THURSDAY, SEPTEMBER 1, 1881

THE BRITISH ASSOCIATION

THE Fifty-first Annual Meeting of the British Association was opened yesterday under the presidency of Sir John Lubbock, Bart., M.P., F.R.S., at York, the birthplace of the Association fifty years ago (September 27, 1831). Almost as easily might we compare the first meeting of the Accademia del Cimento when Roberval and Mersennus and Torricelli discussed the nature of the vacuum with the last meeting of the Nuovo Cimento, as compare the meeting of the British Association of 1831 with that of 1881. Railways, telegraphs, telephones, and electric lighting were unknown; the doctrines of evolution and the conservation of energy had not been developed; geology, palæontology, and petrology were in their infancy; the modern applications of spectroscopy were scarcely thought of; the mechanical equivalent of heat had not been determined. Several sciences, which at that time consisted of a mere collection of ill-arranged facts, have since, by the application of logical methods, had conferred upon them an individuality which they never before possessed. Science schools have arisen in all directions; the State yearly examines some thousands of its subjects; the Universities have created new professorships, have vitalised the old ones, and have placed science scholarships on an equality with those which formerly were only given for classics and mathematics. The Universities having opened their doors to the new culture, and it has become a necessary part of elementary education; while technical schools in all our large centres instruct thousands of artisans in the rudiments of natural knowledge. Has the British Association kept pace with this prodigious development?

What were the ideas of its founders? William Vernon Harcourt, "the lawgiver and proper founder of the British Association," said at the opening meeting that its objects should be "to give a stronger impulse and more systematic direction to scientific inquiry, to obtain a greater degree of national attention to the objects of science, and a removal of those disadvantages which impede its progress, and to promote the intercourse of the cultivators of science with one another and with foreign philosophers." By its reports, committees, recommendations, and grants, the Association has to some extent succeeded in each of these objects. But Mr. Vernon Harcourt planned the Association on a wider basis than that upon which it rests. "I propose to you," he said, "to found an association, including all the strength of Great Britain, which shall employ a short period of every year in pointing out the lines of direction in which the researches of science should move; in indicating the particulars which most immediately demand investigation; in stating problems to be solved and data to be fixed; in assigning to every class of mind a definite task; and suggesting to its members that there is here a shore of which the soundings should be more accurately taken, and there a line of coast along which a voyage of discovery should be made." We venture to think that this course of action might be more closely followed with advantage. It is true that a few committees are appointed to report upon, and sometimes to experiment upon, certain defined objects, but if each section could give a list of the most important questions awaiting answer in its particular science—somewhat in the form of a modernised *Inquisitio de Naturâ Calidi*—energy would less often be expended about the mint, the anise, and the cumin, and more often applied to the weightier matters of the sciences. Men would then more frequently forge connections in the mighty chain, in place of separate links which sometimes rust away before a place is found for them.

The earlier presidents delighted to find in the Association the development of Bacon's idea of the "New Atlantis." But we venture with great deference to submit that it never has and never can approach the character of that academy of universal science. A nearer approach to it was to be found in the old Gresham College, and may now be met with in any one of the new colleges of sciences. Bacon's idea was to have a vast inclosure containing "elaboratories chymicall and phisicall," anatomical and metallurgical, observatories of every kind, botanical gardens, museums, and operatories for every science. Connected with these there was to be a staff of workers and a staff of thinkers; also a kind of scientific society, or collection of societies, in which the results should be discussed. There are a thousand workers in the domains of the sciences now where there was one fifty years ago; discoveries and inventions multiply, and scientific literature is assuming vast proportions; but at present we are as far from the lofty and majestic ideal of the New Atlantis as we were in 1831.

But let us not for a moment underrate the valuable work which the Association has accomplished. Many of the Reports of committees or individuals are classical, and the suggestions which they furnish have led to considerable results. Take one example: the establishment of magnetic observatories all over the world is mainly due to the action of the Association. "By no sudden impulse or accidental circumstance," said Prof. Phillips in the Birmingham presidential address in 1865, "rose to its high importance that great system of magnetic observations on which for more than a quarter of a century the British Association and the Royal Society, acting in concert, have been intent. First we had reports on the mathematical theory, and experimental researches of magnetism by Christie, 1833; Whewell, 1835; and Sabine, 1835. Afterwards a magnetic survey of the British Islands; then the establishment of a complete observatory at Dublin, with newly arranged instruments, by Dr. Lloyd in 1838. On all this gathered experience we founded a memorial to Her Majesty's Government, made a grant of 400*l*. from our funds for preliminary expenses, and presented to the meeting of this Association in Birmingham in 1839 a report of progress signed by Herschel and Lloyd. From that time how great the labour, how inestimable the fruits! Ross sails to the magnetic pole of the south; America and Russia co-operate with our observers at Kew, Toronto, and St. Helena; and General Sabine, by combining all this united labour, has the happiness of seeing results established of which no man dreamed—laws of harmonious variation affecting the magnetic elements of the globe, indefinite relation to the earth's movement, the position of the sun and moon, the distribution of temperature, and the situation in latitude and longitude."

We must bear in mind, however, that the great mass of members at any one meeting are not made up of scientific men who can appreciate the full development of a train of ideas or results, but of people who have not the advantages of attending the meetings of the London scientific societies, or of being *au courant* with scientific progress, and we may fitly inquire by what means their interests are best served. The President's address is perhaps the most powerful stimulus. Such addresses usually belong to one of three classes:—they are either distinguished by a fine display of oratory; or by the discussion of some leading theory concerning which the president has a right to speak *ex cathedrâ;* or they give a *résumé* of the scientific progress of the year. This last is of the greatest utility to the general run of members. Sometimes the three classes are judiciously combined, and these addresses are commonly the best of all. In former years the Presidential Address was very short, and chiefly discussed the results obtained by the Committees, and the Reports thereon. Occasionally an unscientific nobleman has opened the proceedings by a *quasi* after-dinner speech, while anon we have a sophistical declamation dealing with some of the burning questions of the hour, and disposing of them bravely.

Many cities have received the Association twice, but few three times. York will now be one of the latter, but it is thirty-six years since the last meeting was held there. Murchison called it the "cradle of the Association," and at the second York meeting the tickets bore the inscription, *Antiquam exquirite Matrem.* If the Association carries out the ideas of its founders, we may fairly hope that a centennial and even a millennial meeting will be held in the place of its birth. The city has many objects of interest: it possesses convenient accommodation for all the sections, and a number of important manufactories can be easily visited from it. The local committee have issued an extremely useful programme of their arrangements, which not only contains all the necessary information concerning trains, posts, lodgings, and the places of meeting, but also articles on the zoology, botany, and geology of the neighbourhood, and a description of the various excursions. An interesting article on "The York Founders of the Association" is contributed by Archdeacon Hey. An exhibition of art and industrial produce, and a collection of scientific apparatus, will be open during the week. Four excursions are organised for Saturday, September 3: to Scarborough; to Castle Howard; to Helmsley and Rievaulx; and to Brimham Rocks and Harrogate. On the following Thursday there will be seven excursions: to Bolton Abbey and the Strid; to Cleveland; a coast excursion; to Gristhorpe, Speeton, and Scarborough; to Whitby; to Wensleydale; and to Aldborough and Boroughbridge. Among the more important manufactories which will be visited are the telescope works of Messrs. Cooke and Sons, the workshops of the North Eastern Railway, the York glass works, and some extensive confectionery works. Naturalists will be glad to learn that the county possesses a fauna which comprises 513 out of the 717 British Vertebrata, viz. 46 mammals, 307 birds, 12 reptiles, and 148 fishes. It also furnishes 71 per cent. of the British flowering-plants and ferns. Geologically the county consists of rounded Chalk Hills, Oolite overlying the Lias, Trias covered with glacial drift and alluvial deposit, and a narrow band of Permian strata. Many opportunities will be afforded to members of studying the geology of the district.

The famous Kirkdale Cave, which was the first to be scientifically examined, gave rise to the Yorkshire Philosophical Society. The numerous remains found in it became the basis of a museum, and to it was attached the scientific society of which John Phillips was one of the secretaries. The idea of the Association was broached by Brewster in a letter to Phillips. The Council of the Yorkshire Society issued the first invitations, and its president, vice-president, treasurer, and secretaries filled the same offices at the first meeting of the Association. The writer of an able article in the *Times* of last Friday points out that in place of the few philosophical societies of fifty years ago there are now a hundred or two scattered all over the country often doing good work, which is to a great extent lost or wasted because inaccessible to the scientific world, and he suggests that the Association should act as a bond of union between these societies, proposing methods of work and special kinds of research suitable to the particular district. This might surely be done with great advantage in the case of the natural history sciences and geology; and we think the idea is worthy the attention of the Association. If, furthermore, it could publish a *résumé* of the more important results obtained by the several local societies during each year, it would be a boon to scientific literature.

As might have been expected, the "Jubilee Meeting" of the Association is likely to attract an unusually large gathering. On Tuesday upwards of 1500 names had been enrolled. The special character of the meeting is likely to have an influence not only on the presidential addresses, but on the nature of the entire proceedings.

INAUGURAL ADDRESS BY SIR JOHN LUBBOCK, BART., M.P., F.R.S., D.C.L., LL.D., PRESIDENT

In the name of the British Association, which for the time I very unworthily represent, I beg to tender to you, my Lord Mayor, and through you to the City of York, our cordial thanks for your hospitable invitation and hearty welcome.

We feel, indeed, that in coming to York we are coming home: gratefully as we acknowledge and much as we appreciate the kindness we have experienced elsewhere, and the friendly relations which exist between this Association and most—I might even say, all—our great cities, yet Sir R. Murchison truly observed at the close of our first meeting in 1831, that to York, "as the cradle of the Association, we shall ever look back with gratitude; and whether we meet hereafter on the banks of the Isis, the Cam, or the Forth, to this spot we shall still fondly revert." Indeed, it would have been a matter of much regret to all of us, if we had not been able on this, our fiftieth anniversary, to hold our meeting in our mother city.

My Lord Mayor, before going further, I must express my regret, especially when I call to mind the illustrious men who have preceded me in this chair, that it has not fallen to one of my eminent friends around me, to preside on this auspicious occasion. Conscious, however, as I am of my own deficiencies, I feel that I must not waste time in dwelling on them, more especially as in doing so I should but give them greater prominence. I will, therefore, only make one earnest appeal to your kind indulgence.

The connection of the British Association with the City of York does not depend merely on the fact that our first meeting was held here. It originated in a letter addressed by Sir D. Brewster to Prof. Phillips, as Secretary to your York Philosophical Society, by whom the idea was warmly taken up. The first meeting was held on September 26, 1831, the chair being taken by Lord Milton, who delivered an address, after which Mr. William Vernon Harcourt, Chairman of the Committee of Management, submitted to the meeting a code of rules which had been so maturely considered, and so wisely framed, that they have remained substantially the same down to the present day.

The constitution and objects of the Association were so ably described by Mr. Spottiswoode, at Dublin, and are so well known to you, that I will not dwell on them this evening. The excellent President of the Royal Society, in the same address, suggested that the past history of the Association would form an appropriate theme for the present meeting. The history of the Association, however, is really the history of science, and I long shrank from the attempt to give even a panoramic survey of a subject so vast and so difficult; nor should I have ventured to make any such attempt, but that I knew I could rely on the assistance of friends in every department of science.

Certainly, however, this is an opportunity on which it may be well for us to consider what have been the principal scientific results of the last half-century, dwelling especially on those with which this Association is more directly concerned, either as being the work of our own members, or as having been made known at our meetings. It is of course impossible within the limits of a single address to do more than allude to a few of these, and that very briefly. In dealing with so large a subject I first hoped that I might take our annual volumes as a text-book. This, however, I at once found to be quite impossible. For instance, the first volume commences with a Report on Astronomy by Sir G. Airy; I may be pardoned, I trust, for expressing my pleasure at finding that the second was one by my father, on the Tides, prepared like the preceding at the request of the Council; then comes one on Meteorology by Forbes, Radiant Heat by Baden Powell, Optics by Brewster, Mineralogy by Whewell, and so on. My best course will therefore be to take our different Sections one by one, and endeavour to bring before you a few of the principal results which have been obtained in each department.

The Biological Section is that with which I have been most intimately associated, and with which it is, perhaps, natural that I should begin.

Fifty years ago it was the general opinion that animals and plants came into existence just as we now see them. We took pleasure in their beauty; their adaptation to their habits and mode of life in many cases could not be overlooked or misunderstood. Nevertheless, the book of Nature was like some richly illuminated missal, written in an unknown tongue; the graceful forms of the letters, the beauty of the colouring, excited our wonder and admiration; but of the true meaning little was known to us; indeed we scarcely realised that there was any meaning to decipher. Now glimpses of the truth are gradually revealing themselves; we perceive that there is a reason—and in many cases we know what that reason is—for every difference in form, in size, and in colour; for every bone and every feather, almost for every hair. Moreover, each problem which is solved opens out vistas, as it were, of others perhaps even more interesting. With this great change the name of our illustrious countryman, Darwin, is intimately associated, and the year 1859 will always be memorable in science as having produced his great work on "The Origin of Species." In the previous year he and Wallace had published short papers, in which they clearly state the theory of natural selection, at which they had simultaneously and independently arrived. We cannot wonder that Darwin's views should have at first excited great opposition. Nevertheless from the first they met with powerful support, especially, in this country, from Hooker, Huxley, and Herbert Spencer. The theory is based on four axioms:—

"1. That no two animals or plants in nature are identical in all respects.

"2. That the offspring tend to inherit the peculiarities of their parents.

"3. That of those which come into existence, only a small number reach maturity.

"4. That those, which are, on the whole, best adapted to the circumstances in which they are placed, are most likely to leave descendants."

Darwin commenced his work by discussing the causes and extent of variability in animals, and the origin of domestic varieties; he showed the impossibility of distinguishing between varieties and species, and pointed out the wide differences which man has produced in some cases—as, for instance, in our domestic pigeons, all unquestionably descended from a common stock. He dwelt on the struggle for existence (which has since become a household word), and which, inevitably resulting in the survival of the fittest, tends gradually to adapt any race of animals to the conditions in which it occurs.

While thus, however, showing the great importance of natural selection, he attributed to it no exclusive influence, but fully admitted that other causes—the use and disuse of organs, sexual selection, &c.—had to be taken into consideration. Passing on to the difficulties of his theory he accounted for the absence of intermediate varieties between species, to a great extent, by the imperfection of the geological record.

But if the geological record be imperfect, it is still very instructive. The further palæontology has progressed the more it has tended to fill up the gaps between existing groups and species, while the careful study of living forms has brought into prominence the variations dependent on food, climate, habitat, and other conditions, and shown that many species long supposed to be absolutely distinct are so closely linked together by intermediate forms that it is difficult to draw a satisfactory line between them.

The principles of classification point also in the same direction, and are based more and more on the theory of descent. Biologists endeavour to arrange animals on what is called the "natural system." No one now places whales among fish, bats among birds, or shrews with mice, notwithstanding their external similarity; and Darwin maintained that "community of descent was the hidden bond which naturalists had been unconsciously seeking." How else, indeed, can we explain the fact that the framework of bones is so similar in the arm of a man, the wing of a bat, the fore-leg of a horse, and the fin of a porpoise—that the neck of a giraffe and that of an elephant contain the same number of vertebræ?

Strong evidence is, moreover, afforded by embryology; by the presence of rudimentary organs and transient characters, as, for instance, the existence in the calf of certain teeth which never cut the gums, the shrivelled and useless wings of some beetles, the presence of a series of arteries in the embryos of the higher Vertebrata exactly similar to those which supply the gills in fishes, even the spots on the young blackbird, the stripes on the lion's cub; these, and innumerable other facts of the same character, appear to be incompatible with the idea that each species was specially and independently created; and to prove, on the contrary, that the embryonic stages of species show us more or less clearly the structure of their ancestors.

Darwin's views, however, are still much misunderstood. I believe there are thousands who consider that according to his theory a sheep might turn into a cow, or a zebra into a horse. No one would more confidently withstand any such hypothesis, his view being, of course, not that the one could be changed into the other, but that both are descended from a common ancestor.

No one, at any rate, will question the immense impulse which Darwin has given to the study of natural history, the number of new views he has opened up, and the additional interest which he has aroused in, and contributed to, Biology. When we were young we knew that the leopard had spots, the tiger was striped, and the lion tawny; but why this was so it did not occur to us to ask; and if we had asked no one would have answered. Now we see at a glance that the stripes of the tiger have reference to its life among jungle-grasses; the lion is sandy, like the desert; while the markings of the leopard resemble spots of sunshine glancing through the leaves.

The science of embryology may almost be said to have been created in the last half-century. Fifty years ago it was a very general opinion that animals which are unlike when mature, were dissimilar from the beginning. It is to Von Baer, the discoverer of the mammalian ovum, that we owe the great generalisation that the development of the egg is in the main a progress from the general to the special, in fact, that embryology is the key to the laws of animal development.

Thus the young of existing species resemble in many cases the mature forms which flourished in ancient times. Huxley has traced up the genealogy of the horse to the Miocene Anchitherium. In the same way Gaudry has called attention to the fact that just as the individual stag gradually acquires more and more complex antlers: having at first only a single prong, in the next year two points, in the following three, and so on; so the genus, as a whole, in Middle Miocene times, had two pronged horns; in the Upper Miocene, three; and that it is not till the Upper Pliocene that we find any species with the magnificent antlers of our modern deer. It seems to be now generally admitted that birds have come down to us through the Dinosaurians, and, as Huxley has shown, the profound break once supposed to exist between birds and reptiles has been bridged over by the discovery of reptilian birds and bird-like reptiles; so that, in fact, birds are modified reptiles. Again, the remarkable genus Peripatus, so well studied by Moseley, tends to connect the annulose and articulate types.

Again, the structural resemblances between Amphioxus and the Ascidians had been pointed out by Goodsir; and Kowalevsky in 1866 showed that these were not mere analogies, but indicated a real affinity. These observations, in the words of Allen Thomson, "have produced a change little short of revolutionary in embryological and zoological views, leading as they do to the support of the hypothesis that the Ascidian is an earlier stage in the phylogenetic history of the mammal and other vertebrates."

The larval forms which occur in so many groups, and of which the Insects afford us the most familiar examples, are, in the words of Quatrefages, embryos, which lead an independent life. In such cases as these external conditions act upon the larvæ as they do upon the mature form; hence we have two classes of changes, adaptational or adaptive, and developmental. These and many other facts must be taken into consideration; nevertheless naturalists are now generally agreed that embryological characters are of high value as guides in classification, and it may, I think, be regarded as well-established that, just as the contents and sequence of rocks teach us the past history of the earth, so is the gradual development of the species indicated by the structure of the embryo and its developmental changes.

When the supporters of Darwin are told that his theory is incredible, they may fairly ask why it is impossible that a species in the course of hundreds of thousands of years should have passed through changes which occupy only a few days or weeks in the life-history of each individual?

The phenomena of yolk-segmentation, first observed by Prevost and Dumas, are now known to be in some form or other invariably the precursors of embryonic development; while they reproduce, as the first stages in the formation of the higher animals, the main and essential features in the life-history of the lowest forms. The "blastoderm" as it is called, or first germ of the embryo in the egg, divides itself into two layers, corresponding, as Huxley has shown, to the two layers into which the body of the Cœlenterata may be divided. Not only so, but most embryos at an early stage of development have the form of a cup, the walls of which are formed by the two layers of the blastoderm. Kowalevsky was the first to show the prevalence of this embryonic form, and subsequently Lankester and Hæckel put forward the hypothesis that it was the embryonic repetition of an ancestral type, from which all the higher forms are descended. The cavity of the cup is supposed to be the stomach of this simple organism, and the opening of the cup the mouth. The inner layer of the wall of the cup constitutes the digestive membrane, and the outer the skin. To this form Hæckel gave the name Gastræa. It is perhaps doubtful whether the theory of Lankester and Hæckel can be accepted in precisely the form they propounded it; but it has had an important influence on the progress of embryology. I cannot quit the science of embryology without alluding to the very admirable work on "Comparative Embryology" by our new general secretary, Mr. Balfour, and also the "Elements of Embryology" which he had previously published in conjunction with Dr. M. Foster.

In 1842, Steenstrup published his celebrated work on the "Alternation of Generations," in which he showed that many species are represented by two perfectly distinct types or broods, differing in form, structure, and habits; that in one of them males are entirely wanting, and that the reproduction is effected by fission, or by buds, which, however, are in some cases structurally indistinguishable from eggs. Steenstrup's illustrations were mainly taken from marine or parasitic species, of very great interest, but not generally familiar, excepting to naturalists. It has since been shown that the common Cynips or Gallfly is also a case in point. It had long been known that in some genera belonging to this group, males are entirely wanting, and it has now been shown by Bassett, and more thoroughly by Adler, that some of these species are double-brooded; the two broods having been considered as distinct genera.

Thus an insect known as *Neuroterus lenticularis*, of which females only occur, produces the familiar oak-spangles so common on the under sides of oak leaves, from which emerge, not *Neuroterus lenticularis*, but an insect hitherto considered as a distinct species, belonging even to a different genus, *Spathegaster baccarum*. In Spathegaster both sexes occur; they produce the currant-like galls found on oaks, and from these galls Neuroterus is again developed. So also the King Charles oak-apples produce a species known as *Teras terminalis*, which descends to the ground, and makes small galls on the roots of the oak. From these emerge an insect known as *Biorhiza aptera*, which again gives rise to the common oak-apple.

It might seem that such inquiries as these could hardly have any practical bearing. Yet it is not improbable that they may lead to very important results. For instance, it would appear that the fluke which produces the rot in sheep, passes one phase of its existence in the black slug, and we are not without hopes that the researches, in which our lamented friend Prof. Rolleston was engaged at the time of his death, which we all so much deplore, will lead, if not to the extirpation, at any rate to the diminution, of a pest from which our farmers have so grievously suffered.

It was in the year 1839 that Schwann and Schleiden demonstrated the intimate relation in which animals and plants stand to each other, by showing the identity of the laws of development of the elementary parts in the two kingdoms of organic nature.

As regards descriptive biology, by far the greater number of species now recorded have beeen named and described within the last half-century.

Dr. Günther has been good enough to make a calculation for me. The numbers, of course, are only approximate, but it appears that while the total number of animals described up to 1831 was not more than 70,000, the number now is at least 320,000.

Lastly, to show how large a field still remains for exploration, I may add that Mr. Waterhouse estimates that the British Museum alone contains not fewer than 12,000 species of insects which have not yet been described, while our collections do not probably contain anything like one-half of those actually in existence. Further than this, the anatomy and habits even of those which have been described offer an inexhaustible field for research, and it is not going too far to say that there is not a single species which would not amply repay the devotion of a lifetime.

One remarkable feature in the modern progress of biological science has been the application of improved methods of observation and experiment; and the employment in physiological research of the exact measurements employed by the experimental physicist. Our microscopes have been greatly improved. The use of chemical re-agents in microscopical investigations has proved most instructive, and another very important method of investigation has been the power of obtaining very thin slices by imbedding the object to be examined in paraffin or some other soft substance. In this manner we can now obtain, say, fifty separate sections of the egg of a beetle, or the brain of a bee.

At the close of the last century, Sprengel published a most suggestive work on flowers, in which he pointed out the curious relation existing between these and insects, and showed that the latter carry the pollen from flower to flower. His observations, however, attracted little notice until Darwin called attention to the subject in 1862. It had long been known that the cowslip and primrose exist under two forms, about equally numerous, and differing from one another in the arrangements of their stamens and pistils; the one form having the stamens on the summit of the flower and the stigma half-way down; while in the other the relative positions are reversed, the stigma being at the summit of the tube and the stamens half-way down. This difference had, however, been regarded as a case of mere variability; but Darwin showed it to be a beautiful provision, the result of which is that insects fertilise each flower with pollen brought from a different plant; and he proved that flowers fertilised with pollen from the other form yield more seed than if fertilised with pollen of the same form, even if taken from a different plant.

Attention having been thus directed to the question, an astonishing variety of most beautiful contrivances have been observed and described by many botanists, especially Hooker, Axel, Delpino, Hildebrand, Bennett, Fritz Müller, and above all Hermann Müller and Darwin himself. The general result is that to insects, and especially to bees, we owe the beauty of our gardens, the sweetness of our fields. To their beneficent, though unconscious action, flowers owe their scent and colour, their honey—nay, in many cases, even their form. Their present shape and varied arrangements, their brilliant colours, their honey, and their sweet scent are all due to the selection exercised by insects.

