of cells, characteristic of the yeast-plant and other very lowly organised Fungi,

Fig. 6.



A, B, C, a cylindrical cell of *Spirogyra longata* in successive stages of division; *D, E,* the central portion of the same cell; *q,* the new wall of cellulose in the process of formation.

is a simple process of cell-division. The protoplasm-sac of the cell swells on one side, carrying the cell-wall along with it; the papilla thus formed becomes subsequently separated by constriction, secreting an intermediate wall of cellulose, and ultimately becomes detached as an independent cell.

It only remains to describe the mode in which cells develop into tissues. The simplest forms of vegetable life, as the *Palmella cruenta* and *nivalis*, or "Red-snow," among Algæ, consist of single cells, which perform all the functions of life, both vegetative and reproductive. Others again, both Algæ and Fungi, are formed of simple rows of cells; but in all the higher forms of plants the cells are united into tissues, the various forms of which have formerly been distinguished into two classes, Cellular and Vascular Tissue. *Cellular Tissue* is the only form of tissue found in the lower classes of Cryptogams, Algæ, Fungi, Lichens, and Mosses, and in the rapidly growing parts of all plants. It consists of masses of cells which have undergone no material change from their original condition, being still closed sacs without communication with one another except through their permeable cell-walls, and containing protoplasm, since it is only by cells in this condition that the processes of vegetable life and growth can be carried on. The "cambium region," in which is formed the new wood of all woody plants, consists entirely of cellular tissue. When the cells retain nearly their original form, being still roundish or elliptical, the tissue is termed *Parenchyma*; when the cells are greatly elongated in one direction, more or less attenuated, and overlapping one another, the term *Prosenchyma* is applied to it. *Vascular Tissue*, on the other hand, consists of cells which have undergone modification in two ways: by the thickening and hardening of the cell-wall, and by the fusion together of a number of cells. The thickening of the cell-wall seldom takes place uniformly, but more often in regular patterns; and thus are produced the forms known as annular, spiral, pitted cells, &c., giving rise to the production of tissues of a similar character.

Prof. Sachs, in his "Text-book of Botany," proposes the following classification of tissues:—

1. *The Epidermal Tissue.*—The outer layer of cells of an organ consisting of a mass of tissue becomes distinguished by the thickness and firmness of its cell-walls, and hence usually by the cell-cavity being less. In the lower forms of plants the passage from this to the internal tissue is gradual; in all the higher plants the epidermis is much more strongly differentiated. The hairs which so commonly cover the young parts of plants are invariably developments of particular cells of the epidermis, and the stomata (often erroneously called "breathing-pores," from a mistaken view of their function) are openings through the epidermis to the intercellular spaces that lie beneath. Cork is another form in which the epidermal tissue occasionally develops; and in the majority of our forest-trees it assumes the form known as Bark.

2. *The Fibro-vascular Bundles.*—The tissue of Vascular Cryptogams and Flowering Plants is traversed by separate string-like masses of tissue, which can often be completely isolated from the rest of the plant, and are the Fibro-vascular Bundles. Each separate fibro-vascular bundle consists, when it is sufficiently developed, of several different forms of tissue, and must there-

fore be considered as a tissue-system. A portion of the bundle always remains for a time in a condition capable of further development, constituting the *Cambium*, which frequently divides the bundles into two distinct portions, called by Nägeli the *Phloëm* and the *Xylem* portions of the bundle; the former consisting of succulent, generally thin-walled cells, the latter having mostly a strong tendency to thicken its cell-walls. It is not, however, necessary for the fibro-vascular bundle to contain pure woody tissue. Spiral vessels occur always in the veins of the leaves and in the medullary sheath of the stem of Exogens. The fibro-vascular bundles are extremely well seen in a transverse section of a leaf-stalk of any exogenous plant; from the petiole they ramify into the blade of the leaf, forming the veins or nerves.

3. *The Fundamental Tissue* is the term given by Sachs to those masses of tissue of a plant or of an organ which still remain in their original condition after the differentiation of the epidermal tissue and the fibro-vascular bundles. It may consist of various descriptions of tissue, but is always to a large extent parenchymatous. In the leaves of Ferns and Flowering Plants the fundamental tissue forms by far the larger portion, constituting the "mesophyll."

4. *Laticiferous Vessels* and *Intercellular Spaces* may occur in any of the three systems of tissue now described, and sometimes pass into them by insensible gradations. The true Laticiferous Vessels are canals filled with milky sap resulting from the coalescence of rows of cells, and lying in the phloëm portion of the fibro-vascular bundles. They occur abundantly in many orders of plants, as the Papaveraceæ, Convolvulacæ, Cichoriaceæ, Euphorbiaceæ, &c. The Intercellular Passages result from the separation from one another of rows of cells, and are also frequently filled with a milky, oily, or resinous fluid, as in Umbelliferæ and Coniferæ.

The different kinds of tissue now described are to be met with only in the more or less mature parts of plants. The parts which are actually in a state of growth, as the ends of shoots, leaves, and roots, consist of a uniform tissue, the cells of which are all capable of division, rich in protoplasm, with thin and smooth walls, and containing no coarse granules, to which the term *Primary Meristem* has been given. From this the Epidermal Tissue and Fibro-vascular Bundles are differentiated as the organ develops, the portion which undergoes comparatively little change being distinguished as the Fundamental Tissue. The terminal portion of an organ with permanent apical growth, which consists entirely of primary meristem, is termed the *Punctum Vegetationis*; or when, as is sometimes the case, it projects as a conical elongation, the

Vegetative Cone. A remarkable difference in the mode of development of the punctum vegetationis occurs almost uniformly between Cryptogams and Phanerogams. In Cryptogams the whole of the cells of the primary meristem almost invariably owe their origin to a single mother-cell lying at the apex of the punctum vegetationis, and called the *Apical Cell.* In Phanerogams, on the other hand, without exception, there is no single apical cell of this character; even when a cell lies at the apex, it is not, as in the former case, distinguished by its greater size, nor can it be recognised as the single original mother-cell of all the cells of the primary meristem.

We have attempted to give here only the merest outline of the constitution of the cell and of tissues. For the further elucidation of the subject we must refer our readers to the more modern text-books of botany, especially Prof. Sachs's "Lehrbuch," an English edition of which is about to be published by the Clarendon Press, Oxford.

THE CHANNEL TUNNEL.

By W. TOPLEY, F.G.S., Assoc. Inst. C.E.,

GEOLOGICAL SURVEY OF ENGLAND.

[PLATE CXIV.]

THERE have not been wanting men who held that, in some respects, our forefathers were better off than ourselves, notwithstanding all the triumphs of modern science and civilisation. In one particular, at least, the primitive inhabitants of Western Europe are to be envied. The men who waged war with the ancient British lion, the cave bear, or the hairy mammoth, had no need to cross the sea if they wished to extend their hunting-grounds far to the east. Similarly, the old inhabitant of central France could as easily, if he so chose, follow the hippopotamus in his migrations to the Thames. For, in those old times—as there is good reason for believing—a band of Chalk extended across what is now the Straits of Dover, and the area now known as the British Isles was united to the continent of Europe.

How long ago it may be since this old "bridge" was broken we cannot know. Probably no Englishman will regret that the bridge was broken, and few will wish it wholly restored. But, certainly, most of those who have occasion to cross the Channel will wish that we could again have the personal convenience of continuous land communication, without the political disadvantages which such communication would involve. Modern civil engineering has resolved to attack this problem. Some of the proposals for accomplishing it we will presently examine.

The plans which have been advanced for lessening the inconveniences of the Channel passage have taken various shapes. It would be impossible, within the limits of a short article, to go over them all; we will therefore confine our attention to those schemes which propose to solve the difficulty by constructing a submarine tunnel. Proposals not falling under this head are various enough. Embankments, bridges, and tubes, in one form or other, have been proposed; but the scheme which seems to have found most favour, with engineers and with the public, is that of a tunnel.

The idea of constructing a tunnel between England and France seems to have originated with a French engineer, M.

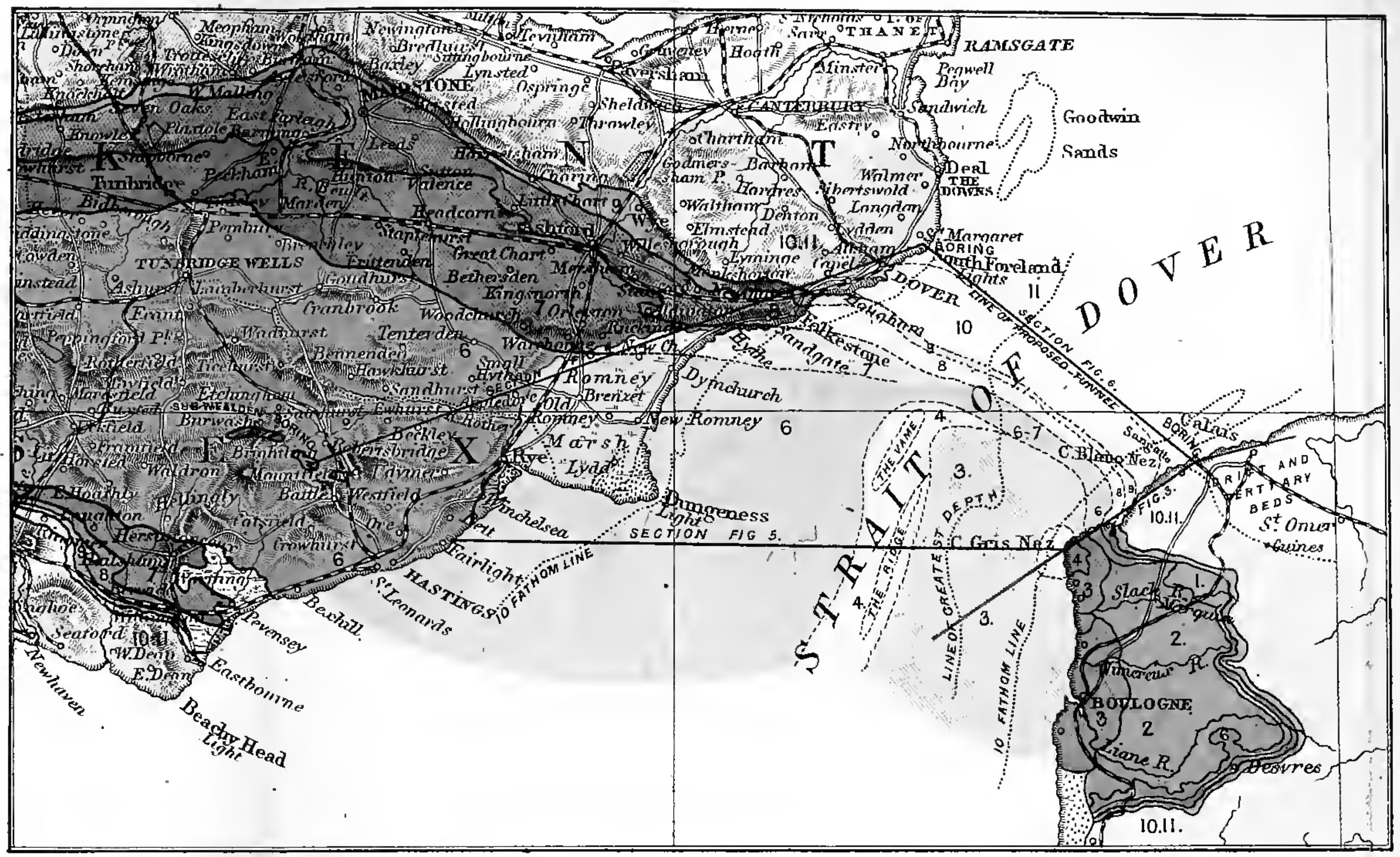
GEOLOGICAL MAP AND SECTIONS, ILLUSTRATING THE STRUCTURE OF THE STRAIT OF DOVER, AND THE PROPOSED CHANNEL TUNNEL.

by W. Topley, F.G.S. Assoc. Inst. C.E.

SCALE OF MAP.

Eng. Sta. 5 0 5 10 15 20 Miles

10 0 10 20 30 Kilometres.



Vertical Scale of Sections

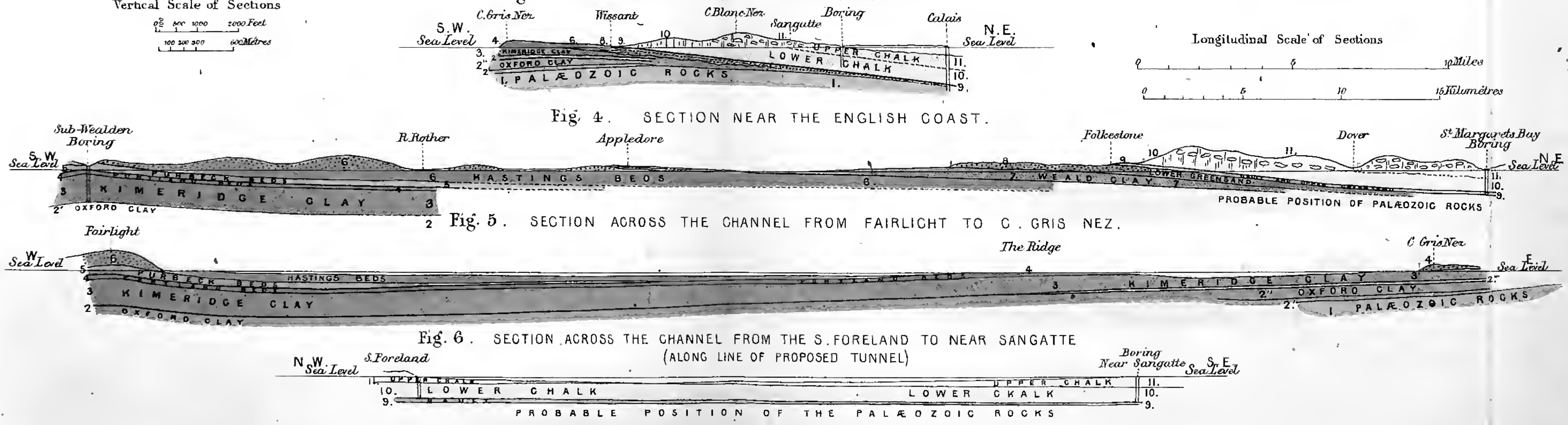
0 500 1000 2000 Feet

100 200 300 600 Metres

Longitudinal Scale of Sections

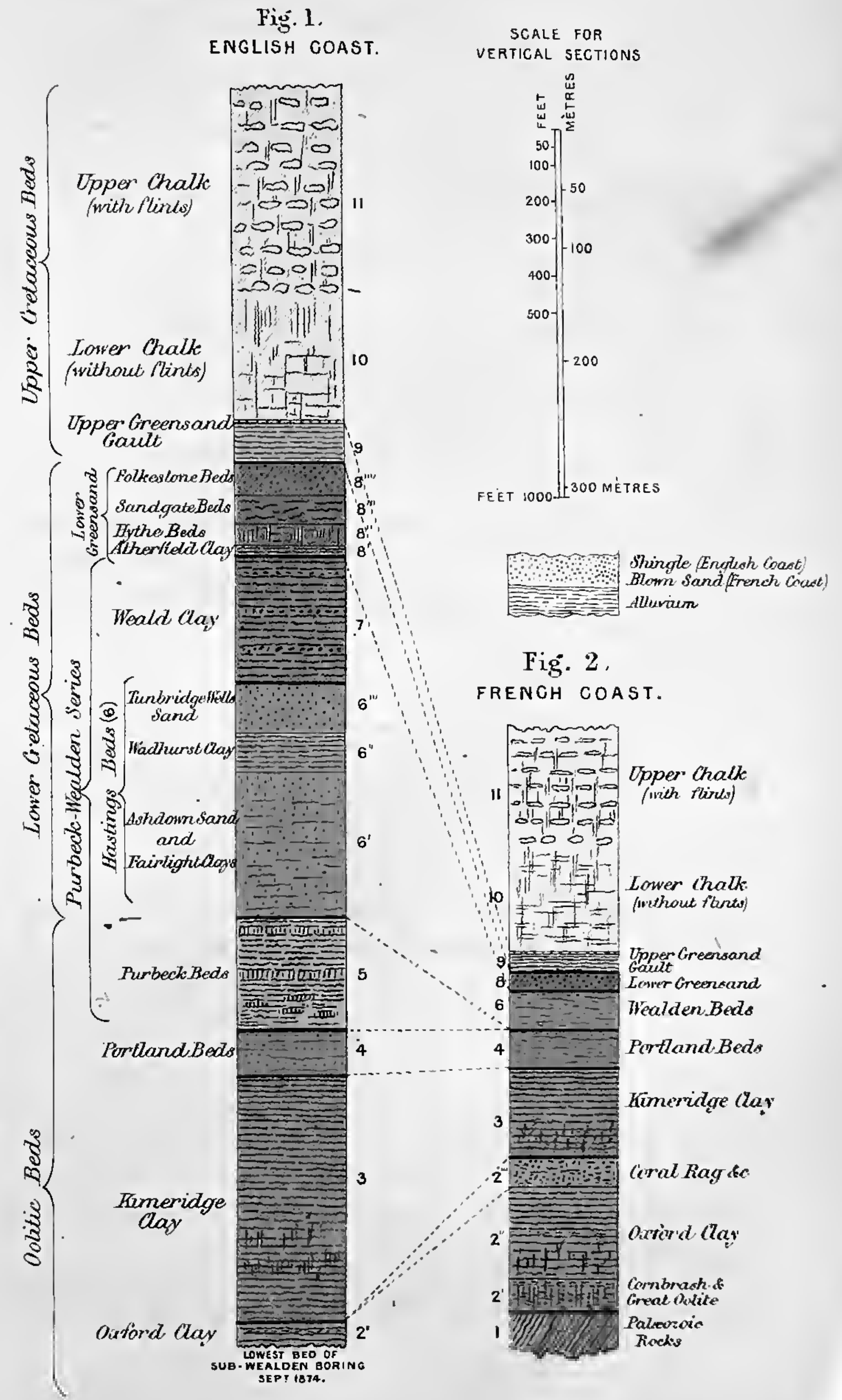
0 5 10 Miles

0 5 10 15 Kilometres



Stanford's Geogl. Estabt. Charing Cross.

