THURSDAY, AUGUST 16, 1877

THE BRITISH ASSOCIATION

PLYMOUTH, *Tuesday*

THERE is every sign that the meeting of the British Association for 1877 will be a very successful one as far as attendance is concerned. It is too early in the day to quote figures, but in the spaces of time between the arrival of one London train and another a marked difference may be seen in the reception-room, and the clerks at the tables are already under a state of siege taking subscriptions, issuing tickets, answering innumerable questions, and indeed converting themselves into a long book-shelf of encyclopædias of useful knowledge.

It is a significant fact in connection with the numbers who have secured their tickets for the meeting, that at least half the reserved seats for the President's address and the two evening discourses, have already been applied for and appropriated. The Committee have, however, made a very fair arrangement with regard to these places. On the block plan of the noble Guildhall in which the addresses are to be delivered, a line has been drawn down the centre from the middle of the platform to the back of the hall which is thus divided longitudinally into two equal portions. One of these portions has been reserved for members coming from a distance, and will not be thrown open for general application until to-morrow. By this arrangement local residents applying for tickets have the advantage of priority over one another, but do not, through living on the spot, have any advantage over those arriving early to-morrow, which is the first day of the meeting.

Among early arrivals we noticed several familiar faces, among which may be mentioned Prof. Williamson, the general treasurer, Prof. Redfern, Mr. Glaisher, Mr. J. W. L. Glaisher, Mr. Pengelly, Mr. W. Chandler Roberts, Admiral Sir Erasmus Ommanney, K.C.B., Prof. Gladstone, Mr. Woodward, of the British Museum; but the great bulk of the visitors to the present meeting will be arriving by the late trains this evening and the early trains to-morrow (Wednesday) morning.

The following is the programme of the daily arrangements for the meeting:—

Wednesday, August 15.—The Address by the President, Prof. Allen Thomson, F.R.S., 8 P.M., in the Guildhall.

Thursday, August 16.—Sectional meetings. *Soirée* at 8 o'clock in Guildhall.

Friday, August 17.—Sectional meetings.—Lecture in Guildhall, 8.30 P.M., by Prof. Warington Smyth, F.R.S.

Saturday, August 18.—Sectional meetings. Excursions. Lecture to working men in Guildhall, 7 P.M., by Mr. Preece.

Monday, August 20.—Sectional meetings. Lecture in Guildhall, 8.30 P.M., by Prof. Odling, F.R.S.

Tuesday, August 21.—Sectional meetings.—*Soirée* in Guildhall, 8 P.M.

Wednesday, August 22.—Sectional meetings. Concluding meeting, 2.30 P.M.

Thursday, August 23.—Excursions.

The business of the sections will not be considered until to-morrow's meeting of the committees, but the excursions are pretty well settled. The first excursion-day will be Saturday next, the 18th instant, on which day there will be four excursions, in addition to a visit to Mount Edgcumbe, which will be thrown open to the members of the Association by the kindness of the Earl of Mount Edgcumbe. The other excursions are as follows:—

1. To Lee Moor and Dartmoor. An excursion will be made in waggonettes, by Plympton and Newnham Park, to the Lee Moor China Clay Works, the largest in the West of England. When the works have been inspected the company will be entertained at luncheon by the Messrs. Martin, the proprietors. After luncheon some of the party will inspect the pre-historic remains on the Plym, under the guidance of Mr. C. Spence Bate, F.R.S., one of the vice-presidents of the Association, who, besides his well-known biological memoirs, is the author of a most valuable work upon the remains, both historic and prehistoric, to be found in Devon and Cornwall. Others will visit Shell Top and Pen Beacon, from which the most magnificent views of Devonshire may be had. The party will be limited to one hundred, and application for tickets, which will be free, must be made before twelve o'clock on Friday next.

2. Another excursion for Saturday will be to the ancient city of Exeter. This excursion is by special invitation of the Mayor of Exeter on behalf of the citizens and inhabitants of the neighbourhood. The company, after visiting the Cathedral, Guildhall, and Museum, will be entertained at luncheon by the Mayor and Corporation at four o'clock. This excursion being only by special invitation partakes more or less of a private character, and therefore no tickets can be applied for.

3. One of the most interesting excursions for Saturday will be a visit to the Eddystone Lighthouse, the Plymouth Breakwater, and the Hamoaze, where a visit will be made to the gunnery ship *Cambridge*, to witness the great gun drill and torpedo practice, which will derive an especial interest from the affairs going on in Eastern Europe, in which torpedo warfare seems destined to play a hitherto unrivalled part. After leaving the *Cambridge*, the steamers which have been kindly placed at the disposal of the Association by Sir T. Symonds, the Naval Commander at this port, will go up the Hamoaze and Tamar to the Brunel's Royal Albert Bridge at Saltash, and the industrial training ship, *Mount Edgcumbe*. After leaving the Bridge the party will return down the Hamoaze, and proceed to sea, taking the breakwater on their way; here the work will be explained by one of the engineering staff, after which the boats will go on to the Eddystone Lighthouse, where some of the party will "land," if such it can be called. On the way between the Breakwater and the Lighthouse experiments will be made with Sir William Thomson's pianoforte-wire sounding apparatus, and with his very beautiful compass, which has already been described in these pages, but which we may remind our readers consists of a ring of aluminium which is supported from an agate centre, moulded in an aluminium boss, by means of a lacing of silk thread, so that the ring is kept in its place and concentric with the pivot by the tension of the threads. In this it bears some resemblance to the *tension* or "*spider*" wheels which are now fitted to the best bicycles. The magnetic portion of the instrument consists of two, four, six, or eight magnetised needles of steel wire, supported also to the ring by a lacing of silk threads. To the outer ring is attached the card or dial, which is of paper, and in order to prevent errors through the expansion or contraction of this material due to variations in the humidity of the atmosphere; it is divided radially into small sectors, each of which can expand or contract on its own account, but can have no effect on its neighbours. The advantages of this compass are extreme lightness of the movable portion, whereby friction on the pivot is reduced to a minimum, and variations in the direction of magnetic force, however slight, are indicated by the movement of the card; and owing to the smallness of the needles which are capable of performing the work, Sir William Thomson's compass comes to rest very quickly when disturbing influences are removed, and it is easily adjusted both in its permanent and in its temporary adjustments. After visiting Smeaton's masterpiece of engineering construction, which has stood the fury of the Atlantic storms for so many years, the party will return to Mill-Bay in time for the many private evening engage-

ments for that day. Tickets for this excursion will be freely given to a limited number of members, but none can be issued after Friday the 17th instant.

4. On the same day there will be a dredging excursion in Plymouth Sound and offing, under the direction of the Plymouth Institution. As this district is particularly rich in the crustacea, echinodermata, and the rarer southern fauna, there will no doubt be many applicants for tickets. The boats for this excursion will leave Mill Bay pier at ten o'clock, and the number of the tickets, which must be applied for before twelve o'clock on Friday, will be limited to fifty.

The excursions for Thursday, the 23rd inst., will consist of the following:—

1. Up the River Tamar to visit the Great Devon Consols Copper Mines.
2. To Liskeard, the Cheese-Wring, and the Phœnix and South Carradon Tin Mines.
3. To Totnes, Torquay, and Brixham, including visits to Kent's Cavern, to the Brixham Caves and the experimental works of Mr. Froude at Chelston Cross.
4. To Penryn, Falmouth, Penzance, and the Land's End.

For the first two of these excursions applications for tickets, which are limited, must be made before noon of Wednesday, the 22nd. The Totnes and Torquay excursion is by special invitation of the chairman of the Torquay Local Board of Health, on behalf of the inhabitants of the town, and, as such, is of a semi-private nature.

The "Red Lion" dinner, which is now almost as great an institution as the British Association itself, will come off on Tuesday next, at five o'clock, at Farley's Menagerie, Union Street.

As we intimated last week, Mr. John Edward Henry Gordon, B.A., late of Cambridge, has been appointed as the successor to Mr. Griffith. Mr. Griffith will, however, retain office until the Dublin meeting, Mr. Gordon being his assistant for the year, after which Mr. Gordon will be fully installed as Assistant General Secretary.

Inaugural Address of Prof. Allen Thomson, M.D., LL.D., F.R.S., F.R.S.E., President.

After the long interval of six-and-thirty years the British Association for the Advancement of Science holds its annual meeting, the forty-seventh since its foundation, in this beautiful and interesting locality; and, strangely enough, on this occasion as on the former, it passes from Glasgow to Plymouth. We are delighted to be assembled here, and are even surprised that the Association has been able so long to resist the power of attraction by which it has been gravitating towards this place. While we are prepared to be charmed by the surpassing beauty of its scenery, and know the deep interest of its prehistoric vestiges, its historic memories, and its artistic associations, we have been frequently reminded of its scientific vigilance by the records of its active scientific work; and we are now ready and anxious to witness all we can behold of its energy and success in the application of scientific discovery to the practical arts. Should we, as might be expected in a place hitherto so famous in its relations to our naval and military history, find most prominent those relating to the mechanism of war, we shall still hope that the effect of greater perfection in the engines of destruction may only be the means of rendering peace more permanent and secure.

It is a source of regret to myself, and may be, I fear, a cause of detriment to this meeting that the choice of a President should have fallen upon one whose constant occupation with very special branches of science has fitted him so inadequately for the distinguished position to which he has been called. I can only derive comfort from knowing that, wherever it may be necessary, there are many others present most able to supply what may be wanting on my part; and I must, therefore, at once bespeak their assistance and your indulgence.

I have selected for the subject of the remarks which I am about to offer for your acceptance a biological topic, namely, the "Development of the Forms of Animal Life," with which my studies have been occupied, and which has important bearings on some of the more interesting biological questions now agitating the scientific world. But before proceeding with the discussion of my special subject, it is my desire to call your attention shortly to the remarkable change in the manner of viewing biological questions which has taken place in this country during the last half century—a change so great, indeed, that it can scarcely be fully appreciated except by those who have lived through the period of its occurrence.

In the three earlier decades of this century it was the common belief, in this country at least, shared by men of science as well as by the larger body of persons who had given no special attention to the subject, that the various forms of plants and animals recognized by naturalists in their systematic arrangements of genera and species were permanently fixed and unalterable; that they were not subject to greater changes than might occur as occasional variations, and that such was the tendency to the maintenance of uniformity in their specific characters that, when varieties did arise, there was a natural disposition to the return, in the course of succeeding generations, to the fixed form and nature supposed to belong to the parental stock; and it was also a necessary part of this view of the permanency of species that each was considered to have been originally produced from an individual having the exact form which its descendants ever afterwards retained. To this scientific dogma was further added the quasi-religious view that in the exercise of infinite wisdom and goodness, the Creator, when He called the successive species of plants and animals into existence, conferred upon each precisely the organization and the properties adapting it best for the kind of life for which it was designed in the general scheme of creation. This was the older doctrine of "Direct Creation," of "Teleological Relation," and of "Final Causes"; and those only who have known the firm hold which such views had over the public mind in past times can understand the almost unqualified approbation with which the reasoning on these questions in writings like the Bridgewater Treatises (not to mention older books on Natural Theology) were received in their time, as well as the very opposite feelings excited by every work which presented a different view of the plan of creation.

On the continent of Europe, it is true, some bold speculators, such as Goethe, Oken, Lamarck, and Geoffroy St. Hilaire, had in the end of the last and commencement of this century broached the doctrine that there is in living beings a continuous series of gradations as well as a consistent and general plan of organization; and that the creation, therefore, or origin of the different forms of plants and animals must have been the result of a gradual process of development or of derivation one from another, the whole standing connected together in certain causal relations. But in Britain such views, though known and not altogether repulsive to a few, obtained little favour, and, by some strange process of reasoning, were looked upon by the great majority as little short of impious questionings of the supreme power of the Almighty.

How different is the position of matters in this respect in our day!—when the cautious naturalist receives and adopts with the greatest reserve the statement of fixed and permanent specific characters as belonging to the different forms of organized beings, and is fully persuaded of the constant tendency to variation which all species show even in the present condition of the earth, and of the still greater liability to change which must have existed in the earlier periods of its formation—when the belief prevails that so far from being the direct product of distinct acts of creation, the various forms of plants and animals have been gradually evolved in a slow gradation of increasing complexity; and when it is recognized by a large majority of naturalists that the explanation of this wonderful relation of connection between previously existing and later forms is to be found in the constant tendency to variation during development and growth, and the perpetuation of such variations by hereditary transmission through successive generations in the long but incalculable lapse of the earth's natural mutations. These, as you must all be aware, are in their essential features the views now known as Darwinism, which were first simultaneously brought forward by Wallace and Darwin in 1858, and which, after being more fully elaborated in the works of the latter and ably supported by the former, secured, in the incredibly short space of ten or twelve years, the general approval of a large portion of the scientific world. The change of opinion is, in fact, now such that there are few scientific works on Natural History, whether of a special or more general character, in which the relation which the facts of science bear to the newer doctrines is not carefully pointed out; that, with the general public too, the words "Evolution" and "Development" have ceased to excite the feelings, amounting almost to horror, which they at first produced in the minds of those to whom they were equally unfamiliar and suspicious; and that even in popular literature and ephemeral effusions, direct or metaphorical illustrations are drawn in such terms of the Darwinian theory as "struggle for existence," "natural selection," "survival of the fittest," "heredity," "atavism," and the like.

It cannot be doubted that in this country, as on the Continent, the influence of authority had much to do with the persistence of the older teleological views; and, as has been well remarked by Häckel, one of the ablest and keenest supporters of the modern doctrine, the combined influence more especially of the opinions held by three of the greatest naturalists and biologists who have ever lived, viz., Linnæus, Haller, and Cuvier, men unsurpassed in the learning of their time, and the authors of important discoveries in a wide range of biological science, was decidedly adverse to the free current of speculative thought upon the more general doctrines of biology. And if it were warrantable to attribute so great a change of opinion as that to which I have adverted as occurring in my own time, to the influence of any single intellect, it must be admitted that it is justly due to the vast range and accuracy of his knowledge of scientific facts, the quick appreciation of their mutual interdependence, and above all the unexampled clearness and candour in statement of Charles Darwin.

But while we readily acknowledge the large share which Darwin has had in guiding scientific thought into the newer tracks of biological doctrine, we shall also be disposed to allow that the slow and difficult process of emancipation from the thraldom of dogmatic opinion in regard to a system of creation, and the adoption of large and independent views more consistent with observation, reason, philosophy, and religion, has only been possible under the effect of the general progress of scientific knowledge and the acquisition of sounder methods of applying its principles to the explanation of natural phenomena.

I have already referred to Goethe, Oken, Lamarck, and Geoffroy St. Hilaire as among the most prominent of the earlier pioneers in the modern or reformed conceptions of biological laws. But were it desirable to mark the progress of opinion by quoting other authors and labourers whose contributions have mainly supplied the materials out of which the new fabric has been constructed, I should have to produce a long catalogue of distinguished names, among which would be found those of Lyell and Owen, as earliest shaping the doctrines and guiding opinion in this country, Johannes Müller and von Baer, as taking the places of Haller and Cuvier on the Continent; and a host of other faithful workers in Biology belonging to the earlier part of this century, such as those of G. Treviranus, J. F. Meckel, Carus, and many more.[1] To Huxley more especially, and to Herbert Spencer, the greatest influence on British thought in the same direction is to be ascribed.

Let us hope that in these times, when it has been found necessary to modify the older teleological views to so great an extent, although there may still be much that is unknown, and wide differences of opinion in regard to the nature and sequence of natural phenomena and the mode of their interpretation, all naturalists will now concur in one important principle, viz., that truthful observation and candid judgment must alone be our guides in the interpretation of nature, and that that theory of creation is most deserving of our adoption which is most consistent with the whole body of facts carefully observed and compared.

To attempt to trace, within the limits to which my remarks must be confined, the influence which the progress of knowledge has exercised upon the scientific and general conception of biological doctrines would be impossible, for the modification of opinion on these subjects has proceeded not less from the rapid advance which our age has witnessed in the progress of general science, especially of physics and chemistry, than from that of departments belonging to biology itself.

Thus, to go no further than the most general laws of nature, the whole doctrine of the conservation and transmutation of force in physics, so ably expounded to this Association by Mr. Justice Grove, the theory of compound radicals and substitution, with the discovery of organic synthesis, in chemistry, and the more recent advance in speculation with regard to the molecular constitution and properties of matter, with which we must associate the names of our last President and of Clerk Maxwell, in completely changing the aspect of physical and chemical sciences within the last thirty-five years, have paved the way for views of the constitution and action of organised bodies very different from those which could be formed at the time of the first meeting of the Association in this place. And if, confining ourselves to the department of Biology, we add the discovery by microscopical observation of the minuter elementary forms of organisation, more especially as flowing from the comprehensive views of organised structure promulgated by Schleiden and Schwann nearly forty years ago, the later discovery and investigation of living protoplasmic substances, the accumulated evidence of progressive types of animal and vegetable forms in the succession of superimposed strata composing the crust of the earth, the recent discoveries as to the conditions of life at great depths in the ocean, the vast body of knowledge brought together by the labours of anatomists and physiologists as to the structure and functions of almost every plant and animal, and (still more, perhaps, than any other single branch of biological inquiry) if we note the rapid and immense progress which has been made during the last fifty years in the study of the entirely modern science of the development of living beings—we shall be able to form some conception of the enormous extension in our time of the basis of observation and fact from which biological phenomena may now be surveyed, and from which just views may be formed as to their mutual relations and general nature.

It is now familiarly known that almost all (if not indeed all) the plants and animals existing on the earth's surface derive their origin from parents or previously-existing beings whose form and nature they closely reproduce in their life's history. By far the greater number spring from germs in the form of visible and known spores, seeds, or eggs. A few may be traced to germs, or to vestiges of the parental body, the exact nature of which may be doubtful; and some, including even a certain number of those also produced from known germs, are either constantly or occasionally multiplied by budding, or by a process of cleavage, or direct and visible division of the parent body.

The germ constituting the basis of new formation, whether it have the form of spore, seed, or ovum, is of the simplest kind of organisation, and the process by which a new plant or animal is produced is necessarily one of gradual change and of advance from a simpler to a more complex form and structure: it is one of "evolution," or, as I would rather name it, "development." But before proceeding to discuss the subject of development in the higher animals, it is right to advert to the preliminary and often debated question, which naturally presents itself, viz.—Do all living or organised beings, without exception, spring from germs, or from any kind of organised matter that has belonged to parents? or may there not be some, especially among the simpler forms (with regard indeed to which alone there has of late been any question), which are produced by the direct combination of their component elements, in the way of the so-called spontaneous or equivocal generation, heterogenesis or abiogenesis?

The importance of the right solution of this problem is not confined merely to the discovery of the mode of origin of the lowly organisms which have been the more immediate object of investigation by naturalists in recent times, but is one of much wider significance, seeing that, if it shall be satisfactorily proved or even rendered probable that in the course of cosmical development all the various kinds of plants and animals have been gradually produced by evolution out of pre-existing simpler forms, and thus the whole series of organised beings in nature has been shown to be one of hereditary connection and derivation, then it would follow that the history of the origin of the simplest organisms may be the key to that of the first commencement of life upon the earth's surface, and the explanation of the relation in which the whole succeeding progenies stand to their parental stocks.

From the very lucid and masterly view of this subject, given by Prof. Huxley in his address to the Association at Liverpool, so recently as in 1870, in which the conclusion he formed was based very much on the exhaustive and admirable researches of Pasteur, I might almost have dispensed with making further reference to it now, but for the very confident statements since made by the supporters of the doctrine of abiogenesis, among whom Dr. Bastian stands most prominent in this country, and for the circumstance that the life-history of many of the lower organisms was still imperfectly known.

During the last seven or eight years, however, renewed inves-

[1] It would also be unjust to omit to mention here one of the earliest attempts to bring British opinion into a new channel, by the remarkable work entitled "Vestiges of Creation," which appeared in 1844, nor to conceal from ourselves the unmerited ridicule and obloquy attempted to be thrown upon the author, not perhaps so much on account of the many inaccuracies unavoidable in an attempt at the time to overtake so large a field, as directed against the dangerous tendencies supposed to lurk in its reasoning.

tigations by most competent inquirers have followed one another in quick succession, from a review of which we cannot but arrive at a conclusion adverse to the theory of heterogenesis, viz., that no development of organisms, even of the most simple kind, has been satisfactorily observed to occur in circumstances which entirely excluded the possibility of their being descended from germs, or equivalent formative particles, belonging to pre-existing bodies of a similar kind. I can do no more now than name the authors of the most conclusive experiments on this subject, which I do nearly in the order of the publication of their researches, as those of Mr. W. N. Hartley in 1872, Messrs. Pode and Ray Lankester in 1873, Dr. Burdon Sanderson in that and the following years, Dr. W. Roberts in 1874, Prof. Lister in 1875, and most recently of Prof. Tyndall, Prof. Cohn, and of Messrs. Dallinger and Drysdale.[1]

But, admitting that the evidence from direct experiment is such as entirely to shut us out from entertaining the view that spontaneous generation occurs in the present condition of the earth, we are not relieved from the difficulty of explaining how living organisms or their germs first made their appearance; nor are we debarred from attempting to form hypotheses as to how this may have taken place. First, upon the theory of evolution, which, strictly carried out, supposes the more complex organisms to be derived from the more simple, it might be held that the conditions affecting the combination of the primary elements of matter into organic forms may at one time have been different from those which now prevail; and that, under those different conditions, abiogenesis may have been possible, and may have operated to lay the foundations of organic life in the simpler forms in which it at first appeared—a state of things which can only be vaguely surmised, but in regard to which no exact information can be obtained. Or, secondly, evading the difficulty of strict cosmical evolution, we might suppose that vital conditions may have been coeval with the first existence of physical and chemical properties in the rest of natural bodies. But this hypothesis would be exposed to the objection that, according to the cosmical view generally held by physicists, the whole materials composing the earth have originally been subject to incandescent heat. Nor is the difficulty abolished, but only removed to a more remote period by the supposition of the transport of germs from another planet or their introduction by means of meteorites or meteoric dust; for, besides the objection, arising from the circumstance that these bodies must have been subjected to a very high temperature, we should still have everything to learn as to the way in which the germs arose in the far distant regions of space from which they have been conveyed.

The incompleteness of the geological record leaves us in the dark as to the time at which the first dawnings of life appeared in the lower strata of the earth's surface. The most recent researches tend to carry the origin of life back to a much earlier period than was at one time believed, and if the famous *Eozoon* be admitted as evidence, even into that of the Laurentian strata. But even if doubts should prevail with regard to the presence of definite organised forms in the older sedimentary strata, the occurrence in them of carbon in the form of graphite in large quantities makes the previous existence of living organisms at least possible, and it may be that the complete metamorphosis which these rocks have undergone has entirely removed all definite traces of organisation.

Nor have we the means from geological data of determining whether the beings of the vegetable or of the animal kingdom first made their appearance. If we adopt the view which has for some time been entertained by physiologists that animals are entirely dependent, directly or indirectly, on plants for the material which constitutes their living substance, and that plants, as constructive agents, alone have the power to bring together the elements of lifeless matter from such states as carbonic acid, water, and ammonia, into the condition of the living solid, the inference would be inevitable, at least for the great majority of the animal creation, that they must have been preceded by plants. But palæontology is as yet silent on this interesting question; and, if we consider the remarkable approach which is made in structure and properties between the lowest and simplest members of the two kingdoms of organic nature, so that at last all distinction between them seems entirely to vanish, and a set of organisms is found which partake equally of animal and vegetable characters, or, rather, exhibit properties which are common to them both, we shall hesitate to postulate confidently for the primitive antecedence of vegetable life, although, perhaps, in later epochs the pre-existence of vegetables may be looked upon as necessary to the life of more developed animal organisms.

The reflection forces itself upon us that we are just as ignorant of the mode of first origin of all the compounds of the inorganic elements as we are of that of living matter; and we may therefore be excused if we suspend all theory and conjecture until we shall be guided to more reliable hypotheses through the plain track of observation and experiment.