In these cases the relation between plants and insects is one of mutual advantage. In many species, however, plants present us with complex arrangements adapted to protect them from insects; such, for instance, are in many cases the resinous glands which render leaves unpalatable; the thickets of hairs and other precautions which prevent flowers from being robbed of their honey by ants. Again, more than a century ago, our countryman, Ellis, described an American plant, Dionæa, in which the leaves are somewhat concave, with long lateral spines and a joint in the middle; close up with a jerk, like a rat-trap, the moment any unwary insect alights on them. The plant, in fact, actually captures and devours insects. This observation also remained as an isolated fact until within the last few years, when Darwin, Hooker, and others have shown that many other species have curious and very varied contrivances for supplying themselves with animal food.

Some of the most fascinating branches of botany—morphology, histology, and physiology—scarcely existed before 1830. In the two former branches the discoveries of von Mohl are pre-eminent. He first observed cell-division in 1835, and detected the presence of starch in chlorophyll-corpuscles in 1837, while he first described protoplasm, now so familiar to us, at least by name, in 1846. In the same year Amici discovered the existence of the embryonic vesicle in the embryo sac, which develops into the embryo when fertilised by the entrance of the pollen-tube into the micropyle. The existence of sexual reproduction in the lower plants was doubtful, or at least doubted by some eminent authorities, as recently as 1853, when the actual process of fertilisation in the common bladderwrack of our shores was observed by Thuret, while the reproduction of the larger fungi was first worked out by De Bary in 1863.

As regards lichens, Schwendener proposed, in 1869, the startling theory, now however accepted by some of the highest authorities, that lichens are not autonomous organisms, but commensal associations of a fungus parasitic on an alga. With reference to the higher Cryptogams it is hardly too much to say that the whole of our exact knowledge of their life-history has been obtained during the last half-century. Thus in the case of ferns the male organs, or antheridia, were first discovered by Nägeli in 1844, and the archegonia, or female organs, by Suminski in 1848. The early stages in the development of mosses were worked out by Valentine in 1833. Lastly, the principle of Alternation of Generations in plants was discovered by Hofmeister. This eminent naturalist also, in 1851-4, pointed out the homologies of the reproductive processes in mosses, vascular cryptogams, gymnosperms, and angiosperms.

Nothing could have appeared less likely than that researches into the theory of spontaneous generation should have led to practical improvements in medical science. Yet such has been the case. Only a few years ago Bacteria seemed mere scientific curiosities. It had long been known that an infusion—say, of hay—would, if exposed to the atmosphere, be found, after a certain time, to teem with living forms. Even those few who still believe that life would be spotaneously generated in such an infusion, will admit that these minute organisms are, if not entirely, yet mainly, derived from germs floating in our atmosphere; and if precautions are taken to exclude such germs, as in the careful experiments especially of Pasteur, Tyndall, and Roberts, every one will grant that in ninety-nine cases out of a hundred no such development of life will take place.

These facts have led to most important results in Surgery. One reason why compound fractures are so dangerous, is because, the skin being broken, the air obtains access to the wound, bringing with it innumerable germs, which too often set up putrefying action. Lister first made a practical application of these observations. He set himself to find some substance capable of killing the germs, without being itself too potent a caustic, and he found that dilute carbolic acid fulfilled these conditions. This discovery has enabled many operations to be performed which would previously have been almost hopeless.

The same idea seems destined to prove as useful in Medicine as in Surgery. There is great reason to suppose that many diseases, especially those of a zymotic character, have their origin in the germs of special organisms. We know that fevers run a certain definite course. The parasitic organisms are at first few, but gradually multiply at the expense of the patient, and then die out again. Indeed, it seems to be thoroughly established that many diseases are due to the excessive multiplication of microscopic organisms, and we are not without hope that means will be discovered by which, without injury to the patient, these terrible, though minute, enemies may be destroyed, and the disease thus stayed. The interesting researches of Burdon Sanderson, Greenfield, Koch, Pasteur, Toussaint, and others, seem to justify the hope that we may be able to modify these and other germs, and then by appropriate inoculation to protect ourselves against fever and other acute diseases.

The history of Anæsthetics is a most remarkable illustration of how long we may be on the very verge of a most important discovery. Ether, which, as we all know, produces perfect insensibility to pain, was discovered as long ago as 1540. The anæsthetic property of nitrous oxide, now so extensively used, was observed in 1800 by Sir H. Davy, who actually experimented on himself, and had one of his teeth painlessly extracted when under its influence. He even suggests that "as nitrous oxide gas seems capable of destroying pain, it could probably be used with advantage in surgical operations." Nay, this property of nitrous oxide was habitually explained and illustrated in the chemical lectures given in hospitals, and yet for fifty years the gas was never used in actual operations.

Few branches of science have made more rapid progress in the last half-century than that which deals with the ancient condition of man. When our Association was founded it was generally considered that the human race suddenly appeared on the scene, about 6000 years ago, after the disappearance of the extinct mammalia, and when Europe, both as regards physical conditions and the other animals by which it was inhabited, was pretty much in the same condition as in the period covered by Greek and Roman history. Since then the persevering researches of Layard, Rawlinson, Botta and others have made known to us, not only the statues and palaces of the ancient Assyrian monarchs, but even their libraries; the cuneiform characters have been deciphered, and we can not only see, but read in the British Museum, the actual contemporary records, on burnt clay cylinders, of the events recorded in the historical books of the Old Testament and in the pages of Herodotus. The researches in Egypt also seem to have satisfactorily established the fact that the pyramids themselves are at least 6000 years old, while it is obvious that the Assyrian and Egyptian monarchies cannot suddenly have attained to the wealth and power, the state of social organisation, and progress in the arts, of which we have before us, preserved by the sand of the desert from the ravages of man, such wonderful proofs.

In Europe, the writings of the earliest historians and poets indicated that, before iron came into general use, there was a time when bronze was the ordinary material of weapons, axes, and other cutting implements, and though it seemed *à priori* improbable that a compound of copper and tin should have preceded the simple metal iron, nevertheless the researches of archæologists have shown that there really was in Europe a "Bronze Age," which at the dawn of history was just giving way to that of "Iron."

The contents of ancient graves, buried in many cases so that their owner might carry some at least of his wealth with him to the world of spirits, left no room for doubt as to the existence of a Bronze Age; but we get a completer idea of the condition of Man at this period from the Swiss lake-villages, first made known to us by Keller. Along the shallow edges of the Swiss lakes there flourished, once upon a time, many populous villages or towns, built on platforms supported by piles, exactly as many Malayan villages are now. Under these circumstances innumerable objects were one by one dropped into the water; sometimes whole villages were burnt, and their contents submerged; and thus we have been able to recover, from the waters of oblivion in which they had rested for more than 2000 years, not only the arms and tools of this ancient people, the bones of their animals, their pottery and ornaments, but the stuffs they wore, the grain they had stored up for future use, even fruits and cakes of bread.

But this bronze-using people were not the earliest occupants of Europe. The contents of ancient tombs give evidence of a time when metal was unknown. This also was confirmed by the evidence then unexpectedly received from the Swiss lakes. By the side of the bronze-age villages were others, not less extensive, in which, while implements of stone and bone were discovered literally by thousands, not a trace of metal was met with. The shell-mounds or refuse-heaps accumulated by the ancient fishermen along the shores of Denmark, fully confirmed the existence of a "Stone Age."

No bones of the reindeer, no fragment of any of the extinct mammalia, have been found in any of the Swiss lake-villages or in any of the thousands of tumuli which have been opened in our own country or in Central and Southern Europe. Yet the contents of caves and of river-gravels afford abundant evidence that there was a time when the mammoth and rhinoceros, the musk-ox and reindeer, the cave lion and hyena, the great bear and the gigantic Irish elk wandered in our woods and valleys, and the hippopotamus floated in our rivers; when England and France were united, and the Thames and the Rhine had a common estuary. This was long supposed to be before the advent of Man. At length, however, the discoveries of Boucher de Perthes in the valley of the Somme, supported as they are by the researches of many Continental naturalists, and in our own country of MacEnery and Godwin-Austen, Prestwich and Lyell, Vivian and Pengelly, Christy, Evans, and many more, have proved that Man formed a humble part of this strange assembly.

Nay, even at this early period there were at least two distinct races of men in Europe; one of them—as Boyd Dawkins has pointed out—closely resembling the modern Esquimaux in form, in his weapons and implements, probably in his clothing, as well as in so many of the animals with which he was associated.

At this stage Man appears to have been ignorant of pottery, to have had no knowledge of agriculture, no domestic animals, except perhaps the dog. His weapons were the axe, the spear, and the javelin; I do not believe he knew the use of the bow, though he was probably acquainted with the lance. He was, of course, ignorant of metal, and his stone implements, though skilfully formed, were of quite different shapes from those of the second Stone age, and were never ground. This earlier Stone period, when man co-existed with these extinct mammalia, is known as the Palæolithic, or Early Stone Age, in opposition to the Neolithic, or Newer Stone Age.

The remains of the mammalia which co-existed with Man in pre-historic times have been most carefully studied by Owen, Lartet, Rütimeyer, Falconer, Busk, Boyd Dawkins, and others. The presence of the mammoth, the reindeer, and especially of the musk-ox, indicates a severe, not to say an arctic, climate, the existence of which, moreover, was proved by other considerations; while, on the contrary, the hippopotamus requires considerable warmth. How then is this association to be explained?

While the climate of the globe is, no doubt, much affected by geographical conditions, the cold of the glacial period was, I believe, mainly due to the excentricity of the earth's orbit, combined with the obliquity of the ecliptic. The result of the latter condition is a period of 21,000 years, during one half of which the northern hemisphere is warmer than the southern, while during the other 10,500 years the reverse is the case. At present we are in the former phase, and there is, we know, a vast accumulation of ice at the south pole. But when the earth's orbit is nearly circular, as it is at present, the difference between the two hemispheres is not very great; on the contrary, as the excentricity of the orbit increases the contrast between them increases also. This excentricity is continually oscillating within certain limits, which Croll and subsequently Stone have calculated out for the last million years. At present the excentricity is ·016 and the mean temperature of the coldest month in London is about 40°. Such has been the state of things for nearly 100,000 years; but before that there was a period, beginning 300,000 years ago, when the excentricity of the orbit varied from ·26 to ·57. The result of this would be greatly to increase the effect due to the obliquity of the orbit; at certain periods the climate would be much warmer than at present, while at others the number of days in winter would be twenty more, and of summer twenty less than now, while the mean temperature of the coldest month would be lowered 20°. We thus get something like a date for the last glacial epoch, and we see that it was not simply a period of cold, but rather one of extremes, each beat of the pendulum of temperature lasting for no less than 21,000 years. This explains the fact that, as Morlot showed in 1854, the glacial deposits of Switzerland, and, as we now know, those of Scotland, are not a single uniform layer, but a succession of strata indicating very different conditions. I agree also with Croll and Geikie in thinking that these considerations explain the apparent anomaly of the co-existence in the same gravels of arctic and tropical animals; the former having lived in the cold, while the latter flourished in the hot, periods.

It is, I think, now well established that man inhabited Europe during the milder periods of the glacial epoch. Some high authorities indeed consider that we have evidence of his presence in pre-glacial and even in Miocene times, but I confess that I am not satisfied on this point. Even the more recent period carries back the record of man's existence to a distance so great as altogether to change our views of ancient history.

Nor is it only as regards the antiquity and material condition of man in prehistoric times that great progress has been made. If time permitted I should have been glad to have dwelt on the origin and development of language, of custom, and of law. On all of these the comparison of the various lower races still inhabiting so large a portion of the earth's surface, has thrown much light; while even in the most cultivated nations we find survivals, curious fancies, and lingering ideas; the fossil remains as it were of former customs and religions embedded in our modern civilisation, like the relics of extinct animals in the crust of the earth.

In Geology the formation of our Association coincided with the appearance of Lyell's "Principles of Geology," the first volume of which was published in 1830, and the second in 1832. At that time the received opinion was that the phenomena of Geology could only be explained by violent periodical convulsions, and a high intensity of terrestrial energy culminating in repeated catastrophes. Hutton and Playfair had indeed maintained that such causes as those now in operation would, if only time enough were allowed, account for the geological structure of the earth; nevertheless the opposite view generally prevailed, until Lyell, with rare sagacity and great eloquence, with a wealth of illustration and most powerful reasoning, convinced geologists that the forces now in action are powerful enough, if only time be given, to produce results quite as stupendous as those which science records.

As regards Stratigraphical Geology, at the time of the first meeting of the British Association at York, the strata between the Carboniferous Limestone and the Chalk had been mainly reduced to order and classified, chiefly through the labours of William Smith. But the classification of all the strata lying above the Chalk and below the Carboniferous Limestone respectively, remained in a state of the greatest confusion. The year 1831 marks the period of the commencement of the joint labours of Sedgwick and Murchison, which resulted in the establishment of the Cambrian, Silurian, and Devonian systems. Our pre-Cambrian strata have recently been divided by Hicks into four great groups of immense thickness, and implying, therefore, a great lapse of time; but no fossils have yet been discovered in them. Lyell's classification of the Tertiary deposits, the result of the studies which he carried on with the assistance of Deshayes and others, was published in the third volume of the "Principles of Geology" in 1833. The establishment of Lyell's divisions of Eocene, Miocene, and Pliocene, was the starting-point of a most important series of investigations by Prestwich and others of these younger deposits; as well as of the post-Tertiary, Quaternary, or drift beds, which are of special interest from the light they have thrown on the early history of Man.

As regards the physical character of the earth, two theories have been held: one, that of a fluid interior covered by a thin crust; the other, of a practically solid sphere. The former is now very generally admitted, both by astronomers and geologists, to be untenable. The prevailing feeling of geologists on this subject has been well expressed by Prof. Le Conte, who says, "the whole theory of igneous agencies—which is little less than the whole foundation of theoretic geology—must be reconstructed on the basis of a solid earth."

In 1837 Agassiz startled the scientific world by his "Discours sur l'ancienne extension des Glaciers," in which, developing the observation already made by Charpentier and Venetz, that boulders had been transported to great distances, and that rocks far away from, or high above, existing glaciers, are polished and scratched by the action of ice, he boldly asserted the existence of a "glacial period," during which Switzerland and the North of Europe were subjected to great cold and buried under a vast sheet of ice.

The ancient poets described certain gifted mortals as privileged to descend into the interior of the earth, and have exercised their imagination in recounting the wonders there revealed. As in other cases, however, the realities of science have proved more varied and surprising than the dreams of fiction. Of the gigantic and extraordinary animals thus revealed to us by far the greatest number have been described during the period now under review. For instance, the gigantic Cetiosaurus was described by Owen in 1838, the Dinornis of New Zealand by the same distinguished naturalist in 1839, the Mylodon in the same year, and the Archæopteryx in 1862.

In America, a large number of remarkable forms have been described, mainly by Marsh, Leidy, and Cope. Marsh has made known to us the Titanosaurus, of the American (Colorado) Jurassic beds, which is, perhaps, the largest land animal yet known, being a hundred feet in length, and at least thirty in height, though it seems possible that even these vast dimensions were exceeded by those of the Atlantosaurus. Nor must I omit the Hesperornis, described by Marsh in 1872, as a carnivorous, swimming ostrich, provided with teeth, which he regards as a character inherited from reptilian ancestors; the Ichthyornis, tranger still, with biconcave vertebræ, like those of fishes, and teeth set in sockets.

As giving, in a few words, an idea of the rapid progress in this department, I may mention that Morris's "Catalogue of British Fossils," published in 1843, contained 5300 species; while that now in preparation by Mr. Etheridge enumerates 15,000.

But if these figures show how rapid our recent progress has been, they also very forcibly illustrate the imperfection of the geological record, and give us, I will not say a measure, but an idea, of the imperfection of the geological record. The number of all the described recent species is over 300,000, but certainly not half are yet on our lists, and we may safely take the total number of recent species as being not less than 700,000. But in former times there have been at the very least twelve periods, in each of which by far the greater number of species were distinct. True, the number of species was probably not so large in the earlier periods as at present; but if we make a liberal allowance for this, we shall have a total of more than 2,000,000 species, of which about 25,000 only are as yet upon record; and many of these are only represented by a few, some only by a single specimen, or even only by a fragment.

The progress of palæontology may also be marked by the extent to which the existence of groups has been, if I may so say, carried back in time. Thus I believe that in 1830 the earliest known quadrupeds were small marsupials belonging to the Stonesfield slates; the most ancient mammal now known is *Microlestes antiquus* from the Keuper of Würtemberg: the oldest bird known in 1831 belonged to the period of the London Clay, the oldest now known is the Archæopteryx of the Solenhofen slates, though it is probable that some at any rate of the footsteps on the Triassic rocks are those of birds. So again the Amphibia have been carried back from the Trias to the Coal-measures; Fish from the Old Red Sandstone to the Upper Silurian; Reptiles to the Trias; Insects from the Cretaceous to the Devonian; Mollusca and Crustacea from the Silurian to the Lower Cambrian. The rocks below the Cambrian, though of immense thickness, have afforded no relics of animal life, if we except the problematical *Eozoon Canadense*, so ably studied by Dawson and Carpenter. But if palæontology as yet throws no light on the original forms of life, we must remember that the simplest and the lowest organisms are so soft and perishable that they would leave "not a wrack behind."

Passing to the science of Geography, Mr. Clements Markham has recently published an excellent summary of what has been accomplished during the half-century.

But the progress in our knowledge of geography is, and has been, by no means confined to the improvement of our maps, or to the discovery and description of new regions of the earth, but has extended to the causes which have led to the present configuration of the surface. To a great extent indeed this part of the subject falls rather within the scope of geology, but I may here refer, in illustration, to the distribution of lakes, the phenomena of glaciers, the formation of volcanic mountains, and the structure and distribution of coral islands.

The origin and distribution of lakes is one of the most interesting problems in physical geography. That they are not scattered at random, a glance at the map is sufficient to show. They abound in mountain districts, are comparatively rare in equatorial regions, increasing in number as we go north, so that in Scotland and the northern parts of America they are sown broadcast.

Perhaps *à priori* the first explanation of the origin of lakes which would suggest itself, would be that they were formed in hollows resulting from a disturbance of the strata, which had thrown them into a basin-shaped form. Lake-basins, however, of this character are, as a matter of fact, very rare; as a general rule lakes have not the form of basin-shaped synclinal hollows, but, on the contrary, the strike of the strata often runs right across them. My eminent predecessor, Prof. Ramsay, divides lakes into three classes:—(1) Those which are due to irregular accumulations of drift, and which are generally quite shallow. (2) Those which are formed by moraines, and (3) those which occupy true basins scooped by glacier-ice out of the solid rock. To the latter class belong most of the great Swiss and Italian lakes. Prof. Ramsay attributes their excavation to glaciers, because it is of course obvious that rivers cannot make basin-shaped hollows surrounded by rock on all sides. Now the Lake of Geneva, 1230 feet above the sea, is 984 feet deep, the Lake of Brienz is 1850 feet above the sea, and 2000 feet deep, so that its bottom is really below the sea-level. The Italian lakes are even more remarkable. The Lake of Como, 700 feet above the sea, is 1929 feet deep. Lago Maggiore, 685 feet above the sea, is no less than 2625 feet deep. It will be observed that these lakes, like many others in mountain regions, those of Scandinavia, for instance, lie in the direct channels of the great old glaciers. If the mind is at first staggered at the magnitude of the scale, we must remember that the ice which scooped out the valley in which the Lake of Geneva now reposes, was once at least 2700 feet thick; while the moraines were also of gigantic magnitude, that of Ivrea, for instance, being no less than 1500 feet in height. Prof. Ramsay's theory seems, therefore, to account beautifully for a large number of interesting facts.

Passing from lakes to mountains, two rival theories with reference to the structure and origin of volcanoes long struggled for supremacy.

The more general view was that the sheets of lava and scoriæ which form volcanic cones—such, for instance, as Ætna or Vesuvius—were originally nearly horizontal, and that subsequently a force operating from below, and exerting a pressure both upwards and outwards from a central axis towards all points of the compass, uplifted the whole stratified mass and made it assume a conical form, giving rise at the same time, in many cases, to a wide and deep circular opening at the top of the cone, called by the advocates of this hypothesis a "crater of elevation."

This theory, though, as it seems to us now, it had already received its death-blow from the admirable memoirs of Scrope, was yet that most generally adopted fifty years ago, because it was considered that compact and crystalline lavas could not have consolidated on a slope exceeding 1° or 2°. In 1858, however, Sir C. Lyell conclusively showed that in fact such lavas could consolidate at a considerable angle, even in some cases at more than 30°, and it is now generally admitted that though the beds of lava, &c., may have sustained a slight angular elevation since their deposition, still in the main, volcanic cones have acquired their form by the accumulation of lava and ashes ejected from one or more craters.

The problems presented by glaciers are of very great interest. In 1843 Agassiz and Forbes proved that the centre of a glacier, like that of a river, moves more rapidly than its sides. But how and why do glaciers move at all? Rendu, afterwards Bishop of Annecy, in 1841 endeavoured to explain the facts by supposing that glacier ice enjoys a kind of ductility. The "viscous theory" of glaciers was also adopted, and most ably advocated, by Forbes, who compared the condition of a glacier to that of the contents of a tar-barrel poured into a sloping channel. We have all, however, seen long narrow fissures, a mere fraction of an inch in width, stretching far across glaciers—a condition incompatible with the ordinary idea of viscosity. The phenomenon of regelation was afterwards applied to the explanation of glacier motion. An observation of Faraday's supplied the clue. He noticed in 1850 that when two pieces of thawing ice are placed together they unite by freezing at the place of contact. Following up this suggestion, Tyndall found that if he compressed a block of ice in a mould it could be made to assume any shape he pleased. A straight prism, for instance, placed in a groove and submitted to hydraulic pressure, was bent into a transparent semicircle of ice. These experiments seem to have proved that a glacial valley is a mould through which the ice is forced, and to which it will accommodate itself, while as Tyndall and Huxley also pointed out, the "veined structure of ice" is produced by pressure, in the same manner as the cleavage of slate rocks.

It was in the year 1842 that Darwin published his great work on "Coral Islands." The fringing reefs of coral presented no special difficulty. They could be obviously accounted for by an elevation of the land, so that the coral which had originally grown under water, had been raised above the sea-level. The circular or oval shape of so many reefs, however, each having a lagoon in the centre, closely surrounded by a deep ocean, and rising but a few feet above the sea-level, had long been a puzzle to the physical geographer. The favourite theory was that these were the summits of submarine volcanoes on which the coral had grown. But as the reef-making coral does not live at greater depths than about twenty-five fathoms, the immense number of these reefs formed an almost insuperable objection to this theory. The Laccadives and Maldives, for instance—meaning literally the "lac of islands' and the "thousand islands"—are a series of such atolls, and it was impossible to imagine so great a number of craters, all so nearly of the same altitude. Darwin showed, moreover, that so far from the ring of corals resting on a corresponding ridge of rock, the lagoons, on the contrary, now occupy the place which was once the highest land. He pointed out that some lagoons, as, for instance, that of Vanikoro, contain an island in the middle; while other islands, such as Tahiti, are surrounded by a margin of smooth water, separated from the ocean by a coral reef. Now, if we suppose that Tahiti were to sink slowly, it would gradually approximate to the condition of Vanikoro; and if Vanikoro gradually sank, the central island would disappear, while on the contrary the growth of the coral might neutralise the subsidence of the reef, so that we should have simply an atoll, with its lagoon. The same considerations explain the origin of the "barrier reefs," such as that which runs, for nearly one thousand miles, along the north-east coast of Australia. Thus Darwin's theory explained the form and the approximate identity of altitude of these coral islands. But it did more than this, because it showed us that there were great areas in process of subsidence, which though slow, was of great importance in physical geography.[1]

Much information has also been acquired with reference to the abysses of the ocean, especially from the voyages of the *Porcupine* and the *Challenger*. The greatest depth yet recorded is near the Ladrone Islands, where a sounding of 4575 fathoms was obtained.

Ehrenberg long ago pointed out the similarity of the calcareous mud now accumulating in our recent seas to the Chalk, and showed that the green sands of the geologist are largely made up of casts of foraminifera. Clay, however, had been looked on, until the recent expeditions, as essentially a product of the disintegration of older rocks. Not only, however, are a large proportion of siliceous and calcareous rocks either directly or indirectly derived from material which has once formed a portion of living organisms, but Sir Wyville Thomson maintains that this is the case with some clays also. In that case the striking remark of Linnæus, that "fossils are not the children but the parents of rocks," will have received remarkable confirmation. I should have thought it, I confess, probable that these clays are, to a considerable extent, composed of volcanic dust.

