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THURSDAY, AUGUST 22, 1872

## AIR AND RAIN

*Air and Rain.* By R. A. Smith, Ph.D., F.R.S., General Inspector of Alkali Works. (Longmans, 1872.)

THIS work contains the germs of a system of chemical climatology. It indicates a plan of testing the purity of the atmosphere of localities with regard to certain constituents of organic origin—the *débris* of living things—by washing the air, and determining the character and amount of the substances in solution by certain microchemical methods. By the systematic repetition of these testings, the possibility is foreshadowed that we may be enabled to classify such atmospheres, and actually to assign to them quantitative sanitary values. It thus points out how we may be able to estimate the difference between the vitiated air of the town and the pure air of the country. Our senses and experience tell us plainly of the existence of such differences; but chemistry has been hitherto powerless to detect them. "It seemed to many as if the eye had obtained a mysterious power of seeing what was scarcely capable of being proved to be within the domain of substance, and the smell had a power of observing what was more an influence than a positive thing." Cavendish, nearly a century ago, asserted that chemical experiments could not distinguish the air of London from the air of the country; and in spite of the labours of Bunsen and Regnault, Frankland and Williamson, which have rendered gasometry more susceptible of refinement and accuracy than any other branch of chemical analysis, this assertion seems as true of to-day as it was of the time when uttered. Hitherto chemists, in judging of the quality of the air of any locality, have been obliged to content themselves with determining the proportion of oxygen and carbonic acid which it contains, in conformity with the practice of their ancestors of a century back. Gradually, however, they have been forced to the conclusion that such determinations have very little positive value in enabling them to assign a value to the sanitary condition of an atmosphere—that oxygen was no panacea, nor carbonic acid as deadly as strychnine; and thus we have been thrown back upon our unaided senses to distinguish between the good and the evil. Supposing that some Martin Chuzzlewit, going out to another Eden, required information respecting the sanitary condition of the settlement, the chemist could tell him something concerning the water he might have to drink, but he would be utterly unable to enlighten him respecting the air he would be compelled to breathe. Some such considerations prompted the inquiries which have resulted in this book. Dalton's assertion that he could not distinguish the air of Manchester from that of Helvellyn, or generally the air which depresses from that which cheers and invigorates, seems to have forcibly impressed the author. For upwards of forty years he has laboured to remove the stigma on chemical analysis, and in this volume he concentrates his thoughts and experimental results. "It was with the desire," he says, "of clearing the mystery of air to some extent that I have devoted so much of my time to the subject; and now I feel that, whilst I have succeeded in doing much of that which I intended to do, I have not got beyond the limits which earlier observers attained by the mere fineness of unaided sense, and by sound reasoning without experiment. Still I hope I shall be found to have put their suspicions into plainer language, proved that which they only imagined, and given in detail that which they only in a general, and, we may add, in a vague manner, had attained."

Dr. Smith first sets out by defining the composition of a normal atmosphere, as deduced from the many analyses which have been published, and from numerous supplementary determinations of his own made on air collected in various parts of Great Britain and on the Continent. In the outset he insists on the value of minuteness in reading the figures; differences which in the eyes of most chemists are of little value, are to him full of meaning. Every deviation from the standard of purity is to be rigidly criticised. Thus, the difference between 20·980 and 20·999 in the percentage amount of oxygen means a difference of 190 parts in a million. If this consisted of organic matter, or the gases of putrefaction, it might become of the gravest consequence. Certainly 190 parts of putrefying matter in 1,000,000 parts of water—equal to 13·3 grains per gallon—would be considered as an enormous quantity. But, comparatively speaking, we drink only a small quantity of water, and the whole 13 grains would not be swallowed in a single day; whereas we draw through our lungs nearly a couple of thousand gallons of air daily. But, indeed, differences much greater than this are found to exist. Thus, the air of a theatre sometimes contains as little as 20·7 per cent. of oxygen, and even this is by no means an exceptionally small quantity for such a place; and yet this amounts to a deviation of 3,000 parts in a million from the standard of purity.

In speaking of the proportion of carbonic acid in the air, the author bases certain considerations (p. 11) upon the assumption that this gas is washed out by falling rain. But is this supposition exactly confirmed by experiment? Saussure, it is true, thought that he could detect a difference in the amount of carbonic acid between the air over the Lake of Geneva and that over the land; but such differences have not been found by other experimenters. Sea air contains about three volumes of the gas in 10,000 volumes; whilst the air of the land contains only four volumes. But this difference is due more to the influence of the land than to any absorptive action exerted by the sea. Indeed, from a consideration of the laws of gaseous absorption, it can be shown that the pressure exerted by the relatively small quantity of carbonic acid present in the air is unable to bring about any perceptible variation in its amount over sea and land.

Having fixed on his standard of purity, Dr. Smith proceeds to examine vitiated air and to trace its effects on breathing. For this purpose he used an air-tight chamber in which one or more persons could be seated; and from time to time he collected and analysed samples of the enclosed air, and compared the analytical numbers with the sensations experienced and noted at the moment of collection. The details of these experiments are of great interest, and merit careful study. That they were not unattended with danger is evident from the experience of a young lady "who was extremely fond of pure air," but who in the cause of science "was anxious to be in the chamber when the candles went out." At such a time there would be about 19 per cent. of oxygen and 2·1 per cent. of carbonic acid in the chamber. No person had been in the chamber previously. "She stood five minutes perfectly well, and making light of the difficulty; but suddenly became white, and could not come out without help. She was remarkably healthy, never was ill, and was troubled with no fear of the air in which she stood."

From the air to the rain which falls through it, is but a single step; for if, as our author says, there is life and death in the air, we must believe the same of the rain, which collects the solids and liquids, gases and vapours which float about in the atmosphere. These ingredients of rain water can, indeed, be shown by chemical analysis; and by the microscope distinctions may be drawn between the air of various localities, without putting the health to the test. The author proceeds to describe his methods of testing rain water; but as the details of the scheme are mainly of interest to chemists, we must refer any curious readers to the book itself. Much of the work herein detailed was done years ago; long, indeed, before Pasteur had enlightened us as to the great reservoir of life which exists in our atmosphere. In 1852 Dr. Smith showed how complicated a fluid rain is. However carefully collected, albuminous bodies, the remains of living creatures, and minute animalculæ, may invariably be detected in it. "These creatures," says Dr. Smith—anticipating Dr. Frankland's aphorism, "Ohne Phosphor gar kein Leben"—"are sufficient of themselves to show the existence of phosphates, whilst sulphates and lime may be readily obtained. In examining the Thames water I often found that the readiest way of collecting phosphates and magnesia was to wait for the animalcules to do it."

Through the kindness of a number of gentlemen, Dr. Smith was enabled to make numerous collections of rain water from as far north as the Hebrides and as far west as Valentia. The results of the samples of the analysis may be thus briefly summarised:—The rain over the sea contains chiefly common salt, which crystallises clearly; but it also contains sulphates, and in larger proportion to the chlorides than is found in sea water. The amount of these sulphates increases inland before large towns are reached. They are to be regarded as the measure of the products of decomposition, the sulphuretted hydrogen from putrifying organic compounds becoming oxidised in the atmosphere. Within certain limitations, they may be taken as an index of the amount of sewage in the air. We accordingly find in the large towns the amount of the sulphates is greatly increased, owing to the combustion of the sulphur in coal, as well as the decomposition of organic matter contained in protein substances. When the sulphuric acid increases more rapidly than the ammonia, the rain-water becomes acid, and when the amount of this free acid reaches two or three grains in a gallon, or forty parts in a million, there is no hope for vegetation in a climate such as we have in the northern parts of this country. These free acids are not found with certainty where combustion or manufactures are not the cause. The amount of ammoniacal salts in the rain water increases with the number of towns in the district. This ammonia comes partly from the coal, and partly from the decomposition of albuminoid substances, which, indeed, may also be detected in the rain water. It is very interesting to compare the relative purity of the atmospheres of our cities and large towns, as determined by this method of air-washing. Upon the whole, that of London appears to be the best, and that of Glasgow decidedly the worst. Calling the amount of sulphuric acid in sea air 100, the average amount in that of London is 352, and in that of Manchester 513. In Glasgow the amount of ammonia is 150, in London it is 115; the amount of albuminoid ammonia in London air is only 109, whereas in Glasgow it is more than twice that amount, viz. 221. These analyses show unmistakably in what the evil of overcrowding consists; and it is with this subject on which he is thus led to speak that Dr. Smith closes his book. We commend his remarks to our City Improvement officials:—"Let those courts, alleys, and streets, which show the greatest mortality and the worst air, be destroyed or improved, without foolish mercy. There is a want of willingness to pull down dangerous property, but a readiness to rush forward to save the life of the greatest criminals. Reason is out of the question in the matter. We are misled by an uneducated feeling. We like to save property, forgetting that deadly weapons and poisons are subject to peculiar laws, and their indiscriminate use is forbidden to the nation. And houses that produce death are not property; as well might a man claim his debts as such. If a man sells unwholesome meat the law interferes; if he sells the use of a room with fever in it, the nation seems not to complain. Officers of health point out such places, but the public still refuse to destroy them, and great numbers are slain annually by legal methods, while strict methods are taken to prevent a few annually being killed by arsenic—a death more agreeable than the lingering misery in the lower parts of our crowded towns. I know that the lowest classes living in poisoned houses die from other causes than bad air; but I am speaking of air at present, and that is one of the causes. The time must come—and the sooner the better—when it shall be enacted that no land shall contain more people per acre than we know by experience in several places can live healthily thereon. The same thing must be said regarding houses, although these are more difficult for governments to deal with, because of the degradation of some of the population. Still the limitation must be attained, and for that we must strive."

T. E. THORPE

## THE IRON AND STEEL INSTITUTE

THE Fourth Annual Provincial Meeting of the Iron and Steel Institute was held recently (August 6th to 9th inclusive) in Glasgow, under the presidency of Mr. Henry Bessemer, and it has been, if it were possible, even more successful than any of the previous reunions, furnishing thereby the best proof that such an association actually was a desideratum, and of the hearty co-operation which its establishment has called forth, from all interested directly or indirectly in the development of the Iron and Steel manufactures of Great Britain.

Since the three previous meetings were all held in the iron districts south of the Tweed, it is the more gratifying on this occasion to find that the first meeting of the Institution in Scotland should have turned out so eminently successful, and so marked by the hearty welcome with which Scotch ironmasters have received their Southern competitors in the trade; and it cannot but be observed that since the foundation of the Institute, a very different spirit has infused itself among the members of the iron and steel trades in general, as they no longer keep themselves jealously aloof from one another, but, on the contrary, are now pleased to meet, and in a spirit of generous rivalry to interchange their ideas, thereby to some extent, at least, tacitly acknowledging that the advancement of industry of the country at large, so far from being prejudicial to, is in reality ultimately connected with, the interests of each individual manufacturer also. It is the recognition of this principle which has enlisted the sympathies of those engaged in the iron and steel industries, and has contributed so much to the success of the Institute, which at this moment, including those candidates nominated at the present meeting, numbers no less than 602 members—a surprising result when it is remembered that the Association is now only in the fourth year of its existence.

The Glasgow meeting of the Institute commenced on Tuesday, August 6th, when its members assembled in the Corporation Galleries, Sauchiehall Street, which had most liberally been placed at the disposal of the Council of the Institute by the Lord Provost and Magistrates of the City; the proceedings being prefaced by a short introductory address from the President (Mr. Bessemer), together with a few words of welcome from the Lord Provost of Glasgow.

The Secretary then announced that the Council had recommended Mr. Isaac Lothian Bell, of Newcastle, as president-elect, and nominated Mr. Edward Williams, of Middlesborough, as vice-president in the place of Mr. Bell, as also Messrs. W. S. Roden, C. W. Siemens, H. Sharpe, W. Nielson, and J. Hunter to the vacancies caused by the retirement of the other members of the Council by rotation, which recommendations were unanimously adopted by the meeting.

The Foreign Secretary read out the names of those gentlemen connected with the foreign iron trade then present at the meeting, amongst whom were representatives of France, Belgium, Germany, Sweden, and the United States of America.

