THURSDAY, SEPTEMBER 20, 1883

SCIENCE WORTHIES

XXII.—ARTHUR CAYLEY

IT is natural that the public in general should wish to know something of the life and work of one whom the British Association for the Advancement of Science has honoured by placing him this year at its head, an honour indeed which could not much longer have been withheld, considering the foremost place which our new President occupies among English mathematicians. But when asked to tell the story I am tempted to exclaim with the needy knifegrinder—

"Story, God bless you, there is none to tell, Sir."

The quiet life of a student is not likely to be rich in sensational incidents, and of the nature of the work done by a labourer in the field of pure mathematics it is not possible to give more than a vague idea to the outside world. Some slight sketch I must attempt to give, and in doing so I must express my obligations to Mr. J. W. L. Glaisher, without the help of whose greater knowledge of Cambridge matters and of the recent progress of mathematics I could not have undertaken this task.

Arthur Cayley was born August 16, 1821. His father, a grandson of Cornelius Cayley—who was Recorder of Kingston-on-Hull from 1725 to 1771—was settled at St. Petersburg as partner in the firm of Russian merchants—Thornton, Melville, and Cayley. It was during a short visit of his parents to England that their second son, Arthur, was born at Richmond, Surrey. An elder brother had died in infancy; a younger brother has since become well known as an Italian scholar and a translator of Dante. In 1829 the family returned permanently to England, and after a while fixed their residence at Blackheath. At a very early age Arthur gave the usual indication by which mathematical ability is wont first to show itself, namely, great liking and aptitude for arithmetical calculations. A lady, who was one of his first instructors, has told that he used to ask for sums in Long Division to do while the other little boys were at play. After four years' teaching at a private school at Blackheath he was sent at the age of fourteen to King's College School, London, the principal of which (Hugh Rose), being struck by the indications of mathematical genius which he gave, prevailed on his father to abandon his intention of bringing the boy up to his own business and induced him to send him instead to Cambridge, where he entered Trinity College at the rather unusually early age of seventeen. At his college examinations Cayley was first by an enormous interval; but it was fortunate for him that the wares in which he dealt were those which fetched the highest price; for, if classics had been given the preference over mathematics instead of *vice versâ*, he had in his class at Trinity College two most formidable competitors, namely, Mr. Munro, the well known scholar and editor of Lucretius, and Mr. Justice Denman, who afterwards came out as Senior Classic at the same time that Cayley came out as Senior Wrangler and first Smith's Prizeman.

This was in 1842. In University as in other harvests, there sometimes comes a run of unusually good years, and this certainly appears to have been the case at the period in question. The Senior Wrangler in 1840 was Leslie Ellis, in 1841 Stokes, in 1842 Cayley, in 1843 Adams; the last three of whom have, for now over twenty years, given lustre to the Cambridge mathematical school, of which they have formed part of the working staff. I do not know whether Cayley's success at the Tripos Examination was as little a surprise to himself as it was to others. Stories were current in Cambridge at the time of the equanimity with which he received the news of his success. The best authenticated one is that he was on the top of the coach on a night journey from London to Cambridge when the tripos list was put into his hands; he quietly put it into his pocket, resigning himself very contentedly to the necessity of waiting till the morning light for a knowledge of its contents. Cayley's name cannot be added to the list of those who have combined distinction in the boats or on the cricket field with high University honours. He was, however, an active pedestrian, and was a member of the Alpine Club in its comparatively early days.

While still an undergraduate, Cayley commenced his career of mathematical publications by a paper in the *Cambridge Mathematical Journal* for 1841. This periodical had been founded a little time before by Leslie Ellis, who has been just mentioned, in conjunction with his friend, Mr. Gregory, who thereby rendered a service to English mathematics that it would be difficult to estimate. One who devotes himself to original mathematical research must make up his mind to forego the pecuniary rewards which attend other forms of successful literary labour. The public which he addresses is so limited that, instead of expecting to be paid for what he writes, he has to think how he can give it to the world without too severe pecuniary loss. If it were not for the help given by learned societies and by mathematical periodicals, every mathematician who was not rich would be forced to keep his discoveries to himself, and on such terms few would have spirit to persevere in research. At the time of which I speak mathematical periodicals open to young students scarcely existed, so that to young mathematicians doubtful of the value of their own speculations, and whose modesty would hardly permit them to ask for publication from the Royal Society, an immense stimulus was given by the foundation of the periodical just mentioned, the *Cambridge Mathematical Journal*, afterwards continued under the names of the *Cambridge and Dublin Mathematical Journal* and the *Quarterly Journal of Mathematics*. This journal roused the energies of the younger members of the University by making known to them that others of no higher standing than themselves were engaged in original research and by promising them the means of publishing whatever they might discover; and certainly it is no small thing that it can boast to have given Cayley his first opportunity of coming before the world.

His prodigious activity however could not long be content with a single outlet, and there were few organs of mathematical publication at home or abroad which did not receive communications from him. If his memoirs were now collected, they would form a mass exhibiting a spectacle of enormous literary industry. It appears, however, not to have been until 1852 that he addressed a memoir to the Royal Society, of which he was elected a Fellow in the same year.

His mathematical activity during this period was the more surprising, as he was able to devote to these studies only a limited portion of his time. He had been elected a Fellow of Trinity College in 1842; but as he was not willing to take Holy Orders, this was but a temporary provision, for he could only hold his Fellowship for seven years after his Master's degree. It became necessary for him therefore to look out for some profession more remunerative than mathematics, and very soon after taking his Master's degree he became a pupil of the eminent conveyancer, Mr. Christie. It is said that when offering himself as a pupil he modestly suppressed all mention of his antecedents, and that Mr. Christie was much surprised to find out on cross-examining him that he had to do with a Senior Wrangler and Fellow of Trinity. However this may be, he soon became Mr. Christie's favourite pupil, as indeed was not wonderful in the case of one who possessed a very clear head, immense capacity for work, and the power of throwing his whole mind into the work on which he was at the time engaged. After he was called to the bar he never had occasion to look elsewhere for business, for Mr. Christie was always glad to supply him with as much conveyancing work as he was willing to undertake. I have been told that some of his drafts were made to serve as models for students. But nothing that her wealthy rival had to offer could seduce Cayley into unfaithfulness to his first love, Mathematics. For Mathematics he always jealously reserved a due portion of time free from the encroachments of his business relations with Law, and it was during the time of his legal practice that some of his most brilliant mathematical discoveries were made. At last he obtained release from the embarrassment of a divided allegiance. By placing Lady Sadler's trusts on a new footing and founding the Sadlerian Professorship, his University was able to invite him to return, and he gladly accepted what was at the time a very modest provision, but which would enable him to give his whole time to the pursuits most congenial to him. Some time after his return to Cambridge his pecuniary position was improved. His College, which on his return had speedily made him an honorary Fellow, after a time reelected him to a foundation Fellowship, necessarily a very rare distinction, since the reelection of an ex-Fellow involves the exclusion of the claims of a younger candidate. Later still, in the course of University legislation about Professorships, the position of the Sadlerian Professorship was improved. But these things could not have been foreseen at the time that Cayley accepted the office.

It was in 1863 that, after fourteen years of chamber life in Lincoln's Inn, he married and settled permanently in Cambridge. He never would own to any regret when his friends spoke to him of the prospects of professional advancement which he sacrificed by not remaining at the bar. He knew what mode of life would best promote his own happiness, and he had strength of mind to follow it without troubling his head about the riches or honours a different course might bring. His mathematical work gave him pleasure which he never found in law; and in his hatred of unnecessary words he was once wicked enough to say that the object of law was to say a thing in the greatest number of words, and of mathematics to say it in the fewest. But, jesting apart, the University had no reason to regret the legal training and knowledge which he had acquired during his absence from it. It has much added to his usefulness as a member of the Council of the Senate, where his opinion has carried the greatest weight, and it has enabled him to be particularly useful both to his College and to the University in the drafting of new statutes and in the necessary preliminary deliberations. At the last contested Parliamentary election Cayley presided at one of the three polling places, and gave universal satisfaction, hearing patiently the arguments on both sides on all disputed points, and then promptly making a decision in a few words in such a way as to inspire general confidence.

But after all it is as a mathematical professor that Cayley is eminently "the right man in the right place." No one could be better fitted to discharge the duties prescribed for the Sadlerian Professor, "to explain and teach the principles of pure mathematics, and to apply himself to the advancement of the science." It is seldom that one man so well combines the two qualifications here indicated, viz. power to teach what is known already, and ability to extend the boundaries of knowledge. It constantly happens that men of great originality of genius find it irksome to study what has been done by others. And now every department of science has so enlarged its borders that it has not only become impossible for one man to master the whole circle of the sciences; but even a single department, such as pure mathematics, includes under it so great a variety of subjects that most men are content to be specialists, and, devoting themselves to their favourite topic, are satisfied with a very superficial knowledge of other branches. Cayley is quite as distinguished for the amount and universality of his reading as for his power of original work, and may fairly count as the most learned mathematician of the present day. I suppose that, if all European mathematicians could be subjected to a tripos competition, no matter who might come out first on the "problem" papers, Cayley would be far ahead in the "book work." And his tastes are so catholic that no form of mathematics comes amiss to him. I remember how we in Dublin were struck by his proficiency in pure geometry, a subject then much cultivated with us, but which we had been accustomed to look on as too little esteemed at Cambridge.

This wideness of knowledge has made Cayley invaluable as a mathematical referee. To several scientific societies (the Royal Society, the Mathematical Society, the Royal Astronomical Society, the Cambridge Philosophical Society) he has long been a principal adviser as to the merits of mathematical papers presented for publication, no one being more willing to take the trouble of examining such papers, or being better able to pronounce how much of their contents is new or important. And no one could be more ready and obliging with his advice to private students who have desired to interest him in their investigations, and to be assured by him that no unscrupulous predecessor has plagiarised their discoveries. Repeatedly have foreign mathematicians expressed their surprise at the rapidity with which he has dealt with such inquiries, an answer commonly coming by return of post, probably giving a new proof of some of the results, or pointing out that some of them were capable of greater generalisation. By his services in this way he has made himself so widely popular that if European mathematicians had to elect themselves a head I could not name any one likely to have a larger number of votes.

With respect to Cayley as an original inquirer, his special merit has in my opinion been truly seized by Mr. Glaisher, who has described him as the greatest living master of algebra. While, as I have said, no part of mathematics comes amiss to him, he is always happiest when he can translate his theorems into pure algebra and show that a proposed result is but the expression of an algebraical fact. In this respect he differed from H. J. Smith, by whose recent loss English mathematics has so terribly suffered, who was entirely arithmetical in his thoughts and work.

Mathematicians, like chess-players, may be divided into the book-learned and the original, the highest amount of excellence being attained by those who combine great knowledge of books with the power to strike into new paths of their own. Of this I have spoken already. But there is another division of chess-players, the solid and the brilliant, some being full of ingenious devices which, however, will not bear a careful examination; others being quite free from mistake but wooden in their style. Cayley combines the excellences of the two kinds in a very high degree, though his merits in the one respect appear to me to be more marked than in the other. Men weak in power of calculation have often exhibited beautiful exercises of ingenuity in their attempts to arrive at results by some shorter process. Such a master of algebra in all its forms as Cayley was not to be dismayed by any amount of calculation, and he therefore has been able to trample down many a difficulty which an inferior in this respect [might have evaded by some ingenious oblique method.

As Cayley is not afraid of hard work himself, so it is necessary for the readers of his papers not to be easily discouraged by formidable calculations. But in my opinion it is not this so much that makes Cayley's papers difficult to read as the fact that he usually proceeds by the synthetic, not the analytic, method. It usually happens that a mathematical inquirer begins by proposing to himself some comparatively simple question. By the time he has found the answer to it, the subject opens on him; the first question suggests others, the theorem first discovered is found to admit of wide generalisations, and perhaps it may be found that these could have been arrived at in quite another way. When the time comes for the inquirer to publish his results to the world, the most attractive course is to take his readers by exactly the same road he has travelled himself, beginning with the simple problem which first attracted attention, and leading on step by step to the highest results arrived at. Cayley on the contrary usually begins by trying to establish at once the highest generalisation he has reached, writing down equations and proceeding to make calculations as to the good of which he has not taken his readers into his confidence. The consequence is that few master his papers but those who have found a clue to them by some previous work in the same direction.

I fancy that the difficulty of Cayley's papers is to be accounted for by his having had comparatively little experience in teaching mathematics until rather late in life, and then only to students of the highest order. He lectured for a few years at Trinity after taking his degree, but I dare say that he did wisely in going to the bar instead of making a livelihood by mathematical teaching at Cambridge, for one who loved mathematics so much for its own sake, would hardly sympathise with the many whose only object in coming to him would be to learn how they could successfully get through an examination. On his return to Cambridge he possibly would have extended his influence more widely if he had taken what may seem the lazier course of giving the same series of lectures year after year. But Cayley preferred to give his classes his latest and highest work, and each year has taken for his subject that of the memoir on which he was for the time engaged. The result has been that he has been brought little in contact with any but the most advanced students, who alone could profit by such instruction, nor even they, indeed, unless they were as high-minded as himself, and were content to spend a great amount of time and labour on work that could not "pay" at the great University examination.

As I have spoken of Cayley's lectures I ought not to omit to mention the honour done him by the heads of the Johns Hopkins University of Baltimore, Maryland, an institution which numbers among its professors, as head of its mathematical department, Cayley's distinguished friend and fellow worker, Sylvester. They invited Cayley to go over to lecture at Baltimore in the winter session of 1882. He accepted a proposal in every way so flattering, and lectured at Baltimore in the months of January to May, 1882, returning to England in June. His subject was Elliptic and Abelian functions, and his lectures, in which he considered from an algebraic point of view the geometrical theories of Clebsch and Gordan, were given for publication to the *American Journal of Mathematics*, and are likely to form a classic memoir on the subject.

As I have said so much of Cayley's mathematical labours, it will probably be expected that I should speak a little less vaguely, and endeavour to explain more particularly the nature and progress of his discoveries; yet it is not easy to make the history of discovery in the higher branches of pure mathematics readable even for so select a class as the subscribers to NATURE. It requires but a small stock of technical knowledge to enable a reader to follow with interest a history of mechanical inventions, or of discoveries admitting of useful practical applications, or of the skilled organisation of labour; but what is to be said of the work done by a solitary student in his closet, the result of which will not so much as cheapen one yard of calico?

It would be out of place if I were to take trouble here to show that pure mathematics have after all added much to the material wealth of the world. My subject is the life of a great artist who has had courage to despise the allurements of avarice or ambition, and has found more happiness from a life devoted to the contemplation of beauty and truth than if he had striven to make himself richer, or otherwise push himself on in the world. We do not classify painters according to the numbers capable of appreciating their respective productions. On the contrary, we can understand that it is often the lowes style of art which will attract round it the largest circle of admirers. So the fact that it is a very limited circle which is capable of appreciating the beauty of the work done by a great mathematician should not prevent men from understanding that it is like the work done by a poet or a painter, work done entirely for its own sake, and capable of affording lively pleasure both to the worker himself and his admirers, without any thought of material benefit to be derived from it.

But in point of fact mathematics stand midway between the arts which minister to man's sense of beauty and those which supply his material comforts. The name "pure mathematics" suggests that there is such a thing as "applied mathematics," and it is well known that the mathematician furnishes the instruments employed by cultivators of sciences whose practical utility is beyond dispute. If the mathematician did no more than manufacture such instruments precisely as the demand arose for them, his might count as one of the arts which are valued only for their practical utility. But actually the invention of the mathematical instruments usually comes first, and the use to be made of them is found out afterwards. The stock example of the kind is the debt which physical astronomy owes to the labours of the early geometers on the theory of conic sections, a theory cultivated without any suspicion that it could be turned to practical account. Yet it was because Newton was in his day the greatest master of this as of every other branch of pure mathematics that he was able to bring all the motions of the heavenly bodies under the dominion of mathematical calculation, and to convert the moon into a timepiece by which the mariner can ascertain his position on the seas. With the advance of physical science greater refinement and power in the mathematical instruments of investigation have become necessary; but pure mathematicians have ever outrun the demands of the practical workers, for instrument-making has delights of its own. The late Lord Rosse I have no doubt found more pleasure in devising the innumerable ingenious and beautiful contrivances necessary for the manufacture of his huge telescope than he ever did from observing with it after it was made. It is impossible for any one now to say what advantages future investigators will derive from the perfection to which the mathematical instruments have been brought by the labours of such men as Cayley, who have invented mathematical steam hammers by which ponderous masses of formulæ can be manipulated with ease and calculations made simple which in former times were looked on as impracticable.

There is hardly anything that comes under the head of pure mathematics at which Cayley has not worked, but it will be enough if I try to say something as to that by which his name is likely to be best remembered—his creation of an entirely new branch of mathematics by his discovery of the theory of invariants, which has given quite a new aspect to several departments of mathematics. It has introduced such a host of new ideas, and consequently of new words, that a Senior Wrangler of forty years ago, who had not kept pace with modern investigations, would find, on taking up a book of the present day on geometry or algebra, that he could not read it without a glossary, and must go to school again to learn what the writer was speaking of. It would be out of place if I were to enter into a very long technical exposition here, but it is possible, without assuming in the reader more than a moderate knowledge of analytic geometry, to make him at least understand what the word "invariant" means. Suppose that we have written down the general equation of a curve of any degree, and also have found the relation that must subsist between the coefficients in order that the curve should assume some special form. For simplicity I suppose the equation to be of the second degree, and I take the well known relation between the coefficients which is satisfied when the curve represented reduces itself to two right lines. Now imagine the equation to be transformed to any new coordinates whatever, this can make no change in the form of the curve represented. If the relation in question were satisfied by the coefficients of the original equation, it must also be satisfied by the coefficients of the transformed equation. But by actually performing the transformation we can express these new coefficients in terms of the old ones and of the constants introduced in the process of transformation. The expression will be complicated enough, and that of the relation of which I am speaking still more so. But since the relation must vanish whenever the corresponding relation expressed in terms of the old coefficients vanishes, the one must contain the other as a factor. The remaining factor, it will be seen on examination, contains nothing but the constants introduced by transformation. All this can be verified by actual work; but the result which I have stated can be foreseen without any calculation.

The principle which I have described has proved to be very fertile in applications. The late Dr. Boole made, in 1841, some interesting use of a simple case of the same principle. But it was Cayley who set himself the problem to determine *a priori* what functions of the coefficients of a given equation possess this property of *invariance*, viz., that when the equation is linearly transformed the same function of the new coefficients is equal to the given function multiplied by a quantity independent of the coefficients. The result of his investigations was to bring to light a number of important functions (some of them involving the variables as well as the coefficients) whose relations to the given equation are unaffected by linear transformation. And the effect has been that the knowledge which mathematicians now possess of the structure of algebraic forms is as different from what it was before Cayley's time as the knowledge of the human body possessed by one who has dissected it and knows its internal structure is different from that of one who has only seen it from the outside.

In an age when the work of mathematical research is so actively carried on, whenever one worker finds a nugget there is an immediate rush to the spot of other searchers. In the present case Cayley's friend Sylvester was one of the first on the spot, and both being resident in London were able by frequent oral communication to stimulate each other's ideas. As I am not relating the history of mathematical science, I need not name the foreign mathematicians who rapidly came in to labour in the same field; but it is agreed on all hands that it was Cayley who both discovered the "diggins" and got out some of the biggest nuggets. It is not always the case that the history of a mathematical discovery has not to tell of some contests for priority. All pure mathematics consists in the drawing out of ideas latent in admitted principles, and it is a curious fact how men will fail to draw the consequences which to another will appear irresistibly suggested by something they have themselves asserted, and consequently how near they will come to the brink of a discovery without actually making it. And controversies as to mathematical priority naturally arise because it seems so cruel to the man who has taken all the steps except the very last, that another should step in and get the credit of the discovery, when it seems to him that he himself had done all the difficult part of the work and the other only drawn an inference so simple that no credit should be given to any one for making it. If no controversy of the kind has arisen in the present case, perhaps the cause is not exclusively the indisputable character of Cayley's claims, but something is also due to the moral nature of the man. His motto has always been "esse quam videri," and I do not know any one to whom it would be more repulsive to engage in a personal contest by claiming for himself a particle of honour or of money more than was spontaneously conceded. He would be apt to take for his model the patriarch Isaac, who, when the Philistines claimed a well which he had dug, went on and dug another, and when they claimed that too, went on and dug a third.

The place of a more minute account of his mathematical discoveries may be supplied by a mention of the wide recognition which his labours have received. He was given the honorary degrees of D.C.L. Oxford, 1864, LL.D. Dublin, 1865, and was elected Fellow or Correspondent of the following Societies:—Philosophical Society, Manchester, 1859; French Institute, 1863; Royal Societies, Edinburgh and Berlin, 1865; Boston, 1866; appointed a Member of the Board of Visitors, Greenwich Observatory, 1866; Milan, 1868; St. Petersburg and Göttingen, 1871; Royal Irish Academy, 1873; Upsala, Leyden, and Rome, 1875; Hungary, 1881; Sweden, 1882. I should add that the Royal Society awarded him a Royal Medal in 1859, and last year (1882) the Copley Medal; the latter a distinction seldom conferred on a pure mathematician.

Though his principal interests are mathematical, they are far from being exclusively so. He is a good linguist, and, as was said of Moltke, there are few European languages in which he does not know how to hold his tongue. He is chairman of the Association for Promoting the Higher Education of Women. When seats in the University Council are contested, his name always appears on both the rival lists. By all who know him he is as much respected as a high-minded man as he is admired as a mathematician.

GEORGE SALMON

BENTHAM AND HOOKER'S "GENERA PLANTARUM"

Genera Plantarum ad exemplaria imprimis in herbariis Kewensibus servata definita. By G. Bentham and J. D. Hooker. 3 vols. (London, 1862-1883.)

THE completion of the "Genera Plantarum" of Messrs. Bentham and Hooker, an event long impatiently desired by all botanists, has been recently effected by the publication of the second and concluding part of the third volume. This great work has required more than five-and-twenty years of assiduous labour, during which the authors have devoted themselves to their formidable task with untiring perseverance, and with a degree of unity both in the plan and the execution of the work which would have been impossible but for their constant daily intercourse, and their relations of intimate personal friendship.

Before undertaking the publication of the "Genera" its authors had already given to the world important works which had placed them in the foremost rank as botanists, and both were familiarly acquainted with the scientific wealth accumulated in the museums and gardens at Kew. Mr. Bentham, whose botanical collections were united to those of the Royal Herbarium as long as thirty-six years ago, had already in connection with his various works and memoirs had occasion to study nearly the entire vegetable kingdom; while Sir Joseph Hooker, in addition to an equally wide range of study, had the inestimable advantage of having during his extensive travels been able to observe in the living state numerous species of many genera characteristic of the tropical and antarctic regions, and of having fixed their analytical characters by sketches and diagrams of singular elegance and accuracy.

With a rare amount of abnegation of personal feeling the authors of this work were content to let it go forth under their joint names, without in any way indicating the separate share contributed by each of them, desiring, as it would appear, that it should be regarded as the collective result of their joint labours—the product of two minds working harmoniously for a common object. Only very recently, under the pressure of urgent requests from many different quarters, Mr. Bentham consented, in a short note communicated to the Linnean Society,[1] to explain in a summary way the share contributed by each of the authors. This is of so much interest to botanists that the present writer does not hesitate to give here the substance of Mr. Bentham's note.

The *Polypetalæ*, which fill the first volume, were pretty equally divided. While Mr. Bentham was engaged on the earlier orders, Sir J. Hooker undertook the *Cruciferæ*, *Capparideæ*, and *Resedaceæ;* and to his share also fell most of the numerous families of the *Discifloræ*, while Mr. Bentham elaborated the remaining families of the *Thalamifloræ*, along with the *Lineæ*, *Humiriaceæ*, *Geraniaceæ*, and *Olacineæ*. Of the group of the *Calycifloræ* it was natural that Mr. Bentham should undertake the *Leguminosæ*, which he had already illustrated by a series of important memoirs, and to him also fell the *Myrtaceæ*, *Umbelliferæ*, and *Araliaceæ*. The remaining families of this group, including the *Rosaceæ*, *Saxifrageæ*, *Melastomaceæ*, and *Cucurbitaceæ*, besides many others less important, were assigned to Sir J. Hooker.

The first portion of the second volume is almost entirely occupied by the two great families of *Rubiaceæ* and *Compositæ*. To the former of these Sir J. Hooker devoted two years of constant study which involved very numerous dissections of a difficult nature, and he also elaborated the *Caprifoliaceæ*. During the same period Mr. Bentham was mainly occupied with the vast family of *Compositæ*, comprising nearly 800 genera, and not much

[1] "On the joint and separate work of the authors of Bentham and Hooker's 'Genera Plantarum.'" *Journal of the Linnean Society*—Botany, vol. xx. pp. 304-308.

fewer than 10,000 species. To assign definite generic characters to a series of forms so closely allied was an undertaking which, in spite of the previous labours of many eminent botanists, required the most careful examination of an almost overwhelming mass of materials, along with the severest critical acumen. The second portion of the second volume includes the great mass of the Gamopetalous families. At this period the pressure of official duties, and those devolving upon him as President of the Royal Society, prevented Sir J. Hooker from devoting much of his time to the laborious tasks of critical systematic botany; and to this portion of the work he contributed only the allied families of the *Vacciniaceæ*, *Ericaceæ*, and *Epacrideæ*, in addition to the *Myrsineæ*, *Primulaceæ*, and a part of the *Sapotaceæ*. On Mr. Bentham devolved all the remaining families of this vast group; and to show the prodigious amount of labour accomplished by this remarkable man, it is sufficient to say that, along with minor families, these included the *Apocyneæ*, *Asclepiadeæ*, *Gentianeæ*, *Boragineæ*, *Convolvulaceæ*, *Solaneæ*, *Scrophularineæ*, *Gesneriaceæ*, *Bignoniaceæ*, *Verbenaceæ*, and, finally, the *Labiatæ*. Some additional years might have been requisite for such an undertaking if his classical monographs on the two great families *Scrophularineæ* and *Labiatæ* had not supplied Mr. Bentham with the materials for his subsequent work.

The first part of the third volume is occupied by the *Monochlamydeæ* and the Gymnosperms. To this part the group of the *Curviembryeæ*, including the important families *Amarantaceæ* and *Chenopodiaceæ*, was contributed by Sir J. Hooker, who further undertook the *Nepenthaceæ*, *Cytinaceæ*, and *Balanophoreæ*. The materials for the latter were ready to hand, being for the most part contained in the remarkable monographs long since published by himself. The remaining families of *Monochlamydeæ* were elaborated by Mr. Bentham. Amongst the more important must be mentioned the *Laurineæ*, *Proteaceæ*, *Thymelæaceæ*, and *Santalaceæ*. But it was especially the great families *Euphorbiaceæ* and *Urticaceæ* which, in spite of recent monographs, demanded a vast amount of minute examination and careful revision of all existing sources of information. The Gymnosperms had originally been undertaken by Sir J. Hooker, who possesses so wide an acquaintance with these plants in the living state; but the pressure of other occupations again interfered, and this group was also executed by Mr. Bentham, doubtful questions here as well as throughout the entire work being reserved for discussion between the joint authors.