INDEX OF COLOURS and COMPARATIVE VERTICAL SECTIONS OF STRATA



Mathieu. About the year 1800 he drew up plans, which were for a while exhibited in Paris, but which have long since been lost. In 1853 Mr. W. Austin devoted some attention to this subject, and published his plans in 1856, proposing to construct a tunnel from Boulogne to Cape Grisnez. Nearly the same line was chosen by M. Thomé de Gamond, who, in 1857, published an elaborate work on the subject. Since that time the "Channel Tunnel," in one form or other, has been pretty constantly before the public. Mr. W. Low had also devoted much attention to this question, and he concluded that the only mode of making a tunnel would be by taking it through the Lower Chalk. With this proposal M. de Gamond subsequently agreed, and he has now abandoned his original scheme in favour of that of the English engineers. To Sir J. Hawkshaw (the President Elect of the British Association) is due the credit of fully working out and developing the plan of carrying a tunnel through the Chalk. At his request Mr. E. C. H. Day, in 1864–5, made a detailed geological examination of the coasts, and constructed maps and sections of the strata. Sir J. Hawkshaw, with others, subsequently employed a steamer to take soundings in the Channel along or near to the line of the proposed tunnel, and he also made deep borings through the Chalk on each coast. The results of all these investigations are accessible, and we therefore possess a good deal of information bearing upon the question.

For several years it seems to have been generally conceded that the plan with which the names of Sir J. Hawkshaw and Mr. Brunlees are associated—that of taking a tunnel through the Chalk—is the most feasible, and comparatively little attention has been paid to other propositions. In December 1873 Prof. J. Prestwich read a very interesting paper before the Institution of Civil Engineers, in which he carefully examined the geological questions connected with the various schemes; and, disagreeing to some extent with all, he proposed to construct a tunnel through the Palæozoic rocks which underlie the secondary strata of the south-east of England, and which come to the surface near Marquise, in the Boulonnais. A long discussion took place after the reading of the paper, in the course of which much information was elicited regarding points connected with the tunnel.*

In considering the suitability of certain strata for sub-aqueous tunnelling, there are three main points to be considered. We should select strata which are (1) *impervious*, (2) *continuous*, and (3) *sufficiently thick*. Perhaps the only beds which fulfil all these conditions are the London Clay and the Kimeridge Clay; but any tunnel to pass wholly through either of these formations must be very long. Permeability and impermeability

* *See* the full report in "Proc. Inst. Civ. Eng.," vol. xxxvii., p. 110.

are, however, merely relative terms; no formation can be expected to be absolutely impervious for a long distance, and it becomes merely a question as to whether our methods of pumping can keep under the water likely to be met with.

Having now stated the main conditions of the question, we will proceed to examine the actual geological structure of the Straits of Dover, so far as this is known, in order to ascertain what is likely to be the arrangement of the strata along the lines of the proposed tunnels.

Sufficient information for our purpose is given on Plate CXIV., and if the arrangement of the rocks on the surface (as shown in the map) be compared with the arrangement beneath the surface, along certain lines (as shown in the sections), a very fair idea of the geological conditions will be obtained. The lowest bed yet known on the English side is the Oxford Clay, which has been reached in the Sub-Wealden Boring. Above this come the Kimeridge Clay and the Portland Beds, none of which appear on the English coast of the Channel until we get as far west as Dorsetshire. As yet we know little of the underground range of these beds to the north, but of the higher beds we do know something. These all dip gradually to the north (as shown in fig. 4), each passing regularly under the one next above it, until we reach the Chalk, which is the highest English formation with which we are now concerned.

Underneath the Chalk the Gault will everywhere occur (in the area in question); wherever the Gault is exposed at the surface we also know that the Lower Greensand will occur beneath it; and, again, beneath the area coloured as Lower Greensand on the map, we shall certainly find Weald Clay. But it does not follow, because we thus get a complete succession along the outcrops, that therefore the succession is complete at any one place. In sinking a well, for instance, near Dover, we should reach the Gault; perhaps also the Lower Greensand; but we might not find the Weald Clay, or the beds below it; probably we should come at once upon the Palæozoic rocks. In the section (fig. 4) this incomplete succession is shown, but here it is a matter of inference only; we have, as yet, no absolute proof that certain beds are wanting under Dover. In the section (fig. 3) which shows the structure along the French coast the case is different; here we have proof that the general structure is as is there shown.

The Oxford and Kimeridge Clays, and the Portland Beds, which nowhere appear at the surface in the south-east of England, occupy a great part of the ground in the Bas Boulonnais. They regularly pass under each other to the north-west, and are finally covered up by the Wealden Beds, the Lower Greensand, Gault, and Chalk. But we certainly know, from a deep boring at Calais, that these lower strata are there absent, and that the Gault (with a thin representative of the Lower Greensand beneath it) there rests upon the Palæozoic rocks.

The vertical sections (figs. 1 and 2 of Plate CXIV.) show the complete successions of strata near each shore, and some attempt has here been made to indicate the *characters* of the various beds. Horizontal shading indicates clay or shale, and consequently *impervious* beds; dots indicate sand, or *pervious* beds; vertical lines indicate limestones or calcareous beds, which are not usually very pervious in the mass of the beds, but only by reason of the joints or fissures which traverse them. The greatest *thickness* of the various beds is also shown in figs. 1 and 2; it often happens that the thickness of a bed varies in different places; this is shown in the longitudinal sections (figs. 3 to 6). The *continuity* of a bed on the surface is shown in the map; the continuity below ground, which is of more importance, is shown in the sections.

The map and sections, therefore, contain, as well as can be expressed on so small a scale, all the more important geological data which we at present possess for testing any proposed line of tunnel. But it may be necessary to state that the information here given is of very unequal value. The outcrops on the surface are sufficiently exact; the lines of the Chalk, Gault, and Lower Greensand beneath the Channel are probably tolerably near the truth; but the Wealden and Oolitic lines beneath the Channel are wholly conjectural. The evidence upon these points is excessively meagre, and future researches may show that the actual outcrops between Dungeness and C. Grisnez are very different from those here sketched. With the sections the case is different, and I have little doubt that the *general* geological structure along the various lines is very much as is there shown.

We will now examine the various strata in respect to their suitability for tunnelling. Applying the first test, that of impermeability, we at once discard the Lower Greensand, the Hastings Beds, and the Portland Beds.* These will certainly contain water in considerable quantity; most of the minor divisions of each of these formations are highly porous, and the divisions which are less so are not thick or continuous. No

* To describe in detail all the characters of the various divisions would carry this article to too great a length. Mr. F. Drew has described the English beds in the "Memoirs of the Geological Survey," Sheet 4; the French strata have been studied by Rozet, Hébert, Rigaux and others. The whole question has been stated by Prof. Prestwich in his paper already quoted. The "Geology of the Straits of Dover" (with especial reference to the Channel tunnels) was described by myself in the "Quarterly Journal of Science," April 1872.

project for a tunnel which runs the slightest risk of encountering any of these formations should be entertained.

The Weald Clay, on the English coast, is from 300 to 400 ft. thick; for the most part it is an impervious clay, well adapted for tunnelling, though it often contains beds of sand which yield water. But, however suitable for tunnelling the Weald Clay may be on the English coast, it does not exist at all as a recognisable division on the French coast, and therefore it, too, must be discarded.

The Purbeck Beds (formerly known as the Ashburnham Beds) come to the surface on the north-west of Battle, and in them the Sub-Wealden boring begins. Altogether they are about 300 ft. in thickness; they consist chiefly of shale and clay, but contain some beds of sandstone and limestone. The sandstones almost certainly, and the limestones probably, would yield water; * but here again, as with the Weald Clay, the strata occur only on the English side of the Channel, and we have no means of knowing how far under the bed of the Channel they may extend. We have thus successively discarded all the strata which lie between the Gault and the Kimeridge Clay; all between these formations are either inconstant or are unsuited for tunnelling.

The Gault is a very stiff and impervious clay, with very few partings or divisions, and none of those of any consequence; without much doubt, it passes regularly across the Channel, and, but for its thinness, would be better suited for tunnelling than any other formation. But on the English coast it is only about 100 ft. thick, whilst on the French coast it can scarcely be more than 50 ft. Along its outcrop on the north side of the London Basin, and occasionally when pierced in wells, the Gault is found to be of greater thickness than is here stated, and it has been suggested that probably the thickness has been wrongly estimated on the shores of the Channel.† There is, however, no good reason for supposing that such is the case. Although no well near Folkestone has been carried entirely through the Gault, its thickness is well known. Mr. F. G. H. Price has lately published a careful description of the "Gault of Folkestone;" and he, from actual measurement, makes the thickness to be 99 ft. at Copt Point.‡ No exact measurement has yet been made at Wissant, but the Gault is certainly thinner there than at Folkestone; it has been proved to be so in borings at Calais and near Guines.

Even if the Gault were much thicker than it is, there would

* Some springs were tapped in the Sub-Wealden Boring.

† See especially the remarks by Mr. Homersham in the discussion on Prof. Prestwich's paper.

‡ "Quart. Journ. Geol. Soc.," vol. xxx., p. 342.

not be room for tunnelling. The tunnel must rise from the ends towards the centre, and the Gault would not allow of this, unless the tunnel were curved, rising from either shore obliquely to the rise of the beds. But we must allow for the possible existence of small faults; and a fault which in a thick bed would be of very small consequence might be sufficient to throw the Gault out of the line altogether, and this would be a very serious matter.

The Kimeridge Clay has not hitherto attracted much attention as bearing upon this question. In some of the early tunnel-schemes it was proposed to commence the work in the Kimeridge Clay of the French coast and to carry it westwards into the Cretaceous or Wealden Beds of the English coast. In those days the succession of strata beneath the Weald was quite unknown, and it is only during the present year that the progress of the Sub-Wealden Boring has made us, to some extent, acquainted with it. The boring is still incomplete, but some important additions to our knowledge have already been made. We now know that beneath the Purbeck Beds there are the Portlands, and beneath these again there is a thick mass of Kimeridge Clay.* The succession of beds, as yet proved at the boring, is as follows:—

		Feet
Purbeck Beds.	Shale, impure limestone, and gypsum	180·0
Portland Beds.	Shale, sand and sandstone, with nodules of chert . . .	110·0
Kimeridge Clay.	Clay and shale; sometimes slightly sandy and calcareous, with bands of cement-stones in the lower part	about 670·0
Oxford Clay.	Dark clay; with hard sandy bands .	about 53·0
		1013·0

Seventeen feet more have been bored, making a total of 1,030 ft., but the lowest cores are not yet drawn.

Although the Kimeridge Clay contains some bands of cement-stone, yet there is no reason to doubt that the whole mass is practically impervious to water. The diamond system of boring brings up long unbroken cores of the strata passed through, and enables us to judge with perfect accuracy of the mineral character of the beds. The cores are always wet on the outside, from the water which fills the hole, but when the more massive pieces are broken across, they are generally found to be quite dry inside. Judging, then, from the evidence which the boring supplies, it would seem that a tunnel could be driven through the Kimeridge Clay with little or no trouble from water.

On the French coast the Kimeridge Clay is thinner, and

* The suitability of the Kimeridge Clay for the Channel tunnel has been noticed by Mr. Henry Willett, but the subject has not yet received the attention which it deserves.

there are, towards the middle and lower part, some sandy and calcareous bands which might yield water. But the upper part of the Clay is impervious; and, taking all things into consideration, there is no formation below the Chalk which offers so many advantages. Another point of some consequence is that a tunnel through the Kimeridge Clay would pass under "The Ridge"—a shoal in mid-Channel; and on this a shaft could be sunk, which would aid both in the first construction of the tunnel and in its permanent ventilation.

The only serious objection to this proposal is perhaps a fatal one: a tunnel through the Kimeridge Clay might have to be nearly fifty miles long. In fig. 5 a section is shown from Fairlight to C. Grisnez. The succession beneath Fairlight is inferred from what has been made known to us by the Sub-Wealden boring; that at C. Grisnez is sufficiently well known. The exact range under the Channel cannot be known until a boring has been made on the Ridge. It would be advisable to bore near Appledore, or at some other point nearer Hythe, in order to ascertain the thickness and character of the Kimeridge Clay in that part. If the clay should continue in sufficient force as far as Hythe or Lympne, the tunnel could be carried under the Varne, on which a shaft could be sunk; and the length of the tunnel would not greatly exceed that now proposed through the Chalk.

It may be desirable here to point out that the whole of Romney Marsh must be left out of consideration in estimating the length of tunnels from coast to coast. We must regard that area as though it were sea, and reckon the shore to be the rising ground which bounds the marsh, from Winchelsea, past Rye, Appledore, and Lympne, to Hythe. This line was once the shore, and Romney Marsh has been formed by the slow accumulation of sediment. It would be impossible to tunnel through this recent deposit, and any tunnel crossing in this direction must be carried well under the sea-level, in an impervious bed, before it reaches Romney Marsh.

We will now turn to the Lower Chalk, in which the first attempt to tunnel will almost certainly be made. No formation is better known, and more tunnels have been carried through this than through any other bed which occurs in the south-east of England. It is certainly sufficiently thick, being nearly 400 feet on the English coast, and about 300 feet thick on the French coast. The Lower Chalk, as a mass, is practically impervious, water only passing through it very slowly. The only way by which water in any quantity can occur is by means of fissures. These do exist, and yield water to wells, in the Upper Chalk, or the Chalk-with-flints; but they occur more rarely in the Lower Chalk. Moreover, as to the actual amount of water which these fissures yield; well-sinkers are quite aware that this amount is comparatively small. In works, such as breweries, &c., where large quantities of water are required, galleries are often driven through the Chalk in hopes of reaching fissures which may yield a good supply. But in no case is more water found than can readily be pumped up; and there is no reason to suppose that more water would be met with in the tunnel than could be kept under by powerful pumps. The water-bearing fissures will be encountered separately, and most of them can be separately dealt with. Sir J. Hawkshaw has stated that in the drainage-works at Brighton a tunnel $5\frac{1}{4}$ miles long has been constructed. This, being through the Upper Chalk, and only a short distance below the sea-level, has yielded a large quantity of water; 10,000 gallons per minute have been pumped, by engines of 150 horse-power; but in the Channel tunnel provision has been made for engines of 2,000 horse-power.

The ordinary water-bearing fissures of the Chalk need not therefore cause us any alarm. We need only consider the possibility of encountering large open faults, fissures, or pot-holes, communicating with the sea. Prof. Ramsay, in the course of the discussion on Prof. Prestwich's paper, called attention to the possible existence of pot-holes filled with sand and gravel, such as are well known over the surface of the Chalk. If these exist under the bed of the Channel and continue downwards to the lower beds of Chalk, they may offer serious obstacles to the work. But, on the land, we find that they generally occur over the Upper Chalk; they rarely penetrate downwards so far as the Lower Chalk; and, as the Chalk now beneath the Channel has been there for long ages, protected from the sub-aerial action which causes pot-holes, we may expect that such obstacles are not likely to occur along the proposed line of tunnel.

Again, as to faults; it is almost certain that in a tunnel 23 miles long many faults will be met with, but there is no reason to suppose that any one of them will yield an overpowering quantity of water. The coal mines of Durham, Northumberland and Cumberland, are carried well under the sea; faults are cut in the workings, which give no more trouble than those under the dry land; often, indeed, they give much less, for the quantity of water met with does not depend upon whether the works lie under sea or land, but mainly upon the water-bearing qualities of the rocks which lie above the coal. It has been suggested that probably there is a great fault or fissure in the Channel to which the Straits of Dover owe their origin; but of this there is no evidence whatever. Even if such a fault occurred it might be closed with clayey matter, and so cause no great inconvenience. In coal-mining the faults met with are numerous enough, but they do not generally very greatly increase the quantity of water; they never yield more water than the ordinary methods of pumping can control. It sometimes happens that collieries are "drowned out;" but this happens either through tapping old and abandoned workings which are full of water, or through following a coal-seam too near to the "crop," when this approaches the sea or a river. There can be no such danger with the Channel tunnel, for the works will be kept at least 200 feet below the sea-bed. Horizontal boreholes will be kept well ahead of the faces of work; points of unusual difficulty will thus be early known, and proper precautions can be taken.

Prof. Prestwich's scheme differs from all those which we have hitherto been considering. He proposes to tunnel through the floor or ridge of older rocks which underlie the Secondary strata. The Palæozoic rocks were bent and twisted, and subsequently very largely worn away by denudation, before the Secondary rocks were deposited. There is no necessary relation of character or sequence between the two series of rocks; and no amount of investigation into the range of the Secondary rocks can give us any direct evidence as to the range of the Palæozoic rocks which underlie them. This will be evident from an inspection of fig. 3. Each of the sections shows that there is a certain regularity in the succession of the Secondary strata, though all are not always present; but, in the succession from the Palæozoic to the Secondary formations, there is, in this area, no such regularity. In the Boulonnais the older rocks are immediately overlain indifferently by Gault, Lower Greensand, Wealden Beds, Oxford Clay, and Great Oolite.

While, however, we cannot by an examination of the surface strata form any idea as to the underlying Palæozoic rocks, we are not without some knowledge as to their underground range. We know that these rocks come to the surface on the north-east of Marquise, and that they strike north-west in the direction of Wissant and Folkestone. Along this line they will probably be nearer the surface than in the neighbouring districts. Prof. Prestwich supposes that under Folkestone they may perhaps be met with at a depth of from 300 to 400 feet.

The rocks most likely to be met with are the Devonian and Lower Carboniferous; they may consist of sandstones, conglomerates, schistose shales, or limestones. In any one of these the tunnel might have to be commenced on the English side, and there is no certainty or probability that the rock in which the tunnel might begin would be continued far along the required line. The work would be much more expensive through these harder rocks than through the Chalk or clays of the Secondary strata; this, although a matter of minor importance, is a point to be considered. Prof. Prestwich relies upon the Palæozoic rocks because he believes that they would be compact and comparatively free from water; the impervious clays of the Secondary strata forming a coating over the surface of the older rocks, and so protecting them from the penetration of water. This may be the case; but it is by no means certain that these rocks are always overlain by impervious clays; often the Lower Greensand and the Wealden Beds are known to rest upon them, and the same may be the case under the Channel. In most of the borings which have been made to the Palæozoic rocks, no water has been obtained. This is probably because the rocks, where thus reached, are really dry; but it may also be that water occurs in them, but does not rise in the bore-holes.