The practical applications of the increased knowledge of the origin of minute animal and vegetable organisms are so numerous that it would occupy a much longer time than is at my disposal to give any detailed account of them; but they are of such immense importance in their commercial, social, and sanitary relations that they ought never to be lost sight of.

It is now proved beyond doubt that the origin of putrefaction and fermentation is dependent on the presence in the substances which are the seat of change in these processes, or in the surrounding air, of the germs of minute organisms of an animal or vegetable nature, and that the maintenance of the chemical changes in which these processes mainly consist is coincident with and casually (if not essentially) dependent upon the growth and multiplication of these organisms.

Prof. Lister had the merit of being the first to apply the germ-theory of putrefaction to explain the formation of putrid matters in the living body; and he has founded on this theory the now well-known antiseptic treatment of wounds, the importance of which it would be difficult to over-estimate.

The success or failure of plans for the preservation of meat and other articles of food without question depends on the possibility of the complete exclusion of the germs which are the cause of putrefaction and fermentation; and their management must therefore be founded on the most accurate knowledge of these organisms, and the circumstances influencing the persistence of their vitality and the vigour of their growth.

The theory of biogenesis has also lately been the guide in the investigation of the causes of various forms of disease, both in the lower animals and in man, with the result of showing that in many of them the infective substance consists in all probability of germs of minute animal or vegetable organisms.

There is very great probability, indeed, that all the zymotic diseases, by which we understand the various forms of fevers, have a similar origin. As has been well remarked by Baxter in an able paper on "The Action of Disinfectants," the analogies of action of contagia are similar to those of septic organisms, not to processes simply of oxidation or deoxidation. These organisms, studied in suitable fluids, multiply indefinitely when introduced in all but infinitesimal proportions. Thus they are, as near as we can perceive, the very essence of contagia.[1]

Leaving, however, these and many other general questions regarding the origin of the lowest forms of animal and vegetable life, let us now turn our attention to the mode of development of a new being in those belonging to the higher groups. The general nature of the formative process, in all instances where fertilised germs are produced, will be best understood by a short sketch of the phenomena ascertained to occur in different kinds of plants.

In the higher or phanerogamic plants it is generally well known that the combination of two parts of the flower is necessary to the production of a seed containing the embryo or young plant. Beginning with the discovery of the pollen tubes by Amici in 1823, the careful and minute investigations of a long

[1] I may refer to Dr. Bastian's paper in NATURE of June 30, 1870, and to his two works, "The Origin of the Lowest Organisms," and "The Beginnings of Life." Mr. Hartley's researches, which were commenced in 1865, are described in a paper printed in the *Proceedings* of the Royal Society for 1872, and in his "Lectures on Air," 2nd edition, 1876, where an interesting account of the whole subject will be found. The experiments of Mr. Pode, of Oxford, and Prof. Ray Lankester are described in a paper on the "Development of Bacteria in Organic Infusions" in the Roy. Soc. *Proc.* for 1873, p. 349. Dr. Burdon Sanderson's researches are contained in the Reports of the Medical Officer of the Privy Council, and in various papers in NATURE; Dr. W. Roberts's paper is printed in the *Transactions* of the Royal Society for 1874, vol. clxiv. p. 457. Prof. Lister's "Contribution to the Germ Theory of Putrefaction and other Fermentative Changes," &c., is contained in the *Transactions* of the Royal Society of Edinburgh for 1875, p. 313, and is also given in NATURE. Prof. Tyndall's researches are described in his papers in the *Proceedings* of the Royal Society during the last two years. The work of Prof. Cohn, of Breslau, entitled "Beiträge zur Biologie der Pflanzen," 1873-76, contains many memoirs bearing upon this subject, which have been partly published in abstract in the *Microscopical Journal*, in which also will be found, in a series of contributions extending from 1873 to the present time, the interesting observations of Mr. W. H. Dallinger and Dr. J. Drysdale.

[1] For the most interesting information on this subject, I cannot do better than refer to the very able reports by Dr. Burdon Sanderson in the "Reports of the Medical Officer of the Privy Council," 1873, 1874, and 1875.

line of illustrious vegetable physiologists have brought to light the details of the process by which fertilisation is effected, and have shown in fact how the minute tube developed from the inner membrane of the pollen-granule, as soon as it falls upon the stigmatic tissue of the seed-bearing plant, insinuates itself by a rapid process of development between the cells of the style, and reaches at last the ovule, in the interior of which is the embryo-sac; how, having passed into the micropyle, or orifice of the ovule, it makes its way to the embryo-sac; how a minute portion of the fertilising substance of the fovilla transudes from the pollen tube into the cavity of the embryo-sac, in which by this time a certain portion of the protoplasm has become differentiated into the germinal vesicle, thereby stimulating it to further growth and development, the earliest phenomena of which manifest themselves by the formation of an investing cell-wall, and by the occurrence of cell-division, which results in the formation of the embryo or plantule of the seed.

Thus it appears that the essential part of the process of production in phanerogamic plants is the formation in the parent plant of cells of two different kinds, which by themselves have little or no independent power of further growth, but which, by their union, give rise to a product in which the power of development is raised to the highest degree.

By further researches it is now known that the same law prevails in all the remaining members of the vegetable kingdom, with the exception only of the very simplest forms.[1]

In viewing the reproductive process in the series of cryptogamic plants, two facts at once strike us as remarkable in the modifications which are observed to accompany the formation of a productive germ, viz.: First, that the difference between the two productive elements becomes, as it were, more prominent, or more highly specialised, in the cryptogamic than in the phanerogamic plants; and second, that in the simpler and lower forms this difference gradually disappears till it is lost in complete uniformity of the productive elements.

Thus in the whole tribe of the ferns and vascular cryptogams, in the higher algæ and fungi, in the characeæ and in the mosses, the differentiation of the productive elements is carried to a very high degree; for while that belonging to the embryo or germ presents the structure of a simple cell which remains at rest, or in a comparatively passive state, and, absorbing into itself the substance of the other, becomes the seat of subsequent development; the other, corresponding to the pollen of the staminiferous phanerogam, is usually separated from the place of its formation, and having undergone a peculiar modification of structure by which it acquires active moving cilia, it changes place, and is directed towards the germinal structure, and coming in contact with its elementary cell, is more or less absorbed, or lost in the fertilising process. The protoplasm of the germinal cell, thus acted on and fertilised, then proceeds to undergo the changes of development by which the foundation is laid for the new plant.

In the algæ and fungi, however, there are gradations of the differentiation of the two reproductive cells, which are of the greatest interest in leading to a comprehension of the general nature of the formative process. For in the lower and simpler forms of these plants, such as the Desmidiæ, Mesocarpeæ, and other Conjugatæ, we find that there is no distinction in structure or form to be perceived between the two cells which unite or undergo conjugation; and a complete fusion or intermixture of the two masses of protoplasm results in the production of a single, usually spherical, mass holding the place of an embryo. And that there is an absence of specialisation between the two uniting cells is clearly shown in both *Desmidium* and *Mesocarpus*, by the fact that the embryo or zygospore is formed in the mass resulting from the union of the protruded portions of the two cells; and in more ordinary cases, as in *Spirogyra*, where the embryo is formed in one of the two cells, it seems to be indifferent in which of them it is formed.

From this, which may be regarded as the most elementary type of new production by the union of the two cells, the transition is not a great one to the development of a progeny without any such union. We might conjecture, then, that the capacity for separate or individual existence extends in the lowest organisms to the whole, or to each structural element of their organisation, while as we rise in the scale of vegetable life (and the same view might apply to the animal kingdom) this capacity is more and more divided between the two productive elements, or, at least, is only called into full action by their combination.

The germinal element consists of a simple primordial cell, varying in different kinds of plants, but in all of them probably containing the essential substance protoplasm, and the most immediate result or effect of fertilisation is the multiplication by repeated fissiparous division of the previously-existing cells. The new individual resulting from this cellular growth usually remains within the parent body, without, however, direct union or continuity of tissue, till the embryo has attained some advancement, as in the well-known case of the seeds of a phanerogam; but there are many varieties in the mode of its disposal among the lower plants.

A remarkable exception to the more direct relation of the process of fertilisation to the formation of the new individual or embryo occurs in some plants, simulating in some respects that kind of variation in animal reproduction which has been named alternate generation. A well-known instance of this belongs to the vascular cryptogams. The prothallium of the ferns, for example, results from the development of so-called spores or unicellular buds, which are familiar as being formed in small capsules on the lower leaf-surface; and in this prothallium, when it has reached a certain stage of vegetation, there are formed the archegonia, containing the oospheres, or germ cells, which are fertilised by the moving ciliated particles developed in the cells of the antheridia, leading to the production of a new spore-bearing plant.

Recent researches have also called attention to the remarkable arrangements in phanerogamic plants for the prevention of fertilisation of the pistils by pollen from the same flower, or even from the same plant. In the latter case this is effected by the separation of stamens and pistils in different flowers on the same, or on different plants. In the former case, where both organs occur in the same flower, the adaptations, whether of a mechanical or of a physiological character, by which self-fertilisation is prevented, as ascertained by numerous recent investigations (among which those of Darwin are most conspicuous), are of the most varied and often the most complicated kind.

Let us now turn to the consideration of the development of animals, and let me say in the outset that it will be necessary for me to confine my remarks chiefly to the higher or vertebrated animals, and to certain parts only of the history of their development; more particularly the structure and formation of the ovum or egg, its earlier developmental changes, and the relation of these to the formation of the new animal.

I cannot enter upon the consideration of this topic without adverting to the very recent acquisition of some of the most important facts upon which this branch of knowledge is founded; and I feel it to be peculiarly appropriate, in the year of his death, to refer to a biologist whose labours contributed more powerfully than those of any other person to give to animal embryology the character of a systematic branch of science, and to whom we owe some most important original discoveries,—I mean Karl Ernst von Baer of Königsberg, St. Petersburg, and Dorpat.

Of observers who, previous to von Baer, were mainly instrumental in preparing the way for the creation of a more exact modern science of embryology only two can be mentioned, viz., Caspar Frederick Wolff of St. Petersburg, well known as the author of a work entitled "Theoria Generationis," published in 1759, by which the *epigenesis*, or actual formation of the organs in a new being, was first demonstrated, and Christian Pander, who by his researches made at Würzburg explained, in a work published in 1817, the principal changes by which the embryo arises and is formed.

Von Baer was born in the Russian province of Esthonia on February 29, 1792. After having been fifteen years professor in the Prussian University of Königsberg, he was called to St. Petersburg, and having some years later been appointed to a newly-established professorship of Comparative Anatomy and Physiology, he remained in that city for nearly thirty years as the most zealous and able promoter of scientific education and research, stimulating and guiding all around him by his unexampled activity, comprehensive and original views, sound judgment, and cordial co-operation. In 1868, at the age of seventy-six, he retired to Dorpat, from the University of which he had received his degree in 1814, and continued still to occupy himself with working and writing in his favourite subjects, as well as interesting himself in everything that was related to educational and scientific progress, to very near the time of his

[1] It will be observed that I leave entirely out of view the whole subject of the multiplication of plants by budding or simple division.

death, which occurred on November 28, 1876, in his eighty-fifth year.

Although von Baer's researches, according to the light in which we may now view them, contributed in no small degree to the introduction of the newer views of the morphological relations of organic structure which have culminated in the Theory of Descent, yet he was unwilling to adopt the views of Darwin; and one of his latest writings, completed in the last year of his life, was in vigorous opposition to that doctrine.

It would have been most interesting and instructive to trace the history of the progress of discovery in embryology from the period of von Baer down to the present time, but such a history would not be suitable to the purpose of this address; and I can only venture here, in addition to Rathke, the colleague of Baer in Königsberg, to select two names out of the long list of distinguished workers in this field during the last forty years, viz.: Thomas Bischoff, of Giessen and Munich, to whom we owe the greatest progress in the knowledge of the development of mammals, by his several memoirs, appearing from 1842 to 1854; and Robert Remak, of Berlin, whose researches on the development of birds and batrachia, appearing from 1850 to 1855, gave greatly-increased exactness and extension to the general study of development.

The germinal element, from which, when fertilised, the new animal is derived, is contained within the animal ovum or egg—a compact and definite mass of organic matter, in which, notwithstanding great apparent variations, there is maintained throughout all the members of the animal kingdom, excepting the protozoa, which are destitute of true ova, a greater uniformity in some respects than belongs to the germinal product of plants.

Usually more or less spherical in form, the animal ovum presents the essential characters of a "complete cell," in the signification given by Schwann to that term. The germinal substance is inclosed by an external vesicular membrane or *cell-wall*. Within this covering the *cell-substance*, generally named yolk or vitellus, from the analogy of the fowl's egg, consists, to a greater or less extent, of a mass of protoplasm, and imbedded in this mass, in a determinate situation, there is found a smaller internal vesicular body, the *germinal-vesicle* or nucleus, with its more or less constant or variable *macula* or nucleolus.

Now the first thing which strikes us as remarkable connected with the ovum is the very great variation in size as compared with the entire animal, while in all of them the same simple or elementary structure is maintained. The ovum of mammals is, for example, a comparatively small body, of which the average diameter is about the $\frac{1}{100}$th of an inch, and which consequently scarcely weighs more than a very minute fraction of a grain, which may be calculated perhaps only at the $\frac{1}{12000}$th part. And further, in two animals differing so widely in size as the elephant and the mouse, the weights of which may be held to stand towards each other in the proportion of 150,000 to 1, there is scarcely any difference in the size of the mature ovum.

On the other hand, if we compare this small ovum of the mammal with the yolk of the egg in the common fowl, the part to which it most nearly corresponds, it may be estimated that the latter body would contain above three millions of the smaller ova of a mammal.

The attribute of size, however, in natural objects ceases to excite feelings of wonder or surprise as our knowledge of them increases, whether that be by familiar observation or by more scientific research. We need not, at all events, on account of the apparent minuteness of the ovum of the mammifer, or of any other animal, have any doubts as to the presence of a sufficient amount of germinal substance for explaining in the most materialistic fashion the transmission of the organic and other properties and resemblances between the parent and offspring. For we are led to believe, by those who have recently given their attention to the size of molecules composing both living and dead matter, that in such a body as this minute ovum of the mammal, there may be as many as five thousand billions of molecules, and even if we restrict ourselves to the smaller germinal vesicle, and, indeed, to the smallest germinal particle which might be made visible by the highest microscopic enlargement, there are still sufficient molecules for all the requirements of the most exacting material biologist.[1]

This great disparity of size is, however, connected with an important difference in the disposition of the yolk-substance, according to which ova may be distinguished as of two kinds—the large- and the small-yolked ova, between which there are also many intermediate gradations. The larger yolked ova belong to the whole tribe of birds, scaly reptiles, osseous and cartilaginous fishes, and the cephalopods among the invertebrates; and are distinguished by the strictly germinal part or protoplasm being collected into a small disc, known familiarly as the cicatricula of the fowl's egg, and to be seen as a whitish spot on that side of the yolk which naturally floats uppermost; while the rest of the yolk, of a deeper yellow colour, contains a large quantity of vitelline granules or globules of a different chemical nature from the protoplasm.

The phenomena of embryonic development are, in the first instance at least, confined to the germinal disc, and the rest of the yolk serves in a secondary or more remote manner to furnish materials for nourishment of the embryo and its accessory parts. Thus we distinguish the germinal from the nutritive or food-yolk, or, as the younger Van Beneden has named them, the *protoplasm* and the *deutoplasm*.

In the smaller ovum of the mammal, on the other hand, it seems as if the whole, or nearly the whole, of the yolk were protoplasmic or germinal. There may be some admixture of yolk-granules; but there is not the marked separation or limitation of the protoplasmic substance which is so distinct in birds, and the earliest changes of development extend to the whole component substance of the yolk, or, in other words, the yolk is entirely germinal. Hence some have given the names of *meroblastic* and *holoblastic*, meaning partially and entirely germinal, to these two contrasting forms of ova. There are many of the invertebrate animals of which the ova present the same entirely germinal arrangement as in those of mammals, and the *amphioxus* may be included in the same group.

The amphibia stand in some measure between the two extremes; the purely protoplasmic or germinal part occupying one side, and the nutritive or vitelline the other. But among the invertebrates the gradations are often such as to make it difficult to determine under which group the ova should be placed.

The genesis or formation of the ovum itself, if it be considered with reference to its first origin, carries us back to a very early period of the formation of the parent in which it is produced; and it is one of the most interesting problems to determine what is the source of the cells in the parent from which the ova originally spring. All that I can venture to say at present in regard to this point is, that the primordial ova or germs appear in the parental body while still embryonic, at a very early period of its development, and clearly derive their origin from a deeply-seated part of the formative cells which are undergoing transformation into the primitive organs; but the exact seat of the origin of the reproductive cells is still a matter of doubt.

When the ovum attains its full maturity in the ovary, the seat of its formation within the parent, it is separated from that organ, and when perfected proceeds to undergo embryonic development; a marked difference in this respect existing between the germinal product of the higher plants and animals.

The period of maturation of the ovum is marked in the greater number of animals by a series of phenomena which have generally been interpreted as the extrusion or absorption of the germinal vesicle; and various observers have actually traced the steps of the process by which that vesicle appears to leave the yolk and is lost to sight, or has passed into the space betwene the yolk and its membrane in the shape of the peculiar hyaline bodies named the *polar* or *directing* globules. But recent researches, afterwards to be referred to, tend to show that some part at least of the substance of the germinal vesicle remains to form, when combined with the fertilising element, the newly-endowed basis of future development.

Among the earliest changes to which the perfect animal ovum is subject, I have first to refer to the segmentation of the germ, a series of phenomena the observation of which has been productive of most important results in leading to a comprehension of the intimate nature of the formative process, and which is of the deepest interest both in a morphological and histological point of view. This process, which was first distinctly observed by Prevost and Dumas more than fifty years ago, and is now known to occur in all animal ova, consists essentially in the

[1] According to a calculation made by Mr. Sorby, the number of molecules in the germinal vesicle of the mammalian ovum is such that if one molecule were to be lost in every second of time, the whole would not be exhausted in seventeen years. See Address to the Microscopic Society in *Journ. of Microscop. Science*, vol. xv. p. 225, and NATURE, vol. xiii. p. 332. See also Darwin on Pangenesis, in his work on "Variations," &c. (1868), vol. ii. p. 374, and the Review by Ray Lankester of Haeckel's work, "Perigenesis der Blastidule," &c., in NATURE for 1876, p. 235, an Ray Lankester's Essay on "Comparative Longevity," 1870.

cleavage or splitting up of the protoplasmic substance of the yolk, by which it becomes rapidly subdivided into smaller and more numerous elements, so as at last to give rise to the production of an organised stratum of cells out of which by subsequent changes the embryo is formed.

The process of yolk segmentation may at once be distinguished as of two kinds, according as it affects in the small-yolked ova the whole mass of the yolk simultaneously, or in the large-yolked ova is limited to only one part of it. The cleavage process, in fact, affects the germinal and not the food-yolk; so that to take the two most contrasting instances of the bird and mammal to which I have before referred, it appears that while the mammal's ovum undergoes entire segmentation, this process is confined to the substance of the cicatricula or germinal disc of the bird's egg. This process is essentially one of cell-division, but it is also in some measure one of cell-formation. The best idea of its nature will be obtained from a short description of the total segmentation occurring in the mammal's ovum.

When, as before mentioned, the germinal vesicle has been in part extruded or lost to sight, the whole yolk-substance of the ovum forms a nearly uniform mass of finely granular protoplasm, inclosed within the external cell-membrane. Within a few hours later a clear nucleus has arisen in this mass. To this more definite form of organisation, assumed by the germinal substance of the future animal which is about to be the subject of the segmenting process, the name of the first segment-sphere may be given.

By the process of cleavage, which now begins, this first segment-sphere and its nucleus undergo division into two nucleated spheres of smaller size, the whole substance of the yolk, in a holoblastic ovum, such as that of the mammal, being involved in the segmenting process.

The second stage of division follows after the lapse of a few hours, and results in the formation of four nucleated segment spheres; and the process of division being repeated in a certain definite order, there result in the succeeding stages, that is, the third, fourth, fifth, and up to the tenth, the numbers of 8, 12, 16, 24, 32, 48, 64, and 96 nucleated yolk-spheres, germ-spheres, or formative cells.

In the rabbit's ovum the tenth stage is reached in less than three days; and as during that time the size of the whole ovum has undergone very little increase, it follows that the spheres of each succeeding set, as they become more numerous, have diminished greatly in size. These segment-spheres are all destitute of external membrane, but are distinctly nucleated; and their protoplasmic substance is more or less granular, presenting the usual histological characters of growing cells.

By the time that segmentation has reached the seventh or eighth stage, when 32 or 48 spheres have been formed, the ovum has assumed the appearance of a mulberry, in which the outer smaller spheres, closely massed together, project slightly and uniformly over the whole surface; while the interior of the ball is filled with cells of a somewhat larger size and a more opaque granular aspect, also resulting from the process of segmentation.

Already, however, the mutual compression of the spheres or cells on the surface, by their crowding together, has led to the flattening of their adjacent sides; and by the time the tenth stage is reached, when the whole number of the cells is about 96, the more advanced superficial cells having ranged themselves closely together, form a nucleated cellular layer or covering of the yolk, inclosing within them the larger and more opaque cells, derived like the first from the segmenting process. In a more advanced stage, the deeper cells now referred to having also taken the form of a layer, there results at last the bilaminar blastoderm or embryonic germinal membrane.

The process of partial segmentation, such as occurs in the bird's egg, though perhaps fundamentally the same as that of the mammal previously described, stands in a different relation to the parts of the whole yolk or egg, and consequently differs in its general phenomena. The segmentation is mainly restricted in the meroblastic ova of birds to the germinal disc or cicatricula, and does not immediately involve any part of the larger remainder of the yolk. This takes place during the time of the descent of the yolk through the oviduct, when the yolk is receiving the covering of the white or albumen, the membrane and the shell, previous to being laid—a progress which, in the common domestic fowl, usually occupies less than twenty-four hours. Corresponding essentially to the more complete segmentation of the mammal's ovum, the process leads to the same result in the production of two layers of nucleated formative cells in the original seat of a protoplasmic disc; a bilaminar blastoderm resulting as in the mammal's ovum, though in a somewhat different relation to the yolk.

I will not fatigue you with a description of the details of these phenomena, interesting as they may be, but only mention generally that they consist in the formation of deep fissures running from the surface into the substance of the germ-disc. The first of these fissures crosses the disc in a determinate direction, dividing it into two nearly equal semicircular parts. In the next stage another fissure, crossing the first nearly at right angles, produces four angular segments. Then come four intervening radial fissures, which subdivide the four segments into eight; and next afterwards the central angles of these eight radial segments are cut off from their peripheral portions by a different fissure, which may be compared to one of the parallels of latitude on the globe near the pole where the radial or *longitude* fissures converge. And so thereafter, by the succession and alternation of radial and circular clefts, which, however, as they extend outwards, come soon to lose their regularity, the whole germinal disc is divided into the two layers of nucleated cells, constituting the blastoderma or germinal membrane of Pander and all subsequent embryologists.[1] If a laid egg be subjected to the heat of incubation for eight or ten hours, the cicatricula, now converted into this segmented blastoderm, is found to be considerably expanded by a rapid multiplication of its constituent cells, and in as many more hours, by further changes in its substance, the first lineaments of the chick begin to make their appearance. Similar changes affect the blastoderm of the mammal, and thus it appears that the result of segmentation, in the bird as well as in the mammal and other animals, is the production of an organised laminar substratum, which is the seat of the subsequent embryonic development.

I must still request your attention to some details connected with the process of segmentation, which bear upon the question of the origin of the new cells, and on which recent research has thrown a new and unexpected light.

