THURSDAY, AUGUST 3, 1871

THE ADVANCEMENT OF SCIENCE IN SCHOOLS

WHILE the leaders of Science are in session, and every topic of scientific interest can be brought before them with unusual force and most favourable publicity, we desire to urge the claims of one particular subject as lying at the foundation of all real scientific progress in this country. It is impossible that Science can take root amongst us, that it can inform the national mind or raise the national reputation, while it is excluded from the vast majority of our schools, and while the few schools which have ventured to introduce it are left to struggle unassisted against almost overwhelming difficulties. There are those who congratulate us on the advances made within the last two years, who point with pride to the Eton telescope and the Rugby laboratory, to the Botanical Garden of Clifton and the Scientific Society of Harrow. No doubt the evidence thus cited is most gratifying; no doubt the thanks of the community are due to the men whose individual wisdom and energy have made so admirable a beginning; but if their success is to produce in us only self-complacency, and to hide the enormous deficiencies which it ought to make more glaring and conspicuous, their efforts have been worse than vain.

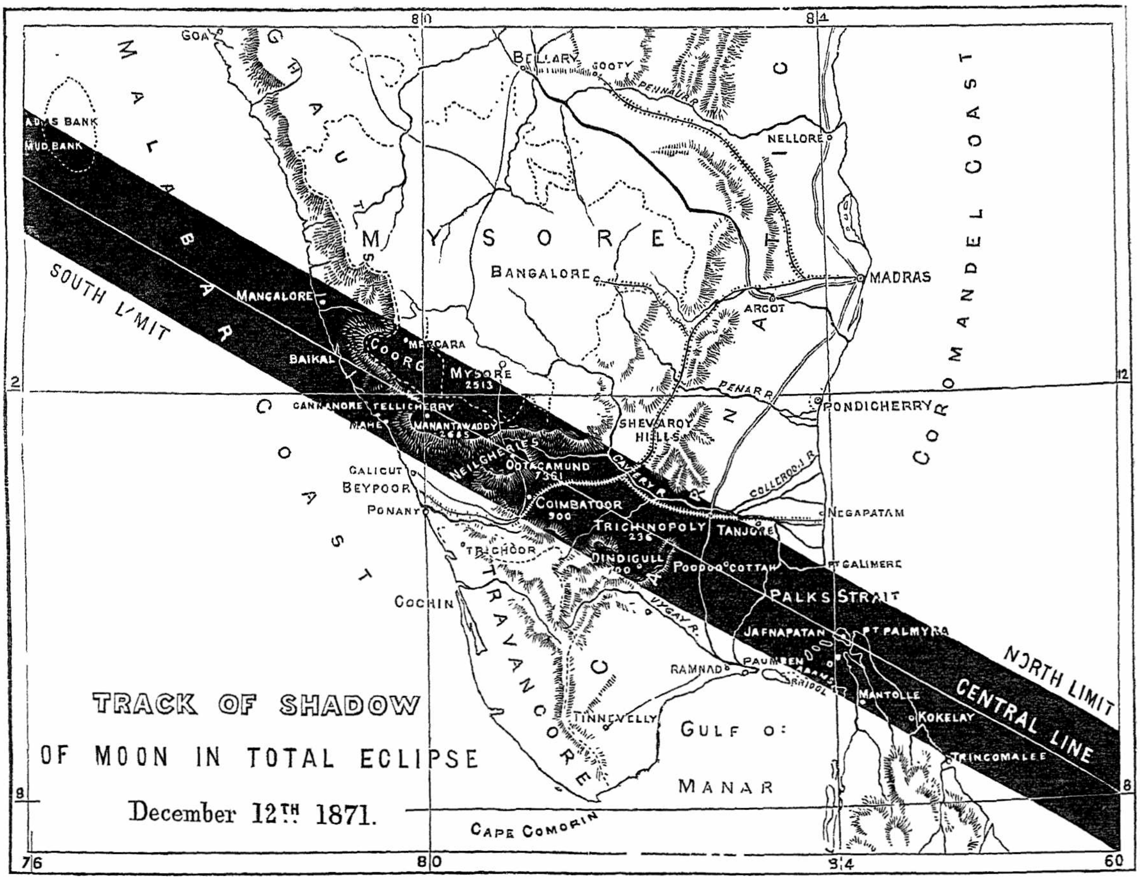
Let us ask the following questions. Of our countless Secondary Schools how many teach or profess to teach Natural Science in any shape whatever? Are there twenty schools in England which teach it systematically on a scale at all extensive, with special master and necessary apparatus? Is there one which accords to it such a place in comparison with other subjects of school teaching as is due to its inherent educational value, its practical use in after life, and the extent to which it is attracting and unfolding the chief intellects of the day? Lastly, are the schools which teach it honestly working on a well-considered plan, agreed amongst themselves as to the economies of methods, subjects, tests; or are their systems contradictory and chaotic, are they ignorant of each others' experience, are their efforts tentative and independent, their results often nugatory, their progress necessarily slow?

There is but one answer to these questions. Science teaching in our schools is as yet potential merely. It rests with those whom we are addressing to make it actual. Observers most conversant with the difficulties which have hitherto kept Science out of schools or paralysed it when nominally admitted, feel most strongly that combined and intelligent action on its behalf, undertaken by men of commanding influence and reputation, is the one thing needful to ensure for it existence, vitality, and permanence. So long as the necessity of teaching it to boys was denied, the action of authority would have been premature. It was necessary that public opinion should be formed, and that experience and argument should work the slow process of conversion. But its claims are now, in theory, established. The most bigoted no longer venture to question its utility; the champions of the old exclusive and one-sided culture are silenced, if not convinced; the general public has pronounced warmly in its favour; the masters and managers of schools are prepared in almost all cases, freely or grudgingly, to admit it. And if this be so; if the principles of opposition are surrendered, and objection rests only upon details; if, further, the deterrent details thus interposing are notorious, and are of a kind which authority, or enlightenment, or guidance, placed in sufficient hands and wielded with sufficient energy, can obviate, surely we may call upon the men whom the suffrage of the scientific world has saluted as its leaders to originate such a plan and to carry out such measures as may supplement the victory of reason over prejudice by assisting willing votaries and kindling half-roused enthusiasm.

There are cases in which the support of external authority is needful for the introduction of Science into schools. Probably few of the readers of NATURE are aware how bitter an opposition is offered to Science teaching by the clergy in many parts of England. The schoolmaster, who, being himself a clergyman, ventures to insist on Science as a necessity in his school curriculum, finds himself the object of a conspiracy as adroit as it is unscrupulous. No matter how able and energetic he may be; no matter how unmistakeably he may care for the moral and religious training of his boys; there is an accursed thing in the midst of him; the word goes forth to ostracise him; the dextrous calumny is dropped in fitting places, his neighbours send their sons elsewhere, and his schemes are broken up. This, which has happened more than once, must happen many times, unless such hapless pioneers of Science can be made to feel that they are backed by men of character, by men whose names are known, to whom they can appeal, who will interfere on their behalf with weight to convince or to overawe their persecutors.

In quite another way again authority is needed. Public competitive examinations, for the universities or elsewhere, must always exercise a paramount influence upon the schools, and must stamp in great measure the value of the subjects taught. It may well be doubted whether in the examinations for India and for Woolwich scientific excellence is appraised sufficiently high. It is quite certain that the influence of the universities both on the higher and lower schools is what it ought not to be in this respect. The local examinations, excellent in many points, vicious in some few, are most vicious in their operation upon Science. The unwise limitation of the subjects taken up, with the certainty that classical and mathematical papers gain many more marks than chemistry or mechanics, prevent the boys in a widely taught school from taking Science in at all, and help to deter masters from a subject which will not count in the examination. And unless they are closely watched, the "matriculation" or "leaving" examinations now contemplated both by Oxford and by Cambridge will be more disastrous still. Between the universities clinging to old subjects as desperately as they distrust the new, and the schoolmasters defeating by nearly ten to one the proposal to give boys the choice between a "linguistic" and a "scientific" matriculation, an obstacle more serious than any which now exists will be built up in the path of Science teaching, if its natural supporters stand aloof from the progress of a mischief which it now lies within their power to avert.

But if School Science lacks authority to help it, it lacks guidance and enlightenment still more. For it may be taken as an established fact that the head masters are, as a body, absolutely helpless. No one can doubt this who will peruse their published utterances on the subject at the Sherborne Meeting in December last. Nor need they be ashamed of the imputation. They owe their position in almost every case to their high classical or mathematical reputation. They are so large minded as to appreciate and to wish to foster in their school studies of whose details they know nothing, and should be allowed to feel that in opening their doors to Science they may fall back with confidence upon supreme and accredited advisers. Think of the difficult points which, without previous experience of any kind, they are called upon to settle. The main subjects of teaching, their relative value, and the order in which they should be taught, the age at which scientific study should commence, the extent to which it may be optional or must be compulsory, the merits and demerits of bifurcation, the text-books to be used, the time to be allowed, the methods of teaching, the frequency of examinations, the mode of obtaining teachers, the necessary apparatus, the arrangement of museums, laboratories, botanic gardens,—on all these points and on more blank and total ignorance holds the minds of many masters, while others are puzzling them out with cruel



MAP OF THE PATH OF THE TOTAL SOLAR ECLIPSE IN DECEMBER NEXT

waste of force, destitute of traditions, ignorant of each others' experience, lacking central guidance.

For such guidance where are they to look, if not to the British Association? It includes men fitted for such a task beyond any others in the country, men individually of commanding reputation, representing severally the great towns, the Universities, the commercial centres. Is it too much to hope that a board of such men as these might assume, at the request and by the appointment of their brethren, the task of counsellers and supporters to the schools in the difficult task which lies before them? They might deliberate on the points which we have noticed, and draw up rules for a scientific course which all schools would adopt. They might send missionaries to schools newly entering upon their task, who should advise upon the many points no published rules could cover. They might suggest and accredit text-books, might bespeak and cheapen apparatus, might secure from Government facilities for obtaining specimens, for stocking gardens, for borrowing or renting instruments. Established more and more securely as the representatives and controllers of scientific education, they would see their power spread from the schools to the Universities, from the Universities throughout the country.

But we forbear. We have stated the difficulties which beset scientific education in our schools, we have hinted at means which may remove them. Our description is only too real, our project may be too chimerical. Be it so. The chimera of one age is often the truism of the next. Let us only call upon our friends at Edinburgh, before they separate for another year, to take this great subject into consideration, and to weigh its claims on their activity. Many a solitary teacher will be cheered, many a half-abandoned scheme will be preserved and furthered, if not by the certainty of their support, yet at any rate by the knowledge of their sympathy.

THE APPROACHING TOTAL SOLAR ECLIPSE

WE regret that we have, as yet, nothing very definite to announce in addition to what has been already stated with reference to the observations of the Total Solar Eclipse of the 12th of December next. We believe that an appeal is about to be made to Government, and if this be so, we may trust that anything that may be asked in the interests of Science will readily be granted by the Government. It is unfortunate that the Astronomer Royal's official position prevents his joining in the request, for his experience in connection with the large expenditure (10,000*l.* has already been voted) incurred by him for the approaching observation of the Transit of Venus, would be valuable in showing the necessity for the sum now required. This amounts only to a few hundreds in excess of the sum saved by the rigid economy practised by the Committee appointed to organise the arrangements connected with the late expedition.

We trust that the proposed arrangements will be brought before the British Association, in order that the influence of that important body may be made to bear upon this matter. We have recently shown the important results obtained by the late observations. It seems clear that the weather prospects for the approaching event are good, while recent calculations made by Mr. Hind show that the totality in Ceylon is much longer than had been at first imagined, amounting to as much as $2^m\ 11^s$ for Trincomalee, and therefore longer in the central line a few miles to the north. The accompanying map shows approximately the shadow path over India, and gives us good ground for congratulating ourselves that there are already in that country such observers as Tennant, Pogson, Herschel, Hennessy, and others, ready to occupy the best stations. The appeal made to Government includes funds for an expedition to Ceylon, under the charge of Mr. Lockyer, who has been requested by the Royal Astronomical Society to undertake spectroscopic observations there, while M. Janssen will probably take up his station in Java. We have already stated that a strong party from Melbourne and Sydney will observe in the north of Australia. All then is in order, provided our scientific leaders will put their shoulders to the wheel.

NOTES

THE American Association for the Advancement of Science, which meets a fortnight later than our own at Indianapolis, is modelled in most respects after the pattern of the parent institution, but presents some features which the managers of our own Association may do well to take into consideration. The arrangements with regard to the opening address, sectional proceedings, &c., are very similar, the following being the officers for the Indianapolis meeting:—President, Prof. Asa Gray, of Cambridge; Vice-president, Prof. George F. Barker, of New Haven; Permanent Secretary, Prof. Joseph Lovering, of Cambridge; General Secretary, Mr. F. W. Putman, of Salem; Treasurer, Mr. Wm. S. Vaux, of Philadelphia. Special convenience will be provided for microscopists in relation to the exhibition and care of any instruments or apparatus, a suite of rooms having been secured in the State House for their special use. It will be remembered that the same thing was attempted at the Liverpool meeting, but in rather a private and unacknowledged manner. Excursions are arranged to Terre Haute, a distance of seventy-three miles, including a visit to the celebrated block coal field and blast furnaces of Clay county, and to New Albany on the Ohio river, where there are a number of interesting manufactories, among them the only finishing plate-glass works in the United States. Special arrangements have been made as to terms for the accommodation of the members of the Association at hotels and boarding-houses, and it is expected that all the railroads will carry the visitors at half fares.

ALTHOUGH the Report of the Science and Art Department in the year 1870 is not yet published, we believe that the following chief results, taken from the *Times*, may be relied upon as accurate. The numbers who during 1870 have attended the schools, museums, and other institutions receiving Parliamentary aid, considerably exceed those of 1869. There is a very large increase in the number of persons receiving instruction in science applicable to industry, which has risen from 24,865 in 1869 to 34,283 in 1870, or upwards of 37 per cent. At the Royal School of Mines there were 17 regular and 124 occasional students, at the Royal College of Chemistry 121 students, at the Royal School of Naval Architecture there were 40, and at the Metallurgical Laboratory 24. The evening lectures at the Royal School of Mines were attended by 2,574 artisans, school teachers, and others; and 243 science teachers attended the special courses of lectures provided for their instruction. At the Royal College of Science, Ireland, there were 17 associate or regular students and 21 occasional students. The various courses of lectures delivered in connection with the department in Dublin were attended by 1,152 persons, and at the Evening Popular Lectures, which were given in the Edinburgh Museum of Science and Art during the session 1869-70, there was an attendance of 1,195. The total number of persons who received direct instruction as students or by means of lectures in connection with the Science and Art Department in 1870 was upwards of 254,000, showing an actual increase as compared with the number in the previous year of 67,000, or nearly 36 per cent., and an increase in the rate of progress of 8 per cent.; the numbers in 1869 having been nearly 28 per cent. higher than in 1868. The museums and collections under the superintendence of the department in London, Dublin, and Edinburgh, have been visited during the past year by 1,847,929 persons, showing an increase of 49,087 on the number in 1869. As we have said before, it is impossible to over-estimate the importance of the work which is being done.

THE correspondence between the Royal Commission on Scientific Instruction and the Advancement of Science and the Science and Art Department on the subject of the transfer of the School of Mines to South Kensington, has been presented to Parliament.

THE assertion made by a contemporary relative to the endowment at University College of a De Morgan professorship of mathematics, has given rise to the statement by Prof. T. Hewitt Key, to the effect that he now withdraws the proposal, not merely because it is said by the family to be at variance with the expressed wishes of the deceased, but more because it has been hinted that he has been unworthily "using Prof. de Morgan's name against such expressed wishes for the emolument of the college." The endowment of a mathematical chair still remains as an object to which his best energies will be applied.

Prof. Key points out that the doctrine is now practically admitted that for all chairs in a college of any pretensions a fixed salary is essential, and that the principle has been recognised in Owens College, Manchester, the Queen's College in Ireland, the Government School of Mines, and the new Indian College for Engineering.

AT an extraordinary General Meeting of the members of University College held on Saturday last, the Right Hon. Lord Belper, LL.D., F.R.S., was unanimously elected President of the College in the place of the late Mr. George Grote. At a session of the council, on the same day, the following appointments were made:—Mr. W. K. Clifford, Fellow of Trinity College, Cambridge, to be Professor of Applied Mathematics and Mechanics; Prof. H. C. Bastian, M.D., F.R.S., to be Physician to University College Hospital; Mr. Berkeley Hill, M.B., Mr. Christopher Heath, and Mr. Marcus Beck, M.S, M.B., to be teachers of Practical Surgery. The Sharpey Scholarship, recently established for the promotion of the study of Biological Science in the college, was conferred upon Mr. E. A. Schäfer.

UNIVERSITY COLLEGE, London, has recently been enriched by several valuable donations and legacies. Mr. Grote left 6,000*l.* for the endowment of a chair of Mental Philosophy, and Mr. James Yates legacies to be similarly applied for the teaching of Geology and Archæology. The treasurer of the College has given an endowment of 200*l.* for five years for the chair of Applied Mathematics, and the late Prof. Graves left a legacy to the College, without a rival of its kind, in the shape of a Mathematical Library, consisting of more than 10,000 volumes, besides some 500 pamphlets.

THE Senate of University College has appointed as its Professor of Hindustani, Kazi Shahabudeen Ibrahim. This gentleman is an accomplished scholar, and held a high position in our service in India. He was afterwards Dewar of the Rajah of Kutch, and is now resident for him in London. He also acts as hon. secretary of the East India Association. Kazi Shahabudeen being a thorough master of our own language, has a great advantage, and we may indeed observe that the progress of English studies in India ought greatly to promote those of the Indian languages in England. We can now get men having literary proficiency in their own languages, and that acquaintance which few but a native can attain, while they have the full power of communicating their knowledge to students in our colleges.

IN an article which will be found elsewhere, we allude to the approaching Total Eclipse of the Sun. On this subject we may refer to a very interesting letter which Mr. Hind has recently addressed to the *Times* on the next Total Solar Eclipse which will be visible in England. Our readers will gather that we shall have some time to wait. Mr. Hind tells us that in the year 1954, June 30, the zone of totality just touches the British Isles, and adds "to discover an eclipse that will be total in England, I have found it necessary to continue the calculations to nearly the close of the same century. Such an eclipse (according to my investigation) will not occur until the 11th of August, 1999, when the circumstances will be nearly as follows:—The central and total eclipse will enter upon the earth's surface in the southern part of the Gulf of Mexico; thence traversing the Atlantic, it meets the English coast at Padstow, in Cornwall, and crossing the south of Devon enters the Channel at Torquay (which will be the most favourable place for observation in this country), and passing over the Eddystone, reaches France about fifteen miles east of Dieppe. It will be central and total, with the sun on the meridian some twenty-five miles south-west of Pesth, and traversing Asia Minor, Persia (at Ispahan), &c., will finally leave the earth's surface in the Bay of Bengal. At Torquay the first contact of limbs, or commencement of the eclipse, occurs at 8.23 A.M. local mean time, and the last contact at 11.20 A.M. Totality begins at 10h. 0m. 43s., with the sun at an altitude of 48°, and continues 2m. 4s. At Plymouth the duration of total eclipse is 1m. 58s., at Weymouth 1m. 55s. The southern part of the Isle of Wight falls within the northern limit of totality according to my calculation." Further, on the subject of the last Total Solar Eclipse visible in London, which occurred on the 3rd of May, 1715, and was successfully observed in the metropolis and at many other English stations, Mr. Hind states, it is "necessary to look further back than the year 1140 for the total solar eclipse in London next preceding that of 1715. I greatly doubt if, excepting the eclipse of August 11, 1999, described above, there can be any total solar eclipse visible in England for two hundred and fifty years from the present time."

THE grounds of the Royal Observatory, Greenwich, are now being rapidly occupied with the temporary observatories and instruments which are to be used for the observations of the Transit of Venus in 1874. We could wish that equal energy were shown in arrangements for other observations which are quite as important as those in question.

THE following are the names of the successful candidates in the competition for the Whitworth Scholarships, 1871, in the Science and Art Department:—Edmund F. Mondy, Rotherhithe; Samuel Anglin, Manchester; George Smith, Birmingham; John Yeo, Portsmouth; Henry H. Greenhill, Portsea; John Armitage, Oldham; William Lee, London; Samuel A. Kirkby, Cambridge; Benjamin A. Raworth, Manchester; George C. V. Holmes, Sydenham.

THE French weekly scientific journal, *Les Mondes*, entered, with its last number, on its 25th volume.

THAT excellent body, the Smithsonian Institution, Washington, has recently issued its report for the year 1869, in addition to which we have, under the same cover, Bertrand's paper on the Life and Works of Kepler, Arago's Eulogy on Thomas Young, Memoirs of Auguste Bravais and von Martius, a paper on the Chemistry of the Earth by Sterry Hunt, another on the Electrical Currents of the Earth by Matteucci, another on the Phenomena of Flight by Marey, and so on,—we really have not space to name all the titles,—and we have already said enough to indicate the extreme value of the volume. Among recent memoirs and papers published by the same Institution, we may mention a paper on the magnetic survey of Pennsylvania by Dr. Bache, on the Gleddon mummy case by Dr. Pickering, and on the phenomena and laws of aurora borealis by Loomis.