The scheme proposed by Prof. Prestwich is entirely new, and as yet there is not sufficient evidence to enable us fairly to decide as to its value. This evidence can only be obtained by experimental boring. Borings for this purpose will have a double value, for they will give important aid in deciding upon the probability of finding coal among the Palæozoic strata of the south-east of England.* One such boring is already in progress in Sussex. This, the first purely scientific boring in England, has already yielded important practical results; if continued, it cannot fail to yield more.†

Briefly reviewing the salient points of our discussion, we remark that, all things considered, the Lower Chalk offers the best chance of constructing a tunnel under the Channel. It is thick, persistent, and, in itself, is practically impervious. It is moreover easily worked, and a tunnel taken through this formation would be shorter than any other. These are points of the greatest importance, and they are sufficient reasons for selecting the Lower Chalk for the work.

Should the attempt to construct a tunnel through the Chalk prove unsuccessful, it may be well to try the Palæozoic rocks. The same shafts will serve this purpose, if the water from the Chalk (which alone will hinder the former scheme) can be tubbed back. These shafts could be continued down to the older rocks, and trial headings be driven under the sea. If the results should be favourable, it would become a question whether to continue the tunnel at that spot or to select another line more to the south-west, where, the Palæozoic rocks being nearer the surface, the approaches could be much shortened.

Lastly, should these attemps fail, there would still remain the Kimeridge Clay. The shortest and most suitable line for a tunnel through this formation can only be determined by borings on the English coast.

* Prof. Prestwich has fully discussed this question in a former number of the Popular Science Review, July 1872.

† I would here like to mention that subscriptions in aid of the Sub-Wealden boring are urgently needed. They may be sent to Mr. H. Willett, F.G.S., Hon. Sec., Arnold House, Brighton.

THE WOOLWICH BALLOON ASCENT.

By Capt. C. ORDE BROWNE.

[PLATE CXV].

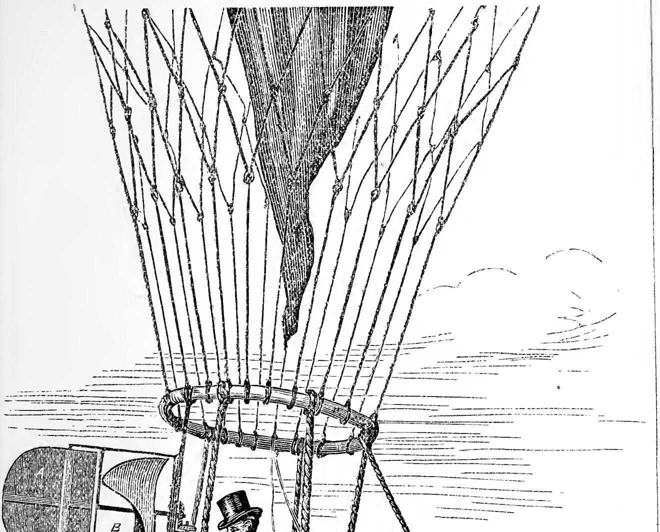
On the 25th of July last a balloon experiment took place at Woolwich Arsenal which excited considerable interest. The object of the trial was to test a steering apparatus, designed by Mr. Bowdler, attached to a balloon which is the property of Mr. Coxwell, the Government supplying the gas and defraying the cost of the ascent. The trial was conducted under the personal direction of Major Beaumont, R.E., the president of the balloon committee. (See Plate CXV.)

The experiment was one that naturally attracted attention, for the control of a balloon is at the same time one of the most difficult problems that has come before scientific men, and one that stands at the gateway of the path to any extended and systematic use of balloons.

A few words will be sufficient to explain this to those who may not have studied the subject.

The motion of a balloon, like that of a projectile or any other body in space, may be resolved into the horizontal and vertical directions; that is, its horizontal motion as it travels over the earth may be viewed as altogether distinct from its rise or descent. It is not merely for analytical purposes that these components are thus separated; they may be said to separate themselves, each one being due to the action of a perfectly distinct cause, and each differing totally in its character from the other.

The vertical, which is due to the difference in the specific gravity of the balloon taken as a whole and the air displaced by it, is clearly subject to control. The balloon may be made to rise by throwing out ballast, and to descend by expelling gas. This plan of operation, however, is attended by inconvenience, and is very limited in its scope. To understand this it may be well to trace the course of a balloon in its ascent. On leaving the earth it is necessarily only imperfectly filled, for to continue



THE WOOLWICH BALLOON EXPERIMENT.

its upward motion through the air, which is less and less dense as the height increases, the balloon must become more and more buoyant. This it does by the expansion of the gas as the pressure of the surrounding air becomes less, and thus the balloon becomes more completely inflated. Beyond a point, however, the safety of tbe balloon becomes jeopardised by excessive stretching, and so gas has to be allowed to escape, and any further ascent must be obtained by throwing out weight in some shape, in other words ballast. A certain quantity of this, however, must be reserved for the descent, which may be otherwise too rapid and violent. Hence it follows that the duration of a balloon voyage is greatly limited, and repeated variations in the height at which it soars are only obtained by taking up a quantity of extra weight and expending an extra quantity of gas.

The horizontal motion of translation, on the other hand, may be said as yet to have defeated all efforts to control it. The balloon, from the instant it leaves the ground, becomes thoroughly committed to the winds; so completely does it become incorporated in the surrounding air, that no matter how violent and gusty a wind may be blowing, the occupants of a balloon, on quitting the earth, experience the sensation of being in a dead calm, and can only detect whether there is any wind by their motion over the earth. Wholly immersed in a surrounding medium, the balloon is totally deprived of any fulcrum such as a ship finds in the water, by which the direction of the force of the wind may be diverted, and its only means of escape from the power of any current of air is by rising above it or descending below it.

To bring these facts to practice, it becomes necessary, in order to have any choice as to the probable course of a balloon, to select a day with the wind in the desired direction, and in this there may be great disappointment and deception. During the siege of Paris balloon voyages assumed an impôrtance altogether without precedent. Science was called into play with its full powers, and nothing was spared to secure success. A map has been brought out by M. Jules Godard, and officially endorsed, showing the result of 66 balloon voyages attempted from Paris. By this it appears that 51 only descended in France, 5 falling in Belgium, 4 in Holland, 2 in Prussia, and 1 in Norway, while 3 were altogether lost. Out of the above, 16 fell in the enemy's lines, of which 5 were captured. These statistics, while they indicate what important work might be done by reliable balloons, give more undoubted evidence as to their uncertain and unmanageable nature than could be shown by any theoretical discussion. The recent Calais ascent is only another instance of the same difficulty being experienced.

In attempting to control and guide a balloon, then, Mr.

Bowdler undertook a most formidable task. The apparatus by which he proposed to accomplish it is shown in Plate CXV. The machine consisted of two fans or propellors, one to give motion in the horizontal, and the other in the vertical direction, either being worked by a hand-winch, with multiplying gear. The entire apparatus weighs only about 70lbs., the fans being made of very thin sheet metal. The highest speed with which the propellers may be made to revolve is from 600 to 720 revolutions per minute. The one on the vertical axis (C, Plate CXV.) is intended to cause the balloon to ascend or descend without loss of gas or ballast. The fan B, on the horizontal axis, is meant to give motion in the horizontal direction; as, however, this may be in any accidental position, a rudder, A, made of canvas with strengthening bands, is fixed opposite to it, being held in the desired position for guiding by means of the rudder-lines. Mr. Bowdler considered that Mr. Coxwell's balloon, which contained about 60,000 cubic feet of gas, was too large to be worked properly by the gear he had made; he hoped, however, that a sufficient result would be obtained to show its efficiency.

The method of attaching the apparatus to the car by lashings is sufficiently shown in the sketch, the only part which is not seen being the hand-winch and some of the pinions, and about these there was nothing specially characteristic.

An official programme, to be carried out at the trial, had been drawn up to the following effect:—(1st) The balloon, when held captive by a rope, was to be carefully balanced, and, if possible, raised and lowered repeatedly to the extent of about 150 feet, by means of the vertical propeller, in order to test its efficiency. It was also to be moved horizontally to and fro. (2nd) The balloon was to be released, and as soon as the course was shown to be steady, and the direction ascertained by means of Mr. Coxwell's indicator, by maps, &c., the horizontal propeller was to be worked at right angles to the course of the balloon, and by this means the maximum effect when unopposed by any wind was to be noted. (3rd) The balloon was to be again raised and lowered by the vertical propeller, without throwing out ballast, or allowing gas to escape.

Major Beaumont, Mr. Coxwell, Mr. Bowdler, and a sergeant of the Royal Engineers, entered the car, which was carefully balanced, and the first part of the programme was commenced, the balloon being held captive. Owing to a deficiency of suitable rope, the raising was only carried out to the height of about 40 feet instead of 150. The difficulty of ascertaining exactly when a captive balloon is balanced, when even a slight wind is blowing so as to stretch the retaining rope, made the first trial a little doubtful; and after one ascent, apparently due to the working of the vertical propeller, a doubt arose as to the exact balance of the balloon, which might have a tendency to rise and only have been held down by the captive line, which, except at very still moments, was pulled taut by the wind acting on the balloon. It being ascertained at a still interval that the balance was good, the vertical gear was worked and the balloon again rose. The rate of ascent was difficult to estimate; it was judged, however, not to exceed 50 feet a minute. A positive indication of the power of the propeller was thus obtained, and it should be noticed that the circumstances, if the rate of *ascent* only was measured, were rather disadvantageous, for the weight of the line, up to the extent of 40 feet, was gradually added to the balloon as it rose. Had the mean rate of ascent and descent been taken, this error would be eliminated, for the descent would be favoured by the weight of the rope from 40 feet in length at the maximum height, down to nothing at the ground. The balloon was now liberated; not, however, until Mr. Bowdler's vertical gear had become broken and unable to work. The wind's direction in the meantime had been ascertained to be suitable by sending off a series of small pilot balloons, and the ascent took place. The horizontal gear, however, throughout the entire voyage, failed to give any satisfactory results; even allowing that the effect was perceptible, it is impossible to lay much stress on it. Any force would give a perceptible effect if recorded with sufficient delicacy. There is no use in an insignificant effect unless it can be shown that means exist by which it could be increased sufficiently to bear a reasonable relation to the forces to which it is to be opposed, or with which it is expected to be compounded.

It is hopeless to look for this from fans or propellers turned by hand, for a balloon must very frequently expect to encounter currents of air that carry it along at least at ten miles an hour. Any motion produced by the force of a man's arm, even at right angles to such a velocity, could hardly produce a sensible deviation from the direction in which the balloon is borne, and so could hardly show any sensible result in steering it. If, however, the more ambitious problem be attempted of propelling a balloon in any desired direction, a power of an entirely different magnitude is required. The bulk of the balloon is enormous in proportion to the weight it carries; hence it is necessary to provide a source of vast power, which shall be almost without weight, and hence the idea of any kind of engine, in the ordinary acceptation of the word, may be at once dismissed. The question arises, Have we a conception of the possibility of any great power existing in so light a form? This, at all events in a degree, may be answered in the affirmative. There exist compositions, somewhat of the character of that used in rockets, that have stored up in them the elements of great power. In the case of a rocket, the process of producing gas by chemical combination is actually employed to propel the rocket, though in a violent and wasteful form. What quantity of force might be stored up in a mass of composition which, with some means of direction, was not beyond the power of a balloon to support, has probably not been investigated; it is hardly likely to approach the amount necessary to produce satisfactory results, but it must be enormously beyond the force of a human arm. Then some form of hydrogen gas might be used to give power by combining with the oxydising agents, and so one part of the fuel might support the other; but the bulk becomes increased. Supposing, however, for a moment, that a great horizontal velocity could be imparted to a balloon, in its present form, held in the vertical direction only by the weight of the car, such an application of force to the latter would tilt it very much over to one side into a position depending on the relative magnitude of the forces in action; so that, on this account, special provisions might have to be made. It is unnecessary, however, to proceed further in such speculations. It is sufficient to observe that, for efficient steerage, it is generally considered that two elements are necessary—the application of great power in a very light form, and a balloon with a rigid frame; to which may be added that, for voyages of extended periods, an envelope much more impervious to the passage of gas than the silk hitherto used is essential.

P.S.—The Plate (CXV.) is produced from a block kindly lent by the editor of "The Engineer" newspaper.

REVIEWS.

CORAL REEFS.*

ALTHOUGH Mr. Dana, a most distinguished American observer and student of coral reefs, opposes some of Mr. Darwin's conclusions, that fact is rather to be rejoiced at than otherwise, since it has doubtless led to the publication of a new edition of a book which first made its appearance thirty-two years since; and we may congratulate the author when we find that he has not in any material way altered his opinions, but has gone into the subject at greater length than ever, and has found an ample supply of facts to bear out his original views. It is to be regretted that we have not space to go fully into his various arguments, but at all events the following extract from the preface will tolerably explain the differences which exist between Professor Dana and Mr. Darwin. "In this work," says Mr. Darwin, referring to Dana's book published in 1871, "he justly says that I have not laid sufficent weight on the mean temperature of the sea in determining the distribution of coral reefs; but neither a low temperature, nor the presence of mud-banks, accounts, as it appears to me, for the absence of coral reefs throughout certain areas; and we must look to some more recondite cause. Professor Dana also insists that volcanic action prevents the growth of coral reefs much more effectually than I had supposed; but how the heat or poisonous exhalations from a volcano can affect the whole circumferance of a large island is not clear. Nor does this fact, if fully established, falsify my generalization that volcanoes in a state of action are not found within the areas of subsidence, whilst they are often present within those of elevation; for I have not been influenced in my judgment by the absence or presence of coral reefs round active volcanoes; I have judged only by finding upraised massive remains within the areas of elevation, and by the vicinity of atolls and barrier-reefs. With reference to the areas of subsidence, Professor Dana apparently supposes that I look at fringing-reefs as a proof of the elevation of the land; but I have expressly stated that such reefs, as a general rule, indicate that the land has either long remained at the same level, or has been recently elevated. Nevertheless, from upraised recent remains having been found in a large number of cases on coasts which are fringed by coral reefs, it appears that of these two alternatives recent elevation has been much more frequent than a stationary

* "The Structure and Distribution of Coral Reefs." By Charles Darwin, M.A., F.R.S., F.G.S. Second edition, revised. London: Smith & Elder, 1874.

condition. Professor Dana further believes that many of the lagoon islands on the Paumotu or Low Archipelago and elsewhere have recently been elevated to a height of a few feet, although originally formed during a period of subsidence; but I shall endeavour to show that lagoon islands, which have long remained at a stationary level, often present a false appearance of having been slightly elevated."

It will readily be seen, from the foregoing quotation, what it is that Mr. Darwin attempts to prove in this volume, and also why the present edition has been given to the world. We shall refer further on to some of his arguments, but in the mean time we may explain in a few words the nature of his book. In the first place we may state that it is to Mr. Darwin that we owe whatever knowledge we possess on the subject of coral reefs. It was he who first explained, more than thirty years ago, the peculiar reason why those singular islands called Atolls had their particular shape. And we are glad to state that the reasons he then gave forth to the world, to explain their origin, are the same he now urges with even stronger arguments than he first advanced in their favour. What, then, is the meaning of an atoll, and how has it arisen from the deep? An atoll is one of those peculiar islands which the navigator sees abundantly in the Pacific Ocean. Taking one that is completely formed, what is its appearance? It is that of a ring of solid rock about a mile or so wide, covered with cocoa-nut trees, and forming a more or less regular circle which is broken at one point. Without, the sea is beating against this belt of rocks with all its fury; within, is a basin ten or twelve or more miles wide, and some thirty or more miles long, of perfectly still water. Finally there is usually an entrance by which boats may enter into this so-called lagoon. This is the constitution of a perfectly formed atoll. Others are not so completly walled in, and in some instances there is an island in the centre. Now leaving for a while the consideration of fringing-reefs and barrier-reefs, we may ask how came about the formation of the Atoll? Well, it is a question whose answer depends on our knowledge of coral reefs, their rapidity and mode and other conditions of growth. In the first place these corals usually commence growing along a shore, for it is found that they cannot live if they are placed at a considerable depth, or if they are exposed by tidal fall to the air; again, that they grow with immense rapidity; and finally, that they will not grow in any position in which fresh water flows. Now, admitting these conditions, and admitting also the fact which no geologist will deny, viz., that in the Pacific Ocean, as elsewhere, islands are constantly sinking and being elevated, we have all the conditions necessary to explain that singular formation, an atoll, or circular coral belt. Suppose, for example, an island has been thrown up in the ocean—no extraordinary thing in a volcanic district—and measures about thirty miles long by ten miles wide. Soon there appears to be coral reefs in its neighbourhood, and these eventually appear all round the island at a certain distance from shore. Here they may go on living and dying for a thousand years or more. Eventually this island begins to descend into the ocean from which it came. But as it descends the corals must grow up, because they cannot live at a great depth. And so on, for thousands of years, this island keeps going down, and, *pari passu*, the coral wall keeps growing upward, till at last the land has entirely disappeared, and we have a circular line of coral reef, representing what was originally the coast line, and enclosing within it a quantity of water, partly brackish, and having an opening at one part of the reef by which boats can pass into the circle from the outside. Eventually cocoa-nuts and seeds of different kinds get among the coral reefs and grow up, and eventually a perfect wall of coral is found enclosing a vast pond of calm water, many miles in length and breadth. Now there remains one question to be asked, and that is, How came there to be any channel as an entrance from the surrounding sea? This seems difficult to understand, but Mr. Darwin furnishes us at once with a lucid and satisfactory answer. It is this. While the island which formerly existed was in progress of descent, of course during many years, it attracted rain, and thus there was fresh water, which flowed down and most probably formed a river at some point. Well, as this river flowed out to the sea, it utterly prevented the growth of coral at the point of exit, and so it left at last a point of entrance to the atoll.

