THURSDAY, SEPTEMBER 15, 1870

THE MUSEUMS AND SCIENTIFIC INSTITUTIONS OF LIVERPOOL

FOR the third time since the founding of the British Association for the Advancement of Science, its annual meeting is being held in Liverpool. Those who, from former visits, are conversant with the town and its multitudinous attractions, will have every inducement to attract them thither once more; but as there are many who are paying Liverpool a visit for the first time, it may not be superfluous to direct special attention to those points which come more especially under the notice of all who are interested in scientific objects and pursuits, the more especially as during the meeting of the Association time is limited, or fully occupied during the chief portion of the day.

In Liverpool, then, first and foremost stands the Derby Free Public Museum, which is for once to belie its name by being closed to the general public during the meeting of the British Association, being only open to the members. As the reception-room occupies the central hall of this building, it will be of necessity much visited, and we have not the slightest doubt that its fine collections will be duly appreciated and admired. Visited yearly by upwards of half a million people of all classes it may be said without prejudice to other and much older country museums, to be the finest museum out of London. It took its rise in the zoological collection presented to the town by the late Earl of Derby some twenty years ago. The munificence and philanthropy of the late Sir William Brown induced him to erect the handsome building in which the Museum and Free Library are now placed, entirely at his own cost (about 35,000*l.*), on land given by the Corporation for that purpose. Since that time the museum has acquired by special gifts some extremely valuable and large collections, to be hereafter noticed more particularly, and is in daily receipt of specimens and contributions from its many admirers. Owing to the great maritime trade of Liverpool, the Museum is being constantly supplied with valuable specimens, collected by ships' officers and seamen all over the world; and as these gifts are acknowledged in the local papers each week, every inducement is held out for others to follow such good examples. The Museum, which is under the able curatorship of Mr. T. J. Moore, aims at being a Zoological Museum, and, moreover, essentially a practical and working one. For this purpose the specimens are arranged as far as possible in open table-cases, and proceed in order from the lowest animal forms, the Protozoa, through all the Invertebrata, to the Reptilia, Aves, and Mammalia.

Beginning in the room labelled Bird-room No. 1, the table-cases are filled with fine specimens of Protozoa, Corals, &c., and as the general arrangement in the cases throughout the Museum is pretty much the same, its completeness and perfection will be gathered if we describe one case more particularly in detail. We will take the large class of Spongida. In the centre of the case is placed a card about the size of a page of an octavo book, on which are legibly printed the chief characteristics, and an accurate description of the class of animals which are represented in the case. In the Spongida and many others, these descriptions are taken from Professor Huxley's work on the Classification of Animals. Around this card are arranged the mounted specimens, each accurately named and its locality given. British species are marked by distinctive labels, and the case is completed by the introduction of fossil specimens. Thus at a glance there is given an accurate description which can be soon copied out, and around it are placed the recent and fossil specimens of the class of object described. In short, upright, cases above these are arranged any more conspicuous and showy objects of the same class. The simplicity and thorough usefulness of the Museum may be easily imagined when it is understood that this system is pursued throughout, and that, in the large classes of Insecta and Crustacea, all the subclasses and distinctive genera are similarly treated.

Before leaving this room we cannot but notice a magnificent specimen of the rare coral *Isis Hippuris*, from Port Elizabeth (?), and the fine collection of the elegant Venus' flower-baskets *(Euplectella)*, in reality a sponge, from the Philippine Islands. In Bird-room No. 2 we proceed, through the classes of Polyzoa and Molluscoida and Ascidia, to the Mollusca proper. There is a fine case of *Terebratulæ*, which deserves notice, as the recent specimens are very numerous, and are contrasted with good fossil specimens. A hasty glance showed us recent specimens from New Zealand, the Moluccas, China, Philippine Islands, South Australia, America, Sandwich Islands, Japan, Sicily, North Britain, Peru, Norway, Panama, Singapore, California, the Mediterranean, &c. &c., —surely not so bad for one case of rare shells in a local museum.

In the next rooms, Bird-rooms Nos. 3 and 4, the Bivalve shells are followed by the Gasteropoda and Cephalopoda, proceeding onward to the Crustacea and Insecta, of which there are—especially in the latter class—very fine and well-arranged collections. In the collection of Mollusca and Gasteropoda is incorporated a valuable typical collection presented to the Museum in December 1869 by Mr. S. Smith. All around the three rooms we have described are arranged large upright wall-cases filled with stuffed specimens, chiefly birds, whilst in Room 5 are arranged the collection of fish and reptilian remains. There is a fine skeleton of the Dodo, from Mauritius, which was presented by Mr. Higginson, and is only excelled by the slightly more perfect ones in the British Museum and the Cambridge Anatomical Museum.

The collection of stuffed animal remains is arranged in the basement of the building, and is chiefly remarkable for the fine specimens of the Gorilla which are there exhibited. It is under process of re-arrangement, and so does not show to much advantage. The British collection is excessively poor, especially in birds, and there is every opportunity to any one who has a collection of British birds to present them to the Museum, where they will be much appreciated, and certainly properly utilised.

In a room below the basement is a small but almost unique collection, which we cannot but think will be largely visited this week. We allude to the collection of aquaria, where many of the rarest and most interesting marine and freshwater animals may be seen to great advantage. It is no slight advantage for the naturalist to study from living specimens the appearance and habits of such rare animals as the Axolotl from Mexico, and the Proteus from the caves of Adelsberg. In other aquaria are specimens of salamanders from Central Europe, the gigantic bull-frog of America, and many curious and rare American fish. There are also examples of the English lump fish, grey mullet, soles, flounders, shanny, and the rare blenny. The beautiful appearance of these fish, and their varied and in some cases gorgeous hues, cannot be imagined by those who have only seen the same fish when dead. There are also several fine tanks full of sea-anemones in very thriving condition.

We have now nearly exhausted the scientific portion of the Museum; it only remains for us to notice the poor collection of geological specimens, which looks particularly bad just at present, as it is under process of arrangement. Still it is not much at the most, and is decidedly inferior in every way to the rest of the contents of the Museum. There is, however, a small case full of fine specimens of ferns and other carboniferous plants, collected by one of the local secretaries, the Rev. H. H. Higgins, from a railway cutting about eight miles from Liverpool. We hope to direct special attention to this new bed in another column, as it is within such easy distance of Liverpool, and the remains there found are so valuable and plentiful that all geologists attending the Association should pay it a visit.

Besides the above, there is still much to be seen in the Derby Museum, for in 1867 Mr. Joseph Mayer, a wealthy Liverpool citizen, presented to the town his exceedingly valuable collection of antiquities, coins, and gems. Its value is shown by the following extract from the report of the Library and Museum Committee:—"This collection, it is no exaggeration to say, is the finest of the kind ever presented to the public. In some of its departments, those of Wedgewood ware and ivory carvings, it is unique. It contains the best collection extant of illustrations of the Liverpool pottery ware, a manufacture for which the town was once celebrated, but which has long been extinct In Egyptian and Assyrian antiquities it is very rich, particularly in gems. The Fausset collection of Anglo-Saxon remains, the finest extant, forms a portion of it, together with a large number of ancient manuscripts and illuminations."

In recognition of this valuable gift a statue has been erected to Mr. Mayer in St. George's Hall. The collection is arranged in three tiers of galleries, round a wide open space in the centre of the Museum, and communicating with each other and the rest of the Museum. We cannot afford space to give any full description of this Museum, but its varied collections of antiquities, ancient armour, ancient weapons, musical instruments, coins, gems, watches, carvings, intaglios, Wedgewood and Majolica ware, rare MSS. and Mexican hieroglyphics, will all command crowds of visitors. In the upper gallery is placed *the Collection of the Lancashire and Cheshire Historic Society*, which holds its meetings in Liverpool, and a collection of valuable antiquities from the sea-coast between Leasowe and Hoylake in Cheshire, which belong to Mr. Ecroyd Smith and others, and concerning which Mr. A. Hume, of Liverpool, recently wrote a long and illustrated description in his "Ancient Meols"—the name of an old Anglo-Saxon and English settlement supposed to have been submerged by the sea.

The next place which will claim the most attention in Liverpool, after the Derby Museum, is the *Bidston Observatory*. This is reached by taking the ferry boat to Seacombe, and thence either omnibus or cab to the Observatory, about two miles distant. It stands in a fine situation, commanding most extensive views, and is about 200 feet above the level of the sea. Its great attraction, beside the fine equatorial telescope it contains, are its ingenious self-registering meteorological instruments. Of these the first to see is King's self-registering barometer, where, by means of an ingenious arrangement, the hourly movements of the barometer are self-recorded. There are only three such instruments in England—this one, one in Birmingham, and one in Mr. Crossley's private observatory in Yorkshire; and they are stated to be superior to the ordinary photographic method employed elsewhere. In an upper part of the building is an instrument whereby, on the same sheet of paper, four different set of observations are self-registered. These are, the velocity of the wind, its pressure and its direction, and the amount of rain-fall. The instrument only requires to be attended to once a day, and then acts twenty-four hours without intermission. When we visited it on Saturday the pressure of the wind had reached as much as 65 lbs. per square foot, and its velocity at that time was about seventy-one miles per hour. The Observatory is open to all members of the Association on production of their tickets, and Mr. Hartnup, its director, will have great pleasure in showing and explaining all the instruments. We must not omit to notice a very ingenious and beautiful chronograph for recording transits by means of a most delicate piece of mechanism acting by electricity, and literally writing its own observations.

Returning to Liverpool, there are one or two more museums which deserve attention. The first of these is the *Museum of the Royal Institution*, in Colquitt-street. Before the opening of the Derby Museum, this was the principal museum in Liverpool, but it is now left comparatively in the shade. It is essentially a natural history museum, but the specimens are not well stuffed, and, together with the rooms, which are badly lighted up, have a very dingy and dirty appearance. There is, however, a fine collection of birds ranged round the walls of the two best rooms, the centre of which is occupied by a large collection of shells, neatly arranged and displayed, and well named, of which the chief specimens came from the collection presented to the museum by Mr. George Green, of Liverpool. The mineralogical collection, which wants re-arranging and classifying, has lately received large additions from the collection of the late Dr. James Stewart Trail, of Edinburgh, lately presented to it. There is a geological collection arranged round an upper gallery, but of which nothing commendable can be said. The materials are there for a good typical collection, and there are a few good American collections which are interesting; but they all sadly want naming and properly arranging, localities given, &c.

Next to this is an Economic Museum, which is worth visiting, arranged somewhat on the system employed at South Kensington. Besides these, there is a herbarium, a fair collection of butterflies and beetles and a collection of fungi from the Liverpool district, and a good collection of zoophytes from Bootle, near Liverpool. There is also a small collection of ethnographical objects, weapons, Indian remains, flint axes, &c., and the usual miscellaneous objects of a local museum. There is a large stuffed specimen of the sea elephant from the South Shetland Islands, which is valuable, as the species is yearly becoming much rarer. It seems a pity that some arrangement could not be made between the two museums, so that this one could be lightened of some of its heterogeneous contents, and more room given for a better display of some special collections. Besides this, those who are interested in mineralogy should not fail to visit the *Medical Institution*, in Hope-street, where Phillips's mineral collection is well exhibited. There is also the *Museum of the Royal Infirmary School of Medicine*, which is more likely to please those interested in medical pursuits than the general public.

To those botanically inclined, the extensive *Botanical Gardens in Edge Lane* will be very attractive. They cover an extent of about eleven or twelve acres, and are very tastefully laid out, and the large conservatories contain many fine and choice exotics.

Those who are interested in the practical application of science to the requirements of the present day, should not fail to visit *the Liverpool Sewage Utilisation Works at Sandhills*. These are open to all members of the Association, and will be found to afford much practical knowledge on a subject that is yearly attracting more attention.

The Liverpool scientific societies, though they have existed many years, have failed to attract very much attention, or to become prominently known. The most important of these is *the Lancashire and Cheshire Historic Society*, which was started about 1847, as a purely antiquarian society in all matters relating to the two counties. It has published a set of transactions containing much valuable information; but of later years it has enlarged its scope and taken in other branches of scientific knowledge, with a result, we fear, not commensurate with the wishes of its promoters. *The Liverpool Philosophical Society*, and *the Liverpool Geological Society*, unlike the similar societies in Manchester and Leeds, are but little known outside the city in which they hold their meetings.

Soon after Owens College was founded in Manchester, a somewhat similar college, called *Queen's College*, was started in Liverpool; but whether owing to lack of energy or good management on the part of its directors, or from the overpowering influence of the old-established Collegiate Institution, it has certainly failed in approximating in any direction to the national importance and value that the former college has obtained.

In some degree compensating for these failures, Liverpool possesses a very extensive and much-patronised *Field Naturalists' Club*, which does a great deal by weekly excursions to infuse into its members a taste for natural science and out-of-door scientific work, and there is little doubt but that from all its members the British Association will receive a hearty welcome; and let us hope that from this year's meeting may date an increased impetus to Science in the neighbourhood.

J. P. E.

NOTES ON THE GEOLOGY OF THE COUNTRY AROUND LIVERPOOL

THE following brief notes on some of the points of geological interest may perhaps be of use to geologists from the South of England visiting the British Association's Meeting at Liverpool.

The tract of country lying between the rivers Dee and Mersey, known as the peninsula of Wirral, is composed of Triassic rocks, forming a series of undulating ridges and valleys, running parallel with the strike of the rocks. The average elevation of the crest of the hills is about 150 feet, and they, as well as the valleys, are more or less covered with glacial drifts. The northward prolongation of the hills and valleys is abruptly terminated by a broad plain, but little raised above the sea-level, which forms the seaward portion of the Hundred of Wirral. This plain, which is composed of peat partly covered with alluvium, is drained by the River Birket, which flows eastward along its whole length, from a little south of Hoylake, until it falls into Wallasey Pool, an arm of the River Mersey, which separates the tract of comparatively high land known as Wallasey (anciently an island) from the Triassic hills south of the Pool.

Crossing the Mersey, the town of Liverpool is found to rest on a continuation of the Triassic hills south of the river, but more deeply covered with drift, which has in most cases so entirely filled up the valleys that the whole of the Coal-measure and Triassic districts of south-west Lancashire are one vast plain, surrounding a central nucleus formed by the spurs of the Pennine Chain, composed of the Millstone Grit.

The latter formation, on the borders of the Lancashire coal-field, reaches an average thickness of 5,000 feet, and is composed of four great beds of grit, divided from each other by thick beds of shale. These beds are known as the *Rough Rock*, or first grit; the *Haslingden Flags*, or second grit; the third grit; and the *Kinder Scout*, or fourth grit. The first and fourth grits are generally coarse, conglomerative, and massive; the third, as a rule, finer and not conglomeratic—both it and the fourth grit are often divided into two, three, and even more beds of thick seams of shale, which thin, wedge out, and thicken in the most irregular and local manner. In fact, though the Millstone Grit, as a whole, maintains a general average thickness in any special area, its members in *detail* appear to be ever changing places in relative consequence with each other, proving the shallowness of the sea, the proximity of land, and the existence of currents laden with different materials, sand and pebbles washed from those deep-seated quartzites which raised their heads above the sea at the time of the deposition of the grit in the area now occupied by the Pennine Chain.* Workable coal-seams occur in the first and third grits, and thin seams in the fourth; associated with them are shales in which a flora and fauna of about 30 species occur, the same species recurring in the different seams of shale: 18 species occur in the shales of the "Feather-edge" coal† (in the Rough Rock); amongst them are *Calamites Suckowii*, and *Pecopteris arborescens*. The former species I recently found in some shales of the second grit, exposed

* Prof. E. Hull, F.R.S., Quart. Journ. Geol. Soc, August, 1868.

† This coal is described by Mr. E. W. Binney, F.R.S., in the Trans. Geol. Soc. Man. vol. i.; by Prof. E. Hull, F.R.S., in Geol. Surv. Mem. "On the Geology of Oldham," and in the "Geology of Bolton-le-Moors."

in the new excavations for the Liverpool Waterworks' upper reservoir, above Alance Bridge, north of Rivington. A little further west another bed of black shale occurs, apparently on the same horizon as that from which the Geological Survey Map (six-inch map, Lancashire, sheet 78) records the presence of *Goniatites*. These and other fossils also found in the shales forming the roof of two coal-seams, occurring between the second and third grits. The shales dividing the latter grit are also fossiliferous in two instances in Lancashire, and *Goniatites* may be found in the shales between the third and fourth grits, at Old Kates Dingle and Shore Brook, below Noon Hill, east of Rivington (reached from Adlington Station). They reach, I should say, a thickness of 600 feet, and contain two thin coal-seams. The Kinderscout grit, though nearly 1,000 feet thick, appears to be almost devoid of organic remains; but the occasional fragments of *Stigmaria* testify to the existence of land during the period of its deposit, as do also the thin seams of coal.

The Coal Measures.—These are so well known through the various survey memoirs of Professor Hull,* and papers of Mr. E. W. Binney, F.R.S., that it is needless for me to describe them. They are divided into three divisions: the upper is devoid of coal, and is absent in South-west Lancashire; the middle coal-measures contain all the valuable coals, the best being that at the base, known as the Arley Mine, which is perhaps equal to any coal in England. The sixth seam, above the Arley, is the celebrated Wigan "cannel coal:" it is three feet thick at that town, thinning out in every direction, with Wigan as a centre, as shown by Mr. Hull.