The second part of the third volume, which concludes the work, contains all the families of Monocotyledonous plants. The examination and revision of the vast store of existing materials appeared to the authors such a formidable task that, in the doubt whether they should be able to complete it, they resolved to attack in the first instance the most difficult families, Sir J. Hooker undertaking the Palms, and Mr. Bentham the *Orchideæ*. As is well known, the study of these families offers peculiar difficulties. In the former the great size of all the parts, as well as their texture, usually makes it impossible to preserve herbarium specimens available for study, and much restricts the supply of materials to be found even in the best-furnished museums. Notwithstanding his very extensive previous knowledge of this family, and the exceptional resources available at Kew, Sir J. Hooker found the task to involve a much greater expenditure of time and labour than he had anticipated, chiefly owing to the necessity for a very extensive correspondence with botanists in various parts of the world who were able to supply special information or materials not otherwise obtainable. Along with other special difficulties, the study of the vast family of the *Orchideæ* is hampered by the unsatisfactory condition of a great proportion of the specimens sent to Europe from countries whose climate makes their preparation and preservation almost unmanageable. It is not surprising that Mr. Bentham found more than a year of unbroken persistent labour no more than sufficient for this family, and that he subsequently required an equally long period in dealing with the *Gramineæ*.

In treating the remaining Monocotyledonous families, the task of the authors was in many cases lightened, though not by any means replaced, by the work of various recent monographers. Sir J. Hooker disposed of the group of *Nudifloræ* (*Aroideæ* and allied families) and that of the *Apocarpeæ*, including the *Triurideæ*, *Alismaceæ*, and *Najadeæ*. To Mr. Bentham fell the heavy task of completing the work by the examination of the numerous remaining families of Monocotyledons, among which may be specified the *Bromeliaceæ*, *Irideæ*, *Amaryllideæ*, *Liliaceæ*, *Commelynaceæ*, *Pandaneæ*, *Restiaceæ*, and *Cyperaceæ*. It is a surprising proof of exceptional mental and bodily activity that in dealing with this portion of the work, and in studying natural families where the floral parts are too often lost or obliterated in dried specimens, and therefore demand the most delicate and careful dissection, Mr. Bentham, in spite of his advanced age, revised and defined in the course of three years more than 1200 genera.

Throughout the progress of the work, as well as in determining its original plan and arrangement, every important question was decided after joint consideration and discussion. In this way the limits and characters of the larger groups, the descriptions of the natural families, their subdivision into suborders and tribes, and the arrangement of genera, were settled by mutual interchange of views. Almost invariably the work of each author was read and criticised by the other before it was sent to press, and the proofs were regularly corrected by both, so as to eliminate as far as possible any chance of divergence of opinion; and, finally, they were fortunate enough to obtain the help of a highly competent friend, the Rev. M. J. Berkeley, who undertook the revision of the Latin text with a view to secure the desirable uniformity of style and diction.

The descriptive characters of the families, or natural orders, are drawn up with the same care as those of the separate genera; they are clear, exactly comparable, and the affinities of families, as well as the exceptional and abnormal forms which they not seldom present, are specially noted. The approximate number of known species belonging to each family, as well as to each separate genus, is stated throughout the work, and the geographical distribution of each genus, as well as of the larger groups, has been recorded as fully as the present state of our knowledge makes it possible. Finally, very full references to the works in which each genus has been first described or best illustrated, with similar references to the authorities for synonyms, add further to the value of the work as a guide to the student of systematic botany.

The descriptive characters of the genera have been throughout verified or established after the previous examination of numerous specimens, and as a rule it may be said that for the purpose of this work the whole of the vast collections in the Royal Herbarium at Kew were passed in review, and especial attention given to the aberrant forms presented by many large genera. In the comparatively few cases where the authors were unable to refer to and examine specimens of a genus enumerated, they are careful to cite the author on whose authority it has been admitted. Genera that appear to the authors to have been founded on insufficient characters, or on an erroneous view of the structural facts, are in some cases reduced to the rank of subgenera or sections of the typical genus, in others simply recorded as synonyms at the conclusion of the description of the genus to which they are referred. There remains a further category of generic names given by authors who, either from ignorance of the science or incomplete materials, have failed to make it possible to identify them at the present day. These are enumerated as *Genera dubia* at the end of the synoptic table of the genera of each family. In short, it may be truly said that the authors have neglected nothing that could make their work useful and practical, as well as a complete storehouse of the present condition of our knowledge of this branch of natural science.

Of the many different points of view in which this great work may be regarded, the most interesting, perhaps, to the scientific naturalist is the consideration that we have here the results of a complete reconsideration of the whole subject of the classification of the flowering plants by two men of remarkable intellectual power, possessing an extent of knowledge and a command of materials far surpassing anything possible to the authors of preceding works of similar scope. In one or two brief sentences of a note already cited, Mr. Bentham has assigned the amply sufficient reasons which induced the authors to maintain in its main features the arrangement of the natural orders established by the elder De Candolle. Every attempt to set forth in a linear series the complex relations which connect together as in a network the various groups of the vegetable kingdom is necessarily incomplete and defective. It is a fortunate circumstance that the authors of the "Genera" have added the weight of their authority to the judgment of those botanists who hold that no one of the various arrangements which have been proposed during the last half century, and more or less extensively adopted in some parts of Europe, possesses advantages which can compensate the serious practical inconvenience of having systematic works of reference arranged after a variety of discordant systems. The Candollean arrangement has therefore been deliberately maintained in this work, with a few not unimportant modifications; but in the arrangement and grouping of the genera into tribes and subtribes there has been ample space for the exercise of the highest faculties of the philosophical naturalist. It is evident throughout the work that every question as it has arisen has received fresh consideration, and in many important families the classification adopted is altogether new. It is remarkable that, even in regard to families previously elaborated by Mr. Bentham, he has not hesitated to introduce important changes suggested by further consideration and study. It is of course impossible to say that the final results of future discovery and research may not lead to further modifications in botanical classification; but for the present generation this will remain as the best result of the comprehensive survey of the whole field of our knowledge.

The number of genera described in the present work, taking into account the addenda, is 7565, while the number described by Endlicher in his "Genera Plantarum," with the supplements, is 7202. These figures give some measure of the progress of botanical discovery during the last thirty years, and at the same time some indications of the amount of labour involved in the collection and examination of the materials scattered throughout the numerous general works and monographs published during that period, and especially throughout hundreds of volumes of scientific periodicals which are now annually produced in every part of the world. The increase in the number of known genera is in truth much greater than the figures above cited would indicate, inasmuch as the tendency of Bentham and Hooker is to unite under the same generic designation plants which do not appear to present sufficient differences of structure, and they have not hesitated to suppress numerous genera that have been admitted in preceding systematic works of authority. Those who may not be disposed to acquiesce in these conclusions may easily continue to regard as genera the subgenera and sections whose distinctive characters are throughout the work subjoined to the descriptions of the respective genera.

It follows from the preceding remarks that for practical use in classing large botanical collections the present work is an indispensable guide. The present writer, who has enjoyed the advantage of daily, almost hourly, reference to its pages, feels that he is merely discharging a debt of gratitude in endeavouring to express his sense of the scientific value of a work which has become a classic from the day of its publication. A work which, at a given period, summarises the entire field of knowledge in one department of science, marks an epoch in its progress, and becomes the starting-point for further advance towards wider knowledge. Such is the work to which Mr. Bentham and Sir Joseph Hooker have devoted a full quarter of a century, and as such, notwithstanding the importance of their other works, it must remain their chief title to enduring fame.

ERN. COSSON

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts, No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

The Red Spot upon Jupiter

THE red spot on Jupiter has really disappeared. I have observed the planet again after conjunction. The region in which the red spot formerly was is now very white; it passed over the central meridian of the planet this morning at 4h. 36m. (M.T. at Palermo), which gives for this place the Jovicentric longitude 63°, plainly corresponding to the longitude that Mr. Marth assigned to the red spot at present, if visible. This proves that the neighbourhood of the red spot had followed the particular motion of the spot itself. This place is well characterised by the permanent depression in the great reddish band of the planet. A. Riccò

Royal Observatory, Palermo, September 10

"Elevation and Subsidence"

Mr. O. Fisher has been so good as to offer a reply to my "remark with a query," his answer being (allowing for an obvious printer's error) that it is "an open question whether the melting temperature of rocky matter is, or is not, raised by pressure."

I cannot for a moment pretend to the same familiarity with the results either of experiment or of calculation as is doubtless possessed by Mr. Fisher. I only claim to speak as representing the class whose knowledge on these subjects is essentially second-hand; but, speaking as such, I think that Mr. Fisher's reply will not generally be regarded as satisfactory. I should, therefore, like to repeat my question with a little extension:—

1. Do not the "rigidity" calculations incontestably show that the earth is extremely rigid, *i.e.* solid? Are not, therefore, all theories which disregard this result (such as that the nucleus may be above its own critical temperature) put out of count?

2. Are not the phenomena of metamorphic and hypogene rocks on too large a scale to be accounted for by heat of merely local origin, whether produced by chemical or mechanical action, such as has been suggested in connection with volcanoes?

3. Do not all reasonable views of the origin of the earth, *i.e.* any form of the nebular hypothesis, point to the same conclusion as (2), viz. that the earth's heat is the residuum of a much greater amount formerly possessed, and not yet entirely lost by radiation?

4. Does not (3), taken in connection with the known laws of conduction, involve a continuous increase of temperature, whether rapid or slow, as we descend below the surface?

5. Although we may have no *direct* evidence as to the "temperature at depths bearing considerable ratios to the radius," is there not ample evidence that at comparatively insignificant depths the temperature is such as would melt not only "rocky matter," but far more refractory substances, if there were no counteracting influence? Even allowing a very slow increase, provided the increase is always positive, as 4 points out, should we not sooner or later almost certainly reach the melting temperature of the most refractory substances with which we are acquainted?

6. Can we then escape the conclusion, either that the nucleus consists of matter of a totally different kind from anything with which we are familiar, or that pressure raises its melting temperature? But does not every fact bearing on the question discredit the former hypothesis?

7. Should we not then accept the view that pressure does raise the melting-point of nucleus stuff, at least as a working hypothesis, only to be overthrown by direct evidence to the contrary, if direct evidence on the subject is ever forthcoming?

Trinity College, Cambridge F. Young

In a paper I read before a full meeting of the Geological Association on March 2 last, of which a brief notice is given in Nature, vol. xxvii. p. 523, I discussed the probability of subsidence of land, in certain cases, being due to *loading* by local accumulations of terrestrial matter acting upon a deflectible crust supported upon a viscous interior. The greatest effects, I imagined, from this cause, were due to local accumulations of ice past and present, particularly about the poles of the earth; but that secondary and important effects were due to the weight of accumulations of solid mineral matter from denudations being carried by oceanic currents and winds, from coral deposition, and the reaction of volcanic outflows. One illustration I proposed was that the sinking of the coast of Greenland was probably due to the weight of inland accumulation of ice, which proposition I thought was original, but Mr. Gardner (Nature, vol. xxviii. p. 324) says—"It has often been supposed that the sinking of the coast of Greenland is similarly due to its icecap." I should feel obliged if Mr. Gardner would point out references where this has been proposed, as I thought I had read the literature of the subject, and I fear that this part of my paper is less original than I assumed. W. F. Stanley

That there is a connection between sedimentation and subsidence on the one hand and between denudation and elevation on the other is a fact now admitted by most geologists. The real question to be answered, however, is:—Are these directly connected as cause and effect? or are they simply concomitant effects of the same cause? If the first be true, we should expect cause and effect to vary together, that is, that subsidence should keep an even pace with sedimentation. That this has not been exceptionally the case is proved by the sections of the carboniferous system in the central valley of Scotland, where the facts point to a continuous subsidence, accompanied by a very irregular sedimentation, with the result that now subsidence gained on sedimentation, now sedimentation on subsidence. Again, once the process commenced—and it is not very evident how on an originally even surface it could have commenced at all—we should expect it to be continuous. Sedimentation causes subsidence, subsidence gives rise to fresh sedimentation, and that again to renewed subsidence, and so on and on. Consequently we should expect that when once an area of sedimentation and subsidence was formed, it would continue an area of sedimentation and subsidence through all geological time.

It appears rather, I think, that the connection between them arises from their being concomitant effects of lateral pressure in the earth's crust (for notwithstanding the Rev. O. Fisher's masterly exposition of the inadequacy of this cause to produce the observed inequalities of the earth's surface, I still believe that, with the exception of the ocean basins, which must be otherwise accounted for, it is quite competent to account for the facts). We may suppose the action to take place so:—

A certain portion of the earth's crust is first thickened and strengthened by volcanic outburst or other accumulation on the surface. This part, when the tangential thrust comes, offers, by reason of its increased weight and thickness, a greater resistance to the elevating force than the parts around, and as a consequence these are raised around the thickened part, while it is at the same time depressed in a corresponding degree; in other words it becomes the centre of a syncline, while the strata around are raised into anticlines. Depression naturally leads to sedimentation, and this still more thickens the part, and enables it to offer greater resistance to the tangential thrust, with the result that it continues to be depressed as the strata around are elevated. The converse is also true. Denudation means the thinning and consequent weakening of the crust, and hence when the thrust comes the denuded part is the more likely to be elevated into the anticline.

This theory provides for the cessation of the phenomena, since the tension of the crust is after a time relieved. It also accounts for the fact that strata around volcanoes and volcanic necks, as also along the base of mountain chains, so frequently appear to dip below them. The rate of subsidence, too, would vary with the intensity of the exciting force, though the consequent sedimentation need not vary with it in the same absolute degree.

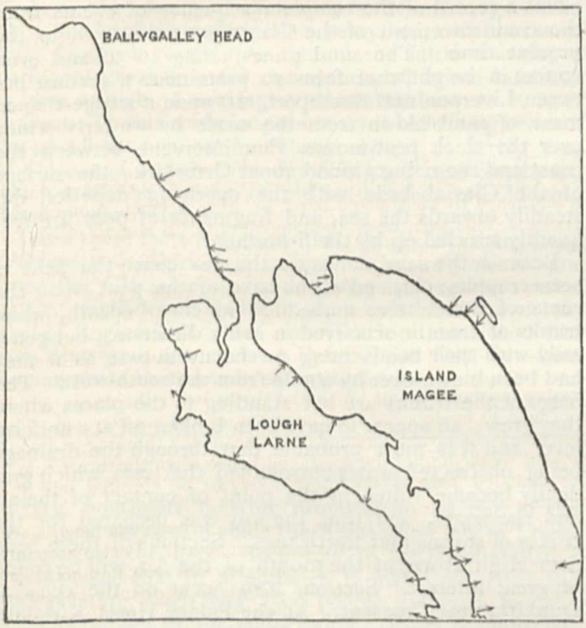
Perth, September 3 William Mackie

My article on elevation and subsidence has provoked considerable and, on the whole, friendly criticism, a so far satisfactory result, though but few points have been raised requiring reply. Dr. Ricketts objects, and very properly, that I have not alluded to his many writings on the subject; and to this I can only plead want of space, that I have not entered at all into its already voluminous bibliography, and that my article was written and in type before his recent contributions to the *Geological Magazine* had appeared. Beyond this I had sufficiently indicated that there were many observers in the field, and every geologist must be aware that the subject has for a long while past excited attention not only in England but in France and America.

The fundamental error in my article is pointed out by the Rev. Mr. Fisher and by Mr. Young, and the assumption that inert pressure induces heat must be abandoned. As I had read the "Physics of the Earth's Crust," I expected that this would be challenged, but I let it stand, as the fallacy has been shared by a large number of geologists, comprising some of the most distinguished, and has even escaped the correction of physicists. But this rectification, while very important, by no means affects the results, and on the contrary facilitates an appreciation of the causes of movements of the earth's crust; for if the fluid or viscous layer is chiefly due to internal heat and the relaxation of pressure near the surface, it may exist much nearer to our feet than could otherwise be admitted.

One of the gravest difficulties that the theory that added weight produces subsidence by acting on a fluid layer has had to contend with has been the great depth at which this fluid layer has had to be placed. It has always seemed to me next to impossible that liquid lava could well up from any such depths as those assigned to the viscous layer, or that a solid crust of so great a thickness should be sensitive to, as it is now shown to be, and rise and fall under, barometric changes. In acknowledging Mr. Fisher's letter and thanking him, I feel I am ungrateful in questioning that part of his work which interposes barriers which would break up the continuity of the viscous layer; I allude to his theory of "the roots of mountains." There does seem to me to be little fact in support of so startling a proposition, and I think the existence of volcanic vents, scattered through and in the midst of some of the highest chains, renders its acceptance difficult.

Mr. Murray restates his theory of the formation of coral atolls and reefs in the clearest manner, but I do not see that he explains any fact left unexplained by Darwin, or exposes any flaw in Darwin's reasoning. These masses of coral may have been continuously forming throughout even successive geological



Sheet 21. Geological Survey of Ireland, Antrim Coast, facing north-east.

periods, and their thickness is perhaps not exceptionally remarkable relative to that of slowly deposited oceanic sediments. There is no evidence that atolls are mere incrustations of volcanic craters, and it seems to me difficult to imagine so great a number of craters at the same level so completely masked. There are volcanic isles in abundance outside coral areas, but none I think, or few, of the form of a coral atoll. After all, Mr. Murray only shows that a second explanation is possible, though I still prefer the first.

I regret, being from home, that I am unable to answer Mr. Stanley. I may have alluded to the sinking of Greenland myself, and if I did not it was because the illustration was too familiar and self-evident. The sinking on the Greenland coast is not, I have understood, universal.

I still think it would render a service to science if readers of NATURE residing on sea-coasts would furnish authentic examples of elevation or subsidence or of waste. The magnificent Antrim coast, which I have recently visited, furnishes examples of subsidence among most unyielding rocks. The cliffs on the mainland are capped with basalt and dip inland, yet the basalt reappears in the Skerries out to sea with the same dip and at a much lower level. The same correspondence in stratification is seen between the mainland and Rathlin, but also with a great difference in elevation. The dip inland in all cases on this coast should bring up much older rocks out to sea, unless we are prepared to admit a fault running parallel to the coast, and following its sinuosities, and at right angles to the general lines of faulting.

The way in which all the strata forming the cliffs along the Antrim coast dip inland is very remarkable. The accompanying tracing from the Geological Survey Map is of a particularly indented coast-line, and the arrows show that the dip is everywhere away from the sea, irrespective of any general strike. In fact the general strike must often be the reverse of that shown on the coast for the same strata crop out at much higher levels on the hills farther inland. I recollect that most cliffs that I have examined, particularly in Hampshire, dip away from the sea. It would appear that the removal of weight along a cliff line causes a local elevation, which gives a cant inward, whilst subsidence takes place under sediment farther out to sea. This seems to explain the observed facts connected with marine denudation; but I must take a future opportunity of entering more thoroughly into this part of the question.

Glasgow, September 12 J. STARKIE GARDNER

"Zoology at the Fisheries Exhibition"

LETTERS have been published in NATURE of August 9 and 16 (pp. 334 and 366) by Mr. Bryce-Wright of Regent Street and Prof. Honeyman of Canada, calling in question the accuracy of statements made in an article in NATURE (vol. xxviii. p. 289) which were condemnatory of exhibits for which these two gentlemen are respectively responsible. It is natural that they should seek to remove the unfavourable impression which the statements in question were intended to convey: they seem, however, to have been unacquainted with the complete character of the information upon which the statements were based. Mr. Bryce-Wright states that it is not the fact that some of the corals exhibited in Lady Brassey's case belong to him. Nevertheless it is the fact that when the jury of Class V. asked Mr. Bryce-Wright to point out the corals entered in the official catalogue under his name, No. "813*b*," he informed them that the corals so entered were in the same case with Lady Brassey's corals, and formed part of that collection. It is also the fact that in the opinion of experts the names attached by Mr. Bryce-Wright to many of these corals are incorrect; and as to his assertion that these specimens have been compared with those in the British Museum and with those obtained during the *Challenger* Expedition, it is a fact that neither the one series nor the other has been accessible for such purposes for some considerable time, and I have reason to believe that no qualified zoologist has made a comparison of the corals exhibited by Lady Brassey and Mr. Bryce-Wright with any collection at all.

The letter of Prof. Honeyman in reference to the naming and state of preservation of the Collection in the Canadian Department, for which he is responsible, is misleading. The discreditable state of that collection, to which a passing allusion only was made in NATURE, has been remedied in one or two instances since the visit of the jury of Class V. Should there be any doubt as to the justice of the opinion expressed in the article in NATURE, I would simply ask Prof. Honeyman whether he would have any objection to allowing the matter to be decided by reference to the report of the jury of Class V., of which he was a member. I should be surprised (and so I think would he) were the report of that jury, when published, found to be at variance with the opinion expressed in the article in NATURE. Prof. Honeyman's statement that the specimen of *Cryptochiton Stelleri* is properly exhibited in a convenient glass jar and labelled inside and out, is calculated to mislead. When first exhibited it was not labelled with any name; subsequently it was labelled with the name of a genus of Holothurians, "Psolus." After the visit of the jury of Class V., probably as the result of information imparted by some of the eminent zoologists who served on that jury, it was labelled with its proper name. Without citing details, I shall simply state that there are (or were when the article in NATURE was written) far more serious blunders in the identification of specimens and worse instances of bad preservation in the Canadian collection of Invertebrata than those to which special allusion has been made.

THE WRITER OF THE ARTICLE

A Complete Solar Rainbow

MR. D. MORRIS, in his account of this rainbow (p. 436) appears to have fallen into a mistake in stating that its inner diameter—taken by Capt. Winchester, R.N.R.—was 43° 08′. It should be, I think, "inner semidiameter." The first circumsolar bow has a semidiameter of 41° 37′. That is almost necessarily invisible. The second circumsolar bow has a semidiameter of 43° 52′, and is rarely visible. I have no doubt that was the bow witnessed on board the *Norham Castle* on August 16

Athenæum Club, September 7 C. M. INGLEBY

Flint Flakes Replaced

As this subject has been more than once adverted to in NATURE, the following recent instances of placing flint flakes on to their original position may be interesting:—

Whilst examining the relics from Cowper's Camp, Epping Forest, in Mr. Raphael Meldola's house last month, I looked over a small number of flakes collected from one spot in the

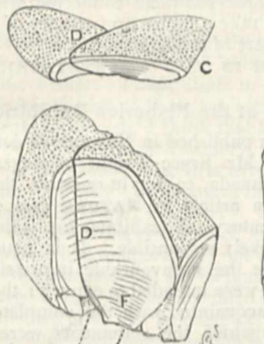


FIG. 1.

rampart of the camp, with remains of burnt wood and late Celtic pottery. I immediately saw that several of the flakes had been struck from the same block of flint, and after a short examination I managed to replace two as illustrated, one-half real size, in Fig. 1. The front of the two conjoined flakes is shown in the lefthand bottom figure, the side at B, the top at C, and the line of junction at D D. Behind E E are two cones of percussion, one belonging to each flake, and at F is the depression into which the cone of the missing frontal flake at one time fitted. The fractured part of the flint is deep chocolate brown, and lustrous, and the bark of the flint is dull ochreous; the flakes are undoubtedly artificial, and as old as the rampart of the camp, not less than two thousand years. This example, with other relics, will be placed in the Guildhall Museum.

Greater interest attaches to the replacing of Palæolithic flakes, as these are enormously older than Neolithic, and the chances are so very much against lighting on a perfectly undisturbed Palæolithic position.

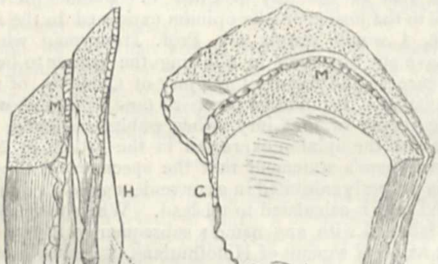


FIG. 2.

At Fig. 2 is illustrated (one-half actual size) two Palæolithic flakes from the "Palæolithic floor" at Stoke Newington Common, found and replaced by me. The front of the conjoined flakes is shown at G and the side at H. I found the lower flake two days before, and some distance from where I found the upper one; but as I have a method of placing newly found sharp flakes on a table, arranged temporarily in accordance with their colour and markings, I speedily saw that the upper flake would fit on to the lower one. Each flake has a cone of percussion, as shown at J K, and the upper flake has a well-marked depression at L, corresponding with the missing flake, which, if it had been found, would have fitted on to the front of the two conjoined examples. Both flakes are sharp and slightly stained with the ochreous river sand which overlaid them. Both (especially the upper one) show unmistakable signs of having been used as scrapers, the upper curved edge (and that edge only) being worn away by use. The worn upper edge of the superimposed flake at M M is distinctly shown in the illustration. A small intermediate piece belonging to the position at N I did not find. Both are naturally mottled in a peculiar manner, and the pattern and colour of the mottling exactly agree.

WORTHINGTON G. SMITH

NOTES ON THE POST-GLACIAL GEOLOGY OF THE COUNTRY AROUND SOUTHPORT

SINCE the writer carried out the geological survey of the western coast of Lancashire in 1868 he has constantly been asked, "Is there any geology to be studied at Southport? Is not the country a sandy expanse fringing peat-mosses of ceaseless monotony?" The meeting of the British Association this week at Southport renders this a fitting time to reply to these questions; for, strange as it may appear, in these apparently unpromising surroundings exists a record of the complete sequence of events from the commencement of the Glacial episode down to the present time. The sand dunes, rising to 50 and even 80 feet in height, that form so prominent a feature between Liverpool and Southport, rest upon a wedge-shaped mass of sand blown from the coast by westerly winds over the thick peat-mosses that intervene between the coast and the rising ground about Ormskirk; the surface of the Glacial beds, with the overlying deposits, dip steadily towards the sea, and fragments of peat are frequently trawled up by the fishermen.

Beneath the sand dunes on the sea coast the peat is seen cropping out, and at the base of the peat occur the roots of forest trees embedded in clay beneath, while trunks of trees lie scattered in many directions, but generally with their heads lying to the north-east, as if they had been blown over by a gale from the south-west. The bases of the trunks are left standing in the places where they grew; all appear to have been broken off at a uniform level, and it is most probable that through the drainage being obstructed water surrounded the trees, which gradually became rotten at the point of contact of the air and the water, and thus the way was prepared for the effects of storms and hurricanes. Sections of these beds near High Town, at the mouth of the Alt, will be found of great interest. Sections also occur on the coast at Dunkirk, near Crossens. At the Palace Hotel, Kirkdale, a boring was put down in 1867, that proved the sand to be 78 feet in thickness, resting on 18 inches of peat, which occurs at about 90 feet beneath high-water mark. When the land stood this amount above its present level, the coast would range in a straight north and south line from St. Bees Head to the mouth of the Clywd at Rhyl, but there is no reason to suppose that this amount represents the subsequent submergence since the era of the peat in Lancashire and North Wales. It is far more probable that when the trees flourished, found at the bottom of the peat fringing these coasts, this coast nearly coincided with the present twenty-fathom line, which passes from Anglesea round the Isle of Man; in that island the same sequence of post-glacial deposits is found, and the Irish elk alike occurs in the grey slags beneath peat.