WE are glad to see that in the list of Civil Service Pensions just issued, the claims of Science have been recognised by the grant of 100*l.* to Mr. Charles Tilston Beke, in consideration of his geographical researches, and especially of the value of his explorations in Abyssinia; and 150*l.* to Mrs. Emily Coles, widow of Captain Cowper Phipps Coles, in consideration of her husband's services as inventor of the turret ship system.

THE *Revue Scientifique* publishes an account of the chemical investigations and works of the late Prof. Payen, the most important of which are as follows:—In 1824 he made his first investigations on the value of manures; in 1830 he presented to the Society of Agriculture a paper on the means of utilising all the parts of dead animals in the country; in 1836 he read a memoir on the elementary composition of starch in different plants; in 1837 he established the composition of dextrine from its definite combinations with oxide of lead and baryta; and in the same year he read a paper on the distribution of nitrogenous matters in the organs of vegetables; in 1838 he presented a very important memoir on the composition of woody tissue, and pointing out the distinction between cellulose and starch: in 1841 he prepared a memoir, in conjunction with M. Boussingault, on the relative value of different manures; in 1847 a paper on sugar in beet-root; in 1852 two very complete memoirs on caoutchouc and gutta-percha, their chemical composition and different characters; in 1859 another paper on starch and cellulose; in 1861 one on dextrine and glucose; in 1867 a paper on the constitution and structure of woody tissue; besides a large number of others on economical and vegetable chemistry. As separate works, Prof. Payen published a compendium of theoretical and practical agriculture, a compendium of industrial chemistry, a work on the diseases of the potato, beet, corn, and wine, a treatise on the distillation of beet, a work on alimentary substances, and a report on the vegetable and animal substances made to the French Committee of the Jury of the International Exhibition in London. He was appointed Professor of Industrial Chemistry at the Central School in 1830, and at the Conservatoire des Arts et Métiers in 1839, and was elected member of the Institute in 1842.

WE are informed by Dr. Edward L. Moss, R.N., that within the last few days he has obtained several specimens of *Appendicularia furcata* and *acrocerca* in the incoming tide off the east coast of Portland. They have in every instance been captured in their "Haus," or have formed it shortly after capture, and have remained in it as long they were left undisturbed. These rare and interesting visitors to our tidal waters were accompanied by oceanic diatoms which Dr. Moss had never before seen near the English coast.

THE sixth annual meeting of the Quekett Microscopical Club was held on Friday evening last at University College. By the annual report of the committee read, it appeared that the number of the members now amounts to 550. The president, Dr. L. S. Beale, F.R.S., gave the usual presidential address. At the election of officers which followed, Dr. L. S. Beale was elected president for the year 1871-72; for vice-presidents, Dr. Robert Braithwaite, F.L.S., Mr. Arthur E. Durham, F.R.C.S., Mr. Charles J. Leaf, F.R.M.S., Mr. Henry Lee, F.L.S.; for four members of committee, Messrs. W. H. Golding, Thomas Greenish, E. Marks, and F. Oxley; for treasurer, Mr. Robert Hardwicke, F.L.S.; hon. secretary for foreign correspondence, Mr. M. C. Cooke, M.A.; hon. secretary, Mr. T. Charters White.

THE Royal Archæological Institute has just held its annual meeting at Cardiff, under the presidency of the Marquis of Bute, who, in his inaugural address, dwelt on the many objects of archæological interest in which South Wales abounds, especially as the locality of some of the best known incidents of the Arthurian romances. The historical section was presided over by Mr. G. A. Freeman, who delivered a very interesting address on the early ethnology of South Wales. A long excursion was undertaken by the members into Monmouthshire, the principal bjects of interest being Caldicot Castle, Caerwent (the Roman Venta Silurium) and Chepstow.

THE annual meeting of the Institution of Mechanical Engineers was held last week at Middlesborough, Mr. John Ramsbottom, of Crewe, being president of the meeting. Papers were read by Mr. William Crossley, of the Askham Ironworks, Lancashire, on the manufacture of hæmatite iron; by Mr. J. Lowthian Bell, upon the preliminary treatment of materials used in the blast furnace; by Mr. Hill, on an improved compound cylinder blowing engines recently erected at the Lackenby Iron Works, Middlesbrough; a description of the geological features of Cleveland by Mr. John Jones, secretary to the iron trade of the district; by Mr. John A. Haswell, of Gateshead, describing the break drums and the mode of working at the Ingleby incline on the Rosedale branch of the North Eastern Railway; by Mr. Jeremiah Head, of Middlesborough, on a simple construction of steam-engine governor, having a close approximation to perfect action; and by Mr. Charles Cochrane, of Middlesborough, on steam boilers with small water-space and Roots' tube boiler. The many objects of interest in the neighbourhood were also visited by the members.

THE BRITISH ASSOCIATION MEETING AT EDINBURGH

EDINBURGH, *Wednesday Morning*

A VERY important point in the peregrination of the British Association lies in the fact that the men of science are now assembled in one of the foci of commercial enterprise, now in an old centre of learning, and now in a locality which, although coming under neither of these heads, yet gives large scope for benefiting the surrounding region. That the Association should meet at Edinburgh at this present juncture is extremely fortunate. In the first place Science was largely taught at Edinburgh by the aid of State-endowed professors before either of our old English Universities thought it worth while to investigate with any earnestness those branches of natural knowledge which are now recognised as not only the necessary accompaniment of a liberal education, but as the foundation of the nation's greatness. In the second place, we learn from the Edinburgh newspapers that the scientific mind of the metropolis of the North has been recently stirred on the subject of the importance of scientific research, and has addressed a memorial to the Royal Commission now sitting, urging that the point shall be strongly taken up.

It may be interesting to mention that this is the third time that the British Association has met at Edinburgh. The first time was in 1834, under the presidency of Sir Thomas Brisbane; the second in 1850, when Sir David Brewster occupied the chair. Already more than 1,300 members have entered their names, a larger number than were present at the last Edinburgh meeting.

The ample accommodation furnished by the Scotch capital is being admirably utilised by the local organisers. The Reception Room is in Parliament House; the sections meet in the University Buildings. In addition to the assemblage of our own *savans*, the following distinguished scientific foreigners are either in Edinburgh or are expected in the course of the meeting:—The Emperor of Brazil; Dr. Janssen, of Paris; Dr. Buys Ballot, of Utrecht; Prof. v. Baumhauer, of Haarlem; Prof. Van Beneden, of Louvain; Dr. D. Bierens de Haan, of Leyden; Dr. Boogaard; Dr. Colding, of Copenhagen; Prof. Delffs, of Heidelberg; Baron Desiderius; Baron Roland Eö oös, of Pesth; Don Asturo de Marcoastin, of Madrid; Prof. Margo, of Pesth; l'Abbé Moigno, of Paris; Prof. Morren, of Liége; Prof. Szabó, of Pesth; Prof. Zenger, of Prague; Dr. Youmans, of New York. Of these Dr. Janssen, Prof. Van Beneden, Dr. Buys Ballot, Profs. Szabó and Zenger, and Dr. Colding, have already arrived. The University of Edinburgh has taken the opportunity of conferring the honorary degree of LL.D. on the following distinguished men of science:- Dr. Gassiot, Prof. Sylvester, Prof. Stokes, Prof. Challis, Dr. Huggins, Dr. Allen Thomson, Dr. Janssen, Prof. Van Beneden, Dr. Colding, Mr. Spottiswoode, Dr. Carpenter, Prof. Andrews of Belfast, and Dr. Paget of Cambridge.

We are enabled, through the courtesy of the officers of the Association, to give in our present number full reports of the president's inaugural address, and of the opening addresses in Sections A, B, and C. In Prof. Geikie's address we have a suitable and altogether to be commended innovation in the shape of an account of the local geology of the neighbourhood, which has been printed separately, and issued with an admirably clear map.

Col. Yule has been appointed president of Section E in the place of the late Dr. Johnston. Geological excursions are projected to East Lothian and the coast of Berwickshire, the latter under the guidance of Prof. Geikie; a botanical excursion to the fertile collecting ground of Ben Ledi, in which Prof. Balfour will take part; a dredging expedition in the Frith of Forth; and visits for antiquarians and the lovers of the picturesque to Melrose, Dryburgh, Abbotsford, and Rosslyn. With this tempting bill of fare, if the weather only proves moderately propitious, the meeting of the British Association in Edinburgh must be an occasion to look back upon with pleasure by all who are fortunate enough to be able to take part in its proceedings.

Inaugural Address of Sir William Thomson, LL.D., F.R.S., President

For the third time of its forty years' history the British Association is assembled in the metropolis of Scotland. The origin of the Association is connected with Edinburgh in undying memory through the honoured names of Robison, Brewster, Forbes, and Johnston.

In this place, from this chair, twenty-one years ago, Sir David Brewster said:—"On the return of the British Association to the metropolis of Scotland, I am naturally reminded of the small band of pilgrims who carried the seeds of this Institution into the more genial soil of our sister land." . . . . . "Sir John Robison, Prof. Johnston, and Prof. J. D. Forbes were the earliest friends and promoters of the British Association. They went to York to assist in its establishment, and they found there the very men who were qualified to foster and organise it. The Rev. Mr. Vernon Harcourt, whose name cannot be mentioned here without gratitude, had provided laws for its government, and, along with Mr. Phillips, the oldest and most valuable of our office bearers, had made all those arrangements by which its success was ensured. Headed by Sir Roderick Murchison, one of the very earliest and most active advocates of the Association, there assembled at York about 200 of the friends of science."

The statement I have read contains no allusion to the real origin of the British Association. This blank in my predecessor's historical sketch I am able to fill in from words written by himself twenty years earlier. Through the kindness of Prof. Phillips I am enabled to read to you part of a letter to him at York, written by David Brewster from Allerly by Melrose, on the 23rd of February, 1831:—

"Dear Sir,—I have taken the liberty of writing you on a subject of considerable importance. It is proposed to establish a British Association of men of science similar to that which has existed for eight years in Germany, and is now patronised by the most powerful Sovereigns of that part of Europe. The arrangements for the first meeting are in progress; and it is contemplated that it shall be held in York, as the most central city for the three kingdoms. My object in writing you at present is to beg that you would ascertain if York will furnish the accommodation necessary for so large a meeting (which may perhaps consist of above 100 individuals), if the Philosophical Society would enter zealously into the plan, and if the Mayor and influential persons in the town and in the vicinity would be likely to promote its objects. The principal object of the Society would be to make the cultivators of science acquainted with each other, to stimulate one another to new exertions, and to bring the objects of science more before the public eye, and to take measures for advancing its interests and accelerating its progress."

Of the little band of four pilgrims from Scotland to York, not one now survives. Of the seven first associates one more has gone over to the majority since the Association last met. Vernon Harcourt is no longer with us; but his influence remains, a beneficent and surely therefore never dying influence. He was a geologist and chemist, a large-hearted lover of science, and an unwearied worker for its advancement. Brewster was the founder of the British Association; Vernon Harcourt was its lawgiver. His code remains to this day the law of the Association.

On the 11th of May last Sir John Herschel died in the eightieth year of his age. The name of Herschel is a household word throughout Great Britain and Ireland—yes, and through the whole civilised world. We of this generation have, from our lessons of childhood upwards, learned to see in Herschel, father and son, a *præsidium et dulce decus* of the precious treasure of British scientific fame. When geography, astronomy, and the use of the globes were still taught, even to poor children, as a pleasant and profitable sequel to "reading, writing, and arithmetic," which of us did not revere the great telescope of Sir William Herschel (one of the hundred wonders of the world), and learn with delight, directly or indirectly from the charming pages of Sir John Herschel's book, about the sun and his spots, and the fiery tornadoes sweeping over his surface, and about the planets, and Jupiter's belts, and Saturn's rings, and the fixed stars with their proper motions, and the double stars, and coloured stars, and the nebulæ discovered by the great telescope? Of Sir John Herschel it may indeed be said, *nil tetigit quod non ornavit.*

A monument to Faraday and a monument to Herschel, Britain must have. The nation will not be satisfied with any thing, however splendid, done by private subscription. A national monument, the more humble in point of expense the better, is required to satisfy that honourable pride with which a high-spirited nation cherishes the memory of its great men. But for the glory of Faraday or the glory of Herschel, is a monument wanted? No!

> What needs my Shakespeare for his honoured bones
> The labour of an age in piled stones?
> Or that his hallowed relics should be hid
> Under a star-ypointing pyramid?
> Dear son of memory, great heir of fame,
> What need'st thou such weak witness of thy name?
> Thou, in our wonder and astonishment,
> Hast built thyself a live-long monument.
> . . . .
> And, so sepulchred, in such pomp dost lie,
> That kings for such a tomb would wish to die.

With regard to Sir John Herschel's scientific work, on the present occasion I can but refer briefly to a few points which seem to me salient in his physical and mathematical writings. First, I remark that he has put forward, most instructively and profitably to his readers, the general theory of periodicity in dynamics, and has urged the practical utilising of it, especially in meteorology, by the harmonic analysis. It is purely by an application of this principle and practical method, that the British Association's Committee on Tides has for the last four years been, and still is, working towards the solution of the grand problem proposed forty-eight years ago by Thomas Young in the following words:—

"There is, indeed, little doubt that if we were provided with a sufficiently correct series of minutely accurate observations on the Tides, made not merely with a view to the times of low and high water only, but rather to the heights at the intermediate times, we might form by degrees, with the assistance of the theory contained in this article* only, almost as perfect a set of tables for the motions of the ocean as we have already obtained for those of the celestial bodies, which are the more immediate objects of the attention of the practical astronomer."

Sir John Herschel's discovery of a right or left-handed asymmetry in the outward form of crystals, such as quartz, which in their inner molecular structure possess the helicoidal rotational property in reference to the plane of polarisation of light, is one of the notable points of meeting between Natural History and Natural Philosophy. His observations on "epipolic dispersion" gave Stokes the clue by which he was led to his great discovery of the change of periodic time experienced by light in falling on certain substances and being dispersively reflected from them. In respect to pure mathematics Sir John Herschel did more, I believe, than any other man to introduce into Britain the powerful methods and the valuable notation of modern analysis. A remarkable mode of symbolism had freshly appeared, I believe, in the works of Laplace, and possibly of other French mathematicians; it certainly appeared in Fourier, but whether before or after Herschel's work I cannot say. With the French writers, however, this was rather a short method of writing formulæ than the analytical engine which it became in the hands of Herschel and British followers, especially Sylvester and Gregory (competitors with Green in the Cambridge Mathematical Tripos struggle of 1837) and Boole and Cayley. This method was greatly advanced by Gregory, who first gave to its working-power a secure and philosophical foundation, and so prepared the way for the marvellous extension it has received from Boole, Sylvester,

* Young's; written in 1823 for the Supplement to the "Encyclopædia Britannica."

and Cayley, according to which symbols of operation become the subjects not merely of algebraic combination, but of differentiations and integrations, as if they were symbols expressing values of varying quantities. An even more marvellous development of this same idea of the separation of symbols (according to which Gregory separated the algebraic signs + and − from other symbols or quantities to be characterised by them, and dealt with them according to the laws of algebraic combinations) received from Hamilton a most astonishing generalisation, by the invention actually of new laws of combination, and led him to his famous "Quaternions," of which he gave his earliest exposition to the Mathematical and Physical Section of this Association, at its meeting in Cambridge in the year 1845. Tait has taken up the subject of quaternions ably and zealously, and has carried it into physical science with a faith, shared by some of the most thoughtful mathematical naturalists of the day, that it is destined to become an engine of perhaps hitherto unimagined power for investigating and expressing results in Natural Philosophy. Of Herschel's gigantic work in astronomical observation I need say nothing. Doubtless a careful account of it will be given in the "Proceedings of the Royal Society of London" for the next anniversary meeting.

In the past year another representative man of British science is gone. Mathematics has had no steadier supporter for half a century than De Morgan. His great book on the differential calculus was, for the mathematical student of thirty years ago, a highly-prized repository of all the best things that could be brought together under that title. I do not believe it is less valuable now; and if it is less valued, may this not be because it is too good for examination purposes, and because the modern student, labouring to win marks in the struggle for existence, must not suffer himself to be beguiled from the stern path of duty by any attractive beauties in the subject of his study?

One of the most valuable services to science which the British Association has performed has been the establishment, and the twenty-nine years' maintenance, of its Observatory. The Royal Meteorological Observatory of Kew was built originally for a Sovereign of England who was a zealous amateur of astronomy. George the Third used continually to repair to it when any celestial phenomenon of peculiar interest was to be seen; and a manuscript book still exists filled with observations written into it by his own hand. After the building had been many years unused, it was granted, in the year 1842, by the Commissioners of Her Majesty's Woods and Forests, on application of Sir Edward Sabine, for the purpose of continuing observations (from which he had already deduced important results) regarding the vibration of a pendulum in various gases, and for the purpose of promoting pendulum observations in all parts of the world. The Government granted only the building—no funds for carrying on the work to be done in it. The Royal Society was unable to undertake the maintenance of such an observatory; but, happily for science, the zeal of individual Fellows of the Royal Society and members of the British Association gave the initial impulse, supplied the necessary initial funds, and recommended their new institution successfully to the fostering care of the British Association. The work of the Kew Observatory has, from the commencement, been conducted under the direction of a Committee of the British Association; and annual grants from the funds of the Association have been made towards defraying its expenses up to the present time. To the initial object of pendulum research was added continuous observation of the phenomena of meteorology and terrestrial magnetism, and the construction and verification of thermometers, barometers, and magnetometers designed for accurate measurement. The magnificent services which it has rendered to science are so well known that any statement of them which I could attempt on the present occasion would be superfluous. Their value is due in a great measure to the indefatigable zeal and the great ability of two Scotchmen, both from Edinburgh, who successively held the office of Superintendent of the Observatory of the British Association—Mr. Welsh for nine years, until his death in 1859, and Dr. Balfour Stewart from then until the present time. *Fruits of their labours* are to be found all through our volumes of Reports for these twenty-one years.

The institution now enters on a new stage of its existence. The noble liberality of a private benefactor, one who has laboured for its welfare with self-sacrificing devotion unintermittingly from within a few years of its creation, has given it a permanent independence, under the general management of a Committee of the Royal Society. Mr. Gassiot's gift of 10,000*l*. secures the continuance at Kew of the regular operation of the self-recording instruments for observing the phenomena of terrestrial magnetism and meteorology, without the necessity for further support from the British Association.

The success of the Kew Magnetic and Meteorological Observatory affords an example of the great gain to be earned for science by the foundation of physical observatories and laboratories for experimental research, to be conducted by qualified persons, whose duties should be, not teaching, but experimenting. Whether we look to the honour of England, as a nation which ought always to be the foremost in promoting physical science, or to those vast economical advantages which must accrue from such establishments, we cannot but feel that experimental research ought to be made with us an object of national concern, and not left, as hitherto, exclusively to the private enterprise of self-sacrificing amateurs, and the necessarily inconsecutive action of our present Governmental Departments and of casual Committees. The Council of the Royal Society of Edinburgh has moved for this object in a memorial presented by them to the Royal Commission on Scientific Education and the Advancement of Science. The Continent of Europe is referred to for an example to be followed with advantage in this country, in the following words:—

"On the Continent there exist certain institutions, fitted with instruments, apparatus, chemicals, and other appliances, which are meant to be, and which are made, available to men of science, to enable them, at a moderate cost, to pursue original researches."

This statement is fully corroborated by information, on good authority, which I received from Germany, to the effect that in Prussia "every university, every polytechnical academy, every industrial school (Realschule and Gewerbeschule), most of the grammar-schools, in a word, nearly all the schools superior in rank to the elementary schools of the common people, are supplied with chemical laboratories and a collection of philosophical instruments and apparatus, access to which is most liberally granted by the directors of those schools, or the teachers of the respective disciplines, to any person qualified, for scientific experiments. In consequence, though there exist no particular institutions like those mentioned in the memorial, there will scarcely be found a town exceeding in number 5,000 inhabitants but offers the possibility of scientific explorations at no other cost than reimbursement of the the expense for the materials wasted in the experiments."

Further, with reference to a remark in the Memorial to the effect that, in respect to the promotion of science, the British Government confines its action almost exclusively to scientific instruction, and fatally neglects the advancement of science, my informant tells me that, in Germany, "professors, preceptors, and teachers of secondary schools are engaged on account of their skilfulness in teaching; but professors of universities are never engaged unless they have already proved, by their own investigations, that they are to be relied upon for the advancement of science. Therefore every shilling spent for instruction in universities is at the same time profitable to the advancement of science."

The physical laboratories which have grown up in the Universities of Glasgow and Edinburgh, and in Owens College, Manchester, show the want felt of Colleges of Research; but they go but infinitesimally towards supplying it, being absolutely destitute of means, material or personal, for advancing science except at the expense of volunteers, or securing that volunteers shall be found to continue even such little work as at present is carried on.