The election of members was then proceeded with, after which an extremely interesting paper, "On the Coal and Ironstone Strata of the West of Scotland," was read by Mr. James Geikie, of the Geological Survey, in which a general sketch of the geology of the district, with special reference to the occurrence and nature of the coal and ironstone deposits, was given in a concise yet extremely lucid communication, the delivery and discussion of which occupied the remaining available time of this day's meeting. Visits were subsequently made by the members to some of the neighbouring works, and more particularly to the Blochairn Iron Works, where considerable attention was paid to Mr. Graham Stevenson's new mode of reversing rolling mills then in operation.

On the following day (Wednesday) the meeting commenced at 10.30 A.M. by the reading of a paper by Mr. J. F. Mayer, of Glasgow, "On the Rise and Progress of the Iron Manufacture of Scotland," which was an historical sketch of the subject, commencing from the year 1760 when iron was first smelted at Carron, near Falkirk, and continuing it down to the present time, when the Scotch iron manufacture occupies so prominent a position in the British iron trade. Attention was specially directed to such improvements in the manufacture of iron as had originated in this part of Scotland, amongst which were mentioned the use of raw coal instead of coke in iron smelting by Condie at Govan, the employment of hot blast by Nielson, and the utilisation of the black band ironstone by Daniel Mushet, discoveries which, it may be said, were the making of the Scotch iron trade.

The next two communications related to the different systems for reversing the rolls in rolling wrought iron; the first of these by Mr. J. D. Napier, "On Napier's differential gear for reversing rolling mills," illustrated by models, described the application at the Codnor Park Iron Works in Derbyshire of a differential clutch, identical in principle with the differential breaks used by him in the windlasses of ships; whilst the second, by Mr. Graham Stevenson, "On Reversing Rolling Mills," advocated the employment of his conical clutch, which had been inspected the previous afternoon at the Blochairn Iron Works. These two papers were discussed together, and gave rise to a very animated debate, in the course of which much valuable information was elicited from the observations made by members practically acquainted with the subject. The balance of opinion appeared, however, to be in favour of Mr. Napier's differential clutch, the extreme simplicity of which appeared to give it advantages over any hitherto applied form of reversing gear, not excluding the conical clutch of Mr. Stevenson, which, nevertheless, was admitted to work very satisfactorily.

The meeting then broke up, most of the members proceeding by a special train to inspect the Iron Works at Gortsherrie, Coatbridge, Summerlee, and Monkland. Amongst the novelties examined on this excursion may be mentioned the new coal-cutting machine, invented by Messrs. Miller and Anderson of Coatbridge, then at work in the No. 3 pit, Gortsherrie, belonging to Messrs. William Baird and Co.

This machine is stated to cut 350 ft. of coal per shift of 8 hours, yielding 75 tons, or equal to the united turn out of 40 men, whilst it only requires two to attend to it, being driven by compressed air at a pressure of 45 lbs. to the square inch, brought in cast iron pipes some 300 fathoms from the shaft. As six additional machines are in course of construction for Mr. Baird, it is evidently regarded as a success; and it is hoped that in these days of incessant strikes it may prove capable of doing all it is represented to do, and thus be the means of checking in some degree the exorbitant demands of the colliers.

At the North British Iron Works, at Coatbridge, M. Dormoy's patent puddling furnace, with revolving rabble, driven by steam power, was shown in full operation, and appeared to elicit the very general expression of opinion that the invention, even if ingenious and practical, had come out too late in the day. Now that the more perfect system of rotary puddling of Mr. Danks has proved successful in dispensing with the labour of the puddler altogether, it is not likely that the ironmaster of the future will be content with a system which at best is only an improved manual process for puddling iron.

At the Monkland Iron and Steel Works the interest of the visitors was concentrated in inspecting the two blast furnaces constructed on Ferrie's patent coking principle. These furnaces have a height of no less than 90 ft., and are so arranged that, when fed at the top with the raw coal and iron ore mixed together, the coal, as it descends, becomes coked in the upper portion of the furnace, before it reaches the smelting region of the furnace lower down. Great economy in fuel is claimed for this arrangement; it being considered that the heat and combustible gases driven off and lost, when the coal is previously coked as at present in separate coking ovens or heaps, are utilised entirely in the Ferrie blast furnace.

The favourable opinion expressed by several of the members of the Institute, will, it is understood, lead to the erection of several of these furnaces, both in the district and probably in England also.

The meeting on Thursday commenced at 10.30 A.M., and was opened by the reading of a paper by Dr. A. K. Irvine, of Glasgow, "On a new miner's safety lamp," in which the author described an invention of a most ingenious character, which is likely to prove of great service in coal mines troubled with explosive gases, since, besides serving the purpose of an ordinary safety lamp, it sounds a note of warning to the workman the moment the air around becomes so charged with firedamp as to be dangerous or explosive. The principle of the lamp is based on the fact that when a mixture of any inflammable gas or vapour, with air in explosive proportions, is lighted on the surface of wire gauze, having meshes sufficiently small to prevent the passage of flame, and a suitable tube or chimney is placed above, so as to prevent admission to the chimney except through the wire gauze, a musical sound is produced varying in pitch with the size of the flame and dimensions of the chimney.

A number of interesting experiments were exhibited to illustrate this principle, and various miner's lamps as constructed were exhibited and tested in mixtures of air with ordinary coal gas, when they at once indicated the danger as soon as the atmosphere by which they were surrounded contained sufficient gas to be dangerous, by emitting a strong clear sound like that of a horn, which could be heard at a considerable distance. Another form of this lamp was also shown, intended to be employed as a stationary warning apparatus or alarum after being placed in any part of the mine considered likely to ensure the safety of the workmen, so that it might sound the danger signal before the air around it was so far charged with fire-damp as to become explosive. The novelty and importance of such an invention were apparent to an audience of practical men; and besides passing a cordial vote of thanks to the inventor, arrangements were made for at once fully testing its merits by its practical employment in some English collieries noted for fire-damp.

After an interesting paper by Mr. D. Rowan, of Glasgow, "On the rise and progress of the iron shipbuilding trade in Scotland," which, however, was of a purely historical and statistical nature; the next communication was made by Mr. Lauth, of Pittsburg, United States, "On Lauth's system of rolling iron by three high rolls." The improvements proposed in this system of rolling, which is in itself very old, consisted in making the central roll of less diameter than the two others, which are of ordinary size, and in having it fixed, whilst the two others are adjustable by screws. In the hard rolls the bottom roll alone is driven, both the middle and top roll being carried round by friction. All expansion or contraction is prevented by a stream of water constantly kept running on to the roll; and great rapidity in rolling, as well as economy in labour, is claimed for this system. In the discussion which followed, the general opinion appeared to be that, although such rolls were well adapted for plates, in this country they were less adapted for rail rolling, owing to the greater difficulty in adjusting the grooves so as to turn out rails as correct in section as was insisted upon by our and most of the Continental engineers, but not in the United States; also because the necessity for three rails would still further augment the immense stock of rolls requisite to suit the multiplicity of sections required in the English trade, as well as increase the labour and time required in changing the rolls.

The next paper was by Mr. A. Spencer, of West Hartlepool, "On further improvements in Spencer's Rotary Puddling Furnace" a model of the furnace in its present form being exhibited, and its construction, mode of fettling, and working, fully entered into by the author; after which, owing to time not permitting, a lengthy paper by Mr. J. Guildford Smith, of Philadelphia, "On the Westward development of the Iron Manufacture of the United States," was taken as read; and after passing votes of thanks to the Lord Provost and civic authorities of Glasgow, the Council of the Philosophical Society, the Committee of the Royal Exchange, the local Committee, and the President of the Institute, the proceedings of the meeting were brought to a close.

In the afternoon an excursion was made by a special train to the Coltness and Mossend Ironworks, the members of the Institute being entertained on their return at a banquet given in the Corporation Galleries by the local Committee of the Institute.

An interesting feature in connection with the meeting was the arrangement of a temporary museum in the Corporation Galleries containing models, specimens, and objects of all kinds bearing more or less directly on the Iron and Steel manufacture, many of the articles exhibited being of great interest.

## LETTERS TO THE EDITOR

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

### Solar Outbursts and Magnetic Storms

In the French *Comptes Rendus* of August 4, which has lately reached this country, is an account by Father Secchi of a remarkable outburst from the sun's limb witnessed by him on July 7, which lasted from $3^h\ 30^m$ to $6^h\ 50^m$ (Roman time, I presume), or nearly $2^h\ 40^m$ to $6^h\ 0^m$ (Greenwich time).

A magnetic storm commenced at Greenwich at $5^h\ 0^m$ precisely on the same day. Its indications began at that time with unusual suddenness and strength, on all the magnetic indicators, namely the declination-needle, the horizontal force magnetometer, the vertical force magnetometer, the earth-current wire, in an approximate N.E. and S.W. direction, and on the earth-current wire in an approximate N.W and S E. direction The disturbance lasted, gradually diminishing, to the evening of July 9. During a part of the time it was accompanied with aurora.

I do not venture upon the question whether there really was any connection between the solar outburst and the terrestrial magnetic storm, but I will remark that, if there was such connection, the transmission of the influence from the sun to the earth must have occupied $2^h\ 20^m$; or a longer time if Father Secchi did not see the real beginning of the outburst. This, if established, would be an important cosmical fact; and, at any rate, the notification of this apparent retardation may direct the attention of observers of similar phenomena in future to a new element in their interpretation. G. B. AIRY

Royal Observatory, Greenwich, August 14

### Ocean Circulation

ALTHOUGH no mathematician, and only an amateur in physics, it appears to me that the difficulties and objections of Mr. Croll on this subject may be obviated, and the whole question elucidated by a reference to the admitted facts, and a common sense interpretation of them. And first, as to the fact that the surface water of the Atlantic Ocean, in moving northwards from the equator to 60° lat., has almost wholly lost the easterly motion of rotation it should have brought with it. This loss is imputed by Mr. Croll to friction only, and he argues that the much lower velocity of the northward current must, therefore, be wholly neutralised by friction. This is his main argument, which he has repeatedly adduced, and to which he has hitherto received no reply. But, although his reasoning might be admitted if the conditions affecting the two motions were the same, it seems to me to be quite inapplicable to the present case. If, in the temperate zone, the ocean extended uninterruptedly in an east and west direction round the globe, it would no doubt retain a considerable portion of the equatorial eastern motion, and whatever deficiency existed might fairly be imputed to friction. But the Atlantic is actually like a huge lake, with continuous eastern and western shores, and the water which flows northwards along the eastern shore is prevented from moving eastwards, not by friction against water or even against the shore, but by having to perform work in lifting or heaping up the water against the shore, just as the water of a pond or lake is heaped up on the leeward side by a strong wind. As the direction of the motion of the water will, however, by the hypothesis, be oblique or somewhat north of east, some of the motion will be diverted northwards along the eastern shore, and thus tend to increase the northerly flow. The 9,925 pounds of energy (according to Mr. Croll) are not therefore consumed in overcoming the frictional resistance to eastward motion, but for the most part in doing the actual work of overcoming gravitation and holding up the waters at a higher level, and the theoretical amount of this rise can, no doubt, be easily calculated for us by Mr. Croll.

The case of the water moving northward is very different.

There is a clear passage into the polar area, and probably up to and beyond the pole; and within this area there is a continual diminution of bulk of the entering water as it becomes cooled, as well as a continual subsidence of the surface water, producing a partial depression to be constantly filled by water from the south. Experiment proves that if at one end of a vessel of warm water ice is applied at the surface, the cooled water instantly sinks, and its place is taken, not by water rising upwards from below, but by a horizontal movement of the surface gradually propagated to the other end of the vessel, while the descending cold water creeps along the bottom, and gradually acquiring a higher temperature, rises and completes the circuit. It is somewhat difficult to conceive, theoretically, how such a circulation can commence, because the cooled atoms of water must displace others before they can descend, and these again must displace others, and so on over the whole mass. The amount of energy due to the uperior weight of the first cooled atoms may appear inadequate to perform so much work, but nevertheless circulation does commence and indefinitely continues so long as a difference of temperature of the two ends of the vessel is kept up. The extreme mobility of the particles of water, and the almost total absence of friction between them, seems to be influential in producing this result; and it is not probable that any minute difference of level that may be caused on the surface of the water by difference of temperature has anything to do with the motion; and I cannot help thinking that the supposed six-feet incline from the equator to lat. 60° is, if it exists, by no means an effective cause of the oceanic circulation.