The above is but a very imperfect sketch of the several operations employed, still it may help the reader to understand the general principle of the process; and we trust he will be led by it to get Mr. Darwin's book, and study the operations more fully for himself. We have given only the description of an atoll-growth, because we doubt not that those who understand it will readily be led to perceive that there is no material difference between the growth of an atoll and that of a barrier- and a fringing-reef. We may therefore pass on to the consideration of some of the points at issue between Mr. Dana and Mr. Darwin. And firstly of the question as to the supposed recent elevation of some of the lagoon islands, Keeling atoll, for example. Of this one Mr. Darwin says:—"Mr. Liesk informed me that he had seen an old chart in which the present long island on the north-east side was divided by several channels into as many islets; and he assures me that the channels can still be distinguished by the smaller size of the trees on them. On several islets also I observed that only young cocoa-nut trees were growing on the extremities, and that older and taller trees rose in succession behind them, which shows that these islets have very lately increased in length. In the upper and south-eastern part of the lagoon I was much surprised at finding an irregular field, of at least a mile square, of branching corals still upright, but entirely dead. . . . They were of a brown colour, and so rotten that in trying to stand on them I sank half way up the leg, as if through decayed brushwood. The tops of the branches were barely covered by water at the time of lowest tide. Several facts having led me to disbelieve in any elevation of the whole atoll, I was at first unable to imagine what cause could have killed so large a field of coral. Upon reflection, however, it appeared to me that the closing up of the above-mentioned channels would be a sufficient cause; for before this a strong breeze, by forcing water into them from the head of the lagoon, would tend to raise its level. But now this cannot happen, and the inhabitants observe that the tide rises to a less height during a high south-east wind at the head than at the mouth of the lagoon."

Thus, in this instance, Mr. Dana is answered; but we must confess that Mr. Darwin does not appear so satisfied with the view that he completely accepts it. We may therefore imagine that this point of the controversy is still open. In regard to Professor Dana's objection that Mr. Darwin has not laid sufficient weight on temperature in the distribution of coral, it seems to us that he not only has done so, but that he has gone into many other questions on this point. Still we cannot say that he has completely made out the reason why it is that coral reefs abound in certain localities, and are as completely absent from other places which have conditions exactly identical. Indeed, it appears to us that the question is still one of those many problems which are at the present moment utterly insoluble.

Another point of difference between the two authors is as to the effect of volcanoes upon coral reefs. Mr. Dana considers that these eruptive mountains have an impeding influence on coral growth, and so undoubtedly they have when excited. But Mr. Darwin's question as to how the volcano can influence a large district clearly shows that the objection has no real value. The other question which Mr. Dana raised was, we may mention, fairly met by Mr. Darwin, when treating of the entire subject, in that spirit of thorough fair play which we have so frequently admired when studying his different books. There are many other points in connection with this subject which we should wish to touch on, but inexorable space prohibits. However, there are one or two we must refer to, and these are the rate at which coral grows, and the depths at which it can and can not live. The first question is one which is frequently answered, but in no instance is it more fully replied to than in the quotation from Dr. Allan's MS. thesis:—" To ascertain the rise and progress of the coral family, and fix the number of species met with at Foul Point (lat. 17° 40′), twenty species of coral were taken off the reef, and planted apart on a sand-bank, *three feet deep at low water*. Each portion weighed ten pounds, and was kept in its place by stakes. Similar quantities were placed in a clump and secured as the rest. This was done in December 1830. In July following each detached mass was nearly level with the sea at low water, quite immovable, and several feet long, stretching like the parent reef, on the line of the coast current from north to south." This shows clearly enough the tremendous rate of growth which corals have. And although, from the loss by shipwreck of Dr. Allan's splendid collection, the exact species has not been determined, yet it was thought by him to have been a Madrepore which grew at the most rapid rate.

The other point in which we must quote from our author is as to the depth at which corals will maintain themselves. It seems that they " wholly disappeared at a greater depth than 20 fathoms on the slope round Keeling Atholl, off the reefs in the Pacific (according to Dana), on the leeward side of the Mauritius, and at rather less depth both within and without the atolls of the Maldiva and Chagos Archipelagoes; and when we know that the reefs round these islands do not differ from other coral formations in their form and structure, we may, I think, conclude that in ordinary cases reef-building polypifers do not flourish at greater depths than between 20 or 30 fathoms, and rarely at above 15 fathoms." From this it seems clear enough, but there are many other facts that point to the same conclusion. We hope we have said and abstracted enough of this clever book to induce the reader to take it up himself. If so, we have done sufficient. Still, if it were allowable, we could go on much further, and we doubt not the matter which we should abstract would prove equally interesting and not less true to nature.

PORTRAITS OF THE MEDICAL PROFESSION.*

THE attempt to produce a series of photographs, accompanied by biographical sketches of the men who are eminent in the medical profession, has been attempted at various times of late years, but unfortunately with very little success; so little indeed, that the work, no matter how well it has been done, has seldom outlived the first year of its existence. In the present issue we have the first volume of a work on this plan, viz., portraits of distinguished men, with short shetches of their lives, and we think it clearly merits the success which its authors, if we may so call the photographers who have executed the portraits, hope it to attain. It contains twenty-four pictures of the cabinet size, of different medical men, some of whom have reached the highest steps of the professional ladder; some who have since its publication deceased, and a few who have hardly merited so much distinction as is awarded to them. Following each portrait is a page sketch of the life of the sitter, and in this we think the artists have done well in confining themselves simply to facts. Thus we are told when the person portrayed was born, when he entered the profession, where he studied, where he graduated or obtained his license, to what hospitals or institutions he belongs, and what he has written, and in some instances to whom he has been married, and what children he possesses. It will thus be seen that it is a fuller biography than that in the "Medical Directory"—in which, by the way, a very excellent though brief account is contained—whilst it possesses the character of that book in strictly confining itself to facts. On the whole, we may express our approval of all the photographs in the present volume; but there are some which merit especial approval, as being eminently life-like portraits. Of these we may mention those of Sir T. Watson, Dr. L. Beale, Sir James Paget, Sir H. Thompson, Sir H. Holland, and Mr. Spencer Smith, the last being an admirable portrait. Some, again, are not so life-like; as, for example, Sir W. Fergusson. Still they all form a very excellent volume, which we trust will meet commercially with so much success that we may soon look forward to this year's volume coming out. We should advise the publishers to issue an index in the next volume, and to have the biographies paged, as thereby much confusion will be avoided in searching for any wished-for biography or portrait.

---

* "The Medical Profession in all Countries;" containing Photographic Portraits from Life. By Barraud & Jerrard. London: Barraud & Jerrard, Gloucester Place, W.

THE CORRELATION OF PHYSICAL FORCES.*

ALTHOUGH this book is in its sixth edition, it is one nevertheless which we cannot dismiss with a few short sentences. It is unquestionably one of the most important works of the age; and although there may be a few reasons for objecting to the style in which the author has expressed some of his views, we should at the same time exhibit our gratitude to the man who expressed those views when there was danger in holding them. It ought to be borne in mind that he who had the boldness to enunciate those opinions more than thirty years ago is now one of the Judges of the Court of Common Pleas, and that he has not had the time to keep himself *au courant* with the progress of modern science. We think, therefore, that allowance should be made—especially by those whose excessive learning compels them to look down upon anyone who has not mastered the few technicalities of the most modern leader—for the opinions of a man who simply claims a hearing for his hypothesis. Assuredly Sir. W. Grove can afford to despise some modern scientists who, having been taught in the principles which he was the first unquestionably to lay down, pretend to have a certain amount of contempt for his work, because indeed it does not distinctly recognise a system of nomenclature which is essentially modern, and may even be itself expelled from science in due course. Indeed, we might almost say that Sir W. Grove's opinions have now become recognised throughout the scientific world; and we would express the belief that in no branch has the idea of correlation and continuity been so thoroughly accepted as in that of modern physiology—a fact which is perhaps due to the able advocacy of Judge Grove's ideas by Dr. Carpenter. There is not the smallest doubt that the idea which runs through the book now before us is strictly and rigidly true. It may no doubt be difficult to prove it in many cases, though these instances become fewer year by year; but we have not the slightest doubt on the question whether any form of force is not a condition that may be, under other circumstances, converted into any other form. And this is, we imagine, what Judge Grove would have us believe. For instance, we look on mechanical force as convertible into heat; this as productive of chemical action; and this, in its turn, as convertible into galvanic action, which then may be converted into the more ordinary force. We regard all the forces as being convertible into each other. Of course there is an apparent loss, but then this is only apparent; and, as our engineering forces are being improved, we see this conversion of, let us say, heat into mechanical force being every day made with less loss of the original. But it was not so easy a matter to detect this conversion of one so-called force into another when *Mr.* Grove—as he then was—first took up this matter for consultation. Every man of science in England must be proud to have to own the discoverer of the correlation of physical forces as a fellow-countryman; for unquestionably, great as may be his legal merits, the author

* "The Correlation of Physical Forces." Sixth Edition, with other Contributions to Science, by the Hon. Sir W. R. Grove, M.A., F.R.S., one of the Judges of the Court of Common Pleas. London: Longmans, 1874.

of the theory of the conservation of energy—for such his hypothesis really is—will be known to many a generation in the future, when the mere lawyer is completely forgotten. We think, therefore, that the writer of the book before us may very well be pardoned for the following sentence:—"Everyone is but a poor judge where he is himself interested, and I therefore write with diffidence; but it would be affecting an indifference which I do not feel if I did not state that I do believe myself to have been the first who introduced this subject as a generalized system of philosophy, and continued to enforce it in my lectures and writings for many years, during which it met with the opposition usual and proper to new ideas."

Of this book it would be difficult to speak too highly, for it is a masterpiece of reasoning extending over the whole range of physical science, and tending in every way to prove the master's idea, viz., that force is indestructible. It would be out of place, in a short notice such as this, to introduce lengthy quotations, and yet without them we could not attempt to show the extremely happy manner in which the author explains what were at one time regarded as the complexities of his case. Of the style of the book, too, we cannot say too much. It is indeed a masterly essay. Whether the author has done well to add his several papers bearing on the subject and printed elsewhere we shall not say, but it appears to us that the book would have had more interest to the general student had they been absent. However, many of them are intensely interesting, and as a whole the volume is one that, like Mr. Darwin's "Origin of Species," should hold a place in every scientific man's library.

GEOLOGICAL SURVEY OF MISSOURI.*

THIS report, accompanied by nearly two hundred illustrations, and a large folio atlas of sections and typographical maps, is divided into two parts, the first of which is chiefly devoted to the distribution, description, and mode of occurrence of the iron ores, by Dr. Adolf Schmidt, and the second part to the area and topographical features of the coal-fields by Mr. G. C. Broadhead, who also furnishes an account of the geology of seven counties, together with a geological report of the county adjacent to the Pacific railroad from Sedalia to Kansas city; and in the same part the general and economic geology of Lincoln county is described by Mr. W. B. Potter. The first chapter, somewhat bearing on the subject of the iron ores, is by Mr. Ralph Pumpelly (the director of the survey), treats of the geology of Pilot Knob and its vicinity, from which it appears that porphyry forms the entire substructure of the region, which, however, had been eroded into hills and valleys before the deposition of the overlying lower Silurian limestone. The porphyries are stratified, belong to the Archæan (Azoic) formation, and are considered to be the near equivalents of the great iron-bearing rocks of Lake Superior, New Jersey, and Sweden. These rocks have undergone immense

* "Geological Survey of Missouri. Preliminary Report on the Iron Ores and Coal-fields from the Field-work of 1872." New York: 1873.

atmospheric denudation, and the gradual removal of the soluble constituents has left important residuary deposits of such substances as were insoluble, especially in the Silurian strata—as clay, flint, &c. The more conspicuous instances of this kind among the pre-Silurian rocks are residuary occurrences of iron ore; the instance of Iron Mountain is an extreme case, where the decomposition of the porphyry in mass facilitated the separation of the ore from the rock and the mechanical removal of the latter.

Although Missouri is one of the richest States of North America in iron ores, they are very unequally distributed. By far the richest portion of the State is in the greater part of the counties situated between the Mississippi in the east and the Upper Osage river in the west, so that according to present knowledge there are three principal and important iron regions in Missouri. (1) The eastern region, composed of the south-eastern limonite district and the Iron Mountain specular ore district. (2) The central region, containing principally specular ores, having its outlet over the St. Louis and the Atlantic and Pacific railroads. (3) The western, or Osage region, with its limonites and red hematites; the upper Osage district also contains good deposits of subcarboniferous red hematites, occurring in the same way as in Callaway county. The mode of occurrence of the iron ores is variable; they are found either as deposits of specular ore in porphyry and sandstone, as disturbed and drifted deposits of the same ore, as strata of, or as disturbed and drifted deposits of, red hematite, or as deposits of limonite on limestone, or as drifted deposits of the same ore.

The coal-measures of Missouri comprise an area of nearly 23,000 square miles, of which the largest portion occurs in the north-west and west districts, with about 160 square miles in St. Louis, and a few outliers in Lincoln and Warren counties. They are divided into the upper, or barren measures, the middle and lower measures, the larger exposed portion, more than one-half, belonging to the last division, and which are the richest in coal. The thickness of the Missouri coal-measures is about 2,000 feet, with a total aggregate of 24 ft. 6 in. of coal; the lower measures, about 300 feet thick, contain five workable seams, in all 13 ft. 6 in. of coal; the middle, about 300 feet thick, with seven feet of coal, including two workable seams of about two feet each, and the upper measures, with a thickness of 1,300 feet, contain only four feet of coal made up of thin seams. The coal seams are intercalated with a rich carboniferous marine fauna, of which a list is given at the end of the volumes.

GEOLOGICAL SURVEY OF THE STATE OF MISSOURI.*

THESE reports scarcely represent the present knowledge of the geology of this State, as they appear to have been prepared some time since, and remained unpublished. The maps were engraved, and the impressions contained

* "Reports of the Geological Survey of the State of Missouri, 1855–71." By G. C. Broadhead, F. B. Meek, and B. F. Shumard. Jefferson City: 1873.

in the volume were struck off previous to 1866, so that in consequence they do not even represent the present political geography of the respective counties. Each of the twenty counties of the State reported upon are treated separately and in the same systematic manner, comprising their chief topographical features, scientific geology and economical geology; under the latter head is included a variety of useful information bearing on agriculture, as to the nature of soils, the corn-bearing lands, springs, timber, mill sites, the occurrence of building and road materials, and the distribution of the metallic ores and other minerals. Saline is mentioned as being one of the finest agricultural counties of the State, in having throughout almost its entire area a deep, rich, black soil of unsurpassed fertility; the staple production of the county is hemp, which is extensively grown; but all the other crops usually cultivated there are also produced in great abundance. A remarkable peculiarity of this county, and which has suggested its name, is the very large number of springs strongly impregnated with common salt; and their value when more generally appreciated must become an important source of wealth, for in no other portion of the State of the same extent, nor, indeed, of the West, does there appear to be so large a number of brine-springs. Many of them are more or less strongly impregnated with sulphur, and a variety of other minerals. The chief geological formations recognised in the State of Missouri are the Lower Silurian, with its numerous sub-divisions; the Upper Silurian, Devonian, Lower Carboniferous, Coal-measures, and the Quaternary deposits; the latter include all the deposits beneath the soils down to the regular stratified rocks, and are almost universally spread over all the counties; according to their character and origin they have received the names of Drift, Bluff formation, Bottom prairie, and Alluvium. The coal-measures are not so extensively developed; they have been observed in Warren, Clark, Shelby counties, form one-third of Saline, occupy nearly the whole of Randolph, and are the only Palæozoic rocks seen in Macon county.

GEOLOGICAL SURVEY OF THE TERRITORIES.*

THE above reports are entirely out of print, and the popular demand for a complete series of the annual reports of the survey of the Territories by Dr. Hayden has been so great, that the Secretary of the Interior has ordered the printing of a second edition of them in one volnme, but no alterations or additions have been made to them. The first report comprises detailed descriptions of the physical features and geological structure of Nebraska, in which the fauna and associated flora of the Cretaceous and Tertiary rocks are noticed and commented upon, from which it appears that in America the relation between the Tertiary and Cretaceous flora is yet more intimate than in Europe, for the plants of Nebraska present

* "First, Second, and Third Annual Reports of the United States' Geological Survey of the Territories for the years 1867-68-69." Reprinted. Washington: 1873.

relations with the existing flora of America, while the Cretaceous flora of Europe has more of an Indo-Australian character.

The second report contains an account of the geological explorations in Wyoming Territory, preceded by a very interesting outline of the physical geography of the Missouri Valley and the sub-hydrographical basins connected with it.

The third report comprises an account of the general geological, mineralogical, and agricultural characters of Colorado and New Mexico, made in a series of traverses across the districts in 1869, followed by two special reports on the mines and minerals and agriculture of Colorado.

GEOLOGICAL SURVEY OF INDIANA.*

THIS volume contains the result of the survey made during the year 1869 by the State geologist, Prof. E. T. Cox, assisted by Prof. Bradley and Drs. Haymond and Levette, but which has since been followed by the reports of the survey for the years 1870–72. It contains descriptions of Clay, Greene, Vermillion, and other counties, as well as investigations in Warren county, with a view of determining the character of the coal-beds on the northern limits of the Indiana coal-field. The area of the coal-measures is computed to approximate 6,500 square miles, or more than half as large as the entire coal area of Great Britain and Ireland. The Indiana measures form a portion of what is sometimes called the "Great Illinois Coal-field," which includes not only the coal area of Illinois, but that of Western Kentucky, Arkansas, Missouri, Iowa, Kansas, and Nebraska, from the facts that they were all considered as parts of one great basin. Taking a connected section of the coal-measures in Clay and Greene counties, the aggregate thickness is about twenty-eight feet of coal, including three seams of the celebrated "Block Coal," a name used by the miners to designate a variety of non-caking bituminons coal, for in coking it scarcely swells, and never cakes or runs together. It was first discovered on the western border of the Appalachian coal-field, where it is extensively used in blast-furnaces. It closely resembles in many respects the Scotch *splint* coal. It is free burning, and is remarkably free from sulphur; the main seam is about four feet in thickness, and is traversed by two sets of vertical fissures, crossed at right angles, so as to allow the coal to be mined in large cubes or blocks—hence the probable origin of the name. With regard to the thickness of a coal seam as a means of identity over any great extent of country, Prof. Cox remarks, that it is only around the rim or margin of the western coal basin, and not throughout its *central area*, that we are to look for a succession of thick beds of coal. As we approach toward the central part of the basin, the coal-beds which surround it are either entirely absent or have dwindled down to seams that are only a few inches in thickness, their places being occupied by a preponderance of argillaceous shale, some sandstone, and an occasional stratum

* "First Annual Report of the Geological Survey of Indiana." By E. T. Cox. Indianapolis: 1869.

of limestone, showing probably that their deposition took place in deep and quiet water, where the conditions, unlike the shallower waters of the margin, would not be favourable for a luxuriant growth of plants so essential for the formation of thick beds of coal.