The whole of Andrews's splendid work in Queen's College, Belfast, has been done under great difficulties and disadvantages, and at great personal sacrifices; and up to the present time there is not a student's physical laboratory in any one of the Queen's Colleges in Ireland—a want which surely ought not to remain unsupplied. Each of these institutions (the four Scotch Universities, the three Queen's Colleges, and Owens College, Manchester) requires two professors of Natural philosophy—one who shall be responsible for the teaching, the other for the advancement of science by experiment. The University of Oxford has already established a physical laboratory. The munificence of its Chancellor is about to supply the University of Cambridge with a splendid laboratory, to be constructed under the eye of Prof. Clerk Maxwell. On this subject I shall say no more at present, but simply read a sentence which was spoken by Lord Milton in the first Presidential Address to the British Association, when it met at York in the year 1831 :—"In addition to other more direct benefits, these meetings [of the British Association]. I hope, will be the means of impressing on the Government the conviction, that the love of scientific pursuits, and the means of pursuing them, are not confined to the metropolis; and I hope that when the Government is fully impressed with the knowledge of the great desire entertained to promote science in every part of the empire, they will see the necessity of affording it due encouragement, and of giving every proper stimulus to its advancement."

Besides abstracts of papers read, and discussions held, before the Sections, the annual Reports of the British Association contain a large mass of valuable matter of another class. It was an early practice of the Association, a practice that might well be further developed, to call occasionally for a special report on some particular branch of science from a man eminently qualified for the task. The reports received in compliance with these invitations have all done good service in their time, and they remain permanently useful as landmarks in the history of science. Some of them have led to vast practical results; others of a more abstract character are valuable to this day as powerful and instructive condensations and expositions of the branches of science to which they relate. I cannot better illustrate the two kinds of efficiency realised in this department of the Association's work than by referring to Cayley's Report on Abstract Dynamics,* and Sabine's Report on Terrestrial Magnetism † (1838).

To the great value of the former, personal experience of benefit received enables me, and gratitude impels me, to testify. In a few pages full of precious matter, the generalised dynamical equations of Lagrange, the great principle evolved from Maupertuis' "least action" by Hamilton, and the later developments and applications of the Hamiltonian principle by other authors are described by Cayley so suggestively that the reading of thousands of quarto pages of papers scattered through the Transactions of the various learned societies of Europe is rendered superfluous for any one who desires only the essence of these investigations, with no more of detail than is necessary for a thorough and practical understanding of the subject.

Sabine's Report of 1838 concludes with the following sentence: "Viewed in itself and its various relations, the magnetism of the earth cannot be counted less than one of the most important branches of the physical history of the planet we inhabit; and we may feel quite assured that the completion of our knowledge of its distribution on the surface of the earth would be regarded by our contemporaries and by posterity as a fitting enterprise of a maritime people, and a worthy achievement of a nation which has ever sought to rank foremost in every arduous and honourable undertaking." An immediate result of his Report was that the enterprise which it proposed was recommended to the Government by a joint Committee of the British Association and the Royal Society with such success, that Capt. James Ross was sent in command of the *Erebus* and *Terror* to make a magnetic survey of the Antarctic regions, and to plant on his way three Magnetical and Meteorological Observatories, at St. Helena, the Cape, and Van Diemen's Land. A vast mass of precious observations, made chiefly on board ship, were brought home from this expedition. To deduce the desired results from them, it was necessary to eliminate the disturbance produced by the ship's magnetism; and Sabine asked his friend Archibald Smith to work out from Poisson's mathematical theory, then the only available guide, the formulæ required for the purpose. This voluntary task Smith executed skilfully and successfully. It was the beginning of a series of labours carried on with most remarkable practical tact, with thorough analytical skill, and with a rare extreme of disinterestedness, in the intervals of an arduous profession, for the purpose of perfecting and simplifying the correction of the mariner's compass—a problem which had become one of vital importance for navigation, on account of the introduction of iron ships. Edition after edition of the "Admiralty Compass Manual" has been produced by the able superintendent of the Compass Department, Captain Evans, containing chapters of mathematical investigation and formulæ by Smith, on which depend wholly the practical analysis of compass-observations, and rules for the safe use of the compass in navigation. I firmly believe that it is to the thoroughly scientific method thus adopted by the Admiralty, that no iron ship of Her Majesty's Navy has ever been lost through errors of the compass. The "British Admiralty Compass Manual" is adopted as a guide by all the navies of the world. It has been translated into Russian, German, and Portuguese; and it is at present being translated into French. The British Association may be gratified to know that the possibility of navigating ironclad war-ships with safety depends on application of scientific principles given to the world by three mathematicians, Poisson, Airy, and Archibald Smith.

Returning to the science of terrestrial magnetism we find in the Reports of early years of the British Association ample evidence of its diligent cultivation. Many of the chief scientific men of the day from England, Scotland, and Ireland, found a strong attraction to the Association in the facilities which it afforded to them for co-operating in their work on this subject. Lloyd, Phillips, Fox, Ross, and Sabine made magnetic observations all over Great Britain; and their results, collected by Sabine, gave for the first time an accurate and complete survey of terrestrial magnetism over the area of this island. I am informed by Prof. Phillips that, in the beginning of the Association, Herschel, though a "sincere well-wisher," felt doubts as to the general utility and probable success of the plan and purpose proposed; but his zeal for terrestrial magnetism brought him from being merely a sincere well-wisher to join actively and cordially in the work of the Association. "In 1838 he began to give effectual aid in the great question of magnetical observatories, and was indeed foremost among the supporters of that which is really Sabine's great work. At intervals, until about 1858, Herschel continued to give effectual aid." Sabine has carried on his great work without intermission to the present day; thirty years ago he gave to Gauss a large part of the data required for working out the spherical harmonic analysis for the altered state of terrestrial magnetism over the whole earth. A recalculation of the harmonic analysis for the altered state of terrestrial magnetism of the present time has been undertaken by Adams. He writes to me that he has "already begun some of the introductory work, so as to be ready when Sir Edward Sabine's Tables of the Values of the Magnetic Elements deduced from observation are completed, at once to make use of them," and that he intends to take into account terms of at least one order beyond those included by Gauss. The form in which the requisite data are to be presented to him is a magnetic Chart of the whole surface of the globe. Materials from scientific travellers of all nations, from our home magnetic observatories, from the magnetic observatories of St. Helena, the Cape, Van Diemen's Land, and Toronto, and from the scientific observatories of other countries, have been brought together by Sabine. Silently, day after day, night after night, for a quarter of a century he has toiled with one constant assistant always by his side to reduce these observations and prepare or the great work. At this moment, while we are here assembled, I believe that, in their quiet summer retirement in Wales, Sir Edward and Lady Sabine are at work on the Magnetic Chart of the world. If two years of life and health are granted to them, science will be provided with a key which must powerfully conduce to the ultimate opening up of one of the most refractory enigmas of cosmical physics, the cause of terrestrial magnetism.

To give any sketch, however slight, of scientific investigation performed during the past year would, even if I were competent for the task, far exceed the limits within which I am confined on the present occasion. A detailed account of work done and knowledge gained in science Britain ought to have every year. The Journal of the Chemical Society and the Zoological Record do excellent service by giving abstracts of all papers published in their departments. The admirable example afforded by the German "Fortschritte" and "Jahresbericht" is before us; but hitherto, so far as I know, no attempt has been made to follow it in Britain. It is true that several of the annual volumes of the Jahresbericht were translated; but a translation, published necessarily at a considerable interval of time after the original, cannot supply the want. An independent British publication is for many obvious reasons desirable. The two publications, in German and English, would, both by their differences and by their agreements, illustrate the progress of science more correctly and usefully than any single work could do, even if appearing simultaneously in the two languages. It seems to me that to promote the establishment of a British Year Book of Science is an object to which the powerful action of the British Association would be thoroughly appropriate.

* Report on the Recent Progress of Theoretical Dynamics, by A. Cayley, (Report of the British Association 1857, p. 1).

† Report on the Variations of the Magnetic Intensity observed at different points of the Earth's Surface, by Major Sabine, F.R.S. (forming part of the 7th Report of the British Association).

In referring to recent advances in several branches of science, I simply choose some of those which have struck me as most notable.

Accurate and minute measurement seems to the non-scientific imagination a less lofty and dignified work than looking for something new. But nearly all the grandest discoveries of science have been but the rewards of accurate measurement and patient long-continued labour in the minute sifting of numerical results. The popular idea of Newton's grandest discovery is that the theory of gravitation flashed into his mind, and so the discovery was made. It was by a long train of mathematical calculation, founded on results accumulated through prodigious toil of practical astronomers, that Newton first demonstrated the forces urging the planets towards the sun, determined the magnitude of those forces, and discovered that a force following the same law of variation with distance urges the moon towards the earth. *Then* first, we may suppose, came to him the idea of the universality of gravitation; but when he attempted to compare the magnitude of the force on the moon with the magnitude of the force of gravitation of a heavy body of equal mass at the earth's surface, he did not find the agreement which the law he was discovering required. Not for years after would he publish his discovery as made. It is recounted that, being present at a meeting of the Royal Society, he heard a paper read, describing geodesic measurement by Picard, which led to a serious correction of the previously accepted estimate of the earth's radius. This was what Newton required. He went home with the result, and commenced his calculations, but felt so much agitated that he handed over the arithmetical work to a friend; then (and not when, sitting in a garden, he saw an apple fall) did he ascertain that gravitation keeps the moon in her orbit.

Faraday's discovery of specific inductive capacity, which inaugurated the new philosophy, tending to discard action at a distance, was the result of minute and accurate measurement of forces.

Joule's discovery of thermo-dynamic law through the regions of electro-chemistry, electro-magnetism, and elasticity of gases was based on a delicacy of thermometry which seemed simply impossible to some of the most distinguished chemists of the day.

Andrews's discovery of the continuity between the gaseous and liquid states was worked out by many years of laborious and minute measurement of phenomena scarcely sensible to the naked eye.

Great service has been done to science by the British Association in promoting accurate measurement in various subjects. The origin of exact science in terrestrial magnetism is traceable to Gauss's invention of methods of finding the magnetic intensity in absolute measure. I have spoken of the great work done by the British Association in carrying out the application of this invention in all parts of the world. Gauss's colleague in the German Magnetic Union, Weber, extended the practice of absolute measurement to electric currents, the resistance of an electric conductor, and the electromotive force of a galvanic element. He showed the relation between electrostatic and electromagnetic units for absolute measurement, and made the beautiful discovery that résistance, in absolute electromagnetic measure, and the reciprocal of resistance, or, as we call it, "conducting power," in electrostatic measure, are each of them a velocity. He made an elaborate and difficult series of experiments to measure the velocity which is equal to the conducting power, in electrostatic measure, and at the same time to the resistance in electromagnetic measure, in one and the same conductor. Maxwell, in making the first advance along a road of which Faraday was the pioneer, discovered that this velocity is physically related to the velocity of light, and that, on a certain hypothesis regarding the elastic medium concerned, it may be exactly equal to the velocity of light. Weber's measurement verifies approximately this equality, and stands in science *monumentum ære perennius*, celebrated as having suggested this most grand theory, and as having afforded the first quantitative test of the recondite properties of matter on which the relations between electricity and light depend. A re-measurement of Weber's critical velocity on a new plan by Maxwell himself, and the important correction of the velocity of light by Foucault's laboratory experiments, verified by astronomical observation, seem to show a still closer agreement. The most accurate possible determination of Weber's critical velocity is just now a primary object of the Association's Committee on Electric Measurement; and it is at present premature to speculate as to the closeness of the agreement between that velocity and the velocity of light. This leads me to remark how much science, even in its most lofty speculations, gains in return for benefits conferred by its application to promote the social and material welfare of man. Those who perilled and lost their money in the original Atlantic Telegraph were impelled and supported by a sense of the grandeur of their enterprise, and of the world-wide benefits which must flow from its success; they were at the same time not unmoved by the beauty of the scientific problem directly presented to them; but they little thought that it was to be immediately, through their work, that the scientific world was to be instructed in a long-neglected and discredited fundamental electric discovery of Faraday's, or that, again, when the assistance of the British Association was invoked to supply their electricians with methods for absolute measurement (which they found necessary to secure the best economical return for their expenditure, and to obviate and detect those faults in their electric material which had led to disaster), they were laying the foundation for accurate electric measurement in every scientific laboratory in the world, and initiating a train of investigation which now sends up branches into the loftiest regions and subtlest ether of natural philosophy. Long may the British Association continue a bond of union, and a medium for the interchange of good offices between science and the world.

The greatest achievement yet made in molecular theory of the properties of Matter is the Kinetic theory of Gases, shadowed forth by Lucretius, definitely stated by Daniel Bernoulli, largely developed by Herapath, made a reality by Joule, and worked out to its present advanced state by Clausius and Maxwell. Joule, from his dynamical equivalent of heat, and his experiments upon the heat produced by the condensation of gas was able to estimate the average velocity of the ultimate molecules or atoms composing it. His estimate for hydrogen was 6,225 feet per second at temperature 60° Fahr., and 6,055 feet per second at the freezing-point. Clausius took fully into account the impacts of molecules on one another, and the kinetic energy of *relative* motions of the matter constituting an individual atom. He investigated the relation between their diameters, the number in a given space, and the mean length of path from impact to impact, and so gave the foundation for estimates of the absolute dimensions of atoms, to which I shall refer later. He explained the slowness of gaseous diffusion by the mutual impacts of the atoms to which I shall refer later. He explained the slowness of gaseous diffusion by the mutual impacts of the atoms, and laid a secure foundation for a complete theory of the diffusion of fluids, previously a most refractory enigma. The deeply penetrating genius of Maxwell brought in viscosity and thermal conductivity, and thus completed the dynamical explanation of all the known properties of gases, except their electric resistance and brittleness to electric force.

No such comprehensive molecular theory had ever been even imagined before the nineteenth century. Definite and complete in its area as it is, it is but a well-drawn part of a great chart, in which all physical science will be represented with every property of matter shown in dynamical relation to the whole. The prospect we now have of an early completion of this chart is based on the assumption of atoms. But there can be no permanent satisfaction to the mind in explaining heat, light, elasticity, diffusion, electricity, and magnetism, in gases, liquids, and solids, and describing precisely the relations of these different states of matter to one another by statistics of great numbers of atoms, when the properties of the atom itself are simply assumed. When the theory, of which we have the first instalment in Clausius and Maxwell's work, is complete, we are but brought face to face with a superlatively grand question, whati is the inner mechanism of the atom?

In the answer to this question we must find the explanation not only of the atomic elasticity, by which the atom is a chronometric vibrator according to Stokes's discovery, but of chemical affinity and of the differences of quality of different chemical elements, at present a mere mystery in science. Helmholtz's exquisite theory of vortex-motion in an incompressible frictionless liquid has been suggested as a finger-post, pointing a way which may possibly lead to a full understanding of the properties of atoms, carrying out the grand conception of Lucretius, who "admits no subtle ethers, no variety of elements with fiery, or watery, or light, or heavy principles; nor supposes light to be one thing, fire another, electricity a fluid, magnetism a vital principle, but treats all phenomena as mere properties or accidents of simple matter." This statement I take from an admirable paper on the atomic theory of Lucretius, which appeared in the *North British Review* for March 1868, containing a most interesting and instructive summary of ancient and modern doctrine regarding atoms. Allow me to read from that article one other short passage finely describing the present aspect of atomic theory:—"The existence of the chemical atom, already quite a complex little world, seems very probable; and the description of the Lucretian atom is wonderfully applicable to it. We are not wholly without hope that the real weight of each such atom may some day be known—not merely the relative weight of the several atoms, but the number in a given volume of any material; that the form and motion of the parts of each atom and the distances by which they are separated may be calculated; that the motions by which they produce heat, electricity, and light may be illustrated by exact geometrical diagrams; and that the fundamental properties of the intermediate and possibly constituent medium may be arrived at. Then the motion of planets and music of the spheres will be neglected for a while in admiration of the maze in which the tiny atoms run."

Even before this was written some of the anticipated results had been partially attained. Loschmidt in Vienna had shown, and not much latter Stoney independently in England showed, how to reduce from Clausius and Maxwell's kinetic theory of gases a superior limit to the number of atoms in a given measurable space. I was unfortunately quite unaware of what Loschmidt and Stoney had done when I made a similar estimate on the same foundation, and communicated to NATURE in an article on "The Size of Atoms." But questions of personal priority, however interesting they may be to the persons concerned, sink into insignificance in the prospect of any gain of deeper insight into the secrets of nature. The triple coincidence of independent reasoning in this case is valuable as confirmation of a conclusion violently contravening ideas and opinions which had been almost universally held regarding the dimensions of the molecular structure of matter. Chemists and other naturalists had been in the habit of evading questions as to the hardness or indivisibility of atoms by virtually assuming them to be infinitely small and infinitely numerous. We must now no longer look upon the atom, with Boscovich, as a mystic point endowed with inertia and the attribute of attracting or repelling other such centres with forces depending upon the intervening distances (a supposition only tolerated with the tacit assumption that the inertia and attraction of each atom is infinitely small and the number of atoms infinitely great), nor can we agree with those who have attributed to the atom occupation of space with infinite hardness and strength (incredible in any finite body); but we must realise it as a piece of matter of measurable dimensions, with shape, motion, and laws of action, intelligible subjects of scientific investigation.

The prismatic analysis of light discovered by Newton was estimated by himself as being "the oddest, if not the most considerable, detection which hath hitherto been made in the operations of nature."

Had he not been deflected from the subject, he could not have failed to obtain a pure spectrum; but this, with the inevitably consequent discovery of the dark lines, was reserved for the nineteenth century. Our fundamental knowledge of the dark lines is due solely to Fraunhofer. Wollaston saw them, but did not discover them. Brewster laboured long and well to perfect the prismatic analysis of sunlight; and his observations on the dark bands produced by the absorption of interposed gases and vapours laid important foundations for the grand superstructure which he scarcely lived to see. Piazzi Smyth, by spectroscopic observation performed on the Peak of Teneriffe, added greatly to our knowledge of the dark lines produced in the solar spectrum by the absorption of our own atmosphere. The prism became an instrument for chemical qualitative analysis in the hands of Fox Talbot and Herschel, who first showed how, through it, the old "blowpipe test" or generally the estimation of substances from the colours which they give to flames, can be prosecuted with an accuracy and a discriminating power not to be attained when the colour is judged by the unaided eye. But the application of this test to solar and stellar chemistry had never, I believe, been suggested, either directly or indirectly, by any other naturalist, when Stokes taught it to me in Cambridge at some time prior to the summer of 1852. The observational and experimental foundations on which he built were:—

1. The discovery by Fraunhofer of a coincidence between his double dark line D of the solar spectrum and a double bright line which he observed in the spectra of ordinary artificial flames.

2. A very rigorous experimental test of this coincidence by Prof. W. H. Miller, which showed it to be accurate to an astonishing degree of minuteness.

3. The fact that the yellow light given out when salt is thrown on burning spirit consists almost solely of the two nearly identical qualities which constitute that double bright line.

4. Observations made by Stokes himself, which showed the bright line D to be absent in a candle-flame when the wick was snuffed clean, so as not to project into the luminous envelope, and from an alcohol flame when the spirit was burned in a watch-glass. And

5. Foucault's admirable discovery (L'Institut, Feb. 7, 1849) that the Voltaic arc between charcoal points is "a medium which emits the rays D on its own account, and at the same time absorbs them when they come from another quarter."

The conclusions, theoretical and practical, which Stokes taught me, and which I gave regularly afterwards in my public lectures in the University of Glasgow, were:

1. That the double line D, whether bright or dark, is due to vapour of sodium.

2. That the ultimate atom of sodium is susceptible of regular elastic vibrations, like those of a tuning-fork or of stringed musical instruments; that like an instrument with two strings tuned to approximate unison, or an approximately circular elastic disc, it has two fundamental notes or vibrations of approximately equal pitch; and that the periods of these vibrations are precisely the periods of the two slightly different yellow lights constituting the double bright line D.

3. That when vapour of sodium is at a high enough temperature to become itself a source of light, each atom executes these two fundamental vibrations simultaneously; and that therefore the light proceeding from it is of the two qualities constituting the double bright line D.

4. That when vapour of sodium is present in space across which light from another source is propagated, its atoms, according to a well-known general principle of dynamics, are set to vibrate in either or both of those fundamental modes, if some of the incident light is of one or other of their periods, or some of one and some of the other; so that the energy of the waves of those particular qualities of light is converted into thermal vibrations of the medium and dispersed in all directions, while light of all other qualities, even though very nearly agreeing with them, is transmitted with comparatively no loss.

5. That Fraunhofer's double dark line D of solar and stellar spectra is due to the presence of vapour of sodium in atmospheres surrounding the sun and those stars in whose spectra it had been observed.

6. That other vapours than sodium are to be found in the atmospheres of sun and stars by searching for substances producing in the spectra of artificial flames bright lines coinciding with other dark lines of the solar and stellar spectra than the Fraunhofer line D.