At the mouth of the Ribble very interesting sections occur at Freckleton and Dow Brook; the latter is crossed by a Roman road, and has upon it a "Roman bath," only ten feet above the present high-water mark, proving the elevation of this coast has not been great since Roman times. The same fact is brought out by the interesting find of Roman coins near Rossal landmark, near Fleetwood, which were found in a salt-marsh clay lying on the peat beds, at about eight feet below the surface, or at about high-water mark, the coins having been apparently lost by the Romans scrambling over the soft slippery mud. This discovery proves the thick peat beds to be of older date than the Romans; this is also borne out by the very remarkable sections along the north coast of Wirral, especially near Leasowe, which have afforded the fine collection of antiquities preserved in the Liverpool Free Museum; the silty beds over the peat yield Roman coins of Nero, Antoninus Pius, and Marcus Aurelius, while in the peat beds beneath occur flint implements of the Neolithic type. When the peat beds of Western Lancashire are followed into the valleys of the large rivers that traverse the country, they are found to pass insensibly into a peaty seam occurring at the base of the alluvium of the lowest plain of these rivers. This is well seen in the valley of the Ribble at Preston; it is more than a mile in width, and 180 feet in depth; it is excavated entirely in the Glacial deposits, down to the rocky floor, which lies somewhat below high-water mark, and nearer the sea slopes down considerably beneath it. On the slopes of the valley lie terraces of old alluvium, marking successive stages in the process of denudation, commenced since the deposition of the Upper Boulder Clay, as the bottom of the valley is the ordinary alluvial plain, made of silt, resting on a peaty bed, with trunks of trees lying on rough river gravel, the latter marking a period of great fluviatile denudation, when the land was at least as high, if not higher, above the sea as it is at present. To this era belong the marine beds lying beneath the peat I have called the *Presall shingle*, occurring east of Fleetwood, and the *Shirdley Hill sands* near Southport, which mark the position of old sea-beaches and old sand dunes respectively.

From these facts it appears that the excavation of the Western Lancashire river valleys was entirely carried out since the Glacial episode, that they had reached their present depth when Neolithic man inhabited the north-west of England, and that since that era much land has been destroyed, now covered by the Irish Sea, but since Roman times there has been but little change.

C. E. DE RANCE

THE BRITISH ASSOCIATION

THE Southport meeting promises to be one of the most successful since the Association met in Liverpool twelve years ago. According to the latest statistics it is expected that in attendance it may even rival the York meeting, when over 2500 people gathered to celebrate the jubilee of the Association. From the information we have already published it will have been seen that Southport has shown the greatest zeal in preparing to give a generous reception to the representatives of British science; and if only the weather be propitious, there can be little doubt that the meeting will be a success. Both the papers to be read and the reports to be presented are expected this year to suggest some specially interesting subjects for discussion.

Last night Sir C. W. Siemens resigned the presidential chair to Prof. Cayley, who then delivered the opening address.

INAUGURAL ADDRESS BY ARTHUR CAYLEY, M.A., D.C.L., LL.D., F.R.S., SADLERIAN PROFESSOR OF PURE MATHEMATICS IN THE UNIVERSITY OF CAMBRIDGE, PRESIDENT.

SINCE our last meeting we have been deprived of three of our most distinguished members. The loss by the death of Prof. Henry John Stephen Smith is a very grievous one to those who knew and admired and loved him, to his University, and to mathematical science, which he cultivated with such ardour and success. I need hardly recall that the branch of mathematics to which he had specially devoted himself was that most interesting and difficult one, the Theory of Numbers. The immense range of this subject, connected with and ramifying into so many others, is nowhere so well seen as in the series of reports on the progress thereof, brought up unfortunately only to the year 1865, contributed by him to the Reports of the Association; but it will still better appear when to these are united (as will be done in the collected works in course of publication by the Clarendon Press) his other mathematical writings, many of them containing his own further developments of theories referred to in the reports. There have been recently or are being published many such collected editions—Abel, Cauchy, Clifford, Gauss, Green, Jacobi, Lagrange, Maxwell, Riemann, Steiner. Among these the works of Henry Smith will occupy a worthy position.

More recently, General Sir Edward Sabine, K.C.B., for twenty-one years general secretary of the Association, and a trustee, president of the meeting at Belfast in the year 1852, and for many years treasurer and afterwards president of the Royal Society, has been taken from us at an age exceeding the ordinary age of man. Born October, 1788, he entered the Royal Artillery in 1803, and commanded batteries at the siege of Fort Erie in 1814; made magnetic and other observations in Ross and Parry's North Polar exploration in 1818–19, and in a series of other voyages. He contributed to the Association reports on Magnetic Forces in 1836-7-8, and about forty papers to the *Philosophical Transactions;* originated the system of Magnetic Observatories, and otherwise signally promoted the science of Terrestrial Magnetism.

There is yet a very great loss: another late president and trustee of the Association, one who has done for it so much, and has so often attended the meetings, whose presence among us at this meeting we might have hoped for—the president of the Royal Society, William Spottiswoode. It is unnecessary to say anything of his various merits: the place of his burial, the crowd of sorrowing friends who were present in the Abbey, bear witness to the esteem in which he was held.

I take the opportunity of mentioning the completion of a work promoted by the Association: the determination by Mr. James Glaisher of the least factors of the missing three out of the first nine million numbers: the volume containing the sixth million is now published.

I wish to speak to you to-night upon Mathematics. I am quite aware of the difficulty arising from the abstract nature of my subject; and if, as I fear, many or some of you, recalling the Presidential Addresses at former meetings—for instance, the *résumé* and survey which we had at York of the progress, during the half century of the lifetime of the Association, of a whole circle of sciences—Biology, Palæontology, Geology, Astronomy, Chemistry—so much more familiar to you, and in which there was so much to tell of the fairy-tales of science; or at Southampton, the discourse of my friend who has in such kind terms introduced me to you, on the wondrous practical applications of science to electric lighting, telegraphy, the St. Gothard Tunnel, and the Suez Canal, gun-cotton, and a host of other purposes, and with the grand concluding speculation on the conservation of solar energy: if, I say, recalling these or any earlier addresses, you should wish that you were now about to have, from a different president, a discourse on a different subject, I can very well sympathise with you in the feeling.

But, be this as it may, I think it is more respectful to you that I should speak to you upon and do my best to interest you in the subject which has occupied me, and in which I am myself most interested. And in another point of view, I think it is right that the Address of a President should be on his own subject, and that different subjects should be thus brought in turn before the meetings. So much the worse, it may be, for a particular meeting; but the meeting is the individual, which on evolution principles must be sacrificed for the development of the race.

Mathematics connect themselves on the one side with common life and the physical sciences; on the other side with philosophy, in regard to our notions of space and time; and in the questions which have arisen as to the universality and necessity of the truths of mathematics, and the foundation of our knowledge of them. I would remark here that the connection (if it exists) of arithmetic and algebra with the notion of time is far less obvious than that of geometry with the notion of space.

As to the former side, I am not making before you a defence of mathematics, but if I were I should desire to do it—in such manner as in the "Republic" Socrates was required to defend justice, quite irrespectively of the worldly advantages which may accompany a life of virtue and justice, and to show that, independently of all these, justice was a thing desirable in itself and for its own sake—*not* by speaking to you of the utility of mathematics in any of the questions of common life or of physical science. Still less would I speak of this utility before, I trust, a friendly audience, interested or willing to appreciate an interest in mathematics in itself and for its own sake. I would, on the contrary, rather consider the obligations of mathematics to these different subjects as the sources of mathematical theories now as remote from them, and in as different a region of thought —for instance, geometry from the measurement of land, or the Theory of Numbers from arithmetic—as a river at its mouth is from its mountain source.

On the other side the general opinion has been and is that it is indeed by experience that we arrive at the truths of mathematics, but that experience is not their proper foundation: the mind itself contributes something. This is involved in the Platonic theory of reminiscence; looking at two things, trees or stones or anything else, which seem to us more or less equal, we arrive at the idea of equality: but we must have had this idea of equality before the time when first seeing the two things we were led to regard them as coming up more or less perfectly to this idea of equality; and the like as regards our idea of the beautiful, and in other cases.

The same view is expressed in the answer of Leibnitz, the *nisi intellectus ipse*, to the scholastic dictum, *nihil in intellectu quod non prius in sensu*: there is nothing in the intellect which was not first in sensation, except (said Leibnitz) the intellect itself. And so again in the "Critick of Pure Reason," Kant's view is that, while there is no doubt but that all our cognition begins with experience, we are nevertheless in possession of cognitions *a priori*, independent, not of this or that experience, but absolutely so of all experience, and in particular that the axioms of mathematics furnish an example of such cognitions *a priori*. Kant holds further that space is no empirical conception which has been derived from external experiences, but that in order that sensations may be referred to something external, the representation of space must already lie at the foundation; and that the external experience is itself first only possible by this representation of space. And in like manner time is no empirical conception which can be deduced from an experience, but it is a necessary representation lying at the foundation of all intuitions.

And so in regard to mathematics, Sir W. R. Hamilton, in an introductory lecture on astronomy (1836), observes: "These purely mathematical sciences of algebra and geometry are sciences of the pure reason, deriving no weight and no assistance from experiment, and isolated or at least isolable from all outward and accidental phenomena. The idea of order, with its subordinate ideas of number and figure, we must not indeed call innate ideas, if that phrase be defined to imply that all men must possess them with equal clearness and fulness: they are, however, ideas which seem to be so far born with us that the possession of them in any conceivable degree is only the development of our original powers, the unfolding of our proper humanity."

The general question of the ideas of space and time, the axioms and definitions of geometry, the axioms relating to number, and the nature of mathematical reasoning, are fully and ably discussed in Whewell's "Philosophy of the Inductive Sciences" (1840), which may be regarded as containing an exposition of the whole theory.

But it is maintained by John Stuart Mill that the truths of mathematics, in particular those of geometry, rest on experience; and, as regards geometry, the same view is on very different grounds maintained by the mathematician Riemann.

It is not so easy as at first sight it appears to make out how far the views taken by Mill in his "System of Logic Ratiocinative and Inductive" (ninth edition, 1879) are absolutely contradictory to those which have been spoken of; they profess to be so; there are most definite assertions (supported by argument), for instance, p. 263:—"It remains to inquire what is the ground of our belief in axioms, what is the evidence on which they rest. I answer, they are experimental truths, generalisations from experience. The proposition 'Two straight lines cannot inclose a space,' or, in other words, two straight lines which have once met cannot meet again, is an induction from the evidence of our senses." But I cannot help considering a previous argument (p. 259) as very materially modifying this absolute contradiction. After inquiring "Why are mathematics by almost all philosophers . . . considered to be independent of the evidence of experience and observation, and characterised as systems of necessary truth?" Mill proceeds (I quote the whole passage) as follows:—"The answer I conceive to be that this character of necessity ascribed to the truths of mathematics, and even (with some reservations to be hereafter made) the peculiar certainty ascribed to them, is a delusion, in order to sustain which it is necessary to suppose that those truths relate to and express the properties of purely imaginary objects. It is acknowledged that the conclusions of geometry are derived partly at least from the so-called definitions, and that these definitions are assumed to be correct representations, as far as they go, of the objects with which geometry is conversant. Now we have pointed out that from a definition as such no proposition, unless it be one concerning the meaning of a word, can ever follow, and that what apparently follows from a definition follows in reality from an implied assumption that there exists a real thing conformable thereto. This assumption in the case of the definitions of geometry is not strictly true: there exist no real things exactly conformable to the definitions. There exist no real points without magnitude, no lines without breadth, nor perfectly straight, no circles with all their radii exactly equal, nor squares with all their angles perfectly right. It will be said that the assumption does not extend to the actual but only to the possible existence of such things. I answer that according to every test we have of possibility they are not even possible. Their existence, so far as we can form any judgment, would seem to be inconsistent with the physical constitution of our planet at least, if not of the universal [*sic*]. To get rid of this difficulty, and at the same time to save the credit of the supposed system of necessary truths, it is customary to say that the points, lines, circles, and squares which are the subjects of geometry, exist in our conceptions merely, and are parts of our minds: which minds, by working on their own materials, construct an *a priori* science, the evidence of which is purely mental and has nothing to do with outward experience. By howsoever high authority this doctrine has been sanctioned, it appears to me psychologically incorrect. The points, lines, and squares which any one has in his mind, are (as I apprehend) simply copies of the points, lines, and squares which he has known in his experience. Our idea of a point I apprehend to be simply our idea of the *minimum visibile*, the small portion of surface which we can see. We can reason about a line as if it had no breadth, because we have a power which we can exercise over the operations of our minds: the power, when a perception is present to our senses or a conception to our intellects, of *attending* to a part only of that perception or conception instead of the whole. But we cannot *conceive* a line without breadth: we can form no mental picture of such a line: all the lines which we have in our mind are lines possessing breadth. If any one doubt this, we may refer him to his own experience. I much question if any one who fancies that he can conceive of a mathematical line thinks so from the evidence of his own consciousness. I suspect it is rather because he supposes that unless such a perception be possible, mathematics could not exist as a science: a supposition which there will be no difficulty in showing to be groundless."

I think it may be at once conceded that the truths of geometry are truths precisely because they relate to and express the properties of what Mill calls "purely imaginary objects"; that these objects do not exist in Mill's sense, that they do not exist in nature, may also be granted; that they are "not even possible," if this means not possible in an existing nature, may also be granted. That we cannot "conceive" them depends on the meaning which we attach to the word conceive. I would myself say that the purely imaginary objects are the only realities, the ὄντως ὄντα, in regard to which the corresponding physical objects are as the shadows in the cave; and it is only by means of them that we are able to deny the existence of a corresponding physical object; if there is no conception of straightness, then it is meaningless to deny the existence of a perfectly straight line.

But at any rate the objects of geometrical truth are the so-called imaginary objects of Mill, and the truths of geometry are only true, and *a fortiori* are only necessarily true, in regard to these so-called imaginary objects; and these objects, points, lines, circles, &c., in the mathematical sense of the terms, have a likeness to and are represented more or less imperfectly, and from a geometer's point of view no matter how imperfectly, by corresponding physical points, lines, circles, &c. I shall have to return to geometry, and will then speak of Riemann, but I will first refer to another passage of the "Logic."

Speaking of the truths of arithmetic Mill says (p. 297) that even here there is one hypothetical element: "In all propositions concerning numbers a condition is implied without which none of them would be true, and that condition is an assumption which may be false. The condition is that $1 = 1$: that all the numbers are numbers of the same or of equal units." Here at least the assumption may be absolutely true; one shilling = one shilling in purchasing power, although they may not be absolutely of the same weight and fineness: but it is hardly necessary; one coin + one coin = two coins, even if the one be a shilling and the other a half-crown. In fact, whatever difficulty be raisable as to geometry, it seems to me that no similar difficulty applies to arithmetic; mathematician or not, we have each of us, in its most abstract form, the idea of a number; we can each of us appreciate the truth of a proposition in regard to numbers; and we cannot but see that a truth in regard to numbers is something different in kind from an experimental truth generalised from experience. Compare, for instance, the proposition that the sun, having already risen so many times, will rise to-morrow, and the next day, and the day after that, and so on; and the proposition that even and odd numbers succeed each other alternately *ad infinitum*: the latter at least seems to have the characters of universality and necessity. Or, again, suppose a proposition observed to hold good for a long series of numbers, one thousand numbers, two thousand numbers, as the case may be: this is not only no proof, but it is absolutely no evidence, that the proposition is a true proposition, holding good for all numbers whatever; there are in the Theory of Numbers very remarkable instances of propositions observed to hold good for very long series of numbers and which are nevertheless untrue.

I pass in review certain mathematical theories.

In arithmetic and algebra, or say in analysis, the numbers or magnitudes which we represent by symbols are in the first instance ordinary (that is, positive) numbers or magnitudes. We have also in analysis and in analytical geometry *negative* magnitudes; there has been in regard to these plenty of philosophical discussion, and I might refer to Kant's paper, "Ueber die negativen Grössen in die Weltweisheit" (1763), but the notion of a negative magnitude has become quite a familiar one, and has extended itself into common phraseology. I may remark that it is used in a very refined manner in bookkeeping by double entry.

But it is far otherwise with the notion which is really the fundamental one (and I cannot too strongly emphasise the assertion) underlying and pervading the whole of modern analysis and geometry, that of imaginary magnitude in analysis and of imaginary space (or space as a *locus in quo* of imaginary points and figures) in geometry: I use in each case the word imaginary as including real. This has not been, so far as I am aware, a subject of philosophical discussion or inquiry. As regards the older metaphysical writers, this would be quite accounted for by saying that they knew nothing, and were not bound to know anything, about it; but at present, and considering the prominent position which the notion occupies—say even that the conclusion were that the notion belongs to mere technical mathematics, or has reference to nonentities in regard to which no science is possible, still it seems to me that (as a subject of philosophical discussion) the notion ought not to be thus ignored; it should at least be shown that there is a right to ignore it.

Although in logical order I should perhaps now speak of the notion just referred to, it will be convenient to speak first of some other quasi-geometrical notions; those of more-than-three-dimensional space, and of non-Euclidian two- and three-dimensional space, and also of the generalised notion of distance. It is in connection with these that Riemann considered that our notion of space is founded on experience, or rather that it is only by experience that we know that our space is Euclidian space.

It is well known that Euclid's twelfth axiom, even in Playfair's form of it, has been considered as needing demonstration; and that Lobatschewsky constructed a perfectly consistent theory wherein this axiom was assumed not to hold good, or say a system of non-Euclidian plane geometry. There is a like system of non-Euclidian solid geometry. My own view is that Euclid's twelfth axiom in Playfair's form of it does not need demonstration, but is part of our notion of space, of the physical space of our experience—the space, that is, which we become acquainted with by experience, but which is the representation lying at the foundation of all external experience. Riemann's view before referred to may I think be said to be that, having *in intellectu* a more general notion of space (in fact a notion of non-Euclidian space), we learn by experience that space (the physical space of our experience) is, if not exactly, at least to the highest degree of approximation, Euclidian space.

But, suppose the physical space of our experience to be thus only approximately Euclidian space, what is the consequence which follows? *Not* that the propositions of geometry are only approximately true, but that they remain absolutely true in regard to that Euclidian space which has been so long regarded as being the physical space of our experience.

It is interesting to consider two different ways in which, without any modification at all of our notion of space, we can arrive at a system of non-Euclidian (plane or two-dimensional) geometry; and the doing so will, I think, throw some light on the whole question.

First, imagine the earth a perfectly smooth sphere; understand by a plane the surface of the earth, and by a line the apparently straight line (in fact an arc of great circle) drawn on the surface; what experience would in the first instance teach would be Euclidian geometry; there would be intersecting lines which produced a few miles or so would seem to go on diverging, and apparently parallel lines which would exhibit no tendency to approach each other; and the inhabitants might very well conceive that they had by experience established the axiom that two straight lines cannot inclose a space, and the axiom as to parallel lines. A more extended experience and more accurate measurements would teach them that the axioms were each of them false; and that any two lines if produced far enough each way would meet in two points: they would in fact arrive at a spherical geometry, accurately representing the properties of the two-dimensional space of their experience. But their original Euclidian geometry would not the less be a true system; only it would apply to an ideal space, not the space of their experience.

Secondly, consider an ordinary, indefinitely extended plane; and let us modify only the notion of distance. We measure distance, say, by a yard measure or a foot rule, anything which is short enough to make the fractions of it of no consequence (in mathematical language by an infinitesimal element of length); imagine, then, the length of this rule constantly changing (as it might do by an alteration of temperature), but under the condition that its actual length shall depend only on its situation on the plane and on its direction: viz., if for a given situation and direction it has a certain length, then whenever it comes back to the same situation and direction it must have the same length. The distance along a given straight or curved line between any two points could then be measured in the ordinary manner with this rule, and would have a perfectly determinate value; it could be measured over and over again, and would always be the same; but of course it would be the distance, not in the ordinary acceptation of the term, but in quite a different acceptation. Or in a somewhat different way: if the rate of progress from a given point in a given direction be conceived as depending only on the configuration of the ground, and the distance along a given path between any two points thereof be measured by the time required for traversing it, then in this way also the distance would have a perfectly determinate value; but it would be a distance, not in the ordinary acceptation of the term, but in quite a different acceptation. And corresponding to the new notion of distance, we should have a new, non-Euclidian system of plane geometry; all theorems involving the notion of distance would be altered.

We may proceed further. Suppose that as the rule moves away from a fixed central point of the plane it becomes shorter and shorter; if this shortening takes place with sufficient rapidity, it may very well be that a distance which in the ordinary sense of the word is finite will in the new sense be infinite; no number of repetitions of the length of the ever-shortening rule will be sufficient to cover it. There will be surrounding the central point a certain finite area such that (in the new acceptation of the term distance) each point of the boundary thereof will be at an infinite distance from the central point; the points outside this area you cannot by any means arrive at with your rule; they will form a *terra incognita*, or rather an unknowable land: in mathematical language, an imaginary or impossible space: and the plane space of the theory will be that within the finite area—that is, it will be finite instead of infinite.

We thus with a proper law of shortening arrive at a system of non-Euclidian geometry which is essentially that of Lobatschewsky. But in so obtaining it we put out of sight its relation to spherical geometry: the three geometries (spherical, Euclidian, and Lobatschewsky's) should be regarded as members of a system: viz., they are the geometries of a plane (two-dimensional) space of constant positive curvature, zero-curvature, and constant negative curvature respectively; or, again, they are the plane geometries corresponding to three different notions of distance;

in this point of view they are Klein's elliptic, parabolic, and hyperbolic geometries respectively.

Next as regards solid geometry: we can by a modification of the notion of distance (such as has just been explained in regard to Lobatschewsky's system) pass from our present system to a non-Euclidian system; for the other mode of passing to a non-Euclidian system it would be necessary to regard our space as a flat three-dimensional space existing in a space of four dimensions (*i.e.* as the analogue of a plane existing in ordinary space); and to substitute for such flat three-dimensional space a curved three-dimensional space, say of constant positive or negative curvature. In regarding the physical space of our experience as possibly non-Euclidian, Riemann's idea seems to be that of modifying the notion of distance, not that of treating it as a locus in four-dimensional space.

I have just come to speak of four-dimensional space. What meaning do we attach to it? or can we attach to it any meaning? It may be at once admitted that we cannot conceive of a fourth dimension of space; that space as we conceive of it, and the physical space of our experience, are alike three-dimensional; but we can, I think, conceive of space as being two- or even one-dimensional; we can imagine rational beings living in a one-dimensional space (a line) or in a two-dimensional space (a surface), and conceiving of space accordingly, and to whom, therefore, a two-dimensional space, or (as the case may be) a three-dimensional space, would be as inconceivable as a four-dimensional space is to us. And very curious speculative questions arise. Suppose the one-dimensional space a right line, and that it afterwards becomes a curved line: would there be any indication of the change? Or, if originally a curved line, would there be anything to suggest to them that it was not a right line? Probably not, for a one-dimensional geometry hardly exists. But let the space be two-dimensional, and imagine it originally a plane, and afterwards bent (converted, that is, into some form of developable surface) or converted into a curved surface; or imagine it originally a developable or curved surface. In the former case there should be an indication of the change, for the geometry originally applicable to the space of their experience (our own Euclidian geometry) would cease to be applicable; but the change could not be apprehended by them as a bending or deformation of the plane, for this would imply the notion of a three-dimensional space in which this bending or deformation could take place. In the latter case their geometry would be that appropriate to the developable or curved surface which is their space: viz. this would be their Euclidian geometry: would they ever have arrived at our own more simple system? But take the case where the two-dimensional space is a plane, and imagine the beings of such a space familiar with our own Euclidian plane geometry; if, a third dimension being still inconceivable by them, they were by their geometry or otherwise led to the notion of it, there would be nothing to prevent them from forming a science such as our own science of three-dimensional geometry.

Evidently all the foregoing questions present themselves in regard to ourselves, and to three-dimensional space as we conceive of it, and as the physical space of our experience. And I need hardly say that the first step is the difficulty, and that granting a fourth dimension we may assume as many more dimensions as we please. But whatever answer be given to them, we have, as a branch of mathematics, potentially, if not actually, an analytical geometry of n-dimensional space. I shall have to speak again upon this.

Coming now to the fundamental notion already referred to, that of imaginary magnitude in analysis and imaginary space in geometry: I connect this with two great discoveries in mathematics made in the first half of the seventeenth century, Harriot's representation of an equation in the form $f(x) = 0$, and the consequent notion of the roots of an equation as derived from the linear factors of $f(x)$ (Harriot 1560-1621: his "Algebra," published after his death, has the date 1631), and Descartes' method of coordinates, as given in the "Géometrie," forming a short supplement to his "Traité de la Méthode, &c." (Leyden, 1637).

I show how by these we are led analytically to the notion of imaginary points in geometry; for instance, we arrive at the theorem that a straight line and circle in the same plane intersect *always* in two points, real or imaginary. The conclusion as to the two points of intersection cannot be contradicted by experience: take a sheet of paper and draw on it the straight line and circle, and try. But you might say, or at least be strongly tempted to say, that it is meaningless. The question of course arises, What is the meaning of an imaginary point? and, further, In what manner can the notion be arrived at geometrically?

There is a well known construction in perspective for drawing lines through the intersection of two lines which are so nearly parallel as not to meet within the limits of the sheet of paper. You have two given lines which do not meet, and you draw a third line, which, when the lines are all of them produced, is found to pass through the intersection of the given lines. If instead of lines we have two circular arcs not meeting each other, then we can, by means of these arcs, construct a line; and if on completing the circles it is found that the circles intersect each other in two real points, then it will be found that the line passes through these two points: if the circles appear not to intersect, then the line will appear not to intersect either of the circles. But the geometrical construction being in each case the same, we say that in the second case also the line passes through the two intersections of the circles.

Of course it may be said in reply that the conclusion is a very natural one, provided we assume the existence of imaginary points; and that, this assumption not being made, then, if the circles do not intersect, it is meaningless to assert that the line passes through their points of intersection. The difficulty is not got over by the analytical method before referred to, for this introduces difficulties of its own: is there in a plane a point the coordinates of which have given imaginary values? As a matter of fact, we do consider in plane geometry imaginary points introduced into the theory analytically or geometrically as above.

The like considerations apply to solid geometry, and we thus arrive at the notion of imaginary space as a *locus in quo* of imaginary points and figures.