The Lower Coal Measures, or Gannister beds, from their containing siliceous concretions, locally called "Gannisters," have five or six workable coals which are known as Mountain Mines. Coal-measures occur at Neston on the west coast of Cheshire, and again at Croxteth Park, near Liverpool, and it is probable that they underlie the whole of the Triassic rocks of Wirral, and of part of Liverpool itself.

Permian.—No geologist should leave Liverpool without visiting the two Permian outliers, discovered by Professor Hull, at Skillaw Clough and in Bentley Brook, Bispham (north-east of Ormskirk); they consist of sandstones, marls, and magnesian limestone.†

Trias, or New Red Sandstone.—This is divided into the following subdivisions:—

Keuper Series	Grey and Red Marls	1,000	feet
	Lower Keuper Sandstone	400	"
Bunter Series	Upper Mottled Sandstone	500	"
	Pebble Beds	800	"
	Lower Mottled	100	"

The *Lower Mottled Sandstone* is best seen in the Liverpool district, at Eastham, on the Cheshire side of the Mersey, where it forms a cliff capped by the *Pebble Beds*. The latter are well seen in the quarries at Everton, above Liverpool; they are generally stained a deep brick-red with peroxide of iron, and contain seams of quartz-pebbles running along the lines of current-bedding seams of grey and red marl, which also occurs in small pockets in the rock. The *Upper Mottled Sandstone* is, as a rule, pebbleless, much false-bedded, streaked and mottled in its middle portion, yellow above, and deep bright red below. The latter beds are well seen at the mouth of Bromborough Pool, near Birkenhead; the middle beds at Ormskirk, where the celebrated section, first described by Mr. Hull,* occurs, where nearly level beds of conglomeratic Lower Keuper Sandstone rest on the denuded upturned edges of the variegated beds of the Upper Mottled Sandstone: it is exposed in the railway cutting leading towards St. Helen's, a little east of the town.

Lower Keuper Sandstone.—The base of this sandstone in this as in other districts, is extremely pebbly, and consists of hard grit, not unlike some of the beds of the millstone grits, and like them, and the sands and gravels of the Middle Drift Period, is much false-bedded. The middle portion consists of fine-grained freestones, separated by thin seams of grey marl, supporting water: these are well seen in the railway cutting at Orrel (east of Waterloo),† first described by the writer in the Survey Memoir on the district. The Labyrinthodon bed (3 to 4 feet thick) occurs near the base of this part of the Keuper Sandstone: it is best seen at Storeton, near the windmill (on the Cheshire side of the river): many fine footprints may be seen in the Liverpool museums. In the Orrel Railway cutting, magnesia is found to occur in the shale, as well as pseudomorphous crystals of salt, which also occur in the shales at the top of the sandstone, or at the base of the marls, whichever way they may be taken; for the sandstones, shales and marls, are in reality only one series gradually passing from the one into the other, as the sea grew shallower and shallower, and became supersaturated with salt, until at length the sea became a salt lake. From this sequence there is, however, one exception in this district, which I have not noticed elsewhere. A conglomeratic bed occurs near the very top of the Keuper Sandstone, immediately below the horizon of the shales; the pebbles consist of quartz, and are apparently derived from the same source as those occurring in some of the millstone-grit beds. The existence of round pebbles in a deposit proves either the proximity of a coast line or the shallowness of the water at the period of deposition; for unless the water is shallow, currents, I know by experience, have not the power to move pebbles. The Trias, as a whole, appears to have been formed during a period in which subsidence hardly kept pace with the deposition of sands and clays brought down by rivers from continental lands. This upper pebble bed would appear to have been thrown down at a moment when the movement of subsidence was greater than usual, causing islands of quartzites, or possibly of millstone grit, containing quartz-pebbles.

Keuper Marls.—This division attains an immense thickness in the country between Liverpool and Southport; but is so deeply covered with drift, glacial and post-glacial, that sections are very rare. Much of it, like the northern end of Cheshire, is scarcely above high-water mark, forming a low-level plain covered with peat-moss; between it and the sea intervenes a tract of blown sand, forming dunes or "hoes" as they are locally called, which is traversed by the railway from Liverpool to Southport.

* Mem. Geol. Surv., "Geology of Oldham," "Geology of Wigan," "Geology of Bolton-le-Moors," "Geology of Prescot."
† See "Geology of Wigan," p. 27, and Geol. Surv. Map, 89, S.W.

* Mem. Geol. Surv. "Geology of Wigan." By E. Hull, F R S.
† Mem. Geol. Surv., "Description of 90 S.E.," and "Geology of the country between Liverpool and Southport," By C. E, De Rance, F.G.S.

Looking seaward from the train, the eye can descry nothing but range behind range of dunes, the only variety being produced by the irregularity of their heights; the only change from the ceaseless monotony of their yellow slope, the dark green stripes of dwarf willows that serve still more strongly to bring out the sterility o the scene. Leaving the train at Ainsdale, or Hightown, and examining the Lancashire Sahara more closely, it is found to consist of three portions :—a range or series of ranges of sand hills, from one to two miles in breadth. sloping down to the peat-moss,—a central plain,—and a range of sand-hills between the plain and the sea, protecting the former from the latter. Their incoherent masses would, however, be of little avail, were it not for the matted roots of the Sand-reed (*Ammophila arundinacea*), locally called "starr-grass," and woven by the people into mats and other articles, and which grass they were unable, until lately, by an old Act of Parliament, to cut or destroy, under the most severe penalties. In the plain, or rather in the series of small oval plains divided from each other by little ridges, running from the sea to the land, there is, in the summer, a dense carpet of spongy moss, mixed with sedges, and sprinkled with flowers. The great quantity of lime constantly set free by the dissolving of marine shells in the sand, causes many of the plants to be of species generally found on a chalk soil. Here occur various plants belonging to the Gentian tribe, as the Perfoliate Yellow-wort (*Chlora perfoliata*), the Red Centaury (*Erythræa Centaurium*, *E. pulchella*, and *E. latifolia*). *Gentiana Amarella*, with its purplish-blue flowers, will be also found about this time. On the adjoining moss-land occurs the rare marsh-gentia, *Gentiana Pneumonanthe*, with its large blue bell with five green stripes, With it grows the Buckbean (*Menyanthes trifoliata*). In the "slacks" (the local name for the little oval plains in the sand-hills), the beautiful *Pyrola media* occurs in great abundance, as does also the Grass of Parnassus (*Parnassia palustris*.) Each of these slacks has a distinguishing name, as "Bull-rush," "Long," "Mayflower," "Round," and "Dale Slacks :" these in winter receive the drainage of the sand hills, which, being stopped by the carpet of vegetation, forms a series of large standing pools of water, in the midst of apparently porous sand.

To return to the Keuper Marl: it is a series of red, green, and grey marls, with occasional seams of freestone, much ripple-marked, and beds of shale, generally with pseudo-morphous crystals of salt, and often veins of fibrous gypsum. Through denudation, the top of it is never seen, and it is therefore, with the exception of the Drift, the newest formation in the Liverpool district.

Glacial Drift.— Professor E. Hull, F.R.S., proved (in a paper read at Manchester, in 1862) that the Glacial Drift in the Manchester district was capable of division into an Upper and Lower Boulder Clay, divided by a Middle Sand and Gravel, which he called the "Middle Drift." The writer, in a paper on "Glacial Phenomena of Western Lancashire and Cheshire," read at the last meeting of the Geological Society, attempted to prove that this classification holds good, not only in the whole of Western Lancashire, but from the River Dee to the flanks of the Cumberland and Westmoreland Mountains; and since writing that paper he has found that the terrace of Glacial Drift skirting the mountains of North Wales, lying between them and the sea, is capable of that division, the cliffs of boulder clay east of Llandudno (round the Little Orme's Head) being distinctly divided by a *Middle Sand*, containing the same species of shells as those occurring in the Lancashire Middle Drift.

The *Lower Boulder Clay* (the writer endeavoured to show, in the paper alluded to above) was formed by an ice-sheet, which covered nearly the whole country down to a level of about 150 feet to 200 above the present sea-level; this clay he termed the "High-level Low Boulder Clay." Below a level of 100 feet, the clay appears to have been formed by the summer melting of an "ice-foot," which surrounded the sea-margin—at that level the land, through subsidence, standing that amount lower than at present. An amelioration of climate then took place during which the sands and gravels of the *Middle Drift*, with shells of Celtic type, were deposited round the edges of higher, and still higher, successive coast-lines, as the land gradually sank, until the sand and gravel, at Macclesfield, more than 1,200 feet above the present sea-level, was deposited in water of the *same depth*, and containing the *same shells*, as that in which the middle drift of Blackpool, only fifty feet above the sea was deposited. The phenomena exhibited by the middle drift, of the invariable rise from the sea to the land, in an inclined plane—the undulating surface, now far below, now up to, but never above, that plane—can only be explained by the theory that it was formed as *sand-banks* in shallow water on a gradually subsiding tract; and the *Upper Boulder Clay* is a marine deposit, formed of the detritus brought down by glaciers in the valleys of the Cumberland Lake district, to the ice-foot, which melting carried its spoils over the sea-covered plains. All these divisions are more or less well seen in the Liverpool district, especially on the Cheshire side of the river, in the neighbourhood of Egremont and Eastham. Further south, north of Chester, the Middle Drift is particularly well developed. Most of the boulder clay in North Cheshire belongs to the upper division, the lower clay being absent, having been denuded away. Glacial striæ were discovered by Mr. Morton F.G.S., at Flaybrick Hill, the direction being N. 30° W. at an elevation of 120 feet above the sea-level; and at Toxteth Park, the direction being N. 42° W.; also by Mr. Hull, F.R.S., at Kirkdale, the direction N. 15° W., caused probably by icebergs during the Upper Boulder Clay period.

In the district between Liverpool and Southport a bed of sand occurs, forming a line of old sand hills at the inland edge of the peat-moss plain, and making a sort of step between it and the comparatively high-level (80 to 160 feet) boulder-clay plain above. One of these hillocks is called Shirdley Hill; I therefore called the sand the "Shirdley Hill sand."* It is about 30 feet thick, and underlies the later deposits of the peat plain, but rests itself on a thin deposit of what I called the "Lower Peat." Above this sand, and beneath the great bed of peat (Upper Peat), is a bed of grey clay containing freshwater shells, which I called the "Lower *Cyclus* clay." I have observed it from the Island of Walney, in North Lancashire, to the coast of North Wales, and believe it, as well as the peat above it,

* "Description of Geol. Surg. Map, 90 S E." and "Post-Glacial Deposits of Western Lancashire and Cheshire," read at last meeting of the Geol Soc

occurs at the bottom of the greater part of the Irish Sea. All along the coast of Lancashire, Cheshire, and to a certain extent North Wales, the peat, with a forest at its base and the clay containing the roots of the trees, may be seen, nowhere so well as at Leasowe, in Cheshire, and at the mouth of the Alt, Hightown, Lancashire (eight or nine miles from Liverpool). In both localities the peat, the forest at the base, and the grey clay below, occupy the country inland, run *under* the sand dunes, emerge on the coast, and disappear under the sea sand at low-water mark. In North Cheshire the peat is sometimes split into two, a bed of grey clay, with *Scrobicularia piperata*, being intercalated in the mass. This I consider to have been formed when the Mersey entered the sea, through what is now the gorge of Wallesey Pool. Very near the top of the peat a thin seam of sand occurs, both in Cheshire and Lancashire, containing *Tellina Balthica* and *Cardium edule*. I called it the zone of *T. Balthica*. The grey clay of the Isle of Man, with *Cervus Megaceros*, is no doubt of the same age as the "Cyclus Clay" of Lancashire.

Those who visit Hightown will find the peat, which is there from twelve to twenty feet thick, covered on either side of the River Alt with an alluvium which, near the sea, contains *Scrobiculariæ*, and inland graduates into a fluviatile deposit, with freshwater shells. They will find the base of the sand dunes, where they rest on the upper surface of the peat, to be, as at Leasowe, in North Cheshire, a freshwater deposit, which I called the *Bythinia tentaculata* sand; it contains thin seams of peat up to a height of eight or ten feet from the base.

My notes are already so long that I abstain from saying anything of the marine fauna of the coast and other matters: those that I have written refer to districts in which the maps, &c., of the Geological Survey are already published, and I have of course written as a private geologist, stating my individual opinions.

C. E. DE RANCE

H.M. Geological Survey

THE SECOND PROVINCIAL MEETING OF THE IRON AND STEEL INSTITUTE

THE Institute bearing the above name was originated about two years ago, chiefly by the North of England ironmasters, among whom there are many gentlemen who combine in themselves great practical skill and a large amount of scientific knowledge. It was not formally launched into existence till the month of June, 1869, when the inaugural address was delivered to a meeting of the members, held in the Hall of the Society of Arts, by the president, the Duke of Devonshire, who is intimately and extensively connected with the iron and steel trades through the great works of Barrow-in-Furness, perhaps the greatest Bessemer steel works in the world. The aim of the Institute is to hold two meetings annually—one in London, in the spring, and the other in the country in the autumn. The first provincial meeting was held, about a year ago, in Middlesborough, the capital of the Cleveland district, the greatest and most scientific iron-making district either in this or any other country. In May last the second metropolitan meeting was held; and now the second provincial meeting has just been held at Merthyr Tydvil, in South Wales. At all these three meetings there have been read papers of very great interest and importance, both from a scientific and a practical point of view. Then, taking the experience of the two provincial meetings, the members not only have the benefit of hearing the papers read and of taking part in the discussions to which they give rise, but they also have the opportunity of visiting the numerous works that are thrown open for their inspection, where they can see scientific theories put to practical tests, and where they can compare notes with each other upon the subjects which deeply concern them as practical and professional men. It is well known that "iron sharpeneth iron:" and in these country meetings of the Iron and Steel Institute this wise saw has many apt illustrations. Examples to imitate are seen in abundance; many points are observed that are suggestive and that excite to further improvements in other hands; and in numerous instances things are seen which impart lessons of a totally different sort, inasmuch as they show what errors of commission are to be avoided. Both successes and failures can give instruction to thoughtful minds.

This year's provincial meeting of the Institute, as already indicated, has been held at Merthyr. It opened on the morning of Tuesday, 6th September, in the Temperance Hall of that town, the centre and most important seat of the iron trade of South Wales. After the transaction of some formal business, and the appointment of Mr. Henry Bessemer as the president-elect of the Institute, the business of reading and discussing the papers prepared for the meeting was proceeded with. Altogether there were seven papers set down for reading and discussion on the mornings of Tuesday and Wednesday, the 6th and 7th September, which was all the time that could be devoted to that work, as the afternoons were required for visiting and inspecting the works in and immediately around Merthyr, while Thursday and Friday were required for the inspection of works at a distance. One praiseworthy feature in connection with the meetings in Merthyr was the circumstance of printed copies of the papers being in the hands of the members before they were read by the authors. Owing to this arrangement members were generally enabled to come prepared to enter upon the discussion of the papers with intelligence and with some degree of satisfaction. In two instances the papers supplied to the members were accompanied by engravings of the objects described. Both of these features of the Iron and Steel Institute Meeting might be copied with advantage by other learned societies, as they are calculated to impart additional interest to the meetings.

The Iron and Steel Institute already numbers upwards of 350 members, including several peers of the realm, about a dozen members of Parliament, and almost every person in the kingdom who has of late years added to the stock of our knowledge regarding the manufacture, the manipulation, and the use of iron and steel. It has very soon acquired a national importance, and its future prosperity seems to be almost assured. In order to make the Institute more and more useful to the persons who may be connected with it as members, it is intended to publish a Quarterly Journal of the Institute instead of the Transactions. This will commence on the 1st of January, 1871, and its contents will be as follow:—

First, the proceedings of the Institute, and of the Council from time to time. Second, the papers and discussions at the general meetings of the Institute. Third, communications from members upon matters of special interest to the trade, which are approved by the Council. Fourth, a quarterly epitome of inventions, discoveries, publications, and proceedings bearing upon the British iron and steel trades. Fifth, a comprehensive report on matters connected with the iron and steel trades in foreign countries. It is very satisfactory to know that the foreign department of the journal will be under the special management of Mr. David Forbes, F.R.S., and that the general editorship will be conducted by Mr. John Jones, F.G.S., the Secretary to the Institute and to the North of England Iron Trade.