The last of these propositions I felt to be confirmed (it was perhaps partly suggested) by a striking and beautiful experiment admirably adapted for lecture illustrations, due to Foucault, which had been shown to me by M. Dubosque Soleil, and the Abbé Moigno, in Paris in the month of October 1850. A prism and lenses were arranged to throw upon a screen an approximately pure spectrum of a vertical electric arc between charcoal poles of a powerful battery, the lower one of which was hollowed like a cup. When pieces of copper and pieces of zinc were separately thrown into the cup, the spectrum exhibited, in perfectly definite positions, magnificent well-marked bands of different colours characteristic of the two metals. When a piece of brass, compounded of copper and zinc, was put into the cup, the spectrum showed all the bands, each precisely in the place in which it had been seen when one metal or the other had been used separately.

It is much to be regretted that this great generalisation was not published to the world twenty years ago. I say this, not because it is to be regretted that Angström should have the credit of having in 1853 published independently the statement that an "incandescent gas emits luminous rays of the same refrangibility as those which it can absorb"; or that Balfour Stewart should have been unassisted by it when, coming to the subject from a very different point of view, he made, in his extension of the "Theory of Exchanges," * the still wider generalisation that the radiating power of every kind of substance is equal to its absorbing power for every kind of ray; or that Kirchhoff also should have in 1859 independently discovered the same proposition, and shown its

* Edin. Transactions, 1858-59.

application to solar and stellar chemistry; but because we might now be in possession of the inconceivable riches of astronomical results which we expect from the next ten years' investigation by spectrum analysis, had Stokes given his theory to the world when it first occurred to him.

To Kirchhoff belongs, I believe, solely the great credit of having first actually sought for and found other metals than sodium in the sun by the method of spectrum analysis. His publication of October 1859 inaugurated the practice of solar and stellar chemistry, and gave spectrum analysis an impulse to which in a great measure is due its splendidly successful cultivation by the labours of many able investigators within the last ten years.

To prodigious and wearing toil of Kirchhoff himself, and of Angström, we owe large-scale maps of the solar spectrum, incomparably superior in minuteness and accuracy of delineation to anything ever attempted previously. These maps now constitute the standards of reference for all workers in the field. Plucker and Hittorf opened ground in advancing the physics of spectrum analysis, and made the important discovery of changes in the spectra of ignited gases produced by changes in the physical condition of the gas. The scientific value of the meetings of the British Association is well illustrated by the fact that it was through conversation with Plücker at the Newcastle meeting that Lockyer was first led into the investigation of the effects of varied pressure on the quality of the light emitted by glowing gas which he and Frankland have prosecuted with such admirable success. Scientific wealth tends to accumulation according to the law of compound interest. Every addition to knowledge of properties of matter supplies the naturalist with new instrumental means for discovering and interpreting phenomena of nature, which in their turn afford foundations for fresh generalisations, bringing gains of permanent value into the great storehouse of philosophy. Thus Frankland, led, from observing the want of brightness of a candle burning in a tent on the summit of Mont Blanc, to scrutinise Davy's theory of flame, discovered that brightness without incandescent solid particles is given to a purely gaseous flame by augmented pressure, and that a dense ignited gas gives a spectrum comparable with that of the light from an incandescent solid or liquid. Lockyer joined him; and the two found that every incandescent substance gives a continuous spectrum—that an incandescent gas under varied pressure gives bright bars across the continuous spectrum, some of which, from the sharp, hard and fast lines observed where the gas is in a state of extreme attenuation, broaden out on each side into nebulous bands as the density is increased, and are ultimately lost in the continuous spectrum when the condensation is pushed on till the gas becomes a fluid no longer to be called gaseous. More recently they have examined the influence of temperature, and have obtained results which seemed to show that a highly attenuated gas, which at a high temperature gives several bright lines, gives a smaller and smaller number of lines, of sufficient brightness to be visible, when the temperature is lowered, the density being kept unchanged. I cannot refrain here from remarking how admirably this beautiful investigation harmonises with Andrews's great discovery of continuity between the gaseous and liquid states. Such things make the life-blood of science. In contemplating them we feel as if led out from narrow waters of scholastic dogma to a refreshing excursion on the broad and deep ocean of truth, where we learn from the wonders we see that there are endlessly more and more glorious wonders still unseen.

Stokes's dynamical theory supplies the key to the philosophy of Frankland and Lockyer's discovery. Any atom of gas when struck and left to itself vibrates with perfect purity its fundamental note or notes. In a highly attenuated gas each atom is very rarely in collision with other atoms, and therefore is nearly at all times in a state of true vibration. Hence the spectrum of a highly attenuated gas consists of one or more perfectly sharp bright lines, with a scarcely perceptible continuous gradation of prismatic colour. In denser gas each atom is frequently in collision, but still is for much more time free, in intervals between collisions, than engaged in collision; so that not only is the atom itself thrown sensibly out of tune during a sensible proportion of its whole time, but the confused jangle of vibrations in every variety of period during the actual collision becomes more considerable in its influence. Hence bright lines in the spectrum broaden out somewhat, and the continuous spectrum becomes less faint. In still denser gas each atom may be almost as much time in collision as free, and the spectrum then consists of broad nebulous bands crossing a continuous spectrum of considerable brightness. When the medium is so dense that each atom is always in collision, that is to say never free from influence of its neighbours, the spectrum will generally be continuous, and may present little or no appearance of bands, or even of maxima of brightness. In this condition the fluid can be no longer regarded as a gas, and we must judge of its relation to the vaporous or liquid states according to the critical conditions discovered by Andrews.

While these great investigations of properties of matter were going on, naturalists were not idle with the newly recognised power of the spectroscope at their service. Chemists soon followed the example of Bunsen in discovering new metals in terrestrial matter by the old blow-pipe and prism test of Fox Talbot and Herschel. Biologists applied spectrum analysis to animal and vegetable chemistry, and to sanitary investigations. But it is in astronomy that spectroscopic research has been carried on with the greatest activity, and been most richly rewarded with results. The chemist and the astronomer have joined their forces. An astronomical observatory has now, appended to it, a stock of reagents such as hitherto was only to be found in the chemical laboratory. A devoted corps of volunteers of all nations, whose motto might well be *Ubique*, have directed their artillery to every region of the universe. The sun, the spots on his surface, the corona and the red and yellow prominences seen round him during total eclipses, the moon, the planets, comets, auroras, nebulæ, white stars, yellow stars, red stars, variable and temporary stars, each tested by the prism, was compelled to show its distinguishing prismatic colours. Rarely before in the history of science has enthusiastic perseverance directed by penetrative genius produced within ten years so brilliant a succession of discoveries. It is not merely the *chemistry* of sun and stars, as first suggested, that is subjected to analysis by the spectroscope. Their whole laws of being are now subjects of direct investigation; and already we have glimpses of their evolutional history through the stupendous power of this most subtle and delicate test. We had only solar and stellar chemistry; we now have solar and stellar physiology.

It is an old idea that the colour of a star may be influenced by its motion relatively to the eye of the spectator, so as to be tinged with red if it moves from the earth, or blue if it moves towards the earth. William Allen Miller, Huggins, and Maxwell showed how, by aid of the spectroscope, this idea may be made the foundation of a method of measuring the relative velocity with which a star approaches to or recedes from the earth. The principle is, first to identify, if possible, one or more of the lines in the spectrum of the star, with a line or lines in the spectrum of sodium, or some other terrestrial substance, and then (by observing the star and the artificial light simultaneously by the same spectroscope) to find the difference, if any, between their refrangibilities. From this difference of refrangibility the ratio of the periods of the two lights is calculated, according to data determined by Fraunhofer from comparisons between the positions of the dark lines in the prismatic spectrum and in his own "interference spectrum" (produced by substituting for the prism a fine grating). A first comparatively rough application of the test by Miller and Huggins to a large number of the principal stars of our skies, including Aldebaran, α Orionis, β Pegasi, Sirius, α Lyræ, Capella, Arcturus, Pollux, Castor (which they had observed rather for the chemical purpose than for this), proved that not one of them had so great a velocity as 315 kilometres per second to or from the earth, which is a *most momentous result in respect of cosmical dynamics*. Afterwards Huggins made special observations of the velocity test, and succeeded in making the measurement in one case, that of Sirius, which he then found to be receding from the earth at the rate of 66 kilometres per second. This, corrected for the velocity of the earth at the time of the observation, gave a velocity of Sirius, relative to the Sun, amounting to 47 kilometres per second. The minuteness of the difference to be measured, and the smllness ofa the amount of light, even when the brightest star is observed, renders the observation extremely difficult. Still, with such great skill as Mr. Huggins has brought to bear on the investigation, it can scarcely be doubted that velocities of many other stars may be measured. What is now wanted is, certainly not greater skill, perhaps not even more powerful instruments, but *more instruments and more observers*. Lockyer's applications of the velocity test to the relative motions of different gases in the Sun's photosphere, spots, chromosphere, and chromospheric prominences, and his observations of the varying spectra presented by the same substance as it moves from one position to another in the Sun's atmosphere, and his interpretation of these observations according to the laboratory results of Frankland and himself, go far towards confirming the conviction that in a few years all the marvels of the Sun will be dynamically explained according to known properties of matter.

During six or eight precious minutes of time, spectroscopes have been applied to the solar atmosphere and to the corona seen round the dark disc of the moon eclipsing the sun. Some of the wonderful results of such observations, made in India on the occasion of the eclipse of August 1868, were described by Prof. Stokes in a previous address. Valuable results have, through the liberal assistance given by the British and American Governments, been obtained also from the total eclipse of last December, notwithstanding a generally unfavourable condition of weather. It seems to have been proved that at least some sensible part of the light of the "corona" is a terrestrial atmospheric halo or dispersive reflection of the light of the glowing hydrogen and "helium" * round the sun. I believe I may say on the present occasion when preparation must again be made to utilise a Total Eclipse of the sun, that the British Association confidently trusts to our Government exercising the same wise liberality as heretofore in the interests of science.

The old nebular hypothesis supposes the solar system and other similar systems through the universe which we see at a distance as stars, to have originated in the condensation of fiery nebulous matter. This hypothesis was invented before the discovery of thermo-dynamics, or the nebulæ would not have been supposed to be fiery; and the idea seems never to have occurred to any of its inventors or early supporters that the matter, the condensation of which they supposed to constitute the Sun and stars, could have been other than fiery in the beginning. Mayer first suggested that the heat of the Sun may be due to gravitation; but he supposed meteors falling in to keep always generating the heat which is radiated year by year from the Sun. Helmholtz, on the other hand, adopting the nebular hypothesis, showed in 1854 that it was not necessary to suppose the nebulous matter to have been originally fiery, but that mutual gravitation between its parts may have generated the heat to which the present high temperature of the Sun is due. Further he made the important observations that the potential energy of gravitation in the Sun is even now far from exhausted; but that with further and further shrinking more and more heat is to be generated, and that thus we can conceive the Sun even now to possess a sufficient store of energy to produce heat and light, almost as at present, for several million years of time future. It ought, however, to be added that this condensation can only follow from cooling, and therefore that Helmholtz's gravitational explanation of future Sun-heat amounts really to showing that the Sun's thermal capacity is enormously greater, in virtue of the mutual gravitation between the parts of so enormous a mass, than the sum of the thermal capacities of separate and smaller bodies of the same material and the same total mass. Reasons for adopting this theory, and the consequences which follow from it, are discussed in an article "On the Age of the Sun's Heat," published in *Macmillan's Magazine* for March, 1862.

For a few years Mayer's theory of solar heat had seemed to me probable; but I had been led to regard it as no longer tenable, because I had been in the first place driven, by consideration of the very approximate constancy of the Earth's period of revolution round the Sun for the last 2000 years, to conclude that "The principal source, perhaps the sole appreciably effective "source of Sun-heat, is in bodies circulating round the Sun at present inside "the Earth's orbit" †; and because Le Verrier's researches on the motion of the planet Mercury, though giving evidence of a sensible influence attributable to matter circulating as a great number of small planets within his orbit round the Sun, showed that the amount of matter that could possibly be assumed to circulate at any considerable distance from the Sun must be very small; and therefore "if the meteoric influx taking place at present is enough to produce any appreciable portion of the heat radiated away, it must be supposed to be from matter circulating round the Sun, within very short distances of his surface. The density of this meteoric cloud would have to be supposed so great that comets could scarcely have escaped as comets actually have escaped, showing no discoverable effects of resistance, after passing his surface within a distance equal to one-eighth of his radius. All things considered, there seems little probability in the hypothesis that solar radiation is compensated to any appreciable degree, by heat generated by meteors falling in, at present; and, as it can be shown that no chemical theory is tenable,* it must be concluded as most probable that the Sun is at present more an incandescent liquid mass cooling."†

Thus on purely astronomical grounds was I long ago led to abandon as very improbable the hypothesis that the Sun's heat is supplied dynamically from year to year by the influx of meteors. But now spectrum analysis gives proof finally conclusive against it.

Each meteor circulating round the Sun must fall in along a very gradual spiral path, and before reaching the Sun must have been for a long time exposed to an enormous heating effect from his radiation when very near, and must thus have been driven into vapour before actually falling into the Sun. Thus, if Mayer's hypothesis is correct, friction between vortices of meteoric vapours and the Sun's atmosphere must be the immediate cause of solar heat; and the velocity with which these vapours circulate round equatorial parts of the Sun must amount to 435 kilometres per second. The spectrum test of velocity applied by Lockyer showed but a twentieth part of this amount as the greatest observed relative velocity between different vapours in the Sun's atmosphere.

At the first Liverpool meeting of the British Association (1854), in advancing a gravitational theory to account for all the heat, light, and motions of the universe, I urged that the immediately antecedent condition of the matter of which the Sun and Planets were formed, not being fiery. could not have been gaseous; but that it probably was solid, and may have been like the meteoric stones which we still so frequently meet with through space. The discovery of Huggins, that the light of the Nebulæ, so far as hitherto sensible to us, proceeds from incandescent hydrogen and nitrogen gases, and that the heads of comets also give us light of incandescent gas, seems at first sight literally to fulfil that part of the Nebular hypothesis to which I had objected. But a solution, which seems to me in the highest degree probable, has been suggested by Tait. He supposes that it may be by ignited gaseous exhalations proceeding from the collisions of meteoric stones that Nebulæ and the heads of Comets show themselves to us, and he suggested, at a former meeting of the Association, that experiments should be made for the purpose of applying spectrum analysis to the light which has been observed in gunnery trials. such as those at Shoeburyness, when iron strikes against iron at a great velocity, but varied by substituting for the iron various solid materials, metallic or stony. Hitherto this suggestion has not been acted upon; but surely it is one the carrying out of which ought to be promoted by the British Association.

Most important steps have been recently made towards the discovery of the nature of comets; establishing with nothing short of certainty the truth of a hypothesis which had long appeared to me probable,—that they consist of groups of meteoric stones; —accounting satisfactorily for the light of the nucleus; and giving a simple and rational explanation of phenomena presented by the tails of comets which had been regarded by the greatest astronomers as almost preternaturally marvellous. The meteoric hypothesis to which I have referred remained a mere hypothesis (I do not know that it was ever even published) until, in 1866, Schiaparelli calculated, from observations on the August meteors, an orbit for these bodies which he found to agree almost perfectly with the orbit of the great comet of 1862 as calculated by Oppolzer; and so discovered and demonstrated that a comet consists of a group of meteoric stones. Professor Newton, of Yale College, United States, by examining ancient records, ascertained that in periods of about thirty-three years, since the year 902, there have been exceptionally brilliant displays of the November meteors. It had long been believed that these interesting visitants came from a train of small detached planets circulating round the Sun all in nearly the same orbit, and constituting a belt analogous to Saturn's ring, and that the reason for the comparatively large number of meteors which we observe annually about the 14th of November is, that at that time the earth's orbit cuts through the supposed meteoric belt. Professor Newton concluded from his investigation that there is a denser part of the group of meteors which extends over a portion of the orbit so great as to occupy about one-tenth or one-fifteenth of the

* Frankland and Lockyer find the yellow prominences to give a very decided bright line not far from D, but hitherto not identified with any terrestrial flame. It seems to indicate a new substance, which they propose to call Helium.

† On the Mechanical Energies of the Solar System. Transactions of the Royal Society of Edinburgh, 1854; and Phil. Mag. 1854, second half year.

* "Mechanical Energies," &c.

† "Age of the Sun's Heat" (*Macmillan's Magazine*, March 1862).

periodic time in passing any particular point, and gave a choice of five different periods for the revolution of this meteoric stream round the sun, any one of which would satisfy his statistical result. He further concluded that the line of nodes, that is to say, the line in which the plane of the meteoric belt cuts the plane of the Earth's orbit, has a progressive sidereal motion of about $52''\cdot 4$ per annum. Here, then, was a splendid problem for the physical astronomer; and, happily, one well qualified for the task, took it up. Adams, by the application of a beautiful method invented by Gauss, found that of the five periods allowed by Newton just one permitted the motion of the line of nodes to be explained by the disturbing influence of Jupiter, Saturn, and other planets. The period chosen on these grounds is $33\frac{1}{4}$ years. The investigation showed further that the form of the orbit is a long ellipse, giving for shortest distance from the Sun 145 million kilometres, and for longest distance 2,895 million kilometres. Adams also worked out the longitude of the perihelion and the inclination of the orbit's plane of the ecliptic. The orbit which he thus found agreed so closely with that of Tempel's Comet I. 1866 that he was able to identify the comet and the meteoric belt.* The same conclusion had been pointed out a few weeks earlier by Schiaparelli, from calculations by himself on data supplied by direct observations on the meteors, and independently by Peters from calculations by Leverrier on the same foundation. It is therefore thoroughly established that Temple's Comet I. 1866 consists of an elliptic train of minute planets, of which a few thousands or millions fall to the earth annually about the 14th of November, when we cross their track. We have probably not yet passed through the very nucleus or densest part; but thirteen times, in Octobers and Novembers, from October 13, A.D. 902 to November 14, 1866 inclusive (this last time having been correctly predicted by Prof. Newton), we have passed through a part of the belt greatly denser than the average. The densest part of the train, when near enough to us, is visible as the head of the comet. This astounding result, taken along with Huggins's spectroscopic observations on the light of the heads and tails of comets, confirms most strikingly Tait's theory of comets, to which I have already referred; according to which the comet, a group of meteoric stones, is self-luminous in its nucleus, on account of collisions among its constituents, while its "tail" is merely a portion of the less dense part of the train illuminated by sunlight, and visible or invisible to us according to circumstances, not only of density, degree of illumination, and nearness, but also of tactic arrangement, as of a flock of birds or the edge of a cloud of tobabco smoke! What prodigious difficulties are to be explained, you may judge from two or three sentences which I shall read from Herschel's Astronomy, and from the fact that even Schiaparelli seems still to believe in the repulsion. "There is, beyond question, some profound secret and mystery of nature concerned in the phenomenon of their tails. Perhaps it is not too much to hope that future observation borrowing every aid from rational speculation, grounded on the progress of physical science generally (especially those branches of it which relate to the etherial or imponderable elements), may enable us ere long to penetrate this mystery, and to declare whether it is really *matter* in the ordinary anticipation of the term which is projected from their heads with such extraordinary velocity, and if not *impelled*, at least *directed*, in its course, by reference to the Sun, as its point of avoidance." †

"In no respect is the question as to the materiality of the tail more forcibly pressed on us for consideration than in that of the enormous sweep which it makes round the sun *in perihelio*, in the manner of a straight and rigid rod, *in defiance of the law of gravitation*, nay, even *of the received* laws of motion." *

"The projection of this ray . . . to so enormous a length, in a single day conveys an impression of the intensity of the forces acting to produce such a velocity of material transfer through space such as no other natural phenomenon is capable of exciting. It is clear that *if we have to deal here with matter, such as we conceive it*, viz., *possessing inertia—at all*, it must be under the dominion of forces incomparably more energetic than gravitation, and quite of a different nature." †

Think now of the admirable simplicity with which Tait's beautiful "sea-bird analogy," as it has been called, can explain all these phenomena.

The essence of science, as is well illustrated by astronomy and cosmical physics, consists in inferring antecedent conditions, and anticipating future evolutions, from phenomena which have actually come under observation. In biology, the difficulties of successfully acting up to this ideal are prodigious. The earnest naturalists of the present day are, however, not appalled or paralysed by them, and are struggling boldly and laboriously to pass out of the mere "Natural History stage" of their study, and bring Zoology within the range of Natural Philosophy. A very ancient speculation, still clung to by many naturalists (so much so that I have a choice of modern terms to quote in expressing it) supposes that, under meteorological conditions very different from the present, dead matter may have run together or crystallised or fermented into "germs of life," or "organic cells," or "protoplasm." But science brings a vast mass of inductive evidence against this hypothesis of spontaneous generation, as you have heard from my predecessor in the Presidential chair. Careful enough scrutiny has, in every case up to the present day, discovered life as antecedent to life. Dead matter cannot become living without coming under the influence of matter previously alive. This seems to me as sure a teaching of science as the law of gravitation. I utterly repudiate, as opposed to all philosophical uniformitarianism, the assumption of "different meteorological condition"—that is to say, somewhat different vicissitudes of temperature, pressure, moisture, gaseous atmosphere—to produce or to permit that to take place by force or motion of dead matter alone, which is a direct contravention of what seems to us biological law. I am prepared for the answer, "our code of biological law is an expression of our ignorance as well of our knowledge." And I say yes: search for spontaneous generation out of inorganic materials; let any one not satisfied with the purely negative testimony of which we have now so much against it, throw himself into the inquiry. Such investigations as those of Pasteur, Pouchet, and Bastian are among the most interesting and momentous in the whole range of Natural History, and their results, whether positive or negative, must richly reward the most careful and laborious experimenting. I confess to being deeply impressed by the evidence put before us by Professor Huxley, and I am ready to adopt, as an article of scientific faith, true through all space and through all time, that life proceeds from life, and from nothing but life.