I have used the word imaginary rather than complex, and I repeat that the word has been used as including real. But, this once understood, the word becomes in many cases superfluous, and the use of it would even be misleading. Thus, "a problem has so many solutions:" this means so many imaginary (including real) solutions. But if it were said that the problem had "so many imaginary solutions," the word "imaginary" would here be understood to be used in opposition to real. I give this explanation the better to point out how wide the application of the notion of the imaginary is, viz. (unless expressly or by implication excluded) it is a notion implied and presupposed in all the conclusions of modern analysis and geometry. It is, as I have said, the fundamental notion underlying and pervading the whole of these branches of mathematical science.

I consider the question of the geometrical representation of an imaginary variable. We represent the imaginary variable $x + iy$ by means of a point in a plane, the coordinates of which are (x, y). This idea, due to Gauss, dates from about the year 1831. We thus picture to ourselves the succession of values of the imaginary variable $x + iy$ by means of the motion of the representative point: for instance, the succession of values corresponding to the motion of the point along a closed curve to its original position. The value $X + iY$ of the function can of course be represented by means of a point (taken for greater convenience in a different plane), the coordinates of which are X, Y.

We may consider in general two points, moving each in its own plane, so that the position of one of them determines the position of the other, and consequently the motion of the one determines the motion of the other: for instance, the two points may be the tracing-point and the pencil of a pentagraph. You may with the first point draw any figure you please, there will be a corresponding figure drawn by the second point: for a good pentagraph a copy on a different scale (it may be); for a badly-adjusted pentagraph, a distorted copy; but the one figure will always be a sort of copy of the first, so that to each point of the one figure there will correspond a point in the other figure.

In the case above referred to, where one point represents the value $x + iy$ of the imaginary variable and the other the value $X + iY$ of some function $\phi(x + iy)$ of that variable, there is a remarkable relation between the two figures: this is the relation of orthomorphic projection, the same which presents itself between a portion of the earth's surface and the representation thereof by a map on the stereographic projection or on Mercator's projection—viz., any indefinitely small area of the one figure is represented in the other figure by an indefinitely small area of the same shape. There will possibly be for different parts of the figure great variations of scale, but the shape will be unaltered; if for the one area the boundary is a circle, then for the other area the boundary will be a circle; if for one it is an equilateral triangle, then for the other it will be an equilateral triangle.

I have been speaking of an imaginary variable $(x + iy)$, and of a function $\phi(x + iy) = X + iY$ of that variable, but the theory may equally well be stated in regard to a plane curve: in fact the $x + iy$ and the $X + iY$ are two imaginary variables connected by an equation; say their values are u and v, connected by an equation $F(u, v) = 0$; then, regarding u, v as the coordinates of a point *in plano*, this will be a point on the curve represented by the equation. The curve, in the widest sense of the expression, is the whole series of points, real or imaginary, the coordinates of which satisfy the equation, and these are exhibited by the foregoing corresponding figures in two planes; but in the ordinary sense the curve is the series of real points, with coordinates u, v, which satisfy the equation.

In geometry it is the curve, whether defined by means of its equation, or in any other manner, which is the subject for contemplation and study. But we also use the curve as a representation of its equation—that is, of the relation existing between two magnitudes x, y, which are taken as the coordinates of a point on the curve. Such employment of a curve for all sorts of purposes—the fluctuations of the barometer, the Cambridge boat races, or the Funds—is familiar to most of you. It is in like manner convenient in analysis, for exhibiting the relations between any three magnitudes x, y, z, to regard them as the coordinates of a point in space; and, on the like ground, we should at least wish to regard any four or more magnitudes as the coordinates of a point in space of a corresponding number of dimensions. Starting with the hypothesis of such a space, and of points therein each determined by means of its coordinates, it is found possible to establish a system of n-dimensional geometry analogous in every respect to our two- and three-dimensional geometries, and to a very considerable extent serving to exhibit the relations of the variables.

It is to be borne in mind that the space, whatever its dimensionality may be, must always be regarded as an imaginary or complex space such as the two- or three-dimensional space of ordinary geometry; the advantages of the representation would otherwise altogether fail to be obtained.

I omit some further developments in regard to geometry; and all that I have written as to the connection of mathematics with the notion of time.

I said that I would speak to you, not of the utility of the mathematics in any of the questions of common life or of physical science, but rather of the obligations of mathematics to these different subjects. The consideration which thus presents itself is in a great measure that of the history of the development of the different branches of mathematical science in connection with the older physical sciences, astronomy and mechanics: the mathematical theory is in the first instance suggested by some question of common life or of physical science, is pursued and studied quite independently thereof, and perhaps after a long interval comes in contact with it, or with quite a different question. Geometry and algebra must, I think, be considered as each of them originating in connection with objects or questions of common life—geometry, notwithstanding its name, hardly in the measurement of land, but rather from the contemplation of such forms as the straight line, the circle, the ball, the top (or sugar-loaf): the Greek geometers appropriated for the geometrical forms corresponding to the last two of these, the words σφαῖρα and κῶνος, our cone and sphere, and they extended the word cone to mean the complete figure obtained by producing the straight lines of the surface both ways indefinitely. And so algebra would seem to have arisen from the sort of easy puzzles in regard to numbers which may be made, either in the picturesque forms of the Bija-Ganita with its maiden with the beautiful locks, and its swarms of bees amid the fragrant blossoms, and the one queen bee left humming around the lotus flower; or in the more prosaic form in which a student has presented to him in a modern text-book a problem leading to a simple equation.

The Greek geometry may be regarded as beginning with Plato (B.C. 430–347): the notions of geometrical analysis, loci, and the conic sections are attributed to him, and there are in his "Dialogues" many very interesting allusions to mathematical questions: in particular the passage in the "Theætetus," where he affirms the incommensurability of the sides of certain squares. But the earliest extant writings are those of Euclid (B.C. 285): there is hardly anything in mathematics more beautiful than his wondrous fifth book; and he has also in the seventh, eighth, ninth, and tenth books fully and ably developed the first principles of the Theory of Numbers, including the theory of incommensurables. We have next Apollonius (about B.C. 247), and Archimedes (B.C. 287–212), both geometers of the highest merit, and the latter of them the founder of the science of statics (including therein hydrostatics): his dictum about the lever, his "Εὕρηκα," and the story of the defence of Syracuse, are well known. Following these we have a worthy series of names, including the astronomers Hipparchus (B.C. 150) and Ptolemy (A.D. 125), and ending, say, with Pappus (A.D. 400), but continued by their Arabian commentator , and the Italian and other European geometers of the sixteenth century and later, who pursued the Greek geometry.

The Greek arithmetic was, from the want of a proper notation, singularly cumbrous and difficult; and it was for astronomical purposes superseded by the sexagesimal arithmetic, attributed to Ptolemy, but probably known before his time. The use of the present so-called Arabic figures became general among Arabian writers on arithmetic and astronomy about the middle of the tenth century, but it was not introduced into Europe until about two centuries later. Algebra among the Greeks is represented almost exclusively by the treatise of Diophantus (A.D. 150), in fact a work on the Theory of Numbers containing questions relating to square and cube numbers, and other properties of numbers, with their solutions; this has no historical connection with the later algebra introduced into Italy from the East by Leonardi Bonacci of Pisa (A.D. 1202–1208), and successfully cultivated in the fifteenth and sixteenth centuries by Lucas Paciolus, or De Burgo, Tartaglia, Cardan, and Ferrari. Later on we have Vieta (1540–1603), Harriot, already referred to, Wallis, and others.

Astronomy is of course intimately connected with geometry; the most simple facts of observation of the heavenly bodies can only be *stated* in geometrical language; for instance, that the stars describe circles about the Pole-star, or that the different positions of the sun among the fixed stars in the course of the year form a circle. For astronomical calculations it was found necessary to determine the arc of a circle by means of its chord; the notion is as old as Hipparchus, a work of whom is referred to as consisting of twelve books on the chords of circular arcs; we have (A.D. 125) Ptolemy's "Almagest," the first book of which contains a table of arcs and chords with the method of construction; and among other theorems on the subject he gives there the theorem afterwards inserted in Euclid (Book VI. Prop. D.) relating to the rectangle contained by the diagonals of a quadrilateral inscribed in a circle. The Arabians made the improvement of using in place of the chord of an arc the sine, or half chord of double the arc, and so brought the theory into the form in which it is used in modern trigonometry: the before-mentioned theorem of Ptolemy, or rather a particular case of it, translated into the notation of sines, gives the expression for the sine of the sum of two arcs in terms of the sines and cosines of the component arcs; and it is thus the fundamental theorem on the subject. We have in the fifteenth and sixteenth centuries a series of mathematicians who with wonderful enthusiasm and perseverance calculated tables of the trigonometrical or circular functions, Purbach, Müller or Regiomontanus, Copernicus, Reinhold, Maurolycus, Vieta, and many others; the tabulations of the functions tangent and secant are due to Reinhold and Maurolycus respectively.

Logarithms were invented, not exclusively with reference to the calculation of trigonometrical tables, but in order to facilitate numerical calculations generally; the invention is due to John Napier of Merchiston, who died in 1618 at sixty-seven years of age; the notion was based upon refined mathematical reasoning on the comparison of the spaces described by two points, the one moving with a uniform velocity, the other with a velocity varying according to a given law. It is to be observed that Napier's logarithms were nearly but not exactly those which are now called (sometimes Napierian, but more usually) hyperbolic logarithms—those to the base e; and that the change to the base 10 (the great step by which the invention was perfected for the object in view) was indicated by Napier but actually made by Henry Briggs, afterwards Savilian Professor at Oxford (d. 1630). But it is the hyperbolic logarithm which is mathematically important. The direct function e^x or exp. x, which has for its inverse the hyperbolic logarithm, presented itself, but not in a prominent way. Tables were calculated of the logarithms of numbers, and of those of the trigonometrical functions.

The circular function and the logarithm were thus invented each for a practical purpose, separately and without any proper connection with each other. The functions are connected through the theory of imaginaries, and form together a group of the utmost importance throughout mathematics: but this is mathematical theory; the obligation of mathematics is for the discovery of the functions.

Forms of spirals presented themselves in Greek architecture, and the curves were considered mathematically by Archimedes; the Greek geometers invented some other curves, more or less interesting, but recondite enough in their origin. A curve which might have presented itself to anybody, that described by a point in the circumference of a rolling carriage wheel, was first noticed by Mersenne in 1615, and is the curve afterwards considered by Roberval, Pascal, and others, under the name of the Roulette, otherwise the Cycloid. Pascal (1623-1662) wrote at the age of seventeen his "Essais pour les Coniques," in seven short pages, full of new views on these curves, and in which he gives, in a paragraph of eight lines, his theory of the inscribed hexagon.

Kepler (1571-1630) by his empirical determination of the laws of planetary motion, brought into connection with astronomy one of the forms of conic, the ellipse, and established a foundation for the theory of gravitation. Contemporary with him, for most of his life, we have Galileo (1564-1642), the founder of the science of dynamics; and closely following upon Galileo, we have Isaac Newton (1643-1727): the "Philosophiæ naturalis Principia Mathematica," known as the "Principia," was first published in 1687.

The physical, statical, or dynamical questions which presented themselves before the publication of the "Principia" were of no particular mathematical difficulty, but it is quite otherwise with the crowd of interesting questions arising out of the theory of gravitation, and which, in becoming the subject of mathematical investigation, have contributed very much to the advance of mathematics. We have the problem of two bodies, or what is the same thing, that of the motion of a particle about a fixed centre of force, for any law of force; we have also the (mathematically very interesting) problem of the motion of a body attracted to two or more fixed centres of force; then, next preceding that of the actual solar system—the problem of three bodies; this has ever been and is far beyond the power of mathematics, and it is in the lunar and planetary theories replaced by what is mathematically a different problem, that of the motion of a body under the action of a principal central force and a disturbing force; or (in one mode of treatment) by the problem of disturbed elliptic motion. I would remark that we have here an instance in which an astronomical fact, the observed slow variation of the orbit of a planet, has directly suggested a mathematical method, applied to other dynamical problems, and which is the basis of very extensive modern investigations in regard to systems of differential equations. Again, immediately arising out of the theory of gravitation, we have the problem of finding the attraction of a solid body of any given form upon a particle, solved by Newton in the case of a homogeneous sphere, but which is far more difficult in the next succeeding cases of the spheroid of revolution (very ably treated by Maclaurin) and of the ellipsoid of three unequal axes: there is perhaps no problem of mathematics which has been treated by as great a variety of methods, or has given rise to so much interesting investigation as this last problem of the attraction of an ellipsoid upon an interior or exterior point. It was a dynamical problem, that of vibrating strings, by which Lagrange was led to the theory of the representation of a function as the sum of a series of multiple sines and cosines; and connected with this we have the expansions in terms of Legendre's functions P_n, suggested to him by the question just referred to of the attraction of an ellipsoid; the subsequent investigations of Laplace on the attractions of bodies differing slightly from the sphere led to the functions of two variables called Laplace's functions. I have been speaking of ellipsoids, but the general theory is that of attractions, which has become a very wide branch of modern mathematics; associated with it we have in particular the names of Gauss, Lejeune-Dirichlet, and Green; and I must not omit to mention that the theory is now one relating to n-dimensional space. Another great problem of celestial mechanics, that of the motion of the earth about its centre of gravity, in the most simple case, that of a body not acted upon by any forces, is a very interesting one in the mathematical point of view.

I may mention a few other instances where a practical or physical question has connected itself with the development of mathematical theory. I have spoken of two map projections—the stereographic, dating from Ptolemy; and Mercator's projection, invented by Edward Wright about the year 1600: each of these, as a particular case of the orthomorphic projection, belongs to the theory of the geometrical representation of an imaginary variable. I have spoken also of perspective, and (in an omitted paragraph) of the representation of solid figures employed in Monge's descriptive geometry. Monge, it is well known, is the author of the geometrical theory of the curvature of surfaces and of curves of curvature: he was led to this theory by a problem of earthwork—from a given area, covered with earth of uniform thickness, to carry the earth and distribute it over an equal given area, with the least amount of cartage. For the solution of the corresponding problem in solid geometry he had to consider the intersecting normals of a surface, and so arrived at the curves of curvature (see his "Mémoire sur les Déblais et les Remblais," *Mém. de l'Acad.*, 1781). The normals of a surface are, again, a particular case of a doubly infinite system of lines, and are so connected with the modern theories of congruences and complexes.

The undulatory theory of light led to Fresnel's wave-surface, a surface of the fourth order, by far the most interesting one which had then presented itself. A geometrical property of this surface, that of having tangent planes each touching it along a plane curve (in fact, a circle), gave to Sir W. R. Hamilton the theory of conical refraction. The wave-surface is now regarded in geometry as a particular case of Kummer's quartic surface, with sixteen conical points and sixteen singular tangent planes.

My imperfect acquaintance as well with the mathematics as the physics prevents me from speaking of the benefits which the theory of Partial Differential Equations has received from the hydrodynamical theory of vortex motion, and from the great physical theories of electricity, magnetism, and energy.

It is difficult to give an idea of the vast extent of modern mathematics. This word "extent" is not the right one: I mean extent crowded with beautiful detail—not an extent of mere uniformity, such as an objectless plain, but of a tract of beautiful country seen at first in the distance, but which will bear to be rambled through and studied in every detail of hillside and valley, stream, rock, wood, and flower. But, as for anything else, so for a mathematical theory—beauty can be perceived, but not explained. As for mere extent, I might illustrate this by speaking of the dates at which some of the great extensions have been made in several branches of mathematical science.

And in fact, in the Address as written, I speak at considerable length of the extensions in geometry since the time of Descartes, and in other specified subjects since the commencement of the century: these subjects are the general theory of the function of an imaginary variable; the leading known functions, viz. the elliptic and single theta-functions and the Abelian and multiple theta-functions; the Theory of Equations and the Theory of Numbers. I refer also to some theories outside of ordinary mathematics: the multiple algebra or linear associative algebra of the late Benjamin Peirce; the theory of Argand, Warren, and Peacock in regard to imaginaries in plane geometry; Sir W. R. Hamilton's quaternions, Clifford's biquaternions, the theories developed in Grassmann's "Ausdehnungslehre," with recent extensions thereof to non-Euclidian space by Mr. Homersham Cox; also Boole's "Mathematical Logic," and a work connected with logic, but primarily mathematical and of the highest importance, Schubert's "Abzählende Geometrie" (1878). I remark that all this in regard to theories outside of ordinary mathematics is still on the text of the vast extent of modern mathematics.

In conclusion I would say that mathematics have steadily advanced from the time of the Greek geometers. Nothing is lost or wasted; the achievements of Euclid, Archimedes, and Apollonius are as admirable now as they were in their own days. Descartes' method of coordinates is a possession for ever. But mathematics have never been cultivated more zealously and diligently, or with greater success, than in this century—in the last half of it, or at the present time: the advances made have been enormous, the actual field is boundless, the future full of hope. In regard to pure mathematics we may most confidently say:—

"Yet I doubt not through the ages one increasing purpose runs,
And the thoughts of men are widened with the process of the suns."

SECTION A

MATHEMATICAL AND PHYSICAL

OPENING ADDRESS BY PROF. OLAUS HENRICI, PH.D., F.R.S., PRESIDENT OF THE SECTION.

ON reading through the addresses delivered by my predecessors in this office, I was struck by the fact that in nearly every case the speaker began with a lamentation over his unfitness for the work before him, and those seemed to me to be the more eloquent on these points who showed by their address that they least needed an excuse. The amount of excuse given appears in fact to be directly proportional to the gifts of the speaker, and hence inversely proportional to the need of such an excuse.

Under these circumstances I cannot express my sense of my own unfitness for this post better than by saying nothing about it. I must, however, beg your indulgence for my shortcomings, both as regards my address and my manner of conducting the general business of this section.

As the Presidential chair is occupied by one of the most illustrious of mathematicians, it would be presumptuous for me to attempt to give an account of the recent progress of mathematics. I propose only to speak for a short time on that part of mathematics which has always been most attractive to myself—that is, pure geometry as apart from algebra, but I shall confine myself to some considerations relating to the teaching of geometry in this country. Pure geometry seems to me to be of the greatest educational value, and almost indispensable in many applications; but it has scarcely ever been introduced at Cambridge, the centre of mathematics and mathematical education in England.

The number of geometrical methods now in use is astonishingly great. These differ, on the one hand, according to the nature of the result aimed at, but, on the other, according to the amount of algebra employed, and to the relation in which this algebra stands to the pure "*Anschauung*." I use the word *Anschauung* because I know of no English equivalent; the German word has the philosophic meaning rendered by intuition, and retains its original concrete meaning of looking at a thing, and might perhaps be translated: intuition by inspection. It is the inspection of figures which is of the greatest importance in geometry. It is hereby of little consequence whether the figures are seen by the physical eye or only mentally; because the conception of that space in which we perceive everything and without which we can perceive nothing, which therefore is, according to Kant, a form of our *Anschauung*, is built up in our mind through many generations in conformity with sensual impressions.

It would be of interest, if time permitted, to follow up the gradual development and extension of geometry into the wider science of algebra, from the first introduction of the latter in the theory of proportion to the present state, where there exists really no essential difference between the two, where geometry is only one manifestation of algebra, but so complete a one that at least within its number of dimensions it again contains algebra.

In some of the methods just referred to no algebra is used at all, whilst others may be distinguished according to the nature of the algebra used, whether equations containing one, two, three, or more variables are employed. In such a division, Von Staudt's system, without a vestige of algebra, would occupy the one end, and the purely algebraical theory of invariants with geometrical interpretation the other.

There is, however, not only a difference in the amount of algebra used, but, if possible, a greater one in the manner in which the symbols are interpreted. And it is here that algebra has apparently the greater power. One algebraical theorem, by being read in different ways, by giving ever different meanings to the symbols, reveals a variety of geometrical and other theorems. We have in it the crystallised form, the very essence of the mathematical truth, but in the most abstract form conceivable. Now this most abstract form is the highest and the most perfect which mathematical truth as such can assume, and which it must assume before a theory is really complete in the eyes of a pure mathematician. It is only in this shape that it is ready to be turned to account in any direction where it may be needed.

In thus placing algebra on the highest pinnacle, the reasons will be apparent which will make many mathematicians, not to mention others, prefer the truths it reveals cast in a mould which connects them with concrete things rather than with abstract notions. In fact, to be thoroughly at home in the highest theories of pure algebra requires some of the genius of men like Cayley and Sylvester who have founded, and to a great extent built up, modern algebra. But even they constantly make use of geometry to assist them in their investigations, and no one could have expressed this more strongly than Prof. Sylvester himself in his brilliant address delivered from this chair at the Exeter meeting of our Association.

If this is so, surely every progress in the spread of the knowledge of pure geometry should be welcomed and encouraged; but in England pure geometry is almost unknown excepting in the elements as contained in Euclid and in the old-fashioned geometrical conics. The modern methods of synthetic projective geometry as developed on the Continent have never become generally known here. The few men who have thoroughly made themselves acquainted with them, and who have preferred purely geometrical reasoning, have not belonged to Cambridge, and have thus stood somewhat outside the national system of training mathematical teachers. The late Prof. Smith introduced these methods at Oxford, and there was some expectation that he would have written, if he had been spared, a text-book which might have done much to introduce the subject more widely. His principal mathematical work lay, however, in another direction.

The one English mathematician whose mathematical thought is purely geometrical is Dr. Hirst, a pupil of Steiner, who in the position which he has just relinquished has been able to introduce, as the first, modern geometrical methods into a regular system of professional education, whilst showing at the same time by his original work what can be done with these methods.

Other mathematicians who have studied these methods—and I believe there are many—have made use of them by translating the geometrical into algebraical reasoning.

Towards the early possibility of such a translation much was done by the labours of the late Mr. Spottiswoode, who years ago wrote the first connected treatise on the theory of determinants, and who up to the last few years employed some of his leisure hours in working out geometrical problems, the work consisting always of some beautiful piece of algebra.

It is not often that our Section has to mourn in one year the loss of two such men as Smith and Spottiswoode.

It is easy to see how the neglect complained of has come to pass. In England when mathematics, after having lain dormant for about a century, began to revive, the first necessity was to become acquainted with the enormous amount of work meanwhile done on the Continent. This acquaintance was made through France, at that time nearly all the standard works being in the French language, which was at the same time the language best known to English students. The subjects principally taken up were the calculus and its application to mechanics. And I believe I am not far wrong when I say that the wonderful writings of Lagrange, with their extraordinary analytical elegance, had the greatest influence. But in his works anything geometrical was studiously avoided. Lagrange prided himself that there was no figure in his "Mécanique analytique."

The best analytical methods of the Continent were thus introduced into England, rapidly assimilated and made the foundation of new theories, so that the mathematical activity in this country is now at least as great as it ever has been anywhere.

But whilst analysis, algebra, and with it analytical geometry, made rapid progress, pure geometry was not equally fortunate. Here the hold which Euclid had long obtained, strengthened, no doubt, by Newton's example, prevented any change in the methods of teaching.

Most of all, perhaps, solid geometry has suffered, because Euclid's treatment of it is scanty, and it seems almost incredible that a great part of it—the mensuration of areas of simple curved surfaces and of volumes of simple solids—is not included in ordinary school teaching. The subject is, possibly, mentioned in arithmetic, where, under the name of mensuration, a number of rules are given. But the justification of these rules is not supplied, except to the student who reaches the application of the integral calculus; and what is almost worse is that the general relations of points, lines, and planes, in space, is scarcely touched upon, instead of being fully impressed on the student's mind.

The methods for doing this have long been developed in the new geometry which originated in France with Monge. But these have never been thoroughly introduced.

Works written in the German language naturally received even less attention. But it was in Germany, at the beginning of the second quarter of this century, that geometry received at the hands of several masters an impulse which put the subject on an entirely new footing.

I may mention here especially four men of whom each invented a new method and established a new system of geometry. Two of these, Möbius and Plücker, still use algebra, but in perfectly new and original manners, which, although very different from each other, have this in common, that in both we have not algebra interpreted geometrically, but rather geometry veiled in an algebraic garb. The geometrical meaning is never lost sight of.

But perfectly independent of algebra was the great Steiner, the greatest geometrician since the times of Euclid, Appolonius, and Archimedes. In his celebrated "Systematische Entwickelungen" he has laid the foundation of a pure geometry, on which a wonderful edifice has since been raised. His treatment of the principle of duality, and his method of generating conics by projective, or homographic, rows of pencils which have been extended to curves of all degrees, have given to geometrical reasoning a generality never before dreamed of. He is in one respect the opposite of Lagrange, hating and despising analysis as much as ever Lagrange disliked pure geometry. Steiner started from the geometry of the Greeks, Euclid's elements, and a few other *metrical* properties he takes for granted; but then he goes on with essentially modern methods of his own to investigate what are now called projective properties of curves and surfaces.

This metrical foundation Von Staudt changed. In his "Geometrie der Lage," published fifteen years after Steiner's "Entwickelungen," he established a most remarkable and complete system, into which the notion of a magnitude does not enter at all. He shows that projective properties of figures, which have no relation whatsoever to measurements, can be established without any mention of them. He goes so far as even to give a geometrical definition of a number in its relation to geometry as determining the position of a point, in his theory of what he calls "Würfe"; and one of the most interesting parts of his work is the purely geometrical treatment of imaginary points, lines, and planes.

In the hands of these men, and since their time, pure geometry has become a most important instrument for research, rivalling in power the more or less algebraical methods, and surpassing them all in the manner in which they raise before the mind's eye a clear realisation of the forms and figures which are the object of the investigation.

In close connection with these methods stand descriptive geometry and geometrical drawing, which teach how to represent figures on a plane or other surface. These have been treated as arts unknown at English universities, and relegated to the drawing office. Instead of this they ought to be an essential and integral part of the teaching of geometry in connection with the purely geometrical methods.

As far as the progress of science is concerned, this neglect of pure geometry in England has been of little consequence—perhaps it has rather been a gain. For science itself it is often an advantage that a centre of learning becomes one-sided, neglects many parts in order to concentrate all its energy on some particular points and make rapid progress in the directions in which these lie. At present, when mathematics flourishes as never before, when almost every nation, however small, has its eminent mathematician, there are so many such centres that what is neglected at one place is pretty surely taken up and advanced at another. But what may suffer if one side of a science is not cultivated in a country is the industry which would have gained by its applications.

In considering the teaching of any mathematical or other scientific subject, we cannot at the present time neglect the wants of the ever-increasing class of men who require what has been called technical education. Among these the large number who want mathematics at all require geometry much more than algebra and analysis, and geometry as applied to drawing and mensuration.

This want has been supplied by the numerous science classes spread over the country, with their head-quarters at the Science and Art Department at South Kensington, whose examinations—now, however, put in competition with those of the City and Guilds of London Institute, and others—have pretty much guided and regulated the teaching. A great deal of good has thus been done, but there is still much room for improvement. The teaching of geometry especially, as judged by the text-books which have come before me, is somewhat deplorable. And this is so, principally, because the spirit of Euclid and the methods of the ancient Egyptians and Greeks, rather than the fundamentally different ideas and methods of modern geometry, still rule supreme; though the latter have had their origin partly in technical wants.