THE PRINCIPLES OF SCIENCE.*

THAT a work of the present kind is wanted no one will deny; for, with the exception of Whewell's "Philosophy of the Inductive Sciences," there has hardly been published any book dealing with the principles or methods on which scientific men ought to, if they do not, work. However, we cannot see that on the whole the matter is in a much better condition owing to the labour of Mr. S. Jevons. We confess that he has done good in the onslaught he has made on the systems and roundabout measures adopted by his predecessors. But, it must be asked, could he not have put the whole rationale of the scientific method—when truly and properly adopted—forward in a vastly briefer form? We do not see why a system of logic, such as should be followed, need run to such extreme lengths. Nor, we confess, can we see the great efficacy of many of the chapters which the author has given us. For ourselves we should have infinitely more preferred the book had it been less in the old style of such works, and more in the modern type. Still, when we have found fault to the utmost extent, it must be admitted that Professor Jevons has done a good deal to render logic more acceptable to the general man of science than it has been hitherto; and that what he has done exceedingly well in, is the manner in which he has used such a number of illustrations, extremely happy in their tone, and taken from so many different branches of science. The method which the author offers to the student is of course much simpler than the older ones, and in this respect is to be much admired. And in this regard we may refer to his remarks on one subject as being extremely valuable. It is an abbreviation of the indirect method of inference; in this the logical abecedarium is given in a very convenient manner by means of the "logical slate," which is described at p. 111, vol. ii.; problems of less than seven times may very readily be worked out. The other parts of these two volumes are full of interesting material, but they will require every attention from the young student, who cannot read this book as he could a mere historical work. With the author's observations on the subject of Providence we do not at all agree. Indeed, the remarks were not demanded at all unless Mr. Jevons thought that his remarkably fair treatment and just appreciation of the law of evolution demanded a kind of counterscarp, in order to please those who were likely to affect an opposite side to his remarks on the subject of Darwin and Spencer.

---

* "The Principles of Science;" a Treatise on Logic and Scientific Method. By W. Stanley Jevons, M.A., F.R.S., Professor of Logic in Owens College, Manchester. 2 vols. London: Macmillan & Co., 1874.

A MANUAL OF BOTANY.*

THE author of the work which is now before us bears the name of the most able botanist that this country has yet produced—Robert Brown. Is he worthy of it? We can only judge by his efforts. In this volume he puts before the student a fair *résumé* of what has been done in the botanic field within the past ten years. We have seen many books on this subject produced in England, but if we except the one splendid handbook, the work of the late Professor Henfrey, there is not another creditable volume to be named. We have, however, in the work of Dr. Robert Brown, another instance of a book which looks more like an English translation of a first-class German essay than the production of an English worker. However, the book is unquestionably by an Englishman (or Scotchman), and it does him, we must say, infinite credit; for he has not only gone into the common subjects treated of in text-books, but he has departed from the ordinary or routine ones, and has dealt fully—and given references to his authorities—with such important questions as nutrition, fertilization, germination, longevity, mimicry, movement, opening and closing of flowers, vegetable irritability, and the odours, colours, luminiosity, and temperature of plants. All these subjects he has dealt with fully yet concisely, and with reference in many cases to papers and researches which were only published as recently as last year, while he has dealt more fully than usual with the ordinary and less interesting work. The book will be an excellent one for the advanced student, its only objection being the type, which we consider too small. Its illustrations are very well executed woodcuts, nearly 370 in number, and many of them quite new to the botanical student. We certainly commend the work.

METALLURGY.†

IT is certainly a misappropriation of words to call this book the Elements of Metallurgy, for it is a vast work extending over nearly 800 pages of very large 8vo., and dealing fully with the subject. Further, it is partly illustrated, having more than 200 woodcuts interspersed through the text, though the figures are not quite so numerous as we could have wished. Perhaps the works which heretofore we should have considered the best on this important subject are those of Professor Percy and Messrs. Crookes and Röhrig; but unquestionably that of Mr. J. A. Phillips is at once better, and in some respects fuller, and in all more recent than any other work that has been published. The subject of iron manufacture is of course that on which the author has dealt at fullest length, and this is, we may say, ad-

---

* "A Manual of Botany, Anatomical and Physiological," for the Use of Students. By Robert Brown, M.A., Ph.D., F.L.S., &c. Blackwood & Sons, Edinburgh: 1874.

† "Elements of Metallurgy;" a Practical Treatise on the Art of Extracting Metals from their Ores. By J. A. Phillips, Memb. Inst. C.E., F.G.S., F.C.S., &c. London: Charles Griffin & Co., 1874.

mirably done; the many improvements, Bessemer's and many others, being discussed at length. The book is undoubtedly a good one.

SCIENCE PRIMERS; PHYSIOLOGY.*

THIS series is a very good one. We have already noticed one or two of those primers which have been already issued; but that which Dr. Foster has given us on physiology is unquestionably the best. And why is this? Simply because the author has taken the pains to consider carefully what was the best way to address his readers, most of whom are unscientific. And he has selected the very best manner; and he has been, further, wise in not attempting to go over too much ground. We have never seen a complex subject so admirably illustrated and so simply explained as it is done in this volume by Dr. Foster.

TECHNICAL TRAINING.†

HERE is a large book with an amount of matter which might have been put into a volume one-twelfth the size. We do not doubt that Mr. Twining is perfectly sincere in his efforts to improve the public mind, still we are fully convinced that he is not the man to do it. His classes and his lectures all seem very pretty and well modelled, but his methods, we are fully convinced, are utterly unworkable. The man who would write a book which should be taken as a guide by some energetic member of parliament is not Mr. Twining. Therefore, however well we may look upon his efforts in the philanthropic direction, we must not be guided by his views.

PAMPHLETS RECEIVED.

"On the Use of Strichnine in Epilepsy," by Walter Tyrrel (London, Hardwicke); "The Chemistry of the Breakfast Table," by F. R. E. Lowne (London, Simpkin); "The Contagious Diseases Act" (London, Henderson); "Sulphur in Iceland," by C. Blake (London, Spon); "The London Catalogue of British Plants," 7th edition (London, Hardwicke); "Mr. Herring and the Telegraphs" (London, Longmans); "Assaying by the Spectroscope," by W. E. Dubois (Philadelphia)—a very interesting description of a decided novelty in Physics; "The Fifth Report of the Peabody Trustees (Silliman, U.S.A., 1873); "Geological Survey of Victoria," Report of Progress, by R. Brough Smyth, F.G.S. (Melbourne, 1874).

---

* "Science Primers; Physiology." By M. Foster, M.A., M.D., F.R.S., Fellow of Trinity College, Cambridge. With Illustrations. London: Macmillan & Co., 1874.

† "Technical Training." By Thomas Twining, V.P. of the Soc. of Arts. London: Macmillan & Co., 1874.

SCIENTIFIC SUMMARY.

ASTRONOMY.

The Transit of Venus.—M. Faye has been lecturing on this subject in France, "to an audience which filled to overflowing the concert-room of the Cercle du Nord." "St. Paul's Island," we read in the *Times*, "the utterly desolate nature of that volcanic excresence to which Captain Mouchez is obliged to convey food, fuel, water, and shelter, in addition to instruments, the self-sacrificing devotion of the sailors and savants, relegated for six, or possibly twelve and more months, to an inhospitable storm-beaten rock, were leading features in M. Faye's *exposé* of the problem which all civilised nations have combined to solve. The lecturer demonstrated by means of diagrams and instruments why the expeditions of 1761 and 1769 failed under such observers as Captain Cook and La Caille; why an uniform method of observation and registration could not be arranged in our days; why the mode invented in the eighteenth century by Halley, and now supplemented by photography, was adopted this year by France and England, while (*in addition,* the account should have said) other processes are followed by the Germans, Russians, and Americans; and finally, what results the success of labours undertaken at some seventy stations (six being the share of France) in the Austral and Boreal hemispheres would secure for navigation."

The Total Solar Eclipses of 1860, 1869, *and* 1870.—We understand that the volume of the "Memoirs of the Astronomical Society," containing the results obtained during these eclipses, will be in a few months ready for issue. The delay as respects the eclipse of 1860 can but be regarded as monstrous. For this, however, Mr. Ranyard, the nominal sub-editor, but in reality the editor, of the volume, has been in no sense responsible. The volume will be a most valuable contribution to science; and if the literary excellence correspond to the amount of labour which Mr. Ranyard has devoted to this subject, the work will also be a notable contribution to scientific literature. The matter treated in our next paragraph affords significant evidence of the care with which Mr. Ranyard is dealing with the materials placed in his hands. It is also very interesting.

Strange Feature in the Photographs of the Solar Eclipse of December, 12, 1871. *Traces of a Comet?*—"The structure which I am about to describe is by no means a marked feature on the Indian photographs; indeed, it was not observed until after nearly a year had been spent in cataloguing the details which are to be made out on the different negatives. When, however, it has once been pointed out, no careful observer can have any doubt as to its existence; and the tardiness with which it was observed may perhaps be accounted for by the fact that attention was principally directed to an examination of the dark or partially opaque details of the photographs which correspond to the luminous details of the corona, whereas this was a bright or transparent structure; and bright spots, lines, or patches had always been regarded as photographic defects, and consequently but little attention had been paid to them. The original negatives are very small: the dark moon is represented by a transparent circle about $\frac{3}{10}$ths of an inch in diameter, and the whole extension of the corona could be covered by a sixpence. The separate details of the coronal structure are therefore very minute, and it would be impossible from the examination of a single negative to determine whether any small marking has its origin in some almost microscopic impurity on the collodion, or whether it represents a vast mass of many million cubic miles in the corona: it is only by a careful comparison of the different negatives that such photographic flaws can be properly eliminated. For this purpose a catalogue has been made, containing a list of the negatives upon which each detail can be distinguished, and the details entered in the catalogue have also been drawn. In this work I have been fortunate enough to be aided by a most accurate and conscientious artist (Mr. Wesley), for whose laborious perseverance in the task I cannot be too thankful.

"While working at a group of coronal structure on the eastern equatorial limb, Mr. Wesley noticed that a small bright spot, or flaw as we then considered it, occupied apparently the same position in negatives 1 and 4 of Lord Lindsay's series. On examining the others of Lord Lindsay's or the Baikul series, we found that a bright spot or flaw was more or less distinctly to be traced in the same place on all of them. I at first thought that it must be due to a star seen through the corona, but a little reflection showed us that this explanation could not be sustained, for the image of a star would have been represented by a dark or very opaque point, whereas this was bright; and therefore the collodion had at this point been less acted upon than by the light from the surrounding details. On a closer examination of the plates upon the next fine day, three partially transparent circular arcs, concentric with the bright spot, were detected above it. The middle one of the three arcs is the most distinct, and can be traced without any doubt upon four out of the five Baikul negatives.

"At this time I had only Lord Lindsay's negatives and two enlarged copies of the Java photographs to which I could refer. As all traces of the structure were lost on the copies of Lord Lindsay's negatives, I was not surprised to find that it could not be seen on the Java enlargements. After a short time I obtained the loan of Colonel Tennant's original negatives from Mr. De La Rue. They were taken at Ootacamund, a distance of more than 120 miles from Baikul, and I was therefore not a little astonished and pleased to find that the central bright spot was traceable on five out of the six negatives of his series. The central arc was just traceable on four out of the six negatives, and the inner arc is to be made out on three of the negatives.

"No difference can be detected in the position of the central spot and concentric arcs relatively to the details of the corona in passing from the

Baikul to the Ootacamund series; and taking into consideration the distance between the two stations, it is evident that the structure must be either due to some dark body in the corona, or to some semi-transparent body situated between us and the corona, at a great distance from the earth.

"The form of structure is similar to that which has often been observed in the nuclei and the concentric comæ of comets; and it seems not very unreasonable to suppose that this may really be a photograph of a faint though large comet near to perihelion.

"An undoubted comet has been seen projected on the brilliant background of the photoshere (I refer to Pastorff's observations of the comet of 1819*); and much fainter comets would be visible when projected on the comparatively feeble light of the corona. It seems, therefore, not very improbable that both this structure and the parabolic arc observed by Dr. Winnecke may have been due to comets which happened to be situated between us and the corona during times of total eclipse. And it is not impossible that such giant comets may exist in great numbers in the immediate neighbourhood of the sun, though by reason of their faintness, or the short time of their ebullition, they are not visible to us either before or after their perihelion passage."

Total Eclipse of April 16, 1874.—This eclipse did not attract anything like the attention which was directed to the eclipses of 1868, 1869, and 1870—a circumstance showing that the discussion of disputed points may serve a useful scientific purpose; for it can hardly be doubted that the difference resulted from the fact that when the former eclipses were about to occur matters were under discussion of which the observations made during these eclipses disposed, whereas in the present year no such discussion had been aroused. Nevertheless, the observations made by Mr. Stone, the Astronomer Royal at the Cape, are full of interest:—"I observed the eclipse," he says, "from Klipfontein, a station about 3,000 feet above the sea level. The sky was perfectly clear, and no finer day could have been wished for. I had borrowed a four-inch telescope, mounted as an altazimuth, from Mr. H. Solomon. My spectroscope was one with two dense flint prisms of 60°; a fair amount of dispersion, therefore, being thus obtained. My great difficulty was to attach the spectroscope firmly to the telescope. Ultimately I was obliged to give up all idea of using the prism of comparison, and fix the tubes together by wrappers of wash-leather. In this way I secured a very firm connexion between the spectroscope and telescope. I placed two wires in the focus of the telescope of the spectroscope for estimations, and determined to measure only the position of one line in the corona, the micrometer wire being left untouched until the reappearance of the Fraunhofer lines, when the differences between the line measured and these lines could easily be fixed.

"The slit was set as wide as would allow of a clear and distinct view of the Fraunhofer lines. This I did because I expected to find the spectrum of

---

* As well as to the nebulous spot observed by him upon the sun in May 1828. See the "Monthly Notices," November 1873. It may be added that the comet can hardly have been a tailed one, as the envelopes are on the side farthest from the sun.

the corona faint, but was anxious to see whether the Fraünhofer lines wore present or not in the spectrum of the corona. I could not change the width of the slit without taking the spectroscope off the telescope, on account of the way in which I had been compelled to join them together.

"During the partial eclipse I most carefully examined the spectrum near the moon's edge, by comparing it with the spectrum away from the moon's edge, to see whether any fresh lines were produced by any absorbing medium around the moon, but I could not find a trace of any difference in the spectra near the moon's edge and away from it. My slit was so placed that it was parallel to a tangent at the point of last appearance of the sun-light; and as the totality approached, my wife, by the aid of the finder, kept the sun's limb about half way across the slit. At the instant of totality the whole field appeared full of bright lines; but I had scarcely time to begin counting these lines before the greater number of them vanished, and the spectrum resolved itself into little more than that of hydrogen gas. Not wishing to spend the few precious moments available upon the spectrum of the prominences, I determined to see what the brightness of the corona really was before turning the telescope upon it. This was the only view of the eclipse I allowed myself apart from the spectroscope, and I probably spent half a minute lost in admiration of the scene presented to me. The rose-coloured prominences reached very nearly all round the moon's limb. My wife says there was one, and only one, complete apparent break in the continuity. The height varied, of course, very considerably at different parts of the sun's limb. The corona was much brighter than I expected to have found it; but its constitution appeared to me uniform throughout, except that its brightness was less as you proceeded further from the sun. I have a drawing of the corona by Miss Alice Hall, which in my opinion, and that of my wife, who observed the eclipse through a pretty good finder, very correctly represents the corona as seen from our station. I have also a drawing made about 500 miles away, which agrees in all the principal points with that by Miss Hall. I believe that there cannot be a doubt, from these drawings, that the great features of the corona were identical as seen from these two distant stations. One or two smaller drawings also agree in the principal features; and these drawings are, I think, more to be trusted than usual, from my instructions to refer the prominent points to sectors of 30° having been followed. I believe from the comparisons made, and what I myself saw, that the whole corona, as seen in South Africa, was a solar appendage. Returning now to my own work—On first moving the telescope away from the prominences I certainly saw two faint lines less refrangible than E, and one very bright line not far from E; but on moving the telescope more away from the limb of the sun, I appeared to have lost those fainter lines, and I did not subsequently see them. The spectrum of the corona appeared to consist of one very bright line and an ordinary sun-light spectrum. I feel perfectly convinced, although from the faintness of this sun-light spectrum the lines could only be seen with some difficulty, that the Fraunhofer lines were present in the spectrum of the corona. I examined this point again and again, and I feel more than ever certain upon the point from my wife's recollection of my strong expression of opinion upon it at the time I was making the examination.

"Having carefully looked to this point, I unclamped the spectroscope, and swept over the whole spectrum from the extreme red to the extreme violet; but, independently of the bright line referred to before, I could not at this time see any traces whatever of bright lines. My instrument was then again clamped, and the bright line most carefully bisected by the micrometer wire. I then re-examined the spectrum for Fraunhofer's lines, but without touching the micrometer or moving the telescope even, until the total eclipse was over and the Fraunhofer lines appeared in all their distinctness. The micrometer was then read, and the coronal line referred to two known lines near it by the micrometer. The wave length of this bright line agrees so closely with that given by Young, that I could not with my dispersion answer for so small a difference. I am perfectly certain about the numerous lines seen in the spectrum close to the sun's edge at the commencement of the totality, but the strata giving this spectrum must lie very close to the photosphere, for they were almost immediately covered by the advancing limb of the moon. I am not prepared to say that the line spectrum of the corona did consist entirely of one bright line, for, as I have said, I did see three lines near the sun's limb in the brighter part of the corona; but I am prepared to say that in the spectrum of the corona, at some distance from the sun, and away completely from the red prominences, there was no line in the spectrum of any degree of brightness except the one measured, and that there certainly was in addition to this an ordinary sun-light spectrum with Fraunhofer lines. I presume this spectrum must arise from the reflexion of the sun-light from the gas giving the line spectrum, and that we thus account for the polarisation of the light of the corona in the plane through the sun's centre. The natives were much afraid, and went to their huts. They got up a tale that I had brought the eclipse with me, and was looking for a missing star. Independently of the eclipse, I have made magnetical observations at four stations in Namaqualand, one at the Orange River."