How, then, did life originate on the Earth? Tracing the physical history of the Earth backwards, on strict dynamical principles, we are brought to a red-hot melted globe on which no life could exist. Hence when the Earth was first fit for life, there was no living thing on it. There were rocks solid and disintegrated, water, air all round, warmed and illuminated by a brilliant Sun, ready to become a garden. Did grass and trees and flowers spring into existence, in all the fulness of ripe beauty, by a fiat of Creative power? or did vegetation, growing up from seed sown, spread and multiply over the whole Earth? Science is bound, by the everlasting law of honour, to face fearlessly every problem which can fairly be presented to it. If a probable solution, consistent with the ordinary course of nature, can be found, we must not invoke an abnormal act of Creative Power. When a lava stream flows down the sides of Vesuvius or Etna it quickly cools and becomes solid; and after a few weeks or years it teems with vegetable and animal life, which for it originated by the transport of seed and ova and by the migration of individual living creatures When a volcanic island springs up from the sea, and after a few years is found clothed with vegetation, we do not hesitate to assume that seed has been wafted to it through the air, or floated to it on rafts. Is it not possible, and if possible, is it not probable, that the beginning of vegetable life on the earth is to be similarly explained?

* Signor Schiaparelli, Director of the Observatory of Milan, who, in a letter dated 31st December, 1866, pointed out that the elements of the orbit of the August Meteors, calculated from the observed position of their radiant point on the supposition of the orbit being a very elongated ellipse agreed very closely with those of the orbit of Comet II 1862, calculated by Dr. Oppolzer. In the same letter Schiaparelli gives elements of the orbit of the November meteors, but these were not sufficiently accurate to enable him to identify the orbit with that of any known comet. On the 21st January, 1867, M. Leverrier gave more accurate elements of the orbit of the November Meteors, and in the "Astronomische Nachrichten" of January 9, Mr. C. F. W. Peters, of Altona, pointed out that these elements closely agreed with those of Tempel's Comet (I. 1866), calculated by Dr. Oppolzer, and on Feb. 2, Schiaparelli having recalculated the elements of the orbit of the meteors himself noticed the same agreement. Adams arrived quite independently at the conclusion that the orbit of $3\frac{1}{4}$ years period is the one which must be chosen out of the five indicated by Prof. Newton. His calculations were sufficiently advanced before the letters referred to appeared, to show that the other four orbits offered by Newton were inadmissible. But the calculations to be gone through to find the secular motion of the node in such an elongated orbit as that of the meteors, were necessarily very long, so that they were not completed till about March 1867. They were communicated in that month to the Cambridge Philosophical Society, and in the month following to the Astronomical Society.

† Herschel's Astronomy, § 599.

* Herschel's Astronomy, § 599.

† *Ibid.*, 10th Edition, § 589.

Every year thousands, probably millions, of fragments of solid matter fall upon the Earth—whence came these fragments? What is the previous history of any one of them? Was it created in the beginning of time an amorphous mass? This idea is so unacceptable that, tacitly or explicitly, all men discard it. It is often assumed that all, and it is certain that some, meteoric stones are fragments which had been broken off from greater masses and launched free into space. It is as sure that collisions must occur between great masses moving through space as it is that ships, steered without intelligence directed to prevent collision, could not cross and recross the Atlantic for thousands of years with immunity from collisions. When two great masses come into collision in space it is certain that a large part of each is melted; but it seems also quite certain that in many cases a large quantity of *débris* must be shot forth in all directions, much of which may have experienced no greater violence than individual pieces of rock experience in a land-slip or in blasting by gunpowder. Should the time when this earth comes into collision with another body, comparable in dimensions to itself, be when it is still clothed as at present with vegetation, many great and small fragments carrying seed and living plants and animals would undoubtedly be scattered through space. Hence and because we all confidently believe that there are at present, and have been from time immemorial, many worlds of life besides our own, we must regard it as probable in the highest degree that there are countless seed bearing meteoric stones moving about through space. If, at the present instant, no life existed upon this earth, one such stone falling upon it might, by what we blindly call natural causes, lead to its becoming covered with vegetation. I am fully conscious of the many scientific objections which may be urged against this hypothesis, but I believe them to be all answerable. I have already taxed your patience too severely to allow me to think of discussing any of them on the present occasion. The hypothesis that life originated on this earth through moss-grown fragments from the ruins of another world may seem wild and visionary; all I maintain is that it is not unscientific.

From the earth stocked with such vegetation as it could receive meteorically, to the earth teeming with all the endless variety of plants and animals which now inhabit it, the step is prodigious; yet, according to the doctrine of continuity, most ably laid before the Association by a predecessor in this chair (Mr. Grove), all creatures now living on earth have proceeded by orderly evolution from some such origin. Darwin concludes his great work on "The Origin of Species" with the following words:—"It is interesting to contemplate an entangled bank clothed with many plants of many kinds, with birds singing on the bushes, with various insects flitting about, and with worms crawling through the damp earth, and to reflect that these elaborately constructed forms, so different from each other, and dependent on each other in so complex a manner, have all been produced by laws acting around us." . . . . "There is grandeur in this view of life, with its several powers, having been originally breathed by the Creator into a few forms or into one; and that, whilst this planet has gone cycling on according to the fixed law of gravity, from so simple a beginning endless forms, most beautiful and most wonderful, have been and are being evolved." With the feeling expressed in these two sentences I most cordially sympathise. I have omitted two sentences which come between them, describing briefly the hypothesis of "the origin of species by natural selection," because I have always felt that this hypothesis does not contain the true theory of evolution, if evolution there has been in biology. Sir John Herschel, in expressing a favourable judgment on the hypothesis of zoological evolution, with however, some reservation in respect to the origin of man, objected to the doctrine of natural selection, that it was too like the Laputan method of making books, and that it did not sufficiently take into account a continually guiding and controlling intelligence. This seems to me a most valuable and instructive criticism. I feel profoundly convinced that the argument of design has been greatly too much lost sight of in recent zoological speculations. Reaction against the frivolities of teleology, such as are to be found, not rarely, in the notes of the learned Commentators on Paley's "Natural Theology," has I believe had a temporary effect in turning attention from the solid and irrefragable argument so well put forward in that excellent old book. But overwhelmingly strong proofs of intelligent and benevolent design lie all around us, and if ever perplexities, whether metaphysical or scientific, turn us away from them for a time, they come back upon us with irresistible force, showing to us through nature the influence of a free will, and teaching us that all living beings depend on one ever-acting Creator and Ruler.

SECTION A.

MATHEMATICAL AND PHYSICAL SCIENCE.

OPENING ADDRESS BY THE PRESIDENT, PROF. P. G. TAIT, M.A.

IN opening the proceedings of this Section my immediate predecessors have exercised their ingenuity in presenting its widely different component subjects from their several points of view, and in endeavouring to coordinate them. What they were obliged to leave unfinished, it would be absurd in me to attempt to complete. It would be impossible, also, in the limits of a brief address, to give a detailed account of the recent progress of physical and mathematical knowledge. Such a work can only be produced by separate instalments, each written by a specialist, such as the admirable "Reports" which form from time to time the most valuable portions of our annual volume.

I shall therefore confine my remarks in the main to those two subjects, one in the mathematical, the other in the purely physical, division of our work, which are comparatively familiar to myself. I wish, if possible, to induce, ere it be too late, native mathematicians to pay much more attention than they have yet paid to Hamilton's magnificent Calculus of Quaternions, and to call the particular notice of physicists to our President's grand Principle of Dissipation of Energy. I think that these are, at this moment, the most important because the most promising parts of our field.

If nothing more could be said for Quaternions than that they enable us to exhibit in a singularly compact and elegant form, whose meaning is obvious at a glance on account of the utter inartificiality of the method, results which in the ordinary Cartesian coordinates are of the utmost complexity, a very powerful argument for their use would be furnished. But it would be unjust to Quaternions to be content with such a statement; for we are fully entitled to say that in *all* cases, even in those to which the Cartesian methods seem specially adapted, they give as simple an expression as any other method; while in the great majority of cases they give a vastly simpler one. In the common methods a judicious choice of coordinates is often of immense importance in simplifying an investigation; in Quaternions there is usually *no choice*, for (except when they degrade to mere scalars) they are in general utterly independent of any particular directions in space, and select of themselves the most natural reference lines for each particular problem. This is easily illustrated by the most elementary instances, such as the following:— The general equation of Cones involves merely the *direction* of the vector of a point, while that of Surfaces of Revolution is a relation between the *lengths* of that vector and of its resolved part parallel to the axis, and Quaternions enable us by a mere mark to separate the ideas of length and direction without introducing the cumbrous and clumsy square roots of sums of squares which are otherwise necessary.

But, as it seems to me that mathematical methods should be specially valued in this Section as regards their fitness for physical applications, what can possibly from that point of view be more important than Hamilton's ∇? Physical analogies have often been invoked to make intelligible various mathematical processes. Witness the case of Statical Electricity, wherein Thomson has by the analogy of Heat-conduction, explained the meaning of various important theorems due to Green, Gauss, and others; and wherein Clerk-Maxwell has employed the properties of an imaginary incompressible liquid (devoid of inertia) to illustrate not merely these theorems, but even Thomson's Electrical Images. [In fact he has gone much further, having applied his analogy to the puzzling combinations presented by Electrodynamics.] There can be little doubt that these comparisons owe their birth to the small intelligibility, *per se*, of what has been called Laplace's Operator, $\frac{d^2}{dx^2}+\frac{d^2}{dy^2}+\frac{d^2}{dz^2}$, which appears alike in all theories of attraction at a distance, in the steady flow of heat in a conductor, and in the steady motion of incompressible fluids. But when we are taught to understand the operator itself, we are able to dispense with these analogies, which, however, valuable and beautiful, have certainly to be used with extreme caution, as tending very often to confuse and mislead. Now Laplace's operator is merely the negative of the *square* of Hamilton's ∇, which is perfectly intelligible in itself and in all its combinations; and can be defined as giving the vector-rate of most rapid increase of any scalar function to which it is applied—giving, for instance, the vector-force from a potential, the heat-flux from a distribution of temperature, &c. Very simple functions of the same operator give the rate of increase of a quantity in any assigned direction, the condensation and elementary rotation produced by given displacements of the parts of a system, &c. For instance, a very elementary application of ∇ to the theory of attraction enables us to put one of its fundamental principles in the following extremely suggestive form :—If the displacement or velocity of each particle of a medium represent in magnitude and direction the electric force at that particle, the corresponding statical distribution of electricity is proportional everywhere to the condensation produced. Again, Green's celebrated theorem is at once seen to be merely the well-known equation of continuity expressed for a heterogeneous fluid, whose density at every point is proportional to one electric potential, and its displacement or velocity proportional to and in the direction of the electric force due to another potential. But this is not the time to pursue such an inquiry, for it would lead me at once to discussions as to the possible nature of electric phenomena and of gravitation. I believe myself to be fully justified in saying that, were the theory of this operator thoroughly developed and generally known, the whole mathematical treatment of such physical questions as those just mentioned would undergo an immediate and enormous simplification; and this, in its turn, would be at once followed by a proportionately large extension of our knowledge.*

And this is but *one* of the claims of Quaternions to the attention of physicists. When we come to the important questions of stress and strain in an elastic solid, we find again that all the elaborate and puzzling machinery of coordinates commonly employed can be at once comprehended and kept out of sight in a mere single symbol—a linear and vector function, which is self-conjugate if the strain be pure. This is simply, it appears to me, a proof either that the elaborate machinery ought never to have been introduced, or that its use was an indication of a comparatively savage state of mathematical civilisation. In the motion of a rigid solid about a fixed point, a Quaternion, represented by a single symbol which is a function of the time, gives us the operator which could bring the body by a single rotation from its initial position to its position at any assigned instant. In short, whenever with our usual means a result can be obtained in, or after much labour reduced to, a single form, Quaternions will give it at once in that form; so that nothing is ever *lost* in point of simplicity. On the other hand, in numberless cases the Quaternion result is immeasurably simpler and more intelligible than any which can be obtained or even *expressed* by the usual methods. And it is not to be supposed that the modern Higher Algebra, which has done so much to simplify and extend the ordinary Cartesian methods, would be ignored by the general employment of Quaternions; on the contrary, Determinants, Invariants, &c., present themselves in almost every Quaternion solution, and in forms which have received the full benefit of that simplification which Quaternions generally produce. Comparing a Quaternion investigation, no matter in what department, with the equivalent Cartesian one, even when the latter has availed itself to the utmost of the improvements suggested by Higher Algebra, one can hardly help making the remark that they contrast even more strongly than the decimal notation with the binary scale, or with the old Greek Arithmetic—or than the well-ordered subdivisions of the metrical system with the preposterous no-systems of Great Britain, a mere fragment of which (in the form of Table of Weights and Measures) form, perhaps the most effective, if not the most ingenious, of the many instruments of torture employed in our elementary teaching.

It is true that, in the eyes of the pure mathematician, Quaternions have one grand and fatal defect. They cannot be applied to space of *n* dimensions, they are contented to deal with those poor three dimensions in which mere mortals are doomed to dwell, but which cannot bound the limitless aspirations of a Cayley or a Sylvester. From the physical point of view this, instead of a defect, is to be regarded as the greatest possible recommendation. It shows, in fact, Quaternions to be a special instrument so constructed for application to the *Actual* as to have thrown overboard everything which is not absolutely necessary, without the slightest consideration whether or no it was thereby being rendered useless for applications to the *Inconceivable.*

The late Sir John Herschel was one of the first to perceive the value of Quaternions: and there may be present some who remember him, at a British Association meeting not long after their invention, characterising them as a "Cornucopia from which, turn it how you will, something valuable is sure to fall." Is it not strange, to use no harsher word, that such a harvest has hitherto been left almost entirely to Hamilton himself? If but half a dozen tolerably good mathematicians, such as exist in scores in this country, were seriously to work at it, instead of spending (or rather wasting) their time, as so many who have the requisite leisure now do, in going over again what has been already done, or in working out mere details where a grand theory has been sketched, a very great immediate advance would be certain. From the majority of the papers in our few mathematical journals, one would almost be led to fancy that British mathematicians have too much pride to use a simple method while an unnecessarily complex one can be had. No more telling example of this could be wished for than the insane delusion under which they permit Euclid to be employed in our elementary teaching. They seem voluntarily to weight alike themselves and their pupils for the race; and a cynic might, perhaps without much injustice, say they do so that they may have mere self-imposed and avoidable difficulties to face instead of the new, real, and dreaded ones (belonging to regions hitherto unpenetrated) with which Quaternions would too soon enable them to come into contact. But this game will certainly end in disaster. As surely as mathematics came to a relative stand-still in this country for nearly a century after Newton, so surely will it do so again if we leave our eager and watchful rivals abroad to take the initiative in developing the grand method of Hamilton. And it is not alone French and Germans whom we have now to dread, Russia, America, regenerated Italy, and other nations, are all fairly entered for the contest.

The flights of the imagination which occur to the pure mathematician are in general so much better described in his formulæ than in words, that it is not remarkable to find the subject treated by outsiders as something essentially cold and uninteresting—while even the most abstruse branches of physics, as yet totally incapable of being popularised, attract the attention of the uninitiated. The reason may perhaps be sought in the fact that, while perhaps the only successful attempt to invest mathematical reasoning with a halo of glory—that made in this section by Prof. Sylvester—is known to a comparative few, several of the highest problems of physics are connected with those observations which are possible to the many. The smell of lightning has been observed for thousands of years, it required the sagacity of Schönbein to trace it to the formation of Ozone. Not to speak of the (probably fabulous) apple of Newton—what enormous consequences did he obtain by passing light through a mere wedge of glass, and by simply laying a lens on a flat plate! The patching of a trumpery model led Watt to his magnificent inventions. As children at the sea-shore playing with a "roaring buckie," or in later life lazily puffing out rings of tobacco-smoke, we are illustrating two of the splendid researches of Helmholtz. And our President, by the bold, because simple, use of reaction, has eclipsed even his former services to the Submarine Telegraph, and given it powers which but a few years ago would have been deemed unattainable.

In Experimental Physics our case is not hopeless, perhaps not as yet even alarming. Still something of the same kind may be said in this as in pure Mathematics. If Thomson's Theory of Dissipation, for instance, be not speedily developed in this country, we shall soon learn its consequences from abroad. The grand test of our science, the proof of its being a reality and not a mere inventing of new terms and squabbling as to what they shall mean, is that it is ever advancing. There is no standing still; there is no running round and round as in a beaten donkey-track, coming back at the end of a century or so into the old posi-

* The following extracts from letters of Sir W. R. Hamilton have a perfectly general application, so that I do not hesitate to publish them:—"De Morgan was the very *first* person to *notice* the Quaternions *in print*; namely in a paper on Triple Algebra, in the Camb. Phil. Trans. of 18.4. It was, I think, about that time, or not very long afterwards, that he wrote to me, nearly as follows:—'I suspect, Hamilton, that you have *caught the right sow by the ear!*' Between us, dear Mr. Tait, I think *we* shall *begin* the SHEARING of it!" "You might without offence to me, consider that I abused the licence of *hope*, which may be indulged to an inventor, if I were to confess that I expect the Quaternions to supply, hereafter, not merely *mathematical methods*, but also *physical suggestions*. And, in particular, you are quite welcome to smile, if I say that it does not seem extravagant to *me* to suppose that a *full* possession of those *a priori principles* of mine, about the *multiplication of vectors*:—including the Law of the Four Scales, and the conception of the Extra-spatial Unit—which have as yet been not much more than *hinted* to the public—MIGHT have led (I do not at all mean that *in my hands* they ever *would* have done so) to an ANTICIPATION of the great discovery of OERSTED."

"It appears to me that one, and not the least, of the services which Quaternions may be expected to do to mathematical analysis generally, is that their introduction will *compel* those who adopt them,—or even who admit that they *may* be reasonably adopted by other persons—to consider, or to admit, that others may uesfully inquire, *what common grounds* can be established, for *conclusions common to Quaternions* and to older branches of mathematics."

"*Could* anything be simpler, or more satisfactory? Don't you *feel*, as well as think, that we are on a *right track*, and shall be *thanked* hereafter? Never mind when."

tions, and fighting the self-same battles under slightly different banners, which is merely another form of stagnation (Kinetic Stability in fact). "A little folding of the hands to sleep" in chuckling satisfaction at what has been achieved of late years by our great experimenters—and we shall be left hopelessly behind. The sad fate of Newton's successors ought ever to be a warning to us. Trusting to what he had done, they allowed mathematical science almost to die out in this country, at least as compared with its immense progress in Germany and France. It required the united exertions of late Sir J. Herschel and many others to render possible in these islands a Boole and a Hamilton. If the successors of Davy and Faraday pause to ponder even on *their* achievements, we shall soon be again in the same state of ignominious inferiority. Who will then step in to save us?

Even as it is, though we have among us many names quite as justly great as any that our rivals can produce, we have also (even in our educated classes) such an immense amount of ignorance and consequent credulity, that it seems matter for surprise that true science is able to exist. Spiritualists, Circle-squarers, Perpetual-motionists, believers that the earth is flat and that the moon has no rotation, swarm about us. They certainly multiply much faster than do genuine men of science. This is characteristic of all inferior races, but it is consolatory to remember that in spite of it these soon become extinct. Your quack has his little day, and disappears except to the antiquary. But in science nothing of value can ever be lost; it is certain to become a stepping-stone on the way to further truth. Still, when our stepping-stones are laid, we should not wait till others employ them. "Gentlemen of the Guard, be kind enough to fire first" is a courtesy entirely out of date; with the weapons of the present day it would be simply suicide.