In what is called geometrical drawing or practical geometry, for instance, there are first given a number of elementary constructions—such as drawing parallels and perpendiculars, or bisecting the distance between two given points. They are solved by aid of those instruments only which Euclid knew—viz. the pair of compasses for drawing circles, and the straight edge for drawing straight lines. But there is no draughtsman who would not, as a matter of course, use set squares for the former problem, and solve the latter by trial rather than by coustruction. Then again there come constructions like the division of the circumference of a circle into seven parts, which cannot be solved accurately, but which is very easily solved by trial. Instead of that, a *construction* is given which takes much more time, and is by no means more accurate. For, after all, our lines drawn on the paper are not without thickness, so that, for this reason alone, every part of the construction is affected by some small error; and it is absurd to employ a construction, though theoretically true for ideal figures as conceived in our mind, in preference to a much simpler one which, within our practical limits, is equally, or perhaps more, correct.

This is very much like the manner in which I found problems on decimal fractions treated by the candidates for the Matriculation Examination at the London University, and which reflected little credit on the manner in which the important subject of decimals is handled at our schools. It is so characteristic that I may be excused for giving it here. The problem, for instance, being to give the product of two decimal fractions, exact to, say, four decimals, each of the factors having the same number of places. This was almost regularly performed as follows. First, the decimals are converted into vulgar fractions, then these are duly multiplied, numerator by numerator, and denominator by denominator, and then the resulting fraction is again converted to a decimal, with as many places as it may yield, and, lastly, of these the first four are taken and put down, duly marked *Answer*. Or a candidate, standing however on a far higher level, multiplies both decimals out in the proper fashion, but to eight places, and cuts off four places at the end. No wonder that the public at large will hear nothing of the decimal system of weights and measures if the very essence of the decimal system of numbers is so little understood by the men who have to train the minds of the young generation!

I need scarcely say that I do not mean to blame the Science and Art Department, far less the teachers who have simply to follow suit. They act up to their light, and cannot be expected to introduce methods which are practically unknown at Cambridge, and of which the only good text-books are in foreign languages—books which are probably not at all suitable for introduction into our schools without considerable change.

It is satisfactory to learn that an association has recently been formed under the presidency of Prof. Huxley "to effect the general advancement of the profession of science and art teaching by securing improvements in the schemes of study, and the establishment of satisfactory relations between teachers and the Science and Art Department, the City and Guilds of London Institute, and other public authorities."

The good wishes of all who have the cause of sound education at heart must go with such an undertaking, one of the principal aims of which seems to be to save teaching from being any longer enslaved by examinations, and to promote greater accord between the teacher and the examiner. It is to be hoped that this association will consider geometry as one of the subjects included under the designation of science.

It is by the neglect of pure geometry and its applications to geometrical drawing that Cambridge has lost, or rather has never had, contact with the practical needs of the nation. All the marvels of modern engineering have sprung into existence without its help. The great engineers have had to depend to a degree, now unheard of, upon costly experiments, until they themselves gradually discovered mathematical methods adapted to their purposes.

Only the electrical engineer found ready to his hands a complete theory of which the mathematical part has been to a very great extent developed at Cambridge, or by men who have had their mathematical training there. This theory is, however, in its very nature less geometrical. One at least of the great men to whom the present theory of electricity is due, the late Clerk Maxwell, had the keenest appreciation of the value of modern geometry. I remember a characteristic letter of his being read to the Council of the London Mathematical Society, in which the writer, forgetting the subject of his letter, burst out into an enthusiastic praise of a German text-book, the "Geometrie der Lage," by Reye, through which Maxwell, evidently for the first time, got any idea of this subject.

The engineer will always prefer geometrical methods to analysis, and has invented for himself a great variety of them. Originally these are disjointed, being invented for special purposes. It is the business of the mathematician afterwards to connect, simplify, and extend them, as has been done to a great extent by Culmann in Zürich, or by Cremona at the Polytechnic School at Rome.

Of these methods a few may be mentioned. First of all the graphical determination of stresses in certain girders invented both by mathematicians and by engineers. Its application is so simple that no engineer will ever use any other method if once he knows this one. It is so well adapted to its purpose, that I venture to say that a simpler method is impossible, being fully aware how dangerous such a statement is. Nay, if I were asked to give the formulæ to obtain the stresses by calculation, I should write these down from a sketch of the diagram, this being the simplest way of obtaining them.

Another problem which recurs again and again is the determination of the area of a figure representing perhaps a plot of land or the section of a beam. Here also the advantage is altogether on the side of the graphical method.

It is unnecessary to multiply these examples. But to make full use of graphical methods the draughtsman ought to have a thoroughly geometrical education. For instance, the real nature of the reciprocal diagrams already mentioned is only understood by aid of a peculiar reciprocal relation between points and planes in space closely connected with the theory of the linear complex, as has been shown by Cremona.

I have mentioned already the "Analytical Mechanics" of Lagrange, which is without any trace of geometry, although there is scarcely a branch of applied mathematics which is in its very nature more geometrical. In fact one part of it, now separated as kinematics, treats solely of changes in position and shape of geometrical quantities, and differs from pure geometry only in this, that the changes are considered as referring not to space alone, but also to time.

What mechanics gains by introducing geometry to the full will be apparent to all who have become acquainted with modern Continental text-books on the subject.

Let us compare the analytical with the geometrical reduction of a system of forces acting on a rigid body, or, to use Clifford's nomenclature, the reduction of a system of rotors, which may represent either forces or rotations, or any other quantities which have certain fundamental properties in common with those, so that they may be represented by rotors. In the analytical process the system is reduced to a rotor and a vector, that is a resultant force and a couple. In the geometrical treatment we see that this is only one way of reducing the rotors to two, viz. the one which is best fitted to be treated by analysis. But there is a multitude of other reductions. These all appear as of equal importance in the geometrical method. Furthermore, this method shows us in the simplest way possible how all the line pairs which may be the lines of action of two resultant rotors, although there are infinities of infinities of such pairs, are arranged in space, so that one gets a clear picture of all these reductions in one's mind.

Again, compare Möbius's geometrical investigation of the rays of light passing through a system of lenses with that of Gauss, whose very name suggests simplicity and elegance. The celebrated "cardinal points" appear in Gauss's original paper as the result of a somewhat long though certainly elegant analysis, whilst by Möbius they are the natural outcome of his geometry, so that any student once started on this method is bound to come across these points, or rather across pairs of points, of which the cardinal points of Gauss are only one special case. The whole is, in fact, contained in the following easily proved proposition: The rays of light starting from a point in the axis of the system before entering the first lens, and after leaving the last, form two homographic pencils in perspective position.

This is only one small part of the advantage which optics can derive from geometry.

That the old-established mode of teaching the elements of geometry based on Euclid requires a thorough and fundamental change has been often acknowledged, among others, at Exeter and Bradford, by two of the most eminent mathematicians who have occupied this chair, and besides by the many teachers who constitute the Association for the Improvement of Geometrical Teaching, which itself grew out of the action of our Section. I know, therefore, of no opportunity better suited to review the progress made in this direction than the present one, as the subject has on several occasions occupied the attention of our Section. Nevertheless I have hesitated on entering on this somewhat delicate question, because I fear that I have little to offer but criticism, which might seem hostile to the association just named. But I hope that the many earnest workers who have devoted much time and thought to the drawing up of syllabuses on different parts of our subject will excuse the remarks of one who has himself tried his hand at the same work, and who therefore may be supposed somewhat to know the difficulties that have to be overcome.

When the syllabus on the elements of plane geometry appeared, I resolved to give it a thorough trial, and took the best means in my power to form an opinion on its merits by introducing it into one of my classes. The fact that it did not quite satisfy me, and that I gave up its use again, does not of course prove that it fails also for use in schools, for which it was originally intended.

Let me add that the more I have become acquainted with the difficulty of the whole subject the greater has become my admiration for Euclid's book, whilst my conviction of its unfitness as a school book has equally gained in strength.

In considering the merits of Euclid as a text-book it is desirable to distinguish clearly between the general educational value of its teaching and the gain of geometrical knowledge. It is with the latter chiefly that I am concerned, whilst it is of course through the former that Euclid has got so firm a hold at all schools; and to the great majority of boys this is undoubtedly of most importance, and no reform would have the slightest chance of becoming generally introduced which neglects this. But improvement in both directions may well go together, and the logical reasoning employed in Euclid would gain to many boys much, both in clearness and interest, if the subject-matter reasoned about became in itself better understood.

Probably a great deal could be done by introducing some of the elements of logic into the teaching of language. I have been assured by an eminent scholar that the laws of forming a sentence—the fact that a sentence in its simplest form consists of subject, object, and copula—was not explained in English schools. If this grammatical part of logic were properly treated of in connection with language, and if at the same time acquaintance with geometrical objects, particularly through the medium of geometrical drawing and the many methods used in the Kinder-Gartens, were more secured, then a systematic course of geometry would become both easier and more useful.

Much indeed may be done by introducing simple geometrical teaching into the nursery, and into the earliest instruction of children, following the example of the Kinder-Gartens, and it is pleasing to see that the latter are rapidly gaining ground in England. It is true that these schools may still be improved. In geometry they seem to, and perhaps at present are bound to, work mostly towards Euclid. But many able men and women are actively engaged in perfecting them, and it is of interest to know that Clifford had it in his mind to write a geometry for the nursery and the Kinder-Garten.

In a curious contrast to the mode of teaching geometry stands that of teaching algebra. In the first everything is sacrificed to logic. Axioms and definitions without end are given, though to the beginner a more rapid dive into the subject would be much more suitable. In algebra, on the other hand, the boy is at once plunged into the midst of it. No axiom is mentioned. A number of rules are stated, and the schoolboy is made to practise them mechanically until he can perform, and that often with considerable skill, a number of most complicated calculations—but calculations which are often of very little use for actual applications. Simplifications of equations follow in senseless monotony, until the poor fellow really thinks that solving a simple equation does not mean the finding of a certain number which satisfies the equation, but the going mechanically through a certain regular process which at the end yields some number. The connection of that number with the original equation remains to his mind somewhat doubtful. Then there are processes, like the finding of the G.C.M., which most of the boys never have any opportunity of using, excepting, perhaps, in the examination room. A more rational treatment of the subject, introducing from the beginning reasoning rather than calculation, and applying the results obtained to various problems taken from all parts of science as well as from everyday life, would be more interestin to the student, give him really useful knowledge, and would be at the same time of true educational value.

The chief progress in geometrical teaching has to be sought in the introduction of modern ideas and methods into the very elements, and modern teaching ought to take full account of this.

In favour of this view I might bring forward the opinions of many teachers and mathematicians from England as well as from abroad, but I will confine myself to one quotation. Prof. Sylvester gives his opinion thus:—"I should rejoice to see mathematics taught with that life and animation which the presence and example of her young and buoyant sister (viz. natural and experimental science) could not fail to impart, short roads preferred to long ones, Euclid honourably shelved or buried 'deeper than did ever plummet sound' out of the schoolboy's reach, morphology introduced into the elements of algebra—projection, correlation, motion accepted as aids to geometry—the mind of the student quickened and elevated and his faith awakened by early initiation into the ruling ideas of polarity, continuity, infinity, and familiarisation with the doctrine of the imaginary and inconceivable. It is this living interest in the subject which is so wanting in our traditional and mediæval modes of teaching."

If from this point of view we now look towards the work of the Association for the Improvement of Geometrical Teaching, the result is not as satisfactory as might have been wished. There is very little of the influence of modern ideas to be found in the different syllabuses which have been published. Even in the one headed "Modern Geometry" there is nothing of the genius of modern thought. The subject-matter is partly taken from modern geometry, but for modern methods one looks in vain. In the geometrical conics, too, one would like to see Steiner's generation of conics, but of these there is no trace.

Nevertheless it is satisfactory to see that the use of the syllabus on plane geometry has spread pretty widely, and it is to be hoped that it will continue to do so. A thorough reform in the direction indicated will be a difficult task, and it will perhaps be a long time before it is possible. At present it has not even been settled which series of axioms will ultimately be adopted. Of the various systems which have been proposed since the investigations of Riemann and Helmholtz, I may mention here Clifford's suggestion to replace Euclid's axiom about parallels by the new one, which maintains that in a plane similar figures exist, or, more completely, that at any part in a plane a figure is possible which is similar to any given figure in that plane. This axiom is somewhat startling as long as we have the usual theory of similar figures in our mind. But the notion of similar figures is truly axiomatic, and it has lately become my conviction that this axiom may be extremely fruitful, and the working out of a syllabus of plane geometry based on it would be very desirable.

Possibly many such attempts have still to be made before a new Euclid finds the materials sufficiently prepared for him to raise the hoped-for edifice.

SECTION B

CHEMICAL SCIENCE

OPENING ADDRESS BY J. H. GLADSTONE, PH.D., F.R.S., V.P.C.S., PRESIDENT OF THE SECTION.

A SECTIONAL address usually consists either of a review of the work done in the particular science during the past year, or of an exposition of some branch of that science to which the speaker has given more especial attention. I propose to follow the latter of these practices, and shall ask the indulgence of my brother chemists while I endeavour to place before them some thoughts on the subject of Elements.

Though theoretical and practical chemistry are now intertwined, with manifest advantage to each, they appear to have been far apart in their origin. Practical chemistry arose from the arts of life, the knowledge empirically and laboriously acquired by the miner and metallurgist, the potter and the glass-worker, the cook and the perfumer. Theoretical chemistry derived its origin from cosmogony. In the childhood of the human race the question was eagerly put, "By what process were all things made?" and some of the answers given started the doctrine of elements. The earliest documentary evidence of the idea is probably contained in the Shoo King, the most esteemed of the Chinese classics for its antiquity. It is an historical work, and comprises a document of still more venerable age, called "The Great Plan, with its Nine Divisions," which purports to have been given by Heaven to the Great Yu, to teach him his royal duty and "the proper virtues of the various relations." Of course there are wide differences of opinion as to its date, but we can scarcely be wrong in considering it as older than Solomon's writings. The First Division of the Great Plan relates to the Five Elements. "The first is named Water; the second, Fire; the third, Wood; the fourth, Metal; the fifth, Earth. The nature of water is to soak and descend; of fire, to blaze and ascend; of wood, to be crooked and to be straight; of metal, to obey and to change; while the virtue of the earth is seen in seed-sowing and ingathering. That which soaks and descends becomes salt; that which blazes and ascends becomes bitter; that which is crooked and straight becomes sour; that which obeys and changes becomes acrid; and from seed sowing and ingathering comes sweetness."[1]

A similar idea of five elements was also common among the Indian races, and is stated by Professor Rodwell to have been in existence before the fifteenth century B.C., but, though the number is the same, the elements themselves are not identical with those of the ancient Chinese classic; thus, in the Institutes of Menu, the "subtle ether" is spoken of as being the first created, from which, by transmutation, springs air, whence, by the operation of a change, rises light or fire; from this comes water, and from water is deposited earth. These five are curiously correlated with the five senses, and it is very evident that they are not looked upon as five independent material existences, but as derived from one another. This philosophy was accepted alike by Hindoos and Buddhists. It was largely extended over Asia, and found its way into Europe. It is best known to us in the writings of the Greeks. Among these people, however, the elements were reduced to four—fire, air, earth, and water—though Aristotle endeavoured to restore the "blue ether" to its position as the most subtle and divine of them all. It is true that the fifth element, or "quinta essentia," was frequently spoken of by the early chemists, though the idea attaching to it was somewhat changed, and the four elements continued to retain their place in popular apprehension, and still retain it even among many of the scholars who take degrees at our universities. The claim of wood to be considered an element seems never to have been recognised in the West, unless, indeed, we are to seek this origin for the choice of the word ὕλη to signify that original chaotic material out of which, according to Plato and his school, all things were created.[2] The idea also of a primal element, from which the others, and everything else, were originated, was common in Greece, the difficulty being to decide which of the four had the greatest claim to this honour. Thales, as is well known, in the sixth century B.C. affirmed that water was the first principle of things; but Anaxamenes afterwards looked upon air, Heraclytus upon fire, and Theracleides on earth, as the primal element. This notion of elements, however, was essentially distinct from our own. It was always associated with the idea of the genesis of matter rather than with its ultimate analysis, and the idea of *simple* as contrasted with *compound* bodies probably never entered into the thoughts of the contending philosophers.

The modern idea appears to have had a totally different origin, and we must again travel back to China. There, also in the sixth century B.C., the great philosopher Lao-tse was meditating on the mysteries of the world and the soul, and his disciples founded the religion of Taou. They were materialists; nevertheless they believed in a "finer essence," or spirit, that rises from matter, and may become a star; thus they held that the souls of the five elements, water, metal, fire, wood, and earth, arose and became the five planets. These speculations naturally led to a search after the sublimated essences of things, and the means by which this immortality might be secured. It seems that at the time of Tsin-she-hwang, the builder of the Great Wall, about two centuries before Christ, many romantic stories were current of immortal men inhabiting islands in the Pacific Ocean. It was supposed that in these magical islands was found the "herb of immortality" growing, and that it gave them

[1] Quoted from the translation by the Rev. Dr. Legge. In that most obscure classic, the "Yi-King," fire and water, wind and thunder, the ocean and the mountains, appear to be recognised as the elements.

[2] Students of the Apocrypha will remember the expression in the Book of Wisdom, xi. 17. '*ἡ παντοδύναμός σου χεὶρ καὶ κτίσασα τὸν κόσμον ἐξ ἀμόρφου ὕλης*' ('Thy Almighty hand, that made the world of matter without form'). The same book contains two allusions to the ordinary elements, vii. 17, and xix. 18 to 20. The word *στοιχεῖον* is used in the New Testament only in a general sense (2 Pet. iii. 10), or in its more popular meaning of the first steps in knowledge.

exemption from the lot of common mortals. The emperor determined to go in search of these islands, but some untoward event always prevented him.[1]

Some two or three centuries after this a Taouist, named Weipahyang, wrote a remarkable book called "The Uniting Bond." It contains a great deal about the changes of the heavenly bodies, and the mutual relation of heaven and men; and then the author proceeds to explain some transformations of silver and water. About elixir he tells us, "What is white when first obtained becomes red after manipulation on being formed into the elixir" ("tan," meaning red or elixir). "That substance, an inch in diameter, consists of the black and the white, that is, water and metal combined. It is older than heaven and earth. It is most honourable and excellent. Around it, like a wall, are the sides of the cauldron. It is closed up and sealed on every side, and carefully watched. The thoughts must be undisturbed, and the temper calm, and the hour of its perfection anxiously waited for. The false chemist passes through various operations in vain. He who is enlightened expels his evil passions, is delighted morning and night, forgets fame and wealth, comprehends the true objects of life, and gains supernatural powers. He cannot then be scorched by fire, nor drowned in water, &c., &c. . . . The cauldron is round like the full moon, and the stove beneath is shaped like the half-moon. The lead ore is symbolised by the White Tiger; and it, like metal amongst the elements, belongs to the West. Mercury resembles the sun, and forms itself into sparkling globes; it is symbolised by the Blue Dragon belonging to the East, and it is assigned to the element wood. Gold is imperishable. Fire does not injure its lustre. Like the sun and moon, it is unaffected by time. Therefore the elixir is called 'the Golden Elixir.' Life can be lengthened by eating the herb called Hu ma; how much more by taking the elixir, which is the essence of gold, the most imperishable of all things! The influence of the elixir, when partaken of, will extend to the four limbs; the countenance will become joyful; white hair will be turned black; new teeth will grow in the place of old ones, and age at once become youth. . . . Lead ore and mercury are the bases of the process by which the elixir is prepared; they are the hinge upon which the principles of light and darkness revolve."

This description suggests the idea that the elixir of the Taouists was the red sulphide of mercury—vermilion—for the preparation of which the Chinese are still famous. That Weipahyang believed in his own philosophy is testified by a writer named Ko-hung, who, about a century afterwards, wrote the lives of celebrated Taouists. He tells how the philosopher, after preparing the elixir, took it, with his disciples, into a wood, and gave it first to his dog, then took it himself, and was followed by one of his pupils. They all three died, but, it appears, rose to life again, and to immortality. This brilliant example did not remain without imitators; indeed, two emperors of the Tang family are said to have died from partaking of the elixir. This circumstance diminished its popularity, and alchemy ceased to be practised in the Celestial Empire.

At the beginning of the seventh century the doctrine of Lao-tse was in great favour at the Chinese Court; learning was encouraged, and there was much enterprise. At the same time the disciples of Mohammed carried their arms and his doctrines over a large portion of Asia, and even to the Flowery Land. Throughout the eighth century there were frequent embassies between eastern and western Asia, wars with the Caliphs, and even a matrimonial alliance. We need not wonder, therefore, that the teachings of the Taouist alchemists penetrated westward to the Arabian philosophers. It was at this period that Yeber-Abou-Moussah-Djaferal-Sofé, commonly called Geber, a Sabæan of great knowledge, started what to the West was a new philosophy about the transmutation of metals, the Philosopher's Stone, and the Elixir of Life; and this teaching was couched in highly poetic language, mixed with astrology and accompanied by religious directions and rites. He held that all metals were composed of mercury, sulphur, and arsenic, in various proportions, and that the noblest metal could be procured only by a very lengthy purification. It was in the salts of gold and silver that he looked for the Universal Medicine. Geber himself was an experimental philosopher, and the belief in transmutation led to the acquirement of a considerable amount of chemical knowledge amongst the alchemists of Arabia and Europe. This gradually brought about a conviction that the three reputed elementary bodies, mercury, sulphur, and salt or acid, were not really the originators of all things. There was a transition period, during which the notion was itself suffering a transmutation. The idea became gradually clearer that all material bodies were made up of certain constituents, which could not be decomposed any further, and which, therefore, should be considered as elementary. The introduction of quantitative methods compelled the overthrow of mediæval chemistry, and led to the placing of the conception of simple and compound bodies upon the foundation of scientific fact. Lavoisier, perhaps, deserves the greatest credit in this matter, while the labours of the other great chemists of the eighteenth and the beginning of the nineteenth centuries were in a great measure directed to the analysis of every conceivable material, whether solid, liquid, or gaseous. These have resulted in the table of so-called elements, now nearly seventy in number, to which fresh additions are constantly being made.

[1] Nearly all the statements relating to this Taouist alchemy are derived from the writings of the Rev. Joseph Edkins, of Pekin, and the matter is treated in greater detail in an article on the "Birth of Alchemy," in the "Argonaut," vol. iii. p. 1.

Of this ever-growing list of elements not one has been resolved into simpler bodies for three-quarters of a century; and we, who are removed by two or three generations from the great builders of our science, are tempted to look upon these bodies as though they were really simple forms of matter, not only unresolved, but unresolvable. The notation we employ favours this view and stamps it upon our minds.

Is it, however, a fact that these reputed elements are really simple bodies? or, indeed, are they widely different in the nature of their constitution from those bodies which we know to be chemical compounds? Thus, to take a particular instance, are fluorine, chlorine, bromine, and iodine essentially distinct in their nature from the compound halogens, cyanogen, sulphocyanogen, ferricyanogen, &c.? Are the metals lithium, sodium, and potassium essentially distinct from such alkaline bases as ammonium, ethylamine, di-ethylamine, &c.? No philosophical chemist would probably venture to answer this question categorically with either "yes" or "no." Let us endeavour to approach it from three different points of attack—(1) the evidence of the spectroscope, (2) certain peculiarities of the atomic weights, and (3) specific refraction.

1. *The Spectroscope.*—It was at first hoped that the spectroscope might throw much light upon the nature of elements, and might reveal a common constituent in two or more of them; thus, for instance, it was conceivable that the spectrum line of bromine or iodine vapour might consist of the rays given by chlorine *plus* some others. All expectations of this have hitherto been disappointed; yet, of the other, hand, it must not be supposed that such a result disproves the compound nature of elements, for as investigation proceeds it becomes more and more clear that the spectrum of a compound is not made up of the spectra of its component parts.

Again, the multiplicity of rays given out by some elements, when heated, in a gaseous condition, such as iron, has been supposed to indicate a more complex constitution than in the case of those metals, such as magnesium, which give a more simple spectrum. Yet it is perfectly conceivable that this may be due to a complexity of arrangement of atoms all of the same kind.

Again, we have changes of a spectrum at different temperatures; new rays appear, others disappear; or even there occurs the very remarkable change from a fluted spectrum to one of sharp lines at irregular intervals, or to certain recurring groups of lines. This, in all probability, does arise from some redistribution, but it may be a redistribution in a molecular grouping of atoms of the same kind, and not a dissociation or rearrangement of dissimilar atoms.

A stronger argument has been derived from the revelations of the spectroscope in regard to the luminous atmospheres of the sun. There we can watch the effect of heat enormously transcending that of our hottest furnaces, and of movements compared with which our hurricanes and whirlwinds are the gentlest of zephyrs. Mr. Lockyer, in studying the prismatic spectra of the luminous prominences or spots of the sun, has frequently observed that on certain days certain lines, say of the iron spectrum are non-existent and on other days certain other lines disappear, and that in almost endless variety; and he has also remarked that occasionally certain lines of the iron spectrum will be crooked or displaced, thus showing the vapour to be in very rapid motion, while others are straight, and therefore comparatively at rest. Now, as a gas cannot be both at rest and in motion at the same time and the same place, it seems very clear that the two sets of lines must originate in two distinct layers of atmosphere, one above the other, and Mr. Lockyer's conclusion is that the iron molecule was dissociated by heat, and that its different constituents, on account of their different volatility, or some other cause, had floated away from one another. This seems to me the easiest explanation of the phenomenon; and, as dissociation by heat is a very common occurrence, there is no *a priori* improbability about it. But we are not shut up to it, for the different layers of atmosphere are certainly at different temperatures, and most probably of different composition. If they are of different temperatures, the variations of the spectrum may only be an extreme case of what must be acknowledged by every one more or less—that bodies emit, or cease to emit, different rays as their temperature increases, and notably when they pass from the liquid to the gaseous condition. And again, if the composition of the two layers of atmosphere be different, we have lately learnt how profoundly the admixture of a foreign substance will sometimes modify a luminous spectrum.