We now proceed to give a brief notice of the papers read at the recent meeting at Merthyr:—

I. "On the Geological Features of the South Wales Coal-Field." By Mr. William Adams, Cardiff. In this paper, which was of special interest to such of the members as were strangers to the district, the author traced the history of our knowledge regarding the position, nature, and extent of the coal deposits which have given such an industrial importance to South Wales. Attention was directed to this coal-field as far back as the year 1570, but the essay itself did not appear till the year 1796, when it was published in the *Cambrian Register*. Since that time many persons have devoted attention to the subject, including Edward Llwyd (1697, "Philosophical Transactions," 1712); Edward Martin (Royal Society, 22nd May, 1806); Buckland and Conybeare (1822, "Geological Transactions," vol. i.); Robert Bakewell (1833); Sir Henry J. De la Bêche (1846, "Memoirs of the Geological Survey," vol. i.); Mr. Hussey Vivian, M.P. (1860); and Mr. Hull (1861, "Coal Fields of Great Britain"). According to Mr. Adams, the South Wales coal-field extends from Blaenavon and Pont-y-pool, on the east, to St. Bride's Bay, in Pembrokeshire, on the west, a distance of about ninety miles; while its breadth varies from about sixteen miles on the east to about four miles in the extreme west. The superficial area is given as low as 906 square miles (Hull), and as high as 1,200 square miles; but Mr. Adams puts it at 937½ square miles, or 600,000 acres. Taking the average thickness at sixty feet, as given by Mr. Vivian, M.P., one of the Royal Commissioners on the Coal Supply, and deducting one-third for loss in working, for faults, waste, &c., the extent of coal is 36,000,000,000 tons. The author pointed out the qualities of the mineral in different parts of the coal-field. On the east it is bituminous, and makes a very superior coke; it continues so to Rhymney; at Dowlais it becomes free-burning; at Cyfarthfa it begins to take on the characters of anthracite; and further westward the coal becomes more and more distinctly anthracitic, until, in the west, it becomes so pure that it is worked for drying hops and malt and for distilling purposes, &c. Within the field there are obtained the well-known steam coals of Merthyr, and the Aberdare and Rhondda valleys. Argillaceous ironstones are very abundant and good. They are interstratified with the coal, and have an aggregate thickness of from sixty to seventy inches or upwards, and until about thirty years ago they yielded practically all the iron which was smelted in Wales. Since then the deficiency for the ironworks has been drawn from other British and foreign mines. The pig iron made in South Wales in 1854, according to Mr. Hunt's "Mineral Statistics," was 750,000 tons, and the coal raised in the same year was 8,500,000 tons, while the amount raised in 1868 was 13,210,000 tons. Owing to the remarkable denudation in the valleys of the district, the lowest seams of coal can be won by pits of less than 1,000 yards in depth throughout about two-thirds of the basin. A greater depth may be required west of the Vale of Neath and on to Llanelly, where the deepest part of the basin occurs. Throughout the coal-field there are faults, running north-west and south-east, which give a vertical displacement of 250 or 300 yards; and there are others running east and west which give a displacement of from 400 to 500 yards; and one is said to occur in Pembrokeshire which is equal to a displacement of upwards of 666 yards. The deepest pits are those which reach down to the nine-feet seam; their depth varies from 304 to 435 yards. In the coal-field there is found some of the best fire-clay known; there is also a remarkable siliceous stone, which is used to make the well-known refractory fire-bricks called Dinas brick. Alum shales occur, and, near Pont-y-pool, a rich oil shale is found, which yields on distillation from fifty to fifty-five gallons of crude oil, of which twelve to fifteen per cent. may be separated as mineral turpentine. Mr. Adams also referred to the extent and distribution of the mountain limestone—so useful as a flux in smelting the ironstone—and in the fossils which the coal measures of the district contain; and, in concluding his very valuable paper, he also strongly urged the desirability of a new geological survey on the six-inch scale being made.

In the course of a short discussion which followed the reading of the paper, several interesting points were raised, more especially in reference to the effects of the faults in changing the character of the coal.

II. "On Pumping and Winding Machinery." By Mr. G. C. Pearce, Cyfarthfa Iron Works. This paper embraced a short description of the pumping and winding machinery lately erected at the Castle Pit, near Merthyr, the property of Mr. R. T. Crawshay. Notwithstanding the shortness of the paper, it would be difficult to give a satisfactory description of the machinery in the form of an abstract, especially without illustrations. But a few facts may be mentioned. The pit is 333 yards in depth to the bottom of the sump, but the pumps only raise the water 279 yards, where there is an adit. It is of an oval form, 22 feet 8 inches long by 12 feet wide, with a brattice dividing the winding parts from the pumps; and it is all built with brick throughout. The total cost of the machinery and the sinking of the pit was about 30,000*l.*, the winding engine alone costing above 4,000*l.*

In the afternoon of Tuesday the Castle Pit was visited by the members of the Institute, many of whom were much struck with the gigantic and ingenious character of the machinery erected by Mr. Pearce.

Mr. E. A. Cowper, C.E., F.R.S., followed with some observations in reference to the working of steam for pumping, winding, and blowing engines; but no discussion ensued.

III. "On the Condition of Carbon and Silicon in Iron and Steel." By Mr. George J. Snelus, Associate of the Royal School of Mines, chemist at Dowlais Iron Works. This paper was of considerable length and very elaborate. It contained the results of a long course of experimental inquiry, instituted with a view to determine the conditions in which the two non-metallic bodies, carbon and silicon, exist in iron and steel. Dr. Percy had said in his celebrated work, "Iron and Steel," that not a trace of graphite could be detached by the point of a penknife from the fractured surface of highly graphitic iron; but Mr. Snelus had proved the incorrectness of this statement by examining some pig iron which had cooled slowly under a mass of slag, and which had in consequence very large crystals. From the surfaces of these crystals the graphite could not only be separated with the point of a penknife, but even with the finger-nail; and when the graphite was removed the iron underneath rapidly rusted in a damp atmosphere. The same thing was afterwards observed with the fractured surface of Bessemer pig-iron, and the scales removed were found on further examination to be pure carbon. By pulverising pig-iron and then using the magnet a considerable amount of graphite was separated. Other mechanical means were employed, and the same results were obtained. In spiegeleisen the carbon was found to be almost wholly combined. The author had never found as much as five per cent. of combined carbon in pig iron, although many analyses had been published in which the carbon was put down at even six per cent. Mr. Snelus was not inclined to believe that there was any definite compound of carbon and iron but, he thought that the carbon was dissolved in the iron, the amount taken up by the molten iron varying according to several circumstances. According to Mr. Snelus there is no pig iron that is destitute of silicon, and he had never met with a case in which either steel or wrought iron was totally free from it. Good Bessemer and tool steel rarely contains more than two or three parts in 10,000. One part of silicon in 1,000 of Bessemer steel renders it hard and brittle when cold. In ordinary Bessemer pig iron it is present in quantities varying from one to four per cent. The author gave it as his opinion, from experimental inquiries, that silicon is dissolved or "occluded" in iron in the same way as carbon is, but that the solvent power of the metal is so much greater for silicon than for carbon, that it is quite a rare thing, even if it ever occurs, for silicon to separate in a free state from the iron; and he considered that the methods of mechanical separation which he had adopted for the investigation of the condition of carbon and silicon would prove effectual aids to the ultimate analysis of iron, and a valuable supplement to the ordinary methods of research.

Mr. Isaac Lowthian Bell complimented Mr. Snelus upon the great value of his paper, and criticised some of the statements made in it. On the presence of silicon in steel, Mr. Bessemer said that a very general opinion prevailed in the trade (and it was correct when they spoke of large quantities) that silicon was deleterious to the make of iron. It was the impression that they should get rid of silicon, and they would then make better iron. That was not the case, in proof of which assertion he called attention to the old steel process as carried on in Sheffield, and adduced instances to show that the presence of silicon improved the quality of the manufacture. He had not had an analysis of the best quality of Sheffield steel that did not contain silicon.

A discussion was then taken upon a paper read at the meeting held in London in May last, the subject of which was "A Method of Designing Rails," and the afternoon was spent in visiting the works of the Plymouth Iron Company, the Castle Pit winding and pumping machinery, and the Cyfarthfa Iron Works, the property of Mr. Crawshay. At the last-named works there was seen an immense stock of puddled iron, upwards of 20,000 tons, stacked in "houses," each containing 300 tons.

The proceedings were resumed on Wednesday morning. The papers read were the following:—

I. "On a New Form of Pyrometer." By Mr. C. W. Siemens, C.E. F.R.S. D.C.L. After describing briefly the Wedgewood and Gauntlett pyrometers, and one previously constructed by himself, Mr. Siemens proceeded to describe a pyrometer of more universal applicability. It is based upon the peculiar properties of the pure metals to offer an increasing resistance to the passage of an electrical current with increase of temperature. A platinum wire of known electrical resistance is wound upon a cylinder of fire-clay, upon which a helical path has previously been cut to prevent contact between the turns of the wire. The coil of wires, so prepared, is enclosed within a cylindrical casing of platinum if the temperatures to be measured exceed the welding heat; or of iron or copper, if lower temperatures only require to be measured. The two ends of the coil of wire are brought out end ways, and are attached within the protecting tube to thicker leading wires of copper, insulated for a short distance by being passed through pipe-clay tubes, and further on by india rubber or gutta percha, terminating at the measuring instrument, which may be placed at any convenient distance. The characteristic feature of this instrument is that the usual calculations necessary to determine electrical resistances by the Wheatstone or other methods are dispensed with, and a reading in degrees of a large scale is at once obtained by so placing the index lever that the electrical current, generated in a small battery and passed through the measuring instrument, including the platinum wire of the pyrometer, produces no deflection of the galvanometer needle. The temperature which these degrees represent is expressed by a table of reference, which accompanies each instrument. The correctness of this instrument depends solely on the ratio of increase of electrical resistance in the platinum wire, with increase of temperature. This rise is considerable, the resistance being increased fourfold by an increase of temperature from the freezing point to about 3,000° Fahr. The ratio of increase is, however, not uniform, but follows a parabolic law which the author embodied in a table. The pyrometer last described is the result of more careful investigation on the part of Mr. Siemens, who has been animated by a desire to fill up a blank in the means at our disposal to carry on metallurgical inquiries with a high degree of certainty, and he therefore does not seek any commercial compensation, through the Patent Office or otherwise, for using this invention.

A discussion followed, in which Mr. Snelus, Mr. I. L. Bell, and Mr. E. A. Cowper took part.

II. "On the Efficiency and Durability of Plain Cylindrical Boilers." By Mr. Jeremiah Head, Middlesborough. The author of this paper aimed at pointing out the many high qualities possessed by the plain cylindrical boiler, to investigate the cause and extent of the defects which they possess, and to point out satisfactory remedies for them. Such boilers are still in large request. Out of 17,825 boilers now on the books of the various boiler insurance companies 4,052, or 22·7 per cent., are of this type. But such boilers are very liable to fracture and break their backs, and Mr. Head gave an explanation of the causes of such occurrences, and explained the method which he had adopted to prevent the recurrence of such a mishap in a boiler that had been repaired about the beginning of March last. His plan is to suspend the boiler upon volute springs. No springs are necessary for boilers of 30 feet long upon two supports; but for lengths of 60 to 75 feet five supports are required, the end ones being furnished with double springs.

The discussion following this paper was so long that no time was left for the consideration of a paper by Mr. Kohn, C.E., on "The Production of Alloys of Iron and Manganese, and on their Application to the Manufacture of Steel." A paper on "A Contribution to the History of the Puddling Process" was also left over for consideration at the next meeting in London. On that occasion the place will be selected for the next autumn meeting.

In the afternoon of Wednesday, the world-famous works of the Dowlais Company were visited by the members of the Institute. Thursday was occupied with a visit to Swansea, where an inspection was made of the Landore Steel Works, where the Siemens-Martin process is carried on; the Hafod Copper Works, belonging to Mr. Vivian, M.P., and the Spelter Works. Friday was devoted to a visit to the Crumlin Viaduct and the Ebbw Vale Company's Iron and Steel Works. These several inspectorial visits were thoroughly enjoyed, and the hospitality with which they were attended was most profuse. Doubtless, many of the practical and professional men who were present at the inspection gained a large fund of mental insight which will, ere long, be applied to practical advantage, or, at all events, be put to a practical test.

JOHN MAYER

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.]

A Mirage

As several notices of Mirage seen in Britain during the present year have recently appeared in NATURE, I venture to trouble you with the following substance of a few brief notes (made on the spot) of an instance which I witnessed on April 15, 1870.

With a companion I was walking slowly from Chesilton, in the "Isle" of Portland, along the western slope of the famous Chesil Bank, in a north-westerly direction—my friend on one of the numerous narrow terraces into which the slope is broken, I on that next below. The day was bright and warm, and the sea was calm. Having occasion to stoop down, I caught sight of what for the moment I thought a considerable pool of water on the terrace on I which stood, in the direction in which we were moving. A moment's thought reminded me that a pool in such a beach of loose pebbles was out of the question, and convinced me that for the first time in my life I saw a Mirage. The hour proved to be exactly noon, Greenwich time. My friend, on having his attention called to it, saw a similar "pool" on his terrace. On trying a series of experiments, we found that when we were quite erect the phenomenon was barely visible, and would never have commanded attention, but that as the eye was brought nearer and nearer to the surface the "pool" grew larger and larger; that in ordinary cases each observer could see it on his own terrace only, but that when he brought his eye to the outer edge of one terrace the "pools" on it and on that next below were visible at the same time, and were almost blended. As we advanced so did the Mirage. At 12.20, local time, when the sun was, of course, a very few degrees west of south, it bore N. 33° W., magnetic variation being allowed for. The apparent water, like the real water in the adjacent sea, was a dark blue. We left the beach at two o'clock, and noted that the "pools" were as distinct as ever; and, being then some distance from Portland, we observed a similar phenomenon in that, the opposite, direction.

Lamorna, Torquay, Sept. 10th, 1870 W. PENGELLY

External Configuration of the Earth

UNDER the heading of "Volcanic Action *v.* Denudation," I observe in the last number of NATURE (Sept. 8th), a letter from Mr. A. H. Green, in which that gentleman finds fault with the conclusion arrived at in the following quotation, apparently extracted from some, to me unknown, report of my lecture on volcanoes, the original of which is given in the *Geological Magazine* for July 1870; in instituting "a comparison between the relative magnitude of the operations of internal and external forces in determining the main external fractures of our globe, we must grant the first rank to the internal, volcanic, or cataclysmic agencies, since, had it not been for their operations, our globe would have remained without any visible land for the river to traverse, or the rain and ice to disintegrate and wear away;" but as immediately afterwards Mr. Green himself adds, "the latter part of this statement, cannot, of course, be called in question," these very words, alone, seem to me tantamount to a complete admission that he can have no basis whatever for disputing the deduction that the internal agencies must be placed in the first rank; be it remembered, also, that his quotation is merely a summing up of the evidence brought forward in the course of the lecture.

What I maintain—and I imagine every unbiassed person, whether scientific or not, if but endowed with a reasonable amount of common sense, will agree with me,—is, that when comparing the relative importance of the two very opposite forces which have combined to model the external surface of our globe, in all times down to the present, the precedence must be given to that agency which is admittedly, not only the primary one, but also the one which actually called the other into existence.

In answer to Mr. Green's similes, and as he says that, in a case like this "it is hard to attach any definite meaning to the idea of rank," I may assist him by simply asking, for example, whether any reasonable person, after contemplating, say St. Paul's Cathedral, one of the most prominent features of our metropolis, could possibly think of ranking the architect who originated it below the stone-mason, or sculptor who, directed by him, afterwards altered so vastly the external appearance of the rough shell of the edifice as it first rose from the ground.

Every one, whether geologist or not, admits that the most striking features in the world's physical geography are the mountain ranges which rise up and look down upon the plains below, formed from their own *débris*; and when it is remembered that these mountains are but so many grand proofs of the intensity and activity of those internal forces which not only elevated them, but, in most instances also, even supplied the larger portion of the rock substance which builds up their entire masses, does it not seem strange that any question of rank should arise, when comparing them with those external forces (denudation, &c.) which, of very necessity, can only be great in proportion as the anterior internal forces developed the conditions necessary for calling them into existence, and provided materials for them to operate upon?

DAVID FORBES

11, York Place, Portman Sq., W., Sept. 12th, 1870

The Meteor of 15th August

IN NATURE, page 357, you give notices and sketches of the Meteor of 15th August, and invite further description.

The particulars stated by the Duke of Argyll, and specially the very exact location assigned to the Meteor by your Portrush correspondent, render it probable that a tolerable approximation may be made to the exact position as regards height, &c.

To this end, and in supplement of notice accompanying diagram second, NATURE, page 357, the following details are important.

The Meteor's apparent position is recovered by means of its very close proximity to summits of the Binn hill, near and to the north of this place, with assistance of the six-inch Ordnance maps.

Local Time of first appearance	8·50	8·55 P.M. 15 Aug.
Altitude, uncorrected for refraction,	7°35′25″	8°31′30″
Azimuth, west of north,	66°2′3	67°23′10″

Observer's position, exactly in line of High-water level, In Latitude 56°3′51″ North, Longitude 3°12′45″ West.

The apparent size of the Meteor partly explains these pairs of limits, within which the nucleus at least may be located.