ALFRED R. WALLACE

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I THINK the root of Mr. Croll's difficulty (see NATURE, p. 324) is to be found in his overlooking the possibility of energy becoming potential in the distribution of oceanic water.

Water running in any direction in the northern hemisphere tends to swerve to its own right, and if this tendency is checked (as it is in fact by the presence of continents) its layers of equal density will be tilted up on the right, the limit of tilt being the angle whose tangent is the quotient of the tendency to swerve by the force of gravity. This consideration is, I think, sufficient to deprive Mr. Croll's argument of one of the two legs on which it stood.

Mr. Ferrel's argument from the tides is quite conclusive in showing that the forces arising from difference of temperature are of sufficient magnitude to keep up an oceanic circulation. Thus the other leg of Mr. Croll's argument is gone.

Mr. Croll may well retract his previous assertion that the difference of kinetic energy is consumed in friction, for he was in a fair way to bring the earth to a standstill.

Brighton, August 20 J. D. EVERETT

### Spectrum of Aurora

A FINE aurora was seen at Bedford on Thursday night between midnight and one o'clock. It was brightest under the Polar Star near the horizon, where the colour was a pale green; whilst overhead the hue often changed to a rosy red. On directing a spectroscope at the most brilliant part, a bright green line (W.L. 557) was very distinct, and two or three faint nebulous bands more refrangible were visible; but the red line was not to be seen, though carefully looked for on the rosy parts of the aurora. Objects around were faintly illuminated as if by a young moon. At one time two very faint pale green streamers were seen stretching from the north to a little east of the zenith.

Blackheath, August 11 J. P. MACLEAR

### The Method of Least Squares

As the wording of Prof. Hall's letter in NATURE for July 25 might imply that he was calling attention to evidence that would change the opinion expressed in my letter, it seems to me worth while to state that at the time of writing that letter I was acquainted with the passages in question, and to repeat my assertion that with reference to the *method of least squares* I should not regard the neglect of Lagrange's memoir as an omission. Also in spite of Encke's and Prof. Hall's remarks, I think it has received as much attention as, viewed practically, its importance entitled it to.

With regard to the principle of the Arithmetic Mean, I may add that I have devoted the greater part of a tolerably long memoir to its consideration, and feel sure that no remarks on the subject contained in a few lines could be rendered even intelligible. J. W. L. GLAISHER

Blackheath, August 11

## NOTES

WE are informed that M. Faye will in all probability be M. Delaunay's successor as Director of the Observatory at Paris. In the meantime M. Matthieu supplies his place *pro tem.*

THE French Academy has elected two foreign correspondents in the section of botany—M. Planchon in the place of M. Lecoq, and M. Weddel in the place of Prof. Mohl.

THE American Association for the Advancement of Science was to commence its sittings yesterday at Dubuque, Iowa. Prof. J. Lawrence Smith, of Louisville, had been elected President, and Prof. Alexander Winchell, of Ann Arbor, Vice-President. It was announced that the citizens of Dubuque had determined that all members attending the meeting should be entertained at their private residences free of charge during the session; and their travelling expenses would also probably be remitted by the various railroad and steamboat lines. A very successful meeting was anticipated.

AT the recent combined First B.A., First B.Sc., and Preliminary Scientific (M.B.) Examinations of the University of London, Mr. J. M. Lightwood, of Trinity Hall, Cambridge, obtained the Exhibition in Mathematics and Philosophy; Mr. R. E. Carrington, of Guy's Hospital, the Exhibitions in Chemistry and in Zoology; and Mr. J. C. Saunders, of Downing College, Cambridge, the Exhibition in Botany.

WE regret to announce the death of Mr. Frederick Carpenter Skey, C.B., F.R.S., which took place on Thursday last at his residence, Mount Street, Grosvenor Square. Mr. Skey was in his 73rd year. He was in early life a pupil of the celebrated Dr. Abernethy, to whom he was articled in 1816 by the Royal College of Surgeons. About 1826 he was appointed Demonstrator of Anatomy at St. Bartholomew's Hospital. Subsequently he founded the Aldersgate School of Medicine, which became one of the largest in London. From that time to his death Mr. Skey enjoyed the reputation of being in the first rank of London surgeons. His writings on medical subjects were numerous and important, and on subjects connected with sanitary science his communications to the public journals were frequent.

THE following is the list of candidates who have been successful in obtaining Royal exhibitions of 50*l.* per annum each for three years in the Science and Art Department, and free admission to the course of instruction at the following institutions:—To the Royal School of Mines, Jermyn Street—William Carter, Ambrose R. Willis, and Alexander Gibson. To the Royal College of Science, Dublin—Arthur G. Meeze, Denis Coyle, and Ernest H. Cook.

A NATURAL History Society has been formed at Madrid called "La Sociedad Española de Historia Natural," under the presidency of Don Miguel Colmeiro. The first part of its publication has reached this country, and contains the regulations of the Society, an account of the meetings held by it up to this time, and papers by Poey on Ichthyology, by Colmeiro on the Fumitories of Spain and Portugal, by Espada on the Volcano of Ansango, by Solano on a Meteoric Stone, by Espada on New America Batrachians, and by Perez Arcas on New Reptiles and Insects of the Spanish Fauna. It is extremely well printed, and is illustrated by three capital plates. The subscription to the society is 12s. 6d. Anyone desiring to become a member may address himself to Don Serafin de Uhagon, Sordo 27, Madrid, the treasurer, or to any other member.

PRIZES for Collections of Economic Entomology are offered for competition in 1873, and the following rules relating thereto have been issued by the Royal Horticultural Society:—10l. for a collection of British insects injurious to some one order of plant used for food—as Cruciferæ, Leguminosæ, or corn. The order may be selected by the competitor. 3l. for a miscellaneous collection of British insects injurious to plants used as food. 5l. for a collection of British beetles injurious to timber and fruit trees either growing or felled. 2l. for a collection of British insects injurious to some one timber or fruit tree. The insects are to be exhibited in their various stages of development, accompanied by specimens, models, or drawings of the injuries caused by them. The collections are to be sent in addressed to James Richards, Assistant-Secretary, Royal Horticultural Society, South Kensington, on or before November 1, 1873.

THE threatened destruction of the "Cursus," at Stonehenge, by the ploughing up of the land, is attracting so much public attention, that we may hope it will be in time to arrest this piece of Vandalism.

THE largest and most important of the fragments of the carved column dug up by Mr. Wood at a depth of 23 feet on the supposed site of the Temple of Diana at Ephesus, has been set up in the Græco-Roman room at the British Museum. It measures about 6 feet in height and 18½ feet in circumference, and it is supposed to have formed a portion of the first drum of one of the thirty-six Ionic carved columns which, with ninety-one others, supported and adorned the structure. Portions of the base and capital of the column were also found close by. On the side of the drum, which has sustained comparatively slight injury, there are five figures of considerable beauty, but all more or less mutilated. Of only two of these can the identity be determined—namely, the figures of Mercury and Victory. The former is perfect, with the exception of the face, which is slightly mutilated, and is regarded by competent judges as a work of considerable merit.

DR. HAYDEN, in charge of the Geological Survey of the Territories, having completed his preliminary arrangements at Ogden, has separated his forces into two divisions, one of which was to proceed to Fort Hall, with waggons and a suitable outfit, to be changed into a pack train at Fort Hall, and thence to travel up the Snake Valley, under the direction of Mr. Stevenson; the other division, under the doctor's own charge, was to start soon for Fort Ellis, and expected to be at work there by the 1st of July. Among other interesting observations already made by Dr. Hayden's expedition, was the occurrence of invertebrate animal life in great abundance in the Great Salt Lake. This fact is not entirely new, as the existence of dipterous larvæ in these waters has already been recorded by Captain Stansbury and others.

PROF. DAVIDSON, of the United States Coast Survey, has recently, before the Academy of Sciences of San Francisco, contested the theory of Mr. Octave Pavy in regard to polar currents and the topography of the polar regions. In his paper the professor maintains, in opposition to the views of Mr. Pavy, that the currents flow eastward through the straits north of the American continent, and that the current through Behring Straits is local, and unimportant in its effects as regards the polar basin; that Wrangell's Land is not a region continuous to a great distance toward the pole, as contended by Mr. Pavy, but a small island or cluster of islands.

## THE BRITISH ASSOCIATION MEETING AT BRIGHTON

BRIGHTON, *Tuesday Morning*

THERE is a general agreement that the Brighton meeting of the British Association is a brilliant one; though we fear that this phrase, as used among Brightonians, refers rather to the fashionable character of the audiences at the various sections than to the scientific value of the papers read. Not that this latter has been below the average; but so far at least there has been no one paper or discussion which has placed a distinctive character on the meeting of 1872 as marking an epoch in scientific thought. We miss, also (no doubt owing to the remoteness of the locality from the great intellectual foci of the north), the familiar faces of many who are wont to add life and interest to our meetings; while the London *savans* do not appear to muster in greater force than when the meeting is held 300 miles from the metropolis. The spacious dome of the Pavilion was crowded to its utmost capacity on Wednesday evening, to hear Dr. Carpenter's opening address; and so admirable are the acoustic properties of the building, that each word was distinctly heard in every corner, if we may use the term in describing a circular room. Among the distinguished visitors, the curiosity of the audience was about equally divided between Mr. Stanley and the ex-Emperor of the French, both of whom occupied seats on the platform. The total number of tickets issued up to Wednesday evening was 2,140, or only 400 short of the Edinburgh total. Although the rooms in which the various sections are held are scattered, the distances are not great, and the splendid weather makes it easy to get from one to another. The localities selected give rise to some singular incongruities; as when wandering into the Friends' Meeting House, where Section G finds its local habitation, we heard a paper read from the "Ministers' Gallery," "On the progress of invention in breech-loading small arms during the past twenty years."

At the meeting of the General Committee previously held, the Report of the Council was read, in which the following are the more important features:—

The Council announce that a vacancy has occurred in the number of the trustees in consequence of the death of Sir Roderick Murchison, and take this opportunity of expressing their regret at the great loss which science has sustained by his death. He worked long, earnestly, and with eminent success in the sciences of geology and geography, and was at all times a steady patron of rising scientific men in all branches of science. He was a member and strenuous supporter of this Association at its first formation in 1831, and continued until the close of his life a very constant attendant at its meetings and a firm promoter of its interests. The Council recommend that Sir John Lubbock, Bart., be selected to fill the vacancy.

The next subject referred to is the appointment of the Committee to promote observations on the Total Solar Eclipse of December last, from which a Report will be read in due course. The results will be published by the Association to form part of a series uniform with the contemplated reports of the Royal Astronomical Society of the observations of the eclipses of 1860 and 1870.

There were five other resolutions referred to the Council for consideration or action, upon which the proceedings of the Council have been as follows:—

First resolution:

"That the President and Council of the British Association be authorised to co-operate with the President and Council of the Royal Society, in whatever way may seem to them best, for the promotion of a circumnavigation expedition, specially fitted out to carry the physical and biological exploration of the deep sea into all the great oceanic areas."

A copy of this resolution was forwarded to the Royal Society, and a committee was appointed, consisting of the president and officers of the Association, Dr. Carpenter, Prof. Huxley, Mr. Gwyn Jeffreys, Mr. C. W. Siemens, and Prof. Williamson, and authorised to co-operate with the Committee of the Royal Society in carrying out the objects referred to in the resolution. The expedition has been organised, the ship *Challenger* is being fitted out at Sheerness, Capt. Nares has been appointed to the command, and Prof. Wyville Thomson (who has obtained three years' leave of absence from the University of Edinburgh) is appointed to the scientific charge, with an adequate staff under him. It is hoped that the expedition will sail about the end of November.

Second resolution :

1. "That it is desirable that the British Association apply to the Treasury for funds to enable the tidal committee to make observations and to continue their calculations.

2. "That it is desirable that the British Association should urge upon the Government of India the importance, for navigation and other practical purposes and for science, of making accurate and continued observations on the tides at several points on the coast of India."

With the result of these applications we have already informed our readers, and discussed the bearings on the future of Science of the attitude of the Government.*

Third resolution :—

"That the Council of the Association be requested to take such steps as to them may seem most expedient in support of a proposal, made by Dr. Buys Ballot, to establish a telegraphic meteorological station at the Azores."