There is another point which should not be omitted in an address like this. For obvious reasons I must speak of the general question only, not venturing on examples, though I could give many telling ones. Even among our greatest men of science in this country, there is comparatively little knowledge of what has been already achieved, except, of course, in the one or more special departments cultivated by each individual. There can be little doubt that one cause at least of this is to be sought in the extremely meagre interest which our statesmen, as a rule, take in scientific progress. While abroad we find half a dozen professors teaching parts of the same subject in one University—each having therefore reasonable leisure—with us one man has to do the whole, and to endeavour as he best can to make something out of his very few spare moments. Along with this, and in great part due to it, there is often found a proneness to believe that what seems evident to the thinker cannot but have been long known to others. Thus the credit of many valuable discoveries is lost to Britain because her philosophers, having no time to spare, do not know that they are discoveries. The scientific men of other nations are, as a rule, better informed (certainly far better encouraged, and less over-worked), and perhaps likewise are not so much given to self-depreciation. Until something resembling the "Fortschritte der Physik," but in an improved form, and published at smaller intervals and with much less delay, is established in this country, there is little hope of improvement in this respect. Why should science be imperfectly summarised in little haphazard scraps here and there, when mere property has its elaborate series of money articles and exact broker's share lists? Such a work would be very easy of accomplishment; we have only to begin boldly—we do not need to go back, for in every year good work is being done at almost every part of the boundary between, as it were, the cultivated land and the still unpenetrated forest—enough at all events to show with all necessary accuracy whereabouts that boundary lies.

There is no need to enter here on the question of Conservation of Energy. It is thoroughly accepted by scientific men, and has revolutionised the greater part of physics. The facts as to its history also are generally agreed upon, but differences of a formidable kind exist as to the deductions to be drawn from them. These are matters, however, which will be more easily disposed of thirty years hence than now. The Transformation of Energy is also generally accepted, and, in fact, under various unsatisfactory names was almost popularly known before the Conservation of Energy was known in its entirety to more than a very few. But the Dissipation of Energy is by no means well known—and many of the results of its legitimate application have been received with doubt, sometimes even with attempted ridicule. Yet it appears to be at the present moment by far the most promising and fertile portion of Natural Philosophy; having obvious applications of which as yet only a small percentage appear to have been made. Some indeed were made before the enunciation of the principle, and have since been recognised as instances of it. Of such we have good examples in Fourier's great work on Heat-conduction, in the optical theorem that an image can never be brighter than the object, in Gauss's mode of investigating electrical distribution, and in some of Thomson's theorems as to the energy of an electromagnetic field. But its discoverer has, so far as I know, as yet confined himself in its explicit application to questions of Heat-conduction and Restoration of Energy, Geological Time, the Earth's Rotation, and such like. Unfortunately his long-expected Rede Lecture has not yet been published, and its contents (save to those who were fortunate enough to hear it) are still almost entirely unknown.

But there can be little question that the Principle contains implicitly the whole theory of Thermo-electricity, of Chemical Combination, of Allotropy, of Fluorescence, &c., and perhaps even of matters of a higher order than common physics and chemistry. In Astronomy it leads us to the grand question of the *age*, or perhaps more correctly the *phase of life*, of a star or nebula, shows us the material of potential suns, other suns in the process of formation, in vigorous youth, and in every stage of slowly protracted decay. It leads us to look on each planet and satellite as having been at one time a tiny sun, a member of some binary or multiple group, and even now (when almost deprived, at least at its surface, of its original energy) presenting an endless variety of subjects for the application of its methods. It leads us forward in thought to the far-distant time when the materials of the present stellar system shall have lost all but their mutual potential energy, but shall in virtue of it form the materials of future larger suns with their attendant planets. Finally, as it alone is able to lead us, by sure steps of deductive reasoning, to the necessary future of the universe—necessary, that is, if physical laws for ever remain unchanged—so it enables us distinctly to say that the present order of things has *not* been evolved through infinite past time by the agency of laws now at work—but must have had a distinctive beginning, a state beyond which we are totally unable to penetrate, a state in fact which must have been produced by other than the now acting causes.

Thus also, it is possible that in Physiology it may ere long lead to results of a different and much higher order of novelty and interest than those yet obtained, immensely valuable though they certainly are.

It was a grand step in science which showed that just as the consumption of fuel is necessary to the working of a steam-engine, or to the steady light of a candle, so the living engine requires food to supply its expenditure in the forms of muscular work and animal heat. Still grander was Rumford's early anticipation that the animal is a more economic engine than any lifeless one we can construct. Even in the explanation of this there is involved a question of very great interest, still unsolved, though Joule and many other philosophers of the highest order have worked at it. Joule has given a suggestion of great value, viz., that the animal resembles an electromagnetic rather than a heat-engine; but this throws us back again upon our difficulties as to the nature of electricity. Still, even supposing this question fully answered, there remains another—perhaps the highest which the human intellect is capable of directly attacking, for it is simply preposterous to suppose that we shall ever be able to understand scientifically the source of consciousness and volition, not to speak of loftier things—there remains the question of Life. Now it may be startling to some of you, especially if you have not particularly considered the matter, to hear it surmised that possibly we may, by the help of physical principles, especially that of the Dissipation of Energy, some time attain to a notion of what constitutes Life—mere Vitality I repeat, nothing higher. If you think for a moment of the vitality of a plant or a zoophyte, the remark, perhaps, will not appear so strange after all. But do not fancy that the Dissipation of Energy to which I refer is at all that of a watch or such-like piece of mere human mechanism, dissipating the low and common form of energy of a single coiled spring. It must be such that every little part of the living organism has its own store of energy constantly being dissipated, and as constantly replenished from external sources drawn upon by the whole arrangement in their harmonious working together. As an illustration of my meaning, though an extremely inadequate one, suppose Vaucanson's Duck to have been made up of excessively small parts, each microscopically constructed as perfectly as was the comparatively coarse whole, we should have had something barely distinguishable save by want of instincts from the living model. But let no one imagine that, should we ever penetrate this mystery, we shall thereby be enabled to produce, except from life, even the lowest form of life. Our President's splendid suggestion of Vortex-atoms, if it be correct, will enable us thoroughly to understand matter, and mathematically to investigate all its properties. Yet its very basis implies the absolute necessity of an intervention of Creative Power to form or to destroy one atom even of dead matter. The question really stands thus: Is Life physical or no? For if it be in any sense, however slight or restricted, physical, it is to that extent a subject for the Natural Philosopher, and for him alone. It would be entirely out of place for me to discuss such a question as this now and here; I have introduced it merely that I may say a word or two about what has been so often and so persistently croaked against the British Association, viz. that it tends to develope what are called Scientific Heresies. No doubt such charges are brought more usually against other Sections than against this; but Section A has not been held blameless. It seems to me that the proper answer to all such charges will be very simply and easily given, if we merely show that in our reasonings from observation and experiment we invariably confine our physical conclusions strictly to matter and energy (things which we can weigh and measure) in their multiform combinations. Excepting that which is obviously purely mathematical, whatever is certainly neither matter nor energy, nor dependent upon these, *is not a subject to be discussed here*, even by implication. All our reasonings in Physics *must*, so far as we know, be based upon the assumption founded on experience, that in the universe, whatever be the epoch or the locality, under exactly similar circumstances exactly similar results will be obtained. If this be not granted there is an end of Physical Science, or rather, there never could have been such a Science.* To use the word "Heresy" with reference to purely physical reasonings about Geological Time, or matters of that kind, is nowadays a piece of folly from which even Galileo's judges, were they alive, would shrink, as calculated to damage none but themselves and the cause which of old they, according to their lights, very naturally maintained.

There must always be wide limits of uncertainty (unless we choose to look upon Physics as a necessarily finite Science) concerning the exact boundary between the Attainable and the Unattainable. One herd of ignorant people, with the sole *prestige* of rapidly increasing numbers, and with the adhesion of a few fanatical deserters from the ranks of Science, refuse to admit that all the phenomena even of ordinary dead matter are strictly and exclusively in the domain of physical science. On the other hand, there is a numerous group, not in the slightest degree entitled to rank as Physicists—though in general they assume the proud title of Philosophers—who assert that not merely Life, but even Volition and Consciousness are mere physical manifestations. These opposite errors, into neither of which it is possible for a genuine scientific man to fall, so long at least as he retains his reason, are easily seen to be very closely allied. They are both to be attributed to that Credulity which is characteristic alike of Ignorance and of Incapacity. Unfortunately there is no cure—the case is hopeless—for great ignorance almost necessarily presumes incapacity, whether it shows itself in the comparatively harmless folly of the Spiritualist, or in the pernicious nonsense of the Materialist.

Alike condemned and contemned, we leave them to their proper fate—oblivion; but still we have to face the question:—where to draw the line between that which is physical and that which is utterly beyond physics. And again, our answer is—Experience alone can tell us; for experience is our only possible guide. If we attend earnestly and honestly to its teachings, we shall never go far astray. Man has been left to the resources of his intellect for the discovery not merely of physical laws, but of how far he is capable of comprehending them. And our answer to those who denounce our legitimate studies as heretical is simply this:—A revelation of anything which we can discover for ourselves, by studying the ordinary course of nature, would be an absurdity.

* It might be possible, and, if so, perhaps interesting, to speculate on the results of secular changes in physical laws, or in particles of matter which are subject to them, but (so far as experience, which is our *only* guide has taught us since the beginning of modern science) there seems no trace of such. Even if there were, as these changes must be of necessity extremely slow (because not yet even suspected) we may reasonably expect from the analogy of the history of such a question as gravitation, especially in the discovery of Neptune, that our work, far from becoming impossible, will merely become considerably more difficult as well as more laborious, but on that account, all the more creditable when successfully carried out.

A profound lesson may be learned from one of the earliest little papers of our President, published while he was an undergraduate at Cambridge, where he shows that Fourier's magnificent treatment of the Conduction of Heat leads to formulæ for its distribution which are intelligible (and of course capable of being fully verified by experiment) for all time future, but which, except in particular cases, when extended to time past, remain intelligible for a finite period only, and then indicate a state of things which could not have resulted under known laws from any conceivable previous distribution. So far as heat is concerned, modern investigations have shown that a previous distribution of the matter involved may, by its potential energy, be capable of producing such a state of things at the moment of its aggregation; but the example is now adduced, not for its bearing on heat alone, but as a simple illustration of the fact that all portions of our science, and especially that beautiful one the Dissipation of Energy, point unanimously to a beginning, to a state of things incapable of being derived by present laws from any conceivable previous arrangement.

I conclude by quoting some noble words used by Stokes in his Address at Exeter, words which should be stereotyped for every meeting of this Association:—"When from the phenomena of life we pass on to those of mind, we enter a region still more profoundly mysterious. . . . Science can be expected to do but little to aid us here, since the instrument of research is itself the object of investigation. It can but enlighten us as to the depth of our ignorance, and lead us to look to a higher aid for that which most nearly concerns our well-being."

SECTION B.

CHEMICAL SCIENCE.

OPENING ADDRESS BY THE PRESIDENT, DR. ANDREWS, F.R.S.

AMIDST the vicissitudes to which scientific theories are liable, it was scarcely to be expected that the discarded theory of phlogiston should be resuscitated in our day, and connected with one of the most important generalisations of modern science. The phlogistic theory, elaborated nearly two hundred years ago, by Beccher and Stahl, was not, it now appears, wholly founded on error; on the contrary, it was an imperfect anticipation of the great principle of energy, which plays so important a part in physical and chemical changes. The disciple of phlogiston, ignorant of the whole history of chemical combination, connected, it is true, his phlogiston with one only of the combining bodies, instead of recognising that it is eliminated by the union of all. "There can be no doubt," says Dr. Crum Brown, who first suggested this view, "that potential energy is what the chemists of the seventeenth century, meant when they spoke of phlogiston." "Phlogiston and latent heat," playfully remarks Volhard, "which formerly opposed each other in so hot a combat, have entered into a peaceful compact, and to banish all recollection of their former strife, have assumed in common the new name of energy." But as Dr. Odling well remarks, "In interpreting the phlogistic writings by the light of modern doctrine, we are not to attribute to their authors the precise notion of energy which now prevails. It is only contended that the phlogistians had in their time possession of a real truth in nature, which, altogether lost sight of in the intermediate period, has since crystallised but in a definite form."

But whatever may be the true value of the Stahlian views, there can be no doubt that the discoveries which have shed so bright a lustre round the name of Black, mark an epoch in the history of science, and gave a mighty impulse to human progress. A recent attempt to ignore the labours of Black and his great contemporaries, and to attribute the foundation of modern chemistry to Lavoisier alone, has already been amply refuted in an able inaugural address delivered a short time ago from the chair formerly occupied by Black. The statements of Dr. Crum Brown may indeed be confirmed on the authority of Lavoisier himself. Through the kindness of Dr. Black's representatives, I have been permitted to examine his correspondence which has been carefully preserved, and I have been so fortunate as to find in it three original letters from Lavoisier to Dr. Black. They were written in 1789 and 1790, and they appear to comprise the whole of the correspondence on the part of Lavoisier which passed between those distinguished men. Some extracts from these letters were published soon after Dr. Black's death by his friends, Dr. Adam Ferguson and Dr. Robison; but the letters themselves, as far as I know, have never appeared in an entire form. I will crave permission to have them printed as an appendix to this address. Lavoisier, it will be seen, addresses Black as one whom he was accustomed to regard as his master, and whose discoveries had produced important revolutions in science. It may indeed be said with truth that Lavoisier completed the foundation on which the grand structure of modern chemistry has since arisen; but Black, Priestley, Scheele, and Cavendish were before Lavoisier, and their claims to a share in the great work are not inferior to those of the illustrious French chemist.

Among the questions of general chemistry, few are more interesting, or have of late attracted more attention, than the relations which subsist between the chemical composition, and refractive power of bodies for light. Newton, it will be remembered, pointed out the distinction between the refractive power of a medium and its refractive index, and gave for the former the expression $\frac{\mu^2-1}{d}$ where μ is the refractive index, and d the density of the refracting medium. Sir J. Herschel, anticipating later observations, remarked in 1830 that Newton's function only expresses the intrinsic refractive power on the supposition of matter being infinitely divisible, but that if material bodies consist of a finite number of atoms differing in weight for different substances, the intrinsic refractive power of the atoms of any given medium will be the product of the above function by the atomic weight. The same remark has since been made by Bertholet. Later observations have led to an important modification in the form of Newton's function. Beer showed that the experiments of Biot and Arago, as well as those of Dulong on the refractive power of gases, agree quite as well with a simpler expression as with that given by Newton; and Gladstone and Dale proposed in 1863 the formula $\frac{\mu-1}{d}$ as expressing more accurately than any other, the results of their experiments on the refractive power of the liquids. The researches of Landolt and Wüllner have fully confirmed the general accuracy of the new formula. An important observation made, about twenty years ago by Delffs, has been the starting point for all subsequent investigations on this subject. Delffs remarked that the refractive indices of the compound ethers increase with the atomic weight, and that isomeric ethers have the same refractive indices. The later researches of Gladstone and of Landolt have, on the whole, confirmed these observations, and have shown that the specific refractive power depends chiefly on the atomic composition of the body, and is little influenced by the mode of grouping of the atoms. These inquiries have gone further, and have led to the discovery of the refraction equivalents of the elements. By comparing the refractive power of compound bodies differing from one another by one or more atoms of the same element, Landolt succeeded in obtaining numbers which express the refraction equivalents of carbon, hydrogen and oxygen, and corresponding numbers have been obtained for other elements by Gladstone and Haagen. The whole subject has been recently discussed and enriched with many new observations in an able memoir by Gladstone. As might be expected in so novel and recondite a subject, some anomalies occur which are difficult to explain. Thus hydrogen appears in different classes of compounds with at least two refraction equivalents—one three times as great as the other, and the refraction equivalents of the aromatic compounds and their derivations as given by observation are, in general, higher than the calculated numbers.

A happy modification of the ice calorimeter has been made by Bunsen. The principle of the method—to use as a measure of heat the change of volume which ice undergoes in melting—had already occurred to Herschel, and, as it now appears, still earlier to Hermann; but their observations had been entirely overlooked by physicists, and had led to no practical result. Bunsen has indeed clearly pointed out that the success of the method depends upon an important condition which is entirely his own. The ice to be melted must be prepared with water free from air, and must surround the source of heat in the form of a solid cylinder frozen artificially *in situ*. Those who have worked on the subject of heat know how difficult it is to measure absolute quantities with certainty, even where relative results of great accuracy may be attained. The ice calorimeter of Bunsen will therefore be welcomed as an important addition to our means of research. Bunsen has applied his method to determine the specific heats of ruthenium, calcium, and indium; and finds that the atomic weight of indium must be increased by one-half in order to bring it into conformity with the law of Dulong and Petit. He has also made a new determination of the density of ice, which he finds to be 0·9167.

In a Report on the Heat of Combination which was made to this Association in 1849, the existence of a group of isothermal bases was pointed out. "As some of the bases—potash, soda, baryta, strontia—" it was remarked, "form what we may perhaps designate an isothermal group, such bases will develop the same, or nearly the same heat in combining with an acid, and no heat will be disengaged during their mutual displacements." The latest experiments of Thomsen have given a remarkable extension to this group of isothermal bases. He finds that the hydrates of lithium, thallium, calcium and magnesium produce, when all corrections are made, the same amount of heat on being neutralised by sulphuric acid, as the four bases before mentioned. The hydrate of tetramethylammonium belongs to the same class of bases. Ethylamin, on the other hand, agrees with ammonia, which, as has long been known, gives out less heat in combining with the acids than potash or soda. An elaborate investigation of the amount of heat evolved in the combustion of coal of different kinds has been made by Scheurer-Kestner and Meusnier, accompanied by analyses of the coal. Coal rich in carbon and hydrogen disengages more heat in burning than coal in which those elements are partially replaced by oxygen. After deducting the cinders, the heat produced by the combustion of 1 gramme of coal varied from 8215 to 9622 units.

Tyndall has given an extended account of his experiments on the action of a beam of strong light on certain vapours He finds that there is a marked difference in the absorbing power of different vapour for the actinic rays. Thus the nitrate of amyl in the state of vapour absorbs rapidly the rays of light competent to decompose it, while iodide of allyl in the same state allows them freely to pass. Morren has continued these experiments in the south of France, and among other results he finds that sulphurous acid is decomposed by the solar beam.

Roscoe has prosecuted the photo-chemical investigations which Bunsen and he began some years ago. For altitudes above 10 degrees the relation between the sun's altitude and the chemical intensity of light is represented by a straight line. Till the sun has reached an altitude of about 20 degrees, the chemical action produced by diffused daylight exceeds that of the direct sunlight. The two actions are then balanced; and at higher elevations the direct sunlight is superior to the diffused light. The supposed inferiority of the chemical action of light under a tropical sun to its action in higher latitudes proves to be a mistake. According to Roscoe and Thorpe, the chemical intensity of light at Para under the equator in the month of April is more than three times greater than at Kew in the month of August.

Hunter has given a great extension to the earlier experiments of Saussure on the absorptive power of charcoal for gases. Cocoa-nut charcoal, according to Hunter's experiments, exceeds all other varieties of wood charcoal in absorptive power, taking up at ordinary pressures 170 volumes of ammonia and 69 of carbonic acid. Methylic alcohol is more largely absorbed than any other vapour at temperatures from 90° to 127°; but at 159°, the absorption of ordinary alcohol exceeds it. Cocoa-nut charcoal absorbs 44 times its volumes of the vapour of water at 127°. The absorptive power is increased by pressure.

Last year two new processes for improving the manufacture of chlorine attracted the attention of the section; one of these has already proved to be a success, and I am glad to be able to state that Mr. Deacon has recently overcome certain difficulties in his method, and has obtained a complete absorption of the chlorine. May we hope to see oxygen prepared by a cheap and continuous process from atmospheric air? With baryta the problem can be solved very perfectly, if not economically. Another process is that of Tessier de Mothay, in which the manganate of potassium is decomposed by a current of superheated steam, and afterwards revived by being heated in a current of air. A company has lately been formed in New York to apply this process to the production of a brilliant house-light. A compound argand burner is used, having a double row of apertures,—the inner row is supplied with oxygen, the outer with coal gas or other combustible. The applications of pure oxygen, if it could be produced cheaply, would be very numerous, and few discoveries would more amply reward the inventor. Among other uses, it might be applied to the production of ozone free from nitric acid by the action of the electrical discharge, and to the introduction of that singular body in an efficient form into the arts as a bleaching and oxidising agent. Tessier de Mothay has also proposed to prepare hydrogen gas on the large scale by heating hydrate of lime with anthracite.

We learn from the history of metallurgy that the valuable alloy which copper forms with zinc was known and applied long before zinc itself was discovered. Nearly the same remark may be made at present with regard to manganese and its alloys. The metal is difficult to obtain, and has not in the pure state been applied to any useful purpose; but its alloys with copper and other metals have been prepared, and some of them are likely to be of great value. The alloy with zinc and copper is used as a substitute for German silver, and possesses some advantages over it. Not less important is the alloy of iron and manganese prepared according to the process of Henderson, by reducing in a Siemen's furnace a mixture of carbonate of manganese and oxide of iron. It contains from 20 to 30 per cent. of manganese, and will doubtless replace, to a large extent, the spiegeleisen now used in the manufacture of Bessemer steel.

The classical researches of Roscoe have made us acquainted, for the first time, with metallic vanadium. Berzelius obtained brilliant scales which he supposed to be the metal, by heating an oxychloride in ammonia, but they have proved to be a nitride. Roscoe prepared the metal by reducing its chloride in a current of hydrogen, as a light gray powder, with a metallic lustre under the microscope. It has a remarkable affinity both for nitrogen and silicon. Like phosphorus, it is a pentad, and the vanadates correspond in composition to the phosphates, but differ in the order of stability at ordinary temperatures, the soluble tribasic salts being less stable than the tetrabasic compounds.