BOTANY.

Influence of Climate on Vegetation at San Francisco.—Dr. Cooper makes some remarks in a paper on this subject before the Academy of Sciences at California, U.S.A. The comparative scarcity of trees and small number of species around the bay, as compared with the districts immediately north and south, arrested Dr. Cooper's attention. He concludes that the main cause of the deficiency is the prevalence of the sea-winds from the north-west, throughout the dry season; these sweeping in through the Golden Gate and through the depression of the coast range between Petaluma and Tomales Bay. The character of the soil and elevation above the sea are of comparatively little consequence. "Since the general course of the mountain ranges is nearly north-west in this region, and the wind strikes obliquely, and the sun in its daily course shines most intensely and longest upon the same exposure, it follows that this slope is almost everywhere destitute of trees, although along the coast exposed to the greatest rainfall and the most fog. The opposite or north-east slopes, therefore, usually have the greatest tree-growth. . . . These winds seem to act in two ways. First, by their drying power, as seen in the absence of trees on slopes of hills exposed to them, while trees may abound on the opposite slope, though facing the south and more exposed to the sun. Second, by their coolness not permitting the sun's heat to produce the growth, even where moisture is abundant." The grouping of the trees of the district at different distances around San Francisco is then given, in more detail.

The Structure of Bark beneath the Microscope.—Dr. Braithwaite, F.L.S., who has a paper of some considerable length in the last "Journal of the Quekett Club," on the vegetable tissues generally, makes the following observations on the subject of bark. He says that after the production of more or less numerous cork lamellæ, the phellogen dies or loses its vital activity, but a development of secondary cork tissue takes place within the bast part of the vascular bundle, in the form of tangential rows of tabular cork cells, which loosen from the growing outer part of the vascular bundle. The cork lamellæ, as it were, cut out and force off from the rind flat pieces in form of scales or rings; all this outer part is dead, and the process, oft repeated from the circumference of the stem, causes the new cork lamellæ to become gradually imbedded more deeply in the growing cortical tissue, and we get a constantly thickening peripheral layer of dry tissue separating from the living part of the rind; this is the bark. The condition is very evident in the large scales of bark in *Plantanx orientalis* or sycamore, and in old stems of the *Pinus sylvestris* or Scotch pine, and in the ringlike bands of the cherry-tree. In the oak, lime, poplar, elder and horse-chestnut, similar plates of thin-walled cells arise in the interior of the bast bundles, but the old dried scales do not fall off, but tear only at the margins in a longitudinal direction, so that the stem becomes clothed with bark consisting of several dead scales lying under each other, presenting internally all the elements of bast, and externally primary cork tissue. In the pine and larch we have a fissured periderm, like that of the horse-chestnut, and in the pine consisting partly of thin-walled and thickened cells in alternate layers, but the conifers are specially remarkable for the presence of a spurions large-celled parenchym tissue, which appears between the periderm layers, and separates the elements of the bast bundle into smaller or larger groups.

Physiologicul Groups in the Vegetable Kingdom.—In the "Archives des Sciences" for May 1874, M. A. De Candolle makes an attempt to classify plants according to the climate they inhabit. He sketches five groups. First, plants requiring a great amount of heat and moisture. A name expressive of both these requirements would be cumbrous; so he chooses for this group a name referring to the temperature only, and calls them *Megathermal* plants, or in short *Megatherms,* i.e., plants to which much heat is essential. These inhabit the rainy intertropical regions in the plains and sultry valleys up to the 30th parallel. Second, plants requiring about as much heat but far less moisture. These, taking the name from the latter characteristic, he terms *Xerophilous* plants, lovers of dryness. They are pretty widely distributed, but they especially affect the regions bordering the tropics, and extending, say to the 35th parallel in both hemispheres. The third group, *Mesothermal* plants, require, as the name denotes, moderate heat, also a fair supply of moisture, at least in the growing season, say a mean annual temperature of 59°–68° Fahr., that of the rather warm temperate zones. The fourth group, *Microthermal* plants, i.e., those demanding little heat, say a mean annual temperature of 57° or less, down to that of 32°, with of course a good summer temperature. In this group would be included the vegetation of our Northern States and Canada. The fifth group, *Hekistothermal* plants, those requiring least heat, such as make up arctic, antarctic, and alpine vegetation. His sixth group, *Megistothermal* plants, those which require an exceptional amount of heat, or a mean of over 86° Fahr., are mentioned as having probably played a part among the earlier vegetations, but as now represented only by a few lowly organized plants of thermal waters. He then goes on at a great length to consider the subject more fully, but of course we have not space here to follow him.

Wild Flowers and their relations to Insects.—This subject was admirably handled in the able lecture delivered on one of the evenings of the British Association, at Belfast, by Sir John Lubbock. In this, which we regret we have not space to lay before our readers, the author entered upon a summary of all that is known on the subject at present, and gave several of the results arrived at by Darwin and himself. The address we trust he will publish in full, with the illustrations, for assuredly it is one of the most instructive and interesting subjects in the whole range of science.

A Sketch of Systematic Botany.—At the British Association Meeting at Belfast, Mr. G. Bentham, F.R.S., read a paper on "The Recent Progress and Present State of Systematic Botany," commencing with a summary sketch of the state of the science in 1830, when the natural method of Jussieu was beginning to supersede the sexual system of Linnæus; of its progress from that year to 1859, when the study of the general affinity of plants had entirely superseded the classing them according to single organs; and of the great advance effected since 1859 owing to the explanation of affinities given by the adoption of the doctrine of evolution. After some notes on the language to be preferred, systematic works were considered under the six general heads of *ordines plantorum*, *genera plantorum*, *species plantorum*, monographs, floras, and miscellaneous descriptions. Under each head the particulars required were specified, the principal recent works glanced over, together with a short mention of the chief desiderata now recommended to the attention of systematic botanists.

Embryogeny of Tropæolum.—Professor Dickson gave, at the British Association at Belfast, the results of his investigations on the embryogeny of *Tropæolum peregrinum* and *Tropæolum speciosum*. In these species the principal peculiarity consists in the constant penetration of the carpellary tissue by the extra-seminal root process. In *Tropæolum majus* the extra-seminal root process developed from the outer side of the base of the suspensor. After perforating the seed-coat becomes elongated, and finishes its course in the cavity of the seed-vessel. In rare cases, however, this process has been found to penetrate by its very extremity the carpellary tissue. In *Tropæolum peregrinum* the extra seminal process penetrates the carpel after having run in the cavity of the seed-vessel half way. In *Tropæolum speciosum* this process dips into the carpel immediately after emerging from the seed. Dr. Dickson remarked that some would be disposed to look upon the abnormality in *Tropæolum majus* and the normal form in *Tropæolum peregrinum* as forms representing what might be viewed as stages in the evolution of such a species as *Tropæolum speciosum* from some form analagous to *Tropæolum majus.* In regard to this Dr. Dickson adversely criticised the Darwinian hypothesis as, in his opinion, inapplicable to the case under consideration.

Pollen-grains.—Mr. W. S. Bennett, B.Sc., read a paper (Brit. Association, Belfast) on the form of pollen-grains in reference to the fertilisation of flowers. He stated that although not unfrequently a common form of pollen-grains runs through a whole group of plants, yet more often the form is found to be adapted to the requirements of the species, and varies even within a small circle of affinity. In these plants, which are fertilised by the agency of insects, there are three general modes in which the form of the grain is adapted for the purpose. We have, firstly—and this is by far the most common form—an elliptical grain, with three or more longitudinal furrows, as in *Ranunculus ficaria, Acuba japonica,* and *Bryonia dioica;* secondly, spherical and elliptical, and covered with spines, as in many Compositæ, Malvæ; and, thirdly, where they are attached together by threads or a viscid excretion, as in *Ricardia Æthiopica.* In those plants, on the contrary, which are fertilised by the agency of the wind, as most grasses, the hazel, and *Populus balsamifera,* the pollen is uniformly perfectly spherical and unfurnished with any furrows, and is generally, moreover, very light and dry. The genus Viola supplies two very markedly different, in which the grains have the ordinary elliptical three-furrowed form, and where every point of the structure of the style and stigma is favourable to fertilisation by bees. In all Crucifers hitherto known, the pollen has the most common form. In the cowslip and primrose there is a uniform difference in size between the pollen belonging to the two forms, that of the short-styled being always considerably larger than that of the long-styled form.

Algæ from Jersey.—Dr. Williams read a paper on "Specimens of Algæ from Jersey." The paper referred to the large number of specimens of marine algæ to be found at Jersey, and to the favourable position of the island for the development of algæ. Dr. Williams produced a splendid collection of algæ preserved by a lady residing in Dublin. Dr. Moore said that the county Antrim afforded the finest examples of algæ perhaps known.

An Abnormality in Chrysanthemum Leucanthemum.—Professor Dickson exhibited specimens of an abnormal form of the oxeye daisy (*Chrysanthemum lucanthemum*), in which the outer florets of the ray (normally ligulate and and female) exhibit an irregularly tubular corolla, not very unlike that in the neuter florets in certain Centanreas. Structurally these abnormal florets are hermaphrodite, but appear always to be functually neuter or sterile. (Brit. Association, Belfast.)

Stems of Tree Ferns.—Dr. Moore read a paper on "The Growth of the Stems of Tree Ferns." Having dealt fully with the matter, he summed up his observations, stating, first, that some of the kinds of tree ferns grow with greater rapidity and form their stems in a much shorter period than is generally supposed to be the case; secondly, that after they attain a certain height the acrogenous buds are formed much closer together, one above another, then they are lower down the stem, hence their elongation is much slower; and, thirdly, some of the sorts, which at first form short rhizomatous stems before they take an upright position, require a considerable number of years to perfect the early part of their growth; but after the stem has been formed, and an upward condition taken, the growth is much quicker, and the elongation advances rather rapidly, compared with it while the stem retained its rhizomatory shape. (Brit. Association, Belfast.)

The Mosses of North-east of Ireland.—Mr. S. A. Stewart read a paper on this subject before the British Association, Belfast. The district he had investigated embraced the counties of Down and Antrim, with a small portion of county Derry, adjoining Antrim. The species of mosses known to occur in this district he estimated at 196, including four species not previously recorded as Irish—viz. *Fissidens incurvus, Tayloria serrata, Mnium subglobosum*, and *Seligerai pusilla.* We have thus in one district rather more than one-half of the Irish species of mosses, and more than one-third of the entire British moss flora. As might be expected from the geological character of the country, the greatest deficiency is in species that prefer a limestone *habitat.* Only 20 per cent. of that portion of the British mosses occur here.

Structural Peculiarities of the Ampelideæ.—Professor Lawson read a paper on "Certain Peculiarities in the Indian Ampelideæ." He remarked that many of the species were vast climbers, with their branches interlacing in the tops of the highest trees. In the stems of all were to be found numerous very large ducts, and these ducts were filled with inter-cellular vesicles, in which, at a certain time of the year, abundance of starch was developed. He also remarked that in the frond most important differences might be found, but that these afforded no means by which to divide the genus. With respect to the inflorescence, he said that there was great fixity of form. Two species only reached the Eastern coast of Africa, most being confined to India, though some few were common throughout the Malayan Archipelago.

Where are the Saprolegniei to be placed?—The "American Naturalist" (June) says that this family, that seems now finally deposited in the Algæ, has considerable economic interest from the destructive effects produced upon fish-eggs in the hatching trays, supposed to be caused by *Achlya prolifera.* The following summary is translated from advance sheets of "Contributions to the Morphology and Systematic Relations of the Saprolegniei," by N. Pringsheim. The results of his investigations on the Saprolegniei may be condensed as follows:—1. In all the Saprolegniei the male organs of generation develop from the well-known antheridia, that are formed near or grow toward the oogonia. 2. Those in which antheridia or their equivalents are wanting are not, as has been supposed, distinct species, with modified organs, but parthenogenetic forms, whose sporangia ripen and bud without fertilization. 3. In the Saprolegniei there is but one kind of sporangia; those which develop parthenogenitically and those which are fertilized are identical, and show no difference originally. The unfertilized zoospores grow sooner and more readily than those which are fertilized. 4. Several peculiarities in the formation of zoospores, which have been considered sufficient specific distinctions, are not important as such, but are merely evidences of a greater or less tendency to dimorphism, representing various stages of development in the zoospores. 5. Also various sexual forms of growth may appear in the same species, which are not reliable as specific distinctions.

CHEMISTRY.

The Chemical Society's Monthly Reports.—These have formed the subject of a report which was made at the British Association at Belfast. Professor Roscoe brought forward this report. The way in which the Association assisted science was by giving donations of funds for particular scientific purposes. He had to bring forward a short report on a subject on which all chemists were agreed as to its importance, and as to the usefulness of the work carried on. For some years past—for the last four years—the Council of the Association had voted 100*l.* per annum to aid the Chemical Society of London in carrying out a most important work—namely, in publishing abstracts of all the papers of interest and importance which were published during the year in foreign journals, thus bringing home to English chemists—to those who had not the opportunity or ability to peruse the journals in which the original communications appeared the opportunity of obtaining in each month of the year a complete knowledge of what was going on elsewhere. This was the last year in which it was proposed to ask for a grant of money, and the committee of which he was a member had to report to this section, as a matter of form, the result of the work which had been done by the money voted by the Association last year at Bradford. They had here an instance of the way in which the Association really assisted the progress of science—first, by the prosecution of such important determinations as those of which they had just heard so clear an account given by Professor Foster, and by this grant of money for doing what otherwise would not be done. The report mentioned that the work had been continued during the past year, and was conducted by the abstractors with care and accuracy. The undoubted success of the undertaking was due to those gentlemen who had performed this labour, and to whom, therefore, the thanks of all interested in the progress of science were due.

Occurrence of Leucin in the Fresh Juice of the Vetch.—Both leucin and tyrosin stand in very intimate relations with the albuminates. Their frequent occurrence in the body, their appearance in the urine in certain diseases, their rapid production in the peptonizing of the albuminates, all go to show the importance of these relations. Now, since asparaginic acid occurs among the decomposition products of leucin, and since asparagin appears during the sprouting of the papilionaceæ, and disappears later while protein bodies are forming, it occurred to M. Gorup Besanez to examine, for leucin, the juice of the common vetch, grown in rich earth and in the dark. The juice, freed from albuminates by boiling, was dialysed, the diffusate being evaporated till the asparagin crystallised out. The mother liquor, on further concentration, deposited a granular substance, which formed crusts on the surface of the liquid, and which had all the appearance of leucin. Repeated recrystallisation from boiling alcohol gave it pure, in the form of small spheres of sharp outline and radial in structure. Its reactions proved it to be leucin. The author believes, with Dragendorff, that the substance obtained by Reinsch from the juice of *Chenopodium album*, and called chenopodin, is really leucin.

Spontaneous Generation from a Chemical Point of View.—Professor Debus, F.R S., in delivering an address before the British Association at Belfast, said that one of the most perplexing problems in physiological inquiry was the question—What is the origin of the lower forms of life which are supposed by some to be formed spontaneously from inorganic material? He proposed to consider the question entirely from the chemist's point of view. The question was—Was it possible that the lower forms of life could be produced as the mere spontaneous work of inorganic material? Some philosophers believed that such a formation was possible. Darwin, in his work, propounded a certain view about changes which animals underwent when the external conditions under which they lived became changed. By experiments, and drawing conclusions from them, he did not go so far as to assert it was possible that from inorganic matter organic life could be originated. He never said that, though some followers had expounded his theory as going to that length. He (Professor Debus) thought that this question was really one in chemical science. By inorganic matter he meant matter that met their senses—as a piece of flint—and by organic matter was to be understood the substances which were found in the bodies of animals and plants. Professor Debus proceeded at some length to consider the question chemically, and concluded by saying that the result of the experiments of chemists was that there were not any conditions favourable to the formation of organic matter from inorganic material; that, on the contrary, life was destroyed when the temperature was raised beyond a certain point; and chemistry also showed that in former ages there did not exist the conditions which would enable organic life to be formed from inorganic matter.

Blue Pigment of the Egyptians.—Fifteen centuries before the Christian era the Egyptians appear to have been acquainted with the preparation of three distinct kinds of blue pigment, prepared from mixtures of sand, soda, and lime, with oxide of copper. One of these fine colours has been lately examined by M. Henri de Fontenay, who contributes a paper on the subject to the June number of the "Annales de Chimie." The investigation was conducted in Peligot's laboratory, at the Conservatoire des Arts et Métiers, and some examples of the blue frit were then made at the National Porcelain Factory at Sèvres, under M. Salvétat. The author publishes not only analyses of ancient specimens, but recipes for their imitation. A mixture of 70 parts of white sand, 25 of chalk, 15 of oxide of copper, and 6 of dry carbonate of soda, yielded, when fritted together, a blue material said to be equal in colour, texture, and durability to the ancient examples.

Chemical Constitution was the subject of an address before the British Association (Belfast), by Dr. Crum Brown. He first pointed out that the phlogistic controversy, which resulted from the discovery of oxygen, terminated in the total defeat of the phlogistists and the entire destruction of their theory. The issue of the next great chemical controversy, that between the dualistic theory of Berzelius and the substitution theory of Dumas, Laurent, and Gerhardt, was very different. Some historical writers argue as if here also the new doctrine had superseded the old; but the speaker maintained as a more correct view of what really took place, that each theory underwent independent developments; and, inasmuch as both were sound, though imperfect, this development has now brought them very nearly to the same point. He illustrated this statement by an examination of the views of Berzelius as to the constitution of acetic and trichloracetic acids and the constitution of ternary compound, and showed how the difficulties involved in some of these cases were cleared up by the discovery of the multiquivalent character of the atoms of certain elements. Dr. Brown defined chemical constitution as the order in which the constituents are united in the compound, and pointed out that the study of chemical changes (combination and decomposition) cannot lead us to a knowledge of the relative position of the atoms. But such a knowledge is required before a real theory of chemistry can be attained, and a knowledge of the intimate structure of matter may be looked for from an examination of the physical properties of substances, and comparison of these with their chemical constitution. This, he maintained, is truly a branch of chemistry, and the greatest progress in it has been made by chemists, as may be proved by reference to the work of Faraday, of Graham, and of Andrews. By pursuing this branch discoveries may be made which will lead to a hypothesis directly connecting chemistry with dynamics, and enabling us to apply mathematics directly to chemistry. The theory of chemistry will then be a particular case of the theory of dynamics. Such a result must be expected by all who believe in the progress of human knowledge, and in the consistency of nature.