The Council appointed a Committee of their own body to report upon this proposal. The Committee, after due deliberation, reported that while sympathising with the proposal made by Dr. Buys Ballot, they cannot recommend a grant of money to be made by the Association for carrying it out. In this recommendation the Council concur.

Fourth resolution :—

"That the Council be requested to take into consideration the desirability of the publication of a periodic record of advances made in the various branches of sciences represented by the British Association."

The Council, after a careful consideration of this proposal, are not prepared to recommend the Association to undertake the publication of a periodic record of advances made in the various branches of science represented by the sections of the British Association. They are of opinion that in so vast an undertaking special societies should be invited to prepare such records, the action of the Association being limited to occasional grants in aid. They are of opinion, however, that the Association would do well to promote the more frequent publication in their proceedings of critical reports on various branches of science, of the same nature as those which have already rendered previous volumes so valuable to investigators.

Fifth resolution :—

"That the Council of this Association be requested to take such steps as may appear to them desirable with reference to the arrangements now in contemplation to establish 'leaving examinations,' and to report to the Association on the present position of science-teaching in the public and first grade schools.

"That the Council be requested to take such steps as they deem wisest in order to promote the introduction of scientific instruction into the elementary schools throughout the country."

A Committee, consisting of the President and the General Officers, Mr. G. Busk, Mr. Debus, Dr. Duncan, Mr. Fitch, Prof. M. Foster, Mr. F. Galton, Dr. Hirst, Prof. Huxley, Sir John Lubbock, Bart., Sir J. Paget, Bart., Rev. Prof. Price, Prof. H. J. S. Smith, Prof. Stokes, Prof. Tyndall, and Prof. Williamson, was appointed to consider the first of these resolutions, and to report on them to the Council.

In accordance with the recommendation of this Committee the Council adopted the following resolution :—

"That having had under consideration the requests which the Committee of Masters of Schools have made to the Universities of Oxford and Cambridge upon points in which the Education of the Universities and Schools came into contact, the Council of the British Association recommend that arithmetic, and either elementary physics or chemistry experimentally treated, be introduced into the leaving examinations as compulsory subjects.

"That the Head Masters of Public Schools be requested to furnish the Council with information about the present position of Science-teaching in their Schools."

The Council have communicated thereon with the Universities of Oxford and Cambridge, but at present no decision respecting "leaving examinations" has been arrived at in these Universities.

In accordance with the terms of the resolutions passed by the General Committee last year, appointing a Committee on science lectures and organisation, the action proposed to be taken by this Committee in the following resolutions was referred to the Council and sanctioned.

"1. That a Sub-Committee, consisting of Dr. Carpenter, Prof. Williamson, Prof. W. G. Adams, Dr. Hirst, Mr. Geo. Griffith, Dr. Michael Foster, and Prof. Roscoe, be appointed for one year for the purpose of preparing a list of Lecturers for the consideration of this Committee, and of communicating with the various towns with the view of establishing a system of Science Lectures throughout the country.

"2. That the names of the proposed Lecturers be selected (with their consent) from amongst the Members of the General Committee of the Association, or from amongst the Graduates of any University in the United Kingdom ; and that the subjects upon which the Lectures be delivered shall be such as are included in one or other of the Sections of the Association."

The Committee have drawn up a Report, dealing generally with the subject of their inquiry, which the Council recommend should be referred to the Committee of Recommendations.

The Council have had under consideration the question of enabling Members, who are unable to be present at the Meetings, to obtain the Journal and other printed papers, and they have adopted a Regulation as follows :—

"The Journal, President's Address, and other Printed Papers, issued by the Association during the Annual Meeting, will be forwarded daily to Members and others on application and prepayment of *2s. 6d.* to the Clerk of the Association, on or before the first day of the meeting."

The Council have added the following names of gentlemen, present at the last meeting of the Association, to the list of Corresponding Members, viz. :—His Imperial Majesty the Emperor of the Brazils, Prof. Dr. Colding, Dr. Güssfeldt, Dr. Lüroth, Dr. Lutken, Dr. Joseph Szabó.

The following resolution was then agreed to :—"That the members in the following list be constituted, with the president and vice-presidents of the meeting, the past presidents of former years, the trustees, the general and assistant-general secretaries, a Committee of Recommendations, viz. :—Mr. Gassiot, Prof. Henry Smith, Colonel Strange, Prof. Williamson, Mr. Abel, Prof. Martin Duncan, Dr. Burdon Sanderson, Colonel Lane Fox, Prof. Michael Foster, Sir Walter Elliot, Prof. Wyville Thomson, Sir Henry Rawlinson, Mr. Newmarch, Mr. J. G. Fitch, Prof. Hawkins, Mr. Siemens, Mr. Hawkshaw."

* See NATURE, vol. vi., p. 157.

The incident of greatest interest in Thursday's meetings was when, at the close of Sir John Lubbock's presidential address to Section D, he alluded to the unworthy treatment which Dr. Hooker had received at the hands of a department of the Government, the name of the distinguished Director of Kew bringing down a perfect storm of applause, which was repeated when Dr. Carpenter, in proposing a vote of thanks to Sir John, spoke of the "low and degrading view of science" entertained by a member of Her Majesty's Government. The Section adopted the following resolution:—"That this section would view with regret any change introduced into the botanical establishment at Kew which would tend to affect its completeness, or to impair its scientific character; and that the attention of the Council be called to the subject, with a request that they will take any steps they may deem desirable." This resolution was carried to the Committee of Recommendations, where, however, we regret to say, it halted, and was not allowed to proceed.

The *soirée* on Thursday evening was a highly successful one, and well brought out the capabilities of Brighton and the Brightonians for managing entertainments of this kind. The arrangements, indeed, may be said to have been perfect; and there was a veritable *embarras de richesse* of objects of interest. The magnificent array of microscopes, the splendid Willett collection of the fossils of Sussex, the collection of living flowering plants of the county formed by the Brighton and Sussex Natural History Society, and the pictures in the new museum gallery, were alike objects of attraction. It was universally admitted to be one of the most brilliant gatherings ever held in Brighton.

On Friday Section E was of course the one object of popular attraction. The scenic, not to say dramatic, character of the narrative by which Mr. Stanley preceded the reading of his paper produced a great effect, though probably the discussion which followed was hardly so exciting as the popular expectation had calculated on. Sir Henry Rawlinson's graceful acknowledgment, as President of the Geographical Society, of the sense entertained of Mr. Stanley's energy, and of the substantial value of his services to Dr. Livingstone, met with a hearty response. With regard to the general upshot of the "Livingstone Day," it may be said that while Mr. Stanley's narrative itself carried conviction to the minds of some who were previously sceptical, the manner in which it was delivered was unfortunate in the extreme. In the lamentable episode at the dinner of the Medico-Chirurgical Society on Saturday, when Mr. Stanley abruptly left in consequence of a discourtesy offered him by some of the members, there was undoubtedly fault on both sides, though nothing can justify the want of courtesy exhibited towards a stranger and a guest.

Not more than fifty individuals were collected in the room of Section A, at the Albion Hotel, to hear the most important proposition made at this meeting, by Lieut.-Col. Strange, in the form of a paper entitled, "On the duty of the British Association with respect to the distribution of its funds." The view of the distinguished author of the paper was that which has already been advocated in our columns,* that the Association ought not to grant money in aid of objects which it is the duty of the State to undertake. Such a course, he maintains, only encourages the Government in its present Philistine attitude towards Science; while if a contrary course were pursued, though Science might for a time suffer, a sounder and truer policy would ultimately be forced. In the discussion which followed the reading of the paper, the chief objection started to the proposal was that it will be necessary first of all to raise up a race of statesmen who have received a more complete scientific training, and will consequently have minds more open to the value of scientific research. This argument was forcibly advocated by Prof. H. Smith; to whom Col. Strange replied that, if we are to wait for this, we must be content to be a whole generation behind France and Germany, which countries are both keenly alive to the necessity of the promotion of scientific inquiry.

* See Nature Vol. vi. p. 297.

Saturday was, as usual, a kind of half-day, the brilliant weather inducing all who could possibly avail themselves of the various attractions in the form of excursions, scientific or picturesque, in the neighbourhood.

On Monday Section F was crowded to hear Miss Lydia Becker read her paper "On the attendance and education of girls in the elementary schools of Manchester." The object of the paper was to enforce the necessity for giving equal advantages to girls as to boys in the matter of education; and it was listened to with marked attention, the discussion which followed exciting also great interest. Considerable astonishment was caused by the statement that even the last Revised Code enforced a higher standard for male than for female pupil-teachers. On this the President of the Section, Prof. Fawcett, commented severely, and strongly urged the justice of allowing women to exercise the highest gifts of their nature as freely as men. The discussion proceeding this morning in the same section as Miss Shireff's paper on Female Education, embraces the higher rather than the primary department of the subject, and will be the great feature of to-day's proceedings.

At the meeting of the General Committee held yesterday letters of invitation for the meeting of 1874 were read from Belfast, Glasgow, Bristol, and Bath.

Mr. De La Rue proposed that Belfast should be selected for 1874. Mr. Pengelly seconded, and the resolution was carried unanimously.

Prof. Williamson proposed that Dr. James P. Joule, LL.D., D.C.L., F.R.S., be appointed president-elect of the Association for the meeting at Bradford, which was seconded by Prof. Rankine, supported by Sir W. Thomson, and carried by acclamation.

The next meeting was fixed for September 19, 1873.

The Earl of Rosse, Lord Houghton, Right Hon. W. E. Forster, M.P., Mayor of Bradford, Mr. Gassiot, D.C.L., F.R.S., Prof. Phillips, D.C.L., F.R.S., Mr. T. Hawkshaw, F.R.S., were requested to accept the office of vice-presidents-elect of the Association.

The following alterations were made in the list of the ordinary members of the Council:—Mr. De La Rue, Mr. W. H. Flower, Sir Henry Rawlinson, and Mr. Sclater, were substituted for Prof. Foster, Mr. Gassiot, Mr. Simon, and Mr. Wallace.

Dr. Michael Foster was appointed one of the general secretaries, in the place of Dr. T. Thomson, and Mr. John Ball, F.R.S., Colonel A. Lane Fox, F.G.S., F.S.A., and Mr. Gwyn Jeffreys, F.R.S., were appointed auditors. The other officers were re-elected.

The lecture in the evening by Prof. Clifford, "On the Aims and Instruments of Scientific Thought," was undoubtedly the great intellectual treat of the meeting. It is impossible to give in a few words any idea of the lecture, which we hope to reprint at length. Suffice it to say that it presented some of the most abstruse problems which can form the subject of scientific thought, in a manner so lucid and sparkling as to enchain the audience in rapt attention, notwithstanding that it was unillustrated by a single experiment, like the very beautiful ones exhibited at Mr. Spottiswoode's admirable lecture to working men delivered the previous Saturday evening, of which a report will be found in our columns.

The number of distinguished foreigners attending the meeting is considerably larger than was anticipated. Among those not already named may be mentioned Dr. Hilyard, of the U.S. Coast Survey, Prof. Semper, of Wurtemberg, Prof. Gervais, of Paris, Prof. Gaudrey, of Paris, M. Georges Pouchet, Dr. Anton Dohrn, Prof. Richter, of St. Petersburg, &c.

## Mr. Spottiswoode's Lecture to Working Men on Sunlight, Sea, and Sky

There are many ways in which men have looked at life, the higher kind of life, that ideal which each of us forms in his own mind to which we each hope that we are always tending. But all these various ideas may for the most part be grouped under two heads: the Ideal of Rest, and the Ideal of Work. "Rest, rest!" said a brave old German worker, "shall I not have Eternity to rest in?" That represents one view. "Work, work!" said another, "must I not work now, that I may the better work in Eternal Life?" That represents the other. But without entering upon the somewhat transcendental question of a future life, these ideas and aspirations have a meaning and reality even in the life which we now live. How do we hope to spend the leisure which old age may some day bring? Or, nearer still, when the day's work is done, and the day itself is not quite spent; or when such holiday as may befall each of us comes round, how do we hope to spend the time? Do we long for mere rest, for that

> land
> In which it seemed always afternoon.