2. *Peculiarities of Atomic Weights.*—At the meeting of this Association at Ipswich, in 1851, M. Dumas showed that in several cases analogous elements form groups of three, the middle one of which has an atomic weight intermediate between those of the first and third, and that many of its physical and chemical properties are intermediate also. During the discussion upon his paper, and subsequently,[1] attention was drawn to the fact that this is not confined to groups of three, but that there exist many series of analogous elements having atomic weights which differ by certain increments, and that these increments are in most cases multiples of 8. Thus we have lithium, 7; sodium, 23, *i.e.* 7 + 16; potassium, 39, *i.e.* 7 + (16 × 2); and the more recently discovered rubidium, 85, *i.e.* 7 + (16 × 5) nearly; and cæsium, 133, *i.e.* 7 + (16 × 8) nearly. This is closely analogous to what we find in organic chemistry, where there are series of analogous bodies playing the part of metals, such as hydrogen, methyl, ethyl, &c., differing by an increment which has the atomic weight 14, and which we know to be CH_2. Again, there are elements with atomic weights nearly the same or nearly multiples of one another, instances of which are to be found in the great platinum group and the great cerium group.[2] This suggests the analogy of isomeric and polymeric bodies. There is also this remarkable circumstance: the various members of such a group as either of those just mentioned are found together at certain spots on the surface of the globe, and scarcely anywhere else. The chemist may be reminded of how in the dry distillation of some organic body he has obtained a mixture of polymerised hydrocarbons, and may perhaps be excused if he speculates whether in the process of formation of the platinum or the cerium group, however and whenever it took place, the different elements had been made from one another and imperfectly polymerised.

But this is not the largest generalisation in regard to the peculiarities of these atomic weights. Newlands showed that, by arranging the numbers in their order, the octaves presented remarkable similarities, and, on the same principle, Mendeléeff constructed his well-known table. I may remind you that in this table the atomic weights are arranged in horizontal and vertical series, those in the vertical series differing from one another, as a rule, by the before-mentioned multiples of 8—namely 16, 16, 24, 24, 24, 24, 32, 32—the elements being generally analogous in their atomicity and in other chemical characters. Attached to the elements are figures, representing various physical properties, and these in the horizontal series appear as periodic functions of the atomic weights. The table is incomplete, especially in its lower portions, but, with all its imperfections and irregularities, there can be no doubt that it expresses a great truth of nature. Now, if we were to interpolate the compound bodies which act like elements—methyl, 15; ammonium, 18; cyanogen, 26—into Mendeléeff's table, they would be utterly out of place, and would upset the order both of chemical analogy and of the periodicity of the physical properties.

3. *Specific Refraction.*—The specific refraction has been determined for a large majority of the elements, and is a very fundamental property, which belongs to them apparently in all their combinations, so long at least as the atomicity[3] is unchanged. If the figures representing this property be inserted into Mendeléeff's table, we find that in the vertical columns the figures almost invariably decrease as the atomic weights increase. If, however, we look along the horizontal columns, or better still if we plot the figures in the table by which Lothair Meyer has shown graphically that the molecular volume is a periodic function of the atomic weights, we shall see that they arrange themselves in a series of curves similar to but not at all coincident with his. The observations are not so complete or accurate as those of the molecular volumes, but they seem sufficient to establish the fact, while the points of the curves would appear to be, not the alkaline metals, as in Meyer's diagram, but hydrogen, phosphorus and sulphur, titanium and vanadium, selenium, antimony. Now, if we were to insert the specific refractions of cyanogen, ammonium, and methyl into this table, we should again show that it was an intrusion of strangers not in harmony with the family of elements.

But there is another argument to be derived from the action of light. The refraction equivalent of a compound body is the sum of the refraction equivalents of its compounds; and, if there is anything known for certain in the whole subject, it is that the refraction equivalent of an organic compound advances by the same quantity (7·6) for every increment of CH_2. If, therefore, the increment between the different members of a group of analogous elements, such as the alkaline metals, be of the same character, we may expect to find that there is a regular increase of the refraction equivalent for each addition of 16. But this is utterly at variance with fact: thus, in the instance above quoted, the refraction equivalent of lithium being 3·8, that of sodium is 4·8, of potassium 8·1, of rubidium 14·0, and of cæsium about 13·7. Neither does the law obtain in those series in which the increment is not a multiple of 8, as in the case of the halogens, where the increment of atomic weight is 45, and the refraction equivalents are chlorine 9·9, bromine 15·3, and iodine 24·5.

The refraction equivalents of isomeric bodies are generally identical, and the refraction equivalents of polymeric bodies are in proportion to their atomic weights. Among the groups of analogous elements of the same, or nearly the same, atomic weight we do find certain analogies: thus cobalt and nickel are respectively 10·8 and 10·4, while iron and manganese are respectively 12·0 and 12·2. But, as far as observation has gone at present, we have reason to conclude that, if metals stand to one another in the ratio of 2 : 1 in atomic weight, their refraction equivalents are much nearer together than that; while, on the other hand, the equivalent of sulphur, instead of being the double of that of oxygen, is at least five times as great.

The general tendency of these arguments is evidently to show that the elementary radicals are essentially different from the compound radicals, though their chemical functions are similar.

There remains still the hypothesis that there is a "primordial element," from which the others are derived by transmutation. With the sages of Asia it was the "blue ether," with Thales water, with Dr. Prout hydrogen. The earlier views have passed away, and the claims of hydrogen are being fought out on the battle-field of atomic weights and their rigorous determination.

There does not appear to be any argument which is fatal to the idea that two or more of our supposed elements may differ from one another rather in form than in substance, or even that the whole seventy are only modifications of a prime element; but chemical analogies seem wanting. The closest analogy would be if we could prepare two allotropic conditions of some body, such as phosphorus or cyanogen, which should carry their allotropism into all their respective compounds, no compound of the one form being capable of change into a compound of the other. Our present knowledge of allotropism, and of variations in atomicity, affords little, if any, promise of this.

The remarkable relations between the atomic weights of the elements, and many peculiarities of their grouping, force upon us the conviction that they are not separate bodies created without reference to one another, but that they have been fashioned or built up from one another, according to some general plan. This plan we may hope gradually to understand better, but if we are ever to transform one of these supposed elements into another, or to split up one of them into two or three dissimilar forms of matter, it will probably be by the application of some method of analysis hitherto unknown.

Nothing can be of greater promise than the discovery of new methods of research; hence I need make no apology to others who have lately done excellent work in chemistry if I single out the Bakerian Lecture of this year, by Mr. Crookes, on "Radiant Matter Spectroscopy." It relates to the prismatic analysis, not of the light transmitted or absorbed in the ordinary way by a solid or liquid, nor of that given out by incandescent gas, but the

[1] "Phil. Mag.," May, 1853.

[2] Another curious instance is the occurrence of nickel and cobalt in all meteoric irons, with occasionally chromium or manganese, the atomic weights and other properties of which are very similar.

[3] This exception includes not merely such changes as that from a ferrous to a ferric salt, but the different ways in which the carbon is combined in such bodies as ethene, benzene, and pyrene.

analysis of the fluorescence that manifests itself in certain bodies when they are exposed to an electric discharge in a highly exhausted vacuum. He describes, in an interesting and even amusing manner, his three years' quest after the origin of a certain citron band, which he observed in the spectrum of the fluorescence of many substances, till he was led into that wonderful labyrinth of uncertain elements which are found together in samarskite, and eventually he proved the appearance to be due to yttrium. As the test is an extremely delicate one, he has obtained evidence of the very general dissemination of that element, in very minute quantities—and not always very minute—for the polypes that built up a certain pink coral were evidently able to separate the earth from the sea water, as their calcareous secretion contained about ½ per cent. of yttrium. We have reason to hope that this is only the first instalment of discoveries to be made by this new method of research.

I cannot conclude without a reference to the brightening prospects of technical chemistry in this country. I do not allude to the progress of any particular industry, but to the increased facilities for the education of those engaged in the chemical manufactures. First as to the workpeople. Hitherto the young artisan has had little opportunity of learning at school what would be of the greatest service to him in his after career. The traditions of the Middle Ages were all in favour of literary culture for the upper classes, and the education suited for these has been retained in our schools for the sons of the people. It is true that some knowledge of common things has been given in the best schools, and the Education Department has lately encouraged the teaching of certain sciences in the upper standards. In the Mundella Code, however, which came into operation last year, "elementary science" may receive a grant in all the classes of a boys' or girls' school, and in the suggested scheme there is mentioned simple lessons on "the chemical and physical principles involved in one of the chief industries of England, among which Agriculture may be reckoned," while "Chemistry" is inserted among "the specific subjects of instruction" that may be given to the older children. It is impossible, as yet, to form an estimate of the extent to which managers and teachers have availed themselves of this permission, for the examinations of Her Majesty's inspectors under the new code have only just commenced; but one of the best of the Board Schools in London has just passed satisfactorily in chemistry, both with boys and girls. I trust that in those parts of the country where chemical industries prevail, chemistry may be largely taken up in our elementary schools.

The great deficiency in our present educational arrangements is the want of the means of teaching a lad who has just left the common school the principles of that industry by which he is to earn his livelihood. The more purely scientific chemistry, however, may be learnt by him now in those evening classes which may be formed under the Education Department, as well as in those that have long been established under the Science and Art Department. The large amount of attention that is now being given to the subject of technical education is creating in our manufacturing centres many technical classes and colleges for students of older growth.

As to inventors and the owners of our chemical factories, in addition to the Chemical Society and the Chemical Institute, there has recently been founded the Society of Chemical Industry. It came into existence with much promise of success; at the close of its second year it numbered 1400 members; it has now powerful sections in London, Manchester, Liverpool, Newcastle, and Birmingham; and it diffuses information on technical subjects in a well-conducted monthly journal.

May the abstract science and its useful applications ever prove helpful to one another, and become more and more one chemistry for the benefit of mankind.

SECTION C

GEOLOGY

Opening Address by Professor W. C. Williamson, LL.D., F.R.S., President of the Section.

Much of the second decade of my life was spent in the practical pursuit of geology in the field, and throughout most of that period I enjoyed almost daily intercourse with William Smith, the father of English Geology; but in later years circumstances restricted my studies to the Palæontological side of the science. Hence I was anxious that the council of the British Association should place in this chair some one more familiar than myself with the later developments of geographical geology. But my friend, Professor Bonney, failing to recognise the force of my objections, intimated to me that I might render some service to the Association by placing before you a sketch of the present state of our knowledge of the vegetation of the Carboniferous Age.

This being a subject respecting which I have formed some definite opinions, I am going to act upon the suggestion. To some this may savour of "shop-talk." But such is often the only talk which a man can indulge in intelligently, and to close his mouth on his special themes may compel him either to talk nonsense or to be silent.

Whilst undertaking this task I am alive to the difficulties which surround it, especially those arising from the wide differences of opinion amongst palæobotanists on some fundamental points. On some of the most important of these there is a substantial agreement between the English and German palæontologists. The dissentients are chiefly, though not entirely, to be found amongst those of France, who have, in my humble opinion, been unduly influenced by what is in itself a noble motive—viz. a strong reverence for the views of their illustrious teacher, the late Adolphe Brongniart. Such a tendency speaks well for their hearts, though it may, in these days of rapid scientific progress seriously mislead their heads. I shall, however, endeavour to put before you faithfully the views entertained by my distinguished French friends M. Renault, M. Grand-Eury, and the Marquis of Saporta, giving, at the same time, what I deem to be good reasons for not agreeing with them. I believe that many of our disagreements arise from geological differences between the French Carboniferous strata and those in our own islands. There are some important types of Carboniferous plants that appear to be much better represented amongst us than in France. Hence we have, I believe, more abundant material than the French palæontologists possess for arriving at sound conclusions respecting these plants. We have rich sources supplying specimens in which the internal organisation is preserved, in Eastern Lancashire and Western Yorkshire, Arran, Burntisland, and other scattered localities. France has equally rich localities at Autun and at St. Étienne. But some important difference exists between these localities. The French objects are preserved in an impracticable siliceous matrix, extremely troublesome to work, except in specimens of small size. Ours, on the other hand, are chiefly embedded in a calcareous material which, whilst it preserves the objects in an exquisite manner, does not prevent our dissecting examples of considerable magnitude. But, besides this, we are much richer in huge Lepidodendroid and Sigillarian trees, with their Stigmarian roots, than the French are; hence we have a vast mass of material illustrating the history of these types of vegetation, in which they seem to be seriously deficient. This fact alone appears to me sufficient to account for many of the wide differences of opinion that exist between us respecting these trees. My second difficulty springs out of the imperfect state of our knowledge of the subject. One prominent cause of this imperfection lies in the state in which our specimens are found. They are not only too frequently fragmentary, but most of those fragments only present the external forms of the objects. Now, mere external forms of fossil plants are somewhat like similarities of sound in the comparative study of languages. They are too often unsafe guides. On the other hand, microscopic internal organisations in the former subjects are like grammatical identities in the latter one. They indicate deep affinities that promise to guide the student safely to philosophical conclusions. But the common state in which our fossil plants are preserved presents a source of error that is positive as well as negative. Most of those from our coal-measures consist of inorganic shale, sandstone, or ironstone, invested by a very thin layer of structureless coal. The surface of the inorganic substance is moulded into some special form dependent upon structural peculiarities of the living plants, which structures were sometimes external, sometimes internal, and sometimes intermediate ones. Upon this inorganic cast we find the thin film of structureless coal, which, though of organic origin, is practically as inorganic as the clay or sandstone which it invests; but its surface displays specific sculpturings which are apt to be regarded as always representing the outermost surface of the plant when living, whereas this is not always the case. That the coaly film is a relic of the carbonaceous substance of the living plant is unquestionable; but the thinnest of these films are often the sole remaining representatives of structures that must originally been have many inches, and in some instances even many feet, in thickness. In such cases most of the organic material has been dissipitated, and what little remains has often been consolidated in such a way that it is merely moulded upon the sculptured inorganic substance which it covers, and hence affords no information respecting the exterior of the fossil when a living organism. It is, in my opinion, from specimens like these that the smooth bark of the Calamite has been credited with a fluted surface, and the Trigonocarpons with a merely triangular exterior and a misleading name, as it long caused the inorganic casts known as Sternbergiæ to be deemed a strange form of plant that had no representative amongst living types. In other cases the outermost surface of the bark is brought into close contact with the surface of the vascular cylinder. I have a Stigmaria in which the bases of the rootlets appear to be planted directly upon that cylinder, the whole of the thick intermediate bark having disappeared. In other examples that vascular zone has also gone. Thus the innermost and outermost surfaces of a cylinder, originally many inches apart, are, through the disappearance of the intermediate structures, brought into close approximation. In such cases, leaves and other external appendages appear to spring directly from what is merely an inorganic cast of the interior of the pith. I believe that many of our Calamites are in this condition. Such examples have suggested the erroneous idea that the characteristic longitudinal flutings belong to the exterior of the bark.

Fungi.—Entering upon a more detailed review of our knowledge of the Carboniferous plants, and commencing at the bottom of the scale, we come to the lowly group of the Fungi, which are unquestionably represented by the *Peronosporites antiquarius* [1] of Worthington Smith. There seems little reason for doubting that this is one of the Phycomycetous Fungi, possibly somewhat allied to the *Saprolegnieæ*; but since we have as yet no evidence respecting its fructification, these closer relationships must, for the present, remain undetermined. So far as I know, this is the only Fungus satisfactorily proved to exist in the Carboniferous rocks, unless the *Excipulites Neesii* of Goeppert and one or two allied forms belong to the Fungoid group. The *Polyporites Bowmanni* is unquestionably a scale of a Holoptychian fish.

Algæ.—Numerous objects supposed to belong to this family have been discovered in much older rocks than Carboniferous ones. The subject is a thorny one. That marine plants of some kind must have existed simultaneously with the molluscous and other plant-eating animals of Palæozoic times is obviously indisputable. But what those plants were is another question. The widest differences of opinion exist in reference to many of them. A considerable number of those recognised by Schimper, Saporta, and other palæobotanists, are declared by Nathorst to be merely inorganic tracks of marine animals—and in the case of many of these I have little doubt that the Swedish geologist is right. Others have been shown to be imperfectly preserved fragments of plants of much higher organisation than Algæ, branches of Conifers even being included amongst them. I have as yet seen none of Carboniferous age that could be indisputably identified with the family of Algæ, though there are many that look like, and may probably be, such. The microscope alone can settle this question, though even this instrument fails to secure unity of opinion in the case of Dawson's *Prototaxites*, and no other of the supposed seaweeds hitherto discovered have been sufficiently well preserved to bear the microscopic test; hence I think that their existence in Carboniferous rocks can only be regarded as an unproven probability. Mere superficial resemblances do not satisfy the severe demands of modern science, and probabilities are an insufficient foundation upon which to build evolutionary theories.

Seeing what extremely delicate cell-structures *are* preserved in the Carboniferous beds, it cannot appear other than strange that the few imperfect Fungoid relics just referred to constitute the only terrestrial cellular Cryptogams that have been discovered in the Carboniferous strata. The Darwinian doctrine would suggest that these lower forms of plant life ought to have abounded in that primæval age; and that they were capable of being preserved is proved by the numerous specimens met with in Tertiary deposits. Why we do not find such in the Palæozoic beds is still an unsolved problem.

Vascular Cryptogams.—The Vascular Cryptogams, next to be considered, burst upon us almost suddenly and in rich profusion during the Devonian age; they are equally silent in the Devonian and Carboniferous strata as to their ancestral descent.

Ferns.—The older taxonomic literature of Palæozoic Fern-life is, with few exceptions, of little scientific value. Hooker and others have uttered in vain wise protests against the system that has been pursued. Small fragments have had generic and specific names assigned to them, with supreme indifference to the study of morphological variability amongst living types. The undifferentiated tip of a terminal pinnule has had its special name, whilst the more developed structures forming the lower part of a frond have supplied two or three more species. Then the distinct forms of the fertile fronds may have furnished additional ones, whilst a further cause of confusion is seen in the wide difference existing between a young half-developed seedling and the same plant at an advanced stage of its growth. Any one who has watched the development of a young *Polypodium aureum* can appreciate this difference. Yet, in the early stages of palæontological research, observers could scarcely have acted otherwise than as they did in assigning names to these fragments—if only for temporary working purposes. Our error lies in misunderstanding the true value of such names. At present the study of fossil ferns is affording some promise of a newer and healthier condition. We are slowly learning a little about the fructification of some species, and the internal organisation of others. Facts of these kinds, cautiously interpreted, are surer guides than mere external contours; unfortunately, such facts are, as yet, but few in number, and when we have them we are too often unable to identify our detached sporangia, stems, and petioles with the fronds of the plants to which they primarily belonged.

That all the Carboniferous plants included in the genera *Pecopteris*, *Neuropteris*, and *Sphenopteris* are ferns appears to be most probable; but what the true affinities of the objects included in these ill-defined genera may be is very doubtful. Here and there we obtain glimpses of a more definite kind. That the Devonian *Palæapteris Hibernica* is a Hymenophyllous form appears to be almost certain; and on corresponding grounds we may conclude that the Carboniferous forms *Sphenopteris trichomanoides*, *S. Humboltii*,[1] and *Hymenophyllum Weissii*,[2] belong to the same group. The fructification of the two latter leaves little room for doubting their position, whilst the foliage of some other species of Sphenopteris is suggestive of similar conclusions, but until their fructification is discovered this cannot be determined. An elegant form of Sphenopteris (*S. tenella*, Brong., *S. lanceolata* of Gutbier), recently described by Mr. Kidson of Stirling, abundantly justifies caution in dealing with these Sphenopterides. This plant possesses a true Sphenopteroid foliage, but its fructification is that of a Marattiaceous *Danaid*. The sporangia are elongated vertically, and have the round terminal aperture of both the recent and fossil *Danaiæ*—a group of plants far removed from the Hymenophyllaceous type of Sphenopterid already referred to.

Whether or not this *Sphenopteris* was really Marattiaceous in other features than its fructification is uncertain; but I think that we have indisputably got stems and petioles of Marattiaceæ from the Carboniferous strata. My friend M. Renault and I, without being aware of the fact, simultaneously studied the *Medullosa elegans* of Colta. This plant was long regarded as the stem of a true Monocotyledon, a decision the accuracy of which was doubted first by Brongniart and afterwards by Binney. M. Renault's memoir and my part vii. appeared almost simultaneously. We then found that we had alike determined the supposed Monocotyledon to be not only a fern, but to belong to the peculiarly aberrant group of the *Marattiaceæ*. As yet we know nothing of its foliage and fructification.

M. Grand-Eury has figured [3] a remarkable series of ferns from the coal-measures of the basin of the Loire, the sporangia of which exhibit marked resemblances to those of the Marattiaceæ. This is especially the case with his specimens of *Asterotheca* and *Scolecopteris*,[4] as also with his *Pecopteris Marattiætheca*, *P. Angiotheca*, and *P. Danaeætheca*, but there is some doubt as to the dehiscence of the sporangia of these plants; hence their Marattiaceous character is not absolutely established.

That the coal-measures contain the remains of arborescent ferns has long been known, especially from their abundance at Autun. In Lancashire I have only met with the stems or petioles of one species preserving their internal organisation.[5] The Rev. H. H. Higgins obtained stems that appear to have been tree-ferns from Ravenhead, in Lancashire, and it is probable that

[1] "Memoir" xi. p. 299.

[1] "Schimper," vol. i. p. 408. [2] *Ibid.* p. 415.

[3] "Flore Carbonifère du Départment de la Loire et du Centre de la France."

[4] *Loc. cit.* Tab. viii. Figs. 1-5.

[5] *Psaronius Renaultii*, Memoir vii. p. 10, and Memoir xii. Pl. iv. Figs. 16. These and other similar references are to my series of Memoirs "On the Organisation of the Fossil Plants of the Coal-measures," published in the "Philosophical Transactions"

most of the plants included in the genera *Psaronius*, *Caulopteris*, and *Protopteris*, are also tree-ferns.

There yet remains another remarkable group of ferns, the sporangia of which are known to us through the researches of M. Renault. In these the fertile pinnules are more or less completely transmuted into small clusters of oblong sporangia. In one case M. Renault believes that he has identified these organs with a stem or petiole of a type not uncommon at Oldham and Halifax, belonging to Corda's genus *Zygopteris*. Renault has combined this with some others to constitute his group of *Botryoptéridées*, an altogether extinct and generalised type. This review shows that whilst forms identifiable with the *Hymenophyllaceæ* and *Marattiaceæ* existed in the Carboniferous epoch, and we find here and there traces of affinities with some other more recent types, most of the Carboniferous ferns are generalised primæval forms which only become differentiated into later ones in the slow progress of time.

Equisetaceæ and *Asterophylliteæ*, Brong. *Calamariæ*, Endlicher. *Equisetineæ*, Schimper.

Confusion culminates in the history of this variously-named group. Hence the subject is a most difficult one to treat in a concise way. The confusion began when Brongniart separated the plants contained in the group into two divisions—one of which (*Équisétacés*) he identified with the living Equisetums, and the other (*Astérophyllitées*) he regarded as being Gymnospermous Dicotyledons. To Schimper belongs the merit, as I believe it to be, of steadily resisting this division; nevertheless, palæobotanists are still separated into two schools on the subject; Dawson, Renault, Grand-Eury, and Saporta adhere to the Brongniartian idea, whilst the British and German palæontologists have always adopted the opposite view, rejecting the idea that any of these plants were other than Cryptogams.

A fundamental feature of the entire group is in the fact that their foliar appendages, however morphologically and physiologically modified, are arranged in nodal verticils. This appears to be the only characteristic which the plants possess in common.

Calamites and *Calamodendron*. In his "Prodrome" (1828), and in his later "Végétaux Fossiles," Brongniart adopted the former of these generic names as previously employed by Suckow, Schlotheim, Sternberg, and Artis. It was only in his "Tableau des Genres de Végétaux Fossiles" ("Dictionnaire universel d'Histoire Naturelle," 1849) that he divided the genus, introducing the second name to represent what he believed to be the Gymnospermous division of the group. A long series of investigations, extending over many years, has convinced me that no such Gymnospermous type exists.[1] The same conclusion has more recently been arrived at by Vom c. M. D. Stur,[2] after studying many continental examples in which structure is preserved. What I regard as an error appears to have had an intelligible origin—the fertile source of similar errors in other groups.

Nearly all the Calamitean fossils found in shales and sandstones consist of an inorganic, superficially fluted substance, coated over with a thin film of structureless coal (see "Histoire des Végétaux Fossiles," Vol. i., Pl. 22), *the latter being exactly moulded upon and following the outlines of the inorganic fluted cast that underlies it.* Brongniart and those who adopt his views believe that the external surface of this coal-film exactly represents the corresponding external surface of the original plant. Hence the conclusion was arrived at that the plant had a very large central fistular cavity surrounded by a very thin layer of cellular and vascular tissues as in some living Equisetums. On the other hand, Brongniart also obtained some specimens of what he primarily believed to be Calamites, in which the central pith was surrounded by a thick layer of woody tissue arranged in radiating laminated wedges, separated by medullary rays. The exogenous structure of this woody zone was too obvious to escape his practised eye. But, not supposing it possible that any Cryptogam could possess a cambium-layer and an exogenous mode of development, Brongniart came to the conclusion that the thin-walled specimens found in the shales and sandstones were true *Equisetaceæ*, those with the thick woody cylinders being exogens of another type. His conclusion that they were Gymnosperms was a purely hypothetical one, justified by no one feature of their organisation.

My researches, based upon a vast number of specimens of all sizes, from minute twigs little more than the thirtieth of an inch in diameter, to thick stems at least thirteen inches across, led me to the conclusion that we have but one type of Calamite; and that the differences which misled Brongniart are merely due to variations in the mode of their preservation.[1] It became clear to me that the outer surface of the coaly film in the specimens preserved in the shales and sandstones did *not* represent the outer surface of the living plant, but was only a fractional remnant of the carbon of that plant which had undergone a complete metamorphosis; the greater part of what originally existed had disappeared, probably in a gaseous state, and the little that remained, displaying no organic structure, *had been moulded upon the underlying inorganic cast of the medullary cavity.* This cast is always fluted longitudinally and constructed transversely at intervals of varying lengths. Both these features were due to impressions made by the organism upon the inorganic sand or mud filling the medullary cavity whilst it was in a plastic state, and which subsequently became more or less hardened; the longitudinal grooves being caused by the pressure of the inner angles of the numerous longitudinally vascular wedges, and the transverse ones partly by the remains of a cellular nodal diaphragm, which crossed the fistular medullary cavity, and partly by a centripetal encroachment of the vascular zone at each of the same points.[2]

My cabinets contain an enormous number of sections of these plants in which the minutest details of their organisation are exquisitely preserved. These specimens, as already observed, show their structure in every stage of their growth, from the smallest twigs to stems more than a foot in diameter. Yet these various examples are all, without a solitary exception, constructed upon one common plan. That plan is an extremely complicated one; far too complex to make it in the slightest degree probable that it could coexist in two such very different orders of plants as the *Equisetaceæ* and the *Gymnospermæ;* yet, though very complex, it is, even in many of its minuter details, unmistakably the plan upon which the living Equisetums are constructed. The resemblances are too clear as well as too remarkable, in my mind, to leave room for any doubt on this point. The great differences are only such as necessarily resulted from the gradual attainment of the arborescent form so unlike the lowly herbaceous one of their living representatives. On the other hand, no living Gymnosperm possesses an organisation that *in any solitary feature* resembles that of the so called Calamodendra. The two have absolutely nothing in common; hence the conclusion that these Calamodendra were Gymnospermous plants is as arbitrary an assumption as could possibly be forced upon science; an assumption that no arguments derived from the merely external aspects of structureless specimens could ever induce me to accept.