Crystallisation of Glass.—M. Peligot states (in "Bull. de la Soc. d'Encourage. pour l'Industrie Nationale," No. 5), that in clearing out the glass furnace of M. Chagot, at Bianzy, certain crystalline geodes were found, which had been formed during the cooling of the vitreous mass. Their composition is given under No. I., No. II. being the transparent glass from which these crystals had separated, and No. III. the glass in its normal condition:—

	I.	II.	III.
Silica	62·3	61·8	62·5
Lime	22·7	21·5	21·3
Magnesia	8·4	5·4	5·6
Oxide of iron	3·2	3·0	3·0
Alumina	2·5	2·1	2·1
Soda	0·9	6·2	5·5
	100·0	100·0	100·0

Hence it appears that devitrified or crystallised glass has undergone, not a mere physical change of structure, but an alteration in its chemical composition. The increase of magnesia, and the decrease of soda, are remarkable. The form of the crystals approaches that of pyroxene; that is, an oblique but nearly right prism.

Iron and Manganese in Steel by the Bessemer Process.—The "Chemical News," August 7, says, in its excellent summary, that carbon and phosphorous are pronounced mutually exclusive, but either of them yields with iron an alloy possessing the properties of steel. The metallurgic company of Terre-Noire has achieved a result of great importance for the perfectation of the Bessemer and Martin process by the introduction of solid blocks of an alloy of iron and manganese, containing 65 per cent. of the latter metal, and capable of being introduced into the converter in the last stage of the operation.

Absorption of the Ammonia of the Atmosphere by Plants.—M. T. H. Schlœsing says, "Comptes rendus," June 15, he experimented upon two tobacco-plants exposed to confined, but renewable, artificial atmospheres, prevented from coming in contact with the soil. To one of these atmospheres ammonia was regularly supplied in small known quantities. The plant exposed to this atmosphere was richer in nitrogen than the other in every part. The production of nicotine was not sensibly affected.

GEOLOGY AND PALÆONTOLOGY.

The Tenth Report on Kent's Cavern was read by Mr. Pengelly, F.R.S., at the meeting of the British Association at Belfast. This cave contains four layers in ascending order—breccia, crystalline stalagmite, cave earth, and granular stalagmite. The work of exploration is still going on in the Cave of Inscriptions and Clinnick's Gallery. The breccia has yielded a great number of implements, and bones in abundance have been found. One very fine implement was found near to the great boss of stalmagite, from which an inscription on its surface shows that the stalagmite has undergone no change during the last two-and-a-half centuries. Mr. Pengelly differs from Sir C. Lyell, inasmuch as the author holds the opinion that man came to England, not only when it was part of the continent, but men must have been in Devon at a much earlier period, when England was separated from the continent of Europe. In the discussion which ensued, Professor Geikie, of Edinburgh, doubted the conclusion that the author had arrived at—viz. that the men of Devon had looked upon the glaciers of the glacial period. Rev. Mr. Croskey and Professor Harkness took part in the discussion.—Mr. Pengelly, in reply, said he based his opinion regarding the early appearance of man on the remarkable absence of the hyena, which is so abundant in the cave earth.

The Silicified Rock of Lough Neagh.—Professor Hodges read an interesting paper on this subject before the British Association (Belfast), in which he said that analyses show that the water of Lough Neagh, in our time at least, possesses no peculiar qualities, and that the lapidifying material of the petrified wood is silicic acid and not oxide of iron.

Coal (or Lignite) in the Cretaceous of Minnesota.—Professor H. Winchell, in his report for 1873, announces the existence of coaly layers at several points in the cretaceous of Minnesota, especially in the banks of Crow Creek and those also of the Redwood; but states that all explorations thus far made have proved fruitless. The coal found on the Redwood was earthy, passing sometimes into good Cannel coal, or into a bituminous clay; the compact Cannel coal is in detached lumps, and occurs throughout a band about four feet in thickness. At another outcrop the lignitic band is in the form of a black bedded clay or shale, five or six feet thick, containing some coaly or charcoal-like fragments. Mr. Winchell adds that "so far as discovered, there is not enough coal embraced in the cretaceous of this State to warrant sanguine expectation of its becoming economically useful." (See "Silliman's American Journal," July.)

Cockroaches in the Carboniferous of Cape Breton.—In the "Canadian Naturalist" (Vol. VII., No. 5) Mr. Scudder describes two new species, having two wings, described by R. Brown. He has given them the names of *Blattina Bretonensis* and *B. Heeri.* They are in a duskish shale, and are associated with leaves of Sphenophyllum and ferns.

Marine Champlain Deposits on Lands north of Lake Superior.—Dr. Dawson, in his annual address before the Nat. Hist. Society of Montreal, May 18th, says that Professor Bell, in the "Report of the Canadian Geological Survey for 1870-71," states the occurrence of marine shells, similar to those of the Champlain deposits in the vicinity of Montreal, at a height of 547 feet above the sea. Dr. Dawson also remarks that in the hills behind Murray Bay and Les Éboulements, he has observed these shells at a height of at least 600 feet; and also that Mr. Kennedy has recently found marine shell deposits of the same era on Montreal Mountain, at a height of 534 feet above the sea. ("Silliman's American Journal," August.)

Climate of the Champlain Period.—Dr. Dawson repeats his conclusion that the climate of the period when the land stood below the present level, "that which immediately preceded our own modern age," was cold. However, it must be observed that the evidence he adduces only proves the existence of cold as regards the waters. No fact is adduced by him in support of the supposed cold climate on land. (Annual Address before Montreal Nat. Hist. Society, May 18.)

Description of the Italian Geological Map.—"Silliman's American Journal" (August, 1874) says that the first volume of the series, in illustration of the geology and geological map of Italy, was published at Florence in 1871, and the first part of the second in 1873. They are quarto volumes, beautiful in style of execution, and admirable in the maps and engravings. The first has already been briefly noticed in "Silliman's Journal." The second contains papers—"On the Physical Geography of the island of Ischia," by Dr. C. W. C. Fuchs; "On the Alpine Chain of the St. Gothard to be tunnelled for the Italico-Helvetic Railway," by F. Giordano; "On the Tertiary Formations of the Sulphur-bearing Zone of Sicily," by S. Mottura; "Malacologia Pliocencia Italiana," by Dr. C. d'Ancona, Fasc. II., genera *Pisania, Ranella, Triton, Fasciolaria, Turbinella, Cancellaria, Fusus.*

A London Clay Emu.—A tibia from the London Clay has, we learn, been obtained by Mr. H. S. Seeley, F.G.S. It appears to be the distal extremity of the right tibia of some large struthous bird, which approximates both to the emu and the apteryx. However, until some more of the remains be found it will be impossible exactly to fix its true position.

It was obtained from the Isle of Sheppy, which has already yielded a considerable store of valuable fossils.

Further Researches on Eozoon Canadense.—At the meeting of the British Association Dr. Carpenter read some further notes on the structure of this interesting fossil, and there was some conversation on the subject by Prof. Rupert Jones, Mr. Hull, Mr. Harkness, and Mr. Pengelly. Strange to say, neither Mr. King nor Dr. Rowney put in any appearance, though they are strictly the opponents of Dr. Carpenter's ideas. However, there cannot now be any doubt that Eozoon is an animal structure. Any who hold opinions the opposite to Dr. Carpenter are referred to the "Monthly Microscopical Journal" for September, in which they will find an abstract of a recent paper by Dr. Carpenter, accompanied by a sketch, both of which undoubtedly prove the animal nature of *Eozoon Canadense.*

The Committee on Coal Measures: Labyrinthodonts.—Mr. Miall, F.G.S., read the report of this committee before the British Association (Belfast), and dealt with the classification of the Labyrinthodonts. He reported that the committee had gone through all their work. It was their desire not to be reappointed. The following points in the characteristics of the Labyrinthodonts are to be noted, as they differ from the statements usually published in ordinary manuals:—The skull may be regarded as an amphibian skull, overlaid with crocodilian plates. The teeth occur on the palate and maxiliary bones in double rows, and are very numerous. Three thoracic plates are present, and the body is covered with bony scutes. They were all, except two genera, provided with four limbs, which may have been penta- or tetra-dactyla. The vertebræ are numerous, the tail is long, and in some genera makes a most efficient swimming organ. Forty-two genera and 126 species are now known, principally owing to the exertions of recent explorers. Some of these animals in their mode of life appear to have been fish-like; some resembled serpents, others crocodiles; whilst those of Kilkenny appear to have been salamanders.

MEDICAL SCIENCE.

Report on Internal Secretions.—At the meeting of the British Association (Belfast), Dr. Pye Smith read a lengthened report from the committee appointed to investigate into internal secretions. In the course of his remarks, he said for some time the opinion had prevailed among physiologists that the nervous system not only exerts an influence upon the calibre of the vessels supplying glands with blood for secretion, but that the secreting cells themselves are excited to action by nervous stimuli. So firm, indeed, has this opinion been held, that Pflüger's discovery of nerves terminating in the secreting cells has been almost universally accepted, notwithstanding the failure to demonstrate these structures to others. This was partly due to the distinguished physiologist who held this belief, and also to the conviction which prevails that such structures must exist. A distinct proof to this effect has been afforded by the researches of Heidenham on the effect of atropia upon the secretion of the sub-maxillary gland. When one of the nerves going to this gland—viz., the corda tympani—is stimulated, two effects occur. First, the vessels going to the gland dilate, the blood flows quietly through them, and a free supply of lymph is poured out into the lymph spaces surrounding the gland; secondly, the cells of the gland absorb this lymph, convert it into saliva, and pour it out into the duct of the gland. If the animal be poisoned, or other events which he mentioned occur, the result would be different. There was a third way in which secretion might be induced, and that a somewhat extraordinary one—viz., by paralysis of certain nerves going to the gland instead of by irritation. It had not yet been ascertained whether this kind of secretion could be counteracted by atropia or not. With regard to secretion in the intestines very little was known, but it was probable that the process performed was much the same as in the salivary gland. He then instanced the reasons which led to this belief. He also described operations that were made in relation to the discovery of internal secretions, and thus concluded his paper.

Manufacture of Albumen from Blood.—A new journal, the "Laboratory," for July (it is published in the United States), says that albumen is now produced on a large scale at Pesth, Hungary, and in North Germany, from the blood of animals. The serum separating when blood coagulates, consists chiefly of albumen. The best quality of albumen thus obtained is transparent and soluble in water, and is used for mordanting yarns and cloth. At Pesth blood is dried in flat iron pans, by exposure to air at a temperature of from 100° to 112° F. From 300 lbs. of blood about 110 lbs. of albumen are obtained, at a cost of $29; 16,200 eggs would yield the same amount of albumen, at a cost of $96. Although the cost of egg albumen is three times as great as that of blood albumen, the former is preferred for dyeing purposes, on account of its purity. Blood albumen of a second quality, darker in colour, but nearly all soluble in water, is used largely in the process of refining sugar.

The Seventh Report of the Peabody Trustees, at Cambridge, U.S.A., has just been published, and it opens with an account of the Agassiz collection obtained during the voyage of the *Hassler*, with descriptions of the crania and other specimens. One microcephalic skull, from Ancon, of an individual not quite adult, has the internal capacity 33 cubic inches, or 44 per cent. of the average Peruvian cranium, and much smaller that the crania of some Peruvian children not over seven years of age. Though probably idiotic, there are no marked signs of it. The closing article is on Human Remains in the Shell Heaps of the St. John River, East Florida, in which the author, Prof. Wyman, presents reasons for believing that cannibalism was practised by the Indians. In evidence of this, it is stated that the bones were not deposited as in ordinary burial, but scattered in a disorderly manner; secondly, they were broken, as if reduced to a size suitable for the vessels used in cooking; thirdly, there was a degree of method in the breaking of them, showing that it was not done by wild animals. (See also "Silliman's American Journal," August.)

Influence of Food and the Methods of applying it to Plants and Animals.—At the British Association Meeting (at Belfast), the President of the Medical Section, Dr. Redfern, read a paper on the above subject. He said he thought that, though people so frequently act as if food were a matter of indifference as regards their capacity for either mental or bodily work, there is a general conviction that food requires to be taken, or at least that a pretence of taking it must be made. Plants had long shown that they should have food in abundance, and in a condition in which they could easily make use of it. Of this he gave several instances. He then proceeded to give causes showing the necessity for supplying plants with food. The same applied to many persons who had never applied their intelligence to the selection of their food or the method of taking it. He believed there were few social problems of more importance than how we should acquaint the wife of the labourer, the artisan—nay, even the wives or servants of the middle class—how they should expend a fair share of their income upon food to the greatest advantage, and how they should prepare that food when they had purchased it without destroying its nutritive qualities. One or two instances will make his meaning perfectly plain. A savoury dish of meat (very common in some districts) is prepared by mincing or cutting into small more or less cubical blocks. This is then stewed, or more frequently boiled; the outer surface of each little block has its albumen firmly coagulated, and the whole is converted into about as indigestible a mess as can well be imagined, the high-priced and highly nutritious meat having been destroyed.

Small Size of the Brain in Tertiary Mammals.—At the last meeting of the Connecticut Academy of Arts and Sciences, June 17th, Prof. Marsh, of Yale College, made a communication on the size of the brain in Tertiary mammals. His researches on this subject have been mainly confined to the larger extinct mammals which he had obtained in the Rocky Mountain region, and the results are of peculiar interest. The Eocene mammals all appear to have had small brains, and in some of them the brain cavity was hardly more capacious than in the higher reptiles. The largest Eocene mammals are the *Dinocerata*, which were but little inferior to the elephant in bulk. In *Dinoceras* (Marsh), the type genus, the brain cavity is not more than one-eighth the average size of that in existing rhinoceroses. In the other genera of this order, *Tionceras* (Marsh) and *Unitatherium* (Leidy), the smallness of the brain was quite remarkable. The gigantic mammals of the American Miocene are the *Brontotheridæ*, which equalled the *Dinocerata* in size. In *Brontotherium* (Marsh), the only genus of the family in which the skull is known, the brain cavity is very much larger than in the Eocene *Dinoceras*, being about the size of the brain in the Indian rhinoceros. In the Pliocene strata of the West, a species of mastodon is the largest mammal; and although but little superior in absolute size to *Brontotherium*, it had a very much larger brain, but not equal to that of existing Proboscidians. The tapiroid ungulates of the Eocene had small brain cavities, much smaller than their allies, the Miocene *Rhinocertodiæ*. The Pliocene representatives of the latter group had well developed brains, but proportionally smaller than living species. A similar progression in brain capacity seems to be well marked in the equine mammals, especially from the Eocene *Orohippus*, through *Miohippus* and *Anchitherium* of the Miocene, *Pliohippus* and *Hipparion* of the Pliocene, to the recent *Equus*. In other groups of mammals, likewise, so far as observed, the size of the brain shows a corresponding increase in the successive subdivisions of the Tertiary. These facts have a very important bearing on the evolution of mammals, and open an interesting field for further investigation.

METALLURGY, MINERALOGY, AND MINING.

The Crystallography of Datolite.—A paper on this subject appears in "Tschermak's Min-Mittheilemgem," by Mr. S. Dana (Vienna, 1874), and contains the results of a crystallographic study of the datolite of the principal European localities, and is illustrated by a plate containing figures of crystals from Tuscany, Arendal, and Andreasberg, with also, for comparison, three of the forms described and figured by the author in his Bergen Hill paper.* In addition, a catalogue is given of all the known planes of datolite crystals, now numbering seventy-one, of which sixteen were added by Mr. Dana from the Bergen Hill crystals, and ten from those of foreign localities. A table contains the principal angles of all the forms, for the most part recalculated by the author, and a diagram presents a map-projection, after Miller's method, of all known planes in their zones. The crystals studied were from the Royal Mineralogical Museum of Vienna, of which Professor Tschermak is Director.

Mineralogy of Chili.—The President of the University, Don I. Domeyko, has published a work on the mineralogy of Chili, which appears to be of value. The fourth appendix to the second edition of this contains notes on new localities, with descriptions of various minerals, the most of them metallic species. For a double chloride of silver and mercury at Los Bordos, in the department of Coppiapo, the name Bordosite is given by Senor Bertrand. Ulexite and Hayesite are stated to have been found at a locality on the river Loa in littoral Bolivia, and in Carmen Alto, fourteen leagues from Antofagasta, the old localities being in the desert of Atacama in Peru, and at Ascotan in Bolivia. In addition, Domeyko now adds another locality at Ola, about thirty leagues to the east of the mines of copper of Chanaral de las Animas, north-east of the range of Dona Ines; the place appears like a dried lake. The locality of borocalcite (Hayesite), in the dry lake of Maricunga, fifty-nine miles to the north of Puquios, is, according to Fonseca, of great extent, he estimating the amount at 14,000,000 tons. A memoir on the subject has been published by Fonseca in the "Anales de la Universidad." It is mainly a *hydrated borate of lime*—boraclite, mixed with some common salt, but without any ulexite (boronatrocalcite).

A new Mineral: Veszelyte.—A new mineral has recently been described by Professor A. Schrauf, the eminent crystallographer of Vienna, under the name of *Veszelyte.* It is triclinic, resembling distorted liroconite, the crystals being bounded by the prism and dome (100 : 041 = 101° 3′). It has a bluish-green colour, and the composition is expressed by the formula $4CuOP_2O5_2H_2O$. It occurs on garnet at Morawitza, in Banat. It is named from the discoverer.