Do we desire to sit us

> down upon the yellow sand
> Between the sun and moon upon the shore,

and sing with the lotus eaters

> All things have rest; why should we toil alone,
> Nor steep our brows in slumber's holy balm,
> Nor hearken what the inner spirit sings.
> There is no joy but calm.

Or do we rather with Ulysses say,

> How dull to pause, to make an end
> To rust unburnished, not to shine in use!
> As tho' to breathe were life. Life piled on life
> Were all too little, and of one to me
> Too little remains; but every hour is saved
> From that eternal silence, something more,
> A bringer of new things; and vile it were
> For some [few] suns to store and hoard myself,
> And this gray spirit yearning in desire
> To follow knowledge like a sinking star
> Beyond the utmost bounds of human thought.

To which of these two ideals I myself lean has perhaps already betrayed itself; and that being so, I shall venture to consider your presence here a proof that, for this evening at least, you side with me, and that you are willing to spend an hour of your leisure in an intellectual effort to see a little deeper into those phenomena which Nature in this place and at this season displays with such profusion and splendour.

But at the outset I must warn you that we are met by a difficulty, for the surmounting of which you must rely upon yourselves rather than upon me. It is this: the phenomena to which I propose to draw your attention, although taking place nearly every day, and all day long, and in almost every direction, are veiled from our eyes; and it is only by the use of special appliances to aid our eyes that they can be made visible. It will be my business to supply these appliances, and, reproducing on such scale as may be possible within these four walls the optical processes which are going on in the sea and sky outside, to exhibit the hidden phenomena of which I am speaking. But it must be your part to transport yourselves mentally from the mechanism of the lecture-room to the operations of Nature, and by a "scientific use of the imagination" (to adopt what has now become a household word at these meetings) to connect the one with the other.

Now the main point in question is this: that light, when subjected to the very ordinary processes of reflexion from smooth surfaces, such as a window, a mahogany table, or the sea itself, or when scattered to us from the deep clear sky, undergoes in many cases some very peculiar changes, the character and causes of which we have come here to investigate. The principal appliance which will be used to detect the existence of such changes, as well as to examine their nature, consists of this piece of Iceland spar, called—from the man who first constructed a compound block of the kind—a Nicol's prism, and this plate of quartz or rock crystal; both of which, as you will observe when the light passes through them, are clear, transparent, and colourless, and both of which transmit the direct light from the electric lamp with equal facility, however they may be turned round about the beam of light as an axis.

If, however, instead of allowing the beam to fall directly upon the Nicol, we first cause it to be reflected from this plate of glass, we shall find that the process of reflexion has put the light into a new condition. The light is no longer indifferent to the rotation of the Nicol; in one position of the Nicol the light passes as before, but as the instrument is turned round the light gradually fades, and when it is turned through a right angle the light is extinguished. Beyond this position the light reappears, and the same changes of fading and revival are observed in the light for every right angle through which the instrument is turned.

But these phenomena are susceptible of a very beautiful modification by the interposition of this plate of quartz between the reflecting surface and the Nicol. The changes in the light are no longer mere alterations of brightness, but exhibit a succession of colours resembling in their main features those of the rainbow or spectrum.

The peculiar condition to which light must be brought in order that these phenomena may be produced is called polarisation; and although an explanation of its nature must be reserved until later, I beg you to notice that it is effected in this instance by reflexion from a plate of glass. A similar effect is produced if light be reflected from many other substances, such as the leaves of trees, particularly ivy, mahogany furniture, windows, shutters, and often roofs of houses, oil paintings, &c., and last, but not least, the surface of water. In each of these cases the alternations of light and darkness are most strongly marked, and the colours (if a quartz plate be used) are most vivid, or, in technical language, the polarisation is most complete, when the light is reflected from each substance at a particular angle. In proportion as the inclination of the light deviates from this angle the colours become fainter, until, when it deviates very greatly, all trace of polarisation at last disappears. Without occupying the time necessary to shift our apparatus so as to exhibit this with the glass plate, we may alter the reflecting surface from glass to water, and by projecting on the screen the beautiful phenomena of liquid waves, make visible the different degrees of polarisation produced at the variously inclined portions of the surfaces of those waves. A tea-tray will serve as well as anything else to form our little sea, and a periodic tap at one corner will cause ripple enough for our present purpose. The waves now appear bright on the screen, and although brighter in some parts than in others, they are nowhere entirely dark. But on turning round the Nicol the contrast of light and darkness becomes much stronger than before. Here and there the light is absolutely extinguished; in these parts the polarisation is complete, in others incomplete in various degrees. And if the quartz plate be again introduced we have the beautiful phenomena of iris-coloured rings playing over the surface of our miniature sea.

Now, that which you see here produced by our lamp and tea-tray, you may see any day under the bright sky of this southern coast. By using an apparatus such as we have here, or a simpler one which I will immediately describe, you may bring out for yourselves these phenomena of colour, and thereby detect the profusion of polarisation which Nature sheds around us. But before describing it, there is one peculiar feature of all these experiments which must be noticed—namely, that the same results would be produced if we changed the positions of the lamp and the screen. The light which is now polarised by the glass or the water, and examined by the Nicol, might equally well be polarised by the Nicol and examined by the glass or the water. And, therefore, if we find that any contrivance will serve for the one purpose, we may conclude that it will serve equally well for the other.

And now a word about that simpler apparatus. When light falls upon a transparent substance, part is reflected, part transmitted. If, therefore, the reflected part is polarised (and you have already seen that this is sometimes the case), it is not surprising that the transmitted part should be so also. And further, if the polarisation by a single reflexion or transmission is incomplete, it will become more and more complete by a repetition of the processes. This being so, if we take a pile of glass plates—say half-a-dozen, more or less, the thinner the better—and hold them obliquely before our eye at an angle of about 30° (say one-third of a right angle) to the direction in which we are looking, we shall have all that is necessary to detect the presence of polarisation; and if, further, we hold a piece of talc or mica, such as is commonly used as a cover to the globes of gas-burners, beyond the pile of plates, colour will be produced in the same general manner as with the quartz, although with some essential difference in detail.

Suppose that we now turn our attention from the sea to the sky, and that on a clear bright day we sweep the heavens with our apparatus, or polariscope, as it is called, we shall find traces of polarisation colours brought out in a great many directions. But if we observe more closely we shall find that the most marked effects are produced in directions at right angles to that of the sun, when, in fact, we are looking across the direction of the solar beams. Thus, if the sun were just rising in the east or setting in the west, the line of most vivid effect would lie on a circle traced over the heavens from north to south. If the sun were in the zenith, or immediately overhead, the most vivid effects would be found round the horizon; while at intermediate hours the circle would shift round at the same rate as the clock, so as always to retain its direction at right angles to that of the sun.

Now, what is it that can produce this effect—or what even produces the light from all parts of a clear sky? The firmament is not a solid sphere or canopy, as was once supposed; it is clear, pure space, with no contents, save a few miles of the atmosphere of our earth, and beyond that the impalpable fluid or ether, as it is called, which is supposed to pervade all space, and to transmit light from the further limits of the stellar universe. But, apart from this ether, which is certainly inoperative to produce the sky appearance as we see it, a very simple experiment will suffice to show that a diffusion, or, as it has been better called, a scattering of light, is due to the presence of small particles in the air. If a beam from the electric lamp, or from the sun if we had it, be allowed to pass the room, its track becomes visible, as is well known by its reflexion from the motes or floating bodies, in fact by the dust in the air. But if we clear the air of dust, as I now do by burning it with a spirit lamp placed underneath, the beam disappears from the parts so cleared, and the space becomes dark. If, therefore, the air were absolutely pure and devoid of matter foreign to it, the azure of the sky would be no longer seen, and the heavens would appear black; the illumination of objects would be strong and glaring on the one side, and on the other their shadows would be deep, and unrelieved by the diffused light to which we are accustomed.

Now, setting aside the dust, of which we may hope that there is but little on the downs behind your town, or out to sea in front, there are always minute particles of water floating in the atmosphere. These vary in size from the great rain drops which fall to earth on a sultry day, through the intermediate forms of mist and of fine fleecy cloud, to the absolutely invisible minuteness of pure aqueous vapour which is present in the brightest of skies. It is these particles which scatter the solar rays, and suffuse the heavens with light. And it is a curious fact, established by Prof. Tyndall while operating with minute traces of gaseous vapours (which I can only notice in passing, because it belongs only in part to our present subject), that while coarse particles scatter rays of every colour equally—in other words, scatter white light—finer particles scatter fewer rays from the red end of the spectrum, while the finest scatter only those from the blue end. And in accordance with this law, clouds are white, clear sky is blue.

But beside this fact, viz., that light scattered laterally from fine particles is blue, the same philosopher perceived that light so scattered is polarised; and by that observation he again connected the celestial phenomena described above with laboratory experiments.

By a slight modification of his experiment, due to Prof. Stokes, I hope to make this visible to the audience. It will probably be in your recollection that when polarised light passed through a Nicol, its intensity is unaltered when the Nicol is in one position, but it is destroyed when it is in another at right angles to the first. I now pass the beam from the electric lamp through a tube of water containing a few drops of mastic dissolved in alcohol. The mixture so formed holds fine particles of mastic in a state of suspension; these scatter the light laterally, so as to be visible, I hope, to the entire audience. And if we were to examine with a Nicol this scattered light, we should find the phenomena of polarisation. But, better still, we can cause the light to pass through the Nicol before being scattered, and produce the same effect, not only upon the particular part to which our eye is directed, but upon the whole body of scattered light. As the Nicol is turned, the light seen laterally begins to fade; and when the instrument has been turned through a right angle, the only parts remaining visible are those which are reflected from the larger impurities floating in the water independently of the mastic. An effect still more beautiful, and at the same time more instructive, can be produced by interposing, as was done in the case of reflexion, a plate of quartz between the Nicol and the medium which causes polarisation. The whole beam is now suffused with colour, the tint of which changes, as did the tints on the waves, while the Nicol is turned round. And not only so, but while the Nicol remains at rest, the tints are to be seen scattered in a regular and definite order in different directions about the sides of the beam. This may be shown by reflecting from a looking-glass a side of the beam not visible directly, and by comparing the tint seen by reflexion with that seen direct. But this radial distribution of colours may also be shown in a more striking manner, by putting together two half plates of quartz of the kinds which have the property of distributing the colours in opposite orders, and by observing the result along the line of junction. The compound plate here used is known by the name of a biquartz, and affords one of the most delicate tests of the presence of polarised light. In this case, when the Nicol is turned round, the colours of the two halves follow one another in opposite orders; and as each series is completed twice in a revolution of the Nicol, the halves of the quartz will be of the same colour four times in a revolution—twice of one colour and twice of its complementary.

The colours which we have here seen are those which would be observed, as before remarked, upon examining a clear sky in a position at right angles to that of the sun; and the exact tint visible will depend upon the position in which we hold the Nicol, as well as upon that of the sun. Suppose, therefore, we direct our apparatus to that part of the sky which is all day long at right angles to the sun, that is, to the region about the north pole of the heavens (accurately to the north pole at the vernal and autumnal equinox); then, if on the one hand we turn the Nicol round, say in a direction opposite to that of the sun's motion, the colours will change in a definite order; if, on the other, we hold it fixed, and allow the sun to move round, the colours will change in a similar manner. And thus, in the latter case, we might conclude the position of the sun, or, in other words, the time of day, by the colours so shown. This is the principle of Sir Charles Wheatstone's polar clock; one of the few practical applications which this branch of polarisation has yet found. The action of such a clock may be thus roughly shown. There is now projected upon the screen a dial plate, in which the hours are arranged in their usual order, but are crowded together into half their usual space, viz., twelve hours occupy half instead of the entire circle. The inner part of the disc is covered with a plate of selenite (mica would serve the purpose equally well), which is capable of revolving about its centre, and which, as you see, in a particular position shows colour more strongly than in any other. An hour hand is roughly drawn upon the plate. The apparatus here used is furnished with two Nicol's prisms, the hinder one of which imitates the polarising effect of the sun, while that in front is the instrument with which we should examine the north pole of the sky. The whole is now so arranged that when the plate shows brightest colour the hand points to XII., say noon. As the back Nicol is turned round, say as the sun begins to sink, the colour fades; and when the plate is turned so as to restore the colour, the hand points to I. Similarly, as the back Nicol is turned gradually further, representing the passage of the sun westward during the afternoon, the position of the plate giving the strongest colour, as indicated by the hand, corresponds to the successive hours of the dial; and when the Nicol has been turned through 90°, that is, when the sun has reached the horizon, the hand has moved from XII. to VI. In this way, as its inventor has remarked, a dial may be constructed which will work equally well in sunshine or in shade, or even when the sun itself is overcast, provided only that there be a patch of clear sky to the north.