It would appear that calcareous deposits resembling our chalk do not occur at a greater depth than 3000 fathoms; they have not been met with in the abysses of the ocean. Here the bottom consists of exceedingly fine clay, sometimes coloured red by oxide of iron, sometimes chocolate by manganese oxide, and containing with Foraminifera occasionally large numbers of siliceous Radiolaria. These strata seem to accumulate with extreme slowness: this is inferred from the comparative abundance of whales' bones and fishes' teeth; and from the presence of minute spherical particles, supposed by Mr. Murray to be of cosmic origin—in fact, to be the dust of meteorites, which in the course of ages have fallen on the ocean. Such particles no doubt occur over the whole surface of the earth, but on land they soon oxidise, and in shallow water they are covered up by other deposits. Another interesting result of recent deep-sea explorations has been to show that the depths of the ocean are no mere barren solitudes, as was until recent years confidently believed, but, on the contrary, present us many remarkable forms of life. We have, however, as yet but thrown here and there a ray of light down into the ocean abysses:—

> "Nor can so short a time sufficient be,
> To fathom the vast depths of Nature's sea."

In Astronomy, the discovery in 1845 of the planet Neptune, made independently and almost simultaneously by Adams and by Le Verrier, was certainly one of the very greatest triumphs of mathematical genius. Of the minor planets four only were known in 1831, whilst the number now on the roll amounts to 220. Many astronomers believe in the existence of an intra-mercurial planet or planets, but this is still an open question. The Solar System has also been enriched by the discovery of an inner ring to Saturn, of satellites to Mars, and of additional satellites to Saturn, Uranus and Neptune.

The most unexpected progress, however, in our astronomical knowledge during the past half-century has been due to Spectrum Analysis.

The dark lines in the spectrum were first seen by Wollaston, who noticed a few of them; but they were independently discovered by Fraunhofer, after whom they are justly named, and who, in 1814, mapped no fewer than 576. The first steps in "spectrum analysis," properly so called, were made by Sir J. Herschel, Fox Talbot, and by Wheatstone, in a paper read before this Association in 1835. The latter showed that the spectrum emitted by the incandescent vapour of metals was formed of bright lines, and that these lines, while, as he then supposed, constant for each metal, differed for different metals. "We have here," he said, "a mode of discriminating metallic bodies more readily than that of chemical examination, and which may hereafter be employed for useful purposes." Nay, not only can bodies thus be more readily discriminated, but, as we now know, the presence of extremely minute portions can be detected, the $\frac{1}{6000000}$th of a grain being in some cases easily perceptible.

It is also easy to see that the presence of any new simple substance might be detected, and in this manner already several new elements have been discovered, as I shall mention when we come to Chemistry.

But spectrum analysis has led to even grander and more unexpected triumphs. Fraunhofer himself noticed the coincidence between the double dark line D of the solar spectrum and a double line which he observed in the spectra of ordinary flames, while Stokes pointed out to Sir W. Thomson, who taught it in his lectures, that in both cases these lines were due to the presence of sodium. To Kirchhoff and Bunsen, however, is due the independent conception and the credit of having first systematically investigated the relation which exists between Fraunhofer's lines and the bright lines in the spectra of incandescent metals. In order to get some fixed measure by which they might determine and record the lines characterising any given substance, it occurred to them that they might use for comparison the spectrum of the sun. They accordingly arranged their spectroscope so that one-half of the slit was lighted by the sun, and the other by the luminous gases they proposed to examine. It immediately struck them that the bright lines in the one corresponded with the dark lines in the other—the bright line of sodium, for instance, with the line or rather lines D in the sun's spectrum. The conclusion was obvious. There was sodium in the sun! It must indeed have been a glorious moment when that thought flashed across them, and even by itself well worth all their labour.

Kirchhoff and Bunsen thus proved the existence in the sun of hydrogen, sodium, magnesium, calcium, iron, nickel, chromium, manganese, titanium, and cobalt; since which Ångström, Thalèn, and Lockyer have considerably increased the list.

But it is not merely the chemistry of the heavenly bodies on which light is thrown by the spectroscope; their physical structure and evolutional history are also illuminated by this wonderful instrument of research.

It used to be supposed that the sun was a dark body enveloped in a luminous atmosphere. The reverse now appears to be the truth. The body of the sun, or photosphere, is intensely brilliant; round it lies the solar atmosphere of comparatively cool gases, which cause the dark lines in the spectrum; thirdly, a chromosphere,—a sphere principally of hydrogen, jets of which are said sometimes to reach to a height of 100,000 miles or more, into the outer coating or corona, the nature of which is still very doubtful.

Formerly the red flames which represent the higher regions of the chromosphere could be seen only on the rare occasions of a total solar eclipse. Janssen and Lockyer, by the application of the spectroscope, have enabled us to study this region of the sun at all times.

It is, moreover, obvious that the powerful engine of investigation afforded us by the spectroscope is by no means confined to the substances which form part of our system. The incandescent body can thus be examined, no matter how great its distance, so long only as the light is strong enough. That this method was theoretically applicable to the light of the stars was indeed obvious, but the practical difficulties were very great. Sirius, the brightest of all, is, in round numbers, a hundred millions of millions of miles from us; and, though as big as sixty of our suns, his light when it reaches us, after a journey of sixteen years, is at most one two-thousand-millionth part as bright. Nevertheless as long ago as 1815 Fraunhofer recognised the fixed lines in the light of four of the stars, and in 1863 Miller and Huggins in our own country, and Rutherford in America, succeeded in determining the dark lines in the spectrum of some of the brighter stars, thus showing that these beautiful and mysterious lights contain many of the material substances with

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[1] I ought to mention that Darwin's views have recently been questioned by Semper and Murray.

which we are familiar. In Aldebaran, for instance, we may infer the presence of hydrogen, sodium, magnesium, iron, calcium, tellurium, antimony, bismuth, and mercury; some of which are not yet known to occur in the sun. As might have been expected, the composition of the stars is not uniform, and it would appear that they may be arranged in a few well-marked classes, indicating differences of temperature, or in other words, of age. Some recent photographic spectra of stars obtained by Huggins go very far to justify this view.

Thus we can make the stars teach us their own composition with light which started from its source before we were born—light older than our Association itself.

But spectrum analysis has even more than this to tell us. The old methods of observation could determine the movements of the stars so far only as they were transverse to us; they afforded no means of measuring motion either directly towards or away from us. Now Döppler suggested in 1841 that the colours of the stars would assist us in this respect, because they would be affected by their motion to and from the earth, just as a steam-whistle is raised or lowered as it approaches or recedes from us. Every one has observed that if a train whistles as it passes us, the sound appears to alter at the moment the engine goes by. This arises, of course, not from any change in the whistle itself, but because the number of vibrations which reach the ear in a given time are increased by the speed of the train as it approaches, and diminished as it recedes. So like the sound, the colour would be affected by such a movement; but Döppler's method was practically inapplicable, because the amount of effect on the colour would be utterly insensible; and even if it were otherwise the method could not be applied, because, as we did not know the true colour of the stars, we have no datum line by which to measure.

A change of refrangibility of light, however, does occur in consequence of relative motion, and Huggins successfully applied the spectroscope to solve the problem. He took in the first place the spectroscope of Sirius, and chose a line known as F, which is due to hydrogen. Now, if Sirius was motionless, or rather if it retained a constant distance from the earth, the line F would occupy exactly the same position in the spectrum of Sirius, as in that of the sun. On the contrary, if Sirius were approaching or receding from us, this line would be slightly shifted either towards the blue or red end of the spectrum. He found that the line had moved very slightly towards the red, indicating that the distance between us and Sirius is increasing at the rate of about twenty miles a second. So also Betelgeux, Rigel, Castor, and Regulus are increasing their distance; while, on the contrary, that of others, as for instance of Vega, Arcturus, and Pollux, is diminishing. The results obtained by Huggins on about twenty stars have since been confirmed and extended by Mr. Christie, now Astronomer-Royal in succession to Sir G. Airy, who has long occupied the post with so much honour to himself and advantage to science.

To examine the spectrum of a shooting star would seem even more difficult; yet Alexander Herschel has succeeded in doing so, and finds that their nuclei are incandescent solid bodies; he has recognised the lines of potassium, sodium, lithium, and other substances, and considers that the shooting stars are bodies similar in character and composition to the stony masses which sometimes reach the earth as aërolites.

No element has yet been found in any meteorite, which was not previously known as existing in the earth, but the phenomena which they exhibit indicate that they must have been formed under conditions very different from those which prevail on the earth's surface. I may mention, for instance, the peculiar form of crystallised silica, called by Maskelyne, Asmanite; and the whole class of meteorites, consisting of iron generally alloyed with nickel, which Daubrée terms Holosiderites. The interesting discovery, however, by Nordenskjöld, in 1870, at Ovifak, of a number of blocks of iron alloyed with nickel and cobalt, in connection with basalts containing disseminated iron, has, in the words of Judd, "afforded a very important link, placing the terrestrial and extra-terrestrial rocks in closer relations with one another."

We have as yet no sufficient evidence to justify a conclusion as to whether any substances exist in the heavenly bodies which do not occur in our earth, though there are many lines which cannot yet be satisfactorily referred to any terrestrial element. On the other hand, some substances which occur on our earth have not yet been detected in the sun's atmosphere.

Such discoveries as these seemed, not long ago, entirely beyond our hopes. M. Comte, indeed, in his "Cours de Philosophie Positive," as recently as 1842, laid it down as an axiom regarding the heavenly bodies, that "Nous concevons la possibilité de déterminer leurs formes, leurs distances, leurs grandeurs et leurs mouvements, tandis que nous ne saurions jamais étudier par aucun moyen leur composition chimique ou leur structure minéralogique." Yet within a few years this supposed impossibility has been actually accomplished, showing how unsafe it is to limit the possibilities of science.

It is hardly necessary to point out that, while the spectrum has taught us so much, we have still even more to learn. Why should some substances give few, and others many, lines? Why should the same substance give different lines at different temperatures? What are the relations between the lines and the physical or chemical properties?

We may certainly look for much new knowledge of the hidden actions of atoms and molecules from future researches with the spectroscope. It may even, perhaps, teach us to modify our views of the so-called simple substances. Prout long ago, struck by the remarkable fact that nearly all atomic weights are simple multiples of the atomic weight of hydrogen, suggested that hydrogen must be the primordial substance. Brodie's researches also naturally fell in with the supposition that the so-called simple substances are in reality complex, and that their constituents occur separately in the hottest regions of the solar atmosphere. Lockyer considers that his researches lend great probability to this view. The whole subject is one of intense interest, and we may rejoice that it is occupying the attention, not only of such men as Abney, Dewar, Hartley, Liveing, Roscoe and Schuster in our own country, but also of many foreign observers.

When geology so greatly extended our ideas of past time, the continued heat of the sun became a question of greater interest than ever. Helmholtz has shown that, while adopting the nebular hypothesis, we need not assume that the nebulous matter was originally incandescent; but that its present high temperature may be, and probably is, mainly due to gravitation between its parts. It follows that the potential energy of the sun is far from exhausted, and that with continued shrinking it will continue to give out light and heat, with little, if any, diminution for several millions of years.

Like the sand of the sea, the stars of heaven have ever been used as effective symbols of number, and the improvements in our methods of observation have added fresh force to our original impressions. We now know that our earth is but a fraction of one out of at least 75,000,000 worlds.

But this is not all. In addition to the luminous heavenly bodies, we cannot doubt that there are countless others, invisible to us from their greater distance, smaller size, or feebler light; indeed we know that there are many dark bodies which now emit no light or comparatively little. Thus in the case of Procyon, the existence of an invisible body is proved by the movement of the visible star. Again I may refer to the curious phenomena presented by Algol, a bright star in the head of Medusa. This star shines without change for two days and thirteen hours; then, in three hours and a half, dwindles from a star of the second to one of the fourth magnitude; and then, in another three and a half hours, reassumes its original brilliancy. These changes seem certainly to indicate the presence of an opaque body, which intercepts at regular intervals a part of the light emitted by Algol.

Thus the floor of heaven is not only "thick inlaid with patines of bright gold," but studded also with extinct stars; once probably as brilliant as our own sun, but now dead and cold, as Helmholtz tells us that our sun itself will be, some seventeen millions of years hence.

The general result of astronomical researches has been thus eloquently summed up by Proctor:—"The sidereal system is altogether more complicated and more varied in structure than has hitherto been supposed; in the same region of the stellar depths co-exist stars of many orders of real magnitude; all orders of nebulæ, gaseous or stellar, planetary, ring-formed, elliptical, and spiral, exist within the limits of the galaxy; and lastly, the whole system is alive with movements, the laws of which may one day be recognised, though at present they appear too complex to be understood."

We can, I think, scarcely claim the establishment of the undulatory theory of light as falling within the last fifty years; for though Brewster, in his "Report on Optics," published in our first volume, treats the question as open, and expresses himself still unconvinced, he was, I believe, almost alone in his preference for the emission theory. The phenomena of interference, in fact, left hardly any—if any—room for doubt, and the subject was finally set at rest by Foucault's celebrated experiments in 1850. According to the undulatory theory the velocity of light ought to be greater in air than in water, while if the emission theory were correct the reverse would be the case. The velocity of light—186,000 miles in a second—is, however, so great that, to determine its rate in air, as compared with that in water, might seem almost hopeless. The velocity in air was, nevertheless, determined by Fizeau in 1849, by means of a rapidly revolving wheel. In the following year Foucault, by means of a revolving mirror, demonstrated that the velocity of light is greater in air than in water—thus completing the evidence in favour of the undulatory theory of light.

The idea is now gaining ground, that, as maintained by Clerk-Maxwell, light itself is an electro-magnetic disturbance, the luminiferous ether being the vehicle of both light and electricity.

Wünsch, as long ago as 1792, had clearly shown that the three primary colours were red, green, and violet; but his results attracted little notice, and the general view used to be that there were seven principal colours—red, orange, yellow, green, blue, indigo, and violet; four of which—namely orange, green, indigo, and violet—were considered to arise from mixtures of the other three. Red, yellow, and blue were therefore called the primary colours, and it was supposed that in order to produce white light these three colours must always be present.

Helmholtz, however, again showed, in 1852, that a colour to our unaided eyes identical with white, was produced by combining yellow with indigo. At that time yellow was considered to be a simple colour, and this, therefore, was regarded as an exception to the general rule, that a combination of three simple colours is required to produce white. Again, it was, and indeed still is, the general impression that a combination of blue and yellow makes green. This, however, is entirely a mistake. Of course we all know that yellow paint and blue paint make green paint; but this results from absorption of light by the semi-transparent solid particles of the pigments, and is not a mere mixture of the colours proceeding unaltered from the yellow and the blue particles: moreover, as can easily be shown by two sheets of coloured paper and a piece of window glass, blue and yellow light, when combined, do not give a trace of green, but if pure would produce the effect of white. Green, therefore, is after all not produced by a mixture of blue and yellow. On the other hand Clerk-Maxwell proved in 1860 that yellow could be produced by a mixture of red and green, which put an end to the pretension of yellow to be considered a primary element of colour. From these and other considerations it would seem, therefore, that the three primary colours—if such an expression be retained—are red, green, and violet.

The existence of rays beyond the violet, though almost invisible to our eyes, had long been demonstrated by their chemical action. Stokes, however, showed in 1852 that their existence might be proved in another manner, for that there are certain substances which, when excited by them, emit light visible to our eyes. To this phenomenon he gave the name of fluorescence. At the other end of the spectrum Abney has recently succeeded in photographing a large number of lines in the infra-red portion, the existence of which was first proved by Sir William Herschel.

From the rarity, and in many cases the entire absence, of reference to blue, in ancient literature, Geiger—adopting and extending a suggestion first thrown out by Mr. Gladstone—has maintained that, even as recently as the time of Homer, our ancestors were blue-blind. Though for my part I am unable to adopt this view, it is certainly very remarkable that neither the Rigveda, which consists almost entirely of hymns to heaven, nor the Zendavesta, the Bible of the Parsees or fire-worshippers, nor the Old Testament, nor the Homeric poems, ever allude to the sky as blue.

On the other hand, from the dawn of poetry, the splendours of the morning and evening skies have excited the admiration of mankind. As Ruskin says, in language almost as brilliant as the sky itself, the whole heaven, "from the zenith to the horizon, becomes, one molten, mantling sea of colour and fire; every black bar turns into massy gold, every ripple and wave into unsullied shadowless crimson, and purple, and scarlet, and colours for which there are no words in language, and no ideas in the mind—things which can only be conceived while they are visible; the intense hollow blue of the upper sky melting through it all, showing here deep, and pure, and lightness; there, modulated by the filmy, formless body of the transparent vapour, till it is lost imperceptibly in its crimson and gold."

But what is the explanation of these gorgeous colours? why is the sky blue? and why are the sunrise and sunset crimson and gold? It may be said that the air is blue, but if so how can the clouds assume their varied tints? Brücke showed that very minute particles suspended in water are blue by reflected light. Tyndall has taught us that the blue of the sky is due to the reflection of the blue rays by the minute particles floating in the atmosphere. Now if from the white light of the sun the blue rays are thus selected, those which are transmitted will be yellow, orange, and red. Where the distance is short the transmitted light will appear yellowish. But as the sun sinks towards the horizon the atmospheric distance increases, and consequently the number of the scattering particles. They weaken in succession the violet, the indigo, the blue, and even disturb the proportions of green. The transmitted light under such circumstances must pass from yellow through orange to red, and thus, while we at noon are admiring the deep blue of the sky, the same rays, robbed of their blue, are elsewhere lighting up the evening sky with all the glories of sunset.

Another remarkable triumph of the last half-century has been the discovery of photography. At the commencement of the century Wedgwood and Davy observed the effect produced by throwing the images of objects on paper or leather prepared with nitrate of silver, but no means were known by which such images could be fixed. This was first effected by Niepce, but his processes were open to objections which prevented them from coming into general use, and it was not till 1839 that Daguerre invented the process which was justly named after him. Very soon a further improvement was effected by our countryman Talbot. He not only fixed his "Talbotypes" on paper—in itself a great convenience—but, by obtaining a negative, rendered it possible to take off any number of positive, or natural, copies from one original picture.

We owe to Wheatstone the conception that the idea of solidity is derived from the combination of two pictures of the same object in slightly different perspective. This he proved in 1833 by drawing two outlines of some geometrical figure or other simple object, as they would appear to either eye respectively, and then placing them so that they might be seen, one by each eye. The "stereoscope," thus produced, has been greatly popularised by photography.

For 2000 years the art of lighting had made little if any progress. Until the close of the last century, for instance, our lighthouses contained mere fires of wood or coal, though the construction had vastly improved. The Eddystone lighthouse, for instance, was built by Smeaton in 1759; but for forty years its light consisted in a row of tallow candles stuck in a hoop. The Argand lamp was the first great improvement, followed by gas, and in 1863 by the electric light.

Just as light was long supposed to be due to the emission of material particles, so heat was regarded as a material, though ethereal, substance, which was added to bodies when their temperature was raised.

Davy's celebrated experiment of melting two pieces of ice by rubbing them against one another in the exhausted receiver of an air-pump had convinced him that the cause of heat was the motion of the invisible particles of bodies, as had been long before suggested by Newton, Boyle, and Hooke. Rumford and Young also advocated the same view. Nevertheless, the general opinion, even until the middle of the present century, was that heat was due to the presence of a subtle fluid known as "caloric," a theory which is now entirely abandoned.

The determination of the mechanical equivalent of heat is mainly due to the researches of Mayer and Joule. Mayer, in 1842, pointed out the mechanical equivalent of heat as a fundamental datum to be determined by experiment. Taking the heat produced by the condensation of air as the equivalent of the work done in compressing the air, he obtained a numerical value of the mechanical equivalent of heat. There was, however, in these experiments, one weak point. The matter operated on did not go through a cycle of changes. He assumed that the production of heat was the only effect of the work done in compressing the air. Joule had the merit of being the first to meet this possible source of error. He ascertained that a weight of 1 lb. would have to fall 772 feet in order to raise the temperature of 1 lb. of water by 1° Fahr. Hirn subsequently attacked the problem from the other side, and showed that if all the heat passing through a steam-engine was turned into work, for every degree Fahr. added to the temperature of a pound of water, enough work could be done to raise a weight of 1 lb. to a height of 772 feet. The general result is that, though we cannot create energy, we may help ourselves to any extent from the great storehouse of nature. Wind and water, the coal-bed and the forest, afford man an inexhaustible supply of available energy.

It used to be considered that there was an absolute break between the different states of matter. The continuity of the gaseous, liquid, and solid conditions was first demonstrated by Andrews in 1862.

Oxygen and nitrogen have been liquefied independently and at the same time by Cailletet and Raoul Pictet. Cailletet also succeeded in liquefying air, and soon afterwards hydrogen was liquefied by Pictet under a pressure of 650 atmospheres, and a cold of 170° Cent. below zero. It even became partly solidified, and he assures us that it fell on the floor with "the shrill noise of metallic hail." Thus then it was shown experimentally that there are no such things as absolutely permanent gases.

The kinetic theory of gases, now generally accepted, refers the elasticity of gases to a motion of translation of their molecules, and we are assured that in the case of hydrogen at a temperature of 60° Fahr. they move at an average rate of 6225 feet in a second; while as regards their size, Loschmidt, who has since been confirmed by Stoney and Sir W. Thomson, calculates that each is at most 1-50000000th of an inch in diameter.

We cannot, it would seem at present, hope for any increase of our knowledge of atoms by any improvement in the microscope. With our present instruments we can perceive lines ruled on glass of 1-90,000th of an inch apart. But, owing to the properties of light itself, the fringes due to interference begin to produce confusion at distances of 1-74,000. It would seem then that, owing to the physical characters of light, we can, as Sorby has pointed out, scarcely hope for any great improvement so far as the mere visibility of structure is concerned, though in other respects no doubt much may be hoped for. At the same time, Dallinger and Royston Pigott have shown that, so far as the mere presence of simple objects is concerned, bodies of even smaller dimensions can be perceived.

Sorby is of opinion that in a length of 1-80,000th of an inch there would probably be from 500 to 2000 molecules—500, for instance, in albumen and 2000 in water. Even, then, if we could construct microscopes far more powerful than any we now possess, they would not enable us to obtain by direct vision any idea of the ultimate molecules of matter. Sorby calculates that the smallest sphere of organic matter which could be clearly defined with our most powerful microscopes would contain many millions of molecules of albumen and water, and it follows that there may be an almost infinite number of structural characters in organic tissues, which we can at present foresee no mode of examining.

Electricity in the year 1831 may be considered to have just been ripe for its adaptation to practical purposes; it was but a few years previously, in 1819, that Oersted had discovered the deflective action of the current on the magnetic needle, that Ampère had laid the foundation of electro-dynamics, that Schweizzer had devised the electric coil or multiplier, and that Sturgeon had constructed the first electro-magnet. It was in 1831 that Faraday, the prince of pure experimentalists, announced his discoveries of voltaic induction and magneto-electricity, which with the other three discoveries constitute the principles of nearly all the telegraph instruments now in use; and in 1834 our knowledge of the nature of the electric current had been much advanced by the interesting experiment of Sir Charles Wheatstone, proving the velocity of the current in a metallic conductor to approach that of the wave of light.

Practical applications of these discoveries were not long in coming to the fore, and the first telegraph line on the Great Western Railway from Paddington to West Drayton was set up in 1838. In America Morse is said to have commenced to develop his recording instrument between the years 1832 and 1837.

In 1851, submarine telegraphy became an accomplished fact through the successful establishment of telegraphic communication between Dover and Calais. Submarine lines followed in rapid succession, crossing the English Channel and the German Ocean, threading their way through the Mediterranean, Black and Red Seas, until in 1866, after two abortive attempts telegraphic communication was successfully established between the Old and New Worlds, beneath the Atlantic Ocean.

Duplex and quadruplex telegraphy, one of the most striking achievements of modern telegraphy, the result of the labours of several inventors, should not be passed over in silence. It not only serves for the simultaneous communication of telegraphic intelligence in both directions, but renders it possible for four instruments to be worked irrespectively of one another, through one and the same wire connecting to distant places.

Another more recent and perhaps still more wonderful achievement in modern telegraphy is the invention of the telephone and microphone, by means of which the human voice is transmitted through the electric conductor, by mechanism that imposes through its extreme simplicity. In this connection the names of Reiss, Graham Bell, Edison, and Hughes are those chiefly deserving to be recorded.