With respect to the nature of the first segment-sphere of the ovum and the source of its nucleus, as well as of the other segment-spheres or cells which follow each other in the successive steps of germ-subdivision, it appears probable from the researches of several independent observers, and more especially of Edward Van Beneden and Oscar Hertwig, that in the course of the extrusion of the germinal vesicle, a small portion of it remains behind in the form of a minute mass of hyaline substance, to which Van Beneden has given the name of *pronucleus*, and that, as the result of the fertilising process, there is formed a second similar hyaline globule or pronucleus, situated near the surface, which gradually travels towards the centre and unites with the first pronucleus, and that these two pronuclei, being fused together, form the true nucleus of the first segment sphere. According to this view the original germinal vesicle, when it disappears, or is lost to sight, as described by so many embryologists, is not dissipated, but only undergoes changes leading to the formation of the new and more highly endowed nucleus of the first embryonic or segmental sphere. It further appears that the sub-division of each segmenting mass is preceded by a change and division of the nucleus, and that this division of the nucleus is accompanied by the peculiar phenomenon of a double conical or spindle-shaped radial lineation of the protoplasm, which, if we were inclined to speculate as to its nature, seems almost as if it marked out the lines of molecular force acting in the organising process. These lines, however, it will be understood, if visible with the microscope, even of the highest magnifying power yet attained, belong to much larger particles than those of the supposed molecules of the physicist; but considered in connection with what we know of the movements which frequently precede the act of division of the yolk-spheres, we seem in this phenomenon to have made some near approach to the observation of the direction in which the molecular forces operating in organisation may be supposed to act.[2]

[1] The more exact nature of the process of segmentation was first made known by the interesting researches of Bagge in 1841, and more especially of Kölliker in 1843. The phenomena of complete segmentation were first fully described in the mammal's ovum in Bischoff's "Description of the Development of the Rabbit," 1842, and followed out in his succeeding "Memoirs on the Dog, Guinea-pig, and Roe-deer." The phenomena of partial segmentation were first made known, in their more exact form, by Kölliker's "Researches on the Development of the Cephalopoda," published in 1844. In birds the process was first described by Bergmann in 1846, and more fully by Coste in 1848.

[2] The observations referred to above as to the division of the nucleus are so novel and of such deep interest that I am tempted to add here a short abstract of their more important results from a very clear account given of them by Dr. John Priestley, of Owens College, Manchester, in the *Journal of Microscopical Science* for April, 1876.

With respect to the nature of the blastoderm, the organised cellular stratum resulting from segmentation, and its relation to the previous condition of the ovum on the one hand, and the future embryo on the other, there is presented to us, by modern research, the interesting view that the blastoderm consists, after completion of the segmenting process, of two layers of cells, an outer or upper, usually composed of smaller, clearer, and more compact nucleated cells, named *ectoderm*, or *epiblast*, and an inner or lower, consisting of cells which are somewhat larger, more opaque, and granular, but also nucleated, and named *endoderm*, or *hypoblast*.

In the meroblastic ova, such as those of birds, the bilaminar blastoderm is discoid and circumscribed, as it lies on the yolk surface, and only comes to envelope the whole of the food-yolk in the progress of later development; while in the holoblastic ova, and more especially in mammals, the blastoderm from the first extends over the whole surface of the yolk, and thus forms an entire covering of the yolk known as the "vesicular blastoderm;" the space within being occupied by fluid.

Huxley long ago presented the interesting view that these two layers are essentially the same in their morphological relations and histological structure with the double wall of the body in the simplest forms of animals above the protozoa; and Haeckel has more recently followed out this view, and supported it by his researches in the Calcareous Sponges, and has founded upon it his well-known *Gastræa* theory. According to this view all animals take their origin from a form of *Gastrula*. In the simpler tribes, as in the instance of the common fresh-water polype or hydra, they proceed no further than the gastrula stage, unless by mere enlargement and slight differentiation of the two primitive layers of cell, representing the persistent ectoderm and endoderm.[1]

If, pursuing this idea, we take a survey of the whole animal kingdom in its long gradation of increasing complexity of form and structure from the simplest animal up to man himself, we find that all the various modifications of organic structure which present themselves are found, in the history of the individual or ontological development of the different members of the series, to spring originally from two cellular laminæ, ectoderm and endoderm, the component elements of which may again be traced back to the first segment-sphere and primitive protoplasmic elements of the ovum.

Time does not admit of my conducting you through the chain of observation and reasoning by which Haeckel seeks to convince us of the universal applicability of his theory, but I cannot avoid calling your attention to the extremely interesting relation which has been shown to exist between the primary phases of development of the ovum and the foundation of the blastoderm in very different groups of animals, more especially by the researches of Haeckel himself, of Kowalevsky, Edward Van Beneden, and others, and which has received most efficient support from the investigations and writings of E. Ray Lankester in our own country; so that now we may indulge the well-grounded expectation that, notwithstanding the many and great difficulties which doubtless still present themselves in reconciling various forms with the general principle of the theory, we are at least in the track which may lead to a consistent view of the relations subsisting between the ontogenetic, or individual, and the phylogenetic, or race, history of the formation of animals and of man.[1]

In all animals, then, above the protozoa, the ovum presents, in some form or other, the bilaminar structure of ectoderm and endoderm at a certain stage of its development, this structure resulting from a process of segmentation or cell cleavage; and there are three principal modes in which the double condition of the layers is brought about. In one of these it is by inward folding or invagination of a part of the single layer of cells immediately resulting from the process of segmentation that the doubling of the layers is produced; in the second, perhaps resolvable into the first, it may be described rather as a process of inclosure of one set of cells within another; while in the third the segmented cells arranged as a single layer round a central cavity of the ovum, divide themselves later into two layers. But the distinction of ectodermic and endodermic layers of cells is maintained, whether it be primitive and manifested from a very early period, or acquired later by a secondary process of differentiation. Thus, in many invertebrates, as also in *Amphioxus* among the vertebrates, a distinct invagination occurs, while in mammals, as recently shown by Van Beneden's most interesting observations in the rabbit's ovum, and probably also in some invertebrates, the cells of the ectoderm gradually spread over those of the endoderm during the progress of segmentation, and thus the endodermic comes to be inclosed by the ectodermic layer of cells.

From the very novel and unexpected observations of Van Beneden it further appears that from the earliest period in the process of segmentation in the mammal's ovum it is possible to perceive a distinction of two kinds of segment-spheres, or cells, and that when this process is traced back to its first stage it is found that the whole of the cells belonging to the ectoderm are the progeny of, or result from the division of the upper of the two first formed segments, and that the whole of the endodermic cells are the descendants of the lower of the two first segmented cells. This, however, is not an isolated fact belonging only to mammalian development, but one which very nearly repeats a process ascertained to occur in a considerable number of the lower animals, and it seems to promise the means of greatly advancing the comprehension of the whole process of blastodermic formation. Thus, ectoderm and endoderm, or the primordial rudiments of the future animal and vegetative systems of the embryo, are traced back as distinct from each other to the first stage of segmentation of the germ.

Accepting these facts as ascertained, they may be regarded as of the deepest significance in the phylogenetic history of animals; for they appear to open up the prospect of our being able to trace transitions between the earliest embryonic forms occurring in the most different kinds of ova, as between the discoid or meroblastic, and the vesicular or holoblastic, through the intermediate series which may be termed amphiblastic ova.

In the lowest animals, the two layers already mentioned, viz., ectoderm and endoderm, are the only ones known to constitute the basis of developmental organisation; but as we rise in the scale of animals we find a new feature appearing in their structure which is repeated also in the history of the formation of the blastoderm in the higher animals up to man. This consists in the formation of an intermediate layer or layers constituting the *mesoderm*, with which, in by far the greater number, is connected the formation of some of the most important bodily structures, such as the osseous, muscular, and vascular systems.

I will not stop to discuss the very difficult question of the first origin of the mesoderm, upon which embryologists are not yet entirely agreed, but will only remark that a view originally taken of this subject by the acute von Baer appears more and more to gain ground; and it is this—that the mesoderm, arising as a secondary structure, that is, later than the two primary layers of ectoderm and endoderm (corresponding to the serous and mucous layers of Pander), is probably connected with or derived from both of these primitive layers, a view which it will afterwards appear is equally important ontogenetically and phylogenetically.

The researches now referred to are those of Auerbach, Butschli, Strasburger, Hertwig, and Edw. Van Beneden, and the following may be stated as the points in which they mainly agree:—

The nucleus when about to divide elongates into a spindle-shaped body, becomes irregular and indistinct, acquires a granular disc or zone in the plane of its equator; this divides into two, and each half moves towards the pole of the spindle on its own side, there being radiated lines of protoplasm between the poles and the equatorial disc.

The disc segments are the new nuclei, and the subsequent division of the cell takes place in the intermediate space.

Although these observers still differ in opinion upon some of the details of this process, and especially as to the fate of the germinal vesicle, all of them seem to agree that there are two pronuclei or distinct hyaline parts of the yolk protoplasm, a superficial and a deep one, engaged in the formation of the new nucleus, and both Hertwig and Van Beneden are of opinion that the two proceed from different productive elements.

The radiated structure of the nuclei had been previously recognised by Fol and Flemming, and further observed by Oellacher.

1. Butschli's researches are published in the *Nov. Act. Nat. Cur.*, 1873, and in the *Zeitschr. für wissensch. Zool.*, vol. xxv.

2. Auerbach's observations in his *Organolog. Studien*, 1874.

3. Strasburger's observations in his memoir "Ueber Zell-bildung und Zelltheilung," Jena, 1875.

4. Edward Van Beneden's researches, partly in his memoir "On the Composition and Significance of the Egg," &c, presented to the Belgian Academy in 1868, and more particularly in the extremely interesting preliminary account of "Researches on the Development of Mammalia," &c., 1875; and in a separate paper in the *Journ. of Microscopical Science* for April, 1876.

5. Oscar Hertwig's Memoirs are contained in the *Morpholog. Jahrbuch*, 1875, and his most interesting and novel observations in the same work, 1877.

[1] At this place I will only refer to one of the most recent of Haeckel's works, in which the views alluded to above are fully exposed in a series of most interesting memoirs, viz., "Studien zur Gastræa-Theorie," Jena, 1877.

[1] I ought here to refer to the elaborate memoirs of Prof. Semper on the morphological relations of the vertebrate and invertebrate animals contained in the "Arbeiten aus dem zoolog. zootom. Institut in Würzburg," 1875 and 1876, in which the conclusions arrived at do not coincide with the views above stated.

But whatever may be the first origin of the mesoblast, we know that in the vertebrata this layer, separating from between the other two, and acquiring rapidly by its cell multiplication larger proportions and much greater complexity than belongs to either ectoderm or endoderm, speedily undergoes further subdivision and differentiation in connection with the appearance of the embryonic organs which arise from it, and in this respect contrasts greatly with the simplicity of structure which remains in the developed parts of the ectodermic and endodermic layers. Thus, while the ectoderm supplies the formative materials for the external covering or epidermis, together with the rudiments of the central nervous organs and principal sense-organs, and the endoderm by itself only gives rise to the epithelial lining of the alimentary canal and the cellular part of the glands connected with it, the mesoblast is the source of far more numerous and complex parts, viz., the whole of the true skin or corium, the vertebral column and osseous system, the external voluntary muscles and connective tissue, the muscular walls of the alimentary canal, the heart and blood-vessels, the kidneys, and the reproductive organs thus forming much the greatest bulk of the body in the higher animals.[1]

There is, however, a peculiarity in the mode of the earliest development of the mesoblast which is of great importance in connection with the general history of the disposition of parts in the animal body to which I must now refer. This consists in the division of the mesoblast in all but its central part into two laminæ, an outer or upper and an inner or lower, and the separation of these by an interval or cavity which corresponds to the space existing between the outer wall of our bodies and the deeper viscera; and which from the point of view of the vertebrate animals is called the pleuro-peritoneal cavity, but viewed in the more extended series of animals down to the annuloida, may receive the more general appellation of pleurosplanchnic or parieto-visceral cavity, or, shortly, the *cœlom*. Thus, from an early period in the vertebrate embryo, and in a considerable number of the invertebrate, a division of the mesoderm takes place into the somato-pleural or outer lamina, and the splanchno-pleural or inner lumina; the outer being the seat of formation of the dermal, muscular, and osseous systems —the volunto-motory of Remak; and the inner of the muscular wall of the alimentary canal, as well as of the contractile substance of the heart and the vascular system generally.

It is interesting to find that there is a correspondence between the later division of the mesoderm of the higher animals derived from the two primitive blastodermic laminæ, and the original absence of mesodermic structure in the lowest animals, followed by the gradual appearance, first of one layer (the external muscular in the higher cœlenterata), and soon afterwards by the two divisions or laminæ with the intermediate cœlom.

In this account of what may be termed the organised foundation of the new being, I have entered into some detail, because I felt that our conception of any relation subsisting between the ontogenetic history of animals and their phylogenetic evolution can only be formed from the careful study of the earliest phenomena of embryonic organisation. But, notwithstanding the many difficulties which unquestionably still block the way, I am inclined to think that there is great probability in the view of a common bilaminar origin for the embryo of all animals above the protozoa, and that the vertebrate equally with the invertebrate animals may be shown to possess in the first stages of their blastodermic or embryonic formation the two primitive layers of ectoderm and entoderm.

To attempt, however, to pursue the history of the development of animals in detail would be equivalent to inflicting upon you a complete system of human and comparative anatomy. But I cannot leave the subject abruptly without an endeavour to point out in the briefest possible manner the bearing of one or two of the leading facts in embryology upon the general relation of ontogeny and phylogeny.

We are here brought into the contemplation of those remarkable changes, all capable of being observed and demonstrated, by which the complex organisation of the body is gradually built up out of the elementary materials furnished by the blastodermic layers,—a process which has been looked upon by all those who have engaged in its study with the greatest interest and admiration. And if, by comparing these phenomena as observed in individuals belonging to different classes and orders of animals, it is found not only that they are not different, but on the contrary, that they present features of the most remarkable resemblance and conformity, we shall be led to conclude that there is a general plan of development proved to extend to the members of considerable groups, and possibly capable of being traced from one group to another. But this is clearly nothing else than another way of stating that there is a similar type of structure pervading the animals of each group, and a probability of a common type being ascertained to belong to them all. The main question, therefore, to be answered is whether there is or is not a general correspondence between the phenomena of development and the gradation of type in animal structure upon which anatomists and zoologists are agreed; and my object will now be to bring rapidly before you one or two of the most marked illustrations of the correspondence, drawn from the early history of development in the higher animals.

As one of the examples of the earlier phenomena of development I may refer to the change which is perceptible as early as the eighteenth or twentieth hour of incubation in the chick, and which is reproduced in the course of development of every member of the vertebrate sub-kingdom. It consists in the formation of cross clefts on each side of the primitive neural cavity which divide off from each other a number of segments of this wall in the length of the axis of the embryo. At first there are only one or two such clefts; but they rapidly increase in a backward direction in the body of the embryo, and as development proceeds they extend into the tail itself. These are the *protovertebræ* of embryologists, not corresponding, as might at first be supposed, with the true or actual vertebræ which are formed later, but representing in an interesting manner transverse *vertebral segments* of the body, and containing within each the elements of a great part of the structure belonging to the body-wall afterwards to be developed, including the true cartilaginous or osseous vertebral arches, and the muscular plates.

This change, however, belongs to the mesodermic lamina, and occurs in an elongated thick portion of it, which makes its appearance on each side of the primitive neural canal between the epiblast and the hypoblast. The transverse cleavage is ascertained to commence near what afterwards forms the first cervical vertebra, but does not extend into the base of the cranium. And it is most interesting to note in this cleavage the formation at so early a period of the succession of *metameres* or series of similar parts, which forms a main characteristic of vertebral organisation.

As intimately connected with the formation of the vertebral column, the appearance of the chorda dorsalis, or *notochord*, presents many points of peculiar interest in embryological inquiries.

The notochord is a continuous median column or thread of cellular structure, running nearly the whole length of the rudimentary body of the embryo, and lying immediately below the cerebro-spinal canal. It occupies in fact the centre of the future bodies of the vertebræ. It exists as a primordial structure in the embryo of all vertebrates, including man himself, and extending down to the amphioxus, and, according to the remarkable discovery of Kowalevsky in 1866, it is to be found among the invertebrates in the larva of the ascidia.[1]

In amphioxus and the cyclostomatous fishes the notochord, growing with the rest of the body into a highly developed form, acts as a substitute for the pillar of the bodies of the vertebræ, no vertebral bodies being developed; but in cartilaginous and osseous fishes various gradations of cartilaginous and osseous structures come to surround the notochord and give rise to the simpler forms of vertebral bodies, which undergo more and more distinct development in the higher vertebrates. In all instances the substance forming the vertebral bodies is deposited on the surface of or outside the notochord and its sheath, so that this body remains for a time as a vestigial structure within the vertebral bodies of the higher animals.

The observations of Kowalevsky with respect to the existence of a notochord in the ascidia, which have been confirmed by Kupfer and others, have produced a change little short of

[1] If we reserve the words ectoderm and endoderm to designate the two layers of the primary bilaminar blastoderm, we may apply the terms epiblast and hypoblast to their derivatives after the formation of the mesoderm, and indicate the relations of the whole to the secondary or quadrilaminar blastoderm, by the accompanying Table:—

Primary Blastoderm	Ectoderm ...	....	Epiblast..........	Secondary Blastoderm.
		Mesoderm ...	Somatopleure ...	
			Splanchnopleure.	
	Endoderm ...	....	Hypoblast........	

[1] *Mém. de l'Acad. de St. Pétersbourg*, vol. x.

revolutionary in embryological and zoological views, leading as they do to the support of the hypothesis that the ascidia is an earlier stage in the phylogenetic history of the mammal and other vertebrates. The analogy between the amphioxus and ascidian larva is certainly most curious and striking as regards the relation of the notochord to other parts, and it is not difficult to conceive such a change in the form and position of the organs in their passage from the embryonic to the adult state as is not inconsistent with the supposition that the vertebrates and the ascidia may have had a common ancestral form. Kowalevsky's discovery opens up at least an entirely new path of inquiry; and we must be prepared to modify our views as to the entire separation of the vertebrates from the other groups of animals, if we do not at once adopt the hypothesis that through the ascidian and other forms the origin of the vertebrates may be traced downwards in the series to the lower grades of animal organisation.

The notochord extends a short way forward into the cranial basis, and an interesting question here presents itself, beginning with the speculations of Goethe and Oken, and still forming a subject of discussion, whether the series of cranial or cephalic bones is comparable to that of the vertebræ. On the whole it appears to me that it is consistent with the most recent views of the development and anatomy of the head to hold the opinion that it is composed of parts which are to some extent homologous with vertebral metameres.[1]

The history of the formation of the vertebral column presents an interesting example of the correspondence in the development of the individual and the race, in that all the stages which have been referred to, as occurring in the gradual evolution of the vertebral column in the series of vertebrates, are repeated in the successive stages of the embryonic development of the higher members of the series.

There is perhaps no part of the history of development in the vertebrates which illustrates in a more striking manner the similarity of plan which runs through the whole of them than that connected with what I may loosely call the region of the face and neck, including the apparatus of the jaws and gills. The embryonic parts I now refer to consist of a series of symmetrical pairs of plates which are developed at an early period below the cranium, and may therefore, in stricter embryological terms, be styled the *subcranial plates*.

Without attempting to follow out the remarkable changes which occur in the development of the nose and mouth in connection with the anterior set of these plates, which, from being placed before the mouth, are sometimes named *preoral*, I may here refer shortly to the history of the plates situated behind the mouth, which were discovered by Rathke in 1826, and formed the subject of an elaborate investigation by Reichert in 1837.

These plates consist of a series of symmetrical bars, four in number in mammals and birds, placed immediately behind the mouth, separated by clefts passing through the wall of the throat, and each traversed by a division of the great artery from the heart; thus constituting the type of a branchial apparatus, which in fishes and amphibia becomes converted into the well-known gills of these animals, whilst in reptiles, birds and mammals they undergo various changes leading to the formation of very different parts, which could not be recognised as having any relation to gill structure but for the observation of their earlier embryonic condition. The history of this part of development also possesses great interest on account of the extraordinary degree of general resemblance which it gives to the embryos of the most different animals at a certain stage of advancement—so great, indeed, that it requires a practised eye to distinguish between the embryos of very different orders of mammals, and even between some of them and the embryos of birds or reptiles, as well as in connection with the transformations of the first pair of branchial apertures, which lead to the formation of the passage from the throat to the ear in the higher vertebrata. There is equal interest attached to the history of the development of the first pair of arches which include the basis of formation of the lower jaw with the so-called *cartilage of Meckel*, and which, while furnishing the bone which suspends the lower jaw in reptiles and birds, is converted in mammals into the hammer-bone of the ear.

[1] See the interesting and valuable memoirs of W. K. Parker, "On the Anatomy and Development of the Vertebrate Skull," in *Trans.* of Roy. Soc., the researches of Gegenbaur, Mihalkovics, and more particularly the Memoir by F. M. Balfour, "On the Development of the Elasmobranchs," in the *Journ. of Anat. and Physiol.*, vols. x. and. xi.

The other arches undergo transformations which are hardly less marvellous, and the whole series of changes is such as never fails to impress the embryological inquirer with a forcible idea of the persistence of type and the inexhaustible variety of changes to which simple and fundamental parts may be subject in the process of their development.

It is also of deep significance in connection with the foregoing phenomena, to observe the increase in the number of the gill-bars and apertures as we descend in the scale to the cartilaginous fishes and lampreys, and the still further multiplication of these metameres or repeated parts in the amphioxus; and it is, perhaps, also interesting to note that in the ascidia the arrangement of the gills is exactly similar to that of the amphioxus.

The study of the comparative anatomy of the heart and its mode of formation in the embryo furnishes also most striking illustrations of the relation between ontogenetic and phylogenetic development in the vertebrates, and is not without its applications to some of the invertebrate groups of animals.

I need only recall to your recollection the completely double state of this organ in warm-blooded animals, by which a regular alternation of the systemic and pulmonary circulations is secured, and the series of gradations through the class of reptiles by which we arrive at the undivided ventricle of the amphibian, and the further transition in the latter animals by which we come at last to the single heart of fishes; and to state that in the embryo of the higher animals the changes by which the double heart is ultimately developed out of an extremely simple tubular form into which it is at first moulded from the primitive-formative cells are, in the inverse order, entirely analogous to those which I have just now indicated as traceable in the descending series of vertebrate animals; so that at first the embryonic heart of man and other warm-blooded animals is nothing more than a rhythmically contractile vascular tube. By the inflection of this tube, the constriction of its wall at certain parts, and the dilatation at others, the three chambers are formed which represent the single auricle, the single ventricle, and the aortic bulb of the fish. By later changes a septum is formed to divide the auricles, becoming completed in all the air-breathing animals, but remaining incomplete in the higher animals so long as the conditions of fœtal life prevent the return of arterialised blood to the left auricle. The growth of another septum within the ventricular portion gradually divides that cavity into two ventricles, repeating somewhat in its progress the variations observed in different reptiles, and attaining its complete state in the crocodile and warm-blooded animals.

I must not attempt to pursue this interesting subject further, but I cannot avoid making reference to the instructive view presented by the embryological study of the nature of the malformations to which the heart is subject, which, as in many other instances, are due to the persistence of transitory conditions which belong to different stages of progress in the development of the embryo. Nor can I do more than allude to the interesting series of changes by which the aortic-bulb, remaining single in fishes, and serving as the channel through which the whole stream of blood leaving the heart is passed into the gills, becomes divided in the higher animals into the roots of the two great vessels, the aorta and the pulmonary artery, and the remarkable transformations of the vascular arches which proceed from the aortic-bulb along the several branchial arches, and which, in the gills of fishes and aquatic amphibia, undergo thatt minute subdivision which belongs to the vascular distribution o gills, but which in the higher non-branchiated animals are the subject of very different and various changes in the partial obliteration of some, and the enlargement of others, by which the permanent vessels are produced.

These changes and transformations have for many years been a subject of much interest to comparative anatomists, and will continue to be so, not only from their presenting to us one of the most remarkable examples of conformity in the plan of development and the type of permanent or completed organisation in the whole series of vertebrated animals, but also because of the manifest dependence of the phenomena of their development upon external influences and atmospheric conditions which affect the respiration, nutrition, and modes of life of the animal.