Up to this point we have reproduced in an experimental fashion the general every-day phenomena, both celestial and terrestrial, which give rise to polarisation; and we have given such general account of them as will serve to connect them together, and to show that they all belong to one system of laws affecting the nature of light. I should, however, regret, and I feel confident that you would share in that regret, if we were to leave the subject with its surface as it were merely scratched, and without any attempt to penetrate deeper into its substance. With your permission, therefore, we will devote such time as you may be still willing to grant me to a few elementary experiments in polarisation, which, while certainly not less beautiful than those which you have already seen, will, perhaps, better illustrate the nature of the processes which we are now trying to investigate.

Polarised light, as indicated at the outset, is distinguished from common light by the presence of certain peculiarities not ordinarily found, and these peculiarities are to be detected only by means of special instruments. Light which has been reflected or transmitted at particular angles from various substances, light which has been scattered by small particles, is found to be in this peculiar condition. So likewise is light which has passed through this transparent piece of Iceland spar, or Nicol's prism, as it is called. Yet the light which has so passed through, and which is now projected on the screen, is to the unaided eye in no way different from the same light before its passage. Nevertheless, if we examine or analyse it by means of a second Nicol, we shall find the peculiarity of its condition revealed. For if either of the Nicols be turned gradually round (and remember that they are both transparent colourless blocks of crystal) the light gradually fades until, when it has been turned through a right angle, the light is absolutely extinguished. On turning the Nicol further the light revives, and afterwards again fades, in such a manner that in a complete revolution the light is twice at its brightest, and twice is extinguished. Now, light is due to extremely small and rapid vibrations of a very subtle medium, which is supposed to pervade all space. The fact that vibrations (*i.e.* motions to and fro) in one direction can produce waves advancing in another will be familiar to all of you who have watched the movement of a cork floating on the sea. You will have noticed that the cork has simply moved up and down, or nearly so, while the waves have passed, as it were, under it, along the surface of the water.

Now, in order to make clearer to our minds how this wave motion is produced, I will throw the electric light upon a machine devised for the purpose. You now see a horizontal row of knobs As the slider is pushed in the knobs at one end begin to rise in succession until each has in turn attained its greatest elevation. Immediately after reaching its highest position it begins to descend; so that the knobs first rise and then fall in regular succession, and continue to rise and fall in the same manner so long as the motion is continued. Each of the knobs, beginning from number one, is thus successively at the highest position, while at the same moment those immediately before and behind it are at lower positions. And as the knob which is at the highest position represents what we call the crest of the wave, the crest will pass successively along all the knobs, beginning from the first. Thus the waves are transmitted along the line, while the vibrations take place across it. If the line of knobs represent the direction of a ray, their motions will represent the vibrations and waves to which the light is supposed to be due. In ordinary light these vibrations may take place in any directions perpendicular to the ray; and the effect of the crystal of which the Nicol is made, is to restrict these vibrations to a particular direction. In the arrangement now before you the first Nicol causes the vibrations to be altogether horizontal. When the second Nicol is placed similarly to the first, it will obviously have no further effect upon the light; but if it be turned through an angle, it will transmit only vibrations inclined to the horizontal at that angle; that is, only such part of the original horizontal vibrations as can be brought into the inclined direction; in other words, it will transmit only part of the light. And as the inclination is increased the part of the light transmitted will diminish, until, when the second Nicol is in a position to transmit only vertical vibrations (*i.e.*, when it has turned through a right angle), the light will vanish. Such is an explanation of this fundamental experiment in polarisation on the principle of what is called the Wave Theory of Light; and I have ventured to give it in some detail, because it is the key to all others, and forms a starting point for any who may desire to go further in the subject; and it is a remarkable feature in this Wave Theory of Light that the results of many other experimental combinations, to some of which we will now proceed, might be predicted upon the principles already laid down.

If a plate of crystal, such as selenite, be placed between the two Nicols, and turned round in its own plane, it will be found that in certain positions at right angles to one another no effect is produced. These may be called neutral positions. In all other positions the field is tinted with colour, which is most brilliant when the plate has been turned through half a right angle from a neutral position. If one of the Nicols be turned, the selenite remaining still, the colour will fade and entirely vanish when the Nicol has turned through half a right angle. After this position the complementary colour will begin to appear, and will be brightest when the Nicol has completed a right angle.

The colours so produced depend upon the thickness of the plate; thus, if we take a plate of selenite merely split and not ground to a uniform thickness, we shall have a variety of tints indicating the thickness of each particular part; or we may, by a careful arrangement of suitable thicknesses, produced a coloured pattern of delicacy and variety dependent only upon the skill with which the pieces have been worked.

A plate of the same crystal worked into a concave form is interesting as showing not only that the colours are dependent upon the thickness, but also that when, with an increasing or diminishing thickness of crystal, they have run through their cycle, they begin again; in other words, that the phenomenon is periodic. The field is then covered with a series of concentric rings, each of which is tinted with colours in a regular order.

In all these instances it is clear, from the experiments themselves, as well as from other experiments which form no part of our present subject, that the modifications which light undergoes are due to the internal structure of the crystals used. And it becomes a question of interest whether it be not possible, by some mechanical process, performed upon a non-crystalline substance, such as glass, so far to imitate a crystalline structure as to reproduce some of the optical results already shown. For this purpose let us take a bar of glass. On interposing it in its natural state between the Nicols when crossed, we find that no effect is produced in the dark field upon the screen. If, however, I merely press it as though with the intention of bending or breaking it, there will be at once brought about a condition of strain capable of affecting the vibrations of the ray falling upon it, to such a degree that some of them will find their way through the screen. And this result may be explained on precisely the same mechanical principles as in the case of the crystal. The effect may be heightened by placing the piece of glass in a vice, and screwing it up so as to bend or compress it to a greater degree than was possible by the hand alone. When this is done the direction and even the relative amount of torsion or compression of the different parts will be noted down as it were by the forms and hues of the figures thrown upon the screen.

The same kind of effect is shown by a piece of glass unevenly heated; but better still by glass which has been rapidly and unevenly cooled,—unannealed glass, as it is called. In the pieces now before you, the outside, having become first cooled and solidified, has formed a rigid framework, to which all the interior has been obliged to conform. The interior parts have consequently undergone strains and pressures in different directions and in different degrees, in accordance with which each part has become the subject of a definite internal molecular arrangement; and these, by each in its own way, modifying the light which they transmit, give rise to the figures now before you.

I will conclude this series of experiments by one which, although not so beautiful or striking as those which you have already seen, is still interesting as bringing the subject home to us, and as the only application of polarisation to commercial life which has yet been made. You will recollect the brilliant sequence of colour shown by a quartz plate when submitted to polarised light. Well, the effects produced by that quartz plate are also produced by not only some other crystals, but, what is very remarkable, also by many of their solutions, *e.g.* by that of sugar. Into this tube I have put a solution of sugar; when it is placed before the lamp, polarisation colours are shown on the screen, while the liquid itself remains colourless. If the solution be strengthened by the addition of more sugar, the tints vary; and by accurate observation of the colours for different positions of the Nicol, the strength of the solution may be determined. An instrument constructed with proper means of registering these phenomena with accuracy is called a saccharometer.

These experiments might be multiplied almost indefinitely, and many a long winter evening might be spent in following polarisation into other branches of science upon which it has something to say. For example, on examining a variety of vegetable and animal tissues, slices of wood, fronds of fern, scales of fish, hair, horn, mother of pearl, &c., with a suitable polariscope, we should find that they exhibit, internally, definite structural characters, capable of affecting the light, which they transmit in the same general way as do crystals. Or again, if we were to apply the principles established in an early part of this lecture to the conditions of sky, aspect, and time of day under which the photographer notices that he can obtain the most perfect image in his picture, we should find that they correspond with those which will furnish him with daylight in the most perfectly polarised condition.

Once more, among the many and curious phenomena which are visible during a solar eclipse, there is one which has longer than any other refused to lift its veil to the solicitations of science. I mean that halo of light, or corona as it is called, which extends beyond the dark disc of the moon, beyond those red flames of burning gas which the researches of Lockyer, of Janssen, and of others have brought almost home to us, far away for millions of miles into distant regions of space. It was pre-eminently to investigate this phenomenon that the last Eclipse Expedition, furnished with funds by Her Majesty's Government at the instance of this British Association, was sent out. And upon this investigation all the powers of the twin instruments of modern times, the spectroscope and the polariscope, were turned. The spectroscope could tell us the nature of the substances to the combustion of which the light is due, and even the conditions of temperature and of pressure under which the combustion is taking place; but it could not disentangle those parts of the phenomenon which are due to direct, from those which are due to reflected or to scattered light. It was for the polariscope to tell us whether the corona is a terrestrial effect,—a mere glare, in fact, from our own atmosphere,—or a true solar phenomenon; and in the latter issue, whether any of it is due to direct rays from incandescent matter, or all of it to rays originating in such incandescent matter below, but scattered laterally from gases which have cooled in the upper regions surrounding the sun. This question has not even yet received a definitive answer. But the brief account given within the last few days by Mr. Lockyer, in anticipation of his more complete digest of the voluminous reports from the various branches of the Expedition, seems to justify us in the conclusion that the corona is substantially a solar phenomenon due not to direct but to reflected or scattered rays.

The principle upon which the polariscope enables us to make these refined distinctions in such far off phenomena is after all very simple. If the corona were due wholly to the effect of our atmosphere on such light as reaches us during a total eclipse of the sun, the whole of that light would be similarly affected, because it comes very nearly from the same part of the heavens. In other words, its polarisation would be uniform, and the corona, when examined by a Nicol and quartz, would appear of a uniform colour. But if the phenomenon were wholly due to the sun and its surroundings, the light would be affected, if at all, differently in different directions drawn outwards (like spokes or radii of a wheel) from the sun as a centre. In other words, its polarisation would be arranged spoke-wise, or, to use the technical term, radially; and the corona, when examined as before, would vary in colour on different sides of the sun.

I have already drawn largely, perhaps too largely, upon your patience. But it will not have been without purpose that, besides witnessing the exhibition of a few experiments, you should have seen, at least in outline, what manner of thing a scientific investigation is. Well, whatever it is (and I will not weary you with a dry statement of its processes), the foundation of it must always be laid in careful, accurate, and intelligent observation of facts. And it is a consideration which may well stir the hearts of us outsiders of science, especially on an occasion when we come face to face with some of the greatest philosophers of our time, than any one of us, by practising his eye and riveting his attention, may contribute some natural fact, some fragment of knowledge, to the common stock. And surely has not this a particular significance and importance to us, at a period when, by shortening the hours of labour, more leisure, as we may hope, will be at the command of many? It will, I take it, be our own fault if we spend that leisure in walking through dry places seeking rest; for, to those who have the eyes to see and the spirit to discern, the world is neither dry nor barren; but rather, it is like the mountain as it appeared to the servant of the prophet when his eyes were opened, full of beauty and wonder, of mystery and power,—full of hosts from all nations, striving manfully onward to promised lands of knowledge and of truth, and waging ceaseless warfare against ignorance and prejudice, and the long train of evils which are consequent upon them. And if, as the eventide of life draws on, our eye wax dim, and our step grow weary, so that we can no longer follow, we may still lay us down to rest in some unknown spot, in the full confidence that others will not be wanting to fill our places and gain fresh ground, though we may not live to see it.

## SECTION A

### Sectional Proceedings

*Ad interim Report on the Results Obtained by the British Association Eclipse Expedition of* 1871, by J. Norman Lockyer, F.R.S.

*I. New Instruments*

These were as follows:—

1. A train of five prisms to view the corona.
2. A large prism of small angle placed before the object glass of a telescope.