By the electric transmission of power, we may hope some day to utilise at a distance such natural sources of energy as the Falls of Niagara, and to work our cranes, lifts, and machinery of every description by means of sources of power arranged at convenient centres. To these applications the brothers Siemens have more recently added the propulsion of trains by currents passing through the rails, the fusion in considerable quantities of highly refractory substances, and the use of electric centres of light in horticulture as proposed by Werner and William Siemens. By an essential improvement by Faure of the Planté Secondary Battery, the problem of storing electrical energy appears to have received a practical solution, the real importance of which is clearly proved by Sir W. Thomson's recent investigation of the subject

It would be difficult to assign the limits to which this development of electrical energy may not be rendered serviceable for the purposes of man.

As regards Mathematics I have felt that it would be impossible for me, even with the kindest help, to write anything myself. Mr. Spottiswoode, however, has been so good as to supply me with the following memorandum.

In a complete survey of the progress of science during the half-century which has intervened between our first and our present meeting, the part played by mathematics would form no insignificant feature. To those indeed who are outside its enchanted circle it is difficult to realise the intense intellectual energy which actuates its devotees, or the wide expanse over which that energy ranges.

In the extension of mathematics it has happened more than once that laws have been established so simple in form, and so obvious in their necessity, as scarcely to require proof. And yet their application is often of the highest importance in checking conclusions which have been drawn from other considerations, as well as in leading to conclusions which, without their aid, might have been difficult of attainment. The same thing has occurred also in physics; and notably in the recognition of what has been termed the "Law of the Conservation of Energy."

Energy has been defined to be "The capacity, or power, of any body, or system of bodies, when in a given condition, to do a measurable quantity of work." Such work may either change the condition of the bodies in question, or it may affect other bodies; but in either case energy is expended by the agent upon the recipient in performance of the work. The law then states that the total amount of energy in the agents and recipients taken together remains unaltered by the changes in question.

Now the principle on which the law depends is this: "that every kind of change among the bodies may be expressed numerically in one standard unit of change," viz., work done, in such wise that the result of the passage of any system from one condition to another may be calculated by mere additions and subtractions, even when we do not know how the change came about.

The history of a discovery, or invention, so simple at first sight, is often found to be more complicated the more thoroughly it is examined. That which at first seems to have been due to a single mind proves to have been the result of the successive action of many minds. Attempts more or less successful in the same direction are frequently traced out; and even unsuccessful efforts may not have been without influence on minds turned towards the same object. Lastly also, germs of thought, originally not fully understood, sometimes prove in the end to have been the first stages of growth towards ultimate fruit. The history of the law of the conservation of energy forms no exception to this order of events. There are those who discern even in the writings of Newton expressions which show that he was in possession of some ideas which, if followed out in a direct line of thought, would lead to those now entertained on the subjects of energy and of work. But however this may be, and whosoever might be reckoned among the earlier contributors to the general subject of energy, and to the establishment of its laws, it is certain that within the period of which I am now speaking, the names of Séguin, Clausius, Helmholtz, Mayer, and Colding, on the Continent, and those of Grove, Joule, Rankine, and Thomson, in this country, will always be associated with this great work.

Prof. Frankland has been so good as to draw up for me the following account of the progress of Chemistry during the last half-century.

Most of the elements had been discovered before 1830, the majority of the rarer elements since the beginning of the century. In addition to these the following five have been discovered, three of them by Mosander, viz.:—lanthanum in 1839, didymium in 1842, and erbium in 1843. Ruthenium was discovered by Claus in 1843, and niobium by Rose in 1844. Spectrum analysis has added five to the list, viz.:—Cæsium and rubidium, which were discovered by Bunsen and Kirchhoff in 1860; thallium, by Crookes in 1861; indium, by Reich and Richter in 1863; and gallium, by Lecoq de Boisbaudran in 1875.

In organic chemistry the views most generally held about the year 1830 were expressed in the radical theory of Berzelius. This theory, which was first stated in its electro-chemical and dualistic form by its author in 1817, received a further development at his hands in 1834 after the discovery of the benzoyl-radical by Liebig and Wöhler. In the same year (1834), however, a discovery was made by Dumas, which was destined profoundly to modify the electro-chemical portion of the theory, and even to overthrow the form of it put forth by Berzelius. Dumas showed that an electro-negative element, such as chlorine, might replace, atom for atom, an electro-positive element like hydrogen, in some cases without much alteration in the character of the compound. This law of substitution has formed a necessary portion of every chemical theory which has been proposed since its discovery, and its importance has increased with the progress of the science.

Chemists have been engaged in determining, by means of decompositions, the molecular architecture, or *constitution* as it is called, of various compounds, natural and artificial, and in verifying by synthesis the correctness of the views thus arrived at.

It was long supposed that an impassable barrier existed between inorganic and organic substances: that the chemist could make the former in his laboratory, while the latter could only be produced in the living bodies of animals or plants,—requiring for their construction not only chemical attractions, but a supposed "vital force." It was not until 1828 that Wöhler broke down this barrier by the synthetic production of urea, and since his time this branch of science in the hands of Hofmann has made great strides.

In connection with the rectification of the atomic weights it may be mentioned that a so called natural system of the elements has been introduced by Mendelejeff (1869), in which the properties of the elements appear as a periodic function of their atomic weights. By the aid of this system it has been possible to predict the properties and atomic weights of undiscovered elements, and in the case of known elements to determine many atomic weights which had not been fixed by any of the usual methods. Several of these predictions have been verified in a remarkable manner. A periodicity in the atomic weights of elements belonging to the same class had been pointed out by Newlands about four years before the publication of Mendelejeff's memoir.

In Mechanical Science the progress has not been less remarkable than in other branches. Indeed to the improvements in mechanics we owe no small part of our advance in practical civilisation, and of the increase of our national prosperity during the last fifty years.

This immense development of mechanical science has been to a great extent a consequence of the new processes which have been adopted in the manufacture of iron, for the following data with reference to which I am indebted to Captain Douglas Galton. About 1830, Neilson introduced the Hot Blast in the smelting of iron. At first a temperature of 600° or 700° Fahrenheit was obtained, but Cowper subsequently applied Siemens' regenerative furnace for heating the blast, chiefly by means of fumes from the black furnace, which were formerly wasted; and the temperature now practically in use is as much as 1400° or even more: the result is a very great economy of fuel and an increase of the output.

Bessemer, by his brilliant discovery, which he first brought before the British Association at Cheltenham in 1856, showed that Iron and Steel could be produced by forcing currents of atmospheric air through fluid pig metal, thus avoiding for the first time the intermediate process of puddling iron, and converting it by cementation into steel. These changes, by which steel can be produced direct from the blast furnace instead of by the more cumbersome processes formerly in use, have been followed by improvements in manipulation of the metal.

The inventions of Cort and others were known long before 1830, but we were then still without the most powerful tool in the hands of the practical metallurgist, viz., Nasmyth's steam hammer.

Steel can be produced as cheaply as iron was formerly; and its substitution for iron as railway material and in shipbuilding, has resulted in increased safety in railway travelling, as well as in economy, from its vastly greater durability.

The introduction of iron, has, moreover, had a vast influence on the works of both the civil and military engineer. Before 1830, Telford had constructed an iron suspension turnpike-road bridge of 560 feet over the Menai Straits; but this bridge was not adapted to the heavy weights of locomotive engines. At the present time, with steel at his command, Mr. Fowler is engaged in carrying out the design for a railway bridge over the Forth, of two spans of 1700 feet each; that is to say, of nearly one-third of a mile in length.

But it is in railroads, steamers, and the electric telegraph, that the progress of mechanical science has most strikingly contributed to the welfare of man. To the latter I have already referred.

As regards railways, the Stockton and Darlington Railway was opened in 1825, but the Liverpool and Manchester Railway, perhaps the first truly passenger line, dates from 1830, while the present mileage of railways is over 200,000 miles, costing nearly 4,000,000,000*l*. sterling. It was not until 1838 that the *Sirius* and *Great Western* first steamed across the Atlantic. The steamer, in fact, is an excellent epitome of the progress of the half-century; the paddle has been superseded by the screw; the compound has replaced the simple engine; wood has given place to iron, and iron in its turn to steel. The saving in dead weight, by this improvement alone, is from 10 to 16 per cent. The speed has been increased from 9 knots to 15, or even more. Lastly, the steam-pressure has been increased from less than 5 lbs. to 70 lbs. per square inch, while the consumption of coal has been brought down from 5 or 6 lbs. per horse-power to less than 2. It is a remarkable fact that not only is our British shipping rapidly on the increase, but it is increasing relatively to that of the rest of the world. In 1860 our tonnage was 5,700,000 against 7,200,000; while it may now be placed as 8,500,000 against 8,200,000; so that considerably more than half the whole shipping of the world belongs to this country.

If I say little with reference to Economic Science and Statistics, it is because time, not materials, are wanting.

I scarcely think that in the present state of the question I can be accused of wandering into politics if I observe that the establishment of the doctrine of free trade as a scientific truth falls within the period under review.

In Education some progress has been made towards a more rational system. When I was at a public school, neither science, modern languages, nor arithmetic formed any part of the school system. This is now happily changed. Much, however, still remains to be done. Too little time is still devoted to French and German, and it is much to be regretted that even in some of our best schools they are taught as dead languages. Lastly, with few exceptions, only one or two hours on an average are devoted to science. We have, I am sure, none of us any desire to exclude, or discourage, literature. What we ask is that, say, six hours a week each should be devoted to mathematics, modern languages, and science, an arrangement which would still leave twenty hours for Latin and Greek. I admit the difficulties which schoolmasters have to contend with; nevertheless, when we consider what science has done and is doing for us, we cannot but consider that our present system of education is, in the words of the Duke of Devonshire's Commission, little less than a national misfortune.

In Agriculture the changes which have occurred in the period since 1831 have been immense. The last half century has witnessed the introduction of the modern system of subsoil drainage founded on the experiments of Smith of Deanston. The thrashing and drilling machines were the most advanced forms of machinery in use in 1831. Since then there have been introduced the steam-plough; the mowing-machine; the reaping-machine, which not only cuts the corn but binds it into sheaves; while the steam-engine thrashes out the grain and builds the ricks. Science has thus greatly reduced the actual cost of labour, and yet it has increased the wages of the labourer.

It was to the British Association, at Glasgow in 1841, that Baron Liebig first communicated his work "On the Application of Chemistry to Vegetable Physiology," while we have also from time to time received accounts of the persevering and important experiments which Mr. Lawes, with the assistance of Dr. Gilbert, has now carried on for more than forty years at Rothamsted, and which have given so great an impulse to agriculture by directing attention to the principles of cropping, and by leading to the more philosophical application of manures.

I feel that in quitting Section F so soon, I owe an apology to our fellow-workers in that branch of science, but I doubt not that my shortcomings will be more than made up for by the address of their excellent President, Mr. Grant-Duff, whose appointment to the governorship of Madras, while occasioning so sad a loss to his friends, will unquestionably prove a great advantage to India, and materially conduce to the progress of science in that country.

Moreover, several other subjects of much importance, which might have been referred to in connection with these latter Sections, I have already dealt with under their more purely scientific aspect.

Indeed, one very marked feature in modern discovery is the manner in which distinct branches of science have thrown, and are throwing, light on one another. Thus the study of geographical distribution of living beings, to the knowledge of which our late general secretary, Mr. Sclater, has so greatly contributed, has done much to illustrate ancient geography. The existence of high northern forms in the Pyrenees and Alps points to the existence of a period of cold when Arctic species occupied the whole of habitable Europe. Wallace's line—as it has been justly named after that distinguished naturalist—points to the very ancient separation between the Malayan and Australian regions; and the study of corals has thrown light upon the nature and significance of atolls and barrier-reefs.

In studying the antiquity of man, the archæologist has to invoke the aid of the chemist, the geologist, the physicist and the mathematician. The recent progress in astronomy is greatly due to physics and chemistry. In geology the composition of rocks is a question of chemistry; the determination of the boundaries of the different formations falls within the limits of geography; while palæontology is the biology of the past.

And now I must conclude. I fear I ought to apologise to you for keeping you so long, but still more strongly do I wish to express my regret that there are almost innumerable researches of great interest and importance which fall within the last fifty years (many even among those with which our Association has been connected) to which I have found it impossible to refer. Such for instance are, in biology alone, Owen's memorable report on the homologies of the vertebrate skeleton, Carpenter's laborious researches on the microscopic structure of shells, the reports on marine zoology by Allman, Forbes, Jeffreys, Spence Bate, Norman, and others; on Kent's Cavern by Pengelly; those by Duncan on corals; Woodward on crustaceæ; Carruthers, Williamson, and others on fossil botany, and many more. Indeed no one who has not had occasion to study the progress of science throughout its various departments can have any idea how enormous—how unprecedented—the advance has been.

Though it is difficult, indeed impossible, to measure exactly the extent of the influence exercised by this Association, no one can doubt that it has been very considerable. For my own part, I must acknowledge with gratitude how much the interest of my life has been enhanced by the stimulus of our meetings, by the lectures and memoirs to which I have had the advantage of listening, and above all, by the many friendships which I owe to this Association.

Summing up the principal results which have been attained in the last half-century we may mention (over and above the accumulation of facts) the theory of evolution, the antiquity of man, and the far greater antiquity of the world itself; the correlation of physical forces and the conservation of energy; spectrum analysis and its application to celestial physics; the higher algebra and the modern geometry; lastly, the innumerable applications of science to practical life—as, for instance, in photography, the locomotive engine, the electric telegraph, the spectroscope, and most recently the electric light and the telephone.

To science, again, we owe the idea of progress. The ancients, says Bagehot, "had no conception of progress; they did not so much as reject the idea; they did not even entertain it." It is not, I think, now going too far to say that the true test of the civilisation of a nation must be measured by its progress in science. It is often said, however, that great and unexpected as the recent discoveries have been, there are certain ultimate problems which must ever remain unsolved. For my part I would prefer to abstain from laying down any such limitations. When Park asked the Arabs what became of the sun at night, and whether the sun was always the same, or new each day, they replied that such a question was childish, and entirely beyond the reach of human investigation. I have already mentioned that, even as lately as 1842, so high an authority as Comte treated as obviously impossible and hopeless any attempt to determine the chemical composition of the heavenly bodies. Doubtless there are questions, the solution of which we do not as yet see our way even to attempt; nevertheless the experience of the past warns us not to limit the possibilities of the future.

But however this may be, though the progress made has been so rapid, and though no similar period in the world's history has been nearly so prolific of great results, yet, on the other hand, the prospects of the future were never more encouraging. We must not, indeed, shut our eyes to the possibility of failure; the temptation to military ambition; the tendency to over-interference by the State; the spirit of anarchy and socialism; these and other elements of danger may mar the fair prospects of the future. That they will succeed, however, in doing so, I cannot believe. I cannot but feel confident hope that fifty years hence, when perhaps the city of York may renew its hospitable invitation, my successor in this chair—more competent, I trust, than I have been to do justice to so grand a theme—will have to record a series of discoveries even more unexpected and more brilliant than those which I have, I fear so imperfectly, attempted to bring before you this evening. For one great lesson which science teaches is, how little we yet know, and how much we have still to learn.

SECTION B

CHEMICAL SCIENCE

Opening Address by Prof. A. W. Williamson, Ph.D., LL.D., F.R.S., V.P.C.S., President of the Section

On the Growth of the Atomic Theory

It has been thought desirable that on the occasion of this half-centenary celebration of the foundation of this great Association, some notice should be presented to the members of what has been doing in the respective branches of science during the period of our activity; and I have, accordingly, traced out for your consideration a very imperfect sketch of the theories which guided chemical inquiry at the beginning of that period, and of the leading changes which have been wrought in them by fifty years' work.

There is perhaps hardly any branch of science which during the last fifty years has made such great and steady progress as chemistry. Let any one compare recent dictionaries of the science (including the bulky supplements, which contain a record of the chief discoveries made while the body of the work was being compiled) with a treatise of chemistry fifty years old. Let him compare a published record of one year's progress of the science fifty years ago with one of modern date. Let him compare, as far as may be possible, the number of men who formerly devoted their whole time and energy to the advancement of chemistry, or who were engaged in industrial pursuits involving a knowledge of the science, with the corresponding number nowadays. Let him count up the services which chemistry had rendered to common life at the commencement of the epoch with those which it has now to show.

Everywhere he will see marvellous evidences of increasing growth. But if he be a reflecting man, he will not be satisfied with wondering at results: he will endeavour to trace them to their causes, and to discover the guiding principles which have brought them about: he will try to derive, from a knowledge of those guiding principles, a perception of the means by which such progress can best be continued and extended—how it can be most effectively directed to the benefit of his fellow-men.

It is on this aspect of the question that I propose to address you to-day.

The process of scientific investigation includes a great variety of operations, which may be considered under three headings, mental, sensual, and physical. We think, we observe, and we work with our hands. In planning a new experiment we call to mind what is known of the phenomena in question, and form an opinion as to what is likely to happen under conditions somewhat different from those which existed in previous experiments. We regulate by careful observations the necessary manual operations, so as to obtain with accuracy the desired conditions for the new experiment, and we observe attentively the changes which take place in the course of that experiment. The result of such observations is sometimes in accordance with our anticipation, but very frequently at variance with it. If it accords with our anticipation, we put on record the extension which it has given to the application of the general theory on which that anticipation was founded. But if the result is not what we expected, we carefully and critically revise the reasoning which had led us to expect a particular result, and often repeat the same experiment with greater care, or some modification of it.

Materials for a new theory are gained when logically faultless reasoning, checked by accurate observations, have led to results which could not have been foreseen by the aid of any previous theory. When a theory has thus gained a footing in science, it serves as a guide in further work. It guides us in arranging known facts. It guides us to the discovery of new facts. Sometimes it does these things for a short time only, and is then superseded by some more general theory derived from a wider and more comprehensive view of the facts.

There is, perhaps, nowhere so severe and rigorous a test of the truth of an idea as that which is afforded by its use in any accurate department of experimental science; and it is worth while, on philosophical grounds, to consider briefly the conditions of growth of the chief chemical theories which have withstood this ordeal and proved themselves to be trustworthy guides in experimental science.

Now as far as I know them, the general theories which have played the chief part in the development of chemistry are mere condensed statements of fact.

Every thoughtful man of science has doubtless indulged in speculations to find the cause of facts which are as yet unexplained; has imagined some fundamental condition or property of matter which might cause it to produce effects such as are witnessed. It is to be hoped that the time may be far distant when men of science will confine their thoughts within the range of ideas which are proved to be true. But it is most important that they should not confuse such hypothetical speculations with theories which have received experimental verification, and that while employing any theory they should not lose sight of the limits within which it has been proved to be correct, beyond which it can only be used as an hypothesis.

The foundation of the science of chemistry was laid by the discovery of chemical elements; those distinct varieties of matter which we can neither produce nor destroy. Chemical science treats of those changes of property in matter which can be represented as due to changes of combination of elementary atoms. It knows nothing of the production or destruction of those elementary atoms. Speculations respecting their ultimate form or structure will have found a place in the science as soon as such speculations have helped to arrange the facts which are known, and to discover new chemical facts.

At the commencement of our epoch chemists had classified elements according to their electro-chemical properties. Chemical analysis had established the fact that a good many compounds could be represented as consisting of elementary atoms of two kinds combined in small number. Thus carbonic oxide and carbonic acid had been found to possess respectively a composition which could be represented (adopting our present atomic symbols) by the formulæ CO and CO_2, water by the formula H_2O, marsh gas CH_4, olefiant gas CH_2. The oxides and acids of nitrogen were represented by formulæ corresponding empirically to those which we now adopt. So also ammonia and hydric chloride had their present formulæ. Sulphurous and sulphuric acid had the respective formulæ SO_2 and SO_3. Phosphorus and phosphoric acid had the formulæ P_2O_3 and P_2O_5. Baryta and the oxides of iron had the formulæ BaO, FeO, Fe_2O_3.

Such primary compounds were classified upon the same principle which served for the classification of the elements themselves, into electro-positive or basylous and electro-negative or chlorous compounds, and the smallest quantity of each of them, which consistently with an atomic representation of the results of analysis, was deemed capable of existing, was called an atom of that compound.

Very simple compounds possessed of prominent characteristics and distinct reactions had first been isolated and identified. They were found to contain their constituent elements in proportions easily recognisable as multiples of atomic weights. But such simple compounds are rare exceptions among mineral and organic materials, and if the atomic theory could have gone no further than to guide us to an understanding of these few simple compounds, it must soon have given place to some more fundamental conception. It is moreover worthy of notice that in this its most elementary form the atomic theory was not the only conceivable interpretation of the proportions of combination between elements. Those proportions could be as consistently represented by fractions as by integral multiples. Thus, instead of representing carbonic acid as containing twice as much oxygen as is contained in carbonic oxide, we might have represented it as containing the same quantity of oxygen combined with half as much carbon, and using for the moment atomic symbols for a non-atomic theory, we might have written carbonic acid thus $\frac{C}{2}O$. Or we might represent them both by percentage numbers.

It was so simple and natural to adopt the atomic hypothesis, and to represent compounds as built up of atoms, that chemists seem to have paid little attention to any other mode of representing the proportions of combination. They assumed that the variable proportions of elements, which were observed in compounds, were due to the various numbers of elementary atoms respectively aggregated together in each compound. They perceived that the existence of elementary atoms involved the existence of compound atoms, or molecules, as we now call them, and accordingly they represented each known compound of two elements by a molecular formula as simple as possible, consistently with the view of its atomic constitution. Many of these molecules, such as those of the acids, were found to be capable of combining with others of the other class, forming salts, and those combinations were found to take place in proportions corresponding to the weights of the respective molecules, or to very simple multiples of those weights, and the secondary compounds or salts thus formed combined (if at all) in proportions corresponding to simple multiples of their molecular weights. The dualistic representation of the constitution of salts served to represent the results of their analysis consistently with the atomic theory, and a vast number of fundamental facts were collected and arranged by the aid of the dualistic theory of combination.

The actual numbers obtained by analysis of any particular compound exhibited sometimes a very near approximation to those required by an atomatic formula of its composition. Sometimes they differed considerably from those required by theory; but it was always found that the more pure in substance and the more accurate the analytical operations, the more nearly did the result agree with some atomic formula of the substance.

The compound atoms were units which had grown out of the atomic theory. Each of them was the smallest quantity of a compound, which (consistently with the results of analysis) could be represented as built dualistically of its constituent atoms.

Chemical combination was viewed as a process of juxtaposition, of simple or compound atoms, little account being taken of the disturbance of the previous arrangement of those compound atoms. It was when a constitution, similar to that attributed to salts, was imagined for other compounds not saline in their character, that the dualistic theory broke down. Thus chlorocarbonic acid was represented as a compound of carbonic acid with carbonic chloride, and was accordingly designated as carbonate of carbonic chloride, while the formula was made to contain the formulæ of those bodies. Chlorosulphuric acid and chlorochromic acid were in like manner represented as compounds of sulphuric and chromic acid respectively with imaginary hexachlorides.

Careful investigations of the reactions in which chlorocarbonic acid takes part showed, however, that in each of them it behaves as a compound containing only two atoms of chlorine. It was found that the commonest and best-known carbonates and sulphates have a fundamentally similar constitution. Thus potassic carbonate may be represented as a compound in which the two atoms of chlorine in phosgene are replaced by two atoms of the radical O K; and oil of vitriol, as a compound of two atoms of hydroxyl with the same group, $S O_2$, which in chlorosulphuric acid is combined with two atoms of chlorine. Chlorochromic acid has not been examined to as great an extent as the above compounds, but all we know of it points clearly to its having molecular constitution similar to that of chlorosulphuric acid, viz. $Cl_2 Cr O_2$, for not only do their vapour densities agree, but the chromates in their constitution and crystalline forms exhibit a clear analogy to the sulphates.

Moreover, the simpler molecular formulæ, which a fuller knowledge of their chemical behaviour suggested for these bodies, were found in all cases to agree with the volume belonging to the molecule of every pure substance known in the state of vapour.

A difficulty of another kind had been foreseen by the great founder of the dualistic system, and it was by the investigations in organic chemistry that it assumed serious proportions.

Carbon compounds were discovered possessing definite and specific properties, and presenting the characteristics of pure substances, but of which the results of analysis did not agree with any simple proportion between the numbers of their constituent atoms. Their empirical composition could not be decided by the aid of the so-called law of multiple proportions, for two or more atomic formulæ required percentages of the constituents differing so little from one another that analysis could not decide which was the true one.