Nor is the correspondence to which I now refer entirely limited to the vertebrata. For here, again, through the amphioxus and the ascidia, we come to see how an affinity may be traced between organs of circulation and respiration which at first appear to belong to very different types. The heart of vertebrates is, as is well known, an essentially concentrated form of vascular development in the ventral aspect of the body; while the heart of the invertebrate, whether in the more concentrated form existing in the articulata and muscula, or in a more subdivided shape prevalent in the annelida, is most frequently dorsal; yet the main aorta of the vertebrates is also dorsal; and it is not impossible through the intermediate form of amphioxus, to understand how the relation between the vertebrate and the invertebrate type of the blood-vascular system may be maintained.

But I am warned by the lapse of time that I must not attempt to pursue these illustrations further. In the statement which I have made of some of the more remarkable phenomena of organic production—too long, I fear, for your endurance, but much too brief to do justice to the subject—it has been my object mainly to show that they are all more or less closely related together by a chain of similarity of a very marked and unmistakable character; that in their simplest forms they are indeed, in so far as our powers of observation enable us to know them, identical; that in the lower grades of animal and vegetable life they are so similar as to pass by insensible gradations into each other; and that in the higher forms, while they diverge most widely in some of their aspects in the bodies belonging to the two great kingdoms of organic nature, and in the larger groups distinguishable within each of them, yet it is still possible, from the fundamental similarity of the phenomena, to trace in the transitional forms of all their varieties one great general plan of organisation.

In its simplest and earliest form that plan comprises a minute mass of the common nitrogenous hydrocarbon compound to which the name of protoplasm has been given, exhibiting the vital properties of assimilation, reproduction, and irritability; the second stage in this plan is the nucleated and inclosed condition of the protoplasmic mass in the organised cell. We next recognise the differentiation of two productive elements, and their combination for the formation of a more highly-endowed organising element in the embryonic germ-sphere or cell; and the fourth stage of advance in the complexity of the organising phenomena is in the multiplication of the fertilised embryo-cell, and its conversion into continuous organised strata, by further histological changes in which the morphological foundations of the future embryo or new being are laid.

I need not now recur to the further series of complications in the formative process by which the bilaminar blastoderm is developed, and becomes trilaminar or quadrilaminar, but only recall to your recollection that while these several states of the primordial condition of the incipient animal pass insensibly into each other, there is a pervading similarity in the nature of the histological changes by which they are reached, and that in the production of the endless variations of form assumed by the organs and systems of different animals in the course of their development, the process of cell-production, multiplication and differentiation remains identical. The more obvious morphological changes are of so similar a character throughout the whole, and so nearly allied in the different larger groups, that we are led to regard them as placed in some very close and intimate relation to the inherent properties of the organic substance which is their seat, and the ever-present influence of the vital conditions in which alone these properties manifest themselves.

The formative or organising property, therefore, resides in the living substance of every organised cell and in each of its component molecules, and is a necessary part of the physical and chemical constitution of the organising elements in the conditions of life; and it scarcely needs to be said that these conditions may be as varied as the countless numbers of the molecules which compose the smallest particles of their substance. But, setting aside all speculation of a merely pangenetic kind, it appears to me that no one could have engaged in the study of embryological development for any time without becoming convinced that the phenomena which have been ascertained as to the first origin and formation of textures and organs in any individual animal are of so uniform a character as to indicate forcibly a law of connection and continuity between them; nor will his study of the phenomena of development in different animals have gone far before he is equally strongly convinced of the similarity of plan in the development of the larger groups, and, to some extent, of the whole. I consider it impossible, therefore, for any one to be a faithful student of embryology, in the present state of science, without at the same time becoming an evolutionist. There may still be many difficulties, some inconsistencies, and much to learn, and there may remain beyond much which we shall never know; but I cannot conceive any doctrine professing to bring the phenomena of embryonic development within a general law which is not, like the theory of Darwin, consistent with their fundamental identity, their endless variability, their subjugation to varying external influences and conditions, and with the possibility of the transmission of the vital conditions and properties, with all their variations, from individual to individual, and, in the long lapse of ages, from race to race.

I regard it, therefore, as no exaggerated representation of the present state of our knowledge to say that the ontogenetic development of the individual in the higher animals repeats in its more general character, and in many of its specific phenomena, the phylogenetic development of the race. If we admit the progressive nature of the changes of development, their similarity in different groups, and their common characters in all animals, nay, even in some respects in both plants and animals, we can scarcely refuse to recognise the possibility of continuous derivation in the history of their origin; and however far we may be, by reason of the imperfection of our knowledge of palæontology, comparative anatomy, and embryology, from realising the precise nature of the chain of connection by which the actual descent has taken place, still there can be little doubt remaining in the minds of any unprejudiced student of embryology that it is only by the employment of such an hypothesis as that of evolution that farther investigation in these several departments will be promoted so as to bring us to a fuller comprehension of the most general law which regulates the adaptation of structure to function in the universe.

SECTION A.

MATHEMATICAL AND PHYSICAL.

Opening Address by the President, Prof. G. Carey Foster, F.R.S.

When any one fears that he has accepted a duty that is too difficult for him, or that he has allowed himself to be placed in a position, the responsibilities of which are greater than he can properly discharge, probably the very worst thing he can do is to proclaim his misgivings to the world. But though I fully believe in this rather obvious maxim, I cannot avoid saying that I enter upon my duties here to-day with very great diffidence, and that I feel the necessity of asking your indulgence at the outset for what I fear will be my inevitable shortcomings in discharging the functions of the honourable post that has been assigned to me. And I am sure that no one who calls to mind the names of some of those who, within recent years, have occupied the Chair of this Section, and who knows—however imperfectly—what those names stand for in connection with mathematics and physics, will be surprised that I should deprecate comparisons which might tend to degenerate into contrasts, or that I should shrink from having my performances measured by the standard of such predecessors. But I have neither the right nor the desire to detain you longer with this purely personal topic, and I therefore proceed to ask your attention to matters more closely connected with the business which has brought us here.

The periodically recurring character of these meetings unavoidably suggests, at each recurrence, a retrospect at the scientific work of the year, and an attempt to estimate the advances which have been the result of this work. At first sight nothing would seem to be more natural or appropriate than that each president of a section should occupy the introductory remarks, which the custom of the Association demands from him, with an account of the chief forward steps made during the past year in the branches of science represented by his Section.

Very little consideration, however, is sufficient to show that, in the case at least of Section A, to give anything like a general report of progress would be a task which few, if any, men could perform single-handed. To say nothing of the enormous *amount* of the material which is now the result of a year's scientific activity, the *variety*—or I might even say the unlikeness—of the subjects of which this Section takes cognizance is so great that, in most cases, it would be safe to conclude, from the mere fact of a man being able adequately to expound the recent advances in one of these subjects, that he must have given so much attention to this one as to have made it impossible for him to have followed carefully the progress of the rest.

But even supposing that all presidents of Section A were able to discourse with full and equal knowledge of hyper-Jacobian surfaces, the influence of temperature on the capillary constant of dilute sulphuric acid, or the latest improvement in the construction of aneroid barometers, some consideration would still be due to their audience. And, long-suffering as British Association audiences have often shown themselves to be, there is no doubt that before a tenth part could be read of a report on the year's work on the subjects included in this Section, the room would be cleared and most of those who came to hear about mathematics and physics would have gone to try whether they could not find in Section E or F something appealing more directly to the common sympathies of mankind.

But although a serious report of progress would thus be both impossible and unsuitable in the form of an Address to the Section, it remains none the less true that such reports are in themselves of the utmost scientific value, and, as has been pointed out repeatedly, there are few ways in which the British Association could more effectually fulfil its function of promoting the advancement of science than by aiding in their preparation and publication. But when one tries to think out in detail the way in which the Association could do this, the practical difficulties of the scheme are seen to be neither few nor trifling. It may be sufficient to point out that there is no evident reason why help of this kind should be afforded to one branch of science rather than to another, and that the publication of reports upon all branches would completely overtax the resources of the Association.

In the case of some important sciences, however, the work of reporting recent advances is already undertaken by other bodies, —thus there are the "Abstracts" published monthly in the *Journal* of the Chemical Society, and there are the *Zoological Record*, the *Geological Record*, and other publications of a like nature,—but hitherto nothing of the kind has been done in this country for those departments of science with which this Section is specially concerned. But without attempting to commit the Association to any burdensome outlay, or to any larger scheme than it would be practicable to carry out, it seems to me possible that a systematic series of reports might be established in connection with this Section which would have a very high value. In the early volumes of the British Association's *Transactions* we find more frequently than in recent ones, reports, not merely on some special investigation, but on the recent progress and present state of some more or less comprehensive branch of science. Thus in the first four volumes we find the following, among other reports, presented to this Section:—On the Progress of Astronomy, On the Present State of Meteorology, On the Present State of the Science of Radiant Heat, On the Progress of Optics, On the Magnetism of the Earth, On Capillary Attraction, On Physical Optics, On the Recent Progress and Present Condition of the Mathematical Theories of Electricity, Magnetism, and Heat. Now I venture to think that this form of the activity of the Association might with great advantage be revived and systematised. I would suggest, as a plan that seems to me worth consideration by the Committee of this Section, the appointment of committees charged to report to the Section periodically on the advances made in each of the chief departments of science of which we here take cognizance. For example, to confine myself specially to physics, we might have a committee on Optics, a committee on Acoustics, one on Heat, one on Electricity, and so on. It would not be in accordance with the usages of the Association to nominate these as standing committees, but they might be made virtually such by annual reappointment. I would suggest that they should not report annually, but at intervals of perhaps five or six years, the times being so arranged that different committees should report in different years, the report in each case being a systematic account of all the work of any importance done on the subject and within the period to which it related. In order not to make the work too heavy, it would probably be needful to make each committee comparatively numerous, so that individual members might each undertake to report upon some limited part of the general subject. Some one member of each committee would also require to act as editor; his function would be not merely to put together the detached fragments sent in by his colleagues, but to distribute to them the materials on which they would have to report. For this purpose it would be needful that copies of all the important scientific periodicals relating to Physics should be supplied to the committee; but besides providing these and printing the reports, I do not see that the Association need be put to any expense; and if it were thought well to sell the reports independently of the yearly volumes of the Association, probably a good part even of this expense might be recovered.

The mutual relations subsisting between the two great groups of sciences, which we discuss in this Section under the names Mathematics and Physics, offer so many deeply interesting points for consideration that, at the risk of reminding you how admirably and with what fulness of knowledge the same subject has been treated by more than one of my predecessors in this Chair, I venture to ask your attention once more to a few remarks on this topic.

The intimate connection between Mathematics and Physics arises out of the fact that all scientific knowledge of physical phenomena is based upon *measurements*,—that is to say, upon the discovery of relations of number, quantity and position, of the same kind as those which form the subject matter of mathematics. It is true that in studying physics we have to learn much about the quality of phenomena and of the conditions under which they occur, as well as about their purely quantitative relations; but even in the qualitative study of physical phenomena we find it impossible to determine what is really characteristic and to distinguish the essential from the accidental, except by the aid of measurements. In fact if we take the most elementary treatise upon any branch of physics that we can meet with, a book it may be which aims at giving a purely descriptive account of phenomena, we find, when we examine it, that numberless careful measurements have been required to establish the truth of the merely qualitative statements which it contains. To take a simple and well-known example, the old question whether the ascent of water in a pump was due to the pressure of the atmosphere, or to Nature's horror of a vacuum, was not conclusively settled by Torricelli's discovery that mercury would not rise beyond a certain height in a glass tube, even to prevent a vacuum being formed at the top of it, for the same thing was already known about the water in a pump. But, when he measured the height of the mercury-column in his tube and found that if he multiplied it by the specific gravity of mercury, the product was equal to thirty-two feet, the height to which, as Galileo said (probaby between jest and earnest) nature's abhorrence of a vacuum in a pump extended, it was clear that the ascent both of water and of mercury depended upon the particular depth of each liquid that was needed to produce some definite pressure; and when Pascal had persuaded his brother-in-law to carry a Torricelli's tube to the top of the Puy de Dôme, and he had measured the height of the mercury-column at the top of the mountain as well as at the foot, the proof was completed that the pressure which determined the height of both the water and the mercury was the pressure of the atmosphere.

Again, let us examine a still more familiar phenomenon, the falling of heavy bodies to the ground. So long as we consider this merely under its general, or, as we may call them, its qualitative aspects, we might reasonably infer that it is the result of some inherent tendency of bodies; and, so far from its seeming to be true, as stated in Newton's "First Law of Motion," that bodies have no power to alter their condition of rest or of motion, we might infer that however indifferent they may be as to horizontal motion, they have a distinct tendency to move downwards whenever they can, and a distinct disinclination to move upwards. But when we measure the direction in which bodies tend to fall and the amount of the tendency in different places, and find that these vary in the way that they are known to do with geographical position and distance from the sea-level, we are obliged to conclude that there is no inherent tendency to motion at all, but that falling is the result of some mutual action exerted between the earth and the falling body. For if we suppose falling to be due to any internal cause, we must imagine something much more complicated than a mere tendency to motion in one direction, else, how could a stone that has always fallen in one direction in England, fall in almost exactly the opposite direction as soon as it is taken to New Zealand?

These two simple examples illustrate a principle that we meet with throughout Physics: namely that, in the investigation of the causes of physical phenomena, or in other words, of the connection between these phenomena and the conditions under which they occur, the really decisive guidance is afforded by the study of their measurable aspects.

The consequence is that from the very outset of his investigations the physicist has to rely constantly on the aid of the mathematician, for even in the simplest cases, the direct results of his measuring operations are entirely without meaning until they have been submitted to more or less of mathematical discussion. And when in this way some interpretation of the experimental results has been arrived at, and it has been proved that two or more physical quantities stand in a definite relation to each other, the mathematician is very often able to infer, from the existence of this relation, that the quantities in question also fulfil some other relation, that was previously unsuspected. Thus when Coulomb, combining the functions of experimentalist and mathematician, had discovered the law of the force exerted between two particles of electricity, it became a purely mathematical problem, not requiring any further experiment, to ascertain how electricity is distributed upon a charged conductor, and this problem has been solved by mathematicians in several cases.

It thus happens that a very large part of our knowledge of physics is due in the first instance to the mathematical discussion of previous results, and is experimental only in the second, or perhaps still more remote degree.

Another way in which the mathematician co-operates in the discovery of physical truths is almost exactly the converse of that last-mentioned. In very many cases the most obvious and direct experimental method of investigating a given problem is extremely difficult, or for some reason or other untrustworthy. In such cases the mathematician can often point out some other problem more accessible to experimental treatment, the solution of which involves the solution of the former one. For example, if we try to deduce from direct experiments the law according to which one pole of a magnet attracts or repels a pole of another magnet, the observed action is so much complicated with the effects of the mutual induction of the magnets and of the forces due to the second pole of each magnet, that it is next to impossible to obtain results of any great accuracy. Gauss, however, showed how the law which applied in the case mentioned can be deduced from the deflections undergone by a small suspended magnetic needle when it is acted upon by a small fixed magnet placed successively in two determinate positions relatively to the needle; and being an experimentalist as well as a mathematician, he showed likewise how these deflections can be measured very easily and with great precision.

It thus appears not only that mathematical investigations have aided at every step whereby the present stage in the development of a knowledge of physics have been reached, but that mathematics has continually entered more and more into the very substance of physics, or, as a physiologist might say, has been assimilated by it to a greater and greater extent.

Another way of convincing ourselves how largely this process has gone on would be to try to conceive the effect of some intellectual catastrophe, supposing such a thing possible, whereby all knowledge of mathematics should be swept away from men's minds. Would it not be that the departure of mathematics would be the destruction of physics? Objective physical phenomena would, indeed, remain as they are now, but physical science would cease to exist. We should no doubt see the same colours on looking into a spectroscope or polariscope, vibrating strings would produce the same sounds, electrical machines would give sparks, and galvanometer needles would be deflected; but all these things would have lost their meaning; they would be but as the dry bones—the *disjecta membra*—of what is now a living and growing science. To follow this conception further, and to try to image to ourselves in some detail what would be the kind of knowledge of physics which would remain possible, supposing all mathematical ideas to be blotted out, would be extremely interesting, but it would lead us directly into a dim and entangled region where the subjective seems to be always passing itself off for the objective, and where I at least could not attempt to lead the way, gladly as I would follow any one who could show where a firm footing is to be found. But without venturing to do more than look from a safe distance over this puzzling ground, we may see clearly enough that mathematics is the connective tissue of physics, binding what would else be merely a list of detached observations into an organised body of science.

In my opinion, however, it would be a very serious misconception to suppose that on this account an elaborate apparatus of technical mathematics is in general needful for the proper presentation of physical truths. The ladders and ropes of formulæ are no doubt often essential during the building up of a newly-discovered physical principle, but the more thoroughly the building is finished, the more completely will these signs that it is still in progress be cleared away, and easy ascents be provided to all parts of the edifice. In an address delivered from the Chair of this Section four years ago, Prof. Henry Smith quoted the saying of an old French geometer, "that a mathematical theory was never to be considered complete till you had made it so clear that you could explain it to the first man you met in the street." Very likely Prof. Smith was right to call this "a brilliant exaggeration," at any rate I know of no reason for disputing his opinion, but I believe the exaggeration would really be very small if the dictum were applied to the theories of physics instead of to those of pure mathematics. When a physical principle or theory is grasped with thorough clearness, I believe it is possible to explain it to the man in the street; only he must not be hurrying to catch a train; and it would, I think, be difficult to find a more wholesome maxim to be kept in mind by those of us whose business it is to teach physics, than that we should never think we understand a principle till we can explain it to the man in the street. I do not say that our modes of exposition should always be adapted to him, for as a rule, he forms but a small part of our audience, but even when the conditions are such that a teacher is free to avail himself to the fullest extent of mathematical methods, I believe he would find his mathematical discussions gain marvellously in freshness and vigour if he had once made up his mind how he would treat his subject supposing all use of mathematical technicalities denied him.

So far, in considering the mutual relations of mathematics and physics, I have placed myself, as it was natural for me to do, at a physical point of view, and, starting from the fact that the existence and progress of the latter science are essentially dependent upon help derived from the former, I have tried to point out some of the ways in which this help is rendered. If we turn now to inquire in what light the relations between the two sciences appear from the side of mathematics, we find that mathematicians are not slow to admit the advantages which their science derives from contact with physics. It was a saying of Fourier that "a more attentive study of nature is the most fruitful source of mathematical discoveries;" and Prof. Henry Smith, in the Address I have already referred to, says that "probably by far the greater part of the accessions to our mathematical knowledge have been obtained by the efforts of mathematicians to solve the problems set to them by experiment." We may perhaps regard such expressions as equivalent to the statement that the law of inertia is not without application even to the mind of the mathematician, and that it, too, continues to move in a straight line "except in so far as it may be compelled by impressed forces" to change its direction; or, to put the matter a little differently, may we not look upon the fact as illustrating what is probably a general principle of mental action, namely, that the human mind has no more power to create an idea than the hand has to create matter or energy—our seemingly most original conceptions being in reality due to suggestions from without? But however this may be, the fact remains that the origin of many most important mathematical theorems, and even entire departments of mathematics, can be distinctly traced to the attempt to express mathematically the observed relations among physical magnitudes. By way of illustration of this statement, it may suffice to refer to the well-known cases of the theory of fluctions, to Fourier's theorem and the doctrine of harmonic analysis, to spherical harmonics, and to the theory of the potential.

The way in which physics reacts, so as to promote the advancement of a knowledge of mathematics, finds in many respects a close parallel in the influence exerted by the practical industrial arts on the progress of physics. This influence shows itself very distinctly, first, in the new conceptions and new points of view which practical pursuits supply to scientific physics, and, secondly, in the new subjects and opportunities which they offer for physical investigation.

A very remarkable and important example of the former kind of influence is afforded by the idea of Work and the correlative one of Energy. These ideas, which have been found to have a most far-reaching significance, and have exerted a transforming effect upon every branch of physics, owe their recognition, not to the spontaneous growth of science, but to their having been forced on the attention of physicists by the cultivators of practical mechanics.[1] Very much the same thing may also be said of the modern conception of the nature of heat, and of the relation between thermal phenomena and those of other branches of physics. The notion of heat as a measurable magnitude, of which definite quantities could be given to or taken away from

[1] See, on this point, Dühring, "Kritische Geschichte der allgemeinen Principien der Mechanik" (Berlin, 1873), pp. 483–486.

bodies, was fully established by the researches of Black and Wilke on latent and specific heat. This was at the time when the idea of chemical composition was just taking its modern shape through the recognition of aëriform bodies as possible constituents of solids and liquids, and it was natural that the new knowledge with regard to heat should be embodied in the conception of a matter of heat, or caloric, capable of entering into or separating from combination like fixed air or dephlogisticated air. And in fact this conception not only took the place of philosophical speculations upon the nature of heat such as those of Bacon and Locke, but it withstood the experimental onslaughts of Rumford and Davy, as well as the penetrating scientific criticism of Thomas Young. It is to the steam-engine, and to the attempt to find out the connection obviously existing between the amount of heat supplied and the work done by the engine, that we must trace the downfall of the idea of the materiality of heat and the origin of our modern views.[1]

Probably it would be impossible to find a more remarkable instance of what I referred to just now as the second way in which practice may react upon science so as to promote its advancement, than is presented to us in the case of electric telegraphy. This is an example of an industrial undertaking which is the direct offspring of scientific research, and could not have co-existed in its actual state of development with a less advanced condition of electrical science; but if it were possible to establish any common measure for such things, it may be doubted whether it would not be found that telegraphy has repaid to science benefits equal to those it has received. For instance, the discovery of earth-currents was a direct result of the large scale of the instrumental arrangements which are needed for telegraphic purposes, and is one which would probably have long remained unmade in the absence of some inducement to make experiments on a scale greater than that indicated by the visible wants of scientific inquiry. The same is true of the discovery of the influence of electro-static induction upon the transmission of electric currents through metallic conductors, and of the consequent additions to our knowledge of the specific inductive capacity of insulators and of the whole subject of electrostatic capacity. But by far the most important of the benefits conferred by electric telegraphy upon electrical science have resulted from the necessity under which the practical electrician found himself, of not only being able to produce certain results, but of producing them under definitely ascertained conditions as to the expenditure of time and material. When it was perceived that slight variations in the electrical conductivity, insulating power, or specific inductive capacity of certain materials might affect the pecuniary return upon investments reckoned in millions of pounds sterling, measuring instruments were devised which far surpassed in delicacy and accuracy those that had been previously made for purely scientific purposes, or the cost of which exceeded the means usually at the disposal of scientific investigators. The multiplication and wide diffusion of such instruments has led to the rapid accumulation of numerical data of great scientific importance, and has largely contributed to the spread of accurate conceptions as to the quantitative laws of electrical phenomena. But the further necessity experienced by practical electricians, that, besides being able to make accurate measurements, they should be able mutually to communicate and to understand each other's results, has probably done more than anything else to hasten the introduction for scientific purposes of so-called "absolute" measurements, instead of mere comparisons of each quantity to be estimated with a standard magnitude of its own kind. The use of absolute measures constitutes one of the most characteristic differences between the physics of to-day and that of the time when the British Association was instituted, and it may be even said to lie at the base of the doctrine of the Conservation of Energy, which implies the principle that every kind of energy can be reduced to the same denomination.

Perhaps, after speaking as I have done of the necessity for the co-operation of mathematics in the advancement of physics, it is not inappropriate that I should, in conclusion, refer to the possibility that, by a too implicit reliance upon mathematical guidance, the physicist may be led away from the discovery of fresh truth, or even into actual error. Mathematics is seen to be so indispensable and usually so powerful an aid in physical investigation, that there is a danger of forgetting that there are after all limits to its power. Partly from want of sufficient knowledge of the physical data on which mathematical discussion must be based, and partly from the imperfection of mathematical methods themselves, it happens that it is not possible to give a thoroughly complete mathematical account of even the simplest physical phenomenon. In all real cases, although some one effect may often predominate so greatly as alone to attract attention on a cursory view, the actual complexity is so great that it is only by deliberately leaving out of consideration what we believe to be the accidental accompaniments of a phenomenon, and confining our attention to what seems to be its essential and characteristic part that it is possible to make it the subject of mathematical calculation. The consequence is that the problems treated of in mathematical physics are not the problems presented by nature, but are problems suggested by these, and derived from them by a process of ideal simplification. There is, therefore, always a possibility that, in this simplifying process, some apparently trivial but really important feature of the actual phenomenon, to which the ideal one is meant to correspond, may have been overlooked. When this is the case, the fact will reveal itself sooner or later by the occurrence of discrepancies between the results of mathematical theory and those of experimental investigation. Such discrepancies are the finger-posts, pointing to new discoveries; but the experimenter who forgets the inevitable limitation of the authority of theoretical conclusions, arising from the conditions I have alluded to, is apt to disregard them, and, perhaps conscious of laziness and want of care in his method of working, or sometimes from a want of proper self-confidence, he attributes all anomalous results to "the unavoidable errors of observation."