On these instruments I may remark that the Royal Astronomical Society, in the first instance, invited me to take charge of an Expedition to India merely to conduct spectroscopic observations; but although this request did me infinite honour, I declined it, because the spectroscope alone, as it had been used before, was, in my opinion, not competent to deal with all the questions now under discussion. Thus some of the most eminent American observers had come to the conclusion that the spectrum of hydrogen observed in the last eclipse round the sun, to a height of 8 minutes, was a spectrum of hydrogen "far above any possible hydrogen" at the sun. Hence it was in some way reflected. Now with our ordinary spectroscopic methods it was extremely difficult, and one might say impossible, to determine whether the light which the spectroscope analysed was really reflected or not; and that was the whole question.

It became necessary, therefore, in order to give any approach to hopefulness, to proceed in a somewhat different way in the 1871 expedition, with regard to the spectroscope, and, to guard against failure, to supplement such observations with photographs.

To understand the method adopted, let us suppose a train of prisms. Take one prism out of the train, and consider what will happen if we illuminate a slit with a monochromatic light and observe it through the prism. If we render sodium vapour incandescent and illuminate the slit by means of it, we get a bright yellow image of the slit, due to the vapour of the metallic sodium only giving us yellow light. But why is it that we get a line? Because we employ a line slit. If, instead of a straight line, we have a crooked line for the slit, then we see a crooked line through the prism. Going one step further: Suppose that instead of a line, whether straight or crooked, we have a slit in the shape of a ring, we see a ring image through the prism. And then comes this point: If, when we work in the laboratory, we examine these various slits, illuminated by these various vapours, if we observe the corona in the same way, we shall get a ring built up by each ray of light which the corona gives to us; since we know, from the American observations, that there were bright lines in the spectrum of the corona, as observed by a line slit: in other words, the corona examined by means of a long train of prisms should give us an image of itself painted by each ray which the corona is competent to radiate towards us.*

These were the considerations which led to the adoption of this new attempt to investigate the nature of the corona now in question. It was, to use a train of prisms, pure and simple, using the corona as the slit, a large number of prisms being necessary to separate the various rings we hoped to see, by reason of their strong dispersion.

This principle, good for a train of prisms such as I have referred to, is good also for a single prism in front of the object-glass of a telescope. Such was the method adopted by Prof. Respighi, the distinguished Director of the Observatory of the Capitol of Rome, who accompanied the expedition.

This method, if it succeeded, would be superior to the ordinary one in this way. If we were dealing merely with scattered light, then all the rings formed by vapours of equal brilliancy at the base of the chromosphere would be of the same height, while, if such scattering were not at work, the rings would vary according to the actual height of the vapours in the sun's atmosphere.

3. Integrating spectroscopes *driven by clockwork*.
4. A self-registering integrating spectroscope, furnished with telescopes and collimators of large aperture and large prisms. (This instrument was lent by Lord Lindsay.)
5. A polariscope-telescope, so arranged that the same observer could almost simultaneously observe both with the Savart and the Biquartz.
6. A polariscope-telescope, arranged for rapid sweeping round the corona at a given distance from moon's limb.

* After I had thought of this arrangement, and had secured an instrument to carry it out, Prof. Young, in a communication to NATURE, suggested the same method of observation.

## II. *The Main Results—Spectroscopic Observations*

It has been established that the idea that we do not get hydrogen above 10 seconds above the sun is erroneous, for we obtained evidence that hydrogen exists to a height of 8 or 10 minutes at least above the sun.

Just as the sun disappeared Prof. Respighi employed the instrument to which I have already referred to determine the materials of which the prominences which were then being eclipsed were composed, and he got the prominences shaped out in red, yellow, and violet light; a background of impure spectrum filling the field; and then as the moon swept over those prominences they became invisible. He saw the impure spectrum and the yellow and violet rings gradually die out, and then three broad rings painted in red, green and blue gradually form in the field of view of his instrument; and as long as the more brilliant prominences on both sides of the sun were invisible he saw these magnificent rings.

These rings were formed by C and F, which show us that hydrogen extends at least 7 minutes high, for had we been dealing with mere glare, *had we not been dealing with hydrogen itself we should have got a yellow ring as well.* In addition to the red ring and the blue and violet, which indicate the spectrum of hydrogen, he saw a bright green ring, much more brilliant than the others due to 1474.

While Prof. Respighi was observing these rings by means of a single prism and a telescope of some four inches aperture, some 300 miles away from him—he was at Poodocottah and I was at Bekul—I had arranged the train of five prisms. My observation was made intermediately, as it were, between the two observations of Prof. Respighi's. The observations may be thus compared:—

| | | | | |
|---|---|---|---|---|
| Respighi ... | C $D^3$ | | F G | Prominence at beginning of eclipse. |
| Lockyer ... | C | 1474 | F G | Corona 80 seconds after beginning of totality. |
| Respighi ... | C | 1474 | F | Corona mid eclipse. |

I had no object-glass to collect light, but I had more prisms to disperse it, so that with me the rings were not so high as those observed by Respighi, because I had not so much light to work with, but such as they were I saw them better because the continuous spectrum was more dispersed, and the rings—the images of the corona—therefore did not overlap. Hence doubtless Respighi missed the violet ring which I saw, but both that and 1474 were very dim, while C shot out with marvellous brilliancy, and $D^3$ was absent.

These observations thus tend to show, therefore, that instead of the element—the line of which corresponds with 1474—existing alone just above the prominences, the hydrogen accompanies it to what may be termed a great height above the more intensely heated lower levels of the chromosphere, including the prominences in which the lower vapours are thrown a greater height. With a spectroscrope of small dispersion attached to the largest mirror of smallest focus which I could obtain in England, the gaseous nature of the spectrum, as indicated by its *structure*, that is bands of light and darker intervals as distinguished from a continuous spectrum properly so called, was also rendered evident.

### *Photographs and Structure of Corona*

The photographic operations (part of the expense of which was borne by Lord Lindsay) were most satisfactory, and the solar corona was photographed to a greater height than it was observed by the spectroscope, and with details which were not observed in the spectroscope. Mr. Davis was fortunate enough to obtain five photographs of great perfection at Bekul, and Captain Hogg obtained some at Jaffna, but the latter lack in detail. The solar nature of most, if not all, of the corona recorded on the plates is established by the fact that the plates, taken in different places, and both at the beginning and end of totality, closely resemble each other, and much of the exterior detailed structure is a continuation of that observed in the inner portion independently determined by the spectroscope to belong to the sun.

This structure I was also enabled to observe in my 6¼ in. equatorial, even three minutes after totality was over, and we may now say that we know all about the corona, so far as the structure of its lower brighter levels—that portion, namely, which time out of mind has been observed both before and after totality—is concerned. It may be defined as consisting of cool prominences, that is to say, in this region of the corona we will find the same appearances as in prominences, minus the brightness. We find the delicate thread-like filaments which all are now so familiar with in prominences,—the cloudy light masses, the mottling, the nebulous structure, all are absolutely produced in the corona; and I may add that the fainter portion of the ring, some 5 minutes round the sun, reminded me forcibly in parts of the nebula of Orion, and of that surrounding $\eta$ Argus, as depicted by Sir John Herschel in his Cape observations.

While both in the prism and the 6¼-inch equatorial the corona seemed to form pretty regular rings round the dark moon, of different heights according to the amount of light utilised by the instrument, on the photographic plates the corona, which, as I have before stated, exceeds the limits actually seen in the instrument I have named, has a very irregular, somewhat stellate outline, most marked breaks or rifts *(ignored by the spectroscope)*, occurring near the sun's poles, a fact perhaps connected with the other fact that the most active and most brilliant prominences rarely occur there.

### *Sketches*

From the photographs in which the corona is depicted actinically we pass to the drawings in which it is depicted visually. I would first call attention to two drawings made by Mr. Holiday, who formed part of the expedition, and in whose eye every one who knows him will have every confidence.

First there is a drawing made at the commencement of the totality, and then a drawing made at the end. There is a wonderful difference between these drawings; the corona is in them very much more extensive than it is represented actinically on our plates.

In another drawing, made by Captain Tupman, we have something absolutely different from the photographs and from Mr. Holiday's sketches, inasmuch as we get an infinite number of dark lines and a greater extension than in the photographs, though in the main the shape of the actinic corona is shown.

The corona, as it appeared to me, was nothing but an assemblage of such bright and dark lines; it lacked all the structure of the photographs, and appeared larger; and I have asked myself whether these lines do not in some way depend on the size of the telescope, or on the absence of a telescope. It seems as if observations of the corona with the naked eye, or with a telescope of small power, may give us such lines; but that when we use a telescope of large power, it will give, close to the moon, the structure to which I have referred, and abolish the exterior structure altogether, leaving a ring round the dark body of the moon such as Prof. Respighi and myself saw in our trains of prisms, and I in the 6-inch telescope, in which the light was reduced by high magnification so as to bring the corona to a definite ring some 5 minutes high, while Prof. Respighi, using a 4-in. telescope, brought the corona down to a ring something like 7 minutes high.

Many instances of changing rays, like those seen by Plantamour in 1860, were recorded by observers in whom I have every confidence. One observer noted that the rays revolved and disappeared over the rifts.

### *Polariscopic Observations*

Mr. Lewis, in sweeping round the corona at a distance of some 6′ or 7′ from the sun's limb, using a pair of compensating quartz wedges as an analyser, which remained parallel to itself while the telescope swept round, observed the bands gradually to change in intensity, then disappear, bands of a complementary character afterwards appearing, thereby indicating radial polarisation.

Dr. Thomson at Bekul saw strong traces of atmospheric, but none of radial polarisation, with a Savart. With the same class of instrument the result obtained by myself was precisely similar, while on turning in the Biquartz, at the top and bottom of the image of the corona, *i.e.*, near the sun's equator, faint traces of radial polarisation were perceptible for a short distance from the moon's limb. Captain Tupman, who observed with the polariscope after totality, announces strong radial polarisation extending to a very considerable distance from the dark moon.

### *Reversal of Lines at beginning and end of Totality*

Captain Maclear, who was observing with me at Bekul, for some time just before the commencement of totality, but when the light of our atmosphere was cut off by the interposition of the dark moon, saw a large number of very fine lines of different heights at the base of the chromosphere.

Mr. Pringle, also at Bekul, saw many lines flash into the field of an analysing spectroscope carried by clockwork at the end of totality.

Captain Fyers, the Surveyor-General of Ceylon, observing with an integrating spectroscope, saw somethinglike a reversal of all the lines at the beginning, but nothing of the kind at the end.

Mr. Fergusson, observing with an instrument of the same kind, saw reversal neither at the beginning nor the end, though during totality he saw more lines than Captain Fyers.

Mr. Moseley states that at the beginning of the eclipse he did not see this reversal of lines. Whether it was visible at the end he could not tell, because at the close the slit had travelled off the edge of the moon.

Prof. Respighi, using no slit whatever, and being under the best conditions for seeing the reversal of the lines, certainly did not see it at the beginning, but he considers he saw it at the end, though about this he is doubtful.

From the foregoing general statement of the observations made on the eclipse of last year, it will be seen that knowledge has been very greatly advanced, and that most important data have been obtained to aid in the discussion of former observations. Further, many of the questions raised by the recent observations make it imperatively necessary that future eclipses should be carefully observed, as periodic changes in the corona may then possibly be found to occur. In these observations the instruments above described should be considered normal, and they should be added to as much as possible.

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## SECTION D

### SUB-SECTION ZOOLOGY AND BOTANY

### Opening Address by the President, Sir John Lubbock, Bart., M.P., F.R.S.

Although this would not, perhaps, be a fitting opportunity for discussing the importance and best mode of introducing the study of Natural Science into our great public schools, and though the question is still in a far from satisfactory position, yet I think I may congratulate the Section that some progress has been made in that direction during the last few years. To this result the influence of the British Association has no doubt greatly conduced. As yet, indeed, Natural Science is generally taught but to some of the elder boys, and certainly is very far indeed from having its due share of attention in relation to other subjects. I am happy to say, however, that most of the regulations which are being drawn up under the Public Schools Act, by the new governing bodies of the public schools, contain a provision that Natural Science shall be taught to all boys in their passage through the school. As the Royal Society has a representative in the governing bodies of all the public schools, we may fairly hope that this clause will not be allowed to remain a dead letter. I have no reason to suppose that any head-master will oppose the change; but it will of course be necessary for the governing bodies to allot a sufficient amount of funds to this purpose, so as to enable the head-masters to carry out the clause in an efficient manner. In several cases, moreover, and eventually I hope in all public schools, special scholarships and exhibitions will be devoted to Natural Science. When these changes come into full operation, they will doubtless greatly influence the system of education pursued in our preparatory schools. At present, I regret to say, that I know of no private school in England where Natural Science receives the attention it deserves. I must, however, in fairness add, that private schoolmasters are almost compelled to give, not the kind of education which they would themselves prefer, but that which is the most effective preparation for the course of study pursued at the public schools.