Sainte-Claire Deville, in continuation of his researches on dissociation, has examined the conditions under which the vapour of water is decomposed by metallic iron. The iron maintained at a constant temperature, but varying in different experiments from 150° C. to 1600° C., was exposed to the action of the vapour of water of known tension. It was found that for a given temperature the iron continued to oxidise, till the tension of the hydrogen formed reached an invariable value. In these experiments, as Deville remarks, the iron behaves as if it emitted a vapour (hydrogen) obeying the laws of hygrometry. An interesting set of experiments has been made by Lowthian Bell on the power possessed by spongy metallic iron of splitting up carbonic oxide into carbon and carbonic acid, the former being deposited in the iron. A minute quantity of oxide of iron is always formed in this reaction.

The fine researches of Graham on the colloidal state have received an interesting extension by Reynolds's discovery of a new group of colloid bodies. A solution of mercuric chloride is added to a mixture of acetone and a dilute solution of potassium hydrate, till the precipitate which at first appears is redissolved, and the clear liquid poured upon a dialyser which floated upon water. The composition of the colloid body thus obtained in the anhydrous state was found to be $(CH_3)_2 (CO)_2 Hg_3 O_3$. The hydrate is regarded by Reynolds as a feeble acid even more readily decomposed than alkaline silicates. A solution containing only five per cent. forms a firm jelly when heated to 50° C. Analogous compounds were formed with the higher members of the fatty kenone series. In the same direction are the researches of Marcet on blood, which he finds to be a strictly colloid fluid containing a small proportion of diffusable salts.

In organic chemistry the labours of chemists have been of late largely directed to a group of hydrocarbons which were first discovered among the products of the destructive distillation of coal or oil. The central body round which these researches have chiefly turned is benzol, whose discovery will always be associated with the name of Faraday. With this body naphthaline and anthracene form a series, whose members differ by $C_4 H_2$, and their boiling points by about 140°. The recent researches of Liebermann have proved, as was before suspected, that chrysene is a fourth member of the same series. I may add that ethylene, which boils at about 70°, corresponds in composition and boiling point to a lower member of the same series. Kekulé propounded some time ago with great clearness the question as to whether the six atoms of hydrogen in benzol are equivalent, or on the contrary play dissimilar parts. According to the first hypothesis, there can be only one modification of the mono- and penta-derivatives of benzol; while three modifications of the bi-, tri-, and tetra-derivatives are possible. On the second hypothesis, two modifications of the mono-derivatives are possible, and in general a much larger number of isomeric compounds than on the first hypothesis. Such is the problem which has of late occupied the attention of some of the ablest chemists of Germany, and has led to a large number of new and important investigations. The aromatic hydrocarbons, toluol, xylol, &c., which differ from one another by $C H_2$, have been shown by Fittig to be methyl derivatives of benzol. According to the first of the two hypotheses to which I have referred, only one benzol and one methyl benzol (toluol) are possible, and accordingly no isomeric modifications of these bodies have been discovered. But the three following members of the series ought each to be capable of existing in three distinct isomeric forms. The researches of Fittig had already established the existence of two isomeric compounds having the formula $C_8 H_{10}$—methyl toluol obtained synthetically from toluol, and isoxylol prepared by the removal of an atom of methyl from the mesytelene of Kane. The same chemist has since obtained the third modification, orthoxylol, by the decomposttion of paraxylylic acid. These three isomeric hydrocarbons may be readily distinguished from one another by the marked difference in the properties of their tri-nitro-compounds, and also by their different behaviour with oxydising agents. Other facts have been adduced in support of the equality or homogeneity of position of the hydrogen atoms in benzol. Thus Hübner and Alsberg have prepared aniline, a mono-derivative from different bi-derivatives, and have always obtained the same body. The latest researches on this subject are those of Richter.

Baeyer has prepared artificially picoline, a base isomeric with aniline, and discovered by Anderson in his very able researches on the pyridine series. Of the two methods described by Baeyer, one is founded on an experiment of Simpson, in which a new base was obtained by heating tribromallyl with an alcoholic solution of ammonia. By pushing further the action of the heat, Baeyer succeeded in expelling the whole of the bromine from Simpson's base in the form of hydrobromic acid, and in obtaining picoline. The same chemist has also prepared artificially collodine, another base of the pyridine series. To this list of remarkable synthetical discoveries, another of the highest interest has lately been added by Schiff—the preparation of artificial coniine. He obtained it by the action of ammonia on butyric aldehyde (C_4H_8O). The artificial base has the same composition as coniine prepared from hemlock. It is a liquid of an amber-yellow colour, having the characteristic odour and nearly all the ordinary reactions of ordinary coniine. Its physiological properties, so far as they have been examined, agree with those of coniine from hemlock, but the artificial base has not yet been obtained in large quantity, nor perfectly pure.

Valuable papers on alizarine have been published by Perkin and Schunck. The latter has described a new acid—the anthraflavic—which is formed in the artificial preparation of alizarine. Madder contains another colouring principle, purpurine, which, like alizarine, yields anthracene when acted on by reducing agents, and has also been prepared artificially. These colouring principles may be distinguished from one another, as Stokes has shown, by their absorption bands; and Perkin has lately confirmed by this optical test the interesting observation of Schunck, that finished madder prints contain nothing but pure alizarine in combination with the mordaunt employed.

Hofmann has achieved another triumph in a department of chemistry which he has made peculiarly his own. In 1857 he showed that alcohol bases, analogous to those derived from ammonia, could be obtained by replacement from phosphuretted hydrogen; but he failed in his attempts to prepare the two lower derivatives. These missing links he has now supplied, and has thus established a complete parallelism between the derivatives of ammonia and of phosphuretted hydrogen. The same able chemist has lately described the aromatic cyanates, of which one only, the phenylic cyanate (CO, C_6H_5, N), was previously known, having been discovered about twenty years ago by Hofmann himself. He now prepares this compound by the action of phosphoric anhydride on phenylurethane, and by a similar method he has obtained the tolylic, xylylic and naphthylic cyanates.

Stenhouse had observed many years ago that when aniline is added to furfurol, the mixture becomes rose-red, and communicates a fugitive red stain to the skin, and also to linen and silk. He has lately resumed the investigation of this subject, and has obtained two new bases, furfuraniline and furfurtoluidine, which, like roseaniline, form beautifully coloured salts, although the bases themselves are nearly colourless or of a pale brown colour. The furfuraniline hydrochlorate ($C_{17} H_{19} O_3 N_2 Cl$) is prepared by adding furfurol to an alcoholic solution of aniline hydrochlorate containing an excess of aniline. We have also from Stenhouse a new contribution to the history of orcin, in continuation of his former masterly researches on that body. He has prepared the trinitroorcin ($C_7 H_5 (NO_2)_3 O_2$), a powerful acid having many points of resemblance to picric acid. In connection with another research of Stenhouse, made many years ago, it is interesting to find his formula for esexanthron, which was also that of Erdmann, confirmed by the recent experiments of Baeyer.

The interesting work of Dewar on the oxidation of picoline must not be passed over without notice. By the action of the permanganate of potassium on that body, he has obtained a new acid, which bears the same relation to pyridine that phthalic acid does to benzol. Thorpe and Young have published a preliminary notice of some results of great promise, which they have obtained by exposing paraffin to a high temperature in closed vessels. By this treatment it is almost completely resolved into liquid hydrocarbons, whose boiling points range from 18° C. to 300° C.; those boiling under 100° C. have been examined, and consist chiefly of olefines. In connection with this subject, it may be interesting to recall the experiments of Pelouze and Cahours on the Pennsylvanian oils, which proved to be a mixture of carbohydrogens belonging to the marsh-gas series.

An elaborate exposition of Berthelot's method of transforming an organic compound into a hydrocarbon containing a maximum of hydrogen, has appeared in a connected form. The organic body is heated in a sealed tube, with a large excess of a strong solution of hydriodic acid, to the temperature of 250°. The pressure in these experiments Berthelot estimated at 100 atmospheres, but apparently without having made any direct measurements. He has thus prepared ethyl hydride ($C_2 H_6$) from alcohol, aldehyde, &c.; hexyl hydride ($C_6 H_{14}$) from benzol. Berthelot has submitted both wood charcoal and coal to the reducing action of hydriodic acid, and, among other interesting results, he claims to have obtained in this way oil of petroleum.

By the action of chloride of zinc upon codeia, Matthiessen and Burnside have obtained apocodeia, which stands to codeia in the same relation as apomorphia to morphia, an atom of water being abstracted in its formation. Apocodeia is more stable than apomorphia, but the action of reagents upon the two bases is very similar. As regards their physiological action, the hydrochlorate of apocodeia is a mild emetic, while that of apomorphia is an emetic of great activity. Other bases have been obtained by Wright by the action of hydrobromic acid on codeia. In two of these bases, bromotetracodeia and chlorotetracodeia, four molecules of codeia are welded together so that they contain no less than 72 atoms of carbon. They have a bitter taste, but little physiological action. The authors of these valuable researches were indebted to Messrs. Macfarlane for the precious material upon which they operated.

We are indebted to Crum Brown and Fraser for an important work on a subject of great practical as well as theoretical interest, the relation between chemical constitution and physiological action. It has long been known that the ferrocyanide of potassium does not act as a poison on the animal system, and Bunsen has shown that the kakodylic acid, an arsenical compound, is also inert. Crum-Brown and Fraser find that the methyl compounds of strychnia, brusia, and thebaia are much less active poisons than the alkaloids themselves, and the character of their physiological action is also different. The hypnotic action of sulphate of methyl-morphium is less than that of morphia. But a reverse result occurs in the case of atropia, whose methyl and ethyl derivatives are much more poisonous than the salts of atropia itself.

Before proceeding to the subject of fermentation, I may refer to Apjohn's chemico-optical method of separating cane sugar, inverted sugar, and grape sugar from one another when present in the same solution, by observing the rotative power of the syrup before and after inversion, and combining the indications of the saccharometer with the results of an analysis of the same syrup after inversion. Heisch's test for sewage in ordinary water is also deserving of notice. It consists in adding a few grains of pure sugar to the water, and exposing it freely to light for some hours, when the liquid will become turbid from the formation of a well-marked fungus, if sewage to the smallest amount be present. Frankland has made the important observation that the development of this fungus depends upon the presence of a phosphate, and that if this condition be secured, the fungus will appear even in the purest water.

The nature of fermentation, and in particular of the alcoholic fermentation, has been lately discussed by Liebig with consummate ability, and his elaborate memoir will well repay a careful perusal. Dr. Williamson has also given a most instructive account of the subject, particularly with reference to the researches of Pasteur, in his recent Cantor lectures. A brief statement of the present position of the question will therefore not be out of place here. It is now 34 years since Cagniard de la Tour and Schwann proved by independent observations that yeast globules are organised bodies capable of reproduction by gemmation; and also inferred as highly probable that the phenomena of fermentation are induced by the development or living action of these globules. These views, after having fallen into abeyance, were revived and extended a few years ago by Pasteur, whose able researches are familiar to every chemist. Pasteur, while acknowledging that he was ignorant of the nature of the chemical act, or of the intimate cause of the splitting up of sugar in the alcoholic fermentation, maintained that all fermentations, properly so called, are co-relative with physiological phenomena. According to Liebig, the developement and multiplication of the yeast plant, or fungus, is dependent upon the presence and absorption of nutriment which becomes part of the living organism, while in the process of fermentation, an external action takes place upon the substance, and causes it to split up into products which cannot be made use of by the plant. The vital process and the chemical action, he asserts, are two phenomena which in the explanation must be kept separate from one another. The action of a ferment upon a fermentable body he compares to the action of heat upon organic molecules, both of which cause a movement in the internal arrangement of the atoms. The phenomena of fermentation Liebig refers now as formerly to a chemico-physical cause, the action, namely, which a substance in a state of molecular movement exercises upon another of highly complex constitution, whose elements are held together by a feeble affinity, and are to some extent in a state of tension or strain. Baeyer, who considers that in the alcoholic and lactic fermentations one part of the compound is reduced and another oxidised, adopts the view of Liebig that the molecules of sugar which undergo fermentation do not serve for the nourishment of the yeast plant, but receive an impulse from it. All are however agreed that fermentation is arrested by the death of the plant, and even a tendency to the acetous fermentation in wine may be checked, as Pasteur has shown, by heating the wine to a temperature a little below boiling point in the vessel in which it is afterwards to be kept.

I regret that the limits of an address like the present forbid me to pursue further this analysis of chemical work. Had they admitted of abridgment I should gladly have described the elaborate experiments of Gore on hydro-fluoric acid and the fluoride of silver. The important researches of Abel on explosive compounds will be explained by himself in a lecture with which he has kindly undertaken to favour the Association. Mr. Tomlinson will also communicate to the section some observations on catharism and nuclei, a difficult subject to which he has of late devoted much attention. And I am also informed that we shall have important papers on recent improvements in chemical mauufactures.

No one can be more painfully alive than myself to the serious omissions in the historical review I have now read, more particularly in organic chemistry, where it was wholly impossible to grapple with the large number of valuable works which even a few months produce. I cannot, however, refrain from bearing humble tribute to the great ability and indomitable perseverance which characterise the labourers in the great field of organic chemistry. It would scarcely be possible to conceive any work more intelligently undertaken or more conscientiously performed than theirs, yet much of it, from its abstruse character, receiving little sympathy or encouragement except from the band of devoted men who have made this subject the chief pursuit of their lives. They will, however, find their reward in the consciousness that they have not lived in vain, but have been engaged, and successfully engaged, in the noble enterprise of extending for the benefit of the human family the boundaries of scientific knowledge. Nor is there any real ground for discouragement; Faraday, Graham, Magnus, and Herschel, who have left their impress on this age, were all distinguished chemical as well as physical discoverers; and the relations of the sciences are becoming every day so intimate that the most special research leads often to results of wide and general interest. No one felt this truth more clearly or illustrated it better than our lamented and distinguished friend, Dr. Miller, whose presence used to cheer our meetings, and whose loss we all most sincerely deplore.

SECTION C.

GEOLOGICAL SECTION

Opening Address by the President, Archibald Geikie, F.R.S.

Instead of offering to the Geological Section of the British Association an opening Address on some special aspect, or branch of general Geology, I have thought that it might be more interesting, and perhaps even more useful, if I were to lay before you an outline of the geology of the district in which we are now assembled. Accordingly, in the remarks which I am now about to make, I propose to sketch to you the broader features of the geological structure and history of Edinburgh and its neighbourhood, dwelling more especially on those parts which have more than a mere local interest, as illustrative of the general principles of our science.

It would be as unnecessary, as it would be out of place here, to cite the long array of authors who have each added to our knowledge of the geology of this district; and many of them also, at the same time, to the broad fundamental truths of Geology. And yet it would be strange to speak here of the rocks of Edinburgh without even a passing tribute of gratitude to men like Hutton, Hall, Jamieson, Hay Cunningham, Hibbert, Hugh Miller, Fleming, Milne Home, and our late esteemed and venerable associate, Charles Maclaren—men who have made the rocks of Edinburgh familiar to geologists all over the world. If, therefore, I make no further allusion to these and other names, it is neither that I forget for a moment their claims, nor that I now bring forward any new material of my own, but because I wish to be understood as dealing with facts which, thanks to the labours of our predecessors, have become part of the common stock of geological knowledge.

For the purpose of gaining as clear an idea as may be of the rocks among which Edinburgh lies, and of the way in which they are grouped together, let us imagine ourselves placed on the battlements of the Castle, where, by varying our position, we may obtain a clear view of the country in every direction for many miles round. To the south-east the horizon is bounded by a range of high ground, rising as a long table-land above the lowland of Midlothian. That is a portion of the wide Silurian uplands of the south of Scotland, forming here the chain of heights known as the Lammermuir and Moorfoot Hills. Along most of its boundary line, in this district, the Silurian table-land descends with tolerable rapidity towards the plain, being bounded on its north-west side with a long fault, by which the Carboniferous Rocks are brought down against the hills. These Silurian rocks are the oldest strata of the district; and it is on their contorted and greatly denuded beds that the later formations have been laid down.

Turning now to the south, we see the chain of heights known as the Pentland Hills, striking almost from the very suburbs of Edinburgh south-westward in the direction of the Silurian uplands, which they eventually reach in the county of Lanark. This line of hills rises along an anticlinal axis by which the broad Carboniferous tract of the Lothians is divided into two distinct portions. The Pentlands themselves consist, as I shall afterwards point out, chiefly of rocks of Old Red Sandstone age, but the anticlinal fold along which they rise is prolonged through the Braid Hills, and through the Carboniferous ground by the Castle Rock of Edinburgh, even as far as the opposite shores of Fife. From the Castle we can readily follow with the eye the effects of this great dominant fold of the rocks. To the east, we mark how the strata dip away eastward from the axis of movement, as is shown in the escarpments of Salisbury Crag, Arthur Seat, and Calton Hill's, while, on the opposite or western side, the escarpment of the wooded hill of Corstorphine, facing towards us, points out the westward dip. From the same stand-point we can even detect the passage of the arch into Fife, for the rocks about Aberdour are seen dipping to the west, while eastward, they bend over and dip towards the east, at Kinghorn.

Although the structure of the district is simple when the existence and position of this anticlinal axis is recognised, some little complication is introduced by a long powerful fault which flanks the axis on its south-eastern side. The effect of this fault is to throw out a great part of the lower division of the Carboniferous formations, and to bring the Carboniferous Limestone series in some places close against the Lower Old Red Sandstone and its volcanic rocks. Another result has been the extreme tilting of the strata, whereby the Limestone series along the east side of the fault, has been thrown on end, and even in some parts bent back into a reversed dip. Hence, while on one side of the axis, the Limestone series is sometimes only a few hundred yards distant from the Old Red Sandstone, on the opposite or north-west side, the distance is fully eleven miles, the intervening space being there occupied by endless undulations of the lower divisions of the Carboniferous system. Hence, too, the Millstone Grit and Coal-Measures come in along the centre of the Midlothian basin a short way to the east of the Pentland axis; while, on the west side, they are not met with till we reach the borders of Stirlingshire and Linlithgow.

Another remarkable and readily observable feature is that on the west side of the Pentland ridge, the Carboniferous formations from almost their base up to the top of the Carboniferous Limestone series, abound in contemporaneous volcanic rocks; while, on the east side, beyond Edinburgh and Arthur's Seat, such rocks are absent until we reach the Garlton Hills, to the north of Haddington, where they reappear, but in a very different type from that which they exhibit to the west.

Let us now pass in review the different geological formations which come into the district around us, beginning with the oldest and ascending through the others, till we reach the superficial accumulations, and mark, in conclusion, how far the present surface features are connected with geological structure.

[The author then describes the various geological formations of the district—Silurian, Old Red Sandstone, and Carboniferous—dwelling in particular upon the history of volcanic action in that part of Scotland. On this subject he remarks:—]

Outline of the History of Volcanic Action around Edinburgh

The oldest volcanoes of this part of Scotland were those which, during the time of the Lower Old Red Sandstone, poured out the great sheets of porphyrite and the showers of tuff which now form the main mass of the range of the Pentland Hills. During the same long geological period, volcanic action was rife, as we have seen, along the whole of the broad midland valley of Scotland, since to that time we must refer the origin of the Sidlaw and the Ochil Hills, part of eastern Berwickshire, and the long line of uplands stretching from the Pentland Hills through Lanarkshire, and across Nithsdale, far into Ayrshire.

Of volcanic action, during the remainder of the Old Red Sandstone period, there is around Edinburgh no trace. But early in the following or Carboniferous period, the volcano of Arthur's Seat and Calton Hill came into existence, and threw out its tiny flows of basalt and porphyrite, and its showers of ashes. From that time onwards, through nearly the whole of the interval occupied by the deposition of the Carboniferous Limestone series, the district to the west of Edinburgh was dotted over with small cones, usually of tuff, but sometimes emitting limited currents of different basalt rocks, more especially in the space between Bathgate and the Forth, where a long bank, chiefly formed of such lava-currents, was piled up over and among the pools and shallows in which the limestones, sandstones, shales, and coal-seams were accumulated. To the north, also, similar volcanic activity was shown in the Fife tracts nearest the Forth; while eastwards between Haddington and Dunbar there lay a distinct volcanic focus, where great showers of red felspathic tuff and wide-spread sheets of porphyrite were ejected to form a bank over which the Carboniferous Limestone series was at length tranquilly deposited.