Two classes of experimenters are safe from falling into this danger. There are first, those who, the first time they observe anything that is not provided for in their text-books, conclude that the law of gravitation ought to be reconsidered. Secondly, there are those who, with scrupulous care, take account of all the conditions which are known to be able to affect the phenomenon they are investigating, and are thus able to say, with well-founded confidence, when they meet with some unforeseen result, that it must indicate the operation of some unrecognised cause.

A brilliant example of this latter mode of working and of the discoveries to which it may lead has recently been afforded to us by the researches of Mr. Crookes, some of whose results, as embodied in the now well-known instrument which he has called the *radiometer*, have attracted much attention. It has appeared to me however, that the surprising nature of these results has to some extent called off attention from the remarkable character of the scientific investigation which led to them, and it was at one time my intention to take advantage of the present opportunity for the purpose of trying, on the one hand, to render to Mr. Crookes the credit which I think his researches deserve, and, on the other hand, to give a connected account of the further investigations, both experimental and theoretical, to which these researches have given rise. There seemed to be the more reason for endeavouring to carry out the former part of my intention, inasmuch as an eminent and accomplished scientific man had published, within the last few months, an account of the discovery of the radiometer, the unmistakable tendency of which was, either intentionally or unintentionally, to depreciate Mr. Crookes's merits, and to make it appear that he had put a wrong interpretation upon his own results. I found, however, that the time at my disposal would not enable me to make myself sufficiently master of the whole subject to treat it in the way that I wished, and I have therefore been obliged to content myself with merely making this allusion to it as an illustration of the more general considerations to which I have ventured to ask your attention.

SECTION B.

CHEMICAL SCIENCE.

Opening Address by the President, Prof. Abel, F.R.S.

The subject which my predecessor in the honourable position of President of this Section, made the chief topic of his interesting and instructive address, affords excellent illustrations of the operation of purely scientific research in creating and developing important branches of industry. Mr. Perkin, whose name has from the very commencement of the history of coal-tar colours been identified with their discovery and their scientific and technical history, referred to several series of researches, each one of which formed a link in a chain of discoveries in

[1] Conf. Dühring, *loc. cit.*

organic chemistry of the highest value, as establishing, illustrating, or extending important chemical theories, but, at the time, and for long afterwards, of value purely from a scientific point of view. These researches, undertaken and pursued by ardent and philosophical investigators under more or less formidable difficulties, and solely in the interests of science, resulted in the discovery of certain organic bodies which were produced originally only on a very small scale and at great cost, but which, after the lapse of years, have been readily manufactured from abundant sources, and have constituted important elements in the development of the industry of artificial colouring matters. In fact, this industry, which owes its origin to the discovery of mauve by Mr. Perkin about twenty years ago, and which is second to no branch of chemical industry in regard to the rapidity of its development, and its influence upon other important branches of manufacture, affords more copious illustrations than any other of the immediate influence of pure science upon industrial progress. It therefore affords a topic which the chemist may well be excused for continually recurring to, with an interest bordering on enthusiasm, when illustrating the material advantages which accrue to communities from the promotion of scientific training and the encouragement of chemical research.

The iron and steel industry presents a great contrast to that of the artificial colours in regard to the extent of influence which the labours of purely scientific investigators have exerted upon its development. The efforts of scientific men to unravel such problems as, for instance, the true chemical constitution of steel, or the precise differences between the various combinations known as cast iron, and the conditions which determine their individual production or conversion from one to another, have hitherto been attended by results not at all proportionate to the patient experimental investigation of which from time to time they have been made the subject. Thus, the protracted experiments and discussion carried on by Frémy and Caron some years back, with reference to the dependence of the characteristics of steel upon the existence in it of nitrogen, cannot be said to have led to results of a more conclusive or even definite nature, regarding the conditions which regulate the production, composition, and properties of steel, than those arrived at by previous distinguished experimenters;—and the same must be said, with respect to cast iron, of such experiments as those carried on for several years by Matthiessen (in which I also took some part) under the auspices of the Association, with the view to eliminate many existing points of doubt regarding the chemical constitution of cast iron, by preparing chemically pure iron, and studying its combination with carbon and other elements occurring in cast iron.

The prosecution of purely scientific investigation may, therefore, of itself fail to bear *direct* fruit in regard to the development of new metallurgic achievements, or even to the elucidation of the comparatively complicated and numerous reactions which occur in furnaces, either simultaneously or in rapid and difficultly controllable succession, between materials composed of a variety of constituents in variable proportions. There can, however, be no question regarding the important benefits which have accrued from the application of chemical knowledge to the study and the perfection of furnace-operations by those who happily combine that knowledge with practical experience, and with the power of putting to the test of actual practice, the results of reasoning upon an intelligent observation of the phenomena exhibited in such operations, and upon the data which chemical analysis has furnished. In the hands of such men, the scientific results arrived at by Karsten, Berthius, Bunsen, Scheerer, Percy, and other eminent investigators, acquire new value, and by them the fruits of the labours of the patient toiler at analytical processes meets with that appreciation which their solid and permanently valuable work does not always command at the hands of their numerous brother-workers in chemical science, who follow the far more attractive paths of organic research.

Naturally, the brilliant results achieved from time to time by investigators in organic chemistry, the rapidity with which, by those results, theories are established or extended, types founded, their offspring multiplied, and their connection with other families traced and developed, impart to organic research a charm peculiarly its own. This, and the general ease with which new results are obtained by the pursuit of methods of research comparatively simple in their nature and few in kind, have for many years not only secured to organic chemistry an overwhelming majority of workers, they also appear to have had a tendency to lead the younger labourers in the field of organic research to under-estimate the value and importance, in reference to the advancement of science, of the labours of the plodding investigator of analysis. Yet no higher example can be furnished of the patient pursuit of scientific work purely for its own sake than that of the deviser or improver of analytical processes, who, undeterred by failure upon failure, indefatigably pursues his laborious work, probing to its foundation each possible source of error, carefully comparing the results he obtains with those furnished by other methods of analysis, and patiently accumulating experimental data, till they suffice fully to establish the value and trustworthiness of the process which he then publishes for the benefit of his fellow-workers in science. Truly, the results of such labours do not stand in unfavourable contrast, from whatever light they may be viewed, to those of the investigator of organic chemistry. It is not to be denied that the labourer at organic research may, so far as the analytical work which should fall to his share in the course of his investigations is concerned, be tempted to reduce this, the least attractive portion of his work, to within the smallest possible limits; and having, for example, by a boiling point determination, or a single analytical operation of the simplest kind, such as the examination of a platinum-salt, obtained a numerical result approximative to that which his theory demands, may hasten on to the further development of his airy structure, possibly not without risk to its stability. Unquestionably there are instances of frequent occurrence, in the pursuit of a particular line of organic research, in which more is not required than the identification of a particular product by some such simple means as above indicated. It is certain moreover that the labours of the organic investigator also not unfrequently afford bright examples of indomitable perseverance under formidable difficulties, and this alone should constitute a strong bond of union between the worker in organic research and his brother worker in analytical chemistry, if one did not already exist in the active interest which each, if a true lover of science, must take in the work of the other.

It has been remarked by one of the most distinguished investigators and, at the same time, one of the most brilliant lecturers and successful teachers of our time, that the contrivance of a new and good lecture-experiment may rank in importance with the preparation of a new organic compound; and it may certainly be said with equal truth that the elaboration of a new and good method of analysis may rank in importance with a good research in organic chemistry, in reference both to the part it plays in the advancement of science, and to its influence upon industrial progress.

An excellent illustration of this is afforded by reference to the Proceedings of the British Association when it met in this town thirty-six years ago. In a letter to Dr. Playfair, Liebig, who took a very active part in the proceedings of the Association in the earlier years of its existence, reports that Doctors Will and Varrentrapp have devised an excellent method for determining the amount of nitrogen in organic bodies, "very exact and easily performed." He then described in a few lines the process so well known to chemists, which not only has been, and continues to be, invaluable to those engaged in organic research, but which, as may be testified by such researches as those of Lawes and Gilbert, has borne a most important and indispensable part in the advancement of agricultural chemistry. It is, I believe, but an expression of the unanimous conviction of chemists to say that the achievements in analytical chemistry of such men as Berzelius, Heinrich, Rose, and Fresenius, take equal rank with the brilliant researches and theoretical expositions of such chemists as Liebig, Laurent, Gerhardt, and Berthelot; and that of all the important contributions to the development of organic chemistry which we owe to Liebig, there is none which has exerted so great an influence on the progress of this branch of chemical science as his beautifully simple method of organic elementary analysis.

Reverting to the industry of iron and steel, which, in regard to some of the most important branches, cannot fail to be a subject of special interest in Plymouth and Devonport, it is not difficult to demonstrate that the labours of the analytical chemist have exercised, and continue to exert, an important influence on the very considerable advance which has in recent years been made, and still proceeds towards securing complete control over the quality and character of the products obtained. The epoch is well within the recollection of chemists of my generation, when the British iron-master first awoke to the benefits which might accrue to him from an application of the labours of the analytical chemist in connection with iron-smelting.

When the last great stride was made in the manufacture of cast-iron by the introduction of the hot blast, the iron-smelter was naturally led to seek profit, to the fullest extent, with respect both to the great increase in the rate of production of pig-iron attainable thereby and to the economy achievable in regard to the proportions and characters of the materials employed in the production of pig-iron. But after a time the great falling-off in the quality of a large proportion of the products of the blast-furnace, and the difficulties experienced in the production of malleable iron of even very moderate quality, aided by the great impetus to competition in respect of quality, given by the first International Exhibition in 1851, directed the attention of our more enlightened iron-masters to the likelihood of their deriving important aid from chemical science, and more especially from the investigations of the analytical chemist.

Among the earliest to realise the importance of trustworthy and detailed information regarding the composition of the iron ores of the country was Mr. S. H. Blackwell, who, in presenting to the Royal School of Mines a very extensive and interesting series of British ores which he had collected with great labour and expense for exhibition in 1851, placed at the disposal of Dr. Percy the requisite funds for engaging the services of competent analysts (Messrs. J. Spiller and A. H. Dick) who, under his direction and with subsequent pecuniary aid from himself and from Government funds, carried out a very careful and complete examination of this series, the results of which have been of great value, for purposes of reference, to those actively interested in the iron industry. It was, however, the first connection of Messrs. Nicholson and D. S. Price and of Mr. E. Riley with two of the most important iron works of this country, about a quarter of a century ago (*i.e.* at the time when the above investigation was commenced), that marked, I believe, the commencement of systematic endeavours to apply the results of analytical research to the improvement and regulation of the quality of the products of our iron works.

It is, perhaps, but natural that the primary object sought by applications of the knowledge of the analytical chemist should have been to eliminate or reduce the existing elements of uncertainty in obtaining the most abundant yield of pig-iron capable of conversion into railway-bar sufficiently good to meet the minimum standard of quality, and to reduce still further the cost of production of such bar-iron by utilising materials concerning the composition of which (richness in iron, &c.) the iron-smelter was completely in the dark. The information accumulated by the analyst respecting the composition of the ores, fuel and fluxes available at the works, and the composition of the pig-iron and slags or cinders, produced under varied conditions, in regard to materials employed, and to the proportion of ore, fuel, and flux used in the blast furnace, could not, however, exist long without exerting a marked beneficial influence upon the quality of iron produced, and generally upon the iron industry of the country.

Percy's invaluable work of reference on Metallurgy furnishes abundant evidence of the scientifically interesting, as well as practically useful, nature of the results obtained at that time by the chemists above named, and others, working under Dr. Percy, with respect both to the elaboration of important analytical processes (in which direction Mr. Riley has continued to the present day to do valuable work) and to the elucidation of the reactions occurring in the processes of reduction and refining of the metal. It is needless to dwell upon the fact that the aid of the analyst has now long since become absolutely indispensable to the iron and steel manufacturer; but I may, perhaps, be allowed briefly to refer to one or two recent illustrations of the indispensable part which analytical research has played, and continues to play, in the extension of our knowledge of the chemical reactions involved in the production of cast and wrought iron and of steel, and of the influences which the chief associates of iron in its mercantile forms exert upon its physical characters.

Among the many valuable communications made to that most important body, the Iron and Steel Institute of Great Britain, by men who combine great practical knowledge and experience in iron and steel manufacture with high attainments in mechanical science and such knowledge of chemical science as insures a full appreciation of its value at their hands, one of the most interesting and suggestive to the chemist is that on the separation of carbon, sulphur, silicon, and phosphorus in the refining and puddling furnace and in the Bessemer Converter, contributed to the *Transactions* of the Institute's recent meeting, by Mr. Lowthian Bell, whose valuable investigations in connection with the iron industry are as interesting to the chemist as they are useful to the manufacturer. Mr. Bell has brought together the results of an extensive series of practical experiments on the treatment of different kinds of pig-iron of known composition, in the finery, the puddling-furnace, and the Bessemer Converter, and, by comparing the results of analytical investigation of the products of those experimental operations with each other and with those of the materials operated upon, he has obtained valuable confirmation of the views already held by metallurgic chemists regarding the succession in which carbon, silicon, sulphur, and phosphorus are attacked when pig-metal is submitted to the above purifying processes, and the extent to which those foreign associates of iron are abstracted or resist removal, by the more or less thorough application of those several modes of treatment. He has also thrown new light on the reasons why the most difficultly-available impurity, phosphorus, obstinately resists all attempts to effect even a slight diminution in its amount by application of the Bessemer treatment. The earnestness with which Mr. Bell wages war against this enemy of the iron-master in one of its most favourite haunts, the Cleveland District, not simply with the old British pluck, which acknowledges not defeat, but systematically, on scientific principles, calling to his aid all the resources which the continual advances in applied mechanical and chemical research place within his reach, cannot fail to contribute importantly, if it does not of itself directly lead, to the complete subjection of this most untractable of the associates to which iron becomes linked in the blast-furnace. Indications have lately not been wanting that the existence of phosphorus in very notable proportion in iron may not of necessity be inimical to its conversion into steel of good quality, and it may be that this element, which is now turned to useful account to impart particular characteristics to the alloys of copper and tin, is even destined to play a distinctly useful part in connection with the production of steel possessed of particular characters valuable for some special purpose.

In the great development which steel manufacture has received within the last few years, one most prominent feature has been the production, with precision, upon a large scale, of steel of desired characteristics, in regard to hardness, &c., by first adding to fluid cast-iron of known composition the requisite proportion of a rich iron ore (with or without the addition of scrap iron) to affect a reduction of the carbon to the desired amount, concurrent with a refining of the metal by the oxidising action of the ore, and then giving to the resulting steel the desired special qualities by the addition of suitable proportions of iron compound of known composition, rich in manganese and carbon (Spiegeleisen and the similar product called ferro-manganese). The germ of this system of producing steel varieties of predetermined characteristics exists in crucible processes like that of Uchatius, which have been in more or less extensive use for many years past, but it is to such invaluable arrangements as are most prominently represented by the Siemens-Martin Furnace—wherein several tons of metal may be fused and maintained at a very high temperature with a little liability to change from causes not under control, as if the operation were conducted in a crucible—that we are indebted for the very great expansion which the direct application of the analytical chemist's labours to the development of the steel industry is now receiving.

The production of steel upon the open hearth, to the elaboration of which Dr. C. K. Siemens has so largely contributed since he first established the process at Llandore in 1868, has in fact, become assimilated in simplicity of character and precision of results to a laboratory operation, and may be justly regarded as a triumph of the successful application of chemical principles and of the power of guidance and control afforded by utilising analytical research, to the attainment of prescribed results upon a stupendous scale, with an accuracy approaching that which the experienced chemical operator secures in the laboratory upon a small scale, under conditions which he can completely control. The production of steel by a large number of small separate operations in pots has now become supplanted with great advantage by the Siemens-Martin system of working at some of our largest establishments at Sheffield; this system has also secured a footing at highly renowned Continental works, which are formidable competitors with us in the manufacture of steel, such as those of Essen, Creusot, and Terrenoire. It is specially interesting to notice that, in the hands of those who, on the Continent at least equally with ourselves, have learned to combine the results of practical experience with the teachings of chemical science, the facilities now existing for dealing in a single receptacle with large masses of fluid steel have greatly facilitated the application of chemical means to the production of *solid* masses of considerable size, thereby reducing, if not altogether dispensing with the necessity for submitting large steel castings to costly mechanical operations with the object of closing up cavities caused by the escape of occluded gas as the liquid metal cools. The success in this direction which appears to have attended the addition of silicon, in combination with iron and manganese, to the steel before casting in preventing the formation of so-called *blow-holes*, and in contributing at the same time to the production of the particular character of steel required, bids fair to be of special importance in connection with the application of steel to the production of projectiles for use against armour-plates, as affording ready and comparatively very economical means of ensuring the production of perfectly sound castings, or which in compactness of structure will, it is asserted, compete successfully with carefully forged castings, and even with the magnificent material which Whitworth produces by submitting the fluid metal to powerful pressure.

The part which silicon plays by its comparatively high susceptibility to oxidation, in promoting the production of sound steel castings is readily intelligible, but the functions of the manganese compounds which are an indispensable adjunct to the *Bessemer* process, and the application of which has become an integral part of steel manufacture, are still far from being thoroughly understood, and there is ample scope for chemical research, in co-operation with practical experiment, in the further study of the influence not only of manganese in the production, and upon the properties of steel, but also of elements such as titanium, tungsten, and boron, and of chromium, which exists, associated in considerable quantities with iron, in a very abundant Tasmanian ore, to which prominent attention has lately been directed. The achievements of the mechanical engineer have so facilitated the handling and perfected the means of production and the mechanical treatment of malleable iron and of steel, that the full advantage may now be reaped of any improvement of a chemical nature which may be effected in the production of those materials; and it must be a source of pride to the chemist to observe with what success the teachings of his science are being applied by practical men of the present day in the construction of furnaces capable of withstanding the high temperatures required for the production and working of iron and steel in large masses, and in combining the perfect consumption and consequent great economy of fuel with the attainment of those high temperatures and with a thorough control over the character of the gaseous agents to which the fluid metal is exposed in the furnace. I need not quote the names of those men who have already rendered themselves prominent by their services in this particular direction, but may refer in special illustration of the results achieved by purely practical men to the success in applying very simple furnace-arrangements to the attainment of the above results which has recently attended the labours of Mr. William Price, a principal foreman in the Royal Gun Factories at Woolwich.

A few experiments made in the early days of the application of armouring to ships and forts appeared to demonstrate on the one hand that steel was quite incapable of competing with malleable iron of even very moderate quality as a material for armour-plates, and, on the other hand, that the penetrative power of projectiles made of chilled iron upon the Palliser system could not be surpassed or even attained with any degree of certainty, by projectiles of steel produced at comparatively very great cost. But some recent results obtained on the Continent, and especially in the course of the important experiments instituted by the Italian Government at Spezzia, have afforded decisive indications that steel, the application of which to the construction of ordnance has since that time been very greatly extended, may now be looked to hopefully as capable of affording greater protection against the enormous projectiles of the present day than can be secured by proportionately large additions to the stupendous iron-armouring of the most modern ironclads, and also as applicable at a cost very moderate, when compared with that of ten years ago, to the production of projectiles of large dimensions superior in point of penetrative power and of uniformity in this respect to those of chilled iron, the difficulties in the production of which are very greatly increased by the formidable increase which has lately been made in their size. Promising results have also qu ie recently been obtained at Shoeburyness with a new system of applying steel in conjunction with malleable iron, by which a perfect union of the two materials at one of their surfaces is effected by the aid of heat.

The superiority of soft and very homogeneous steel over wrought iron of the best quality in regard to lightness, combined with strength and toughness, are leading to its very advantageous employment in the construction of a particular class of vessels for the navy; and the perfect confidence which can be placed in the uniformity in structure and strength of steel of such character as is produced by the Whitworth system of manufacture has greatly facilitated the production of air-chambers of small weight, but capable of being quite safely charged with sufficient air, under a pressure of 1,000 pounds on the square inch, to carry the Whitehead torpedo through water to a distance of 1,000 yards in little more than a minute and a half.

Thus, the results of the recent development of steel-industry, to which the labours of the chemist have not unimportantly contributed, give promise of erecting a great influence upon the resources of nations for defence and attack. Although the necessity for the continual expansion of such resources cannot but be deeply deplored, there can be no doubt that the problems which it presents, and the special requirements to which it gives rise, must operate, and perhaps as importantly as the demands created by peaceful industries and commercial enterprise, in encouraging the metallurgist, the chemist, and the engineer to continue their combined work in following up the successes, to the achievement of which the results of scientific research have greatly, though indirectly, contributed.

If it were necessary to add to the illustrations which Mr. Perkin gave in his address last year of the practical fruits of research in *organic* chemistry, I might be tempted to dilate upon the importantr esults which have, especially during the last ten years, grown out of the discovery and study of the products of the action of nitric acid upon cellulose and glycerine. During the six years which have elapsed since I had the honour of bringing before the members of the British Association the chief points of scientific interest and practical importance presented by the history of those remarkable bodies, their application to technical and war purposes has been greatly developed. Nitro-glycerine and gun-cotton may now be justly classed among the most interesting examples of the practical importance frequently attained by the results of chemical research, while the history of the successive steps by which their safe manipulation and efficient application have been developed affords more than one striking illustration of the achievements effected by combined physical and chemical research in the solution of problems of high scientific interest and practical importance, and in the vanquishment of difficulties so formidable as for a time to appear fatal to the attainment of permanently practical success.

It is to a careful study of the influence which the *physical* character of gundowder (its density, hardness, &c.) and its *mechanical* condition (*i.e.*, form and size of the masses and condition of their surfaces), exert upon the rapidity of its explosion under confinement, that we chiefly owe the very important advance which has been made of late years in controlling its explosive force; in its applications as a propelling agent, and the consequent simple and effectual means whereby the violence of action of the enormous charges now used in siege and ship-guns is effectually reduced to within their limits of endurance without diminution of the total explosive force developed. But, concurrently with these important practical results, the application of combined chemical and physical research to a very extended and comprehensive investigation of the action of fired gunpowder has furnished results which possess considerable interest from a purely scientific point of view, as in many respects modifying, in others supplementing, the conclusions based upon earlier experiments and theoretical considerations with respect to the nature and proportions of the products formed, the heat developed by the explosion, the tension of the products of combustion with the conditions which regulate it both when the explosion is brought about in a close vessel and when it occurs in the bore of a gun. The results of these physico-chemical researches have, moreover, already acquired practical importance in regard to the light they have thrown upon the influence exerted by variable conditions of a mechanical nature upon the action of and pressure developed by fired gunpowder in the bore of a gun, and in demonstrating that modifications in the *composition* of gunpowder, not unimportant from an economical point of view in dealing with the very large charges now employed, may importantly contribute to render the storing of the maximum of work in the projectile, when propelled from a gun, compatible with a subjection of the gun to comparatively very moderate and uniform strains.