GEO. J. P. GRIEVE

Kirkbank, Burntisland, Sept. 12th

GEOLOGICAL DISCOVERY IN LIVERPOOL

ALL geologists visiting Liverpool for the approaching Meeting of the British Association for the advancement of Science will be glad to hear of the recent discovery of some new beds in the coal measures near Liverpool, which are exceedingly prolific in fossil remains. The new line of railway between Liverpool and St. Helen's runs at one place not far from Rainhill, through Thatto Heath, where a long cutting was rendered necessary. This cutting is through beds all belonging to the carboniferous strata—a thick coal seam, and the accompanying beds of shale and fireclay being all cut through. The fireclay contains abundant remains of all the varied plants of this epoch, with a few marine shells, *Anthrocosia* in some places. There is a large collection of these fossils now on exhibition in the Derby Museum, collected by the Rev. H. H. Higgins, who has shown great energy in the matter, and who has most generously presented his collection to the museum. This collection well merits the attention of all geologists, especially those interested in fossil botany. The plants found are in the most perfect state of preservation, and they are by no means difficult to meet with. They are chiefly *Sigillaria, Calamites, Lepidodendron, Neuropteris Loshii, N. nervosa*, and *N. gigantea*: some of the species, the rarer *Asterophyllites* and *Sphenophyllum*, together with many others, being not yet accurately determined. Throughout the clay are found scattered nodules of ironstone, which, on being broken up, are found to contain fossil remains, generally ferns or *Equiseta*. There have, however, been two exceedingly valuable finds lately made, two fine specimens of the wings of Neuropterous insects having come to light. One of these is in the possession of the Rev. H. H. Higgins; and the other, measuring some 3 to 3½ in. in length, is in the possession of Mr. E. Cleminshaw, of Merton College, Oxford. Both of these will be exhibited at the approaching meeting. They were both found in the ironstone nodules, and are very interesting, as only one other specimen, we believe, is known from the English coal measures. Not far from this cutting is a small coal pit, from which many interesting fossils are to be obtained. These are chiefly fish-remains, teeth, jaws, scales and bones, and a few rare ferns. The ease with which the blocks split into thin laminæ render these fossils easily found, and they are in a good state of preservation.

This locality is easily reached by train from Lime Street Station to Rainhill, from which place it is distant some twenty minutes' walk. The navvies on the line are very obliging, and have all the finer specimens, with which they are very willing to part for a few coppers or some tobacco.

J. P. EARWAKER

NOTES

We have reason to believe that the scheme for the proposed Indian Government School of Engineering is being warmly combated by the scientific branches of the army, whose counter proposal is that the Royal Military Academy at Woolwich should be made into an equivalent of the *Ecole Polytechnique*, and that young men intended for the engineering service of the Indian Government should be educated there, or better still, officers of the Royal Engineeers should be lent to the Indian Government. On this very important topic we would ask whether the result of the first report of the Royal Commission on Military Education, which is still sitting, has not been to reduce both in quantity and quality, the scientific teaching at Woolwich, which was already so absurdly small, that, mathematics apart, more science might be learned at many commercial academies.

It will be a long time before England forgets the loss of the *Captain*, though probably when a serious naval engagement does come, the going down of the largest ships, with the loss of all men on board, will be almost a matter of course. But as we have so often heard lately, *à la guerre comme à la guerre*, perhaps a more subdued thrill will then run through us. There seems little doubt that the *Captain* was top-heavy. The fearful rapidity with which, first a roll of 10 deg., then of 22 deg., then of 25 deg., and then a terrible catastrophe were recorded, leave no doubt on this point, and the turrets, the six twenty-five-ton guns, heavy armour, and masts high above the centre of gravity, must bear the blame. Fortunately, however, Captain Moncrieff's last achievement, which we described a few weeks ago, comes to our assistance by abolishing the turrets, and puts the weight of the enormous guns precisely where it is wanted.

The balloon is becoming a war engine with a vengeance. M. Nadar has been appointed a member of the Committee under the presidency of M. Berthelot, for the scientific defence of Paris, and several captive balloons are to be employed; while we learn from Strasburg that captive balloons are to be used for dropping nitro-glycerine bombs on the powder magazines of the town; the operations are to be conducted by an Englishman named Walter, and Herr Mahler, of Berlin.

The annual excursion of the London and Middlesex Archæological Society took place on Tuesday last. Notwithstanding the inclemency of the weather there was a numerous attendance.

The Warwickshire Agricultural Society's annual gathering was held on Tuesday last at Leamington. The Earl of Warwick presided at the annual dinner, which was attended by the representatives of both divisions of the county. As usual at these agricultural meetings, we fail to discover anything in the proceedings calculated to convey any impression to the agricultural mind of the necessary intimate connection between agriculture and science.

A leading article in the *British Medical Journal* for Sept. 10 is very severe on the insufficiency of the education at present given to our medical students, and the consequent inferiority, comparatively speaking, of the medical profession in this country. "The miserable inferiority in scientific research; the dearth of original work, the want of exactness, the poverty of physiological investigation, the ignorant impatience of practical detail which we all have to deplore so much in the mass of professional work at this day, are due to the inadequate preliminary cultivation of our students; to their defective training in scientific method; the small base on which the pyramid of medical lore is made to stand. The solemn deprecation of excessive devotion to microscopical research; the empty sneer at chemical physic; the idle and mischievous disregard of instruments of precision—the sphygmograph, the thermometer, the laryngoscope, the ophthalmoscope—are all the expressions of a Philistine ignorance. . . The inferiority of English to German medicine is due to this inferiority of preliminary training." Judging from the letters we have received, medical men here are very indignant at the strictures on English medical training contained in the two articles by Professor Stricker, recently printed in our columns. This is what one of their own journals says on the subject. Here is food for thought for the members of our Science Commission during the recess.

The editor of the *Gardeners' Magazine* offers a prize of twenty guineas for the best essay on irrigation as applied both to the farm and the garden. There is probably no question of greater material importance to England at the present time. A system of storage and irrigation by which the superfluous rain that falls in autumn and winter, frequently carrying devastation in its course, could be intercepted, stored, and applied to the fertilisation of the soil between May and August, would ensure to the farmer, and consequently to the country, a gain that would be simply incalculable in such summers as the one we have just experienced.

The Museum of Natural History in Madison University, New York, has had its valuable collection lately classified and arranged. Among its most valuable possessions are reckoned the collection of tropical and other rare birds collected by Prof. Bickmore; a group of gay beautiful plumaged birds brought from the Spice Islands, are especially noticeable. There is also a good collection of North American birds.

Mr. J. J. Bennett, the Curator of the Botanical Department of the British Museum, has just issued his annual report for 1869. The principal business done in the department during the year has been:—The rearrangement of a portion of the presses of the general herbarium; the rearrangement of certain orders of *Apetalæ* and *Endogens*, and of the lichens, both British and foreign, with numerous additions to each; the selection of a very large number of specimens from the herbarium of the late Mr. N. B. Ward, and from the collection of Abyssinian plants send by Dr. Schimper through the Foreign Office; the naming, arranging, and laying into the general herbarium of Berlandier's Mexican collection, of Linden's collection from New Granada, Tate's from Nicaragua, Coulter's from California, Sartwell's *Carices* of North America, Wright's collections from the Neilgherry Hills and from India generally, Jameson's from the Andes of Quito, *Orchideæ* from different countries, ferns from the islands of the South Pacific, and of a large number of miscellaneous specimens of various families and from different countries; the examination and arrangement of the recent and fossil *Coniferæ* and *Cycadeæ*, and of Mr. Brown's collection of fossil woods; the arrangement and incorporation in the general herbarium of a large number of European plants; the rearrangement of various portions of the British herbarium, and of the collection of fruits and seeds; and the rearrangement of various parts of the collection contained in the Exhibition Rooms, and especially of the cases containing *Coniferæ* and *Cacteæ*, with large additions. The most important additions to the collection during the year have been:—Upwards of 1,000 European plants from the collection of Dr. Rostan and the late Mr. N. B. Ward; 900 plants of Ingermannland; 300 from Sicily; 200 European fungi; 200 Italian cryptogams; 3,000 plants of Abyssinia, collected by Dr. Schimper; more than 3,000 plants of South Africa, from Mr. Ward's collection; upwards of 500 from Madeira, collected by Lemann and others; nearly 1,000 from the mountains of Altai; 1,000 from India, collected by Dr. Wright; 400 from Malacca, collected by Griffith; 100 from the Feejee Islands, collected by Harvey; 1,000 from North America; 400 fungi of South Carolina; 300 plants from Nicaragua, collected by Tate; 700

from the Andes of Quito; a fine set of pine cones from California; numerous specimens of plants and fruits, chiefly from Africa. The number of visits paid to the herbarium during the year for purposes of scientific research was 974.

WE have already signs of the opening of the medical session on the 1st of October next. The number of the *Chemical News* for Sept. 9th is devoted to a very useful summary of the requirements of the various examining bodies in this country in chemistry and physics, and of the courses of lectures and laboratory instruction given at the different colleges and medical schools in London and the provinces. The *British Medical Journal* for Sept. 10th also gives the regulations of the General Medical Council, and Medical licensing bodies, and notes concerning the hospitals and medical schools.

WITH reference to the paragraph in our last number respecting the use of ammonia in Victoria as a cure for snake-bite, we learn that the inhabitants of that colony are so deeply impressed with the great practical value of the discovery, that they are collecting subscriptions for a fitting testimonial to present to Dr Halford, who was the first to suggest and carry out this mode of treatment. Any contributions for this object will be received by Dr. G. E. Day, of Furzewell House, Torquay, who states that it has been the means of saving a number of cases in an apparently hopeless state of collapse.

SOME important experiments are now been conducted with the new description of torpedo submitted to the Government by Mr. Whitehead. The *Oberon* yesterday floated out of dock at Chatham, and will be sent round to Shoeburyness, where the experiments, which are to be carried out under the superintendence of a commission, of which Lieutenant-Colonel Nugent, Royal Engineers, is the president, are to take place. The wooden vessel to be operated upon with the torpedoes, which are fired from the stem of the *Oberon* beneath the water, is *L'Aigle*, which has been placed at the disposal of the committee by the Admiralty for that purpose. These torpedoes are *locomotive*, the motive power being compressed air, and already the results obtained are surprising. The minutes of evidence taken before the select committee on the Abyssinian expedition are published this morning. They fill a blue-book of 600 pages.

A BOTTLE-NOSED whale, measuring eighteen feet in length by eight feet in girth, came ashore near Burntisland, on the Scotch coast, on Thursday afternoon last. It was drawn up on to the beach while still alive. It is to be hoped that some museum will see about securing the skeleton of this whale; it would be a very welcome addition to many museums, and could, we should think, be obtained for a small cost.

THE extreme rarity of well-authenticated examples of the parasitism of the mistletoe on the oak has induced Dr. Bull, of Hereford, to collect the known instances, which he finds to be eight in number, viz., three in Herefordshire, and one each in Gloucestershire, Monmouthshire, Devonshire, Hants, and Surrey. In the most recently-discovered instance, in the Forest of Deerfold in Herefordshire, the mistletoe was found on an oak of the variety *sessiliflora*, some fifty or sixty years old; it is a female plant, growing high up on the main stem, and forming a large spreading branch with a diameter of three-and-a-half feet, and springing from the oak in a single stem nearly four inches in circumference. The mistletoe also grows on a thorn close by, and has probably sprung from a seed dropped by a bird from above.

A NEW process for making steel has been discovered in America, by means of which it is stated that American steel can be made equal to that usually imported into that country. It is manufactured from a peculiar iron ore found in Codorus township, Pennsylvania. The iron is mixed in a reverberatory furnace with middling pig-iron in the proportion of one to six. It is hoped by means of this valuable discovery to manufacture steel rails at a cost of about sixty dollars per ton.

THE BRITISH ASSOCIATION.—LIVERPOOL MEETING, 1870

AS we go to press the great annual scientific meeting has already commenced; and although the President is at the present moment actually delivering his opening address, we are able, through Prof. Huxley's kindness, to give our readers a verbatim report. We believe it will be found to rank in interest and importance along with any of its predecessors. We are also able to give Prof. Roscoe's Address to Section B; the Kew Report lack of space compels us to defer till next week. We have already given the particulars of the places of meeting and officers of the various sections. Not much remains to be added: by the time this is in the hands of our readers the meeting will be in full swing, and those who are attending it will already be at home in all the various arrangements. Among the most interesting occasions will doubtless be Sir John Lubbock's lecture to working men. The Mayor's reception at the Town Hall, continued for two successive evenings, though not open to all who show the ticket of the Association, is virtually so. All who have arrived in time will receive a formal invitation; and any omission, if such occur, will rest with those who should promptly send forward the names. Another entertainment is that to be given in the Philharmonic Hall on Saturday evening, the 17th, by Dr. E. R. Bickersteth. Besides giving a subscription on the largest scale to the local fund, he will entertain about 700 strangers and 300 of our own townspeople. Eight excursions have been arranged for Thursday, September 22nd, in connection with the Association. The first of these is to Cefn Hall, near St. Asaph, where the party will be received at a luncheon by Mrs. Williams Wynne; the excursionists will start from the George's Landing Stage by the railway boat. An excursion party will also leave for Chester by the same boat. An excursion party to Crewe Works will leave the Lime Street Station by an early train. The guests, whose number is limited to one hundred, are invited to a luncheon at Crewe, provided by the London and North Western Railway Company. A fourth excursion will be to Llandudno, and will start from the Prince's Landing Stage in the *Eblana*, kindly lent for the occasion by the City of Dublin Steam Packet Company. Dinner and tea will be provided on board the vessel at hours most convenient to the excursionists. Another party will leave the George's Landing Stage for Llangollen, and have luncheon at the Hand Hotel, Llangollen. A sixth excursion will visit Widnes, where there will be a dinner in the public hall, by invitation of the Widnes committee of reception. There will likewise be an excursion to Wigan, and an excursion up and down the River Mersey. In Liverpool many of the chief works, manufactories, and public institutions will be open to the inspection of the members of the Association all through the week. Among the papers intended to be read, the titles of which have already reached us, the following are among the most interesting:—In Section A, Francis Galton, F.R.S., "Barometric Predictions of Coming Weather."—John J. Hall, "A new Electro-magnetic Electrometer."—A. W. Bickerton, "A new Heat Engine."—W. Rowett, "Ocean Telegraphy."—Henry Hudson, Glenville, Fermoy, Ireland, "On the Wave Theories of Light."—Dr. Joseph Henry, Smithsonian Institution, Washington, U.S.A., who will be present at the meeting, "On the Rainfall of the United States."—R. S. Ball, Royal College of Science, Dublin, "The small Oscillations of a Rigid Body."—S. Hewett, Marlborough, Wilts, "The Earth's Centre of Gravity, Axis of Revolution, and Magnetic Axis or Centre."—W. M. Watts, "The Existence of two Spectra produced by Carbon incandescent at the same Temperature."—In Section B, C. R. Tichborne, F.C.S., "On the Action of Street Dust as a Ferment."—W. H. Perkin, "On Artificial Alizarine."—A. H. Church, "Experiments on the Preservation of Stone," "Contributions to Mineralogical Chemistry."—John G. Macvicar, "On the Structure and Form of an Atom of Moisture" (illustrated by models).—J. H. Lloyd, M.D., Anglesea, "On the Dry System of Sewage."—In Section C, J. Logan Lobley, "On the Stratigraphical Distribution of the British Fossil Gasteropoda."—W. C. Williamson, "On the Organisation and Affinities of the Calamities of the Coal Measures."—G. A. Laborn, "On the Tertiary Coal-field of Southern Chili."—Charles Ricketts, "On a Railway Section across the Prescot Coal-field."—John W. Judd, "On the Age of the Wealden."—Geo. Busk, a paper by Dr. Leith Adams, "On a New Species of Fossil Elephants from Malta."—Charles Jeaks, "On the Norwich Crag."

ADDRESS OF THOMAS HENRY HUXLEY, LL.D., F.R.S., PRESIDENT.

MY LORDS, LADIES, AND GENTLEMEN,—It has long been the custom for the newly installed President of the British Association for the Advancement of Science to take advantage of the elevation of the position in which the suffrages of his colleagues had, for the time, placed him, and, casting his eyes around the horizon of the scientific world, to report to them what could be seen from his watch-tower; in what directions the multitudinous divisions of the noble army of the improvers of natural knowledge were marching; what important strongholds of the great enemy of us all, ignorance, had been recently captured; and, also, with due impartiality, to mark where the advanced posts of science had been driven in, or a long-continued siege had made no progress.

I propose to endeavour to follow this ancient precedent, in a manner suited to the limitations of my knowledge and of my capacity. I shall not presume to attempt a panoramic survey of the world of science, nor even to give a sketch of what is doing in the one great province of biology, with some portions of which my ordinary occupations render me familiar. But I shall endeavour to put before you the history of the rise and progress of a single biological doctrine; and I shall try to give some notion of the fruits, both intellectual and practical, which we owe, directly or indirectly, to the working out, by seven generations of patient and laborious investigators, of the thought which arose, more than two centuries ago, in the mind of a sagacious and observant Italian naturalist.

It is a matter of every-day experience that it is difficult to prevent many articles of food from becoming covered with mould; that fruit, sound enough to all appearance, often contains grubs at the core; that meat, left to itself in the air, is apt to putrefy and swarm with maggots. Even ordinary water, if allowed to stand in an open vessel, sooner or later becomes turbid and full of living matter.