In order to select the true molecular formulæ of such complex substances from among those which approached most nearly to the results of ultimate analysis, and to determine with certainty their empirical composition, it was necessary to find other methods for the determination of molecular weights. It was necessary to study the various properties of compounds of known composition, and of others which could be prepared in a state of purity; to determine the vapour densities and rates of diffusion of those which could be obtained in the gaseous state without decomposition; to determine boiling points and melting points; to examine crystalline forms of pure compounds and of mixtures; to determine solubilities and densities of solids and of liquids; but above all it was necessary to collect fuller and more accurate knowledge of the chemical changes which take place in the mutual reaction of molecules.

A vast amount of accurate and careful work of these kinds has been done, and has been subjected to rigid and often hostile scrutiny during the various steps of its progress. We now know that compound atoms, or molecules as we call them, which can be identified by their geometrical, mechanical, and other properties, are the same as the compound atoms indicated by the most comprehensive chemical evidences of composition and reactions. The molecular constitution of matter was predicted implicitly by the atomic theory of the constitution of the elements; and, wherever the physical properties of the molecules are such as afford any basis for the determination of their relative weights, such results agree with those derived from purely chemical considerations guided by the atomic theory.

Our knowledge of molecules is as yet in its infancy. Even among the commonest elements and compounds we know the molecular weights of very few, but what we do know of them proves that the idea of compound atoms invented by chemists to explain the elementary facts of chemical action is, as far as it goes, a true representation of what exists in nature.

Many of the molecules thus proved to exist were the same as those suggested under the dualistic system; but many were proved, by the more accurate and extensive knowledge of their reactions and properties, to have a different weight from that which had been at first attributed to them, yet always consistent with the fundamental requirements of the atomic theory. Thus H_2O, CO, CO_2, CH_4, SO_2, SO_3, CaO, FeO, Fe_2O_3, are the formulæ still used to denote the molecules of the respective compounds, though the last three ought probably to be represented by some multiple. On the other hand, the molecule of olefiant gas is now represented by the formula C_2H_4, instead of CH_2. The chloracetate is $C_2Cl_3HO_2$, instead of C_2Cl_6, C_2O_3, H_2O. The molecule of benzoil chloride is C_7H_5OCl, instead of one corresponding to $(C_7H_5)_2O_3$, $C_7H_5Cl_3$, and chlorosulphuric acid is Cl_2SO_2, instead of $2SO_3$, SCl_6.

In proportion as chemists came to know more of the constitution of molecules, and to study chemical reactions from the point of view of the changes which they bring about in the constitution of molecules, did the idea of substitution come to be more and more used in the place of that of mere additive combination. A vast number of processes of chemical combination, which had been considered as consisting of direct combination, were found to be processes of double decomposition.

One of the most important facts which was brought to light by the careful examination of the composition of salts and organic bodies, aided by the molecular method of representing their constitution, was that hydrogen is chemically one of the metals, and that the compounds formed by the combination of water with acids are analogous to other salts of those acids; while compounds of hydrogen with elements or radicals like chlorine are salts, analogous in their constitution to other chlorides, &c.

The molecular or unitary mode of viewing the constitution of each substance affords more *true* as well as more simple records of the facts observed in chemical reactions than could be obtained in the dualistic systems. A salt such as hydric sulphate used to be considered as containing sulphuric acid and water, and represented by a formula such as SO_3, H_2O, implying the presence in it of both the substances from which it was known to be formed.

When two elements combined, their product was considered and described as containing the elementary atoms which had served to form it, and it was consistent with this habit to represent a product which had been formed by the combination of two compound molecules as containing those molecules.

But the main business of chemical investigation is to observe accurately the changes of composition which take place in the reactions of known substances, with a view of discovering the atomic changes to which they are due.

The compound formed by the combination of sulphuric acid and water differs in many physical and chemical properties from both of those bodies. Its name and its atomic formula serve to denote the aggregate of properties which are known to belong to it, whereas the dualistic formula, SO_3, H_2O, served to recall the properties of the acid and base from which it was formed, rather than those of the compound itself.

Elementary chemical reactions which according to the binary mode of viewing compounds were supposed to consist of dualistic processes involving sometimes the assumption of forces (like predisposing affinity) of a purely metaphysical character, were now explained as consisting of atomic displacements, or interchanges of a kind well known to be of common occurrence. Thus the evolution of hydrogen by the action of zinc or aqueous hydric sulphate was supposed to be the result of a decomposition of water by the metal, such decomposition being induced by the presence of the acid (SO_3), which exerted a predisposing affinity for the zinc oxide. Our present explanation is a simple statement of the fact, that under the conditions described, zinc displaces hydrogen from its sulphate.

The recognition and study of the metallic functions of hydrogen enabled chemists to obtain far clearer and simpler views of the constitution of salts, and to observe the differences of property which are produced in them by the replacement of one element by another. It enabled us to see more and more clearly the characteristic functions of each element, by comparing the constitution and properties of salts containing it with those of the corresponding salts containing other elements.

Thus in the dualistic system we had for the three common phosphates, PO_4Na_3, PO_4Na_2H, PO_4NaH_2, molecular formulæ in which sodium was represented with twice as great an atomic weight as that which we attribute to it, and which in our atomic weights may be thus represented, viz. P_2O_5, $3Na_2O$; P_2O_5, $2Na$, O; P_2O_5, Na_2O. In like manner we had such a formula as P_2O_3, $2Na_2O$ (for the phosphite PO_3Na_2H), and for the hypophosphite PO_2NaH_2 we had a formula corresponding to P_2O, Na_2O.

Determinations of water of crystallisation and of chemically combined water proved that many of the compounds assumed on the dualistic system to exist are either not obtainable or have different properties and a different constitution from those which have been described. Thus we now know that the salts PO_4Na_2H, PO_4NaH_2, PO_3Na_2H, and PO_2NaH_2 cannot be deprived of the elements of water without undergoing a fundamental change of composition and of properties.

The atomic weights of the alkali metals and of silver were found to be half of those of the dualistic system, and an atom of one of these metals, in common double decompositions between their salts and hydrogen-salts, changes place with one atom of hydrogen.

Many products of the combination of known molecules were found to be formed by processes of double decomposition, so that each molecule of such products is built up partly of atoms derived from one of the materials, partly of atoms from the other. Thus potassic hydrate is formed by the combination of a molecule of potash with one of water. Yet each molecule of the hydrate is built up of half a molecule of potash and half a molecule of water.

The study of organic compounds played an important part in the improvement of our processes of reasoning. Many of their molecules having a very complex structure were found to undergo in most of their reactions very simple changes, of the same kind as those which mineral compounds undergo. Most of the elements of each organic molecule remained combined together with functions analogous to those of hydrogen or chlorine.

The theory of radicals which had been suggested by the reactions of ammonia-salts and of cyanides was largely extended in organic chemistry.

Many families of organic compounds were discovered, in each of which the members are connected by close analogy of constitution and of properties. Each of these families forms what is called a homologous series, each term of the series being a compound of which the molecule contains one atom of carbon and two atoms of hydrogen more than the previous term.

Thus a series of compounds was proved to have reactions similar to those of common alcohol, and molecular weights ranging from 32 to 438. The lower terms of the series are distinguished from one another by differences of boiling points approximately proportional to the number of atoms of carbon and hydrogen by which they differ from one another; whilst the higher terms undergo decomposition at the higher temperatures required for their evaporation, and are distinguished from one another by differences of melting points, that of the alcohol $C_{30}H_{62}O$ being about 85° C. In their constitution these alcohols were found to be analogous to the alkaline hydrates.

In like manner various other series of alcohols were discovered corresponding respectively in their constitutions to other classes of metallic hydrates. Series were also found of which the members present analogies of reaction with monobasic, bibasic, tribasic hydrogen salts respectively.

These and many other such discoveries were made under the guidance of the atomic theory, developed to the point of systematically recognising and studying the mutual reaction of molecules.

One of the most remarkable and important extensions which our knowledge of molecules has undergone consisted in the discovery that various elements in what we are accustomed to consider the free state, really consist of molecules containing like atoms combined with one another.

Thus chemists adopt the formulæ O_2, H_2, Cl_2, P_4, J_2, As_4, to denote molecules of the respective elements, and we have for these molecular formulæ evidences of the same kinds as those which serve to establish the molecular formulæ ClH, H_2O, NH_3, &c. In all the best-known reactions in which chlorine or hydrogen are either taken up or evolved we find that those elements behave as chemical compounds of two like atoms; and, moreover, their molecules, as determined from a study of their reactions, have the same volume as that of every compound molecule proved to evaporate without decomposition.

With this knowledge of the molecular constitution of hydrogen and of chlorine gases, we come to regard the direct formation of hydric chloride as due to a process of double decomposition between two molecules, like the reaction of chlorine on an equal volume of marsh gas.

Many other reactions, such as the evolution of hydrogen by the action of zinc on a hydrogen salt, the liberation of chlorine and nitrogen on the explosive decomposition of their compound, the direct combination of oxygen and hydrogen, we may expect to be able to resolve into mere processes of double decomposition.

The earliest determinations of combining proportions were made with salts (hydrogen salts and others) which undergo double decomposition by mutual contact, and the term equivalent was subsequently introduced to indicate the proportional weights of analogous substances found to be of equal value in their chemical effects. Tables of equivalent weights of acids consisted of numbers standing to one another in the same proportions as the weights of the respective substances found to be of equal value in neutralising a fixed quantity of a particular base; and in like manner tables of the equivalent weights of bases recorded the proportions by weight in which certain bases might replace one another in the neutralisation of a particular quantity of a given acid. Similar determinations have been tabulated of the so-called equivalent weights of elements. Under the dualistic system chemists paid little attention to the essential difference between atomic weights and equivalent weights; and some were of opinion that the facts of chemistry might be represented as consistently from the point of view of equivalence as from that of atoms, and that the idea of atoms (which they considered to be hypothetical) might be dispensed with.

In the system of atomic weights employed under that system, two atoms of hydrogen were generally represented as reacting together, and the symbol of the double atom was marked thus, Ħ. The alkali metals and silver were represented as having atomic weights twice as great as those which we now adopt, and equivalent to those of the magnesian metals and of oxygen. In a great number of the common reactions of these elements the atomic symbols were consistently used as equivalent symbols. But those who professed to dispense with the atomic theory used atomic symbols, even in cases where they did not represent equivalent weights. Thus nitrogen was always represented by its atomic symbol, and the composition and reactions of nitrogen compounds were always studied and represented in accordance with the atomic theory, using various multiple proportions of what they were still pleased to call equivalent weights, using molecular weights, and various other ideas which formed part of the atomic theory, and which had no known connection with the notion of fixed equivalence. If, however, it be true that all chemical compounds consist of elementary atoms, and that the explanation of chemical reactions consists in stating more and more precisely the changes of combination between the constituent atoms of the reacting molecules; equivalence could only be said to exist between a like number of atoms when they were known to have similar functions. It became necessary to study the relation of equivalence between elementary atoms, instead of studying them from the point of view of elements divisible in any proportion.

It is worth while noticing the general process by which this intellectual change was brought about; for there is a good deal yet to be done in the matter, and our future progress may be guided by experience gained in the past.

It was essentially one-sided. One consideration was brought into very prominent relief, and it threw a marvellous light on the matter. It gave us a clear view of the natural order among elements; but, like every other strong light, it fell on one side only.

The equality of vapour-volumes had been used with great advantage in conjunction with chemical reactions and other evidence as a characteristic of molecules, and the attention of chemists was greatly arrested by the consideration of four typical compounds, which upon the concurrent evidence of very extensive chemical examination and equality of vapour-volumes were known to have respectively a composition corresponding to the formulæ ClH, OH_2, NH_3, CH_4.

It was known that the atom of oxygen in water can be replaced by chlorine, but that two atoms of chlorine are needed for the purpose. The atom of nitrogen in ammonia requires three atoms of chlorine to replace it, whilst in marsh gas the atom of carbon is replaceable by four atoms of chlorine. Other elements were studied from the point of view of their respective resemblance to these, and arranged in classes, each of which consisted of atoms equivalent to one another. Thus chlorine, bromine, iodine, fluorine, hydrogen, potassium, sodium, lithium, silver, &c., constituted a class of atoms of equal value, and were called monads. Oxygen, sulphur, selenium, tellurium, calcium, strontium, barium, magnesium, zinc, cadmium, mercury, lead, copper, &c., were classed together as dyads, having equal value amongst themselves, but double the atomic value of the members of the first class. So nitrogen, phosphorus, arsenic, antimony, bismuth, with boron, and some other elements, were considered as forming a class of atoms each of which has three times the value of the monads. The class of tetrads contained carbon, silicon, tin, platinum, &c.

Many apparent exceptions to these atomic values were satisfactorily explained as due to the partial combination of like atoms with one another. Thus in the vast majority of hydrocarbons, such as C_2H_6, C_2H_4, C_2H_2, &c., the atoms of carbon do not appear to be tetravalent, inasmuch as each of the molecules contains less than four atoms of hydrogen to every one atom of carbon. It was well known, however, that polyvalent atoms can combine partly with one element, partly with another, and also that like atoms can combine with one another. Why then should not two tetravalent atoms like carbon combine respectively with three atoms of a monad, and also combine with one another? The compound must be a single molecule with the properties known to belong to methyle C_2H_6. Again, if this molecule were deprived of two of its atoms of hydrogen, each of the atoms of carbon must combine further with the other atom of carbon forming H_2CCH_2; and a further step in this same direction would give us acetylene HCCH, in which each atom of carbon is combined with the other to the extent of three quarters of its value, and with one atom of hydrogen. An extension of this reasoning led to the discovery of long chains of atoms of carbon, each atom forming a link, and each of them (short of the ends) being combined with two other atoms of carbon, while its saturation is completed by hydrogen.

Similar partial combinations of like atoms with one another were recognised in many other classes of compounds, and there is strong reason to expect that the application of the principle will be far more widely extended in proportion as our knowledge of the silicates and other complex classes of compounds becomes somewhat definite.

This incorporation of the doctrine of equivalence into the atomic theory by the division of the elements into classes consisting respectively of equivalent atoms, was probably one of the most important general steps as yet made in the development of the atomic theory. It was seen to correspond in so clear and striking a manner with a vast number of well-known properties and reactions of compounds as to deserve and acquire the confident trust of chemists. But, as often happens in such cases, this confidence in the result carried many of them too far. It led them to assume that atomic values in all other chemical compounds must be always the same as in the compounds under consideration. They saw that they had got hold of the truth, and they thought it was the whole truth. For instance, one most distinguished chemist assumed that each elementary atom has only one value in its compounds; that the atom of nitrogen has always the value three, as in ammonia and its products of substitution, and that in sal ammoniac the atom of nitrogen is chemically combined only with three atoms of hydrogen, whilst the molecule of ammonia is in a state of molecular combination with hydric chloride. Another most distinguished chemist admitted that nitrogen and phosphorus have two atomic values, but not more than two. He held that the respective combining powers are always satisfied by the same number of atoms, no matter what the character of the uniting atoms may be.

With respect to these views it may be noticed that the assumption of combination between molecules as due to some other force than that which binds together the constituents of each molecule—in fact the assumption of molecular combination as an unknown something different from chemical combination, is open to even more grave objections than those which led us to abandon the dualistic system.

To represent a molecule of sal ammoniac as a compound containing two molecules, each one built up by the chemical combination of the constituent atoms, and the two united together by some other force called molecular, was hardly a step in advance of the view which represented it as containing two molecules united together by the same kind of force as that which holds together the atoms in each of the constituent molecules.

The other form of the theory of atomicity as an inherent property of each atom enabling it to combine with an equal number of other atoms, whatever the character of those other atoms may be, seems difficult to reconcile with such facts as the following:—An atom of nitrogen is not known to combine with more than three atoms of hydrogen alone, or of substances like hydrogen, but it forms stable compounds with five atoms (as in the ammonia salts), when four of them are basylous and one of them is chlorous. An atom of sulphur is not known to combine with more than two atoms of hydrogen alone, but it forms stable compounds with four atoms, if three of them are like hydrogen, while the fourth is chlorous. Instances like these are plentiful, and they lead us to look to the chemical characters of the atoms bound together in one molecule as a fundamental condition of the atomic value of the element which binds them together.

Theoretical limitations of natural forces are very difficult of proof, and it is well to be slow and cautious in adopting any such limitation.

A careful consideration of the facts of the case has led me not only to doubt the validity of the supposed limits of atomic value, but to doubt whether we have grounds for assigning any limits whatever to such values.

Atomic values appear to me to be in their very nature variable quantities, and I venture to think that chemistry will be greatly advanced by a full and careful study of the conditions of variation of atomic values.

Two conditions of change of atomic value are particularly worthy of notice:—

I. Temperature.

II. The chemical character of the uniting atoms.

Atomic values increase with fall of temperature, and diminish with rise of temperature. An atom which is combined with as many basylous monads as it can take up by themselves, will take up chlorous monads, or both chlorous and basylous, and reciprocally.

In illustration of the diminution of atomic values with rise of temperature, I may adduce the following well-known reactions: Sal ammoniac containing nitrogen combined with five monads breaks up at a high temperature into ammonia and hydric chloride; and in like manner other ammonia salts decompose by heat forming ammonia or an amide, with trivalent nitrogen. The highest chlorides of phosphorus and of antimony are decomposed by heat into free chlorine and the lower chloride. Potassic fluosilicate is decomposed by heat into silicic and potassic fluorides; and carbonic acid breaks up at high temperatures into a mixture of carbonic oxide and oxygen.

Amongst illustrations of the greater atomic values which elements assume by combining with both chlorous and basylous atoms than with atoms of the one kind only, we may take the following cases: platinum is a metal of which the atom has been supposed to be always tetravalent, because it has not been found capable of combining with more than four atoms of chlorine. The common solution formed by aqua regia contains the compound H_2PtCl_6, a perfectly definite and crystallisable hydrogen salt. Chemists are constantly making and using the potassium and ammonium salts, &c., corresponding to it, yet they conceal from themselves the fact that the atom of platinum is directly combined with eight monads by calling the compounds double salts. The atom of silicon in the silico-fluorides such as H_2SiF_6, or K_2SiF_6, is combined with twice as many monads as it can take up of one kind; so boron in the crystalline salt $NaBF_4$ has a higher atomic value than in its fluoride, owing to the presence of the atom of sodium.

In like manner the atom of gold in the well-known salt $NaAuCl_4$, has a higher value than it can assume with chlorine alone.

Sulphur, of which the atom does not combine with more than 2 atoms of hydrogen, forms with 3 atoms of methyle, or ethyle, and 1 atom of iodine, or chlorine, &c., the well-known compounds like $JSMe_3$; and iodine, which is considered a monad, forms the crystalline and stable periodate $OJ(OH)_5$ and the various metallic derivatives, such as

$$\mathrm{OJ}\begin{array}{l}\mathrm{ONa}\\(\mathrm{OH})_4\end{array}\qquad\qquad \mathrm{OJ}\begin{array}{l}(\mathrm{ONa})_2\\(\mathrm{OH})_3\end{array}\qquad\qquad \mathrm{OJ}\,(\mathrm{OAg})_5$$

The crystalline compound of the perchlorate with water $(HClO_4 2H_2O)$ has probably a similar constitution. Chemical journals abound with descriptions of definite and well-characterised compounds, which have, like the above, been put aside by the atomicity theory, as mere molecular compounds. The following formulæ are taken almost at random, in illustration of the generality of atomic values far beyond those acknowledged by the theory of atomicity.

$JPCl_6$
$KAsF_6$
K_2AsF_7
$KSbF_6$
K_2SbCl_5
K_2SbF_7
K_2BiCl_5
K_2AgJ_3

K_2FeF_4
$2(K_2FeCl_5)$
$2(K_3FeF_6)$
K_2CuCl_4
K_2CuF_4
$KMgCl_3$
K_2MgBr_4

K_2SnCl_6
$KHgJ_3$
$K_2HgJ_4 4H_2O$
$KHgCl_3$
K_2HgCl_4

I have for convenience written in the middle of each of these formulæ the symbol of the atom which I assume to act as connecting element. If we consider the atomic values usually found in these elements, together with those represented by the above list, we see that their atomic values vary according to the numbers given in a line with them respectively in the following table. It has yet to be proved that the atom of platinum is tetravalent in any known compound, for there is no sufficient evidence to show that platinic chloride has a molecular weight corresponding to the formula $PtCl_4$, instead of one corresponding to Pt_2Cl_8, each atom of platinum being partly combined with the other, partly with chlorine.

Atomic Symbols.	Atomic Values.
C	2, 4
S	2, 4
Pt	4 (?), 8
Si	4, 8
Sn	4, 8
Cu	2, 6
Hg	2, 4, 6
Mg	2, 4, 6
Ag	1, 5
B	3, 5
J	1, 7
N	3, 5
P	3, 5, 7
As	3, 7, 9
Sb	3, 5, 7, 9
Bi	3, 7
Au	3 (?), 5

Not only are there elements of which an atom is found in combination with a greater number of basylous and chlorous monads together than of either kind alone, but there are also elements which are not known to form chemical compounds with hydrogen or potassium alone, and yet which combine with either of them when also combined with chlorine, fluorine, &c. This is illustrated by the following compounds, viz., $HAuCl_4$, H_2PtCl_6, $NaBF_4$, K_2SiF_6, K_2FeF_4, K_2CuCl_4. It is also well known that there are many cases of elements of which an atom cannot combine with as many monads of one kind as of another. For instance an atom of nitrogen or of antimony is only known to be trivalent in combination with hydrogen; but each of them occurs in the form of a pentavalent compound with chlorine. Antimony forms either no compound with five atoms of bromine, or a compound more unstable than the higher chloride.

Many more such instances might easily now be given, and a vast number will doubtless be found when the investigations of chemists are directed to the search for them. I have only given these few by way of illustration of the leading conditions of change of atomic values.

In the course of their investigations of the precise interchanges of atoms which take place between molecules, chemists were frequently led to observe evidences of the order in which the constituent elements are combined; and with the more wide and accurate knowledge of reactions which is now in their possession, they have been enabled to follow up so far the study of the respective state of combination of each atom in a molecule as to arrive at simple and consistent explanations of facts which had previously eluded the grasp of science.

Our knowledge of the order of combination of atoms in a molecule and of the differences between direct and indirect combinations of particular atoms may be said to have originated chiefly in the study of the compounds of nitrogen. Thus it was found that the hydrogen in ammonia differs in many of its chemical functions from hydrogen in hydrocarbons. A base (called methylia) was discovered having a molecular composition corresponding to the empirical formula (CNH_5), and this base was found to contain two atoms of hydrogen like those of ammonia, and three atoms like those in hydrocarbons. Its constitution was accordingly represented by a formula describing it as an ammonia, in which one atom of hydrogen is replaced by the monad methyle, or, to be more explicit, as containing two atoms of hydrogen directly combined with nitrogen, and three atoms of hydrogen indirectly combined with that same atom of nitrogen through the intervening atom of carbon. Writing in juxtaposition to one another the symbols of those atoms which are directly combined, we can express the facts by the following formula, viz. H_2NCH_3.

Those marvellous varieties of matter called isomeric compounds found their natural explanation in differences of the respective arrangements of like atoms. Thus two bases were discovered having the same empirical molecular formula C_2NH_7. One of them is made by different reactions from the other, and in its decompositions differs from the other. All these chemical differences between them are found to be due to the fact that one of them (called ethylia) contains two atoms of hydrogen directly combined with the nitrogen, and the monovalent hydrocarbon ethyle in place of the third atom of hydrogen; whilst the other (called dimethylia) contains only one atom of hydrogen combined directly with nitrogen, the carbon of the two atoms of methyle completing the saturation of the trivalent nitrogen, as expressed by the formula $HN(CH_3)_2$.

It was subsequently proved that an atom of oxygen may combine with two like or unlike monads, such monads being indirectly combined with one another through the intervening atom of oxygen. Thus five of the atoms of hydrogen in common alcohol were proved to be in direct combination with the carbon, whilst the other one is indirectly combined with it through the oxygen, as expressed by the formula $HO(C_2H_5)$.

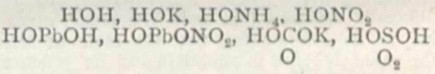
Another compound (called methyl-oxide) was proved to have the same empirical composition, but very different properties and reactions, its constitution being explained by the formula H_3COCH_3.