Other interesting illustrations of the intimate manner in which physical and chemical research are linked together, and of the important extent to which some of our most illustrious workers in chemistry have contributed to demolish the semblance of a barrier which existed in past times between the two branches of science, are furnished and suggested by the recently published List of Grants of Money which the Government has made to scientific men, on the recommendation of the Royal Society, from the fund which, for the first time last year, was added to the very modest sum previously accorded from national resources in support of research. The perusal of that list, representing as it does a most carefully considered selection by the highest representatives of science in the country, from a very large number of applications, affords important evidence, on the one hand, of the active pursuit of science in Great Britain, and, on the other, of the very wide range of subjects of interest and importance, the full investigation of which demands the provision of adequate resources. That the necessity for such resources needs but to be thoroughly made known to ensure their provision, even from other than national sources, has been demonstrated by the success which, in a comparatively brief space of time, has attended the efforts of the Chemical Society to establish, upon the foundation patriotically laid by one of its original members, Dr. Longstaff, a special fund, to be administered by the Society for the advancement of chemical science. An inspection of the list of contributors to this special fund in aid of chemical research which, in about two years, has reached the sum of four thousand pounds, and from the proceeds of which the first applications for grants have recently been met, is suggestive of two observations. One is, that the proportion and amount of contributions hitherto received are comparatively small from the source whence the greatest support of such a fund may naturally be looked for, namely, from those who most directly benefit by the results of chemical research. It is to be hoped that there are many prominent representatives of the chemical and metallurgic industries in this country who still intend to give practical effect to their natural desire to aid in the advancement of chemical science, and to the appreciation which they can hardly fail to entertain of the usefulness of this fund. On the other hand, it is a matter well meriting special notice that a very prominent section of the contributors to the fund is composed of some of the most ancient corporate bodies of the city of London. Most welcome evidence is thereby afforded of the readiness with which the City Companies are prepared to respond to appeals for the substantial support of measures well calculated to promote progress in science. This evidence, and the combined action which they are even now contemplating for promoting the application of scientific research to the advancement of industry and commerce, by establishing an institution for technical education upon a scale worthy to serve as a monument of the true usefulness of wealthy confederations, must be cordially hailed as very substantial proofs that these representatives of our national wealth and commercial supremacy are entering upon a new sphere of activity which will more than restore their ancient prestige, by according them a new rank, more elevated than any which their civic importance could, in the past or future, confer upon them—a rank high among the chief promoters of our national enlightenment.

SECTION C.

GEOLOGY.

OPENING ADDRESS BY THE PRESIDENT, W. PENGELLY, F.R.S., F.G.S.

WHEN, as long ago as 1841, the British Association made its only previous visit to Plymouth, some of us, now amongst its oldest members, thought ourselves too young to take any part in its proceedings. If the effects of that meeting are still traceable in this district, it will be admitted, of course, that the seed then sown was of excellent quality and that it fell on good soil. Be this as it may, the hope may be cherished that thirty-six years will not again be allowed to elapse between two consecutive visits to the capital of the two south-western counties.

One effect of this wide hiatus is the loss of almost all the human links whose presence on this occasion would have pleasantly connected the present with the past. A glance at the lists of Trustees and the General, Sectional, and Local officers in 1841 will show that the presence of scarcely one of them can be hoped for on this occasion; and there is but little probability that any of those who prepared Reports or Papers for the last Plymouth Meeting will have done so for that which is now assembled.

Nor are these the only changes. In 1841 Section C embraced, as at the beginning, the geographers as well as the geologists; but ten years later the geographers were detached, whether to find room for themselves, or to make room for the students of an older geography, it is not necessary to inquire.

Some years afterwards came an innovation which, until entering on the preparation of this address, I always regarded as a decided improvement. The first Presidential Address to this Section was delivered at Leeds in 1858 by the late Mr. Hopkins, so well known to geologists for his able application of his great mathematical powers to sundry important problems in their science; and from that time to the present, with the exception of the Meetings of 1860 and 1870 only, the President of this Section has delivered an address.

None of the local geological papers read in 1841 appear to have attracted so much attention as those on Lithodomous Perforations, Raised Beaches, Submerged Forests, and Caverns (see *Athenæum* for 7th to 28th of August, 1841); and, as an effort to connect the present with the past, I have decided on taking up one of these threads, and devoting the remarks I have now to offer to the History of Cavern-Exploration in Devonshire. I am not unmindful that there were giants in those days; and no one can deplore more than I do our loss of Buckland and De la Beche, amongst many others; nor can I forget the enormous strides opinion has made since 1841, when, in this Section, Dr. Buckland "contended that human remains had never been found under such circumstances as to prove their contemporaneous existence with the hyænas and bears of the caverns," and added that "in Kent's Hole the Celtic knives . . . . were found in holes *dug by art*, and which had disturbed the floor of the cave and the bones below it" (*Athenæum*, 14th Aug. 1841, p. 626). This scepticism, however, did the good service of inducing cavern explorers to conduct their researches with an accuracy which should place their results, whatever they might prove to be, amongst the undoubted additions to human knowledge.

The principal caverns in South Devon occur in the limestone districts of Plymouth, Yealmpton, Brixham, Torquay, Buckfastleigh, and Chudleigh; but as those in the last two localities have yielded nothing of importance to the anthropologist or the palæontologist, they will not be further noticed on this occasion. In dealing with the others it seems most simple to follow mainly the order of chronology; that is to say, to commence with the cavern which first caught scientific attention, and, having finished all that the time at my disposal will allow me to say about it, but not before, to proceed to the next, in the order thus defined; and so on through the series.

Oreston Caverns.—When Mr. Whidbey engaged to superintend the construction of the Plymouth Breakwater, Sir Joseph Banks, President of the Royal Society, requested him to examine narrowly any caverns he might meet with in the limestone-rock to be quarried at Oreston, near the mouth of the river Plym, not more than two miles from the room in which we are assembled, and have the bones or any other fossil remains that were met with carefully preserved (see *Phil. Trans.*, 1817, pp. 176—182). This request was cheerfully complied with, and Mr. Whidbey had the pleasure of discovering bone-caves in November, 1816, November, 1820, August and November, 1822, and of sending the remains found in them to the Royal Society.

It is, perhaps, worthy of remark that, though cavern-researches received a great impulse from the discoveries in Kirkdale, Yorkshire, and especially from Dr. Buckland's well-known and graphic descriptions of them, such researches had originated many years before. The request by Sir Joseph Banks was made at least as early as 1812 (see *Trans.* Devon. Assoc., v., pp. 252, 253), and a paper on the Oreston discoveries was read to the Royal Society in February 1817, whereas the Kirkdale Cavern was not discovered until 1821. British cave-hunting appears to have been a science of Devonshire birth.

The Oreston Caverns soon attracted a considerable number of able observers; they were visited in 1822 by Dr. Buckland and Mr. Warburton; and in a comparatively short time became the theme of a somewhat voluminous literature. Nothing of importance, however, seems to have been met with from 1822 until 1858, when another cavern, containing a large number of bones, was broken into. Unfortunately, there was no one at hand to superintend the exhumation of the specimens; the work was left entirely to the common workmen, and was badly done; many of the remains were dispersed beyond recovery; the matrix in which they were buried was never adequately examined;

and we are utterly ignorant, and must for ever remain so, as to whether they did or did not contain indications of human existence. I visited the spot from time to time, and bought up everything to be met with; but other scientific work in another part of the county occupied me too closely to allow more than an occasional visit. The greater part of the specimens I secured were lodged in the British Museum, where they seem to have been forgotten, whilst a few remain in my private collection.

Some difference of opinion has existed respecting the character of the successive caverns, and much mystery has been imported into the question of the introduction of their contents. Mr. Whidbey, it is said, "saw no possibility of the cavern of 1816 having had any external communication through the rock in which it was inclosed" (*Phil. Trans.*, 1817, pp. 176—182); but Dr. Buckland was of opinion that they were all at first fissures open at the top, and "that the openings had been long filled up with rubbish, mud, stalactite, or fragments of rock cemented, as sometimes happens, into a breccia as solid as the original rock, and overgrown with grass" (*Phil. Trans.*, 1822, pp. 171—240).

The conclusion I arrived at, after studying so much of the roof of the cavern of 1858 as remained intact, was that Dr. Buckland's opinion was fully borne out by the facts; that, in short, the Oreston Caverns were *Fissure Caverns*, not *Tunnel Caverns*.

The cavern of 1858 was an almost vertical fissure, extending a length of about 90 feet from N.N.E. to S.S.W. It commenced at about 8 feet below the surface of the plateau, continued thence to the base of the cliff, but how much further was not known, and its ascertained height was about 52 feet. It was 2 feet wide at top, whence it gradually widened to 10 feet at bottom. The roof, judging from that part which had not been destroyed, was a mass of limestone-breccia, made up of large angular fragments, cemented with carbonate of lime, and requiring to be blasted as much as ordinary limestone. The cavern was completely filled with deposits of various kinds.

The uppermost 8 feet consisted of loose angular pieces of limestone, none of which exceeded 10 lb. in weight, mixed with a comparatively small amount of such sand as is common in dolomitised limestone districts, but without a trace of stalagmite or fossil of any kind. The 32 feet next below were occupied with similar materials, with the addition of a considerable quantity of tough, dark, unctuous clay. Between this mass and the outer wall of the cavern was a nearly vertical plate of stalagmite, usually about 2 feet thick, and containing, at by no means wide intervals, firmly cemented masses of breccia identical in composition with the adjacent bed just mentioned. The bones the cavern yielded were all found within these 32 feet; and were met with equally in the loose and the coherent breccia, as well as in the stalagmite. A somewhat considerable number of ellipsoidal balls of clay, from 1·5 to 2·5 inches in greatest diameter, occurred in the clay of this bone-bed, but not elsewhere. Still lower was a mass of dark, tough, unctuous clay, containing a very few, small, angular stones, but otherwise perfectly homogeneous, and known to be 12 feet deep, but how much more was undetermined.

The osseous remains found at Oreston prior to 1858 have been described by Sir E. Home, Mr. Clift, Dr. Buckland, Prof. Owen, Mr. Busk, and others. The animals represented were *Ursus priscus*, *U. spelæus*, weasel (?), wolf, fox, cave hyæna, cave lion, *Rhinoceros leptorhinus*, *Equus fossilis*, *E. plicidens*, *Asinus fossilis*, *Bison minor*, *Bos longifrons*, and, according to the late Mr. Bellamy, mammoth and hippopotamus (see Nat. Hist. of S. Devon, 1839, p. 82). With regard to hippotamus, I can only say that I have never met with satisfactory evidence of its occurrence in Devonshire; but the mammoth was certainly found at Oreston in 1858; and, unless I am greatly in error, remains of *Rhinoceros tichorhinus* were also met with there, and lodged by me in the British Museum. It may be added that the skull and other relics of a hog were exhumed on that occasion, and now belong to my collection. There was nothing to suggest that the cavern had been the home of the hyæna; and whilst I fully accept Dr. Buckland's opinion that animals had fallen into the open fissures and there perished, and that the remains had subsequently been washed thence into the lower vaultings ("Reliq. Dil.," 2nd. ed., 1834, p. 78), I venture to add that some of the animals may have retired thither to die; a few may have been dragged or pursued there by beasts of prey; whilst rains, such as are not quite unknown in Devonshire in the present day, probably washed in some of the bones of such as died near at hand on the adjacent plateau. Nothing appears to have been met with suggestive of human visits.

Kent's Hole.—About a mile due east from Torquay harbour and half a mile north from Torbay there is a small wooded limestone hill, the eastern side of which is, for the uppermost 30 feet, a vertical cliff, having at its base, and 54 feet apart, two apertures leading into one and the same vast cavity in the interior of the hill, and known as Kent's Hole or Cavern. These openings are about 200 feet above mean sea-level, and from them the hill slopes rapidly to the valley at its foot, at a level of from 60 to 70 feet below.

There seems to be neither record nor tradition of the discovery of the cavern. Richardson, in the 8th edition of "A Tour through the Island of Great Britain," published in 1778, speaks of it as "perhaps the greatest natural curiosity" in the county; its name occurs on a map dated 1769; it is mentioned in a lease 1659; visitors cut their names and dates on the stalagmite from 1571 down to the present century; judging from numerous objects found on the floor, it was visited by man through mediæval back to pre-Roman times; and, unless the facts exhumed by explorers have been misinterpreted, it was a human home during the era of the mammoth and his contemporaries.

In 1824 Mr. Northmore, of Cleve, near Exeter, was led to make a few diggings in the cavern, and was the first to find fossil bones there. He was soon followed by Mr. (now Sir) W. C. Trevelyan, who not only found bones, but had a plate of them engraved. In 1825, the Rev. J. MacEnery, an Irish Roman Catholic priest residing in the family of Mr. Cary, of Tor Abbey, Torquay, first visited the cavern, when he, too, found teeth and bones, of which he published a plate. Soon after, he made another visit, accompanied by Dr. Buckland, when he had the good fortune to discover a flint implement; the first instance, he tells us, of such a relic being noticed in any cavern (see *Trans.* Devon Assoc., iii., p. 441). Before the close of 1825, he commenced a series of more or less systematic diggings, and continued them until, and perhaps after, the summer of 1829 (*ibid.*, p. 295). Preparations appear to have been made to publish the results of his labours; a prospectus was issued, numerous plates were lithographed, it was generally believed that the MS. was almost ready, and the only thing needed was a list of subscribers sufficient to justify publication, when, alas! on February 18, 1841, before the printer had received any "copy," before even the world of science had accepted his anthropological discoveries, before the value of his labours was known to more than a very few, Mr. MacEnery died at Torquay.

After his decease his MS. could not be discovered, and its loss was duly deplored. Nevertheless, it was found after several years, and, having undergone varieties of fortune, became the property of Mr. Vivian, of Torquay, who, having published portions of it in 1859, presented it in 1867 to the Torquay Natural History Society, whose property it still remains. In 1869 I had the pleasure of printing the whole, in the *Transactions* of the Devonshire Association.

Whilst Mr. MacEnery was conducting his researches, a few independent diggings, on a less extensive scale, were taken by other gentlemen. The principal of these was Mr. Godwin-Austen, the well-known geologist, whose papers fully bore out all that MacEnery had stated. (See *Trans.* Geol. Soc. Lond., 2nd series, vi., p. 446). In 1846 a sub-committee of the Torquay Natural History Society undertook the careful exploration of very small parts of the cavern, and their Report was entirely confirmatory of the statements of their predecessors—that undoubted flint implements did occur, mixed with the remains of extinct mammals, in the cave-earth, beneath a thick floor of stalagmite. The sceptical position of the authorities in geological science remained unaffected, however, until 1858, when the discovery and systematic exploration of a comparatively small virgin cavern on Windmill Hill, at Brixham, led to a sudden and complete revolution; for it was seen that whatever were the facts elsewhere, there had undoubtedly been found at Brixham flint implements commingled with remains of the mammoth and his companions, and in such a way as to render it impossible to doubt that man occupied Devonshire before the extinction of the cave mammals.

Under the feeling that the statements made by MacEnery and his followers respecting Kent's Hole were perhaps, after all, to be accepted as verities, the British Association, in 1864, appointed a committee to make a complete, systematic, and accurate exploration of the cavern, in which it was known that very extensive portions remained entirely intact. This committee commenced its labours on March 28, 1865; it has been reappointed, year after year, with sufficient grants of money, up to the present time; the work has gone on continuously throughout the entire thirteen years; and the result has been, not only a complete confirmation of Mr. MacEnery's statements, but the discovery of far older deposits than he suspected—deposits implying great changes of, at least, local geographical conditions; changes in the fauna of the district; and yielding evidence of men more ancient and far ruder than even those who made the oldest flint tools found in Kent's Hole prior to the appointment of the committee.

The cavern consists of a series of chambers and passages, which resolve themselves into two main *divisions*, extending from nearly north to south in parallel lines, but passing into each other near their extremities, and throwing off branches, occasionally of considerable size.

The successive deposits, in descending order, were:—

1st, or uppermost. Fragments and blocks of limestone from an ounce to upwards of 100 tons weight each, which had fallen from the roof from time to time, and were, in some instances, cemented with carbonate of lime.

2nd. Beneath and between these blocks lay a dark-coloured mud or mould, consisting largely of decayed leaves and other vegetable matter. It was from 3 to 12 inches thick, and known as the *black mould*. This occupied the entire eastern division, with the exception of a small chamber in its south-western end only, but was not found in the other, the remoter, parts of the cavern.

3rd. Under this was a stalagmitic floor, commonly of granular texture, and frequently laminated, from less than an inch to fully five feet in thickness, and termed the *granular stalagmite*.

4th. An almost black layer, about four inches thick, composed mainly of small fragments of charred wood, and distinguished as the *black band*, occupied an area of about 100 square feet, immediately under the granular stalagmite, and, at the nearest point, not more than thirty-two feet from one of the entrances to the cavern. Nothing of the kind has occurred elsewhere.

5th. Immediately under the granular stalagmite and the black band lay a light red clay, containing usually about 50 per cent. of small angular fragments of limestone, and somewhat numerous blocks of the same rock as large as those lying on the black mould. In this deposit, known as the *cave-earth*, many of the stones and bones were, at all depths, invested with thin stalagmitic films. The cave-earth was of unknown depth near the entrances, where its base had never been reached; but in the remoter parts of the cavern it did not usually exceed a foot, and in a few localitites it "thinned out" entirely.

6th. Beneath the cave-earth there was usually found a floor of stalagmite having a crystalline texture, and termed on that account the *crystalline stalagmite*. It was commonly thicker than the granular floor, and in one instance but little short of 12 feet.

7th. Below the whole occurred, so far as is at present known, the oldest of the cavern deposits. It was composed of sub-angular and rounded pieces of dark-red grit, embedded in a sandy paste of the same colour. Small angular fragments of limestone, and investing films of stalagmite, both prevalent in the cave-earth, were extremely rare. Large blocks of limestone were occasionally met with; and the deposit, to which the name of *breccia* were given, was of a depth exceeding that to which the exploration has yet been carried.

Except in a very few small branches, the bottom of the cavern has nowhere been reached. In the cases in which there was no cave-earth, the granular stalagmite rested immediately on the crystalline; and where the crystalline stalagmite was not present the cave-earth and breccia were in direct contact. Large isolated masses of the crystalline stalagmite, as well as concreted lumps of the breccia, were occasionally met with in the cave-earth, thus showing that the older deposits had, in portions of the cavern, been partially broken up, dislodged, and re-deposited. No instance was met with of the incorporation in a lower bed of fragments derived from an upper one. In short, wherever all the deposits were found in one and the same vertical section, the order of superposition was clear and invariable; and elsewhere the succession, though defective, was never transgressed.

Excepting the overlying blocks of limestone, of course, all the deposits contained remains of animals, which, however, were not abundant in the stalagmites.

The black mould, the uppermost bed, yielded teeth and bones of man, dog, fox, badger, brown bear, *Bos longifrons*, roe-deer, sheep, goat, pig, hare, rabbit, and seal—species still existing, and almost all of them in Devonshire. This has been called the *Ovine* bed, the remains of sheep being restricted to it. In it were also found numerous flint flakes and "strike-lights," stone spindle-whorls, fragments of curvilineal pieces of slate, amber beads, bone tools, including awls, chisels, and combs; bronze articles, such as rings, a fibula, a spoon, a spear-head, a socketed celt, and a pin; pieces of smelted copper, and a great number and variety of potsherds, including fragments of Samian ware.

The granular stalagmite, black band, and cave-earth, taken together as belonging to one and the same biological period, may be termed the *Hyænine* beds, the cave hyæna being their most prevalent species, and found in them alone. So far as they have been identified, the remains belong to the cave hyæna, *Equus caballus*, *Rhinoceros tichorhinus*, gigantic Irish deer, *Bos primigenius*, *Bison priscus*, red deer, mammoth, badger, cave bear, grizzly bear, brown bear, cave lion, wolf, fox, reindeer, beaver, glutton, *Machairodus latidens*, and man—the last being a part of a jaw with teeth, in the granular stalagmite. In the same beds were found unpolished *ovate* and *lanceolate* implements made from *flakes*, not *nodules*, of flint and chert; flint flakes, chips, and "cores;" "whet-stones," a "hammer-stone," "dead" shells of *Pecten*, bits of charcoal, and bone tools, including a needle or bodkin having a well-formed eye, a pin, an awl, three harpoons, and a perforated tooth of badger. The artificial objects, of both bone and stone, were found at all depths in each of the hyænine beds, but were much more numerous below the stalagmite than in it.

The relics found in the crystalline stalagmite and the breccia, in some places extremely abundant, were almost exclusively those of bear, the only exceptions being a very few remains of cave lion and fox. Hence these have been termed the *Ursine* beds. It will be remembered that teeth and bones of bear were also met with in both the hyænine and the ovine beds; and it should be understood that this biological classification is intended to apply to Kent's Cavern only. The ursine deposits, or rather the breccia, the lowest of them, also yielded evidences of human existence; but they were exclusively tools made from *nodules*, not *flakes*, of flint and chert.

Ansty's-Cove Cavern.—About three furlongs from Kent's Hole towards N.N.E., near the top of the lofty cliff forming the northern boundary of the beautiful Ansty's Cove, Torquay, there is a cavern where, simultaneously with those in Kent's Cavern, Mr. MacEnery conducted some researches, of which he has left a brief account (see *Trans.* Devon. Assoc., vi., pp. 61–69). I have visited it several times, but it seems to be frequently kept under lock and key, as a tool and powder-house, by the workmen in a neighbouring quarry. It is a simple gallery, and, according to Mr. MacEnery, 63 feet long, from 3 to 9 feet high, and from 3 to 6 feet broad. Beneath some angular stones he found a stalagmitic floor 14 inches thick, and in the deposit below remains of deer, horse, bear, fox, hyæna (?), coprolites, a few marine and land shells, one white flint tool with fragments of others, a Roman coin, and potsherds.

In a letter to Sir W. C. Trevelyan, dated 16th December, 1825, Dr. Buckland states that Mr. MacEnery had found in this cave "bones of all sorts of beasts, and also flint knives and Roman coins; in short, an open-mouthed cave, which has been inhabited by animals of all kinds, quadruped and biped, in all successive generations, and who have all deposited their exuviæ one upon another" (*ibid.*, p. 69).

Yealm-Bridge Cavern.—About the year 1832 the workmen broke into a bone-cavern in Yealm-Bridge Quarry, about one mile from the village of Yealmpton, and eight miles E.S.E. from Plymouth; and through their operations it was so nearly destroyed that but a small arm of it remained in 1835, when it was visited by Mr. J. C. Bellamy, who at once wrote an account of it, from which it appears that, so far as he could learn, the cavern was about 30 feet below the original limestone surface, and was filled to from 1 foot to 6 feet of the roof (see "Nat. Hist. S. Devon.," 1839, pp. 86–105). In the same year, but subsequently, it was examined by Capt. (afterwards Col.) Mudge, who states that there were orginally three openings into the cave, each about 12 feet above the river Yealm; that the deposits were, in descending order:—

1.	Loam with bones and stones	3·5 feet
2.	Stiff whitish clay	2·5 ,,
3.	Sand	6·0 ,,
4.	Red clay	3·5 ,,
5.	Argillaceous sand	6 to 18·0 ,,

and that, where they did not reach the roof, the deposits were covered with stalagmite.

On the authority of Mr. Clift and Prof. Owen, Capt. Mudge mentions relics of elephant, rhinoceros, horse, ox, sheep, hyæna, dog, wolf, fox, bear, hare, and water-vole. The bones, and especially the teeth, of the hyæna exceeded in number those of all the other animals, though remains of horse and ox were very abundant. Mr. Bellamy, whilst also mentioning all the foregoing forms, with the exception of dog only, adds deer, pig, glutton, weasel, and mouse. He also speaks of the abundance of bones and teeth of hyæna, but seems to regard the fox as being almost as fully represented; and next in order he places horse, deer, sheep, and rabbit or hare; whilst the relics of elephant, wolf, bear, pig, and glutton are spoken of as very rare. The bones, he says, were found in the uppermost bed only. They were frequently mere fragments and splinters, some being undoubtedly gnawed, and all had become very adherent through loss of their animal matter. Those of cylindrical form were without their extremities; there was no approach to anatomical juxtaposition; and the remains belonged to individuals of all ages. Reliquiæ of carnivorous animals greatly exceeded those of the herbivora, and teeth were very abundant. Coprolites occurred at some depth below the stalagmite, in the upper bed, which also contained granitic and trappean pebbles, and lumps of breccia made up of fragments of rock, bones, pebbles, and stalagmite. The bones found prior to 1835 had been removed as rubbish, and some good specimens were recovered from materials employed in making a pathway. Nothing indicating the presence of man appears to have been found.