The philosophers of antiquity, interrogated as to the cause of these phenomena, were provided with a ready and a plausible answer. It did not enter their minds even to doubt that these low forms of life were generated in the matters in which they made their appearance. Lucretius, who had drunk deeper of the scientific spirit than any poet of ancient or modern times except Goethe, intends to speak as a philosopher, rather than as a poet, when he writes that "with good reason the earth has gotten the name of mother, since all things are produced out of the earth. And many living creatures, even now, spring out of the earth, taking form by the rains and the heat of the sun."* The axiom of ancient science, "that the corruption of one thing is the birth of another," had its popular embodiment in the notion that a seed dies before the young plant springs from it; a belief so wide spread and so fixed, that Saint Paul appeals to it in one of the most splendid outbursts of his fervid eloquence:—

"Thou fool, that which thou sowest is not quickened, except it die."†

The proposition that life may, and does, proceed from that which has no life, then, was held alike by the philosophers, the poets, and the people, of the most enlightened nations, eighteen hundred years ago; and it remained the accepted doctrine of learned and unlearned Europe, through the middle ages, down even to the seventeenth century.

It is commonly counted among the many merits of our great countryman, Harvey, that he was the first to declare the opposition of fact to venerable authority in this, as in other matters; but I can discover no justification for this wide-spread notion. After careful search through the "Exercitationes de Generatione," the most that appears clear to me is, that Harvey believed all animals and plants to spring from what he terms a "*primordium vegetale*," a phrase which may nowadays be rendered "a vegetative germ;" and this, he says, is "*oviforme*," or "egg-like;" not, he is careful to add, that it necessarily has the shape of an egg, but because it has the constitution and nature of one. That this "*primordium oviforme*" must needs, in all cases, proceed from a living parent is nowhere expressly maintained by Harvey, though such an opinion may be thought to be implied in one or two passages; while, on the other hand, he does, more than once, use language which is consistent only with a full belief in spontaneous or equivocal generation.* In fact, the main concern of Harvey's wonderful little treatise is not with generation, in the physiological sense, at all, but with development; and his great object is the establishment of the doctrine of epigenesis.

The first distinct enunciation of the hypothesis that all living matter has sprung from pre-existing living matter, came from a contemporary, though a junior, of Harvey, a native of that country, fertile in men great in all departments of human activity, which was to intellectual Europe, in the sixteenth and seventeenth centuries, what Germany is in the nineteenth. It was in Italy, and from Italian teachers, that Harvey received the most important part of his scientific education. And it was a student trained in the same schools, Francesco Redi—a man of the widest knowledge and most versatile abilities, distinguished alike as scholar, poet, physician, and naturalist—who, just two hundred and two years ago, published his "Esperienze intorno alla Generazione degl' Insetti," and gave to the world the idea, the growth of which it is my purpose to trace. Redi's book went through five editions in twenty years; and the extreme simplicity of his experiments, and the clearness of his arguments, gained for his views, and for their consequences, almost universal acceptance.

Redi did not trouble himself much with speculative considerations, but attacked particular cases of what was supposed to be "spontaneous generation" experimentally. Here are dead animals, or pieces of meat, says he; I expose them to the air in hot weather, and in a few days they swarm with maggots. You tell me that these are generated in the dead flesh; but if I put similar bodies, while quite fresh, into a jar, and tie some fine gauze over the top of the jar, not a maggot makes its appearance, while the dead substances, nevertheless, putrefy just in the same way as before. It is obvious, therefore, that the maggots are not generated by the corruption of the meat; and that the cause of their formation must be a something which is kept away by gauze. But gauze will not keep away aëriform bodies, or fluids. This something must, therefore, exist in the form of solid particles too big to get through the gauze. Nor is one long left in doubt what these solid particles are; for the blowflies, attracted by the odour of the meat, swarm round the vessel, and, urged by a powerful but in this case misleading instinct, lay eggs out of which maggots are immediately hatched upon the gauze. The conclusion, therefore, is unavoidable; the maggots are not generated by the meat, but the eggs which give rise to them are brought through the air by the flies.

These experiments seem almost childishly simple, and one wonders how it was that no one ever thought of them before. Simple as they are, however, they are worthy of the most careful study, for every piece of experimental work since done, in regard to this subject, has been shaped upon the model furnished by the Italian philosopher. As the results of his experiments were the same, however varied the nature of the materials he used, it is not wonderful that there arose in Redi's mind a presumption, that in all such cases of the seeming production of life from dead matter, the real explanation was the introduction of living germs from without into that dead matter.† And thus the hypothesis

* It is thus that Mr. Munro renders

"Linquitur, ut merito maternum nomen adepta
Terra sit, e terra quoniam sunt cuncta creata.
Multaque nunc etiam ex-istant animalia terris
Imbribus et calido solis concreta vapore."

De Rerum Natura, lib. v. 793-796.

But would not the meaning of the last line be better rendered "Developed in rain-water and in the warm vapours raised by the sun"?

† 1 Corinthians xv. 36.

* See the following passage in Exercitatio I.:—"Item *sponte nascentia* dicuntur; non quod ex *putredine* oriunda sint, sed quod casu, naturæ sponte, et æquivocâ (ut aiunt) generatione, a parentibus sui dissimilibus proveniant." Again, in "De Uteri Membranis":—"In cunctorum viventium generatione (sicut diximus) hoc solenne est, ut ortum ducunt a *primordio* aliquo, quod tum materiam tum efficiendi potestatem in se habet: sitque adeo id, ex quo et a quo quicquid nascitur, ortum suum ducat. Tale primordium in animalibus (*sive ab aliis generantibus proveniant, sive sponte, aut ex putredine nascentur*) est humor in tunicâ aliquâ aut putamine conclusus." Compare also what Redi has to say respecting Harvey's opinions, "Esperienze," p. 11.

† "Pure contentandomi sempre in questa ed in ciascuna altro cosa, da ciascuno più savio, là dove io difettuosamente parlassi, esser corretto; non tacerò, che per molte osservazioni molti volti da me fatte, mi sento inclinato a credere che la terra, da quelle prime piante, e da quei primi animali in poi, che ellà nei primi giorni del mondo produsse per comandemento del sovrano

that living matter always arises by the agency of pre-existing living matter, took definite shape; and had, henceforward, a right to be considered and a claim to be refuted, in each particular case, before the production of living matter in any other way could be admitted by careful reasoners. It will be necessary for me to refer to this hypothesis so frequently, that, to save circumlocution, I shall call it the hypothesis of *Biogenesis;* and I shall term the contrary doctrine—that living matter may be produced by not living matter—the hypothesis of *Abiogenesis.*

In the seventeenth century, as I have said, the latter was the dominant view, sanctioned alike by antiquity and by authority; and it is interesting to observe that Redi did not escape the customary tax upon a discoverer of having to defend himself against the charge of impugning the authority of the Scriptures;* for his adversaries declared that the generation of bees from the carcase of a dead lion is affirmed, in the Book of Judges, to have been the origin of the famous riddle with which Samson perplexed the Philistines:—

"Out of the eater came forth meat,
And out of the strong came forth sweetness."

Against all odds, however, Redi, strong with the strength of demonstrable fact, did splendid battle for Biogenesis; but it is remarkable that he held the doctrine in a sense which, if he had lived in these times, would have infallibly caused him to be classed among the defenders of "spontaneous generation." "Omne vivum ex vivo," "no life without antecedent life," aphoristically sums up Redi's doctrine; but he went no further. It is most remarkable evidence of the philosophic caution and impartiality of his mind, that although he had speculatively anticipated the manner in which grubs really are deposited in fruits and in the galls of plants, he deliberately admits that the evidence is insufficient to bear him out; and he therefore prefers the supposition that they are generated by a modification of the living substance of the plants themselves. Indeed, he regards these vegetable growths as organs, by means of which the plant gives rise to an animal, and looks upon this production of specific animals as the final cause of the galls and of at any rate some fruits. And he proposes to explain the occurrence of parasites within the animal body in the same way.†

It is of great importance to apprehend Redi's position rightly; for the lines of thought he laid down for us are those upon which naturalists have been working ever since. Clearly, he held *Biogenesis* as against *Abiogenesis;* and I shall immediately proceed, in the first place, to inquire how far subsequent investigation has borne him out in so doing.

But Redi also thought that there were two modes of Biogenesis. By the one method, which is that of common and ordinary occurrence, the living parent gives rise to offspring which passes through the same cycle of changes as itself—like gives rise to like; and this has been termed *Homogenesis.* By the other mode the living parent was supposed to give rise to offspring which passed through a totally different series of states from those exhibited by the parent, and did not return into the cycle of the parent; this is what ought to be called *Heterogenesis,* the offspring being altogether, and permanently unlike the parent. The term Heterogenesis, however, has unfortunately been used in a different sense, and M. Milne-Edwards has therefore substituted for it *Xenogenesis,* which means the generation of something foreign. After discussing Redi's hypothesis of universal Biogenesis, then, I shall go on to ask how far the growth of science justifies his other hypothesis of Xenogenesis.

The progress of the hypothesis of Biogenesis was triumphant and unchecked for nearly a century. The application of the microscope to anatomy in the hands of Grew, Leeuwenhoek, Swammerdam, Lyonet, Vallisnieri, Reaumur, and other illustrious investigators of nature of that day, displayed such a complexity of organisation in the lowest and minutest forms, and everywhere revealed such a prodigality of provision for their multiplication by germs of one sort or another, that the hypothesis of Abiogenesis began to appear not only untrue, but absurd; and, in the middle of the eighteenth century, when Needham and Buffon took up the question, it was almost universally discredited.*

But the skill of the microscope-makers of the eighteenth century soon reached its limit. A microscope magnifying 400 diameters was a *chef d'œuvre* of the opticians of that day; and at the same time, by no means trustworthy. But a magnifying power of 400 diameters, even when definition reaches the exquisite perfection of our modern achromatic lenses, hardly suffices for the mere discernment of the smallest forms of life. A speck, only $\frac{1}{25}$th *of an inch in diameter,* has, at 10 *inches from the eye,* the same apparent size as an object $\frac{1}{10000}$th of an inch in diameter, when magnified 400 times; but forms of living matter abound, the diameter of which is not more than $\frac{1}{40000}$th of an inch. A filtered infusion of hay, allowed to stand for two days, will swarm with living things, among which, any which reaches the diameter of a human red blood-corpuscle, or about $\frac{1}{3200}$th of an inch, is a giant. It is only by bearing these facts in mind, that we can deal fairly with the remarkable statements and speculations put forward by Buffon and Needham in the middle of the eighteenth century.

When a portion of any animal or vegetable body is infused in water, it gradually softens and disintegrates; and, as it does so, the water is found to swarm with minute active creatures, the so-called Infusorial Animalcules, none of which can be seen, except by the aid of the microscope; while a large proportion belong to the category of smallest things of which I have spoken, and which must have all looked like mere dots and lines under the ordinary microscopes of the eighteenth century.

Led by various theoretical considerations which I cannot now discuss, but which looked promising enough in the lights of that day, Buffon and Needham doubted the applicability of Redi's hypothesis to the infusorial animalcules, and Needham very properly endeavoured to put the question to an experimental test.

ed omnipotente Fattore, non abbia mai più prodotto da se medesima nè erba nè albero, nè animale alcuno perfetto o imperfetto che ei se fosse; e che tutto quello, che ne' tempi trapassati è nato e che ora nascere in lei, o da lei veggiamo, venga tutto dalla semenza reale e vera delle piante, e degli animali stessi, i quali col mezzo del proprio seme la loro spezie conservano. E se bene tutto giorno scorghiamo da' cadaveri degli animali, e da tutte quante le maniere dell' erbe, e de' fiori, e dei frutti imputriditi, e corrotti nascere vermi infiniti—

'Nonne vides quæcunque mora, fluidoque calore
Corpora tabescunt in parva animalia verti'—

Io mi sento, dico, inclinato a credere che tutti quei vermi si generino dal seme paterno; e che le carni, e l'erbe, e l'altre cose tutte putrefatte, o putrefattibili non facciano altra parte, nè abbiano altro ufizio nella generazione degl' insetti, se non d'apprestare un luogo o un nido proporzionato, in cui dagli animali nel tempo della figliatura sieno portati, e partoriti i vermi, o l'uova o l'altre semenze dei vermi, i quali tosto che nati sono, trovano in esso nido un sufficiente alimento abilissimo per nutricarsi: e se in quello non son portate dalle madri queste suddette semenze, niente mai, e replicatamente niente, vi s'ingegneri e nasca."—REDI, *Esperienze,* pp. 14-16.

* "Molti, e molti altri ancora vi potrei annoverare, se non fossi chiamato a rispondere alle rampogne di alcuni, che bruscamente mi rammentano ciò, che si legge nel capitolo quattordicesimo del sacrosanto Libro de' giudici. . . . ."—REDI, *l.c.* p. 45.

† The passage (Esperienze, p. 129) is worth quoting in full:—

"Se dovessi palesarvi il mio sentimento crederei che i frutti, i legumi, gli alberi e le foglie, in due maniere inverminassero. Una, perchè venendo i bachi per di fuora, e cercando l'alimento, col rodere ci aprono la strada, ed arrivano alla più interna midolla de' frutti e de' legni. L'altra maniera si è, che io per me stimerei, che non fosse gran fatto disdicevole il credere, che quell' anima o quella virtù, la quale genera i fiori ed i frutti nelle piante viventi, sia quella stessa che generi ancora i bachi di esse piante. E chi sà orse, che molti frutti degli alberi non sieno prodotti, non per un fine primario e principale, ma bensì per un uffizio secondario e servile, destinato alla generazione di que' vermi, servendo a loro in vece di matrice, in cui dimorino un prefisso e determinato tempo; il quale arrivato escan fuora a godere il sole.

"Io m' immagino, che questo mio pensiero non vi parrà totalmente un paradosso; mentre farete riflessione a quelle tante sorte di galle, di gallozzole, di coccole, di ricci, di calici, di cornetti e di lappole, che son produtte dalle querce, dalle farnie, da' cerri, da' sugheri, da' lecci e da altri simili alberi da ghianda; imperciocchè in quelle gallozzole, e particolarmente nelle più grosse, che si chiamano coronati, ne' ricci capelluti, che ciuffoli da' nostri contadini son detti; nei ricci legnosi del cerro, ne' ricci stellati della quercia, nelle galluzze della foglia del leccio si vede evidentissimamente, che la prima e principale intenzione della natura è formare dentro di quelle un animale volante; vedendosi nel centro della gallozzola un uovo, che col crescere e col maturarsi di essa gallozzola va crescendo e maturando anch' egli, e cresce altresì a suo tempo quel verme, che nell' uovo si racchiude; il qual verme, quando la gallozzola è finita di maturare e che è venuto il termine destinato al suo nascimento, diventa, di verme che era, una mosca. . . . . Io vi confesso ingenuamente, che prima d'aver fatte queste mie esperienze intorno alla generazione degl' insetti mi dava a credere, o per dir meglio sospettava, che forse la gallozzola nascesse, perchè arrivando la mosca nel tempo della primavera, e facendo una piccolissima fessura ne' rami più teneri della quercia, in quella fessura nascondesse uno de suoi semi, il quale fosse cagione che sbocciasse fuora la gallozzola; e che mai non si vedessero galle o gallozzole o ricci o cornetti o calici o coccole, se non in que' rami, ne' quali le mosche avessero depositate le loro semenze; e mi dava ad intendere, che le gallozzole fossero una malattia cagionata nelle querce dalle punture delle mosche, in quella giusa stessa che dalle punture d'altri animaletti simiglievoli veggiamo crescere de' tumori ne' corpi degli animali."

* Needham, writing in 1750, says:—

"Les naturalistes modernes s'accordent unanimement à établir, comme une vérité certaine, que toute plante vient de sa sémence spécifique, tout animal d'un œuf ou de quelque chose d'analogue préexistant dans la plante, ou dans l'animal de même espèce qui l'a produit."—*Nouvelles Observations,* p. 169.

"Les naturalistes ont généralement cru que les animaux microscopique étaient engendrés par des œufs transportés dans l'air, ou déposés dans des eaux dormantes par des insectes volans."—*Ibid.* p. 176.

He said to himself, if these infusorial animalcules come from germs, their germs must exist either in the substance infused, or in the water with which the infusion is made, or in the superjacent air. Now the vitality of all germs is destroyed by heat. Therefore, if I boil the infusion, cork it up carefully, cementing the cork over with mastic, and then heat the whole vessel by heaping hot ashes over it, I must needs kill whatever germs are present. Consequently, if Redi's hypothesis hold good, when the infusion is taken away and allowed to cool, no animalcules ought to be developed in it; whereas, if the animalcules are not dependent on pre-existing germs, but are generated from the infused substance, they ought, by-and-by, to make their appearance. Needham found that, under the circumstances in which he made his experiments, animalcules always did arise in the infusions, when a sufficient time had elapsed to allow for their development.

In much of his work Needham was associated with Buffon, and the results of their experiments fitted in admirably with the great French naturalist's hypothesis of "organic molecules," according to which, life is the indefeasible property of certain indestructible molecules of matter, which exist in all living things, and have inherent activities by which they are distinguished from not living matter. Each individual living organism is formed by their temporary combination. They stand to it in the relation of the particles of water to a cascade, or a whirlpool; or to a mould, into which the water is poured. The form of the organism is thus determined by the reaction between external conditions and the inherent activities of the organic molecules of which it is composed; and, as the stoppage of a whirlpool destroys nothing but a form, and leaves the molecules of the water, with all their inherent activities intact, so what we call the death and putrefaction of an animal, or of a plant, is merely the breaking up of the form, or manner of association, of its constituent organic molecules, which are then set free as infusorial animalcules.