Again, two compounds of distinct reactions and properties were found to have the same empirical molecular composition, C_2NH_3, and it was clearly proved that in one of them the two atoms of carbon are directly combined thus, $NCCH_3$, whilst in the other they are indirectly combined through the atom of nitrogen $CNCH_3$.

An immense amount of admirable work has been done of late years (especially in Germany) in working out the evidences of the atomic order of complex organic bodies, and in thereby obtaining a command of their reactions.

Evidences of the same kind have been obtained of the atomic arrangement of some few of the simplest inorganic bodies, and it is to be hoped that ere long chemists will recognise the importance of examining the constitution of salts with the aid of the principles established in organic chemistry.

The foundation is already laid by our knowledge of the constitution of such compounds as



$$HOH,\ HOK,\ HONH_4,\ HONO_2$$
$$HOPbOH,\ HOPbONO_2,\ H\underset{O}{O}COK,\ H\underset{O_2}{O}SOH$$

and there is a strong probability regarding the atomic constitution of many other water compounds, *e.g.*

$$SO_4H_2 + H_2O = OS(OH_2)_4, SO_5H_4 + H_2O = S(OH)_6.$$

Amongst the extensions of our means of examining the physical properties of matter, and thereby discovering new varieties of matter for chemical investigation, spectrum analysis has played an important part, and is no doubt destined to do far more. It has already led chemists to the discovery of several previously unknown elements, and has led to the detection of various known elements in distant masses of which we had previously no chemical knowledge.

Up to this point the growth of the atomic theory will be seen, from the general outline which I have endeavoured to trace, to have consisted mainly in the more and more full and exact identification of each elementary atom, and in the accumulation of more and more varied and accurate evidences of its functions in relation to other atoms. A step was made towards a knowledge of the general relations of atoms to one another by their preliminary classification according to their best-known values.

But a far greater step has been more recently made, one which is evidently destined to lead to most important results.

It was discovered that if we arrange the elements in the empirical order of their respective atomic weights, beginning with hydrogen and proceeding thence step by step to the heaviest atom, we have before us a natural series with periodically ecurrent changes in the chemical and physical functions of its members.

Of course the series is imperfect, and exhibits gaps and irregularities; but what view of natural order was complete in its infancy?

Some of the gaps have already been filled up by the discovery of elements possessing the anticipated properties. The generalisation affords a brilliant addition to the previous corroborations of the reality of the units of matter which chemists have discovered.

Chemists have as yet taken but little account of atomic motion;

although the most perfect explanation of a chemical reaction consists of a statement of the atomic interchange which takes place between two molecules; or the change of mutual combination between the atoms in one molecule.

It has, however, been proved that the heat of combination affords a measure of its force; and we know that in giving off heat particles of matter undergo a diminution of velocity of motion. We see, accordingly, that substances capable of exerting great force by their combination are those which can undergo a great diminution of the velocity of their internal motions, and reciprocally.

The force of chemical combination is evidently a function of atomic motion.

It has been shown that the relative velocities of certain atomic interchanges afford a measure of the amount of chemical action between two substances; but a vast amount of work will doubtless be required to develop the atomic theory to the point of explaining the force of chemical action in precise terms of atomic motion.

The general terms of chemistry are mere symbols. Each of them serves to recall a group (usually a very large group) of facts established by observation. The explanation of each term is afforded by a careful study of the facts which it is used to denote; and, accordingly, a chain of evidence involving the use of chemical terms can be fully understood only by chemists accustomed to the consideration of such evidence. The general outline of it may perhaps be to some general thinkers of sufficient interest to attract them to further study of our science.

SECTION C

GEOLOGY

OPENING ADDRESS BY A. C. RAMSAY, LL.D., F.R.S., &c., &c., DIRECTOR-GENERAL OF THE GEOLOGICAL SURVEY, PRESIDENT OF THE SECTION

On the Origin and Progress of the Present State of British Geology, Especially since the first Meeting of the British Association at York in 1831

IN the year 1788 Hutton published his first sketch of his "Theory of the Earth," afterwards extended and explained by Playfair in a manner more popular and perspicuous than is done in Hutton's own writings. In this grand work, Hutton clearly explains that the oldest known strata, like their successors, are derivative, and that as far as *observation* can discover, in all geological time, "we find no vestige of a beginning, and no sign of an end." The complement to this far-seeing observation was at length brought about by William Smith, in his original "Geological Map of the Strata of England and Wales" in 1815, followed, in 1816, by his "Strata Identified by Organised Fossils." This great discovery, for such it was, threw a new light on the history of the earth, proving what had before been unknown, that all the "Secondary" formations, at least from the Lias to the Chalk inclusive, contained each a set of distinctive fossils by which it could be recognised. A law was thus provided for the identification of formations which geographically are often widely separated from each other, not only in England in the case of minor outliers, but also easily applicable to great areas on the neighbouring continent of Europe.

In 1811 the first volume of the *Transactions* of the Geological Society was published, and in 1826-27 there appeared the first volume of the *Proceedings*, the object being to communicate to the Fellows as promptly as possible the *Proceedings* of the Society "during the intervals between the appearance of the several parts of the *Transactions.*" The last volume of the *Transactions* contains memoirs read between the years 1845-1856, and only four volumes of the *Proceedings* appeared between the years 1826 and 1845 inclusive, after which the title of the annual volume was changed to that of the "Quarterly Journal of the Geological Society." The Geological Society, to which the science owes so much, was therefore in full action when the British Association was founded in 1831, and the memoirs read before the Society from 1831 to this date may be said to show generally the state of British geology during the last fifty years. To this must be added the powerful influence of the first (1830) and later editions of Lyell's "Principles of Geology," a work which helped to lay the foundations of those researches in Physical Geology which both in earlier and later years have attracted so much attention.

Fifty years ago, in this city, Viscount Milton was president of the first meeting of "The British Association for the Advancement of Science," which he explained had for its chief object "to give a stronger impulse and more systematic direction to scientific inquiry." In his address he pointed out the numbers of Philosophical Societies which had by degrees sprung up in all parts of the kingdom; and the practicability, through the means of the Association, "including all the scientific strength of Great Britain," "to point out the lines in which the direction of science should move."

In that year, 1831, Prof. Sedgwick was president of the Geological Society, and the Geological and Geographical Committee of the British Association recommended that geologists should examine the truth of that part of the theory of Elie de Beaumont, in its application to England, Scotland, and Ireland, which asserts that *the lines of disturbance of the strata assignable to the same age are parallel;* that Prof. Phillips be requested to draw up *a systematic catalogue of all the organised fossils of Great Britain and Ireland;* and that Mr. Robert Stephenson, civil engineer, be requested to prepare a report upon *the waste and extension of the land on the east coast of Britain, and the question of the permanence of the relative level of the sea and land.*

In 1881 it seems strange to us that, in 1831, with William Smith's map of "The Strata of England and Wales, with part Scotland," before them, it should have been considered necessary to institute an inquiry as to the truth of the general parallelism of disturbed strata, which, in a limited area like England, had suffered upheaval at different successive epochs; and we may fancy the internal smile with which Phillips, the nephew of Smith, regarded the needless proposal. The masterpiece of the old land-surveyor and civil engineer remains to this day the foundation of all subsequent geological maps of England and Wales; and *as an unaided effort of practical genius*—for such it was—it seems impossible that it should be surpassed, in spite of all the accuracy and detail which happily modern science has introduced into modern geological maps.

The first paper read at York, in the year 1831, was by Prof. Sedgwick, "On the General Structure of the Lake Mountains of the North of England." This was followed by "Supplementary Observations on the Structure of the Austrian and Bavarian Alps," by the Secretary of the Society, Mr. Murchison, a memoir at that time of the highest value, and still valuable both in a stratigraphical point of view and also for the light which it threw on the nature of the disturbances that originated the Alpine mountains, and their relations in point of date to the far more ancient mountains of Bohemia. In his elaborate address in the same year, on his retiring from the president's chair, he largely expatiates on the parallelism of many of the great lines of disturbance of what were then distinguished as the more ancient *schistose* and *greywacké* mountains, and quotes the authority of Elie de Beaumont for the statement, "that mountain chains elevated at the same period of time have a general parallelism in the bearing of their component strata." On a great scale this undoubtedly holds true, as, for example, in the case of the Scandinavian chain, and the more ancient Palæozoic rocks north of Scotland, Cumberland, and even of great part of Wales. The same holds good with regard to the parallelism of the much more recent mountain ranges of the Apennines, the Alps, the Caucasus, the Atlas, and the Himalayas, all of which strike more or less east and west, and are to a great extent of post-Eocene, and even partly of post-Miocene age. The same, however, is not precisely the case with the Apalachian chain and the Rocky Mountains of North America, the first of which trends N.N.W., and the latter N.N.E. The remarkable chain of the Ural Mountains trends nearly true north and south, and is parallel to no other chain that I know of, unless it be the Andes and the mountains of Japan. It is worthy of notice that the chain of the Ural is of pre-Permian age according to Murchison, while Darwin has shown that the chief upheaval of the Andes took place in post-Cretaceous times.

The Apalachian chain is chiefly of post-Carboniferous date, and the Rocky Mountains have been re-disturbed and re-elevated as late as post-Miocene times.

In the same address Prof. Sedgwick entered an eloquent protest against the broad uniformitarian views so powerfully advocated in the first edition of Lyell's "Principles of Geology" in 1830, in which, throwing aside all discussion concerning cosmogony, he took the world as he found it, and, agreeing with Hutton that geology is in no way concerned with, and not sufficiently advanced to deal "with questions as to the origin of things," he saw that a great body of new data were required, such as engaged the attention of the Geological Society (founded in 1807), and which, along with other foreign societies and private work, has at length brought geological science to its present high position.

And what is that position? With great and consentient labour, many men, gifted with a knowledge of stratigraphical and palæontological geology, have, so to speak, more or less dissected all the regions of Europe and great part of North America, India, and of our colonies, and in vast areas, sometimes nearly adjoining, and sometimes far distant from each other, the various formations, by help of the fossils they contain, have been correlated in time, often in spite of great differences in their lithological characters. It is easy, for example, to correlate the various formations in countries so near as Great Britain and Ireland, or of the Secondary and Lower Tertiary formations of England and France; and what is more remarkable, it is easy to correlate the Palæozoic formations of Britain and the eastern half of the United States and Canada, even in many of the comparatively minute stratigraphical and lithological subdivisions of the Silurian, Devonian, and Carboniferous formations. The same may be said with regard to some of the Palæozoic formations of India, China, Africa, and Australia, and many of the Secondary and Tertiary deposits have in like manner been identified as having their equivalents in Europe. It is not to be inferred from these coincidences that such deposits were all formed *precisely* at the same time, but taken in connection with their palæontological contents, viewed in the light which Darwin has shown with regard to the life of the globe when considered in their relation to masses of stratified formations, no modern geologist who gives his mind to such subjects would be likely to state, for example, that in any part of the globe Silurian rocks may be equivalents in time to any of our Upper Palæozoic, Mesozoic, or Tertiary formations.

For all the latest details of *genera* and *species* found in the British Palæozoic rocks, from those of St. David's, so well worked out by Dr. Hicks, to the Carboniferous series inclusive, I must refer to the elaborate address of Mr. Etheridge, President of the Geological Society, which he delivered at the last anniversary meeting of that society. It is a work of enormous labour and skill, which could not have been produced by any one who had not a thorough personal knowledge of all the formations of Britain and of their fossil contents.[1]

In connection with such subjects I will not in any way deal with the tempting and important subject of cosmological geology, which in my opinion must go back to times far anterior to the date of the deposition, as common sediments, of the very oldest-known metamorphic strata. Cosmological speculations perhaps may be sound enough with regard to refrigeration, and the first consolidation of the crust of the earth, but all the known tangible rocky formations in the world have no immediate relation to them, and in my opinion the oldest Laurentian rocks were deposited long after the beginning and end of lost and unknown epochs, during which stratified rocks were formed by watery agents in the same way that the Laurentian rocks were deposited, and in which modern formations are being deposited now, and the gneissose structure of the most ancient formations was the result of an action which has at intervals characterised all geological time as late as the Eocene formations in the Alps and elsewhere.

The same kind of chronological reasoning is often applicable to igneous rocks. It was generally the custom, many years ago, to recognise two kinds of igneous rocks, viz., Volcanic and Plutonic, and this classification somewhat modified in details is still applicable, the Plutonic consisting chiefly of granitic rocks and their allies, and which though they have often altered and thrust veins into the adjoining strata, have never, as far as I know, overflowed in the manner of the lavas of modern and ancient volcanoes. Indeed, as far as I recollect, the first quoted examples of ancient volcanoes are those of Miocene age in the districts of Auvergne, the Velais, and the Eifel, and the fact that signs of ordinary volcanic phenomena are found in almost all the larger groups of strata was scarcely suspected. Now, however, we know them to be associated with strata of all or almost all geological ages, from Lower Silurian times down to the present day, if we take the whole world into account. Amongst them, those of Miocene date hold a very prominent place, greatly owing, doubtless, to the comparative perfection of their forms, as, for example, those of the South of France and of the Eifel. Their conical shapes, and numerous extinct craters, afford testimony so plain, that he who runs may read their history. The time when they became extinct would doubtless amaze us by its magnitude, if it could be stated in years, but yet it is comparatively so recent that not all the undying forces of atmospheric degradation have been able to obliterate their individual origin.

It is, however, generally very different with respect to volcanoes of Mesozoic age, for though Lyell stated with doubt, that volcanic products of Jurassic date are found in the Morea and in the Apennines; and Medlicott and Blanford consider that probably the igneous rocks of Rajmahal may be of that age, we must, perhaps, wait for further information before the question may be considered as finally settled. Of Jurassic age no actual craters remain. Darwin also has stated, on good grounds, that in the Andes a line of volcanic eruptions has been at work from before the deposition of the Cretaceo-oolitic formation down to the present day.

In the British Islands we have a remarkable series of true volcanic rocks, the chronology of which has been definitely determined. The oldest of these belongs to the Lower Silurian epoch, as shown, for example, on a large scale in Pembrokeshire, at Builth in Radnorshire, in the Longmynd country west of the Stiper stones in Shropshire, and on a far greater scale in North Wales and Cumbria. Of later date we find volcanic lavas and ashes in the Devonian rocks of Devon, and in the Old Red Sandstone of Scotland. The third series is plentiful among the Carboniferous rocks of Scotland, and in a smaller way interstratified with the Coal-measures of South Staffordshire, Warwickshire and the Clee Hills. The fourth series chronologically is associated with the Permian strata in Scotland, and the fifth and last consists of the Miocene basaltic rocks of the Inner Hebrides and the mainland of the West of Scotland.

In the British Islands the art of geological surveying has, I believe, been carried out in a more detailed manner than in any other country in Europe, a matter which has been rendered comparatively easy by the excellence of the Ordnance Survey maps both on the 1-inch and the 6-inch scales. When the whole country has been mapped geologically little will remain to be done in geological surveying, excepting corrections here and there, especially in the earliest published maps of the South-west of England. Palæontological detail may, however, be carried on to any extent, and much remains to be done in microscopic petrology which now deservedly occupies the attention of many skilled observers.

Time will not permit me to do more than advert to the excellent and well-known geological surveys now in action in India, Canada, the United States, Australia, New Zealand, and South Africa.

On the Continent of Europe there are National Geological Surveys of great and well-deserved repute conducted by men of the highest eminence in geological science, and it is to be hoped the day may come when a more detailed survey will follow the admirable map executed by Sir Roderick Murchison, De Verneuil, and Count Keyserling, and published in their joint work, "The Geology of Russia in Europe and the Ural Mountains."

It is difficult to deal with the Future of Geology. Probably in many of the European formations more may be done in tracing the details of subformations. The same may be said of much of North America, and for a long series of years a great deal must remain almost untouched in Asia, Africa, South America, and in the islands of the Pacific Ocean. If, in the far future, the day should come when such work shall be undertaken, the process of doing so must necessarily be slow, partly for want of proper maps, and possibly in some regions partly for the want of trained geologists. Palæontologists must always have ample work in the discovery and description of new fossils, marine, freshwater, and truly terrestrial; and besides common stratigraphical geology, geologists have still an ample field before them in working out many of those physical problems which form the true basis of Physical Geography in every region of the earth. Of the history of the earth there is a long past, the early chapters of which seem to be lost for ever, and we know little of the future except that it appears that "the stir of this dim spot which men call earth," as far as Geology is concerned, shows "no sign of an end."

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[1] I must also, with much pleasure, advert to Prof. Prestwich's inaugural lecture when installed in the Chair of Geology at Oxford in 1875, the subject of which is "The Past and Future of Geology."

SECTION D

BIOLOGY

Opening Address by Richard Owen, C.B., F.R.S., President of the Section

The recent construction of the edifice of the British Museum (Natural History), Cromwell Road,[1] and the transference thereto of three of the Departments, the systematic arrangement of which in their respective galleries approaches closely to completion, have left me little leisure in the present year for other scientific work. The expression, moreover, in divers forms and degrees of the satisfaction and instruction such partial exhibition of the national treasures of natural history has afforded to all classes of visitors since the galleries were open to the public, in April last, encourages me to believe that a few words on this great additional instrument in advancing biological science may not be unacceptable to the Section of the British Association which I have now the honour to address.

It is true that when we last met at Swansea, my accomplished colleague, Dr. Albert Günther, F.R.S., selected a general description of the building as the subject of his address to Section D.

I was unwilling then, in consideration of the time of the Section already given to the matter, to respond to appeals of some of our fellow-members for information as to how, and through whom, the new Museum came to be, and to be where it is; but now, honoured by my present position, I venture to hope that a brief outline of its genetic history, which I have been preparing for publication in a fuller form, may be condoned.

In the actual phase of our Science, its cultivators, especially the younger generation, do not rest upon the determination and description, however minute and exhaustive, of the acquisitions so rapidly accumulating of objects or "new species"; but devote themselves also, and more especially, to the investigation of their developmental phenomena.

It has, therefore, seemed to me that it would not be inappropriate, as being germane to the present phase of research, to submit to the Section a few words on the genesis of this new national edifice, generously provided by the State for the promotion of Biology.

On the demise, in 1856, of Sir Henry Ellis, K.T., then Principal Librarian of the British Museum, the Government, made aware of the growth of the Departments of natural history, more especially of geology and palæontology, since the foundation of the Museum in 1753, when the collections of printed books and manuscripts predominated, determined that, together with a principal librarian, there should be associated a new official having special charge of the collections of natural history, but under similar subordinate relations to the Trustees. To this official was assigned the title of "Superintendent of the Departments of Natural History," and I had the honour to be selected for this office.[2]

Almost my first work was to ascertain the extent of my charges, and I confess that I was unprepared to find that the galleries assigned for the arrangement and public exhibition of the several natural history series in the British Museum were so inadequate to these ends as to necessitate the storage of many unexhibited, and in great proportion rare and valuable specimens. This condition affected principally the collection of fossil remains, but in not much less degree that of the recent natural history.

One of my colleagues, Mr. Charles König, then Keeper of the Department of Mineralogy, and most eminent in that science, applied the gallery assigned thereto principally to the rare and beautiful specimens of his favourite subject. When the newer science of palæontology entered upon its rapid growth, and, on the demise of Mr. König, led to the formation of a distinct Department of Geology, the proportion of the British Museum set apart for natural history could not afford for the exhibition of the fossils and rock specimens more or other space than might be gained from or intercalated among the mineral cabinets in one and the same gallery, viz. that which had been originally assigned to Mr. König.

The store-vaults in the basement of the Museum became accordingly invaded by the rapidly-accumulating unexhibited geological specimens, as those receptacles had been, and continued to be, needed for the storage of such specimens, and especially the osteological ones, of the Department of Zoology.

In 1854 Dr. John Ed. Gray, Keeper of the Zoology, reported on the unfitness of the locality of his stored specimens, and prayed for additional accommodation for them.[1] But, on the report of the architect, to whom such appeal was referred, the Trustees "declined to adopt Dr. Gray's suggestion," and recommended "that steps should be taken to obviate the deterioration of the specimens complained of by Dr. Gray in consequence of the damp condition of the vaults in which they are contained."[2] To renewed appeals by the experienced Keeper, and agreeably with his ideas on the nature and extent of the required additional space for the zoology, the Trustees recommended:—"An additional gallery to the Eastern Zoological Gallery, and the substitution of skylights for the side windows," with a view to an additional gallery at an elevation above the floor of the one in use; they also resolved:—"That accommodation be provided for the officers of the Natural History Departments on the roof of the Print-room."[3]

But the inadequacy for exhibition purposes of additional space which might be gained by the new gallery, or by the accessory wall-gallery attainable by stairs in the one in use,[4] was so impressed on my convictions, that I determined, in 1857, to submit to the Trustees a statement embodying estimates of space required for exhibition of all and several the departments of natural history, with the grounds of such estimates, including considerations based upon the ratio of increase during the ten years preceding my appointment, and the conditions likely to affect the proportional number of future annual additions.

This purpose, which I deemed a duty, I endeavoured to effect in a "Report, with a Plan," submitted on February 10, 1859, which Report, being forwarded by the Trustees to the Treasury, and being deemed worthy of consideration by Parliament, was "Ordered by the House of Commons to be printed, 11th March, 1859," and can still be obtained at the Office of Parliamentary Papers or Blue Books.[5]

The Report included, as I have stated, estimates of space for the then acquired specimens of the several departments of natural history, together with space for the reception of the additional specimens which might accrue in the course of a generation, or thirty years. It further recommended that such museum-building, besides giving the requisite accommodation to the several classes of natural history objects, as they had been by authority exhibited and arranged for public instruction and gratification, should also include a hall, or exhibition-space for a distinct department, adapted to convey an elementary knowledge of the subjects of all the divisions of natural history to the large proportion of public visitors not specially conversant with any of those subjects.

I may crave permission to quote from that part of my Report which has received the sanction of the "Commission on the Advancement of Science" of 1874: "One of the most popular and instructive features in a public collection of natural history would be an apartment devoted to the specimens selected to show type-characters of the principal groups of organised and crystallised forms. This would constitute an epitome of natural history, and should convey to the eye in the easiest way an elementary knowledge of the sciences."[6]

An estimate of the space required for such apartment is given, and it has been obtained in the new Museum of Natural History.

I ventured also on another topic in connection with the more immediate object of my Report. Previous experience at the museum of the Royal College of Surgeons had impressed me with the influence on improved applications of collections and on the ratio of their growth, through Lectures expository of their nature. I felt confident that, with concurrence of authorities, both relations would be exemplified under the actual superintendence at the British Museum. Moreover, such museum of natural history has wider influences over possessors and collectors of rarities and of desiderated specimens than one of restricted kind, as in Lincoln's Inn Fields. I concluded my Report, therefore, by referring to the lecture theatre shown in

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[1] The official designation assigned by the Trustees to the building and its contents.

[2] The date of my appointment is May 26, 1856.

[1] See Parliamentary Paper, or Blue Book, folio 1858, entitled:—"Copies of all Communications made by the Officers and Architect of the British Museum to the Trustees, respecting the want of space for exhibiting the Collections in that Institution," p. 4. [2] *Ib.* p. 5. [3] *Ib.* p. 25 and p. 28.

[4] In his report of December 29. 1856, Dr. Gray states:—"Scarcely half of the zoological collections is exhibited to the public, and their due display would require more than twice the space devoted to them."—*Ib.* p. 21. To any removal of the natural history to another site Dr. Gray was strongly opposed.

[5] Parliamentary Papers, "Report with Plan," &c. (126, i.), fol. 1859.

[6] Report, *ut supra*, p. 22.

my plan, and expressed my belief that "Administrators will consider it due to the public that the gentlemen in charge of the several departments of the National Collection of Natural History should have assigned to them the duty of explaining the principles and economical relations of such departments, in elementary and free lectures, as, *e.g.* on Ethnology, Mammalogy, Ornithology, Herpetology and Ichthyology, Malacology and Conchology, Entomology, Zoophytology, Botany, Geology, Palæontology, Mineralogy."

After the lapse of twenty years I have lived to see the fulfilment of all the recommendations, save the final one, of my Report of 1859. The lecture-theatre was erased from my plan, and the elementary courses of lectures remain for future fulfilment.

Considering that, in the probable communication of this Report to Parliament, I was addressing the representatives of the greatest commercial and colonising nation in the globe, representatives of an empire exercising the widest range of navigation and supreme in naval power, such nation and empire might well be expected by the rest of the civilised world to offer to students and lovers of natural history the best and noblest museum of the illustrations of that great division of general science.