The Ash-Hole.—On the southern shore of Torbay, midway between the town of Brixham and Berry Head, and about half a mile from each, there is a cavern known as the *Ash-Hole.* It was partially explored, probably about, or soon after, the time Mr. MacEnery was engaged in Kent's Hole, by the late Rev. H. F. Lyte, who, unfortunately, does not appear to have left any account of the results. The earliest mention of this cavern I have been able to find is a very brief one in Bellamy's "Natural History of South Devon," published in 1839 (p. 14). During the Plymouth Meeting in 1841, Mr. George Bartlett, a native of Brixham, who assisted Mr. Lyte, described to this Section the objects of interest the Ash-Hole had yielded (see *Report Brit. Assoc.* 1851, *Trans. Sections*, p. 61). So far as was then known the cave was thirty yards long and six yards broad. Below a recent accumulation, four feet deep, of loam and earth, with land and marine shells, bones of the domestic fowl and of man, pottery, and various implements, lay a true cave-earth, abounding in the remains of elephant. Prof. Owen, who identified, from this lower bed, relics of badger, polecat, stoat, water-vole, rabbit, and reindeer, remarks, that for the first good evidence of the reindeer in this island he had been indebted to Mr. Bartlett, who stated that the remains were found in this cavern (see "Brit. Foss. Mam." 1846, pp. 109-110, 113-114, 116, 204, 212, 479-480). I have made numerous visits to the spot, which, when Mr. Lyte began his diggings, must have been a shaft-like fissure, accessible from the top only. A lateral opening, however, has been quarried into it; there is a narrow tunnel extending westward, in which the deposit is covered with a thick sheet of stalagmite, and where one is tempted to believe that a few weeks' labour might be well invested.

Brixham Cavern.—Early in 1858 an unsuspected cavern was broken into by quarrymen at the north-western angle of Windmill Hill at Brixham, at a point seventy-five feet above the surface of the street, almost vertically below, and 100 feet above mean tide. On being found to contain bones, a lease in it was secured for the Geological Society of London, who appointed a committee of their members to undertake its exploration; funds were voted by the Royal Society, and supplemented by private subscriptions; the conduct of the investigation was intrusted to Mr. Prestwich and myself; and the work, under my superintendence, as the only resident member of the committee, was begun in July, 1858, and completed at midsummer, 1859.

The cavern, comprised within a space of 135 feet from north to south, and 100 from east to west, consisted of a series of tunnel galleries from six to eight feet in greatest width, and ten to fourteen feet in height, with two small chambers and five external entrances.

The deposits, in descending order, were:—

1st, or uppermost. A floor of stalagmite, from a few inches to a foot thick, and continuous over very considerable areas, but not throughout the entire cavern.

2nd. A mass of small angular fragments of limestone, cemented into a firm concrete with carbonate of lime, commenced at the principal entrance, which it completely filled, and whence it extended thirty-four feet only. It was termed the *first bed.*

3rd. A layer of blackish matter, about twelve long, and nowhere more than a foot thick, occurred immediately beneath the first bed, and was designated the *second bed.*

4th. A red, tenacious, clayey loam, containing a large number of angular and subangular fragments of limestone, varying from very small bits to blocks a ton in weight, made up the *third bed.* Pebbles of trap, quartz, and limestone were somewhat prevalent, whilst nodules of brown hematite of iron and blocks of stalagmite were occasionally met with in it. The usual depth of the bed was from two to four feet, but this was exceeded by four or five feet in two localities.

5th. The third bed lay immediately on an accumulation of pebbles of quartz, greenstone, grit, and limestone, mixed with small fragments of shale. The depth of this, known as the *fourth* or *gravel bed*, was undetermined; for, excepting a few feet only, the limestone bottom was nowhere reached. There is abundant evidence that this bed, as well as a stalagmitic floor which had covered it, had been partially broken up and dislodged before the introduction of the third bed.

Organic remains were found in the stalagmitic floor and in each of the beds beneath it, with the exception of the second only; but as ninety-five per cent of the whole series occurred in the third, this was not unfrequently termed the *bone bed.*

The mammals represented in the stalagmite were bear, reindeer, *Rhinoceros tichorhinus*, mammoth, and cave lion.

The first bed yielded bear and fox only.

In the third bed were found relics of mammoth, *Rhinoceros tichorhinus*, horse, *Bos primigenius*, *B. longifrons*, red deer, reindeer, roebuck, cave lion, cave hyæna, cave bear, grizzly bear, brown bear, fox, hare, rabbit, *Lagomys spelæus*, water-vole, shrew, polecat, and weasel.

The only remains met with in the fourth bed were those of bear, horse, ox, and mammoth.

The human industrial remains exhumed in the cavern were flint implements and a hammer-stone, and occurred in the third and fourth beds only. The pieces of flint met with were thirty-six in number. Of these fifteen are held to show evidence of having been artificially worked, in nine the workmanship is rude or doubtful, four have been mislaid, and the remainder are believed not to have been worked at all (see *Phil. Trans.*, vol. 163, 1873, pp. 561, 562). Of the undoubted tools, eleven were found in the third and four in the fourth bed. Two of those yielded by the third bed, found forty feet apart, in two distinct but adjacent galleries, and one a month before the other, proved to be parts of one and same *nodule*-tool; and I have little or no doubt that it had been washed out of the fourth bed and redeposited in the third.

The hammer-stone was a quartzite pebble, found in the upper portion of the fourth bed, and bore distinct marks of the use to which it was applied.

Speaking of the discovery of the tools just mentioned, Mr. Prestwich said in 1859:—"It was not until I had myself witnessed the conditions under which flint implements had been found at Brixham, that I became fully impressed with the validity of the doubts thrown upon the previously prevailing opinions with respect to such remains in caves" (*Phil. Trans.*, 1860, p. 280); and according to Sir C. Lyell, writing in 1863:—"A sudden change of opinion was brought about in England respecting the probable co-existence, at a former period, of man and many extinct mammalia, in consequence of the results obtained from the careful exploration of a cave at Brixham. . . . The new views very generally adopted by English geologists had no small influence on the subsequent progress of opinion in France" ("Antiquity of Man," pp. 96, 97).

Bench Cavern.—Early in 1861 information was brought me that an ossiferous cave had just been discovered at Brixham, and, on visiting the spot, I found that, of the limestone quarries worked from time to time in the northern slope of Furzeham Hill, one known as Bench Quarry, about half a mile due north of Windmill Hill Cavern, and almost overhanging Torbay, had been abandoned in 1839, and that work had been recently resumed in it. It appeared that in 1839 the workmen had laid bare the greater part of a vertical dyke, composed of red clayey loam, and angular pieces of limestone, forming a coherent wall-like mass, 27 feet high, 12 feet long, 2 feet in greatest thickness, and at its base 123 feet above sea-level. In the face of it lay several fine relics of the ordinary cave mammals, including an entire left lower jaw of *Hyæna spelæa* replete with teeth, but which had nevertheless failed to arrest the attention of the incurious workmen who exposed it, or of any one else.

Soon after the resumption of the work in 1861, the remnant of the outer wall of the fissure was removed, and caused the fall of an incoherent part of the dyke, which it had previously supported. Amongst the *débris* the workmen collected some hundreds of specimens of skulls, jaws, teeth, vertebra, portions of antlers, and bones, but no indications of man. Mr. Wolston, the proprietor, sent some of the choicest specimens to the British Museum, and submitted the remainder to Mr. Ayshford Sanford, F.G.S., from whom I learn that the principal portion of them are relics of the cave hyæna, from the unborn whelp to very aged animals. With them, however, were remains of bear, reindeer, ox, hare, *Arvicolo ratticeps*, *A. agrestis*, wolf, fox, and part of a single maxillary with teeth not distinguishable from those of *Canis isatis*. To this list I may add rhinoceros, of which Mr. Wolston showed me at least one bone.

From the foregoing undesirably, but unavoidably, brief descriptions, it will be seen that the Devonshire caverns, to which attention has been now directed, belong to two classes, —those of Oreston, the Ash-Hole, and Bench being *Fissure Caves*; whilst those of Yealm Bridge, Windmill Hill at Brixham, Kent's Hole, and Ansty's Cove are *Tunnel Caves.*

Windmill Hill and Kent's Hole Caverns have alone been satisfactorily explored; and besides them none have yielded evidence of the contemporaneity of man with the extinct cave mammals.

Oreston is distinguished as the only known British cavern which has yielded remains of *Rhinoceros leptorhinus* (*Quart. Journ.* Geol. Soc., xxxvi. p., 456).

Yealm Bridge Cavern, if we may accept Mr. Bellamy's identification in 1835, was the first in this country in which relics of glutton were found (*South Devon Monthly Museum*, vi., pp. 218-223; see also "Nat. Hist. S. Devon," 1839, p. 89). The same species was found in the caves of Somerset and Glamorgan in 1865 (*Pleist. Mam.*, *Pal. Soc.*, pp. xxi. xxii.), in Kent's Hole in 1869 (*Rep. Brit. Assoc.*, 1869, p. 207), and near Plas Heaton, in North Wales, in 1870 (*Quart. Journ.* Geol. Soc., xxvii., p. 407).

Kent's Hole is the only known British cave which has afforded remains of beaver, (*Rep. Brit. Assoc.*, 1869, p. 208), and up to the present year the only one in which the remains of *Machairodus latidens* had been met with. Indeed Mr. MacEnery's statement, that he found in 1826 five canines and one incisor of this species in the famous Torquay Cavern was held by many palæontologists to be so very remarkable as, at least, to approach the incredible, until the Committee now engaged in the exploration exhumed, in 1872, an incisor of the same species, and thereby confirmed the announcement made by their distinguished predecessor nearly half a century before (*Rep. Brit. Assoc.*, 1872, p. 46). In April last (1877) the Rev. J. M. Mello was able to inform the Geological Society of London that Derbyshire had shared with Devon the honour of having been a home of *Machairodus latidens*, he having found its canine tooth in Robin Hood Cave in that county, and that there, as in Kent's Hole it was commingled with remains of the cave hyæna and his contemporaries (*Abs. Proc.* Geol. Soc., No. 334, pp. 3, 4).

The Ash Hole, as we have already seen, afforded the first good evidence of a British reindeer.

In looking at the published reports on the two famous Torbay caverns it will be found that they have certain points of resemblance as well as some of dissimilarity:—

1st. The lowest known bed in each is composed of materials which, whilst they differ in the two cases, agree in being such as may have been furnished by the districts adjacent to the cavern-hills respectively, but not by the hills themselves, and must have been deposited prior to the existing local geographical conditions. In each, this bed contained flint implements and relics of bear, but in neither of them those of hyæna. In short, the *fourth bed* of Windmill Hill Cavern, Brixham, and the *breccia* of Kent's Hole, Torquay, are coeval, and belong to what I have called the *Ursine* period of the latter.

2nd. The beds just mentioned were in each cavern sealed with a sheet of stalagmite, which was partially broken up, and considerable portions of the subjacent beds were dislodged before the introduction of the beds next deposited.

3rd. The great bone bed, both at Brixham and Torquay, consisted of red clayey loam, with a large percentage of angular fragments of limestone; and contained *flake* implements of flint and chert, inosculating with remains of mammoth, the tichorhine rhinoceros, and hyæna. In fine, the *cave-earth* of Kent's Hole and the *third bed* of Brixham Cavern correspond in their materials, in their osseous contents, and in their flint tools. They both belong to what I have named the *Hyænine* period of the Torquay Cave.

But, as already stated, there are points in which the two caverns differ:—

1st. Whilst Kent's Hole was the home of man, as well as of the contemporary hyæna during the absences of the human occupant, there is no reason to suppose that either man or any of the lower animals ever did more than make occasional visits to Brixham Cave. The latter contained no flint chips, no bone tools, no utilised *Pecten*-shells, no bits of charcoal, and no coprolites of hyæna, all of which occurred in the cave-earth of Kent's Hole.

2nd. In the Torquay Cave relics of hyæna were much more abundant in the cave-earth than those of any other species. Taking the teeth alone, of which vast numbers were found, those of the hyæna amounted to about 30 per cent. of the entire series, notwithstanding the fact that, compared with most of the cave-mammals, his jaws, when furnished completely, possess but few teeth. At Brixham, on the other hand, his relics of all kinds amounted to no more than 8·5 per cent. of all the osseous remains, whilst those of the bear rose to 53 per cent.

3rd. The entrances of Brixham Cavern were completely filled up and its history suspended not later than the end of the Palæolithic era. Nothing occurred within it from the days when Devonshire was occupied by the cave and grizzly bears, reindeer, rhinoceros, cave lion, mammoth, and man, whose best tools were unpolished flints, until the quarrymen broke into it early in A.D. 1858. Kent's Cavern, on the contrary seems to have never been closed, never unvisited by man, from the earliest Palæolithic times to our own, with the possible exception of the Neolithic era, of which it cannot be said to have yielded any certain evidence.

Though my "History of Cavern Exploration in Devonshire" is now completed, so far as the time at my disposal will allow, and so far as the materials are at present ripe for the historian, I venture to ask your further indulgence for a few brief moments whilst passing from the region of fact to that of inference.

That the Kent's Hole men of the Hyænine period—to say nothing at present of their predecessors of the Breccia—belonged to the Pleistocene times of the biologist, is seen in the fact that they were contemporary with mammals peculiar to and characteristic of those times. This contemporaneity proves them to have belonged to the *Palæolithic* era of Britain and Western Europe generally, as defined by the archæologist; and this is fully confirmed by their unpolished tools of flint and chert. That they were prior to the deposition of even the oldest part of the peat bogs of Denmark, with their successive layers of beech, pedunculated oak, sessile oak, and Scotch fir, we learn from the facts that even the lowest zone of the bogs has yielded no bones of mammals but those of recent species, and no tools but those of *Neolithic* type; whilst even the granular stalagmite, the uppermost of the Hyænine beds in Kent's Hole, has afforded relics of mammoth, *Rhinoceros tichorhinus*, cave bear, and cave hyæna.

That the men of the Cave Breccia, or Ursine period, to whom we now turn, were of still higher antiquity, is obvious from the geological position of their industrial remains. That the two races of Troglodytes were separated by a wide interval of time we learn from the sheet of crystalline stalagmite, sometimes 12 feet thick, laid down after the deposition of the breccia had ceased, and before the introduction of the cave-earth had begun, as well as from the entire change in the materials composing the two deposits. But, perhaps, the fact which most emphatically indicates the chronological value of this interval is the difference in the faunas. In the cave-earth, as already stated, the remains of the hyæna greatly exceed in number those of any other mammal; and it may be added that he is also disclosed by almost every relic of his contemporaries—their jaws have, through his agency, lost their condyles and lower borders; their bones are fractured after a fashion known by experiment to be his; and the splinters into which they are broken are deeply scored with his teeth-marks. His presence is also attested by the abundance of his droppings in every branch of the cavern. In short, Kent's Hole was one of his *homes;* he dragged thither, piecemeal, such animals as he found dead near it; and the well-known habits of his representatives of our day have led us to expect all this from him. When, however, we turn to the breccia, a very different spectacle awaits us. We meet with no trace whatever of his presence, not a single relic of his skeleton, not a bone on which he has operated, not a coprolite to mark as much as a visit. Can it be doubted that had he then occupied our country he would have taken up his abode in our cavern? Need we hesitate to regard this entire absence of all traces of so decided a cave-dweller as a proof that he had not yet made his advent in Britain? Are we not compelled to believe that man formed part of the Devonshire fauna long before the hyæna did? Is there any method of escaping the conclusion that between the era of the Breccia and that of the Cave-earth it was possible for the hyæna to reach Britain?—in other words, that the last continental state of our country occurred during that interval? I confess that, in the present state of the evidence, I see no escape; and that the conclusion thus forced on me compels me to believe also that the earliest men of Kent's Hole were *interglacial*, if not *preglacial*.

The following table will serve to show at one view the co-ordinations and theoretical conclusions to which the facts of Kent's Cavern have led me, as stated briefly in the foregoing remarks. The table, it will be seen, consists of two divisions, separated with double vertical lines. The first, or left hand, division contains three columns, and relates exclusively to Kent's Cavern, as is indicated by the words heading it. The second, or right hand, division is of a more general character, and shows the recognised classification of well-known facts throughout Western Europe. The horizontal lines are intended to convey the idea of more or less well-defined chronological horizons, and their occasional continuity through two or more columns denotes contemporaneity. Thus, to take an example from the two columns headed "Archæological" and "Danish-Bog," in the second division: the horizontal line passing continuously through both, under the words "Iron" and "Beech," is intended to suggest that the "Iron Age" of Western Europe and the "Beech" zone of the Danish Bogs take us back about equally far into antiquity; whilst the position of the line under the word "Bronze" indicates that the "Bronze age" (still of Western Europe) takes us back from the ancient margin of the Beech era, through the whole of that of the Pedunculated Oak, and about half-way through the era of the Sessile Oak; and so on in all other cases.

<table>
<tr><th colspan="3">KENT'S CAVERN.</th><th colspan="5">PERIODS.</th></tr>
<tr><th>Deposits.</th><th>Bones.</th><th>Implements.</th><th>Archæological.</th><th>Danish-Bog.</th><th>Biological.</th><th>Geographical.</th><th>Climatal.</th></tr>
<tr><td rowspan="4">Black Mould.</td><td rowspan="4">Ovine.</td><td rowspan="4">Iron, Bronze, and (?) Neolithic.</td><td>Iron.</td><td>Beech.</td><td rowspan="4">Recent.</td><td rowspan="4">Insular.</td><td rowspan="4">Post-Glacial.</td></tr>
<tr><td rowspan="2">Bronze.</td><td>Pedunculated Oak.</td></tr>
<tr><td>Sessile Oak.</td></tr>
<tr><td>Neolithic.</td><td>Scotch Fir.</td></tr>
<tr><td>Granular Stalagmite.</td><td rowspan="3">Hyænine.</td><td rowspan="3">Palæolithic Flakes.</td><td rowspan="5">Palæolithic.</td><td rowspan="5"></td><td rowspan="5">Pleistocene.</td><td rowspan="3">Continental.</td><td rowspan="4">Glacial and (?) Inter-Glacial.</td></tr>
<tr><td>Black Band.</td></tr>
<tr><td>Cave-earth.</td></tr>
<tr><td>Crystalline Stalagmite.</td><td rowspan="2">Ursine.</td><td rowspan="2">Palæolithic Nodules.</td><td>Insular.</td></tr>
<tr><td>Breccia.</td><td>Continental.</td><td>Pre-Glacial.</td></tr>
</table>

SECTION D.

BIOLOGY.

OPENING ADDRESS BY THE PRESIDENT, J. GWYN JEFFREYS, LL.D., F.R.S., TREAS. G. AND L.SS.

BEING merely an amateur naturalist, and not having had any strictly scientific education, I consider it a great honour to be invited to preside over this important Section of the Association. I cannot pretend to give such an address as may be expected from the president; but I will offer some remarks on a subject in which I take considerable interest and have done some work, viz., the deep-sea mollusca.

The historical part of the subject has been fully treated by Dr. G. C. Wallich in his "North-Atlantic Sea-bed," 1862; by Prof. Prestwich in his Presidential Address to the Geological Society of London in 1871; and by Prof. Sir Wyville Thomson in his "Depths of the Sea," 1873.

By the term "deep-sea" I do not mean the zone which the late Prof. Edward Forbes called the eighth, and which he supposed to be the lowest and the limit of habitability, in his elaborate and excellent "Report on the Ægean Invertebrata," published by the Association in 1844. That zone comprised the depths lying between 105 and 230 fathoms. Nor would I refer to it the "deep-sea" zone which I defined in the Introduction to my work on "British Conchology," 1862; this applied to the British seas only, and extended to the "line of soundings," being about 100 fathoms. Since that time the exploring expeditions in H.M.SS. *Lightning*, *Porcupine*, *Challenger*, and *Valorous*, as well as in the Norwegian frigate *Vöringen*, have shown that mollusca inhabit the greatest depths that have been examined, and that life is not less abundant and varied in the abysses of the ocean than it is in the shallowest water. Instead of 300 fathoms or 1,800 feet, which Forbes assumes to be the extreme boundary of submarine life, we must now take 3,000 fathoms, or 18,000 feet, and even much lower depths. It may be well to distinguish two zones of depth exceeding that which I have termed "the line of soundings"; and I would propose the name "abyssal" for depths between 100 and 1,000 fathoms, and "benthal" (from the Homeric word βένθος, signifying the depths of the sea) for depths of one thousand fathoms and more.

The first knowledge that I had of the mollusca from the lowest or "benthal" zone I owe to Dr. Wallich, who kindly gave me a few small shells which he got in a sounding of 1,622 fathoms in N. Lat. 55° 36′, W. Long. 54° 33′, off the coast of Labrador, during his cruise in H.M.S. *Bulldog* in 1860. These consisted of undescribed species of *Aclis*, *Homalogyra*, and *Pleurotoma*, *Pleurotoma tenuicostata* of M. Sars, and fragments of *Saxicava rugosa*, Linné, and of other shells which are unknown to me. Among these was a dead but perfect specimen of *Crenella faba*, Fabricius, which is a common inhabitant of the laminarian zone in Arctic seas, and may have been voided by a fish or sea-bird. This would account for the occasional occurrence at great depths of other shallow-water shells and fragments.

I had the good fortune to take part in the two *Porcupine* expeditions of 1869 and 1870, and in the *Valorous* cruise of 1875; and the mollusca of the *Lightning* (1868), *Challenger* (1873-76), and *Vöringen* (1876) expeditions have been submitted to my inspection. I am consequently enabled to form some idea of the bathymetrical distribution of the mollusca thus obtained, with the aid of my dredging experience for upwards of forty years.

Perhaps the best way of communicating this idea to others will be by giving the subjoined list of the species of deep-sea mollusca dredged by me in the *Valorous*, all of which are found at depths exceeding 1,000 fathoms. The range of depth there and elsewhere in the North Atlantic and Mediterranean will be noted, as well as some geological and other observations. Four only of such deep dredgings were made during the cruise, viz., in 1,100, 1,750, 1,450, and 1,785 fathoms. The first two were in Davis Strait, and the other two between Cape Farewell and W. Long. 26° on the return voyage.