These Calamites exhibit a remarkable morphological characteristic which presents itself to us here for the first time, but which we shall find recurs in other Palæozoic forms. Some of our French botanical friends group the various structures contained in plants into several "*Appareils*,"[3] distinguished by the functions which those structures have to perform. Amongst others we find the "*Appareil de soutiens*," embracing those hard woody tissues which may be regarded as the supporting skeleton of the plant, and the "*Appareil conducteur*," which M. van Tieghem describes as composed of two tissues: "Le tissu criblé qui transporte essentiellement les matières insolubles, et le tissu vasculaire qui conduit l'eau et les substances dissoutes." Without discussing the scientific limits of this definition, it suffices for my present purpose. In nearly all flowering plants these two "*Appareils*" are more or less blended. The supporting wood cells are intermingled in varying degrees with the sap-conducting vessels. It is so even in the lower Gymnosperms, and in the higher ones these wood cells almost entirely replace the vessels. It is altogether otherwise with the fossil Cryptogams. The vascular cylinder in the interior of the Calamites, for example, consists wholly of *barred* vessels, a slight modification of the scalariform type so common in all Cryptogams. No trace of the "*Appareil de soutiens*" is to be found amongst them. The vessels are, in the most definite sense, the "*Appareils conducteurs*" of these plants; no such absolutely undifferentiated unity of tissue is to be found in any living plants other than Cryptogams.

But these Calamites, when living, towered high into the air. My friend and colleague, Professor Boyd Dawkins, recently assisted me in measuring one found in the roof of the Moorside colliery near Ashton-under-Lyne by Mr. George Wild, the very intelligent manager of that and some neighbouring collieries.

[1] "Memoirs" i. ix. and xii.
[2] "Zur Morphologie der Calamarien."

[1] "Memoirs" i. and ix.
[2] See "Memoir" i. Pl. xxiv. Fig. 10, and Pl. xxvi. Fig. 24.
[3] Van Tieghem, "Traité de Botanique," p. 679.

The flattened specimen ran obliquely along the roof, each of its two extremities passing out of sight, burying themselves in the opposite sides of the mine. Yet the portion which we measured was 30 feet long, its diameter being 6 inches at one end, and 4½ inches at the other. The mean length of its internodes at its broader end was 3 inches, and at its narrower one 1½ inches. What the real thickness of this specimen was when all its tissues were present we have no means of judging, but the true diameter of the cylinder represented by the fossil when uncompressed has been only 4 inches at one end of the 30 feet, and 2½ inches at the other. Whatever its entire diameter when living, the vascular cylinder of this stem must have been at once tall and slender, and consequently must have required some "*Appareil de soutien*," such as its exogenous vascular zone did not supply. This was provided in a very early stage of growth by the introduction of a second cambium-layer into the bark; which, though reminding us of the cork-cambium in ordinary exogenous stems, produced not cork but prosenchymatous cells.[1] In its youngest state the bark of the Calamites was a very loose cellular parenchyma, but in the older stems much of this parenchyma became inclosed in the prosenchymatous tissue referred to, and which appears to have constituted the greater portion of the matured bark. The sustaining skeleton of the plant, therefore, was a hollow cylinder developed centrifugally on the inner side of an inclosing cambium-zone. That this cambium-zone must have had some protective periderm external to it is obvious; but I have not yet discovered what it was like. We shall find a similar cortical provision for supporting lofty cryptogamous stems in the *Lepidodendra* and *Sigillariæ*.

The Carboniferous rocks have furnished a large number of plants having their foliage arranged in verticils, and which have had a variety of generic names assigned to them; such are *Asterophyllites*, *Sphenophyllum*, *Annularia*, *Bechera*, *Hippurites*, and *Schizoneura*. Of these genera, *Sphenophyllum* is distinguished by the small number of its wedge-shaped leaves, and the structure of its stems has been described by M. Renault. *Annularia* is a peculiar form in which the leaves forming each verticil, instead of being all planted at the same angle upon the central stem, are flattened obliquely nearly in the plane of the stem itself. *Asterophyllites* differs from *Sphenophyllum*, chiefly in the larger number and in the linear form of its leaves. Some stems of this type have virtually the same structure[2] as those of *Sphenophyllum*, a structure which differs widely from that of the Calamites, and of which, consequently, these plants cannot constitute the leaf-bearing branches. But there is little doubt that true Calamitean branches have been included in the genus *Asterophyllites*; I have specimens, for which I am indebted to Dr. Dawson, which I should unhesitatingly have designated *Asterophyllites* but for my friend's positive statement that he detached them from stems of a Calamite. Of the internal organisation of the stems of the other genera named we know nothing.

It is a remarkable fact that, notwithstanding the number of young Calamitean shoots that we have obtained from Oldham and Halifax in which the structure is preserved, we have not met with one with the leaves attached. This is apparently due to the fact that most of the specimens are decorticated ones. We have a sufficient number of corticated specimens to show us what the bark was, but such specimens are not common. They clearly prove, however, that their bark had a smooth, and not a furrowed, external surface.

There yet remains for consideration the numerous reproductive strobili, generally regarded as belonging to plants of this class, *Equisetinæ*. We find some of these strobili associated with stems and foliage of known types, as in *Sphenophyllum*,[3] but we know nothing of the internal organisation of these Sphenophylloid strobili. We have strobili connected with stems and foliage of *Annularia*,[4] but we are equally ignorant of the organisation of these; so far as that organisation can be ascertained from Sterzel's specimen, it seems to have alternating sterile and fertile bracts with the sporangia of the latter arranged in fours, as in *Calamostachys*.[5] On the other hand, we are now very familiar with the structure of the *Calamostachys Binneana*, the prevalent strobilus in the calcareous nodules found in the lower coal-measures of Lancashire and Yorkshire. It has evidently been a sessile spike, the axial structures of which were trimerous[1] (rarely tetramerous), having a cellular medulla in its centre. Its appendages were exact multiples of those numbers. Of the plant to which it belonged, we know nothing. On the other hand, we have examples, supposed to be of the same genus, as *C. paniculata*,[2] and *C. polystachya*,[3] united to stems with Asterophyllitean leaves, but whether or not these fruits have the organisation of *C. Binneana*, we are unable to say.

We are also acquainted with the structure of the two fruits belonging to the genera *Bruckmannia*[4] and *Volkmannia*.[5] This latter term has long been very vaguely applied.

There still remain the genera *Stachannularia*, *Palæostachya*, *Macrostachya*, *Cingularia*, *Huttonia*, and *Calamitina*, all of which have the phyllomes of their strobili, fertile and sterile, arranged in verticils, and some of them display Asterophyllitean foliage. But these plants are only known from structureless impressions. That all these curious spore-bearing organisms have close affinities with the large group of the Equisetums cannot be regarded as certain, but several of them undoubtedly have peculiarities of structure suggestive of relations with the Calamites. This is especially observable in the longitudinal canals found in the central axis of each type, apparently identical with what I have designated the internodal canals of the Calamites.[6] The position and structure of their vascular bundles suggest the same relationship, whilst in many the position of the sporangia and sporangiophores is eminently Equisetiform. Renault's *Bruckmannia Grand-Euryi*, and *B. Decaisnei*, and a strobilus which I described in 1870,[7] exhibit these Calamitean affinities very distinctly.

One strobilus which I described in 1880[8] must not be overlooked. As is well known, all the living forms of Equisetaceous plants are isosporous. We only discover heterosporous vascular cryptogams amongst the *Lycopodiaceæ*, and the *Rhizocarpæ*. My strobilus is identical in every detailed feature of its organisation with the common *Calamostachys Binneana*, excepting that it is heterosporous, having microspores in its upper and macrospores in its lower part; a state of things suggestive of some link between the *Equisetinæ* and the heterosporous *Lycopodiaceæ*.

Lycopodiaceæ.—This branch of my subject suggests memories of a long conflict which, though it is virtually over, still leaves, here and there, the ground-swell of a stormy past. At the meeting of the British Association at Liverpool in 1870, I first announced that a thick, secondary, exogenous growth of vascular tissue existed in the stems of many Carboniferous cryptogamic plants, especially in the Calamitean and Lepidodendroid forms. But, at that time, the ideas of M. Brongniart were so entirely in the ascendant, that my notions were rejected by every botanist present. Though the illustrious French palæontologist knew that such growths existed in *Sigillariæ* and in what he designated *Calamodendra*, he concluded that, *de facto*, such plants could not be Cryptogams. Time, however, works wonders. Evidence has gradually accumulated proving that—with the conspicuous exception of the ferns—nearly every Carboniferous Cryptogam was capable of developing such zones of secondary growth. The exceptional position of the ferns still appears to be as true as it was when I first proclaimed their exceptional character at Liverpool. At that time I was under the impression that the secondary wood was only developed in such plants as attained to arboreal dimensions, but I soon afterwards discovered that it occurred equally in many small plants like *Sphenophyllum*, *Asterophyllites* and other diminutive types.

After thirteen years of persevering demonstration, these views, at first so strongly opposed, have found almost universal acceptance. Nevertheless, there still remain some few who believe them to be erroneous ones. In the later stages of this discussion the botanical relations subsisting between *Lepidodendron*, *Sigillaria*, and *Stigmaria* have been the chief themes of debate. In this country we regard the conclusion that *Stigmaria* is not only a root, but the root alike of *Lepidodendron* and *Sigillaria*, as settled beyond all dispute. Nevertheless M. Renault and M. Grand-Eury believe that it is frequently a leaf-bearing rhizome,

[1] "Memoir" ix. Pl. xx. Figs. 14, 15, 18, 19, and 20.
[2] "Memoir," Part v. Plates i.—v., and Part ix. Pl. xxi. Fig. 32.
[3] Lesquereux, "Coal Flora of Pennsylvania," Pl. ii. Fig. 687.
[4] "Ueber die Fruchtähren von Annularia Sphenophylloides." Von T. Sterzel, "Zeitschr. d. Deutschen Geolog. Gesellschaft," Jahrg. 1882.
[5] M. Renault has described a strobilus under the name of *Annularia longifolia*, but which appears to me very distinct from that genus.

[1] It is an interesting fact that transverse sections of the young strobili of *Lycopodium Alpinum* exhibit a similar trimerous arrangement, though differing widely in the positions of its sporangia.
[2] Weiss, "Abhandlungen zur Geologischen Specialkarte von Preussen und Thüringischen Staaten," Taf. xiii. Fig. 1. [3] *Idem*. Taf. xvi. Figs. 1, 2.
[4] Renault, "Annales de Sciences naturelles," Bot., Tome iii. Pl. iii.
[5] *Idem*. Pl. ii.
[6] "Memoir" i. Pl. xxiv. Fig. 14 *e*, and Pl. xxvi. Fig. 24 *e*.
[7] "Memoirs of the Literary and Philosophical Society of Manchester," 3rd series, vol. iv. p. 248. [8] "Memoir" xi. Pl. liv. Figs. 23, 24.

from which aërial stems are sent upwards. I am satisfied that there is not a shadow of foundation for such a belief. The same authors, along with their distinguished countryman, the Marquis de Saporta, believe with Brongniart that it is possible to separate *Sigillaria* widely from *Lepidodendron*. They leave the latter plant amongst the *Lycopods*, and elevate the former to the rank of a Gymnospermous exogen. I have in vain demonstrated the existence of a large series of specimens of the same species of plant, young states of which display all the essential features of structure which they believe to characterise *Lepidodendron*, whilst, in its progress to maturity, every stage in the development of the secondary wood, regarded by them as characteristic of a *Sigillaria*, can be followed step by step.[1] Nay, more: my cabinet contains specimens of young dichotomously branching twigs, on which one of the two diverging branches has only the centripetal cylinder of the *Lepidodendron*, whilst the other has begun to develop the secondary wood of the *Sigillaria*.[2]

The distinguished botanist of the Institut, Ph. van Tieghem, has recently paid some attention to the conclusions adopted by his three countrymen in this controversy, and has made an important advance upon those conclusions, in what I believe to be the right direction. He recognises the Lycopodiaceous character of the *Sigillariæ*, and their close relations to the *Lepidodendra*;[3] and he also accepts my demonstration of the unipolar, and consequently Lycopodiaceous, character of the fibro-vascular bundle of the Stigmarian rootlet, a peculiarity of structure of which M. Renault has hitherto denied the existence. But along with these recognitions of the accuracy of my conclusions he gives fresh currency to several of the old errors relating to parts of the subject to which he has not yet given personal attention. Thus he considers that the *Sigillariæ*, though closely allied to the *Lepidodendra*, are distinguished from them by possessing the power of developing the centrifugal or exogenous zone of vascular tissue already referred to. He characterises the *Lepidodendra* as having "*un seul bois centripète*," notwithstanding the absolute demonstrations to the contrary contained in my "Memoir" xi. Dealing with the root of *Sigillaria*, which in Great Britain at least is the well-known *Stigmaria ficoides*, following Renault, he designates it a "*rhizome*," limiting the term root to what we designate the rootlets. He says, "Le rhizome des Sigillaires a la même structure que la tige aërienne, avec des bois primaires tantôt isolés à la périphérie de la moelle, tantôt confluents au centre et en un axe plein; seulement les fasceaux libéro-ligneux secondaires y sont séparés par de plus larges rayons," &c.

Now, *Stigmaria* being a root, and not a rhizome, contains no representative of the primary wood of the stem. This latter is, as even M. Brongniart so correctly pointed out long ago, the representative of the medullary sheath, and the fibro-vascular bundles which it gives off are all foliar ones, as is the case with the bundles given off by this sheath in all exogenous plants. But in the *Lepidodendra* and *Sigillariæ*, as in all living exogens, it is not prolonged into the root. In the latter, as might be expected *a priori*, we only find the secondary or exogenous vascular zone. Having probably the largest collection of sections of *Stigmariæ* in the world, I speak unhesitatingly on these points. M. van Tieghem further says, 'La tige aërienne part d'un rhizome rameux très-développé nommé *Stigmaria*, sur lequel s'insèrent à la fois de petites feuilles et des racines parfois dichotomées.' I have yet to see a solitary fact justifying the statement that leaves are intermingled with the rootlets of *Stigmaria*. The statement rests upon an entire misinterpretation of sections of the fibro-vascular bundles supplying those rootlets and an ignorance of the nature and positions of the rootlets themselves. More than forty years have elapsed since John Eddowes Bowman first demonstrated that the *Stigmariæ* were true roots, and every subsequent British student has confirmed Bowman's accurate determination.

M. Lesquereux informs me that his American experiences have convinced him that *Sigillaria* is Lycopodiaceous. Dr. Dawson has now progressed so far in the same direction as to believe that there exists a series of Sigillarian forms which link the *Lepidodendra* on the one hand with the Gymnospermous exogens on the other. As an evolutionist I am prepared to accept the possibility that such links may exist. They certainly do, so far as the union of *Lepidodendron* with *Sigillaria* is concerned. I have not yet seen any from the higher part of the chain that are absolutely satisfactory to me, but Dr. Dawson thinks that he has found such. I may add that Schimper and the younger German school have always associated *Sigillaria* with the *Lycopodiaceæ*. But there are yet other points under discussion connected with these fossil Lycopods.

M. Renault affirms that some forms of *Halonia* are subterranean rhizomes, and the late Mr. Binney believed that *Haloniæ* were the roots of *Lepidodendron*. I am not acquainted with a solitary fact justifying either of these suppositions, and unhesitatingly reject them. We have the clearest evidence that some *Haloniæ* at least are true terminal, and, as I believe, strobilus-bearing, branches of various Lepidodendroid plants, and I see no reason whatever for separating *Halonia regularis* from those whose fruit-bearing character is almost absolutely determined. Its branches, like the others, are covered throughout their entire circumference, and in the most regularly symmetrical manner, with leaf-scars, a feature wholly incompatible with the idea of the plant being either a root or a rhizome. M. Renault has been partly led astray in this matter by misinterpreting a figure of a specimen published by the late Mr. Binney. That specimen being now in the museum of Owens College, we are able to demonstrate that it has none of the features which M. Renault assigns to it.

The large round or oval distichously-arranged scars of *Ulodendron* have long stimulated discussion as to their nature. This, too, is now a well-understood matter. Lindley and Hutton long ago suggested that they were scars whence cones had been detached, a conclusion which was subsequently sustained by Dr. Dawson and Schimper, and which structural evidence led me also to support.[1] The matter was set at rest by Mr. d'Arcy Thompson's discovery of specimens with the strobili *in situ*. Only a small central part of the conspicuous cicatrix characterising the genus represented the area of organic union of the cone to the stem. The greater part of that cicatrix has been covered with foliage, which, owing to the shortness of the cone-bearing branch, was compressed by the base of the cone. The large size of many of these biserial cicatrices on old stems has been due to the considerable growth of the stem subsequently to the fall of the cone.

Our knowledge of the terminal branches of the large-ribbed *Sigillariæ* is still very imperfect. Palæontologists who have urged the separation of the *Sigillariæ* from the *Lepidodendra* have attached weight to the difference between the longitudinally-ridged and furrowed external bark of the former plants, along which ridges the leaf-scars are disposed in vertical lines, and the diagonally-arranged scars of *Lepidodendron*. They have also dwelt upon the alleged absence of branches from the Sigillarian stems. I think that their mistake, so far as the branching is concerned, has arisen from their expectation that the branches must necessarily have had the same vertically-grooved appearance, and longitudinal arrangement of the leaf-scars, as they observed in the more aged trunks; hence they have probably *seen* the branches of *Sigillariæ* without *recognising* them. Personally I believe this to have been the case. I farther entertain the belief that the transition from the *vertical* phyllotaxis, or leaf arrangement of the Sigillarian leaf-scars, to the *diagonal* one of the *Lepidodendra* will ultimately be found to be effected through the subgenus *Favularia*, in many of which the diagonal arrangement becomes quite as conspicuous as the vertical one. This is the case even in Brongniart's classic specimen of *Sigillaria elegans*, long the only fragment of that genus known which preserved its internal structure. The fact is, the shape of the leaf-scars, as well as their proximity to each other, underwent great changes as Lepidodendroid and Sigillarian stems advanced from youth to age. Thus Presl's genus Bergeria was based on forms of Lepidodendroid scars which we now find on the terminal branches of unmistakable *Lepidodendra*.[2] The phyllotaxis of *Sigillaria*, of the type of *S. occulata*, passes by imperceptible gradations into that of *Favularia*. In many young branches the leaves were densely crowded together, but the exogenous development of the interior of the stem, and its consequent growth both in length and thickness, pushed these scars apart at the same time that it increased their size and altered their shape. We see precisely the same effects produced upon the large fruit scars of Ulodendron by the same causes. The Carboniferous Lycopods were mostly arborescent, but some few dwarf forms, apparently like the modern *Selaginellæ*, have been found in the Saarbrücken coal-fields. Many, if not all, the arborescent forms produced secondary wood, by means of a cambium-layer, as they increased in age. In the case of some of them[3] this was done in a very rudimentary manner, nevertheless sufficiently so to demonstrate

1 "Memoir" xi. Plates xlvii.—lii. 2 *Idem*. Pl. xlix. Fig. 8.
3 "Traité de Botanique," p. 1304.

1 "Memoir" ii. p. 222. 2 See "Memoir" xii. Pl. xxxiv.
3 *E.g. L. Harcourtii*, "Memoir" ix. Pl. xlix. Fig. 11.

what is essential to the matter, viz. the existence of a cambium-layer producing "centrifugal growth of secondary vascular tissue."

As already pointed out in the case of the Calamites, the vascular axis of these *Lepidodendra* was purely an *appareil conducteur*, unmixed with any wood cells; hence the *appareil de soutien* had to be supplied elsewhere. This was done, as in the Calamites: a thick, persistent, hypodermal zone of meristem[1] developed a layer of prismatic prosenchyma of enormous thickness,[2] which incased the softer structures in a strong cylinder of self-supporting tissue. We have positive evidence that the fructification of many of these plants was in the form of heterosporous strobili. Whether or not such was the case with all these *Lepidostrobi* we are yet unable to determine. But the incalculable myriads of their macrospores, seen in so many coals, afford clear evidence that the heterosporous types must have preponderated vastly over all others.

Gymnosperms.—Our knowledge of this part of the Carboniferous vegetation has made great progress during the last thirty years. This progress began with my own discovery[3] that all our British *Dadoxylons* possessed what is termed a discoid pith, such as we see in the white jasmine, some of the American hickories, and several other plants; at the same time I demonstrated that most of our objects hitherto known as *Artisias* and *Sternbergias* were merely inorganic casts of these discoid medullary cavities. Further knowledge of this genus seems to suggest that it was not only the oldest of the true Conifers in point of time, but also one of the lowest of the coniferous types.

Cycads.—The combined labours of Grand Eury, Brongniart, and Renault have revealed the unexpected predominance in some localities of a primitive but varied type of Cycadean vegetation. Observers have long been familiar with certain seeds known as *Trigonocarpons* and *Cardiocarpons*, and with large leaves to which the name of *Noeggerathia* was given by Sternberg. All these seeds and leaves have been tossed from family to family at the caprice of different classifiers, but in all cases without much knowledge on which to base their determinations. The rich mass of material disinterred by M. Grand-Eury at St. Etienne, and studied by Brongniart and M. Renault, has thrown a flood of light upon some of these objects, which now prove to be primæval types of Cycadean vegetation.

Mr. Peach's discovery of a specimen demonstrating that the *Antholithes Pitcairniæ*[4] of Lindley and Hutton was not only, as these authors anticipated, "the inflorescence of some plant," but that its seeds were the well-known *Cardiocarpons*, was the first link in an important chain of new evidence. Then followed the rich discoveries at St. Etienne, where a profusion of seeds, displaying wonderfully their internal organisation, was brought to light by the energy of M. Grand-Eury, which seeds M. Brongniart soon pronounced to be Cycadean. At the same time I was obtaining many similar seeds from Oldham and Burntisland, in which also the minute organisation was preserved. Dawson, Newberry, and Lesquereux have also shown that many species of similar seeds, though with no traces of internal structure, occur in the coal-measures of North America.

Equally important was the further discovery by M. Grand-Eury that the *Antholithes*, with their Cardiocarpoid seeds, were but one form of the monoclinous catkin-like inflorescences of the *Noeggerathiæ*, now better known by Unger's name of *Cordaites*. These investigations suggest some important conclusions: 1st. The vast number and variety of these Cycadean seeds, as well as the enormous size of some of them, is remarkable, showing the existence of an abundant and important Carboniferous vegetation, of most of which no trace has yet been discovered other than these isolated seeds. 2nd. Most of the seeds exhibit the morphological peculiarity of having a large cavity (the "cavité pollinique" of Brongniart) between the upper end of the nucelle and its investing episperm, and immediately below the micropile of the seed. That this cavity was destined to have the pollen grains drawn into it, and be thus brought into direct connection with the apex of the nucelle, is shown by the various examples in which such grains are still found in that cavity.[1] 3rd. M. Grand-Eury has shown that some of his forms of *Cordaites* possessed the discoid or Sternbergian pith which I had previously found in *Dadoxylon*; and, lastly, these *Cordaites* prove that a diclinous form of vegetation existed at this early period in the history of the flowering plants, but whether in a monœcious or a diœcious form we have as yet no means of determining. Their reproductive structures differ widely from the true cones borne by most Cycads at the present day.

Conifers.—It has long been remarked that few real cones of Conifers have hitherto been found in the Carboniferous rocks, and I doubt if any such have yet been met with. Large quantities of the woody stems now known as *Dadoxylons* have been found both in Europe and America. These stems present a true coniferous structure both in the pith, medullary sheath, wood, and bark.[2] The wood presents one very peculiar feature. Its foliar bundles, though in most other respects exactly like those of ordinary Conifers, are given off, not singly, but in pairs.[3] I have only found this arrangement of double foliar bundles in the Chinese Gingko (*Salisburia adiantifolia*).[4] This fact is not unimportant when connected with another one. Sir Joseph Hooker long ago expressed his opinion that the well-known *Trigonocarpons*[5] of the coal measures were the seeds of a Conifer allied to this *Salisburia*. The abundance of the fragments of *Dadoxylon*, combined with the readiness with which cones and seeds are preserved in a fossil state, make it probable that the fruits belonging to these woody stems would be so preserved. But of cones we find no trace, and, as we discover no other plant in the Carboniferous strata to which the *Trigonocarpons* could with any probability have belonged, these facts afford grounds for associating them with the *Dadoxylons*. These combined reasons, viz. the structure of the stems with their characteristic foliar bundles, and the Gingko-like character of the seeds, suggest the probability that these *Dadoxylons*, the earliest of known Conifers, belonged to the *Taxineæ*, the lowest of these coniferous types, and of which the living *Salisburia* may perhaps be regarded as the least advanced recent form.

Thus far our attention has been directed only to plants whose affinities have been ascertained with such a degree of probability as to make them available witnesses, so far as they go, when the question of vegetable evolution is *sub judice*. But there remain others, and probably equally important ones, respecting which we have yet much to learn. In most cases we have only met with detached portions of these plants, such as stems or reproductive structures, which we are unable to connect with their other organs. The minute tissues of these plants are preserved in an exquisite degree of perfection; hence we are able to affirm that, whatever they may be, they differ widely from every type that we are acquainted with amongst living ones. The exogenous stems or branches from Oldham and Halifax which I described under the name of *Astromyelon*,[6] and of which a much fuller description will be found in my forthcoming Memoir xii., belong to a plant of this description. The remarkable conformation of its bark obviously indicates a plant of more or less aquatic habits, since it closely resembles those of *Myriophyllum*, *Marsilea*, and a number of other aquatic plants belonging to various classes. But its general features suggest nearer affinities to the latter genus than to any other. Another very characteristic stem is the *Heterangium Grievii*,[7] only found in any quantity at Burntisland, but of which we have recently obtained one or two small specimens at Halifax. This plant displays an abundant supply of primary, isolated, vascular bundles, surrounded by a very feeble development of secondary vascular tissue. Still more remarkable is the *Lyginodendron Oldhamium*,[8] a stem not uncommon at Oldham, and not unfrequently found at Halifax. Unlike the *Heterangium*, its primary vascular elements are feeble, but its tendency to develop secondary zylem is very characteristic of the plant. An equally peculiar feature is seen in the outermost layer of its cellular bark, which is penetrated by innumerable longitudinal laminæ of prosenchymatous tissue, which is arranged in precisely the same way as is the hard bast in the lime and similar trees,

1 "Memoir" ix. Pl. xxv. Figs. 93, 94, 98, 99, 100, and 101.
2 "Memoir" xi. Pl. xlviii. Fig. 4 *ff'*. "Memoir" ii. Pl. xxix. Fig. 42 *k*. "Memoir" iii. Pl. xliii. Fig. 17.
3 "On the Structure and Affinities of the Plants hitherto known as Sternbergias," "Memoirs of the Literary and Philosophical Society of Manchester," 1851. M. Renault, in his "Structure comparée de quelques Tiges de la Flore Carbonifère" p. 285, has erroneously attributed this discovery to Mr. Dawes, including my illustration from the Jasminium and Juglans. Mr. Dawes' explanation was a very different one.
4 "Fossil Flora," p. 82.