But for such a museum, a site or superficial space of not less than eight acres was asked for, the proportion of such space to be occupied by the proposed building being, at first, limited, and dependent upon its architectural arrangement in one, two, or more storeys. But the effect of restricting the site or available superficial space to that, *e.g.* on which the Museum at Bloomsbury now stands, was significantly demonstrative of difficulties to come, and concomitantly indicative of the administrative wisdom which would be manifested by securing, in a rapidly growing metropolis, adequate space for future additions to the building which might be in the first place erected thereupon.

Nevertheless one or two of my intimate and confidential friends dissuaded me from sending in a Report which might be construed or misinterpreted as exemplifying a character prone to inconsiderate and extravagant views, and such as might even lead to disagreeable personal consequences. Moreover the extent of space reported for seemed inevitably to involve change of locality. Two of my colleagues occupied the elegant and commodious residences attached to the British Museum; and it was possible that provision for such residences marked in the plan which accompanied my Report might not be adopted. Moreover no statement of grounds for adequate space requirements for the whole of the National Museum of Natural History had previously been submitted to authority. The legislative mind had not been prepared for calm and due consideration of the subject. Still I flattered myself that, by whomsoever the details and aims and grounds of my Report were known and comprehended, any strong opposition on the part of Parliament could hardly be expected. Nevertheless, an Irish Member seeing a way to a position in the House which is gained by the grant of a Committee of Inquiry, of which the Mover becomes Chairman, made my Report and Plan the ground of a motion to that effect, which was carried. The Select Committee, after taking the evidence published in the Blue Book (ordered to be printed August 10, 1860, quarto, pp. 238, with ten plans), reported against the removal of the Natural History Collections from the British Museum. As to the chief reasons alleged for such removal the Report states that with one "eminent exception the whole of the scientific naturalists examined before your Committee, including the Keepers of all the Departments of Natural History in the British Museum, are of opinion that an exhibition on so large a scale tends alike to the needless bewilderment and fatigue of the public, and the impediment of the studies of the scientific visitor . . . . Your Committee, therefore, recommend the adoption of the more limited kind of exhibition advocated by the other witnesses, in preference to the more extended method recommended by Prof. Owen."

Lest however the House might attach undue weight to the exceptional testimony, the chairman of the Committee deemed it his duty, in bringing up the Report, to warn the House of the character of such testimony, and his speech left, as I was told, a very unfavourable impression as regards myself. I was chiefly concerned to know what might be put upon record in "Hansard." In that valuable work hon. members revise their reported utterances before the sheets go to press. I was somewhat relieved to find Mr. Gregory regretting that "a man whose name stood so high should connect himself with so foolish, crazy, and extravagant a scheme, and should persevere in it after the folly had been pointed out by most unexceptionable witnesses. . . . "They had on one side, and standing alone, Prof. Owen and his ten-acre scheme, and on the other side all the other scientific gentlemen, who were perfectly unanimous in condemning the plan of Prof. Owen as being utterly useless and bewildering. . . . "Among these gentlemen were Prof. Huxley, Prof. Maskelyne, Mr. Waterhouse, Dr. Gray, Sir Roderick Murchison, Mr. Thomas Bell, P.G.S., Dr. Sclater, Sec.Z.S., Mr. Gould, and Sir Benjamin Brodie. To give the House some idea of that gigantic plan, he might mention that a part of it consisted of galleries 850 feet in length for the exhibition of whales. The scientific men examined on the subject, one and all, disapproved of that plan *in toto*; and they advocated what was technically called a 'typical mode of exhibition.'"[1]

In point of fact that Supplementary Exhibition Room which was planned and recommended for the purpose I have already cited, was urged by the instructor of Mr. Gregory as the sole reasonably required National Museum of Natural History, for which the nation ought to be called upon to provide space and funds, a conclusion subsequently adopted and unanimously recommended by the Royal Commission on Science.[2]

Although grief was natural and considerable at this result, not without mortification at the reception by Parliament of the "Report and Plan" submitted thereto, I now feel grateful that the sole responsibility of their author is attested in the pages of a Work[3] which will last as long as, and may possibly outlast, the great legislative organisation whose debates and determinations are therein authoritatively recorded.

I was not, however, cast down, nor did I lose either heart or hope; I was confident in the validity of the grounds of my appeal, and foresaw in the inevitable accumulations year by year, the evidence which would attest its soundness and make plain the emergency of the proposed remedy.

Moreover, there was one who, though not a naturalist, had devoted more time, pains, and thought to the subject than had been bestowed by any—whether naturalist or administrator—who testified adversely thereon. The Right Hon. William Ewart Gladstone, an elected Trustee of the British Museum, took nothing on trust; he explored with me in 1861 every vault and dark recess in the Museum which had been or could be allotted to the non-exhibited specimens of the natural history, those, viz. which it was my aim to utilise and bring to light. He gave the same attention to the series selected for exhibition in the public galleries, and appreciated the inadequacy of the arrangements to that end. He listened to my statements of facts, to the grounds of prevision of annual ratios of increase, to the reasons for providing space therefor, to my views of the aims of such exhibitions, and to the proposed extended applications and elucidations of the collections. Mr. Gladstone tested every averment, and elicited the grounds of every suggestion, with a tact and insight that contrasted strongly with the questionings in Mr. Gregory's committee-room, where too often vague interrogations met with answers to match.

Conformably with Mr. Gladstone's convictions, he as Chancellor of the Exchequer moved, May 12, 1862, for "Leave to bring in a Bill for removal of portions of the Trustees' Collections in the British Museum."

On May 19, when the Bill was to be read a second time, a new, unexpected, and formidable antagonist arose. Mr. Disraeli early got the attention of the House to a speech, warning hon. members of the "progressive increase of expenditure on civil estimates," and laying stress on the fact that the "estimates of the actual year showed no surplus."[4] The influence of this advocacy of economy is exemplified in the debate which ensued. For repetitions of the nature and terms of objections to the Report and plan, as already denounced by Mr. Gregory, Mr. Bernal Osborne, and others, reference may be made to the volume of "Hansard" cited below. An estimable hon. member, whose words had always and deservedly carried weight with the country party, lent his influence to the same result. Mr. Henley, representative of Oxfordshire, said:—"All the House knew was that a building was to be put up somewhere. He considered this a bad way of doing business, particularly at a time when nobody could be sanguine that the finances of the country were in a flourishing state. Let the stone once be set rolling, and then all gentlemen of science and taste would have a kick at it, and it would be knocked from one

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[1] "Hansard." Debate of July 22, 1861, pp. 1861, 1918.

[2] Fourth Report, p. 4.

[3] "Hansard," *ut supra*.

[4] *Ib.* 1862, p. 1927.

to the other, and none of them probably would ever live to see an end of the expense."[1]

Permit me to give one more example of the baneful influence of the opening speech on our great instrument of scientific progress. Mr. Henry Seymour, Member for Poole, said:—"If a foreigner had been listening to the debates of that evening it must have struck him that it was, to say the least, a rather curious coincidence that a proposal to vote 600,000*l.* for a new collection of birds, beasts, and fishes at South Kensington should have been brought forward on the very evening when the Leader of the Opposition had made a speech denouncing that exorbitant expenditure—a speech, he might add, which was re-echoed by many Liberal members of the House."[2]

It was however not a "curious," but a "designed coincidence." Mr. Disraeli, knowing the temper of the House on the subject, and that the estimates for the required Museum of Natural History were to be submitted by Mr. Gladstone, chose the opportunity to initiate the business by an advocacy of economy which left its intended effect upon the House. In vain Lord Palmerston, in reply to the Irish denunciators, proposed as a compromise to "exclude whales altogether from disporting themselves in Kensington Gardens."[3] The Government was defeated by a majority of ninety-two, and the erection of a National or British Museum of Natural History was postponed, to all appearance indefinitely, and in reality for ten years.

Nevertheless, neither averments nor arguments in the House on May 19, 1862, nor testimonies in the hostile Committee of 1860, 1861, had shaken my faith in the grounds on which the "Report and Plan of 1859" had been based. The facts bearing thereupon, which it was my duty to submit in my "Annual Reports on the Natural History Departments of the British Museum," would, I still hoped, have some influence with hon. members of the legislature, to whom those Reports are transmitted.

The annual additions of specimens continued to increase in number and in value year by year. I embraced every opportunity to excite the interest of lovers of natural history travelling abroad and of intelligent settlers in our several colonies to this end, among the results of which I may cite the reception of the Aye-Aye, the Gorilla, the Dodo, the Notornis, the maximised and elephant-footed species of Dinornis, the representatives of the various orders and genera of extinct Reptilia from the Cape of Good Hope, and the equally rich and numerous evidences of the extinct Marsupialia from Australia, besides such smaller rarities as the animals of the Nautilus and Spirula.

Wherever room could be found in the exhibition galleries at Bloomsbury for these specimens, stuffed or as articulated skeletons, or as detached fossils, they were squeezed in, so to speak, to mutely manifest to all visitors, more especially administrative ones, the state of cram to which we were driven at Bloomsbury.

Another element of my Annual Reports was the deteriorating influence on valuable specimens of the storage vaults and the danger of such accumulations to the entire Museum and its priceless contents. And here perhaps you may deem some explanation needful of the grounds of the latter consideration addressed to economical granters of the national funds.

The number of specimens preserved in spirits of wine amounted to thousands; any accidental breakage, with conflagration, in the subterraneous localities contiguous with the heating-apparatus of the entire British Museum, would have been as destructive to the building as the gunpowder was meant to be when stored in the vaults beneath King James's Houses of Parliament.

At this crisis the "Leading Journal," after the stormy debate of May 19, 1862, made the following appeal to me:—"Let Mr. Owen describe exactly the kind of building that will answer his purpose, that will give space for his whales and light for his humming-birds and butterflies. The House of Commons will hardly, for very shame, give a well-digested scheme so rude a reception as it did on Monday night."[4]

My answer to this appeal was little more than some amplification, with additional examples, of the several topics embodied in the original Report. The pamphlet "On the Extent and Aims of a National Museum of Natural History," with reduced copies of the plans, went through two editions, and no doubt had the effect anticipated by the able Editor.

Another element of reviving hope was the acceptance by Mr. Gregory of the government of a tropical island.

The sagacious Prime Minister accurately gauged the modified feeling—the subsiding animosity—of Parliament on the subject, and submitted (June 15, 1863) a motion "for leave to purchase five acres for the required Natural History building." The choice of locality he left to honourable members. Lord Palmerston pointed out that the requisite extent of site could be obtained at Bloomsbury for 50,000*l.* per acre, and that it could be got at South Kensington for 10,000*l.* per acre; and his lordship distinctly stated that the space, in either locality, would be bought for the purpose of a Museum of Natural History. The purchase of the land at South Kensington was accordingly voted by 267 against 135, and thus the Government proposition was carried by a majority of 132. By this vote the decision of Mr. Gregory's Committee was virtually annulled.

In a conversation with which I was favoured by Lord Palmerston, I interposed a warning against restriction of space, and eventually eight acres of ground were obtained, including the site of the Exhibition Building of 1862, opposite Cromwell Gardens, and that extent of space is now secured for actual and prospective requirements of our National Museum of Natural History.

I am loth to trespass further on the time of the Section, but a few words may be expected from me of the leading steps to the acquisition of the present edifice, occupying a portion—about one-third—of that extent of ground.

Mr. Gladstone, adhering to the convictions which led him to submit his financial proposition of May, 1862, honoured me, at the close of that session of Parliament, with an invitation to Hawarden to discuss my plans for the Museum Building; and, after consideration of every detail, he requested that they might be left with him. He placed them, with my written expositions of details, in the hands of Sir Henry A. Hunt, C.B., responsible adviser on buildings, &c., at the Office of Works, with instructions that they should be put into working form, so as to support reliable estimates of cost. I was favoured with interviews with Sir Henry, resulting in the completion of such working plans of a museum, including a central hall, an architectural front of two storeys, and the series of single-storeyed galleries extending at right angles to the front, as shown in my original Plan. I was assured that such plan of building affording the space I had reported on, would be the basis to be submitted to the professional Architect whenever the time might arrive for Parliamentary sanction to the cost of such building.

Here I may remark that experiments which preceded the substitution, in 1835, of the actual Museum of the Hunterian Physiology at the Royal College of Surgeons, for the costly, cumbrous, and ill-lit building, with its three-domed skylights, which preceded it, had led to the conclusion that the light best fitted for a museum was that in which most would be reflected from the objects and least directly strike upon the eye; and this was found to be effected by admittance of the light at the angle between the wall and roof. But this plan of illumination is possible only in galleries of one storey, or the topmost in a many-storeyed edifice. Such system of illumination may be seen in every gallery of the museum described to you last year at Swansea, save those of the storeys of the main body below the sky-lit one which necessitate side windows.

I subjoin a copy of the letter from Sir Henry A. Hunt, conveying his conclusions respecting the plan of building discussed with him:—

"4, *Parliament Street, September* 25, 1862

"MY DEAR SIR,—I return you the drawings of the proposed Museum of Natural History at South Kensington. In May last I told Mr. Gladstone that the probable cost of covering five acres with suitable buildings would be about 500,000*l.*, or 100,000*l.* per acre.

"The plan proposed by you will occupy about four acres, and will cost about 350,000*l.*, or nearly 90,000*l.* per acre.

"Having prepared sketches showing the scheme suggested by you, I have been able to arrive more nearly at the probable cost than I had the means of doing in May last. But, after all, the difference is not great; although the present estimate is a more reliable one than the other. It is right, however, to state that the disposition of the building as proposed by you will give a greater amount of accommodation, and admit of a cheaper mode of construction, than I had calculated upon in May (relatively with the space intended to be covered), and therefore I think your plan far better adapted for the Museum than the plan I took the liberty to suggest to Mr. Gladstone.

"Believe me, &c.,

"(Signed) HENRY A. HUNT"

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[1] "Hansard," p. 1932.

[2] *Ib.* 1862, p. 1918.

[3] *Ib.* p. 1931.

[4] The *Times*, May 21, in a leader on the Museum Debate.

Sir H. A. Hunt had previously formed an estimate of cost for the Chancellor of the Exchequer on inspection of the Report and plan in the Parliamentary paper of March, 1859. The letter to which I refer I regard as an antidote to some previous quotations from adverse members of Parliament.

The working plans of Sir Henry A. Hunt were subsequently submitted for competition, and the designs of the accomplished and lamented Capt. Fowke, R.E., obtained the award in 1864. His untimely death arrested further progress or practical application of the prize designs.

In 1867 Lord Elcho pressed upon the House of Commons, through the Hungerford Bridge Committee, the Thames Embankment as a site for the New Museum of Natural History, but unsuccessfully. The debates thereon, nevertheless, caused some further delay.

In 1871 a vote of 40,000*l.* for beginning the Museum Buildings at South Kensington was carried without discussion. In 1872 a vote of 29,000*l.* for the same building was opposed by Lord Elcho, but was carried by a majority of 40 (85 against 45).

On the demise of Capt. Fowke Mr. Alfred Waterhouse was selected as architect. He accepted the general plans which had been sanctioned and approved by Sir H. A. Hunt and by Capt. Fowke, and I took the liberty to suggest, as I had previously done to Capt. Fowke, that many objects of natural history might afford subjects for architectural ornament; and at Mr. Waterhouse's request I transmitted numerous figures of such as seemed suitable for that purpose. I shall presently refer to the beautiful and appropriate style of architecture which Mr. Waterhouse selected for this building, but am tempted to premise a brief sketch of what I may call the "Genealogy of the British Museum," or what some of my fellow labourers, agreeably with the actual phase of our science, may prefer to call its "Phylogeny."

Sir Hans Sloane, M.D., after a lucrative practice of his profession in the then flourishing colony of Jamaica, finally settled at Chelsea, and there accumulated a notable museum of natural history, antiquities, medals, cameos, &c., besides a library of 50,000 volumes, including about 350 portfolios of drawings, 3500 manuscripts, and a multitude of prints. These specimens were specified in a MS. catalogue of thirty-eight volumes in folio, and eight volumes in quarto. Sir Hans valued this collection at the sum of 80,000*l.*; but at his death, in 1753, it was found that he had directed in his "Will" that the whole should be offered to Parliament for the use of the public on payment of a minor sum, in compensation to his heirs. This offer being submitted to the House of Commons, it was agreed to pay 20,000*l.* for the whole. At the same time the purchase of the Cottonian Library and of the Harleian MSS. was included in the Bill.[1]

The following are the terms of the enactment:—

Act 26, George II., Cap. 22 (1753).—*Sections IX. ana X.*

"(IX.) And it be enacted by the authority aforesaid, that within the cities of London or Westminster or the suburbs thereof, one general repository shall be erected or provided in such convenient place and in such manner as the trustees hereby appointed, or the major part of them, at a general meeting assembled, shall direct for the reception not only of the said museum or collection of Sir Hans Sloane, but also of the Cottonian Library and of the additions which have been or shall be made thereunto by virtue of the last will and testament of the said Arthur Edwards, and likewise of the said Harleian collection of manuscripts and of such other additions to the Cottonian Library as, with the approbation of the trustees by this Act appointed, or the major part of them, at a general meeting assembled, shall be made thereunto in manner hereinafter mentioned, and of such other collections and libraries as, with the like approbation, shall be admitted into the said general repository, which several collections, additions, and library so received into the said general repository shall remain and be preserved therein for public use to all posterity.

"(X.) Provided always that the said museum or collection of Sir Hans Sloane, in all its branches, shall be kept and preserved together in the said general repository whole and entire, and with proper marks of distinction."

The trustees appointed under the Act are of four classes: Royal, Official, Family, and Elected. The first class includes one trustee appointed by the Sovereign; the second class includes the Lord Archbishop of Canterbury, the Lord High Chancellor, the Speaker of the House of Commons, and twenty-two other high officials and presidents of societies. The three first in this class are designated "Principal Trustees," and in them is vested the patronage or appointment to every salaried office save one in the British Museum; the exception being the Principal Librarian, who is appointed by the Sovereign. Of the Family Trustees, the Sloane collections are now represented by the Earl of Derby and the Earl of Cadogan, the Cottonian Library by the Rev. Francis Annesley and the Rev. Francis Hanbury Annesley, the Harleian manuscripts by Lord Henry, C. G. Gordon-Lennox, M.P., and by the Right Hon. George A. F. Cavendish Bentinck, M.P. Among the Elected Trustees the honoured name of Walpole, associated with the origin of the British Museum, is continued by the Right Hon. Spencer Horatio Walpole, M.P., to whom the requisite Parliamentary business of the Museum is usually confided.

I may call attention to "the suburbs of London or Westminster" as one of the localities specified in the original Act of Parliament, and such situation was selected for the locality of the Library and the Museum. The Government issued lottery-tickets to the amount of 300,000*l.*, out of the profits of which the 20,000*l.* for the Sloanian Museum was paid and purchase made of a suitable building, with contiguous grounds for its reception and the lodgment of keepers.

To the north of the metropolis, about midway between the two cities of London and Westminster, there stood, in 1753, an ancient family mansion called Montague House. This is defined by Smollet in his "History of England" as "one of the most magnificent edifices in England."[1] Its style of architecture was that of the Tuilleries in Paris. From London it was shut off by a lofty brick wall, in the middle of which was a large ornamental gateway and lodge, through which, in my earlier years as a student of natural history, I have often passed to inspect, through the kindness of the then keepers of mineralogy and zoology, and make notes on, the Sloanian and subsequently-added rarities.

To the north of Montague House were the extensive gardens, beyond which stretched away a sylvan scene to the slopes of Highgate and Hampstead Hills.

The original location of the British Museum was more apart and remote from the actual metropolis and less easy of access than is the present Museum of Natural History at the West End.

The additions to the natural history series, which accrued from 1753 to 1833, together with the growth of other departments, necessitated provision of corresponding conservative and exhibition spaces. These were acquired by the erection, on the site of Montague House, of the present British Museum, the architect, Sir Sidney Smirke, adopting the Ionic Greek style.

The extent of space afforded by this edifice in comparison with that of its predecessor was such as to engender a conviction that it would suffice for all subsequent additions. The difficulty in our finite nature and limited capacity of looking forward is exemplified in such names as New College at Oxford, Newcastle, New Street, New Bridge, &c., as if nothing was ever to grow old; and the same restricted power of outlook affects our provision of requirements of space for ever-growing collections.

The Printed Book Department, which took the lion's share of the then new British Museum, found itself compelled in the course of one generation to appropriate the quadrangle left by Smirke in order to admit light to the windows of the galleries, looking that way or inwards.

From analogy I foresee that some successor of mine may exemplify human short-sightedness in my limit of demand to eight acres for the growth of the present Museum.

However, these acres, after conflicts stretching over a score or more of years, have at last been acquired for due display and facilities of study of the subjects of Section D.

Amongst the works of architectural art which adorn the metropolis, Westminster Abbey and St. Paul's Cathedral stand supreme. Of later additions may with them be named the noble example of the Perpendicular Gothic selected by Barry for the Houses of Parliament, and, I may be permitted to add, the new Law Courts, which exemplify the more severe style of the Thirteenth-century Gothic.

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[1] In his letter of February 14, 1753, to his friend Mann, Horace Walpole, then Member for Lynn, writes:—"You will scarce guess how I employ my time, chiefly at present in the guardianship of embryos and cockle-shells. Sir Hans Sloane is dead, and has made me one of the trustees of his museum, which is to be offered for twenty-thousand pounds to the King and Parliament and (in default of acceptance) to the Royal Academies of Petersburg, Berlin, Paris, and Madrid. He valued it at four-score thousand, and so would any one who loves hippopotamuses, sharks with one ear, and spiders as big as geese. The king has excused himself, saying he did not think that there were twenty thousand pounds in the Treasury."—"Letter to Horace Mann," 8vo, vol. iv. p. 32.

[1] Ed. 1825, p. 332.

Mr. Alfred Waterhouse, R.A., for the realisation of the plans and requirements of our Museum of Natural History, has chosen an adaptation of the Round-arched Gothic, Romanesque, or Romaic of the twelfth century. No style could better lend itself to the introduction, for legitimate ornamentation, of the endless beautiful varieties of form and surface sculpture exemplified in the animal and vegetable kingdoms. But the skill in which these varieties have been selected and combined to produce unity of rich effects will ever proclaim Mr. Waterhouse's supreme mastery of his art.

I need only ask the visitor to pause at the grand entrance, before he passes into the impressive and rather gloomy vestibule which leads to the great hall, and prepares him for the flood of light displaying the richly-ornamented columns, arcades, and galleries of the Index Museum.

In the construction of a building for the reception and preservation of perishable objects, the material should be of a nature that will least lend itself to the absorption and retention of moisture. This material is that artificial stone called terra-cotta. The compactness of texture which fulfils the purpose in relation to dryness is also especially favourable for a public edifice in a metropolitan locality. The microscopic receptacles of soot-particles on the polished surface of the terra-cotta slabs are reduced to a minimum; the influence of every shower in displacing those particles is maximised. I am sanguine in the expectation that the test of exposure to the London atmosphere during a period equal to that which has elapsed since the completion of Barry's richly ornamented palace at Westminster, now so sadly blackened by soot, will speak loudly in favour of Mr. Waterhouse's adoption of the material for the construction of the National Museum of Natural History. A collateral advantage is the facility to which the moulded blocks of terra-cotta lend themselves to the kind of ornamentation to which I have already referred.

In concluding the above sketch of the development of our actual Museum of Natural History, I may finally refer, in the terms of our modern phylogenists, to the traceable evidences of "ancestral structures." In the architectural details of the new Natural History Museum you will find but one character of the primitive and now extinct museum retained, viz. the Central Hall. In Montague House there were no galleries, but side-lit saloons or rooms of varying dimensions and on different storeys.

In its successor, the Museum developed on its site at a later period, we find galleries added: that, for example, which was appropriated to the birds and shells being 300 feet in length. This architectural organisation still exists at Bloomsbury.

The Museum, which may be said to have budded off, has risen to a still higher grade of structure after settling down at South Kensington. In its anatomy we find, it is true, the central hall and long side-lit galleries; but in addition to these inherited structures we discern a series of one-storeyed galleries, manifesting a developmental advance in the better admission of light and a consequent adaptation of the walls as well as the floor to the needs of exhibition.[1]

Should the Section, as did the Académie des Sciences in relation to the passage cited, kindly condone such application to human contrivances of the current genealogical or phylogenetic language applied to vital structures, your President need hardly own his appreciation of the vast superiority of every step in advance which is manifested in existing as compared with extinct organisms. And thus, sensible as far as human faculty may comprehend them, that organic adaptations transcend the best of those conceived by the ingenuity of man to fulfil his special needs, he would ask whether analogy does not legitimately lead to the inference, for organic phenomena, of an Adapting Cause operating in a corresponding transcendent degree?