Volcanic activity seems to have died out here before the close of the Carboniferous Limestone period. It remained quiescent during the deposition of the Millstone Grit and Coal-measures; at least, no trace of any contemporaneous igneous ejection is found in any part of these formations. The intrusive masses of various basalt rocks, which here intersect the older half of the Carboniferous system, are, in all probability, of Lower Carboniferous date, connected with the eruptions of the interbedded volcanic rocks. The next proofs of volcanic action in this neighbourhood are furnished by the upper part of Arthur's Seat. At that locality we discover that after more than 3000 feet of strata had been removed by denudation from the Pentland anticlinal fold so as to lay bare the old Lower Carboniferous volcanic rocks of Edinburgh, a new focus of eruption was formed, from which were ejected the basalts and coarse agglomerates of the summit and shoulders of Arthur's Seat. There is no trustworthy evidence for fixing the geological date of this eruption. Evidently, from the great denudation by which it was preceded, it must belong to a much later period than any of the Carboniferous eruptions. Yet, from the great similarity of the Arthur's Seat agglomerate, both in composition and mode of occurrence, to numerous "necks" which rise through all parts of the Carboniferous system between Nithsdale and Fife, and which I have shown to mark the position of volcanic orifices during Permian times, I am inclined to regard these later igneous rocks of Edinburgh as dating from the Permian period. Arthur's Seat, however, seems to have been the only volcano in action during that period in this neighbourhood.

There still remains for notice one further and final feature of the volcanic history of this part of Scotland. Rising indifferently through any part of the other rocks, whether aqueous or igneous, and marked by a singular uniformity of direction, there is a series of basalt dykes, which deserves attention. They have a general easterly and westerly trend, and even where, as in Linlithgowshire, they traverse tracts of basalt-rocks, they preserve their independence, and continue as readily separable as when they are found intersecting sandstones and shales. These dykes belong to that extensive series which, running across a great part of Scotland, the north of England, and the north-east of Ireland, passes into, and is intimately connected with, the wide basaltic plateaux of Antrim and the Inner Hebrides. They date, in fact, from Miocene times, and, from their numbers, their extent, and the distance to which they can be traced from the volcanic centre of the north-west, they remain as a striking memorial of the vigour of volcanic action during the last period of its manifestation in this country.

Glacial Phenomena

To an eye accustomed to note the characteristic impress of ice-action upon a land-surface – the neighbourhood of Edinburgh presents many features of interest. It was upon Corstorphine Hill, on the western outskirts of the city, that Sir James Hall first called attention to striated rock-surfaces which, though erroneously attributed to the abrasion produced by torrents of water, were even then recognised as trustworthy evidence of the last great geological changes that had passed over the surface of the country. Even before we come to look at the surface in detail, and note the striation of its rocks, we cannot fail to recognise the distinctively ice-worn aspect of the hills round Edinburgh. Each of them is, in fact, a great *roche moutonnée*, left in the path of the vast ice-sheet which passed across the land. That this ice was of sufficient depth and mass to override even the highest hills, is proved not merely by the general ice-worn surface of the landscape, but by the occurrence of characteristic striæ on the summits of the Pentland Hills, 1,600 feet above the sea; that it came from the Highlands, is indicated by the pebbles of granite, gneiss, schist, and quartz rock, occurring in the older boulder-clays which it produced; and that, deflected by the mass of the southern uplands, the ice in the valley of the Lothians was forced to move seawards, in a direction a little north of east, is shown by the trend of the striæ graven on the rocks, as at Corstorphine, Granton, Arthur's Seat, and Pentland Hills.

Connection of the present form of the surface with Geological Structure

In concluding these outlines, let me direct the attention of the Section to the bearing which the geological structure of the district wherein we are now assembled has upon the broad and much canvassed question of the origin of land-surfaces. In the first place, we cannot fail to be struck with the evidence of enormous denudation which the rocks of the district have undergone. Every formation, from the oldest to the latest, has suffered, and the process of waste has been going on apparently from the earliest times. We see that the Lower Silurian rocks were upheaved and denuded before the time of the Lower Old Red Sandstone; that the latter formation had undergone enormous erosion before the beginning of the Carboniferous period; that of the Carboniferous rocks, a thickness more than 3,000 feet had been worn away from the site of Arthur's Seat before the last eruptions of that hill, which are possibly as old as the Permian period; that still further and vaster denudation took place before the setting in of the Ice-age; and finally, that the deposits of that age have since been to a large extent removed. With the proofs, therefore, of such continued destruction, it would be vain to look for any aboriginal outline of the surface, or hope to find any of the later but still early features of the landscape remaining permanent amid the surrounding waste.

In the second place, we note, that in the midst of this greatly denuded area, it is the harder rocks which form the hills and crags. Those masses which in the long process of waste presented most resistance to the powers of destruction, are just those which, as we might expect, rise into eminences, while those whose resistance was least sink into plains and valleys. All the craggy heights which form so conspicuous a feature of Edinburgh and its neighbourhood, are composed of hard igneous rocks, the undulating lowlands lie upon soft aqueous rocks.

In the third place, the coincidence of the position of hills and crags with the existence of ancient igneous rocks, cannot be misinterpreted by inscribing the presence and form of the hills to the outlines assumed by the igneous material ejected to the surface from below. The hills are not due to igneous upheaval at all, but can be shown to have been buried deep under subsequent accumulations, to have been bent and broken with all the bendings and breaks these later formations underwent, and to have been finally brought to light again only after a long cycle of denudation had removed the mass of rock under which they had been concealed. What is true of the hills of Edinburgh, is true also of all the older volcanic districts of Britain. Even where the hills consist of volcanic rocks, their existence, as hills, can be proved to be one of the results not of upheaval, but of denudation.

In the fourth place, this district furnishes an instructive illustration of the influence of faults upon the external contour of a country. The faults here do not form valleys. On the contrary, the valleys have been cut across them in innumerable instances. In the Dalkeith coal-field, for example, the valleys and ravines of the river Esk traverse faults of 190 to nearly 500 feet, yet there is no inequality at the surface, the whole ground having been planed down by denudation to one common level. When, however, a fault brings together rocks which differ much in their relative powers of resistance to waste, the side of the dislocation occupied by the harder rocks will tend to form an eminence, while the opposite side, consisting of softer rocks, will be worn down into a hollow or plain. Conspicuous examples are furnished by the faults which, along the flanks of the Pentland Hills, have brought down the comparatively destructible sandstones and shales of the Carboniferous series, against the much less easily destroyed porphyrites and conglomerates of the Old Red Sandstone.

In fine, we learn here as elsewhere in our country, and here more strikingly than often elsewhere, on account of the varied geological structure of the district, the present landscape has resulted from a long course of sculpturing, and that how much soever that process may have been accelerated or retarded by underground movements, it is to the slow but irresistible action of rain and frost, springs, ice, and the sea, that out of the various geological formations among which Edinburgh lies, her picturesque outline of hill and valley, crag and ravine, has, step by step, been carved.

LETTERS TO THE EDITOR

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

The New Psychic Force

A YEAR ago Mr. Crookes, in a paper published in the *Quarterly Journal of Science*, announced his intention of scientifically investigating a certain class of phenomena, then known as "spiritual," which he complained had been strangely and unwarrantably neglected by those whose duty it was to investigate them. The results of some of these investigations have at last been published, in the same journal, under the title of "An experimental investigation of a new force."

Owing no doubt to the scientific reputation of Mr. Crookes, and the somewhat sensational title of the paper, it has attracted considerable attention. Whilst in quarters not purely scientific, much has been written about it, no attempt has been made, as far as I am aware, to subject the details of the experiment there described to a critical examination. It is the duty of every scientific man to be very anxious that nothing worthy of the name of Science, or calculated to be of permanent injury to Science, should ever obtain general credence. Whilst far from saying that this will be the result of Mr. Crookes' paper, still I must confess that it appears to me that, carried away by enthusiastic impulses, he has trusted to experiments which in matters more purely scientific than his investigations really were, he would never have relied upon without further and more searching examination.

In the first place, then, scientific men will not, cannot admit the validity of a "new force" (of the nature of that which Mr. Crookes calls "psychic") which rests merely on the results of two experiments made in the presence of three or four persons, of whom only two are men of known scientific attainments, the others being but scientific amateurs. Even in the details of these experiments, we find that Dr. Huggins feels himself obliged to confess that the most startling phenomenon of all was *not* witnessed by him, although, of course, he has no doubt that it was seen as described by Mr. Crookes.

The experiments are but two in number; but in them, such is the peculiar nature of this "psychic" force as it manifests itself through the agency of Mr. Home (of spiritual and mediumistic reputation), that there is hardly any known law in Physical and Biological Science which it does not tend to overthrow. Fortunately, however, in the interest of Science and the true bearing of modern scientific education, this force but rarely manifests itself, and it is particularly disobliging when many scientific sceptics wish to investigate it.* Work is done without apparently any force, mental or physical, being used up, so that we have here the direct creation of force—bodies ordinarily susceptible to the action of gravity are seen freely suspended in the air—a musical instrument (a wind instrument ordinarily played by keys), is suddenly imbued with so great a love and accurate knowledge of music that whilst the keys are visibly not touched, it plays "a well-known sad and plaintive melody, and, moreover, executes it perfectly in a very beautiful manner." All these and other phenomena, so varied, so thrilling, so "psychic," we are solemnly informed took place one evening in a room of which the temperature varied from 68° to 70° F. !

To most scientific men I am afraid there will appear something in the above so absurd and ludicrous, something so allied to the performances of professed jugglers and spiritual mediums, that it would not be worth any serious consideration, did not the scientific reputation of Mr. Crookes and Dr. Huggins demand that the experiments which gave the above results should be at once disproved or confirmed. If we proceed to examine these experiments carefully, and rigidly investigate them, we find, however, a complete want of attention to minute but by no means unimportant details—a complete absence of any attempt to ascertain whether it was not possible to produce these results without any "psychic force," and a firm confidence and belief in the ingenuousness (I had almost unwittingly written "ingenuity") of Mr. Home.

Let us now examine the experiments in detail. Firstly, with regard to the accordion, we are not told why the cage was constructed at all, and why, moreover, when constructed it was placed *under a dining-room table* of all places in the world. Does Mr. Crookes wish us to believe that it is only inside such wooden cages and in such peculiar positions that this "psychic force" manifests itself? If that is not the case, why was not the cage placed openly in the room, so that Dr. Huggins might not have had to confess that *he did not see* the accordion freely suspended in air, which Mr. Crookes and the others by dint of straining under the table did see. Then again, the accordion was confessedly placed in Mr. Home's hands before it was placed in the cage under the table—this was certainly unnecessary and is very unsatisfactory. Then it is obvious that to play the accordion the keys must in turn have been depressed. Yet Mr. Crookes does not volunteer a single word to show that he noticed whether the keys were successively pressed down or not, in fact, he rather leads us to infer that they were not. Again, it is clearly a physical impossibility for the accordion to have gone round and round the cage if Mr. Home's hand was quite still, for if he held the accordion at all, his hands must have followed its movements, and what is there to show that the accordion moved his hand or his hand the accordion? Then again, as to the instrument chosen, would a concertina act in the same manner or not? for, from the frequency with which an accordion has been appealed to by "spiritual mediums," it has acquired anything but a good reputation. It is a pity we are not informed whether Mr. Home could in the moments when he is free from "psychic influence" play on the accordion or not, and also as to what were the names of "the simple air" and the "sweet and plaintive melody" which it so obligingly played. We are also not told either how long the experiment lasted, or how long the accordion was playing, or, what is much more to the point, how long it contravened all the laws of gravity and of the acoustics of wind instruments. Surely this is an important question, quite as important as that the temperature varied from 68° to 70° Fahr.

Such are some of the questions which arise with respect to the first experiment, and which must be answered before any reliance can be placed on the results attained.

* *Vide* Mr. Home's St. Petersburg experiments.

There still remains the second experiment, which was of an entirely different kind: the one with the spring-balance. Mr. Crookes here says, "Mr. Home's fingers were never *more than* one-and-a-half inches from the extreme end, and the wooden foot being only one-and-a-half inches wide, and resting flat on the table, it is evident that no amount of pressure exerted in that space could produce any action on the balance;" and in this I quite agree; but did Mr. Crookes notice if the table itself *was moved at all?* From a very slight consideration of the peculiar apparatus employed, it is obvious that were the table to tip up in any so small a manner, the index of the balance must descend; and if the table was to tip up and down successively, the very same effect would be produced on the index of the balance as that which Mr. Crookes ascribes to "successive waves of psychic force." I do not say that the table was tipped up—that would have been trickery—but we have to account for certain results, and I do say that the tipping of the table would produce those very results, and that, moreover, there is nothing said about the table being immovable, or even heavy, or in any way fastened to the ground, as it most assuredly ought to have been. It does not appear so difficult to imagine that the "psychic force," which could produce such a strange effect upon an accordion could also so agitate the table that *it* also should show a tendency to move—and, if this were the case, the whole apparatus was so placed that the very slighest movements of the table would be magnified by the index of the balance.

On account of these and many other objections, I am forced to the conclusion previously stated, that these experiments were inaccurately performed—the details were not sufficiently examined, nor obvious errors apparently avoided, so that until they are repeated in the presence of other scientific men, they are not worthy of scientific consideration. We have read of the same phenomenon over and over again described as due to spiritual manifestations—many of them, as is well known, performed through the same agency—a medium—as those in this case. The British Association is about to meet. Let Mr. Crookes but repeat any one of the experiments at one of the evening *soirées*, and, if he can do this, he will make the Edinburgh Meeting for ever memorable, and will have earned for himself the undying reputation of having been the first to discover that in the midst of apparent humbug true science really and truly did exist. J. P. EARWAKER

PROF. BALFOUR STEWART, in NATURE for July 27, does but scant justice to Mr. Crookes's investigations. "Allowing," he says, "that things of an extraordinary nature are frequently witnessed on such occasions" (he, no doubt, means to refer to the so-called Spiritualistic *séances*) "yet we are by no means sure that these constitute external realities" And he then goes on to suggest that the phenomena may occur rather in the imagination of the spectators than in the outside world; or that the mediums (though he won't give them that name) may be under some mental influence of an "electro-biological" nature. By the way it is a pity that any man of science should help in giving currency to such a quack-scientific word; if this unknown influence must have a name, Mesmerism is the most appropriate; that does not pretend to explain the cause of the phenomena, but only to commemorate their discoverer. Now in the experiments upon which Prof. Stewart comments (I presume he refers to those described in the current number of the *Quarterly Journal of Science*) there does not seem to have been much room for the exercise of the imagination of the spectators, nor for any "electro-biological" influence to act through the medium. Setting aside the accordion performances, which perhaps left a little scope for eye deception, the results of the trial with the spring-balance were quite opposed to the known laws of mechanics. And certainly this trial took place under conditions which should have rendered deception impossible. The evidence of two such careful observers as Mr. Crookes and Dr. Huggins is not readily set down as a phantasm of their imagination; they are men accustomed to weigh the evidence of their senses with the utmost caution, for the slightest error therein would cause grave disturbance in their calculations. When such men testify that some mysterious force acted upon a lever in a way that no known force acts, and produced before their eyes results quite new to their experience, we should be as ready to believe them as if Dr. Huggins announced a new planet or Mr. Crookes a new metal; their testimony is as valuable in the one case as in the other. It is true that here they can only bear witness to the unknown, but the very existence of this unknown has hitherto been questioned. That when it is known this force shall be acknowledged to be a spiritual one is repugnant to all philosophy, and Serjeant Cox's haste to name it "psychic" is neither wise nor politic. Things spiritual have been materialised in the grossest manner by so-called spiritualists until the word has lost its meaning, and come to signify merely a cause unknown of phenomena sensual to the last degree. So it will be with "psychic" unless some one in authority stop this misuse of it at the very beginning. GEORGE FRASER

Height of Auroras

I SAW the aurora of Sept. 3, 1870, described by H. C. Key on p. 121, and I observed it from 10 to 11 P.M., but here it never reached quite to the zenith, and at 11.2 P.M. was no where high. Its brightest feature was then a distinct arch, the apex of whose central line was 12° in altitude. If Mr. Key's description of the clear space of 7° or 8° below the aurora in the S.S.E., applies to that time, it would seem that the part of the aurora bordering the clear space cannot have been more than 25 miles above the earth, and was more likely only 17 or 18 miles.

It would be well if the heights of auroras were better known than they are; and I think if systematic observations were made simultaneously at different stations, our knowledge on the subject would be largely increased. I am willing to be one of the observers in such an investigation, and Mr. G. J. Symons, the editor of the *Meteorological Magazine*, has expressed his readiness to aid. T. W. BACKHOUSE

Sunderland, July 22

Daylight Auroras

ON Sunday, the 23rd July, at 7 40 P.M., there was visible from Blackpool a phenomenon which might readily be mistaken for a daylight manifestation of the Aurora. The phenomenon in question consisted of a number of parallel streamers of light rising vertically and situated from the observer in a north-westerly direction. That portion of the sky occupied by these streamers would be about twenty-five degrees square, its lowest portion being about fifteen degrees above the horizon. At the time of the appearance the sun was obscured by a small but very dense cloud. Large masses of nimbus clouds occupied almost the whole of the north-western, northern, and north-eastern portion of the sky, whilst a few cumulus and cirro-cumulus clouds were visible in the eastern and southern parts of the heavens; one-twentieth part perhaps of the whole sky being apparently free from cloud. The streamers, which, like those of the Aurora, were intermittent in intensity, contrasted greatly in direction with any proximate beams of the sun. The whole thing, however, I am strongly of opinion, was nothing more than a meteorological phenomenon of a very different nature from the Aurora; in short, I believe it was an unusual appearance attendant on a distant and somewhat singularly circumstanced rainfall. Immediately above the uppermost boundary of the space occupied by the streamers there was a large nimbus cloud entirely obscured from the sun's direct rays, whilst that part of the sky occupied by the streamers themselves was in a strong sunshine. The whole phenomenon lasted about half an hour, and my opinion that it was but an unusual aspect of a distant rainfall was strengthened by the fact of a heavy shower of rain descending immediately after the disappearance of the streamers, the upper-current of the air being from the west by north. The rain-fall lasted about a quarter of an hour. and was accompanied by a double rainbow. When it had ceased that portion of the sky previously occupied by the streamers and almost half the remainder contained no visible trace of cloud.

This is the first instance in which I have seen what might be mistaken for a daylight manifestation of the Aurora. The streamers were so like those of the northern light, and the nature of the appearance nevertheless so obviously connected with a transient condition of the atmosphere, that I am very much tempted to doubt the visibility at any time of genuine Aurora by daylight. D. WINSTANLEY

Manchester, July 24

Spectrum of the Aurora

HAVING noticed that Prof. Zöllner observed a *red line* in the spectrum of the aurora on October 25, 1870, and as it appears this was the first time the *red line* had been observed in Europe, I am induced to send you the following extract from a short paper read by me April 12, 1870, before the Royal Society of Victoria, on the great aurora of April 5, 1870:—

"The spectrum of the aurora was obtained with one of Mr. Browning's micro-spectroscopes. When the spectroscope was directed to the red streamers, a red line, more refrangible than C (hydrogen line), a greenish line about the position of the green calcium lines, and an indistinct band more refrangible still, which appeared as if resolvable into lines, were observed. When the spectroscope was directed to the green auroral arch, the red line disappeared, and only green ones remained; the rapid disappearance of the red line as the slit passed across the boundary between the base of the streamers and the green arch, was remarkable."

In this aurora there was the usual auroral cloud-like bank on the horizon (sea-horizon) surmounted by an arch of bright greenish light to an altitude of nearly 20°, terminating with a very defined margin, from which the red streamers sprung upwards as if from behind a screen, which shed enough light at midnight to read a newspaper by. This aurora was ushered in by great magnetic disturbances for days previously, which culminated about the time of the brightest display. ROBERT J. ELLERY

Melbourne Observatory, May 19

Sparrow Cages

A PARAGRAPH in NATURE speaks of the export of sparrows to America. Such long low cages as are described at page 245, *covered with canvas*, may be seen at Leadenhall Market, in which very many thousands of Egyptian quails are brought to London alive and, I am told, disposed of as larks; of course not for the voice. A. H.

July 27

BOOKS RECEIVED

ENGLISH.—Taine on Intelligence, part II.; translated by T. D. Haye (L. Reeve and Co.).—A Treatise on Terrestrial Magnetism (Blackwood).—Text-books of Science; Elements of Geometry: J. Watson (Longmans).—Domestic Botany: John Smith (L. Reeve and Co.).—Lighthouse Illumination: T. Stevenson, second edition: (A. and C. Black.)

PAMPHLETS RECEIVED.

ENGLISH.—On the Dermal and Visceral Structures of the Kagu, Sun-bittern, and Boatbill: Dr. Murie.—Researches on the Anatomy of the Pinnipedia, part I.: Dr. Murie. Poisoning and Pilfering, Wholesale and Retail. —Journal of the Anthropological Institute, part I. March and April, 1871.—Transactions of the Manchester Geological Society, vols. 9—10.—Report of the Meteorological Committee of the Royal Society.—Journal of the Statistical Society, June.

AMERICAN.—On the Secular Perturbations of the Planets: A. Hall.—On the Application of Photography to the Determination of Astronomical Data A. Hall.—Equatorial Observations made at the U. S. National Observatory, Washington: A. Hall.—The School Laboratory of Physical Science, part II.: Prof. G. Hinrichs.

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