It will be perceived that this doctrine is by no means identical with *Abiogenesis*, with which it is often confounded. On this hypothesis, a piece of beef, or a handful of hay, is dead only in a limited sense. The beef is dead ox, and the hay is dead grass; but the "organic molecules" of the beef or the hay are not dead, but are ready to manifest their vitality as soon as the bovine or herbaceous shrouds in which they are imprisoned are rent by the macerating action of water. The hypothesis therefore must be classified under Xenogenesis, rather than under Abiogenesis. Such as it was, I think it will appear, to those who will be just enough to remember that it was propounded before the birth of modern chemistry, and of the modern optical arts, to be a most ingenious and suggestive speculation.

But the great tragedy of Science—the slaying of a beautiful hypothesis by an ugly fact—which is so constantly being enacted under the eyes of philosophers, was played, almost immediately, for the benefit of Buffon and Needham.

Once more, an Italian, the Abbé Spallanzani, a worthy successor and representative of Redi in his acuteness, his ingenuity, and his learning, subjected the experiments and the conclusions of Needham to a searching criticism. It might be true that Needham's experiments yielded results such as he had described, but did they bear out his arguments? Was it not possible, in the first place, that he had not completely excluded the air by his corks and mastic? And was it not possible, in the second place, that he had not sufficiently heated his infusions and the superjacent air? Spallanzani joined issue with the English naturalist on both these pleas, and he showed that if, in the first place, the glass vessels in which the infusions were contained were hermetically sealed by fusing their necks, and if, in the second place, they were exposed to the temperature of boiling water for three-quarters of an hour,* no animalcules ever made their appearance within them. It must be admitted that the experiments and arguments of Spallanzani furnish a complete and a crushing reply to those of Needham. But we all too often forget that it is one thing to refute a proposition, and another to prove the truth of a doctrine which, implicitly or explicitly, contradicts that proposition, and the advance of science soon showed that though Needham might be quite wrong, it did not follow that Spallanzani was quite right.

Modern chemistry, the birth of the latter half of the eighteenth century, grew apace, and soon found herself face to face with the great problems which biology had vainly tried to attack without her help. The discovery of oxygen led to the laying of the foundations of a scientific theory of respiration, and to an examination of the marvellous interactions of organic substances with oxygen. The presence of free oxygen appeared to be one of the conditions of the existence of life, and of those singular changes in organic matters which are known as fermentation and putrefaction. The question of the generation of the infusory animalcules thus passed into a new phase. For what might not have happened to the organic matter of the infusions, or to the oxygen of the air, in Spallanzani's experiments? What security was there that the development of life which ought to have taken place had not been checked or prevented by these changes?

The battle had to be fought again. It was needful to repeat the experiments under conditions which would make sure that neither the oxygen of the air, nor the composition of the organic matter, was altered in such a matter as to interfere with the existence of life.

Schulze and Schwann took up the question from this point of view in 1836 and 1837. The passage of air through red-hot glass tubes, or through strong sulphuric acid, does not alter the proportion of its oxygen, while it must needs arrest or destroy any organic matter which may be contained in the air. These experimenters, therefore, contrived arrangements by which the only air which should come into contact with a boiled infusion should be such as had either passed through red-hot tubes or through strong sulphuric acid. The result which they obtained was that an infusion so treated developed no living things, while if the same infusion was afterwards exposed to the air such things appeared rapidly and abundantly. The accuracy of these experiments has been alternately denied and affirmed. Supposing them to be accepted, however, all that they really proved was that the treatment to which the air was subjected destroyed *something* that was essential to the development of life in the infusion. This "something" might be gaseous, fluid, or solid; that it consisted of germs remained only an hypothesis of greater or less probability.

Contemporaneously with these investigations a remarkable discovery was made by Cagniard de la Tour. He found that common yeast is composed of a vast accumulation of minute plants. The fermentation of must or of wort in the fabrication of wine and of beer is always accompanied by the rapid growth and multiplication of these *Torulæ*. Thus fermentation, in so far as it was accompanied by the development of microscopical organisms in enormous numbers, became assimilated to the decomposition of an infusion of ordinary animal or vegetable matter; and it was an obvious suggestion that the organisms were, in some way or other, the causes both of fermentation and of putrefaction. The chemists, with Berzelius and Liebig at their head, at first laughed this idea to scorn; but in 1843, a man then very young, who has since performed the unexampled feat of attaining to high eminence alike in Mathematics, Physics, and Physiology—I speak of the illustrious Helmholtz—reduced the matter to the test of experiment by a method alike elegant and conclusive. Helmholtz separated a putrefying or a fermenting liquid from one which was simply putrescible or fermentable by a membrane which allowed the fluids to pass through and become intermixed, but stopped the passage of solids. The result was, that while the putrescible or the fermentable liquids became impregnated with the results of the putrescence or fermentation which was going on on the other side of the membrane, they neither putrefied (in the ordinary way) nor fermented; nor were any of the organisms which abounded in the fermenting or putrefying liquid generated in them. Therefore the cause of the development of these organisms must lie in something which cannot pass through membranes; and as Helmholtz's investigations were long antecedent to Graham's researches upon colloids, his natural conclusion was that the agent thus intercepted must be a solid material. In point of fact, Helmholtz's experiments narrowed the issue to this: that which excites fermentation and putrefaction, and at the same time gives rise to living forms in a fermentable or putrescible fluid, is not a gas and is not a diffusible fluid; therefore it is either a colloid, or it is matter divided into very minute solid particles.

The researches of Schroeder and Dusch in 1854, and of Schroeder alone, in 1859, cleared up this point by experiments which are simply refinements upon those of Redi. A lump of cotton-wool is, physically speaking, a pile of many thicknesses of a very fine gauze, the fineness of the meshes of which depends upon the closeness of the compression of the wool. Now, Schroeder and Dusch found, that, in the case of all the putrefiable materials which they used (except milk and yolk of egg), an infusion boiled, and then allowed to come into contact with no air but such as had been filtered through cotton-wool, neither putrefied nor fermented, nor developed living forms. It

* See Spallanzani, "Opere," vi. pp. 42 and 51.

is hard to imagine what the fine sieve formed by the cotton-wool could have stopped except minute solid particles. Still the evidence was incomplete until it had been positively shown, first, that ordinary air does contain such particles; and, secondly, that filtration through cotton-wool arrests these particles and allows only physically pure air to pass. This demonstration has been furnished within the last year by the remarkable experiments of Professor Tyndall. It has been a common objection of Abiogenists that, if the doctrine of Biogeny is true, the air must be thick with germs; and they regard this as the height of absurdity. But Nature occasionally is exceedingly unreasonable, and Professor Tyndall has proved that this particular absurdity may nevertheless be a reality. He has demonstrated that ordinary air is no better than a sort of stirabout of excessively minute solid particles; that these particles are almost wholly destructible by heat; and that they are strained off, and the air rendered optically pure by being passed through cotton-wool.

But it remains yet in the order of logic, though not of history, to show that among these solid destructible particles there really do exist germs capable of giving rise to the development of living forms in suitable menstrua. This piece of work was done by M. Pasteur in those beautiful researches which will ever render his name famous; and which, in spite of all attacks upon them, appear to me now, as they did seven years ago,* to be models of accurate experimentation and logical reasoning. He strained air through cotton-wool, and found, as Schroeder and Dusch had done, that it contained nothing competent to give rise to the development of life in fluids highly fitted for that purpose. But the important further links in the chain of evidence added by Pasteur are three. In the first place he subjected to microscopic examination the cotton-wool which had served as strainer, and found that sundry bodies clearly recognizable as germs, were among the solid particles strained off. Secondly, he proved that these germs were competent to give rise to living forms by simply sowing them in a solution fitted for their development. And, thirdly, he showed that the incapacity of air strained through cotton-wool to give rise to life, was not due to any occult change effected in constituents of the air by the wool, by proving that the cotton-wool might be dispensed with altogether, and perfectly free access left between the exterior air and that in the experimental flask. If the neck of the flask is drawn out into a tube and bent downwards; and if, after the contained fluid has been carefully boiled, the tube is heated sufficiently to destroy any germs which may be present in the air which enters as the fluid cools, the apparatus may be left to itself for any time and no life will appear in the fluid. The reason is plain. Although there is free communication between the atmosphere laden with germs and the germless air in the flask, contact between the two takes place only in the tube; and as the germs cannot fall upwards, and there are no currents, they never reach the interior of the flask. But if the tube be broken short off where it proceeds from the flask, and free access be thus given to germs falling vertically out of the air, the fluid which has remained clear and desert for months, becomes, in a few days turbid and full of life.

These experiments have been repeated over and over again by independent observers with entire success; and there is one very simple mode of seeing the facts for oneself, which I may as well describe.

Prepare a solution (much used by M. Pasteur, and often called "Pasteur's solution") composed of water with tartrate of ammonia, sugar, and yeast-ash dissolved therein.† Divide it into three portions in as many flasks; boil all three for a quarter of an hour; and, while the steam is passing out, stop the neck of one with a large plug of cotton-wool, so that this also may be thoroughly steamed. Now set the flasks aside to cool, and when their contents are cold, add to one of the open ones a drop of filtered infusion of hay which has stood for twenty-four hours, and is consequently full of the active and excessively minute organisms known as *Bacteria*. In a couple of days of ordinary warm weather the contents of this flask will be milky from the enormous multiplication of *Bacteria*. The other flask, open and exposed to the air, will, sooner or later, become milky with *Bacteria*, and patches of mould may appear in it; while the liquid in the flask, the neck of which is plugged with cotton-wool, will remain clear for an indefinite time. I have sought in vain for any explanation of these facts, except the obvious one, that the air contains germs competent to give rise to *Bacteria*, such as those with which the first solution has been knowingly and purposely inoculated, and to the mould-*Fungi*. And I have not yet been able to meet with any advocate of Abiogenesis who seriously maintains that the atoms of sugar, tartrate of ammonia, yeast-ash, and water, under no influence but that of free access of air and the ordinary temperature, rearrange themselves and give rise to the protoplasm of *Bacterium*. But the alternative is to admit that these *Bacteria* arise from germs in the air; and if they are thus propagated, the burden of proof that other like forms are generated in a different manner, must rest with the assertor of that proposition.

To sum up the effect of this long chain of evidence:—

It is demonstrable that a fluid eminently fit for the development of the lowest forms of life, but which contains neither germs, nor any protein compound, gives rise to living things in great abundance if it is exposed to ordinary air, while no such development takes place if the air with which it is in contact is mechanically freed from the solid particles which ordinarily float in it and which may be made visible by appropriate means.

It is demonstrable that the great majority of these particles are destructible by heat, and that some of them are germs or living particles capable of giving rise to the same forms of life as those which appear when the fluid is exposed to unpurified air.

It is demonstrable that inoculation of the experimental fluid with a drop of liquid known to contain living particles gives rise to the same phenomena as exposure to unpurified air.

And it is further certain that these living particles are so minute that the assumption of their suspension in ordinary air presents not the slightest difficulty. On the contrary, considering their lightness and the wide diffusion of the organisms which produce them, it is impossible to conceive that they should not be suspended in the atmosphere in myriads.

Thus the evidence, direct and indirect, in favour of *Biogenesis* for all known forms of life must, I think, be admitted to be of great weight.

On the other side the sole assertions worthy of attention are that hermetically sealed fluids, which have been exposed to great and long-continued heat, have sometimes exhibited living forms of low organization when they have been opened.*

The first reply that suggests itself is the probability that there must be some error about these experiments, because they are performed on an enormous scale every day with quite contrary results. Meat, fruits, vegetables, the very materials of the most fermentable and putrescible infusions are preserved to the extent, I suppose I may say, of thousands of tons every year, by a method which is a mere application of Spallanzani's experiment. The matters to be preserved are well boiled in a tin case provided with a small hole, and this hole is soldered up when all the air in the case has been replaced by steam. By this method they may be kept for years without putrefying, fermenting, or getting mouldy. Now this is not because oxygen is excluded, inasmuch as it is now proved that free oxygen is not necessary for either fermentation or putrefaction. It is not because the tins are exhausted of air, for *Vibriones* and *Bacteria* live, as Pasteur has shown, without air or free oxygen. It is not because the boiled meats or vegetables are not putrescible or fermentable, as those who have had the misfortune to be in a ship supplied with unskilfully closed tins well know. What is it, therefore, but the exclusion of germs? I think that Abiogenists are bound to answer this question before they ask us to consider new experiments of precisely the same order.

And in the next place, if the results of the experiments I refer to are really trustworthy, it by no means follows that Abiogenesis has taken place. The resistance of living matter to heat is known to vary within considerable limits, and to depend, to some extent, upon the chemical and physical qualities of the surrounding medium. But if, in the present state of science, the alternative is offered us, either germs can stand a greater heat than has been supposed, or the molecules of dead matter, for no valid or intelligible reason that is assigned, are able to rearrange themselves into living bodies, exactly such as can be demonstrated to be frequently produced in another way, I cannot understand how choice can be, even for a moment, doubtful.

But though I cannot express this conviction of mine too strongly, I must carefully guard myself against the supposition

* "Lectures to Working Men on the Causes of the Phenomena of Organic Nature," 1863.

† Infusion of hay treated in the same way yields similar results; but as it contains organic matter the argument which follows cannot be based upon it.

* For a full account of the most recent series of experiments of this description see Dr. H. C. Bastian's paper in NATURE, No. xxxv., p. 170; No. xxxvi., p. 193; and No. xxxvii., p. 219.—ED.

that I intend to suggest that no such thing as Abiogenesis ever has taken place in the past or ever will take place in the future. With organic chemistry, molecular physics, and physiology yet in their infancy, and every day making prodigious strides, I think it would be the height of presumption for any man to say that the conditions under which matter assumes the properties we call "vital" may not, some day, be artificially brought together. All I feel justified in affirming is that I see no reason for believing that the feat has been performed yet.

And looking back through the prodigious vista of the past, I find no record of the commencement of life, and therefore I am devoid of any means of forming a definite conclusion as to the conditions of its appearance. Belief, in the scientific sense of the word, is a serious matter, and needs strong foundations. To say, therefore, in the admitted absence of evidence, that I have any belief as to the mode in which the existing forms of life have originated, would be using words in a wrong sense. But expectation is permissible where belief is not; and if it were given me to look beyond the abyss of geologically recorded time to the still more remote period when the earth was passing through physical and chemical conditions, which it can no more see again than a man can recall his infancy, I should expect to be a witness of the evolution of living protoplasm from not living matter. I should expect to see it appear under foims of great simplicity, endowed, like existing fungi, with the power of determining the formation of new protoplasm from such matters as ammonium carbonates, oxalates and tartrates, alkaline and earthy phosphates, and water, without the aid of light. That is the expectation to which analogical reasoning leads me; but I beg you once more to recollect that I have no right to call my opinion anything but an act of philosophical faith.

So much for the history of the progress of Redi's great doctrine of Biogenesis, which appears to me, with the limitations I have expressed, to be victorious along the whole line at the present day.

As regards the second problem offered to us by Redi, whether Xenogenesis obtains, side by side with Homogenesis; whether, that is, there exist not only the ordinary living things, giving rise to offspring which run through the same cycle as themselves, but also others, producing offspring which are of a totally different character from themselves, the researches of two centuries have led to a different result. That the grubs found in galls are no product of the plants on which the galls grow, but are the result of the introduction of the eggs of insects into the substance of these plants, was made out by Vallisnieri, Reaumur, and others, before the end of the first half of the eighteenth century. The tapeworms, bladderworms, and flukes continued to be a stronghold of the advocates of Xenogenesis for a much longer period. Indeed, it is only within the last thirty years that the splendid patience of Von Siebold, Van Beneden, Leuckart, Küchenmeister, and other helminthologists, has succeeded in tracing every such parasite, often through the strangest wanderings and metamorphoses, to an egg derived from a parent, actually or potentially like itself; and the tendency of inquiries elsewhere has all been in the same direction. A plant may throw off bulbs, but these, sooner or later, give rise to seeds or spores, which develop into the original form. A polype may give rise to Medusæ, or a pluteus to an Echinoderm, but the Medusa and the Echinoderm give rise to eggs which produce polypes or plutei, and they are therefore only stages in the cycle of life of the species.

But if we turn to pathology it offers us some remarkable approximations to true Xenogenesis.

As I have already mentioned, it has been known since the time of Vallisnieri and of Reaumur, that galls in plants, and tumours in cattle, are caused by insects, which lay their eggs in those parts of the animal or vegetable frame of which these morbid structures are outgrowths. Again, it is a matter of familiar experience to everybody that mere pressure on the skin will give rise to a corn. Now the gall, the tumour, and the corn are parts of the living body, which have become, to a certain degree, independent and distinct organisms. Under the influence of certain external conditions, elements of the body, which should have developed in due subordination to its general plan, set up for themselves and apply the nourishment which they receive to their own purposes.