In conclusion, I am moved to remark that a Museum giving space and light for adequate display of the national treasures of Natural History may be expected to exert such influence on the progress of Biology as to condone, if not call for, a narrative of the circumstances attending its formation in the Records of the British Association for the Advancement of Science.

OUR ASTRONOMICAL COLUMN

Encke's Comet.—We continue the ephemeris of this comet in the contracted form adopted in Nature, vol. xxiv. p. 292, from the calculations of Dr. O. Backlund of Pulkowa:—

At Berlin midnight

	R.A.			Decl.		Log. distance from	
	h.	m.	s.	°	′	Sun.	Earth.
Sept. 2	4	23	4	+35	53·2	0·1659	0·0222
4	4	31	17	36	35·9		
6	4	40	7	37	19·2	0·1493	9·9885
8	4	49	38	38	3·0		
10	4	59	56	38	46·8	0·1317	9·9535
12	5	11	6	39	30·3		
14	5	23	16	40	12·8	0·1128	9·9173
16	5	36	32	40	53·3		
18	5	51	1	41	30·9	0·0926	9·8805
20	6	6	51	42	4·1		
22	6	24	7	42	30·9	0·0709	9·8439
24	6	42	54	42	49·3		
26	7	3	13	42	56·3	0·0474	9·8089
28	7	25	0	42	49·3		
30	7	48	5	42	24·9	0·0219	9·7776
Oct. 2	8	12	13	+41	40·4		

In 1848, when the perihelion passage occurred eleven days later than it will do in the present year, the comet was remarked to be "just visible" to the naked eye at Harvard Observatory, U.S., on the morning of October 9, when the theoretical intensity of light was 4·3, and it was "plainly visible" to the naked eye on the morning of November 4, with an intensity of 9·5. The latter is a greater value than will be attained at this appearance, the maximum being 7·5 on November 9. On October 10 the calculated brightness will be equal to that, when it was just visible without the telescope in 1848, but moonlight will interfere at the time. For about four weeks after September 10 the comet will not set in London. As we have already stated it will be nearest to the earth on October 11, and in perihelion on November 15.

[Since the above was in type we learn from Mr. A. A. Common that he detected Encke's comet with his three-feet reflector at Ealing, shortly before midnight on Saturday last. On the following night, when it was better seen, its diameter was about 2′, and there was a central condensation of light.]

Schaeberle's Comet.—This comet will soon be well observable in the other hemisphere. The following track depends upon elements which Dr. v. Hepperger has calculated from observations to August 11:—

At Berlin Midnight.

	R.A.		Decl.		Log. distance from Earth.	Intensity of light.
	h.	m.	°	′		
Sept. 1	13	30·0	+11	27	9·8329	12·5
5	13	52·7	+ 1	41	9·8965	8·5
9	14	7·7	− 5	32	9·9606	5·7
13	14	18·1	10	52	0·0198	3·8
17	14	25·7	14	56	0·0725	2·6
21	14	31·4	18	8	0·1191	1·9
25	14	36·1	20	42	0·1601	1·4
29	14	40·0	22	51	0·1962	1·0
Oct. 3	14	43·5	24	41	0·2284	0·8
7	14	46·6	−26	16	0·2569	0·6

The intensity of light on July 18, the date of the first European observation, is taken as unity.

NOTES

The Royal Gardens, Kew, have just received, through the kind exertions on their behalf of Sir Ferdinand von Mueller, K.C.M.G., F.R.S., Government Botanist, Melbourne, perhaps the most remarkable Australian Cycadaceous stem which has ever been imported into this country. It is about four feet high, five and a half feet in circumference, and weighs about six hundredweight. It is the type of a new species described by von

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[1] In the notable reply (*Annales des Sciences Naturelles*, 1829) to an illustration of the unity of composition or of plan in Cephalopods and Vertebrates, by bending one of the latter so as to bring the pelvis in contact with the nape, advocated by Geoffroy St. Hilaire, Cuvier did not deem it too trivial to call in architecture to elucidate his objections. "La *composition d'une maison*, c'est le nombre d'appartemens ou de chambres qui s'y trouve; et son *plan*, c'est la disposition réciproque de ces appartemens et de ces chambres. Si deux maisons contenaient chacune un vestibule, une antichambre, une chambre à coucher, un salon, et une salle à manger, on dirait que leur *composition est la même*; et si cette chambre, ce salon, &c., étaient au même étage arrangés dans le même manière, on dirait aussi que leur *plan est le même*. Mais si leur ordre était différent, si de plain-pied dans une des maisons, ces pièces étaient placées dans l'autre aux étages successifs, on dirait qu'avec une composition semblable ces maisons sont construites sur des plans différens" (p. 245).

Mueller as *Macrozamia Moorei*, in honour of Mr. Charles Moore, F.L.S., the Director of the Botanic Garden, Sydney. The exhibition of two stems (of which that secured for and sent to Kew is one) in the Queensland Court at the Melbourne Exhibition, seems to have drawn attention to the species. The plants appear to have been obtained from the mountainous district near Springsure in Queensland, where specimens have been seen twenty feet in height, with a girth of six feet four inches, cones measuring two to three feet in length, and leaves seven feet long. The stem at Kew has been placed in the Palm House, where it can scarcely fail to be an object of interest. It is in excellent condition, and there is every reason to hope that it will in time push a new crown of leaves. But even if it does not it will at any rate form, as Sir Ferdinand von Mueller has suggested, a unique museum specimen.

The *Gazette* contains the official notice of the appointment of a Royal Commission, consisting of Mr. Bernhard Samuelson, M.P., F.R.S., Prof. H. E. Roscoe, D.C.L., F.R.S., Mr. Philip Magnus, Mr. John Slagg, M.P., Mr. Swire Smith, and Mr. William Woodall, M.P., "to inquire into the instruction of the industrial classes of certain foreign countries in technical and other subjects, for the purpose of comparison with that of the corresponding classes in this country; and into the influence of such instruction on manufacturing and other industries at home and abroad."

The Queen has signified her pleasure to confer upon Mr. MacCormac, of St. Thomas's Hospital, Honorary Secretary-General of the late International Medical Congress, the honour of knighthood.

The Meteorological Station to be erected at Pavia will be under the direction of Prof. Cantoni, who will establish a station for terrestrial physics, for the investigation of the influence of heat, light, electricity, &c., on vegetation in general, and some cultivations in particular, and also for the observation of the diurnal and annual variations of terrestrial magnetism.

The scientific activity of Paris is at present almost exclusively concentrated on electricity, and the Paris Electrical Exhibition will have a scientific significance which is quite unusual. The initiative has been taken by the German Government, which has sent several professors to deliver lectures on the objects exhibited by that nation. Dr. Christian, of the Physiological Museum of Berlin University, gives explanations every day at two o'clock of the galvanometers on the Siemens system constructed by him. On Monday M. du Moncel, Member of the Institute, delivered a lecture on Telegraphy at ten o'clock in the morning, and conducted his audience through the galleries to visit the instruments described by him. Other lectures have been advertised for the different days of the week from August 29 to September 3. The Exhibition was opened to the Press last Friday and to the public last Saturday, at night from eight to eleven.

The electric tramway in Paris has at length begun to work, and has several times gone backwards and forwards. A single overhead tube was tried at first to convey the current, but it was found impossible to insulate the rail by which it returned. Two overhead copper tubes are now used, along each of which at the bottom runs a longitudinal slit. A wire passing through the slit is attached to the tramcar beneath, and above to a small wheel which runs freely in the copper tube. As the car advances it draws along the little wheels through each tube, and thus maintains the connection.

A number of natives of Tierra del Fuego are at present at the Jardin d'Acclimatation in Paris.

The French Government have resolved to grant a subvention for erecting a statue in Franche Comté to Claude de Jouffroy as the inventor of steam vessels. The French Académie des Sciences at its last sitting adopted a report of M. de Lesseps in favour of Jouffroy's claim to that distinction and to public gratitude.

The new botanical lecture theatre of the University of Edinburgh was used by Prof. Dickson for the first time during the past summer session. It is built from the plans of Mr. Robertson, of H.M. Board of Works, and is a large octagonal building lighted from the roof and by windows on six sides. It is seated for 600, and had this year to contain 450 students. The acoustic arrangements are perfect. The old lecture-room has been converted into a general laboratory, while the former laboratory becomes a private room. The practical teaching has been conducted as formerly by Mr. Geddes, lecturer on Zoology in the School of Medicine, assisted by Mr. J. M. Macfarlane, B.Sc. Besides the usual elementary class, a class for advanced workers has also been started, a considerable number of investigations have been prosecuted, and instruction in drawing has been provided. The latter arrangement has been peculiarly successful.

The twelfth meeting of German Anthropologists was opened at Ratisbon on August 8 by the president, Prof. Fraas. Some 250 members were present. The secretary, Prof. Ranke, read the report on the widely-extended activity of the Society. Prof. Ohlenschläger (Munich) spoke on the Roman epoch in Bavaria and the excavations in the Roman burial-ground near Ratisbon. Other addresses were delivered by Professors von Virchow, Tischler, Undset, Groos, Mehlis, Klopfleich, Schaaffhausen, Vater and Török. The next meeting-place will be Frankfort.

The second meeting of Austrian Anthropologists took place at Salzburg on August 12-16. Some 270 gentlemen were present, principally Germans, Norwegians, and Russians. Of eminent scientific men we may mention Prof. von Virchow, the travellers Dr. Holub and Dr. Nachtigal, Prof. Steub (Munich), and Prof. Johannes Ranke. Count Wurmbrand was elected president, and Baron von der Sacken vice-president. Addresses were delivered by Herren Prinzinger, Much, and Zillner on the ancient inhabitants of Noricum, which the former two said were Germans; Herr Zillner however believed them to have been Kelts. On the second day Crown-Prince Rudolf took part in the meeting. Count Wurmbrand spoke on the development of the forms of bronzes and clay vessels, Herr Wolderich on prehistoric dogs, Holub on the South African negro tribes. Herr Maska reported on the discoveries near Schamberg, and Professors von Virchow and Schaaffhausen had an animated debate on the jaw of Neutitschein. Other addresses were delivered by Professors Tischler, Luschan, von der Sacken, Müllner, and Schaaffhausen. The usual excursions terminated the meeting.

The death is announced of Prof. L. Spangenberg, director of the technical Versuchsanstalt and Professor of the Engineering Sciences at the Technical High School of Berlin. He died on August 6 last.

The celebrated Egyptologist, Brugsch Pacha, has changed his residence from Cairo to Berlin, where he will lecture at the University.

The European Vice-Consul at Tchesme telegraphs to Constantinople on August 27 that Tchesme and Chio were, on the night of the 26th, visited by an earthquake still more terrible than that of the 3rd of April. The destruction of property, he says, is considerable, and the inhabitants are in despair. Contemporaneously with the shocks of earthquake felt at Chio and Tchesme, the earth at Zante is reported to have suddenly given out intense heat, accompanied by a strong breeze from the east, causing much alarm. These phenomena, however, subsided immediately. On the 24th inst. the entire island was enveloped in smoke, clouds from the west-south-west obscuring the sea from noon until dusk. Masses of calcined leaves also fell throughout the island.

A SEVERE shock of earthquake is reported to have been experienced in the mining district of Teversal, in Nottinghamshire, about noon on Friday last. In one of the pits belonging to the Stanton Ironworks Company the miners were so alarmed at the shock and the accompanying noise, that, thinking an explosion had occurred, they rushed to the mouth of the pit. In the Pear Tree Inn, Fackley, bricks were removed from the chimney, and the same thing was noticed in a house at Teversal. The station-master at the latter village, while sitting in his house, was thrown from his seat by the shock, and a quantity of plaster was detached from the ceiling. There was no explosion in the mines or other circumstance to account for the phenomena, and an upheaval of the floor of one of the pits indicated that the cause of disturbance was below the workings. One of the pits is 430 yards deep. The shock travelled in a north-west direction.

THE programme of the Congress of German Antiquarians, which will meet at Frankfort on September 11-15, has now been published. On the 11th the twenty-five years jubilee of the Frankfort Antiquarian Society will be celebrated.

THE professorship of Natural History and Geology at the Royal Agricultural College, Cirencester, vacant by the resignation of Prof. M. G. Stuart, has been filled by the appointment of Mr. Allen Harker, late of the Zoological Station, Naples.

ONE of the exhibits at the International Medical and Sanitary Exhibition was a "Compact School Collection for Use in Teaching the Chemistry of Foods," suggested by W. Stephen Mitchell, M.A. This form of case has been arranged for the purpose of affording, at a low cost, help to teachers in giving demonstrations on the chemistry of foods. The leading idea is that the teachers will be able to have on the walls of their schoolrooms the actual objects they are talking about, and the children will be familiarised by having always before them, not simply words or diagrams, but samples of the things themselves. Mr. Mitchell believes, as he stated in a paper read before the Society of Arts and the Domestic Economy Congress held at Birmingham, that with such diagrams as he showed, having lines of different lengths to represent quantities, greater accuracy of the knowledge of quantities can be conveyed than by showing the measured quantities in heaps as is the plan adopted at Bethnal Green. This does not get over the difficulty of showing the gases. Teachers must learn how to prepare these to show to their classes. The apparatus and materials for doing this are not costly, and are described in the "Shilling Chemistry Primer" published by Macmillan and Co. The cases are arranged with a sliding panel in front, so that the bottles can be taken out.

AT Castrop (Westphalia) a meteor was observed in the north-east sky on July 30 at 8.15 p.m. It moved in the direction from north-west to south-south-east. A meteoric stone weighing about 5 lbs. fell in the immediate vicinity of a field labourer, and penetrated into the ground to the depth of 1 metre. It was intensely hot, and was afterwards forwarded to Herr Oberbergrath Runge at Dortmund, in whose possession it now is.

ON August 15 the well-known Professor of Physics, Dr. Wilhelm Weber of Göttingen University, celebrated the day when, fifty years ago, he was called to that University from Halle. He is now seventy-seven years of age, and lectured until a few years ago.

A TERRIBLE catastrophe happened on August 16 at Remscheid (Rhenish Prussia). Suddenly the so-called Brennende Berg opened to the extent of some sixty to one hundred square metres and threw up gigantic flames. A house standing near sank into the burning gulf, and its inmates unfortunately perished. It is believed that the disaster was caused by the ignition of petroleum gas rising from a petroleum vein in the depth of the mountain.

Apropos of the forthcoming erection of a monument to Sauvage at Boulogne-sur-Mer, it may be mentioned that a rival claim to the invention of the screw propeller has been set up on behalf of a person named Dallery, also a Frenchman, whose granddaughter, it is alleged, has submitted certain evidence to the Académie des Sciences, showing that her grandfather, who died in 1835, took out a patent as long ago as 1813 for certain contrivances, including a screw propeller and a tubular boiler. M. de Lesseps is of opinion that although Dallery, like Pauchon, had long ago conceived the idea of the screw, yet it is to Sauvage that the credit is due of having been the first to apply it to practical purposes.

THE following candidates have been successful in obtaining Royal Exhibitions of 50*l*. per annum each for three years, and free admission to the course of instruction at the following institutions:—1. The Normal School of Science and Royal School of Mines, South Kensington and Jermyn Street, London—Thomas Mather, aged twenty-four, pattern maker, Manchester; Alfred Sutton, twenty-one, engine-fitter, Brighton; William H. Littleton, seventeen, student, Bristol. 2. The Royal College of Science, Dublin—Arthur Whitwell, nineteen, ex-pupil teacher, Nottingham; Frederick J. Willis, eighteen, student Bristol; Christopher J. Whittaker, twenty-one, pattern maker, Accrington.

MR. E. B. TYLOR requests us to mention that the portrait of Andaman Islanders in his "Anthropology," p. 88, which was reproduced in Mr. Wallace's review in NATURE, vol. xxiv. p. 242, is from one of the admirable series of photographs taken in 1872 by Dr. G. E. Dobson, now of the Army Medical School, Netley Hospital. By inadvertence, the cut in question was printed in the "Anthropology" without reference to Dr. Dobson. His paper "On the Andamans and Andamanese" in vol. iv. of the *Journal* of the Anthropological Institute, which gives an account of his visit to the natives in their forest-home, is illustrated with a set of three portrait-groups, which show perfectly their peculiar and homogeneous race-type.

MESSRS. CASSELL AND CO. have issued the first part of "an entirely new and revised edition" of Dr. Robert Brown's "Races of Mankind," under the title of "The Peoples of the World."

PHYLLOXERA has made its appearance in Hungarian vineyards. Its occurrence in the district of Szölös Urdo (Torda Comitat) has been officially stated. Also in the Swiss canton of Neufchâtel it is spreading to an alarming extent. The vineyards of Grand-Saconnex, Colombier, and La Coudre are fast succumbing to the plague.

M. SYNYROS, an Athens merchant, has recently given 100,000 francs for building a museum at Olympia.

THE construction of another great Alpine tunnel which should bring Paris and the North of France into more direct communication with Italy than is afforded by the existing tunnel through Mont Cenis, is under consideration with the French Government, the projects including not only one through Mont Blanc, but also through the Simplon or the Great St. Bernard. It is not likely, however, that the latter will meet with much encouragement. The tunnel under the Simplon would be 60,719 feet long, while that under Mont Blanc is only 44,292 feet. As connected with other Alpine tunnels, Mont Cenis is 40,093 feet, and St. Gothard, 48,952 feet. The Simplon would therefore be longest of all; but, on the other hand, it would be on a lower level than the others, the entrance at Brieg being only 2333 feet, and that at Iselle 2253 feet above the sea level. The entrances to the Mont Blanc tunnel would be 3345 feet at Mont Quart, and 4215 feet at Entrèves above the sea level. The Bardonneche entrance to Mont Cenis is 3970 feet, and that at Modane 3799 feet, while in the case of the St. Gothard tunnel the northern entrance at Göschenen is 3638 feet, and the southern, at Airolo, 3756 feet above the sea. Thus the Mont Cenis tunnel is shorter, but 330 feet higher than the Mont Blanc, while the Simplon would be about half as long again, but about 1000 feet lower. Supposing that the operations would be conducted at the same rate as they have been at St. Gothard, the boring will take 4218 days, or, working at both ends, 2109—nearly six years.

THE additions to the Zoological Society's Gardens during the past week include two Guinea Baboons (*Cynocephalus sphinx*), a Grivet Monkey (*Cercopithecus griseo-viridis*, var.) from West Africa, presented by Mr. Lionel Hart; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by the Rev. George Cuffe; two Arabian Gazelles (*Gazella arabica*), three Domestic Pigeons (*Columba œnas*) from Arabia, presented by Mr. Reginald Zohrab; two Common Squirrels (*Sciurus vulgaris*), British, presented by Lieut.-Col. F. D. Waters, 82nd Regiment; a Collared Peccary (*Dicotyles tajaçu*) from Guiana, presented by Capt. W. F. Wardroper; a Ring-tailed Coati (*Nasua rufa*) from South America, presented by Mr. L. H. Haworth; a Cinereous Sea Eagle (*Haliaëtus albicilla*) from Norway, presented by Mr. James Ashbury; a Red and Blue Macaw (*Ara macao*) from South America, presented by Mrs. Supple; two Common Barn Owls (*Strix flammea*), British, presented by Mr. C. T. Foster; an Upland Goose (*Bernicla magellanica*) from South America, presented by Mr. A. Nesbitt; two Common Kestrels (*Tinnunculus alaudarius*), British, presented by Mr. J. Edwards: a Bonnet Monkey (*Macacus radiatus*) from India, a Common Marmoset (*Hapale jacchus*), from South-East Brazil, deposited; two European Scops Owls (*Scops giu*), European, purchased. Amongst the additions to the Insectarium during the same period are larvæ of the Common Butterfly (*Vanessa C. album*), Lobster Moth (*Stauropus fagi*), Pale Tussock Moth (*Orgyia pudibunda*)—the so-called Hop-Dog—*Dipthera orion*, *Halias prasinana*, and *Deilephila euphorbiæ* and *galii*; also a perfect insect of *Cholus forbesi*, being the third known example of this species, originally described from specimens captured in an orchid-house at Highgate. The present specimen was found, under similar conditions, by Dr. Wallace of Colchester.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, August 22.—M. Jamin in the chair.—The following papers were read:—Meridian observations of small planets and of Comet *b* 1881, at Paris Observatory, during the second quarter of 1881, by M. Mouchez.—Remarks on M. Jamin's note on comets, by M. Faye.—On spectrum analysis applied to comets, by the same.—On the nature of the repulsive force exercised by the sun, by the same. He associated it long ago with the state of incandescence of the sun; and, in an experiment, rare gaseous matter made luminous by means of an induction-spark was repelled by an incandescent plate at a sensible distance. Some thought this not decisive, however; for the gaseous matter might become more conductive through heating, so that the effect observed might be a sort of obscure discharge. M. Faye invites physicists to take up the matter afresh.—On the interior state of the terrestrial globe, by M. Roche. Supposing the globe formed of a nucleus or solid block nearly homogeneous, covered with a lighter layer, of density geologically shown to be about 3 in relation to water; he finds it possible to harmonise the general values of precession and flattening, if it be considered that the interior nucleus has solidified and taken its definitive form under influence of a rotation less rapid than that now animating the earth. The central block is probably like meteoric iron in specific gravity, while the enveloping layer is comparable to aëroliths of stony nature, with little iron.—On the irreducible co-variants of the binary quartic of the eighth order, by Prof. Sylvester.—On a new species of Cissus (*Cissus Rocheana*, Planch), from the interior of Sierra Leone, capable of bearing the winter of Marseilles, by M. Planchon. Its endurance is a matter of temperament, and a proof of the extent of the scale of resistance to cold and heat which some plants possess, and which often upsets all prevision. The American *Vitis riparia* lives sixty miles north of Quebec, and is also found in the sub-tropical Southern States.—On the laws of formation of cometary tails, by M. Schwedoff. Starting with the existence of an infinite number of ponderable particles in celestial space, he shows that those with parabolic orbits have most chance of collision and consequent heating and dispersion. The sudden vaporisation of solids, due to passage among them of a cometary nucleus, generates the cometary nebulosity. The velocity of propagation of visible waves accompanying the nucleus is equal to the velocity of the nucleus itself at the moment of departure of these waves. The maximum of intensity of a cosmic wave is found in the tangent to the orbit of the nucleus at the point of departure of the wave. With these two laws he seeks to explain the phenomena observed.—On a particular case of the theory of motion of an invariable solid in a resistant medium, by M. Willotte.—M. Trève communicated the results of some experiments as to the effects produced by shunts in telephonic circuits.—Solar observations at the Royal Observatory of the Roman College during the first quarter of 1881, by P. Tacchini. After the secondary minimum in the end of last year, the solar activity resumed its course towards the maximum. The distribution of protuberances, &c., was the same as in the last quarter of 1880.—Observations of solar spots and faculæ in April to July, 1881, by P. Tacchini. A minimum of spots occurred in May, and an exceptional maximum in July; now, the activity is anew at a minimum. During this year several periods of abundant frequency have recurred.—Spectroscopic studies on comets *b* and *c* 1881, by M. Thollon. Comet *c* seems to be almost wholly gaseous. The brightness of the head and tail of the comets seems to vary rapidly and uniformly with distance from the sun; arguing that their white light is almost wholly reflected sunlight. The slowness of variation of the band spectrum is against the view that the cometary elements are rendered incandescent by calorific action of the sun. The comets have probably a light and heat of their own.—Researches on the telluric lines of the solar spectrum, by M. Egoroff. Sending a strong electric beam through 18m. of aqueous vapour, and increasing the tension to 6 atm., the spectrum was notably changed in aspect. The group *a* in the extreme red he thinks fundamental for aqueous vapour, and he is going to examine it in detail.—On the existence of a new metallic element, actinium, in the zinc of commerce, by Mr. Phipson.—Note relative to a new series of phosphates and arseniates, by MM. Filhol and Senderens.—Fixation of hypochlorous acids on propargylic compounds, by M. Henry.—On the abnormal presence of uric acid in the salivary, gastric, nasal, pharyngeal, sudoral, and uterine secretions, and in menstrual blood; diagnostic and therapeutic indications, by M. Boucheron.—Observations during a thunderstorm on June 25, 1881, by M. Larroque.

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