Names of species.	Range of depth in fathoms.	Observations.
BRACHIOPODA.		
Terebratula tenera, *Jeffreys* ...	1450	
Atretia gnomon, *J.*	1100-1750	
Discina Atlantica, *King* ...	690-2400	Coralline-Crag fossil.
CONCHIFERA.		
Pecten fragilis, *J.*	1000-1785	
Amussium lucidum, *J.* ...	156-1450	
Lima ovata, *S. V. Wood* ...	1450	Coralline-Crag and Monte-Mario fossil.
L. subovata, *J.*	49-1450	
L. gibba, *J.*	1450-1785	
Idas argenteus, *J.*	994-1450	
Dacrydium vitreum, *Möller*..	30-2435	Sicilian fossil.
Nucula reticulata, *J.*	420-1470	
Leda acuminata, *J.*	20-1750	Sicilian fossil, as *L. Messanensis*, Seguenza.
L. pusio, *Philippi*, var. ...	257-1750	Sicilian fossil.
L. pustulosa, *J.*	202-1470	Sicilian fossil.
L. expansa, *J.*	690-1750	
L. lata, *J.*	165-1785	
L. sericea, *J.*	740-1450	
Glomus nitens, *J.*	557-1750	
Limopsis tenella, *J.*	1450	
L. cristata, *J.*	292-1095	
Arca pectunculoïdes, *Scacchi*.	20-1100	Coralline-Crag and Sicilian fossil.
Malletia excisa, *Ph.*	1443-1750	Sicilian fossil.
M. cuneata, *J.*	718-1800	
Montacuta Dawsoni, *J.* ...	3-1750	Fragments only at greatest depth.
Kellia symmetros, *J.*	488-1750	
Axinus cycladius, *S. V. Wood*	30-1750	Coralline-Crag fossil.
A. eumyarius, *M. Sars* ...	114-1456	
A. Croulinensis, *J.*	20-1785	
A. incrassatus, *J.*	40-1750	
Diplodonta Torelli, *J.* ...	30-1450	Fragment only at greatest depth.
Isocardia cor, *L.*	40-1785	Fry only at greatest depth.
Tellina calcaria, *Chemnitz* ...	1-1750	Sicilian fossil. Fragments only at greatest depth.
Poromya rotundata, *J.* ...	1450	
Pecchiolia abyssicola, *M. Sars*	110-1450	Fragments only at greatest depth.
P. gibbosa, *J.*	1450	Fragment only.
P. tornata, *J.*	1785	Fragment only.
Neæra striata, *J.*	435-1450	
N. exigua, *J.*	1450	
N. notabilis, *J.*	1450	
N. circinnata, *J.*	994-1450	
N. papyria, *J.*	1450	
N. angularis, *J.*	290-1785	Fragment only at greatest depth.
SOLENOCONCHIA.		
Dentalium candidum, *J.* ...	410-2435	
D. capillosum, *J.*	220-1785	
D. ensiculus, *J.*	740-1785	
D. subterfissum, *J.*	1000-1476	Fragment only from *Valorous*.
D. vagina, *J.*	1450-1785	
Siphodentalium vitreum, *M. Sars*	150-1750	
S. affine, *M. Sars*	100-1450	
S. Lofotense, *M. Sars* ...	20-1750	
Cadulus tumidosus, *J.* ...	110-1450	
C. Olivi, *Sc.*	539-1450	Sicilian fossil.
C. cylindratus, *J.*	1215-1476	
GASTROPODA.		
Propilidium ancyloïdes, *Forbes*	60-1450	Sicilian fossil, as *Rostrisepta parva*, Seg.
Puncturella profundi, *J.* ...	740-1750	
Scissurella crispata, *Fleming*.	7-1095	Sicilian fossil.
S. tenuis, *J.*	1450	
Cyclostrema basistriatum, *J.*	50-1095	Sicilian fossil.
Acirsa prælonga, *J.*	994-1450	
Eulima stenostoma, *J.* ...	50-1456	
Natica affinis, *Gmelin*	5-1100	Fragments only at greatest depth.
N. sphæroides, *J.*	1750	A young shell.
Seguenzia formosa, *J.*	325-1785	Sicilian fossil, as *S. monocingulata*, Seg.
S. carinata, *J.*	690-1095	
Cerithium procerum	400-1450	*C. Danielseni*, Friele.
Trophon Fabricii, *Beck* ...	35-1450	Fragment only at greatest depth.
Fusus attenuatus, *J.*	690-1215	
F. Sabini, *Gray*	100-1450	Fragments only at greatest depth.
Pleurotoma tenuicostata, *M. Sars*	40-1622	
P. exarata, *Möll*	5-1230	
Cylichna alba, *Brown*	7-1400	Sicilian fossil.
Utriculus lacteus, *J.*	1443-1450	Fragment only at greatest depth.
U. substriatus, *J.*	1750	
Actæon exilis, *J.*	49-1450	Sicilian fossil.
Scaphander puncto-striatus, *Mighels and Adams*	26-1450	Sicilian fossil. Fragment only at greatest depth.

Besides undeterminable fragments of other and probably new species.

The species named in the above list are 75 in number. Of these no less than 46 have been described by me for the first time in the *Annals and Magazine of Natural History*, for 1876 and 1877. Several of them were also procured in the *Porcupine*, *Challenger*, and *Vöringen* expeditions. A great many more deep-sea species remain to be worked out and described by me from the *Porcupine* expeditions of 1869 and 1870.

I have not included the pteropods in the list, although their shells occur at the greatest depths—because they are oceanic, and inhabit only the surface or superficial zone, their shells falling to the bottom after death and when evacuated by predaceous animals.

The mollusca of very deep water, or the benthal zone, are certainly peculiar, and constitute part of a distinct fauna, notwithstanding that some of them frequent shallower water. It is very difficult to say how far they may be affected by bathymetrical conditions. An important contribution to this part of the subject was made by Mr. Buchanan at a recent meeting of the Royal Society of Edinburgh, in which he stated, as the preliminary result of his analysis of the sea-water collected in the *Challenger* expedition, that as regards the percentage of oxygen present at different depths, it diminishes from the surface to a depth of 300 fathoms, and increases from this point to lower depths.[1] See also my account of the behaviour of *Trochus occidentalis*, when dredged from the deep-sea zone on our northern coasts, which is explained by Mr. Buchanan's statement.[2]

They are not always of a small size. In the *Porcupine* expedition of 1869, the dredge brought up, at the depth of 1,207 fathoms, in the Bay of Biscay, a living specimen of *Fusus attenuatus*, which measures two inches and a quarter in length; and another dredging at the depth of 2,435 fathoms (nearly three miles) in the same part of the Bay, yielded a living specimen of *Dentalium candidum* about an inch and a half long. In the *Challenger* expedition was trawled, at the depth of 1,600 fathoms, in the South Atlantic (S. Lat. 46° 16′, E. Long. 48° 27′), a living specimen of a magnificent shell belonging to *Cymbium* or an allied genus, which has a length of six inches and three-quarters and a breadth of four inches! And during my cruise in the *Valorous*, I dredged, at the depth of 1,100 fathoms, in Davis Strait, a living specimen of *Dentalium candidum* an inch and three-quarters long. These treasures of the deep are so apt to entrance the imagination of a naturalist, that I have often dreamt of walking on the sea-bed and picking up unknown and wonderful shells; and in my waking hours I have envied the faculty of the argonaut in Morris's "Life and Death of Jason,"

> "Euphemus, who had power to go
> Dryshod across the plain no man doth sow."

I hope it is pardonable to avail one's self of a little poetical licence to make the quotation applicable to the bottom as well as to the surface of the sea.

The distribution of the deep-sea mollusca is unquestionably caused by submarine currents, with the direction and extent of which, however, we are unacquainted. As far as I have had an opportunity of judging from the mollusca of the North and South Atlantic, I am inclined to think that the Arctic and Antarctic currents do not extend beyond the Equator. The South-Atlantic species procured by the *Challenger* party in deep water appear to be different from those of the North-Atlantic in similar depths, according to our present notion of species. It is unnecessary for me to renew my objection to the phrase "representative species," as Sir Wyville Thompson has satisfactorily disposed of the matter in page 14 of his "Depths of the Sea."

It will be seen, on referring to the list of deep-water mollusca procured in the *Valorous* cruise, that several of the species are also Sicilian fossils. They occur in the Pliocene formation of the south of Italy. Professor Seguenza has just published a very complete and valuable catalogue entitled "Elenco dei Cirripedi e dei Molluschi della zona superiore dell' antico plioceno,' which are arranged in two divisions, "Depositi littorali" and "Depositi submarini." But some further distinction would seem to be necessary in order to separate the strata, inasmuch as certain species which are assuredly littoral are included in the submarine division. For instance, *Actæon pusillus*, Forb. (which lives at depths varying from 40 to 1,456 fathoms), and *Cylichna ovata*, J. (66–862 fathoms), are entered in both divisions; while peculiarly shallow-water species, such as *Patella vulgata*, *Tectura virginea*, and six now also living species of *Chiton*, appear only in the submarine or deep-water division. Many of the species in Seguenza's Catalogue (besides those noticed in the *Valorous* list of deep-water mollusca), which had been previously considered extinct, were discovered by me in the *Porcupine* expeditions to be still living; and I have no doubt that the rest of the so-called extinct species, from the upper zone of the older Pliocene in Sicily, will sooner or later be detected in future deep-sea explorations. In fact our examination of the abyssal fauna has been hitherto extremely slight and cursory, taking into account the enormous extent of area, the difficulties caused by unfavourable weather, and the inadequacy of the instruments used in the investigation. Our good neighbours, the Norwegians, have not relaxed in their work; and while this Address is being delivered their second year's expedition to the Arctic seas will almost have been completed. May every success attend them!

There has been lately a good deal of controversy as to the supposed "continuity of the chalk"; and the affirmative of the proposition has been most ably argued by my colleague and friend, Sir Wyville Thomson, in his "Depths of the Sea."

Prof. E. Forbes, in his "Report on Ægean Invertebrata" (1844), was, I believe, the first to state the proposition. He says, at p. 178, that the strata in his lowest region, or 230 fathoms, would, if filled up, "present throughout an uniform mineral character closely resembling that of chalk, and will be found charged with characteristic organic remains and abounding in foraminifera. We shall, in fact, have an antitype of the chalk."

Sir Wyville Thomson supports his view by the weighty authority of Dr. Carpenter, Prof. Huxley, and Prof. Prestwich; and although the late Sir Charles Lyell entered a vigorous protest against the hypothesis, and went so far as to designate it a "popular error," I will refrain from expressing any opinion of my own, but will content myself with stating a few facts in elucidation of the question.

The comparison of the deep-sea ooze with the geological formation known as chalk depends on two points, viz., the mineral composition and the organisms belonging to each.

1. *Mineral Composition.*—The late Prof. David Forbes, whose knowledge as a mineralogist and chemist was universally recognized, furnished me, on my return from the *Porcupine* expedition of 1869, with a complete analysis of a sample of Atlantic mud procured at a depth of 1,443 fathoms. He proved that it differed from ordinary chalk in containing scarcely more than 50 per cent. of carbonate of lime, whereas chalk consisted all but entirely of carbonate of lime. Indeed Sir Wyville Thompson admits that "a more careful investigation shows that there are very important differences between them."

2. *Organisms.*—I must here confine myself chiefly to the mollusca, which Sir C. Lyell regarded as "the highest or most specialized organization" on which geological reasoning and classification are founded.

Misled by the apparent resemblance of Mediterranean and Atlantic ooze to the ancient chalk, geologists have been accustomed to consider the chalk fauna as having lived in deep water. Let us see how this is with respect to the mollusca. I have lately, with the assistance of Mr. Henry Woodward and Mr. Etheridge, examined the cretaceous mollusca in the British Museum and the Museum of Economic Geology; and Mr. Etheridge has most obligingly prepared and furnished me with a tabular list of the genera and number of species in each genus in the upper cretaceous group (exclusive of the gault and greensand), which list I will, with his permission, here insert:—

Genera.	No. of species in Chalk-marl.	No. of species in Lower Chalk.	No. of species in Upper Chalk.
BRACHIOPODA.			
Argiope … … … …	1	1	…
Crania … … … …	1	2	2
Kingena … … … …	1	1	1
Magas … … … …	2	1	1
Rhynchonella … … …	13	7	3
Terebratella … … …	2	1	2
Terebratula … … …	11	10	4
Terebratulina … … …	2	2	2
Telebrirostra … … …	1	…	…
Thecidea … … … …	…	1	1
Trigonosemus … … …	1	1	1
Total … … …	35	27	17

[1] NATURE, June 14, 1877.
[2] "British Conchology," vol. iii. pp. 335, 336.

Genera.	No. of species in Chalk-marl.	No. of species in Lower Chalk.	No. of species in Upper Chalk.
LAMELLIBRANCHIATA. (Conchifera.)			
Avicula	1	1	...
Exogyra	3	2	3
Gervillia	?	...	...
Inoceramus	6	8	9
Lima	3	9	7
Ostrea	3	5	11
Pecten	5	10	12
Pinna	...	...	2
Plicatula	2	1	1
Spondylus	4	2	4
Arca	2	2	1
Astarte	...	1	1
Chama	...	...	1
Cypricardia	...	...	1
Diceras	...	...	1
Isocardia	...	...	1
Leda	...	1	...
Modiola	...	1	1
Opis	1	1	...
Pholadomya	1	1	3
Teredo	1	1	2
Trigonia	3	...	...
Unicardium	...	1	1
Venus	1	1	...
	36	48	62
Radiolites	...	?	1
(Solenoconchia.)			
Dentalium	1	1	...
GASTROPODA.			
Actæon	1	1	...
Aporrhais	2	3	...
Avellana	1	1	...
Calyptræa	...	...	1
Cerithium	1	1	...
Columbellina	2	...	...
Dimorphosoma...	2	...	...
Dolium	...	1	...
Emarginula	1	2	...
Fusus	...	1	2
Gibbula	1	...	...
Hipponyx	...	...	1
Natica	...	1	...
Patella	...	...	1
Pleurotomaria	3	2	3
Pterocera	1	...	...
Rostellaria	1	...	...
Scalaria	...	1	1
Solarium	1	3	1
Trochus	1	1	2
Turbo...	...	3	...
Turritella	...	1	1
	18	22	13
CEPHALOPODA.			
Ammonites	29	31	9
Ancyloceras	1	...	...
Aptychus	...	...	6
Baculites	1	2	1
Belemnitella	1	2	3
Belemnites...	3	2	...
Crioceras	...	...	1
Hamites	...	3	1
Helicoceras	...	...	1
Nautilus	10	10	2
Scaphites	1	2	1
Turrilites	11	11	...
Annisoceras	1	...	...
	58	63	25

A glance at the above list, and, much more, an inspection of the chalk mollusca in a good collection, ought to convince any conchologist that all these genera were comparatively shallow-water forms. I should infer that the depth might have been from low-water mark to 40 or 50 fathoms. None of the genera are deep-water. *Chama*, *Ostrea*, *Pinna*, *Calyptræa*, *Hipponyx*, and, most assuredly, *Patella* cannot be placed in the latter category; and the old proverb, "noscitur ex sociis," will apply to mollusks as well as to men. *Teredo* may have been littoral or have come from floating wood. Not a single species of *Leda*, *Pecchiolia* (or *Verticordia*), *Neæra*, not one of the Solenoconchia, nor of the *Bulla* family occurs in the upper or white chalk, although they now inhabit the deep-sea ooze and especially characterize the modern deposit.

But *Nautilus* and *Spirula* are believed by some to be deep-water forms. This must be a mistake. Although the animal of that common species *Nautilus pompilius* has rarely been met with, the shells are often found on beaches in the Indian Ocean and South Pacific; and I am not aware of any instance of a deep-water mollusk being cast ashore. It is not likely. Rumphius (the "Plinius Indicus"), in his "Amboinsche Rariteitkamer," or Cabinet of the Curiosities of Amboyna, 1705, has given an interesting account of the habits of the pearly nautilus, a translation of which I will copy from the admirable monograph of Professor Owen:—"When the nautilus floats on the water, he puts out his head and all his tentacles, and spreads them upon the water, with the poop of the shell above water; but at the bottom he creeps in the reverse position, with his boat above him, and with his head and tentacles upon the ground, making a tolerably quick progress. He keeps himself chiefly upon the ground, creeping also sometimes into the nets of the fishermen; but after a storm, as the weather becomes calm, they are seen in troops, floating on the water, being driven up by the agitation of the waves. This sailing, however, is not of long continuance, for having taken in all their tentacles, they upset their boat, and so return to the bottom."

As to the *Spirula*, the old Dutch naturalist remarked that it attaches itself to the rocks, and is thrown up on the beach when the north wind blows. Péron found the first living specimen in Australia; Mr. Percy Earl obtained one on the coast of New Zealand; the late Sir Edward Belcher another in the Indian Archipelago; Mr. Bennett got one off Timor; and an imperfect specimen was procured in the *Challenger* Expedition. I was favoured, in January, 1875, by Mr. J. Tyerman, of Tregeny, sending for my inspection a perfect specimen of *Spirula australis* and one of *Argonauta gondola* in spirit of wine, with a memorandum that "the *Spirula* and *Argonauta* were taken by a friend while dredging or, rather, skimming for pteropods in the Persian Gulf." Mr. Tyerman added that other live specimens of the *Spirula* were captured at the same time. Sir Lewis Pelly informs me that the Persian Gulf is nowhere deeper than between 40 and 50 fathoms. *Spirula* has apparently the same habit as species of *Loligo* and allied genera, in occasionally frequenting the surface of the sea. The shells of *S. australis* are thrown up in considerable numbers on every beach in the North Atlantic, having been wafted northwards by winds and the equatorial current or so-called "Gulf Stream."

Assuming, therefore, that the usual habitat of mollusca in past epochs did not differ from that of recent mollusca of the same kind, I think we may safely conclude that the shells of the cretaceous system, or, more strictly, the upper chalk, belonged to shallow and not deep-water mollusca.

Mr. Woodward tells me that the chalk crustacea are also shallow-water forms.

The white chalk is in many places principally composed of *Globigerinæ*, *Orbulinæ*, and coccoliths or coccospheres, all of which inhabit at present the surface of the sea. According to Dr. Wallich, *Globigerina* is found in all latitudes and at all depths, ranging from 50 to 3,000 fathoms.[1] Mr. Parker and Prof. Rupert Jones (first-rate authorities on the foraminifera) admit that *Orbulina* and *Globigerina* are "occasionally found in shallow water."[2]

I cannot identify a single species of the cretaceous mollusca as now living or recent. All of them are evidently tropical forms. One of the cretaceous species, indeed, *Terebratula striata*, Wahlenberg, has been supposed by some palæontologists to be identical with *T. caput-serpentis*, the latter of which has a range of bathymetrical distribution from low-water mark to

[1] "North-Atlantic Sea-bed," p. 137.
[2] *Quart. Journ.* Geol. Soc., vol. xvi. p. 279.

808 fathoms; its geographical extension is equally great, and it has also not a slight amount of variation in shape and sculpture. But I am not disposed to unite the two species. In *T. striata* the ribs are much narrower than in the typical *T caput-serpentis* and are finely beaded or tuberous, especially towards the beaks, and they are not so close together as in the variety *septentrionalis*. This question of identity depends, however, on the capability of hereditary persistence which some species possess; and although a certain degree of modification may be caused by an alteration of conditions in the course of incalculable ages, our knowledge is not sufficient to enable us to do more than vaguely speculate, and surely not to take for granted the transmutation of species. We have no proof of anything of the kind. Devolution, or succession, appears to be the law of nature; evolution (in its modern interpretation) may be regarded as the product of human imagination. I am not a believer in the fixity of species, nor in their periodical extinction and replacement by other species. The notorious imperfection of the geological record ought to warn us against such hasty theorization. We cannot conceive the extent of this imperfection. Not merely are our means of geological information restricted to those outer layers of the earth which are within our sight, but nearly three-fourths of its surface are inaccessible to us, so long as they are covered by the sea. Were this not the case, we might have some chance of discovering a few of the missing links which would connect the former with the existing fauna and flora. It is impossible even to guess what strata underlie the bottom of the ocean, or when the latter attained its present position relatively to that of the land. The materials of the sea-bed have been used over and over again in the formation of the earth's crust; "Omnia mutantur, nihil interit;"[1] and the future history of our globe will, to the end of time, repeat the past. What does Shakespeare say, as a geologist, to such cosmical changes?

> "O heaven! that one might read the book of fate,
> To see the revolution of the times
> Make mountains level, and the continent
> (Weary of solid firmness) melt itself
> Into the sea! and, other times, to see
> The beachy girdle of the ocean
> Too wide for Neptune's hips."

There is also the difficult problem of submarine light, evidenced by the facts of deep-sea animals having conspicuous and well-formed eyes, and of the shells of deep-sea mollusca being sometimes coloured, which is yet unsolved.

Much more remains to be done; and probably many generations, nay, centuries, must elapse before the very interesting subject which I have now ventured to submit to your consideration will be mastered or thoroughly understood in all its varied aspects. Let us then confess our ignorance, and conclude in the sublime words of the Psalmist:—"Thy way is in the sea, and thy path in the great waters, and thy footsteps are not known."[2]

[1] Ovid, Met. xv. 165.
[2] Ps. lxxvii. 19.

THE BRITISH MEDICAL ASSOCIATION.

DURING the past week the British Medical Association held its forty-fifth annual meeting in Manchester. The Committee of Management for the reception of the Association deserves hearty congratulations on the success which has followed their hospitable effort. For although they were under no disadvantages of position or room, but rather the contrary, they had spared no pains whatever to secure the comfort of their guests, and they may fairly be said to have equalled or outdone their opportunity.

The class-room and lecture theatres of the Owens College and Medical School, were placed at the disposal of the Committee, and gave the Association most convenient means of holding its general business and sectional meetings. The large museum at the Medical School, the dissecting room, the physiological laboratory, the chemical laboratories and the engineering drawing room were set aside for the purposes of the Annual Museum. The museum—very extensive this year—included besides pathological and surgical specimens, plates, casts, &c., an unusually large number of histological specimens, chiefly of morbid tissues. In addition there was the usual display of surgical and scientific instruments, the latter being reinforced by the collection of physiological apparatus belong to the Medical School. The general meetings except the first were held in Prof. Roscoe's lecture theatre, and there also were delivered the special addresses in medicine, Surgery, Obstetrics, and Physiology. The first general meeting, and the address of the President of the Association took place in the Concert Hall, none of the college rooms being large enough for the purpose. A temporary covered way joined the Medical School to the College, and on the ground between the two buildings was erected a tent or series of tents in which were exhibited a large number of sanitary appliances under the auspices of the Manchester and Salford Sanitary Association.

The meeting of the Association was inaugurated on Tuesday morning by the Bishop of Manchester who preached a sermon in the Cathedral; and in the afternoon of the same day the first general meeting was held for the election of the president for the year and for hearing the Report of Council. The retiring president Dr. De Bartolomé, of Sheffield, alluding to the events of his official year, spoke with much spirit of the manner in which the public services of the medical men engaged in the rescue of entombed miners after the Pont-y-pridd colliery accident had been ignored by the Government and the nation; and he announced that the Council, having regard to the fact that there was no provision for the recognition of heroic or meritorious services when performed by medical men as such, had determined to confer upon the medical men concerned in the accident a medal and a testimonial scroll, and had recommended that the medal should be perpetuated as the Medal of the British Medical Association, to be awarded for like acts in the future. The latter suggestion was afterwards adopted at the second general meeting.

The president, Dr. M. A. Eason Wilkinson, Senior Physician to the Manchester Royal Infirmary, having been elected, delivered an address on Hospital Defects and their Remedies a subject which is greatly engaging the attention of local medical men. He gave a history of the Manchester Royal Infirmary and spoke with satisfaction of the union of the School of Medicine and the Owens College.

In the evening there was a reception by the President of the Association and the Senate and Council of the Owens College, held at the College.

On Wednesday a general meeting of the Association assembled to hear the special address in Medicine, by Dr. William Roberts, F.R.S., on the subject of Spontaneous Generation and the doctrine of Contagium Vivum. Dr. Roberts' treatment of the subject may be considered to fall into three divisions—physiological, pathological, and theoretical.

In the first, after alluding to the anology which may possibly be real, between contagious fever and the action, say, of yeast in fermentation, he proceeded to consider two propositions. The first proposition is: That organic matter has no inherent power of generating bacteria, and no inherent power of passing into decomposition. To substantiate this he exhibited specimens of decomposable organic fluids which, having been sterilized, had remained in his possession undecomposed for many months or even years. Sterilization had been effected three ways:—

1. By prolonged boiling, the exclusion of germs being afterwards secured by plugs of cotton-wool.
2. By filtration through unglazed earthenware previously heated to redness, into flasks sterilized by the heat of boiling water.
3. By transferring the organic decomposable fluid, such as blood, urine, pus, etc., directly from the interior of the body to well sterilized flasks and subsequently defending them from germs by plugs of cotton-wool.

The second proposition is:—That bacteria are the actual agents of decomposition. This Dr. Roberts considers to be proved by the following considerations:—

a. That which originates decomposition comes from the air; since removal of the plugs in any of the above cases is infallibly followed by decomposition.

b. That which originates decomposition consists of solid particles floating in the air; since filtration of the air (as above) is able to prevent decomposition: and air which is optically pure (Tyndall) has no fecundating power.

c. That which originates decomposition has not the nature of a soluble ferment; since decomposable fluids *in which putrefaction has already set in* yield filtrates through earthenware, which do not decompose, while pepsin, diastase, &c., readily pass through the same medium.

But it is nevertheless true that certain liquids, as neutralized hay, infusions, and milk, often produce bacteria even after they have been boiled for two or three hours, and when there is no possibility of subsequent infection. And it is equally true