From such innocent productions as corns and warts, there are all gradations to the serious tumours which, by their mere size and the mechanical obstruction they cause, destroy the organism out of which they are developed; while, finally, in those terrible structures known as cancers, the abnormal growth has acquired powers of reproduction and multiplication, and is only morphologically distinguishable from the parasite worm, the life of which is neither more nor less closely bound up with that of the infested organism.

If there were a kind of diseased structure, the histological elements of which were capable of maintaining a separate and independent existence out of the body, it seems to me that the shadowy boundary between morbid growth and Xenogenesis would be effaced. And I am inclined to think that the progress of discovery has almost brought us to this point already. I have been favoured by Mr. Simon with an early copy of the last published of the valuable "Reports on the Public Health," which, in his capacity of their medical officer, he annually presents to the Lords of the Privy Council. The appendix to this report contains an introductory essay "On the Intimate Pathology of Contagion," by Dr. Burdon Sanderson, which is one of the clearest, most comprehensive, and well-reasoned discussions of a great question which has come under my notice for a long time. I refer you to it for details and for the authorities for the statements I am about to make.

You are familiar with what happens in vaccination. A minute cut is made in the skin, and an infinitesimal quantity of vaccine matter is inserted into the wound. Within a certain time a vesicle appears in the place of the wound, and the fluid which distends this vesicle is vaccine matter, in quantity a hundred or a thousandfold that which was originally inserted. Now what has taken place in the course of this operation? Has the vaccine matter, by its irritative property, produced a mere blister, the fluid of which has the same irritative property? Or does the vaccine matter contain living particles, which have grown and multiplied where they have been planted? The observations of M. Chauveau, extended and confirmed by Dr. Sanderson himself, appear to leave no doubt upon this head. Experiments, similar in principle to those of Helmholtz on fermentation and putrefaction, have proved that the active element in the vaccine lymph is non-diffusible, and consists of minute particles not exceeding $\frac{1}{20000}$ of an inch in diameter, which are made visible in the lymph by the microscope. Similar experiments have proved that two of the most destructive of epizootic diseases, sheep-pox and glanders, are also dependent for their existence and their propagation upon extremely small living solid particles, to which the title of *microzymes* is applied. An animal suffering under either of these terrible diseases is a source of infection and contagion to others, for precisely the same reason as a tub of fermenting beer is capable of propagating its fermentation by "infection," or "contagion," to fresh wort. In both cases it is the solid living particles which are efficient; the liquid in which they float, and at the expense of which they live, being altogether passive.

Now arises the question, are these microzymes the results of *Homogenesis*, or of *Xenogenesis*; are they capable, like the *Torulæ* of yeast, of arising only by the development of pre-existing germs; or may they be, like the constituents of a nut-gall, the results of a modification and individualisation of the tissues of the body in which they are found, resulting from the operation of certain conditions? Are they parasites in the zoological sense, or are they merely what Virchow has called "heterologous growths"? It is obvious that this question has the most profound importance, whether we look at it from a practical or from a theoretical point of view. A parasite may be stamped out by destroying its germs, but a pathological product can only be annihilated by removing the conditions which give rise to it.

It appears to me that this great problem will have to be solved for each zymotic disease separately, for analogy cuts two ways. I have dwelt upon the analogy of pathological modification, which is in favour of the xenogenetic origin of microzymes; but I must now speak of the equally strong analogies in favour of the origin of such pestiferous particles by the ordinary process of the generation of like from like.

It is, at present, a well-established fact that certain diseases, both of plants and of animals, which have all the characters of contagious and infectious epidemics, are caused by minute organisms. The smut of wheat is a well-known instance of such a disease, and it cannot be doubted that the grape-disease and the potato-disease fall under the same category. Among animals, insects are wonderfully liable to the ravages of contagious and infectious diseases caused by microscopic *Fungi*.

In autumn, it is not uncommon to see flies, motionless upon a window-pane, with a sort of magic circle, in white, drawn round them. On microscopic examination, the magic circle is found to consist of innumerable spores, which have been thrown off in all directions by a minute fungus called *Empusa muscæ*, the spore-forming filaments of which stand out like a pile of velvet from the body of the fly. These spore-forming filaments are connected with others which fill the interior of the fly's body like so much fine wool, having eaten away and destroyed the creature's viscera. This is the full-grown condition of the *Empusa*. If traced back to its earlier stages, in flies which are still active, and to all appearance healthy, it is found to exist in the form of minute corpuscles which float in the blood of the fly. These multiply and lengthen into filaments, at the expense of the fly's substance; and when they have at last killed the patient, they grow out of its body and give off spores. Healthy flies shut up with diseased ones catch this mortal disease and perish like the others. A most competent observer, M. Cohn, who studied the development of the *Empusa* in the fly very carefully, was utterly unable to discover in what manner the smallest germs of the *Empusa* got into the fly. The spores could not be made to give rise to such germs by cultivation; nor were such germs discoverable in the air, or in the food of the fly. It looked exceedingly like a case of Abiogenesis, or, at any rate, of Xenogenesis; and it is only quite recently that the real course of events has been made out. It has been ascertained, that when one of the spores falls upon the body of a fly, it begins to germinate and sends out a process which bores its way through the fly's skin; this, having reached the interior cavities of its body, gives off the minute floating corpuscles which are the earliest stage of the *Empusa*. The disease is "contagious," because a healthy fly coming in contact with a diseased one, from which the spore-bearing filaments protrude, is pretty sure to carry off a spore or two. It is "infectious" because the spores become scattered about all sorts of matter in the neighbourhood of the slain flies.

The silkworm has long been known to be subject to a very fatal and infectious disease called the *Muscardine*. Audouin transmitted it by inoculation. This disease is entirely due to the development of a fungus, *Botrytis Bassiana*, in the body of the caterpillar; and its contagiousness and infectiousness are accounted for in the same way as those of the fly-disease. But of late years a still more serious epizootic has appeared among the silkworms; and I may mention a few facts which will give you some conception of the gravity of the injury which it has inflicted on France alone.

The production of silk has been for centuries an important branch of industry in Southern France, and in the year 1853 it had attained such a magnitude that the annual produce of the French sericulture was estimated to amount to a tenth of that of the whole world, and represented a money-value of 117,000,000 of francs, or nearly five millions sterling. What may be the sum which would represent the money-value of all the industries connected with the working up of the raw silk thus produced is more than I can pretend to estimate. Suffice it to say that the city of Lyons is built upon French silk as much as Manchester was upon American cotton before the civil war.

Silkworms are liable to many diseases; and even before 1853 a peculiar epizootic, frequently accompanied by the appearance of dark spots upon the skin (whence the name of "Pébrine" which it has received), had been noted for its mortality. But in the years following 1853 this malady broke out with such extreme violence, that, in 1858, the silk-crop was reduced to a third of the amount which it had reached in 1853; and, up till within the last year or two, it has never attained half the yield of 1853. This means not only that the great number of people engaged in silk growing are some thirty millions sterling poorer than they might have been; it means not only that high prices have had to be paid for imported silkworm eggs, and that, after investing his money in them, in paying for mulberry-leaves and for attendance, the cultivator has constantly seen his silkworms perish and himself plunged in ruin; but it means that the looms of Lyons have lacked employment, and that for years enforced idleness and misery have been the portion of a vast population which, in former days, was industrious and well to do.

In 1858 the gravity of the situation caused the French Academy of Sciences to appoint Commissioners, of whom a distinguished naturalist, M. de Quatrefages, was one, to inquire into the nature of this disease, and, if possible, to devise some means of staying the plague. In reading the Report* made by M. de Quatrefages in 1859, it is exceedingly interesting to observe that his elaborate study of the Pébrine forced the conviction upon his mind that, in its mode of occurrence and propagation, the disease of the silkworm is, in every respect, comparable to the cholera among mankind. But it differs from the cholera, and so far is a more formidable disease, in being hereditary, and in being, under some circumstances, contagious as well as infectious.

The Italian naturalist, Filippi, discovered in the blood of the silkworms affected by this strange disease a multitude of cylindrical corpuscles, each about $\frac{1}{6000}$ of an inch long. These have been carefully studied by Lebert, and named by him *Panhistophyton*; for the reason that in subjects in which the disease is strongly developed, the corpuscles swarm in every tissue and organ of the body, and even pass into the undeveloped eggs of the female moth. But are these corpuscles causes, or mere concomitants, of the disease? Some naturalists took one view and some another; and it was not until the French Government, alarmed by the continued ravages of the malady, and the inefficiency of the remedies which had been suggested, dispatched M. Pasteur to study it, that the question received its final settlement; at a great sacrifice, not only of the time and peace of mind of that eminent philosopher, but, I regret to have to add, of his health.*

But the sacrifice has not been in vain. It is now certain that this devastating, cholera-like Pébrine is the effect of the growth and multiplication of the *Panhistophyton* in the silkworm. It is contagious and infectious because the corpuscles of the *Panhistophyton* pass away from the bodies of the diseased caterpillars, directly or indirectly, to the alimentary canal of healthy silkworms in their neighbourhood; it is hereditary, because the corpuscles enter into the eggs while they are being formed, and consequently are carried within them when they are laid; and for this reason, also, it presents the very singular peculiarity of being inherited only on the mother's side. There is not a single one of all the apparently capricious and unaccountable phenomena presented by the Pébrine, but has received its explanation from the fact that the disease is the result of the presence of the microscopic organism, *Panhistophyton*.

Such being the facts with respect to the Pébrine, what are the indications as to the method of preventing it? It is obvious that this depends upon the way in which the *Panhistophyton* is generated. If it may be generated by Abiogenesis, or by Xenogenesis, within the silkworm or its moth, the extirpation of the disease must depend upon the prevention of the occurrence of the conditions under which this generation takes place. But if, on the other hand, the *Panhistophyton* is an independent organism, which is no more generated by the silkworm than the mistletoe is generated by the oak or the apple-tree on which it grows, though it may need the silkworm for its development in the same way as the mistletoe needs the tree, then the indications are totally different. The sole thing to be done is to get rid of and keep away the germs of the *Panhistophyton*. As might be imagined, from the course of his previous investigations, M. Pasteur was led to believe that the latter was the right theory; and, guided by that theory, he has devised a method of extirpating the disease, which has proved to be completely successful wherever it has been properly carried out.

There can be no reason, then, for doubting that, among insects, contagious and infectious diseases, of great malignity, are caused by minute organisms which are produced from pre-existing germs, or by homogenesis; and there is no reason, that I know of, for believing that what happens in insects may not take place in the highest animals. Indeed, there is already strong evidence that some diseases of an extremely malignant and fatal character to which man is subject, are as much the work of minute organisms as is the Pébrine. I refer for this evidence to the very striking facts adduced by Professor Lister in his various well-known publications on the antiseptic method of treatment. It seems to me impossible to rise from the perusal of those publications without a strong conviction that the lamentable mortality which so frequently dogs the footsteps of the most skilful operator, and those deadly consequences of wounds and injuries which seem to haunt the very walls of great hospitals, and are, even now, destroying more men than die of bullet or bayonet, are due to the importation of minute organisms into wounds, and their increase and multiplication; and that the surgeon who saves most lives will be he who best works out the practical consequences of the hypothesis of Redi.

* Etudes sur les Maladies Actuelles des Vers à Soie, p. 53.

* In NATURE, No. xxxvi., p. 181, will be found a *résumé*, by Prof. Tyndall, of Pasteur's investigations of the silkworm disease.—ED.

I commenced this Address by asking you to follow me in an attempt to trace the path which has been followed by a scientific idea, in its long and slow progress from the position of a probable hypothesis to that of an established law of nature. Our survey has not taken us into very attractive regions; it has lain, chiefly, in a land flowing with the abominable, and peopled with mere grubs and mouldiness. And it may be imagined with what smiles and shrugs, practical and serious contemporaries of Redi and of Spallanzani may have commented on the waste of their high abilities in toiling at the solution of problems which, though curious enough in themselves, could be of no conceivable utility to mankind.

Nevertheless you will have observed that before we had travelled very far upon our road there appeared, on the right hand and on the left, fields laden with a harvest of golden grain, immediately convertible into those things which the most sordidly practical of men will admit to have value—viz., money and life.

The direct loss to France caused by the Pébrine in seventeen years cannot be estimated at less than fifty millions sterling; and if we add to this what Redi's idea, in Pasteur's hands, has done for the wine-grower and for the vinegar-maker, and try to capitalise its value, we shall find that it will go a long way towards repairing the money losses caused by the frightful and calamitous war of this autumn. And as to the equivalent of Redi's thought in life, how can we over-estimate the value of that knowledge of the nature of epidemic and epizootic diseases, and consequently of the means of checking, or eradicating, them, the dawn of which has assuredly commenced?

Looking back no further than ten years, it is possible to select three (1863, 1864, and 1869) in which the total number of deaths from scarlet-fever alone amounted to ninety thousand. That is the return of killed, the maimed and disabled being left out of sight. Why, it is to be hoped that the list of killed in the present bloodiest of all wars will not amount to more than this! But the facts which I have placed before you must leave the least sanguine without a doubt that the nature and the causes of this scourge will, one day, be as well understood as those of the Pébrine are now; and that the long-suffered massacre of our innocents will come to an end.

And thus mankind will have one more admonition that "the people perish for lack of knowledge;" and that the alleviation of the miseries, and the promotion of the welfare, of men must be sought, by those who will not lose their pains, in that diligent, patient, loving study of all the multitudinous aspects of Nature, the results of which constitute exact knowledge, or Science. It is the justification and the glory of this great meeting that it is gathered together for no other object than the advancement of the moiety of science which deals with those phenomena of nature which we call physical. May its endeavours be crowned with a full measure of success!

PROFESSOR H. E. ROSCOE'S OPENING ADDRESS TO SECTION B.

GENTLEMEN,—In the midst of the excitement of the horrible war in which the two most scientific nations of the continent are now plunged, and in which even the Professors of Chemistry and their students take a humane part, let us endeavour to turn our thoughts into channels more congenial to the scientific inquirer, and allow me to recount to you, as far as I am able, the peaceful victories which, since our last meeting in Exeter, have been achieved in our special department of chemistry. And here may I remind you of the cosmopolitan character of science, of the fact that it is mainly to the brotherly intercourse of those interested in science, and in its applications to the arts and manufactures in different countries, that we must look as the small but living fire which in the end will surely serve to melt down national animosities, and to render impossible the breaking out of disasters so fatal to the welfare of humanity as that of which we are now unfortunately the spectators.

With regard to the position of chemical science at the present moment, it will not take a careful observer long to see, that in spite of the numerous important and brilliant discoveries of which every year has to boast, we are really but very imperfectly acquainted with the fundamental laws which regulate chemical actions, and that our knowledge of the ultimate constitution of matter upon which those laws are based is but of the most elementary nature. In proof of this, I need only refer to the different opinions expressed by our leading chemists in a discussion which lately took place at the Chemical Society on the subject of the Atomic Theory. The President (Dr. Williamson) delivered a very interesting lecture in which the existence of atoms was treated as "the very life of chemistry." Dr. Frankland, on the other hand, states, that he cannot understand action at a distance between matter separated by a vacuous space, and, although generally granting that the atomic theory explains chemical facts, yet he is not to be considered as a blind believer in the theory, or as unwilling to renounce it if anything better presented itself. Sir B. C. Brodie and Dr. Odling both agree, that the science of chemistry neither requires nor proves the atomic theory; whilst the former points out that the true basis of this science is to be sought in the investigation of the laws of gaseous combination, or the study of the capacity of bodies for heat, rather than in committing ourselves to assertions incapable of proof by chemical means.

Agreeing in the main myself with the opinions of the last chemists, and believing that we must well distinguish between fact and theory, I would remind you that Dalton's discovery of the laws of multiple and reciprocal proportions (I use Dr. Odling's phraseology), as well as the differences in the power of hydrogen replacement in hydrochloric acid water, ammonia, and marsh gas, *are facts*, whilst the explanation upon the assumption of atoms is, as far as chemistry is as yet advanced, *a theory*.

If, however, the existence of atoms cannot be *proved* by chemical phenomena, we must remember that the assumption of the atomic theory explains chemical facts, as the undulatory theory gives a clear view of the phenomena of light; thus, for instance, one of the most important facts and relations of modern chemistry, which it appears difficult if not impossible to explain without the assumption of atoms, is that of Isomerism. How otherwise than by a different arrangement of the single constituent particles are we to account for several distinct substances in which the proportions of carbon, hydrogen, and oxygen are the same? Why, for instance, should 48 parts, by weight, of carbon, 10 of hydrogen, and 16 of oxygen, united together, be capable of existing as three different chemical substances, unless we presuppose a different statical arrangement of the parts by which these differences in the deportment of the whole are rendered possible. If, then, it be true that chemistry cannot give us positive information as to whether matter is infinitely divisible and therefore continuous, or consists of atoms and is discontinuous, we are in some degree assisted in this inquiry by deductions from physical phenomena which have been recently pointed out by the genius of Sir William Thomson. He argues from four different classes of physical phenomena, and comes to the conclusion not only that matter is discontinuous, and therefore that atoms and molecules do exist, but he even attempts to form an idea of the size of these molecules, and he states that in any ordinary liquid, transparent or seemingly opaque solid, the mean distance between the centres of contiguous molecules is less than the 100 millionth, and greater than the 2,000 millionth of a centimetre. Or, to form a conception of this coarse-grainedness, imagine a raindrop, or globe of glass as large as a pea, to be magnified up to the size of the earth, each constituent molecule being magnified in the same proportion, the magnified structure would be coarser grained than a heap of small shot, but probably less coarse grained than a heap of cricket balls. There is, however, another class of physical considerations which renders the existence of indivisible particles more than likely. I refer to the mechanical theory of gases, by means of which, thanks to the labours of eminent English and German philosophers, all the physical properties of gases—their equal expansion by heat, the laws of diffusion, the laws of alteration of volume under pressure—can be shown to follow from the simple laws of mechanical motion. This theory, however, presupposes the existence of molecules, and in this direction, again, we find confirmation of the real existence of Dalton's atoms. Indeed, it has been proved that the average velocity with which the particles of oxygen, nitrogen, or common air are continually projected forwards amounts, at the ordinary atmospheric pressure, to 50,000 centimetres per second, whilst the average number of impacts of each of these molecules is 5,000 million per second.