1 "Memoir" viii. Pl. ii. Figs. 70 and 72. Brongniart, "Recherches sur les Graines Fossiles Silicifiées," Pl. xvi. Figs. 1, 2; Pl. xx. Fig. 2.
2 Dr. Dawson finds the discoid pith in one of the living Canadian Conifers.
3 "Memoir" viii. Pl. lviii. Fig. 48, and Pl. ix. Figs. 44-46.
4 "Memoir" xii. Pl. xxxiii. Figs. 28, 29.
5 "Memoir" viii. Figs. 94-115.
6 "Memoir" ix., in which I only described decorticated specimens. Messrs. Cash and Hick described a specimen in which the peculiar bark was preserved under the name of *Astromyelon Williamsonis*. See "Proceedings of the Yorkshire Polytechnic Society," vol. vii. part iv. 1881.
7 "Memoir" iii. 8 "Memoir" iii.

affording another example of the introduction into the outer bark of the *appareil de soutien*. As might have been anticipated from this addition to the bark, this plant attained arborescent dimensions, very large fragments of sandstone casts of the exterior surface of the bark[1] being very abundant in most of the leading English coal-fields. Corda also figured it[2] from Radnitz, confounding it, however, with his Lepidodendroid *Sagenaria fusiformis*, with which it has no true affinity. Of the smaller plants of which we know the structure but not the systematic position, I may mention the beautiful little *Kaloxylons*.[3] We have also obtained a remarkable series of small spherical bodies, to which I have given the provisional generic name of *Sporocarpon*.[4] Their external wall is multicellular; hence they cannot be spores. Becoming filled with free cells, which display various stages of development as they advance to maturity, we may infer that they are reproductive structures. Dr. Dawson informs me that he has recently obtained some similar bodies, also containing cells, from the Devonian beds of North and South America. Except in calling attention to some slight resemblance existing between my objects and the sporangiocarps of *Pilularia*,[5] I have formed no opinion respecting their nature. Dr. Dawson has pointed out that his specimens also suggest relations with the Rhizocarpæ.

I am unwilling to close this address without making a brief reference to the bearing of our subject upon the question of evolution. Various attempts have been made to construct a genealogical tree of the vegetable kingdom. That the Cryptogams and Gymnosperms made their appearance, and continued to flourish on this earth, long prior to the appearance of the monocotyledonous and dicotyledonous flowering plants, is at all events a conclusion justified by our present knowledge so far as it goes. Every one of the supposed Palms, Aroids, and other Monocotyledons has now been ejected from the lists of Carboniferous plants, and the Devonian rocks are equally devoid of them. The generic relations of the Carboniferous vegetation to the higher flowering plants found in the newer strata have no light thrown upon them by these Palæozoic forms. These latter do afford us a few plausible hints respecting some of their Cryptogamic and Gymnospermous descendants, and we know that the immediate ancestors of many of them flourished during the Devonian age, but here our knowledge practically ceases. Of their still older genealogies scarcely any records remain. When the registries disappeared, not only had the grandest forms of Cryptogamic life that ever lived attained their highest development, but even the yet more lordly Gymnosperms had become a widely diffused and flourishing race. If there is any truth in the doctrine of evolution, and especially if long periods of time were necessary for a world-wide development of lower into higher races, a terrestrial vegetation must have existed during a vast succession of epochs ere the noble Lycopods began their prolonged career. Long prior to the Carboniferous age they had not only made this beginning, but during that age they had diffused themselves over the entire earth. We find them equally in the Old World and in the New. We discover them from amid the ice-clad rocks of Bear Island and Spitzbergen to Brazil and New South Wales. Unless we are prepared to concede that they were simultaneously developed at these remote centres, we must recognise the incalculable amount of time requisite to spread them thus from their birthplace, wherever that may have been, to the ends of the earth. Whatever may have been the case with the southern hemisphere, we have also clear evidence that in the northern one much of this wide distribution must have been accomplished prior to the Devonian age. What has become of this pre-Devonian flora? Some contend that the lower cellular forms of plant life were not preserved because their delicate tissues were incapable of preservation. But why should this be the case? Such plants are abundantly preserved in Tertiary strata, why not equally in Palæozoic ones? The explanation must surely be sought, not in their incapability of being preserved, but in the operation of other causes. But the Carboniferous rocks throw another impediment in the way of constructors of these genealogical trees. Whilst Carboniferous plants are found at hundreds of separate localities, widely distributed over the globe, the number of spots at which these plants are found displaying any internal structure is extremely few. It would be difficult to enumerate a score of such spots. Yet each of those favoured localities has revealed to us forms of plant life of which the ordinary plant-bearing shales and sandstones of the same localities show no traces. It seems, therefore, that whilst there was a general resemblance in the more conspicuous forms of Carboniferous vegetation from the Arctic circle to the extremities of the southern hemisphere, each locality had special forms that flourished in it either exclusively or at least abundantly, whilst rare elsewhere. It would be easy, did time allow, to give many proofs of the truth of this statement. Our experiences at Oldham and Halifax, at Arran and Burntisland, at St. Etienne and Autun, tell us that such is the case. If these few spots which admit of being searched by the aid of the microscope have recently revealed so many hitherto unknown treasures, is it not fair to conclude that corresponding novelties would have been furnished by all the other plant-producing localities if these plants had been preserved in a state capable of being similarly investigated? I have no doubt about this matter; hence I conclude that there is a vast variety of Carboniferous plants of which we have as yet seen no traces, but every one of which must have played some part, however humble, in the development of the plant races of later ages. We can only hope that time will bring these now hidden witnesses into the hands of future palæontologists. Meanwhile, though far from wishing to check the construction of any legitimate hypothesis calculated to aid scientific inquiry, I would remind every too ambitious student that there is a haste that retards rather than promotes progress; that arouses opposition rather than produces conviction; and that injures the cause of science by discrediting its advocates.

[1] "Memoir" iv. Pl. xxvii.
[2] "Flora der Vorvelt," Tab. 6, Fig. 4.
[3] "Memoir" vii. [4] "Memoirs" ix. x. [5] "Memoir" ix. p. 348.

NOTES

We are glad to be able to publish this week an article by a distinguished foreign botanist on Bentham and Hooker's great work, "Genera Plantarum."

We regret to announce the death, on the 15th inst., of the eminent physicist, M. Joseph-Antoine-Ferdinand Plateau, Emeritus Professor at the University of Ghent. Professor Plateau was a Foreign Member of the Royal Society, Member of the Academy of Sciences of Berlin, and Corresponding Member of the Paris Academy of Sciences. He was in his eighty-second year.

Admiral Sir Richard Collinson, K.C.B., Deputy Master of the Trinity Corporation, died last week at his residence, Haven Green, Ealing. He was born in 1811 at Gateshead, of which place his father was rector. He entered the navy in 1823, was employed in various surveying expeditions under Captain Belcher and others from 1831 to 1839, took an active part in the first Chinese war, and remained afterwards four years on the China coast, making plans of harbours and laying down the coast line. He commanded the expedition, consisting of the *Enterprise* and *Investigator*, despatched by the Admiralty in 1850 in search of Sir John Franklin and his companions, and on his return to England in 1854 Captain Collinson received the medal of the Royal Geographical Society for his explorations in Arctic regions. He received his promotion to flag rank in 1862, was elected an Elder Brother of the Trinity House in the same year, and has been Deputy Master of that Corporation since 1875.

The death is announced of Mr. Werdermann, the inventor of the well-known semi-incandescent electric light.

Herr Marno, the well-known explorer of North Central Africa, has died at Khartoum.

The Astronomische Gesellschaft met in Vienna last week.

The Lord President of the Committee of Council on Education has appointed Valentine Ball, M.A., F.R.S., Professor of Geology and Mineralogy in the University of Dublin, Director of the Dublin Museum of Science and Art. Prof. V. Ball is the brother of the Astronomer Royal for Ireland, and the author of several interesting and important works, among which may be enumerated "The Economic Geology of India" and "Experiences of Jungle Life in India"; his appointment is regarded as in every way an excellent one. In addition to his geological attainments, Prof. V. Ball is also known by his papers on various ethnological subjects. This appointment will leave the Chair of Geology and Mineralogy in the University of Dublin vacant after next Michaelmas Term.

THE Improvement Commissioners of Bournemouth, at a meeting on Tuesday, discussed the desirability of inviting the British Association to visit Bournemouth. It was unanimously decided to invite it for 1885.

THE last news received by the Russian Geographical Society from the Lena meteorological station is dated April 3. The observers have suffered to some extent from the hard winter, and especially from the winds, and it was with difficulty that they succeeded in maintaining a moderate temperature in their house. Still they were all in good health. The lowest temperature observed was $-52°\cdot3$ Celsius on February 9. In January and February it usually did not fall below $-40°$, excepting during quite calm weather. In March the thermometer oscillated about $-40°$, and at the beginning of April it began to rise to $-19°$. M. Yurgens found great difficulties with the magnetic instruments, the range of deviation of the needles during the magnetic perturbations being as much as 25° from the magnetic meridian, and those which measure the horizontal intensity showing deviations of as much as 90°.

THE subterranean rooms of the Paris Observatory are ready for the reception of the magnetic instruments. Three sets will be arranged—one for registering, the second for direct observation as established by Lamont at Munich, and the third will be composed of the old instruments used by Arago for comparing the numbers taken in former times.

CIRCUMSTANCES, says *Science*, were not favourable to the production of remarkable essays at the recent meeting of the American Association. The attendance was not large. The officers of the meeting, and especially those who had to make addresses, could scarcely be expected to produce elaborate papers in addition to their other labours. As the number of addresses per meeting has increased, we may observe more readily some of the effects of the system that demands them. The most evident result is that usually where we gain one good address we lose two or three good papers. The distance of the meeting from their homes affected especially members of Sections A, B, C, and D, devoted to the exact sciences. Perhaps it affected the quality as well as the number of their papers. There were not many from the east to present essays, though quite as many as could have reasonably been expected; but there were scarcely any from the locality of the meeting and its neighbourhood. Local interest, both as to authors and hearers, was of course deficient. In short, there was nothing remarkable in those sections to spur production, and the product was not remarkable. It was good, but not great.

THE fourth annual "Cryptogamic Meeting" of the Essex Field Club will take place in Epping Forest on Saturday, September 29. A large number of botanists have promised to be present and act as referees. In the evening a meeting for the exhibition of botanical specimens will be held in the Assembly Room at the "Roebuck" Hotel, Buckhurst Hill, when the following papers will be read:—"Recent Additions to the Fungus Flora of Epping Forest," by Dr. M. C. Cooke, M.A., F.L.S.; "The 'Lower Orders' of Fungi," by Worthington G. Smith, F.L.S.; "Fungi as Poisons," by Dr. Wharton, M.A., F.L.S. Botanists wishing to attend the meeting or to exhibit specimens should communicate with the Hon. Secretary, Mr. W. Cole, Buckhurst Hill, Essex.

MR. SIMMONS and a companion left Hastings in a balloon at 3.20 p.m. on Thursday last, and landed in about seven hours at Cape La Hogue, in France.

THE additions to the Zoological Society's Gardens during the past week include two Chinese Rhesus Monkeys (*Macacus lasiotus* ♂ ♀) from China, presented by Mr. G. A. Conder; a Pig-tailed Monkey (*Macacus nemestrinus* ♂) from Java, presented by Mr. Robert Smith; a Hog Deer (*Cervus porcinus* ♂) from India, presented by Mr. D. Charles Horne; a Snow Bunting (*Plectrophanes nivalis*), European, presented by Mr. E. J. Gibbins; two Ring Doves (*Columba palumbus*), British, presented by Mrs. Courage; two Land Rails (*Crex pratensis*), British, presented by Dr. Marshall; a Robben Island Snake (*Coronella phocarum*), a Rufescent Snake (*Leptodira rufescens*), a Ring-hals Snake (*Sepedon hæmachetes*) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; a Grey Seal (*Halichærus gryphus*) from Cornwall, two Margined Tortoises (*Testudo marginata*), South European, a Glass Snake (*Pseudopus pallasi*) from Dalmatia, deposited.

A PLEA FOR PURE SCIENCE [1]

I AM required to address the so-called Physical Section of this Association. Fain would I speak pleasant words to you on this subject; fain would I recount to you the progress made in this subject by my countrymen, and their noble efforts to understand the order of the universe. But I go out to gather the grain ripe to the harvest, and I find only tares. Here and there a noble head of grain rises above the weeds; but so few are they that I find the majority of my countrymen know them not, but think that they have a waving harvest, while it is only one of weeds after all. American science is a thing of the future, and not of the present or past; and the proper course of one in my position is to consider what must be done to create a science of physics in this country, rather than to call telegraphs, electric lights, and such conveniences by the name of science. I do not wish to underrate the value of all these things: the progress of the world depends on them, and he is to be honoured who cultivates them successfully. So also the cook who invents a new and palatable dish for the table, benefits the world to a certain degree; and yet we do not dignify him by the name of a chemist. And yet it is not an uncommon thing, especially in American newspapers, to have the *applications* of science confounded with pure science; and some obscure American who steals the ideas of some great mind of the past and enriches himself by the application of the same to domestic uses, is often lauded above the great originator of the idea, who might have worked out hundreds of such applications had his mind possessed the necessary element of vulgarity. I have often been asked which was the more important to the world, pure or applied science. To have the applications of a science, the science itself must exist. Should we stop its progress and attend only to its applications, we should soon degenerate into a people like the Chinese, who have made no progress for generations, because they have been satisfied with the applications of science, and have never sought for reasons in what they have done. The reasons constitute pure science. They have known the application of gunpowder for centuries; and yet the reasons for its peculiar action, if sought in the proper manner, would have developed the science of chemistry, and even of physics, with all their numerous applications. By contenting themselves with the fact that gunpowder would explode, and seeking no further, they have fallen behind in the progress of the world; and we now regard this oldest and most numerous of nations as only barbarians. And yet our own country is in this same state. But we have done better; for we have taken the science of the Old World and applied it to all our uses, accepting it like the rain of heaven, without asking whence it came, or even acknowledging the debt of gratitude we owe to the great and unselfish workers who have given it to us. And, like the rain of heaven, this pure science has fallen upon our country, and made it great and rich and strong.

To a civilised nation of the present day the applications of science are a necessity; and our country has hitherto succeeded in this line only for the reason that there are certain countries in the world where pure science has been and is cultivated, and where the study of nature is considered a noble pursuit. But such countries are rare, and those who wish to pursue pure

[1] Condensed abstract of the address of Prof. H. A. Rowland of Baltimore, vice-president of Section B (Physics), before the American Association at Minneapolis, August 15. In using the word science the author refers to physical science, "as I know nothing of natural science. Probably my remarks will, however, apply to both, but I do not know."

science in our own country must be prepared to face public opinion in a manner which requires much moral courage. They must be prepared to be looked down upon by every successful inventor whose shallow mind imagines that the only pursuit of mankind is wealth, and that he who obtains most has best succeeded in this world. Everybody can comprehend a million of money; but how few can comprehend any advance in scientific theory; especially in its more abstruse portions! And this, I believe, is one of the causes of the small number of persons who have ever devoted themselves to work of the higher order in any human pursuit. Man is a gregarious animal, and depends very much, for his happiness, on the sympathy of those around him; and it is rare to find one with the courage to pursue his own ideals in spite of his surroundings. In times past, men were more isolated than at present, and each came in contact with a fewer number of people. Hence that time constitutes the period when the great sculptures, paintings, and poems were produced. Each man's mind was comparatively free to follow its own ideals, and the results were the great and unique works of the ancient masters. To-day, the railroad and the telegraph, the books and newspapers, have united each individual man with the rest of the world: instead of his mind being an individual, a thing apart by itself, and unique, it has become so influenced by the outer world, and so dependent upon it, that it has lost its originality to a great extent. The man who in times past would naturally have been in the lowest depths of poverty, mentally and physically, to-day measures tape behind a counter, and with lordly air advises the naturally born genius how he may best bring his outward appearance down to a level with his own. A new idea he never had, but he can at least cover his mental nakedness with ideas imbibed from others. So the genius of the past soon perceives that his higher ideas are too high to be appreciated by the world: his mind is clipped down to the standard form; every natural offshoot upwards is repressed, until the man is no higher than his fellows. Hence the world, through the abundance of its intercourse, is reduced to a level. What was formerly a grand and magnificent landscape, with mountains ascending above the clouds, and depths whose gloom we cannot now appreciate, has become serene and peaceful. The depths have been filled, and the heights levelled, and the wavy harvests and smoky factories cover the landscape.

As far as the average man is concerned, the change is for the better. The average life of man is far pleasanter, and his mental condition better, than before. But we miss the vigour imparted by the mountains. We are tired of mediocrity, the curse of our country. We are tired of seeing our artists reduced to hirelings, and imploring Congress to protect them against foreign competition. We are tired of seeing our countrymen take their science from abroad, and boast that they here convert it into wealth. We are tired of seeing our professors degrading their chairs by the pursuit of applied science instead of pure science; or sitting inactive while the whole world is open to investigation; lingering by the wayside while the problem of the universe remains unsolved.

For generations there have been some few students of science who have esteemed the study of nature the most noble of pursuits. Some have been wealthy, and some poor; but they have all had one thing in common—the love of nature and its laws. To these few men the world owes all the progress due to applied science, and yet very few ever received any payment in this world for their labours.

But there will be those in the future, as well as in the past, who will do so; and for them higher prizes than any yet obtained are waiting. We have but yet commenced our pursuit of science, and stand upon the threshold wondering what there is within. We explain the motion of the planet by the law of gravitation; but who will explain how two bodies, millions of miles apart, tend to go toward each other with a certain force?

We now weigh and measure electricity and electric currents with as much ease as ordinary matter, yet have we made any approach to an explanation of the phenomenon of electricity? Light is an undulatory motion, and yet do we know what it is that undulates? Heat is motion, yet do we know what it is that moves? Ordinary matter is a common substance, and yet who shall fathom the mystery of its internal constitution?

How shall we, then, honour the few, the very few, who, in spite of all difficulties, have kept their eyes fixed on the goal, and have steadily worked for pure science, giving to the world a most precious donation, which has borne fruit in our greater knowledge of the universe and in the applications to our physical life which have enriched thousands and benefited each one of us? There are also those who have every facility for the pursuit of science, who have an ample salary and every appliance for work, yet who devote themselves to commercial work, to testifying in courts of law, and to any other work to increase their present large income. Such men would be respectable if they gave up the name of professor, and took that of consulting chemists or physicists. And such men are needed in the community. But for a man to occupy the professor's chair in a prominent college, and, by his energy and ability in the commercial applications of his science, stand before the local community in a prominent manner, and become the newspaper exponent of his science, is a disgrace both to him and his college. It is the deathblow to science in that region. Call him by his proper name, and he becomes at once a useful member of the community. Put in his place a man who shall by precept and example cultivate his science, and how different is the result! Young men, looking forward into the world for something to do, see before them this high and noble life, and they see that there is something more honourable than the accumulation of wealth. They are thus led to devote their lives to similar pursuits, and they honour the professor who has drawn them to something higher than they might otherwise have aspired to.

I do not wish to be misunderstood in this matter. It is no disgrace to make money by an invention, or otherwise, or to do commercial scientific work under some circumstances. But let pure science be the aim of those in the chairs of professors, and so prominently the aim that there can be no mistake. If our aim in life is wealth, let us honestly engage in commercial pursuits and compete with others for its possession. But if we choose a life which we consider higher, let us live up to it, taking wealth or poverty as it may chance to come to us, but letting neither turn us aside from our pursuit.

The work of teaching may absorb the energies of many; and indeed this is the excuse given by most for not doing any scientific work. But there is an old saying that where there is a will there is a way. Few professors do as much teaching or lecturing as the German professors, who are also noted for their elaborate papers in the scientific journals. A university should not only have great men on its faculty, but have numerous minor professors and assistants of all kinds, and should encourage the highest work, if for no other reason than to encourage the student to his highest efforts. But, assuming that the professor has high ideals, wealth such as only a large and high university can command is necessary to allow him the fullest development.

And this is specially so in our science of physics. In the early days of physics and chemistry many of the fundamental experiments could be performed with the simplest apparatus. And so we often find the names of Wollaston and Faraday mentioned as needing scarcely anything for their researches. Much can even now be done with the simplest apparatus; and nobody, except the utterly incompetent, need stop for want of it. But the fact remains that one can only be free to investigate in all departments of chemistry and physics, when he not only has a complete laboratory at his command, but a friend to draw on for the expenses of each experiment. That simplest of the departments of physics, namely, astronomy, has now reached such perfection that nobody can expect to do much more in it without a perfectly equipped observatory; and even this would be useless without an income sufficient to employ a corps of assistants to make the observations and computations.

But would it not be possible to so change public opinion that no college could be founded with a less endowment than say 1,000,000 dollars, or no university with less than three or four times that amount?

The total wealth of the 400 colleges and universities was in 1880 about 40,000,000 dollars in buildings, and 43,000,000 dollars in productive funds. This would be sufficient for one great university of 10,000,000 dollars, four of 5,000,000 dollars, and twenty-six colleges of 2,000,000 dollars each. But such an idea can of course never be carried out. Government appropriations are out of the question, because no political trickery must be allowed around the ideal institution.

In the year 1880 the private bequests to all schools and colleges amounted to about 5,500,000 dollars. We must make the need of research and of pure science felt in the country. We must live such lives of pure devotion to our science, that all shall see that we ask for money, not that we may live lives of indolent ease at the expense of charity, but that we may work for that which has advanced and will advance the world more than any other subject, both intellectually and physically. We must live such lives as to neutralise the influence of those who in high places have degraded their profession, or have given themselves over to ease, and do nothing for the science which they represent. Let us do what we can with the present means at our disposal. There is not one of us who is situated in the position best adapted to bring out all his powers, and to allow him to do most for his science. All have their difficulties, and I do not think that circumstances will ever radically change a man. If a man has the instinct of research in him, it will always show itself in some form.

I do not believe anybody can be thorough in any department of science, without wishing to advance it. In the study of what is known, in the reading of the scientific journals, and the discussions therein contained of the current scientific questions, one would obtain an impulse to work, even though it did not before exist. And the same spirit which prompted him to seek what was already known, would make him wish to know the unknown. And I may say that I never met a case of thorough knowledge in my own science, except in the case of well-known investigators. I have met men who talked well, and I have sometimes asked myself why they did not do something; but further knowledge of their character has shown me the superficiality of their knowledge.

What would astronomy have done without the endowments of observatories? By their means, that science has become the most perfect of all branches of physics, as it should be from its simplicity. There is no doubt, in my mind, that similar institutions for other branches of physics, or, better, to include the whole of physics, would be equally successful. A large and perfectly equipped physical laboratory, with its large revenues, its corps of professors and assistants, and its machine-shop for the construction of new apparatus, would be able to advance our science quite as much as endowed observatories have astronomy. But such a laboratory should not be founded rashly. The value will depend entirely on the physicist at its head, who has to devise the plan, and to start it into practical working. Such a man would be always rare, and could not always be obtained. After one had been successfully started, others could follow; for imitation requires little brains.

One could not be certain of getting the proper man every time, but the means of appointment should be most carefully studied so as to secure a good average. There can be no doubt that the appointment should rest with a scientific body capable of judging the highest work of each candidate. Should any popular element enter, the person chosen would be either of the literary-scientific order, or the dabbler on the outskirts who presents his small discoveries in the most theatrical manner. What is required is a man of depth, who has such an insight into physical science that he can tell when blows will best tell for its advancement.

Such a grand laboratory as I describe does not exist in the world, at present, for the study of physics. But no trouble has ever been found in obtaining means to endow astronomical science. Everybody can appreciate, to some extent, the value of an observatory; as astronomy is the simplest of scientific subjects, and has very quickly reached a position where elaborate instruments and costly computations are necessary to further advance. The whole domain of physics is so wide that workers have hitherto found enough to do. But it cannot always be so, and the time has even now arrived when such a grand laboratory should be founded. Shall our country take the lead in this matter, or shall we wait for foreign countries to go before? They will be built in the future, but when and how is the question.

As stated before, men are influenced by the sympathy of those with whom they come in contact. It is impossible to immediately change public opinion in our favour; and, indeed, we must always seek to lead it, and not be guided by it. We must create a public opinion in our favour, but it need not at first be the general public. We must be contented to stand aside, and see the honours of the world for a time given to our inferiors; and must be better contented with the approval of our own consciences, and of the very few who are capable of judging our work, than of the whole world beside. Let us look to the other physicists, not in our own town, not in our own country, but in the whole world, for the words of praise which are to encourage us, or the words of blame which are to stimulate us to renewed effort. For what to us is the praise of the ignorant? Let us join together in the bonds of our scientific societies, and encourage each other, as we are now doing, in the pursuit of our favourite study; knowing that the world will some time recognise our services, and knowing, also, that we constitute the most important element in human progress.

But danger is also near, even in our societies. When the average tone of the society is low, when the highest honours are given to the mediocre, when third-class men are held up as examples, and when trifling inventions are magnified into scientific discoveries, then the influence of such societies is prejudicial. A young scientist attending the meetings of such a society soon gets perverted ideas. To his mind a molehill is a mountain, and the mountain a molehill. The small inventor or the local celebrity rises to a greater height, in his mind, than the great leader of science in some foreign land. He gauges himself by the molehill and is satisfied with his stature; not knowing that he is but an atom in comparison with the mountain, until, perhaps, in old age, when it is too late. But, if the size of the mountain had been seen at first, the young scientist would at least have been stimulated in his endeavour to grow.

We call this a free country, and yet it is the only one where there is a direct tax upon the pursuit of science. The low state of pure science in our country may possibly be attributed to the youth of the country; but a direct tax to prevent the growth of our country in that subject cannot be looked upon as other than a deep disgrace. I refer to the duty upon foreign books and periodicals. One would think that books in foreign languages might be admitted free; but to please the half-dozen or so workmen who reprint German books, not scientific, our free intercourse with that country is cut off.

The time is almost past, even in our own country, when third-rate men can find a place as teachers because they are unfit for everything else. We wish to see brains and learning, combined with energy and immense working power, in the professor's chair; but, above all, we wish to see that high and chivalrous spirit which causes one to pursue his idea in spite of all difficulties, to work at the problems of nature with the approval of his own conscience and not of men before him.

The whole universe is before us to study. The greatest labour of the greatest minds have only given us a few pearls; and yet the limitless ocean, with its hidden depths filled with diamonds and precious stones, is before us. The problem of the universe is yet unsolved, and the mystery involved in one single atom yet eludes us. The field of research only opens wider and wider as we advance, and our minds are lost in wonder and astonishment at the grandeur and beauty unfolded before us. Shall we help in this grand work, or not? Shall our country do its share, or shall it still live in the almshouse of the world?

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