Ataeamite from South Australia.—Mr. A. S. Dana has recently (says "Silliman's American Journal," August 1874) published the results of a

---

* "Silliman's American Journal," III. iv. 16.

large number of measurements of crystals of Atacamite, from Wallarro, South Australia. These prove that the species is, as hitherto supposed, orthorhombic, but show further some irregularities in the planes of the vertical series, which can be explained only by the assumption of a dome 40-i taking the place of i-i, and a corresponding pyramid, in place of the prism I. The crystals under examination were placed in the hands of the author by Dr. A. Schrauf, of the Vienna Mineralogical Museum.

Livingstonite, a New Mineral.—Mr. Marianus Barcena describes this in "El Minero Mexicado," May 1874. He says that Livingstonite much resembles in colour and aspect stibnite or sulphide of antimony. It occurs in prisms, apparently isomorphous with stibnite, and like it in thin columnar groups. Color, bright lead-gray; of powder red, instead of black like stibnite. Hardness, 2 on Breithaupt's scale. Density, at 16° C, 4·81. Fuses at the first touch of the blowpipe flame, and gives out abundant white fumes. Cold nitric acid does not sensibly attack it; but warm dissolves it, and a white residuum falls. Sulphydric acid precipitates it, forming a yellow sulphide and another of a black colour. Reactions show that it contains mercury as well as antimony. An analysis has not yet been completed, but an assay proved the presence of 10 per cent. of mercury, showing that it is in all probability a sulphide of mercury and antimony. It is from Huitzuco, in the State of Guerrero, Mexico. Mr. Barcena has named it in honour of the distinguished African traveller, Dr. Livingstone.

MICROSCOPY.

A Finder for Hartnack's Microscopes.—The following is from "Science Gossip" for September:—Those of our readers who use Hartnack's, or microscopes of similar construction, will find Mr. Hicks's contrivance for the purpose of easily refinding an object at any time very useful. A line is to be ruled across the centre of the stage from side to side. Crossing this line at right angles are ruled two lines about two inches apart, one on either side of the aperture of the stage. In order to use the finder, a label about half an inch in diameter is fixed to each end of the slide, the lines on the stage left uncovered being used as guides for continuing the lines across the label with a pencil-mark. If the lines marked on the label be made to coincide with those on the stage, the object on the slide will be found in the centre of the field. Mr. Hicks says that he finds roughing the ends of the slide with a corundum file, and marking the point of intersection with an ink dot, preferable to the labels, as being more accurate. We think a label somewhat less than an inch square (about nine-tenths of an inch), so as the stage-lines might be seen round the margins, would be more advantageous than those of a circular form, as the position of the stage-lines could be marked on the margin of the label, and the position of the object might be registered by marking down the number of dots from the top left-hand corner; supposing it to be the fourth in a vertical and the seventh in a horizontal direction, it could be registered either 4 | or 7—.

Microscopy of the Quarter.—The following papers, some of which are of considerable interest, have appeared in the July, August, and September numbers of the "Monthly Microscopical Journal":—

Synopsis of the Principal Facts elicited from a series of Microscopical Researches upon the Nervous Tissues. By Dr. H. D. Schmidt, of New Orleans, La.—On Bog Mosses. By R. Braithwaite, M.D., F.L.S.—The Optical Quality of Mr. Tolles' $\frac{1}{6}$th Objective. By Robert B. Tolles.—On the Structure of Diatoms. By G. W. Morehouse, U.S.A.—The Presence of Balbiani's Nucleus in the Ovum of Osseous Fishes. By Dr. Van Bambeke.—Observations on the Tolles' $\frac{1}{6}$th. By R. B. Tolles, Boston, U.S.A.—On the Nervous System of Actinia. By Professor P. Martin Duncan, M.B., Lond., F.R.S., &c.—On Diapedesis; or, the Passage of Blood-corpuscles through the Walls of the Blood-vessels, and how to observe it. By Joseph Needham, F.R.M.S.—On the Morbid Growths from a case of Osteoid Cancer of the left Femur. By Joseph Needham, F.R.M.S., London Hospital.—Discussion of the Formula of an Immersion Objective of greater Aperture than corresponds to the Maximum possible for Dry Objectives. By Mr. R. Keith.—Final Remarks on Immersion Apertures. By J. J. Woodward, Assistant-Surgeon U.S. Army.—Refracting Prism for Binocular Microscopes. By F. H. Wenham, V.P.R.M.S.—On the Value of High Powers in the Diagnosis of Blood Stains. By Joseph G. Richardson, M.D., Lecturer on Pathological Anatomy in the University of Pennsylvania, and Microscopist to the Pennsylvania Hospital.—An Account of certain Organisms occurring in the Liquor Sanguinis. By William Osler, M.D.

PHYSICS.

Some Curious Experiments in Sound.—Mr. A. M. Mayer gives an interesting paper, entitled "Experiments in Acoustics," in "Silliman's American Journal" for August. For example, he says if we take an Ut_3 fork and vibrate it near the ear, and closely apprehend the character of its sound, we shall experience a sensation which certainly does not contain that corresponding to the higher octave of the fork. Now, press firmly the foot of the fork against the zygomatic process, close to the ear, directing the foot of the fork somewhat backward, and we shall distinctly hear the higher octave of the fork singing in concert with its real note. If the auditory canal be now closed by gently placing the tip of the finger over it, we shall perceive the higher octave with an intensity almost equal to that of the fundamental note. The same sensation, though less intense, may be obtained by placing the fork on any part of the temporal bone. One can also perceive distinctly the higher octave when the fork is placed on the parietal bone, about two inches in front of and an inch or so to the side of the foramen, and its foot directed toward the opposite inner ear, while the auditory canal of this ear is gently closed with the tip of the finger. But the higher octave sings out with the greatest intensity when the foot of the fork is placed on the tragus of the outer ear. A friend, who is a musician as well as a physicist, repeated these experiments, and he informs me that when the foot of the fork is placed against the tragus of his ear he hears the higher octave to the almost entire exclusion of the lower, and with a clearness that reminds him of the sensation perceived when an Ut_4 resonator, placed to the ear, reinforces its proper note. The higher octaves of several forks have been thus perceived, but the forks from Ut_3 to Ut_4 inclusive appear to give the best results.

The Laws of Frictional Electricity.—M. L. Joulin has investigated the laws of frictional electricity with a machine suggested by the electrical phenomena often observed in belts used for transmitting power. A very great quantity of electricity is thus produced, capable of giving long brushes and sparks, of deviating a galvanometer needle, of decomposing water, and in Geissler tubes of showing the stratification of the light. A new method of measuring electric tension has been employed, dependent on the greatest distance at which a brush is perceptible on a given sphere when brought near the electrified body. The experiment was varied by using pulleys of various metals or non-conductors, and by changing the velocity tension and temperature. In arranging the results there seem to be three causes influencing the production of electricity—the velocity of separation of the parts of the belt and pulley, the complex mechanical action of bending the belt, and the temperature of the two materials.—"Ann. de Chim. et de Phys." ii. p. 5.

Experiments with Thermometers.—At the British Association Meeting (Belfast), Mr. G. J. Symons exhibited a series of fourteen very carefully made thermometers, all differing either in the size or shape of the bulbs, or in the material with which they were filled, some being mercurial and some containing alcohol. They had been specially constructed with a view to testing the relative sensitiveness of different patterns and sizes. The results of the experiments had been printed in the "Quarterly Journal" of the Meteorological Society, and were as follows:—(1) That very large spherical mercurial bulbs are very little better than those filled with alcohol, but that with small bulbs mercury is much more sensitive. The new minimum thermometers (the bifurcated and the double cylinders), introduced respectively by Messrs. Casella and Hicks, were highly praised. Mr. Symons said that he brought them before the section mainly in order to offer the loan of the entire series to any experimentalist with more leisure than himself, and who would develop and complete the inquiry which he had begun.

Collision of Stars.—Mr. G. J. Stoney, F.R.S., read a paper on the results which would take place supposing two stars to come into collision, the effect being a great increase in light and heat. He pointed out that there might be many cold or dark stars in space of which science knows nothing. There are some stars which appear and disappear at intervals. This might be due to one star passing and "wiping" another.—"British Association Reports."

A New Deep-Sea Thermometer.—Mr. Negretti exhibited (at the British Association, Belfast) a very clever invention in connection with thermometers for ascertaining deep-sea temperatures. A deep-sea minimum thermometer will register the lowest temperature through which it passes, so if a layer of water above the bottom of the ocean is colder than the water quite at the bottom the temperature of the deepest water cannot be ascertained with the ordinary instrument. But by his invention, mechanical apparatus made the thermometer turn head over heels directly it touched the ocean bed, and discharge all the mercury above a certain level into another arm of the tube, where its height could afterwards be observed.

A Fluorescent Eye-piece for the Spectroscope.—M. Lovet has employed a spectroscope in which (says "Silliman's American Journal," August 1874) a plate of some transparent and fluorescent body is placed at the point of the observing telescope, and then viewed through the eye-piece, which is inclined at an angle. The fluorescent plate may be made of uranium glass, or of thin pieces of glass a short distance apart, containing any fluorescent liquid between them. Two lines drawn at right angles on the glass take the place of cross-hairs. It is a good plan to interpose a plate of cobalt blue glass to cut off the more brilliant portion of the spectrum. If the eye-piece is not inclined, the presence of the fluorescent plate does not prevent our observing the lines of the luminous part of the spectrum with accuracy, but the fluorescent spectrum produced by the plate is then seen but poorly. On inclining it, however, the luminous spectrum can no longer be seen, but the fluorescent spectrum then appears very clearly, of a uniform tint traversed by dark lines. These lines may be brought to coincide with the lines drawn on the glass, and their deviation measured.

Dr. Carpenter on Deep-sea Temperatures.—Dr. Carpenter, who is known to be investigating this subject, read a paper upon it at the meeting of the British Association at Belfast. He stated that while a strong surface current is flowing into the Mediterranean through the Straits of Gibraltar, a deep-sea current is flowing out into the Atlantic below it. The Mediterranean tended to grow salter in consequence of losing fresh water by evaporation. As the remaining water grew salter it also grew heavier, and so formed a lower current flowing out through the Straits, whilst lighter and fresher water from the Atlantic was flowing in above. This upper current kept the Mediterranean from growing too salt. There were similar under and upper currents in the Dardanelles. The former carried the drag along so rapidly, and drew the buoy to which it was attached so quickly against the upper current, that the sailors could not keep up with it by rowing, and would have lost it but for the aid of a steam launch. In the Atlantic the Gulf Stream was a trumpery thing, only forty miles wide between New York and England, and not very deep. It had little or nothing to do with keeping our climate warm in winter. The bottom of the Atlantic was covered to a vast depth with icy cold water, caused by the melting of Polar snows and ice. This cold water had a tendency, he believed, to surge up on certain portions of the North American coast, where it washed the shores of some of the Southern States. A broad, slow, warm current travels up the western shores of Europe and Africa; a swifter cold current from the north washes the eastern shores of North America, bringing down with it ice and icebergs from the Polar regions. The Caspian Sea was the only sea in the world where the rainfall and river supply of fresh water exactly balanced the evaporation. This was because it was a closed sea, and had dried up to the point where two things balanced each other year by year.

He entered into certain minuter details connected with these main facts of oceanic circulation.

ZOOLOGY AND COMPARATIVE ANATOMY.

Organs of Hearing in Insects.—At the last meeting of the American National Academy of Sciences, Professor A. M. Mayer exhibited experimental confirmation of the theorem of Fourier as applied by him in his propositions relating to the nature of a simple sound, and to the analysis by the ear of a composite sound into its elementary pendulum-vibrations; and to show experiments elucidating the hypothesis of audition of Helmholtz. Placing a male mosquito under the microscope, and sounding various notes of tuning-forks in the range of a sound given by a female mosquito, the various fibres of the antennæ of the male mosquito vibrated sympathetically to these sounds. The longest fibres vibrated sympathetically to the grave notes, and the short fibres vibrated sympathetically to the higher notes. The fact that the nocturnal insects have highly organized antennæ, while the diurnal ones have not, and also the fact that the anatomy of these parts of insects shows a highly developed nervous organization, lead to the highly probable inference that Prof. Mayer has here given facts which form the first sure basis of reasoning in reference to the nature of the auditory apparatus of insects.

Identity of American Hydra with European Species.—At a late meeting of the Academy of Natural Sciences of Philadelphia, Prof. Leidy made some remarks on the native *Hydra*, and described the common green and brown species. He stated that they had been regarded as distinct species from the green and brown ones of Europe, but he could perceive no difference. He described the habits of some of our *Rhizopoda* in eating *Diatomaceæ;* they absorb the chlorophyl and reject the silicious shell. *Amœba* devours *Arcella.*

Relation between Colouration of Birds and Distribution.—M. Alph. Milne-Edwards remarks, in the "Comptes Rendus," that in certain ornithological families (swans and parrots, *e.g.*) the tendency to melanism, or black plumage, only appears in the Southern Hemisphere, and more particularly in the region comprising New Zealand, Papua, Madagascar, and intermediate parts.

New Fish from the Bermuda Islands.—In "Silliman's American Journal" for August, Mr. G. Brown Goode says as follows:—"In a collection of fishes, including some seventy species, made at the Bermudas in the spring of 1872, I find two forms apparently undescribed, descriptions of which are given below. As the marine life of the Bermuda group is essentially West Indian in its character, these species may be regarded as additions to the ichthyological fauna of the West Indies." *Diapterus Lefroyi*, sp. nov., and *Engraulis Chœrostomus.*

A New Order of Hydrozoa has been discovered by Professor Allman, who has recently published a very short account of them. The animal, which was discovered in the South of France, is attached to a sponge, and permeates the spongy tissue. Although a hydrozoan, it is not a hydroid, and cannot be referred to any of the existing orders of the hydrozoa. The chitonous tubes which permeate the sponge tissue are united towards the base of the sponge, and constitute a composite colony of zooids. The tubes are increased in width towards their free extremity, and the polypite inhabiting each puts forth a retractile crown of tentacles. In many respects this new hydrozoan resembles the Campanularian zoophytes. The name of *Stephanocyphus mirabilis* has been given by the Professor to this beautiful object. In no instance was this zoophyte unaccompanied by the sponge. The new order to which it belongs has been named *Thecomedusæ*.

The Habits of Peripatus Capensis have been described in a paper presented to the Royal Society by Mr. H. N. Moseley, Naturalist to the "Challenger Expedition." He says that the species he examined was *Peripatus Capensis*, described by Grube in the Zoological Series of the "Novara Expedition." The animal has invariably seventeen pairs of ambulatory members, a pair of oral papillæ, and two pairs of horny hooked jaws, shut in by tumid lips. The specimens found varied in length from 1·6 to 7 centims. in the contracted condition. About thirty specimens were found, all of them but one at Wynberg, between Simon's Bay and Cape Town. The animals appear to be somewhat local, and not very abundant; they live in damp places under trees, and especially frequent rotten willow-wood. They feed on rotten wood. They are nocturnal in their habits. They coil themselves up spirally like *Iulus* when injured. They have a remarkable power of extension of the body, and when walking stretch to nearly twice the length they have when at rest. They can move with considerable rapidity. They walk with the body entirely supported on their feet. Their gait is not in the least like that of worms, but more like that of caterpillars. When irritated they shoot out with great suddenness from the oral papillæ a peculiarly viscid tenacious fluid, which forms a meshwork of fine threads, with viscid globules on them at intervals, the whole resembling a spider's web with the dew upon it. The fluid is ejected at any injuring body, and is probably used in defence against enemies, such as insects, which would be held powerless for some time if enveloped in its meshes. The fluid is not irritant when placed on the tongue, but slightly bitter and astringent; it is as sticky as bird-lime; flies, when they light in it, are held fast at once. The fluid is structureless, but presents an appearance of fine fibrillation when dry. The animal is best obtained dead in an extended condition by drowning it in water, which operation takes four or five hours.

Some Peculiarities of Actinophrys Sol.—Professor Leidy has made some recent observations on this subject which are not without interest. After describing the structure and habits of this curious rhizopod, Professor Leidy said that he had recently observed it in a condition which he had not seen described. He had accidentally found two individuals including between them a finely-granular rayless sphere nearly as large as the animals themselves. These measured, independent of the rays, 0·064 mm. in diameter; the included sphere 0·06 mm. He supposed that he had been so fortunate as to find two individuals of Actinophrys in conjunction with the production of an ovum. Preserving the animals for observation, on returning after an absence of three hours, the animals were observed connected by a broad isthmus including the granular sphere reduced to half its original diameter.

Two hours later the granular sphere had melted in the isthmus, leaving behind what appeared to be a large oil globule and half-a-dozen smaller ones. The isthmus in the former time measured $\frac{1}{25}$ mm., at the later time $\frac{1}{28}$ mm. Shortly afterwards, the isthmus elongated and contracted to $\frac{1}{50}$ mm. on the left, while the right half, retaining the oil globules, remained as thick as before. At the same time the animals became flattened at the opposite poles. The latter subsequently became depressed, so that the animals assumed a reniform outline. The isthmus, now more rapidly narrowed and elongated, became a mere thread, and finally separated about one hour from the last two hours indicated. The oil globules were retained in the right-hand individual, which, with the remaining projection of the isthmus, appeared broadly coniform in outline. In the left-hand individual all remains of the isthmus at once disappeared, and the animal appeared reniform in outline, but now contracting on the same side it assumed the biscuit form. The constriction rapidly increased, and in thirty minutes from the time of separation from the right-hand individual it divided into two separate animals presenting the ordinary appearance of *Actinophrys Sol.* Thus this second division took place in an opposite direction from the first. The right-hand individual, retaining the oil globules apparently unchanged, more slowly assumed the reniform outline, and then became constricted all around. The constriction elongated to an isthmus, in the centre of which were the oil globules. Three hours after the separation of the right-hand animal, the isthmus was narrowed to about half the diameter of the two new individuals which were about to be formed. At this moment other engagements obliged me to leave the examination of the animals. Six hours after, in the animalcule cage, I observed only half-a-dozen individuals of the *Actinophrys Sol.*

INDEX.

END OF VOL. XIII.

LONDON: PRINTED BY
SPOTTISWOODE AND CO., NEW-STREET SQUARE
AND PARLIAMENT STREET