The mention of the molecular motions of gases will recall to the minds of all present the great loss which English science has this year sustained in the death of the discoverer of the laws of gaseous diffusion. Throughout his life Graham's aim was the advancement of our knowledge in the special subject of the molecular properties of gases. With this intent he unceasingly laboured up to the moment of his death, in spite of failing health and pressure of official business, unfolding for posterity some of the most difficult as well as the most interesting secrets of nature in this branch of our science. "What do you think," he writes to Hoffmann, "of metallic hydrogen, a white magnetic metal?" and yet now, through his labours, the fact of the condensation of hydrogen in the solid state by metallic palladium, and to a less extent by other metals, has become familiar to all of us. Then, again, I would remind you of Graham's recent discovery of the occlusion of hydrogen gas in certain specimens of meteoric iron, whilst earth-manufactured iron contains, not hydrogen, but absorbed carbonic oxide gas, proving that the meteorite had probably been thrown out from an atmosphere of incandescent hydrogen, existing under very considerable pressure, and therefore confirming in a remarkable degree the conclusions to which spectrum analysis had previously led us. The position in the ranks of British science left by Graham's death will not be easily filled up. He accomplished to a certain extent for dynamical chemistry what Dalton did for statical chemistry; and it is upon his experimental researches in molecular chemistry that Graham's permanent fame as one of England's greatest chemists, will rest.

As closely connected with the above subjects I have next to mention a most important research by Dr. Andrews, of Belfast, which, marking an era in the history of gases, shows us how our oldest and most cherished notions must give way before the touchstone of experiment. No opinion would appear to have been more firmly established than that of the existence of three separate states or conditions of matter, viz. the solid, the liquid, and the gaseous. A body capable of existing in two or more of these states was thought to pass suddenly from one to the other by absorption or emission of heat, or by alterations of the superincumbent pressure. Dr. Andrews has shown us how false are our views on this fundamental property of matter, for he has proved that a large number of, and probably all, easily condensible gases or vapours possess a critical point of temperature at and above which no increase of pressure can be made to effect a change into what we call the liquid state, the body remaining as a homogeneous fluid. Whilst below this critical temperature certain increase of pressure always effects a separation into two layers of liquid and gaseous matter. Thus with carbonic acid the point of critical temperature is 30°·92 C., and with each given substance this point is a specific one, each vapour exhibiting rapid changes of volume and flickering movements when the temperature or pressure was changed, but showing no separation into two layers. Under these circumstances it is impossible to say that the body exists either in the state of a gas or of a liquid; it appears to be in a condition intermediate between the two. Thus carbonic acid under the pressure of 108 atmospheres, and at 35° 5′C., is reduced to $\frac{1}{430}$ of the volume which it occupies at one atmosphere: it has undergone a regular and unbroken contraction, and it is a uniform fluid. If we now reduce the temperature below 31° C., the liquid condition is assumed without any sudden change of volume or any abrupt evolution of heat. We can scarcely too highly estimate the value of the researches of Andrews.

As examples of the power which modern methods of research give of grappling with questions which only a few years ago were thought to be insoluble, I may quote the beautiful observations, now well known, by which Lockyer determined the rate of motion on the sun's surface, together with those of Frankland and Lockyer respecting the probable pressure acting in the different layers of the solar atmosphere; and, lastly, the results obtained by Zöllner respecting solar physics, and especially the probable absolute temperature of the sun's atmosphere, as well as that of the internal molten mass. These last results are so interesting and remarkable, as being arrived at by the combination of recent spectroscopic observation with high mathematical analysis, that I may perhaps be permitted shortly to state them. Starting from the fact of the eruptive nature of a certain class of solar protuberances, Zöllner thinks that the extraordinary rapidity with which these red flames shoot forth proves that the hydrogen of which they are mainly composed must have burst out from under great pressure; and, if so, the hydrogen must have been confined by a zone or layer of liquid from which it breaks loose. Assuming the existence of such a layer of incandescent liquid, then applying to the problem the principles and methods of the mechanical theory of gases, and placing in his formulæ the data of pressure and rate of motion as observed by Lockyer on the sun's surface, Zöllner arrives at the conclusion that the difference of pressure needed to produce an explosion capable of projecting a prominence to the height of 3·0 minutes above the sun's surface (a height not unfrequently noticed) is 4,070,000 atmospheres. This enormous pressure is attained at a depth of 139 geographical miles under the sun's surface, or at that of the $\frac{1}{668}$ part of the sun's semi-diameter. In order to produce this gigantic pressure, the difference in temperature between the enclosed hydrogen and that existing in the solar atmosphere amounts to 74,710° C.! In a similar way Zöllner calculates the approximative absolute temperature of the sun's atmosphere, which he finds to be 27,700° C.; a temperature about eight times as high as that given by Bunsen for the oxyhydrogen flame, and one at which iron must exist in a permanently gaseous form.

Passing on to more purely chemical subjects, we find this year signalized by the redetermination of a most important series of chemical constants, viz. that of the heat of chemical combination by Julius Thomsen, of Copenhagen. This conscientious experimentalist asserts that the measurements of the heat evolved by neutralizing acids and bases hitherto considered most correct, viz. those made with a mercury calorimeter by Favre and Silbermann, differ from the truth by 12 per cent.; whilst the determination by these experimenters of the heat of solution of salts is frequently 50 per cent. wrong. As the result of his numerous experiments, Thomsen concludes that when a molecule of acid is neutralized by caustic alkali the heat evolved increases nearly proportionally to the quantity of alkali added until this reaches 1, $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$ of a molecule of alkali, according as the acid is mono-, di-, tri-, or tetra-basic. Exceptions to the law are exhibited by silicic, and also partly by boracic, orthophosphoric, and arsenic acids. In the two latter, the heat of combination is proportional for the two first atoms of replaceable hydrogen, but much less for the third atom. A second unexpected conclusion which Thomsen draws from his calorific determinations is, that sulphuretted hydrogen is a monobasic acid, and that its rational formula is therefore HSH.

Another important addition made to chemistry since our last meeting is a new, very powerful, and very simple form of galvanic battery discovered, though not yet described, by Bunsen. In this second Bunsen's battery only one liquid, a mixture of sulphuric and chromic acids, and therefore no porous cells, are employed. The plates of zinc and carbon can all be lowered at once into the liquid and raised again at will. The electromotive force of this battery is to that of Grove (the most powerful of known forms) as 25 to 18; it evolves no fumes in working, and can be used for a very considerable length of time without serious diminution of the strength of the current, so that Bunsen writes me that no one who has once used the new battery will ever think of again employing the old forms. I had hoped to be able to exhibit to the Section this important improvement in our means of producing a strong current; but war has demanded the use of other batteries, and Bunsen has been unable to send me a set of his new cells.

Amongst the marked points of interest and progress in inorganic chemistry during the past year, we have to notice the preparation of a missing link amongst the oxy-sulphur acids by Schützenberger. It is the lowest known, and may be called Hydro-Sulphurous Acid, $H_2 SO_2$. The sodium salt, $Na H SO_2$, is obtained by the action of zinc on the bisulphite. As might be expected, it possesses very powerful reducing properties, and bleaches indigo rapidly. The metallic vanadates have also been carefully examined, and the existence of three distinct series of salts proved, corresponding to the phosphates, viz. the ortho or tribasic vanadates, the pyro or tetra-basic vanadates, and the meta or monobasic vanadates. Of these, the ortho-salts are most stable at a high temperature, whilst at the ordinary atmospheric temperature the meta-salts are most stable. In the phosphorus series, as is well known, the order of stability is the reverse; and thus the points of analogy and of difference between phosphorus and vanadium become gradually apparent.

As an illustration of the results of modern organic research—for in viewing the year's progress in this ever-widening branch of chemistry it is impossible to do more than give a few illustrations—I may quote Baeyer's remarkable investigations on Mellitic Acid. Originally discovered by Klaproth in honeystone or mellite (a substance which yet remains the only source of the acid), mellitic was supposed to be a four-carbon acid. Baeyer has quite recently shown that the acid contains 12 atoms of carbon, or has a molecular weight three times as great as was originally supposed. He has shown that mellitic acid is benzolhexacarbonic acid, $C_{12} H_6 O_{12}$, or benzol in which the 6 atoms of hydrogen are replaced by the monad radical carboxl (C O O H); as benzoic is Benzol Monocarbonic acid, or benzol in which one of hydrogen is replaced by carboxyl. The most interesting portion of Baeyer's research, however, lies in the intermediate acids, partly new and partly acids already prepared, which he has shown lie between mellitic and benzoic acid, and in which from 1 to 6 atoms of hydrogen in benzol are respectively replaced by carboxyl. Nor is this all, for he has proved that, with two exceptions, each of these six acids is capable of existing in three isomeric modifications, thus giving us an insight into the arrangement of the molecule of these aromatic compounds. For the simplest mode of explaining these numerous isomers is that given by Baeyer in the different order in which the several atoms of hydrogen in the benzol molecule are replaced. Thus in the first or ortho series, the hydrogen atoms in benzol being numbered in regular succession, they are replaced in the same regular succession. In the second or meta series, the order is 1, 2, 3, 5, &c., whilst the third or para series take open order as 1, 2, 4, 5.

Thus we have :—

	Ortho Series.	Para Series.	Meta Series.
$C_{12}H_6O_{12}$ Hexeabasic	Mellitic or Benzolhexacarbonic.	—	—
$C_{11}H_6O_{10}$ Penta .	Unknown.	—	—
$C_{10}H_6O_8$ Tetra . .	Pyromellitic or Benzoltetracarbonic.	Isopyromellitic.	Unknown.
$C_9H_6O_6$ Tri . . .	Trimesinic, or Benzoltricarbonic.	Hemimellitic.	Trimellitic.
$C_8H_6O_4$ Di . . .	Phtalic or Benzoldicarbonic	Isophtalic.	Tetraphtalic.
$C_7H_6O_2$ Mono . .	Benzoic or Benzol Monocarbonic.	—	—

Amongst the most interesting series of new organic bodies are those in which tetrad silicon partly replaces carbon. Our knowledge of these substances is gradually becoming more complete; the last new member prepared by Friedel and Ladenburg is silico-propionic acid $C_2H_5SiO_2H$, the first of a series of carbo-silicic acids containing the radical SiO_2H.

The interesting researches of Matthiessen and Wright on Morphine and Codeine have thrown a new light on the constitution of these opium alkaloids. Treated with hydrochloric acid, morphine loses one molecule of water, and gives rise to a new base, called apomorphine. Thus:

$$\underset{\text{Morphine.}}{C_{17}H_{19}NO_3} = H_2O + \underset{\text{Apomorphine}}{C_{17}H_{17}NO_2}$$

which differs in a remarkable manner from morphine, both in its chemical and physiological actions, being soluble in alcohol, ether, and chloroform, whereas morphine is nearly insoluble, and acting as the most powerful emetic known, $\frac{1}{10}$th of a grain producing vomiting in less than ten minutes. Codeine, which only differs from morphine by CH_2, also yields apomorphine on treatment at a high temperature with hydrochloric acid, methylchloride being at the same time eliminated.

An important application of the dehydrating and carbon condensing power of zinc chloride, long known in its action on alcohol to produce ether, has been made by Kekulé in the reduplication of aldehyde to form croton-aldehyde with loss of water.

$$2\,(C_2H_4O) - H_2O = C_4H_6O.$$

This croton-aldehyde is also probably formed as an intermediate product in the manufacture of chloral from aldehyde, and gives rise to the formation of crotonchloral

$$C_4H_3Cl_3O.$$

The discovery of the sedative properties of chloral-hydrate by Liebreich marks an era in medical chemistry second only to the discovery of the anæsthetic properties of chloroform. Chloral not only combines with water to form a solid hydrate, but also forms solid alcoholates; but these bodies appear to possess quite different medicinal properties from the hydrate, and it is important that no alcoholate should be present in the officinal preparation. The chemistry of colouring matters has lately received an enormous impetus in the working of the brilliant discovery of the production of artificial alizarine, the colouring matter of madder, by Messrs. Graebe and Liebermann. This discovery, announced at our last meeting, is of the highest importance, whether we regard its scientific interest or its practical and commercial value, and it differs from all the former results which have been brought about by the application of science to the production of colouring matters, inasmuch as this has reference to the artificial production of a natural vegetable colouring substance which has been used as a dye from time immemorial, and which is still employed in enormous quantities for the production of the pink, purple, and black colours which are seen everywhere on printed calicoes. During the past year much progress has been made in the practical working of the processes by which this colouring matter is obtained from the hydrocarbon anthracene contained in coal tar, and new and more economical plans for effecting the transformation have been independently proposed by Perkin and Caro, and Schorlemmer and Dale. The theoretical investigation of the reaction, and especially of the nature of some other peculiar products formed in addition to alizarine, which render the artificial colouring matter different from natural alizarine, has been carried out by Mr. Perkin, and especially by Dr. Schunck. As we are promised papers on this subject from both these gentlemen, I need not now enter further into these interesting questions.

The surest proof of perfection in a manufacture is the degree in which the waste products are utilised, and in which the processes are made continuous. One by one the imperfections of the original discovery are made to disappear, and the products which were wasted become sources of profit, whilst in many cases their utilisation alone renders possible the continuance of the manufacture in the midst of a rapidly increasing district. The Section will have the opportunity of inspecting the practical working of at least two of the most valuable of these new processes which have lately been introduced into our most important chemical manufacture, that of alkali. The first of these has been at work for some time; it is that of the recovery of sulphur from the vat waste, that *bête noire* of the alkali makers and of their neighbours. Dr. Mond has now, I believe, satisfactorily solved the difficult problem of *economically* regaining the sulphur by oxidising the insoluble monosulphide of calcium in the lixiviating vat itself to the soluble hyposulphite, and decomposing this by hydrochloric acid, when all the sulphur is deposited as a white powder.

The second of these discoveries relates to the recovery or regeneration of the black oxide of manganese, used for the evolution of chlorine in the manufacture of bleaching powder. This subject has long attracted the attention of chemists, and a feasible though somewat costly process, that of Dunlop, has been at work for some time at Messrs. Tennant's works at St. Rollox.

During the last year a very beautifully simple and economical process, proposed by Mr. Weldon, and first successfully carried out on a practical scale in Messrs. Gamble's works at St. Helen's, has quickly obtained recognition, and is now worked by more than thirty-seven firms throughout the kingdom. The principle upon which this process depends was explained by Mr. Weldon at the Exeter meeting. It depends on the fact that although when alone the lower oxides of manganese cannot be oxidised by air and steam under the ordinary pressure to the state of dioxide, yet that this is possible when one molecule of lime is present to each molecule of oxide of manganese. The manganous oxide is precipitated from the still liquors with the above excess of lime, and by the action of steam and air on this a black powder consisting of a compound of manganese, dioxide and lime, MnO_2CaO, or calcium manganite, is formed. This, of course, is capable of again generating chlorine, on addition of hydrochloric acid, and thus the chlorine process is made continuous with a working loss of only $2\frac{3}{4}$ per cent. of manganese. The Section will have the advantage of seeing Mond's process at work at Messrs. Hutchinson and Weldon's process, at Messrs. Gaskell, Deacon, and Co.'s, at Widnes.

A third process, which may possibly still further revolutionise the manufacture of bleaching powder, is the direct production of chlorine from hydrochloric acid without the use of manganese at all. In presence of oxygen and of certain metallic oxides, such as oxide of copper, hydrochloric acid gas parts at a red heat with all its hydrogen, water and chlorine being formed. This interesting reaction is employed by Mr. Deacon for the direct manufacture of bleaching powder from the gases issuing directly from the salt-cake furnace. Air is admitted together with hydrochloric acid gas, and the mixture is passed over red-hot bricks impregnated with copper salt. The oxide of copper acts as by contact and remains unaltered, whilst the chlorine, watery vapour, and excess of air pass at once into the lime-chamber. The difficulty in this process is the large volume of diluting nitrogen which accompanies the chlorine, but I believe we shall hear from Mr. Deacon that, notwithstanding this drawback, he has accomplished his end of making good bleaching powder by this process.

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