The Association has also urged on Government the importance of introducing the elements of Science into the elementary schools of the country, and a deputation from the Council waited on Mr. Forster with this object. In the new code shortly afterwards promulgated, Mr. Forster has admitted the principle, and allotted certain payments to extra subjects, coupled, however, with conditions which, as stated in the report of the Royal Commission on Scientific Instruction, render the promise to a great extent illusory. The subject is no doubt one of great difficulty; but Mr. Forster has distinctly stated that the Government have discarded the idea that the educational functions of Government should be confined to the encouragement of reading, writing, and arithmetic. The experience of some of our best schools, such, for instance, as those of Dean Dawes and Prof. Henslow, show clearly that elementary science can be introduced with the most excellent results, and I rejoice to see that some of our most important School Boards, for instance, those of London and Liverpool, have determined that elementary instruction in science shall be given in all the schools under their control.

If it is said that in such cases but a smattering can be given, I might ask, who has more? Those who are the most advanced in knowledge will be the first to admit how slight that knowledge is.

Indeed, every fresh observation, every new discovery, opens up new lines of inquiry. Take, for instance, the results of Mr. Darwin's great work on the Origin of Species. Mr. Darwin, as almost all biologists would now admit, has thrown a great light on a very interesting and difficult problem; yet in doing so, it suggested various new lines of inquiry, and in removing to a certain extent the veil from our eyes, discovered to us fresh fields for research, which promise most interesting results to those who will study them with diligence.

It is surprising how much, in spite of all that has been written, Mr. Darwin's views are still misunderstood. Thus Browning in one of his recent poems says:—

> That mass man sprung from was a jelly lump
> Once on a time; he kept an after course
> Through fish and insect, reptile, bird, and beast,
> Till he attained to be an ape at last,
> Or last but one.*

Speaking to such an audience as the present, it is unnecessary for me to point out that this is a theory which Mr. Darwin would entirely repudiate, which is utterly inconsistent with his views. Whether fish and insect, reptile, bird, and beast are derived from one original stock or not, they are certainly not links in one sequence. I do not, however, propose on this occasion to discuss the question of Natural Selection. But I may observe that it is one thing to acknowledge that in Natural Selection or the Survival of the Fittest Mr. Darwin has called attention to a *vera causa*, has pointed out the true explanation of certain phenomena: it is quite another thing to assume, as is too often done, that all animals are descended from one primordial source. For my own part, I am quite satisfied that Natural Selection is a true cause. Whatever may be the final result of our present inquiries, whether animated nature is derived from our ancestral source, or from a number of successive creations, the publication of the Origin of Species will not the less have constituted an epoch in the History of Biology.

How far the present condition of living beings is due to Natural Selection; how far, on the other hand, the action of Natural Selection has been modified and checked by other natural laws, by the unalterability of types, by atavism, &c.; how many types of life originally came into being; whether they arose simultaneously or successively,—these and many other similar questions remain to be solved, even if we admit the theory of Natural Selection. All this has, indeed, been clearly pointed out by Mr. Darwin himself, and would not need repetition, but for the careless criticism by which in too many cases the true question has been obscured.

[The remainder of the President's Address was occupied by an enlargement of his views respecting the Origin of Insects,† which we hope to present to our readers at a future time in a complete form.]

### SUB-SECTION ANATOMY AND PHYSIOLOGY

### Opening Address by the President, Prof. Burdon Sanderson, F.R.S.

We are met here for the purpose of hearing papers on Anatomy and Physiology. It would not have been inappropriate to have given you some account of the limits of the two very distinct sciences which are so designated; but as I am anxious to occupy your time for as short a period as possible, I shall content myself with saying that the few observations I have to make will have reference only to the two sciences to which I am myself attached. I make this preliminary explanation; for the positions of the two sciences in England are so different that much that I may say about Physiology is not applicable to Anatomy.

I should have been glad if it had been possible to have occupied this time in giving you a retrospective account of the

* Prince Hohenstiel Schwangau, p. 68.

† See Sir John Lubbock's paper in Nature, vol. v. p. 27.

*Progress* of *Physiological Research* during the past year. I had intended to do so, but was led to abandon my intentions on the ground that although the work done has not been inconsiderable, we in England have taken very little part in it. If I had attempted the task, I should have been but chronicling the doings of our friends in Germany, who are now holding their own scientific assembly in Leipzig. As I do not wish to talk about German physiologists to-day, I find it more agreeable and more encouraging to look forward than to look back; for although we English physiologists (I say physiologists advisedly, because the anatomist is not in the same position) must admit with regret that we have had very little to do with the unprecedented development of our science during the last two decades, we do not intend to continue in the same inactive condition in future.

Considering that half the purpose of our meeting in this section is to promote the progress of physiology, I do not think I can more properly occupy your time than in endeavouring to show in what direction efforts must be made to improve its position, and particularly to secure a future more fruitful of substantial results than the past has been.

I shall begin by asserting a general principle, which, as I go on, I shall endeavour to justify—that one great reason why physiological research is less successfully pursued in England than we could wish it to be, lies in the general want of scientific education. In illustration of this position, I shall refer first to that higher training which is required for the production of scientific workers or investigators; secondly to what may be called the education of public opinion, by the popularising agency of books and lectures; and lastly to the introduction of Natural Science as an element of education in our great schools and universities.

*Training of scientific workers.*—If a man wants to be a physiologist, he must, as things at present stand, study medicine. There is no logical reason for this; for, although medicine ought to be built on physiology, there is no reason why a physiologist should know anything about the art of curing diseases. Practically, however, it is the case that the kind of education which a man requires in order to be a physiologist is best obtained through a course of medical study. I confess myself to be of the opinion that this close relation between medicine and physiology is likely to be a permanent one, on the general ground that any science is likely to be studied with more earnestness by those who have to practise an art founded upon it than by others. For example, in England there can be little doubt that it is to our pre-eminence over all countries in the mechanical arts that our exceptionally greater men in the physical sciences on which those arts are built, is due. The reason why the same sort of beneficial reaction of art upon science has not manifested itself in our own sphere is that the connection between the two, *i.e.*, between physiology and medicine, is much less substantial. We physiologists are not yet in a position to advise the doctors, and they, resting on the more reliable teaching of experience, are quite willing to do without us.

If I am right in supposing that the pursuit of physiological research will always be closely connected with medical study, it becomes a matter of interest to us to know in how far the existing institutions for teaching are fitted for the training of scientific men.

We, who are personally concerned in the teaching of medicine, must, I think, admit that, as regards English schools, an ordinary medical course is not a very good preparation for scientific work. The reason of this is that the "medical sciences" as they are called—chemistry, anatomy, and physiology—have developed far too fast for the resources of our schools. Physiology, which twenty years ago might (without very flagrant absurdity) have been called the handmaid of medicine, has become a great science quite independent of the art which brought her into existence. No longer learning from medicine as she used to do, but based entirely on experiment, she claims much closer relationship with the other experimental sciences, and particularly, of course, with physics and chemistry, than with her parent art.

Let us suppose ourselves carried back, say twenty years. Twenty years ago a lecture-room, with a gallery for showing preparations under the microscope, was all that was thought necessary for teaching physiology, even in the best appointed schools; but then how different was that time from the present as regards the position of the science. I can only refer to one or two of the directions in which progress has been made. All that we knew twenty years ago about the gases of the blood was founded on the imperfect methods and erroneous results of Magnus. Take, for example, the exchange of gases in respiration. In 1852 all that we knew on this subject was founded on the imperfect methods and analyses of the physicist Magnus. Now Ludwig and his pupils have put us in possession of a knowledge which for exactitude may be compared with that of the fundamental facts of physics, with methods directly applicable to a number of most important questions. The same physiologist, Ludwig, had lately written his earliest papers on arterial pressure, and has thus by the introduction of new methods inaugurated a new era in the physiology of two mechanical functions. Du Bois Reymond has scarcely begun that series of researches by which he, like Ludwig, rather founded a new science than extended the limits of an old one. In France Brown-Séquard had discovered the functions of vasomotor nerves, and Bernard the glycogenic function of the liver.

Great as was the intrinsic value of all these investigations, it was surpassed by that of the influence which they exercised on the future progress of science. How rapid that progress has been may be readily judged of by any one who chooses to read any of the text-books of twenty years ago in the light of recent researches. With the exception of the somewhat obscure region of what is called animal chemistry, every chapter has been re-written on the sure basis of direct observation and experiment, the mechanics of the circulation, the chemical changes in the blood and tissues in respiration, the relation between muscular movements and the central organs of the nervous system which preside over them, the electrical changes which go on in nerves and muscles when in and out of action, and in physiological histology, the mode of central and peripheral termination of nerve fibres, and the anatomy of the lymphatic glands and the mode of origin of the absorbent system in the tissues.

In this great progress one would rather not have to admit that Germany has done so large a proportion of the work; for France, notwithstanding her great leaders in science and her great scientific institutions, has accomplished much less than she ought to have done. In taking her part England has been represented by us, the teachers in her medical schools; but we, possessing neither space nor appliances for the prosecution of experimental inquiries, have contented ourselves only too readily to reap the fruits of other men's labours.

It would not be pleasant to make this admission, were it not possible to look forward with considerable confidence to something better. In the great medical schools of London, in the old Universities, and in one or two, at least, of the provincial schools, great efforts are now being made to provide adequate buildings and competent persons for the experimental teaching and study of physiology. It is, I think, a most encouraging sign of the times that the initiative in this movement has been taken by Trinity College, Cambridge. That wealthy corporation, whose very name recalls to our recollection the intellectual glories of our country, has condescended to provide a place for physiologists to study and labour in, from which (short though the time is for which it has existed) one or two valuable researches have already sprung. To what the University of London has done during the last twelve months in establishing a laboratory for inquiries into that most important though comparatively new branch of physiology which relates to the origin and nature of diseases, it is scarcely possible for me to refer, excepting in so far as to express my hope that its influence will eventually be felt in strengthening the hold of physiology on practical medicine.

Notwithstanding these efforts, it will take years to regain the position which we in England once had, and ought never to have lost. The appliances and places for work are now forthcoming, and can be extended as they are required. This is a great step forwards, but we still want the pecuniary resources requisite for carrying out systematic and continuous researches, and above all, we have still to educate workers.

Of the two wants I have mentioned, the want of money and the want of workers, the second is the most important. The difficulties which lie in our way in this respect are very great indeed. The obvious difficulty—the objection, I mean—which is always adduced by young men as a sufficient reason for not giving up their time to scientific research, is that it does not pay; but it need scarcely be said that the real difficulty is a more general one. It lies in that practical tendency of the national mind which leads us Englishmen to underrate or depreciate any kind of knowledge which does not minister directly to personal comfort or advantage, a tendency which was embodied in the philosophy of Bacon, and has been thought by some to constitute its great weakness. I have no doubt there are as many in England as in Germany who would not be deterred by the prospect of comparative poverty, which in every country must be the part of those who devote themselves to abstract science, but very few who have the courage and resolution to follow this course in spite of a public opinion which estimates science on utilitarian principles.

This leads me naturally to my second position, which is that the most efficient means we can take to improve the position of our science in England are those which have for their object the enlightenment of public opinion, and that this is to be effected partly by diffusing this knowledge of our labours among the public, and so inducing them to take an interest in them, partly by introducing training in physical science into our schools.

In the art of exposition, *i.e.*, of making difficult subjects plain, we have one among us who is a master—whose powers in this respect have been acknowledged, not only in England, but in France, and still more emphatically in Germany. His work on elementary physiology has been presented to the German public by one of the leading German physiologists (who is himself a model of clearness of style), who tells his countrymen in his preface that no German writer could expound the experimental facts which are the basis of physiological knowledge as Huxley can.

In the existence of such a man as Huxley I find a great source of encouragement for the future of English physiology, not only on account of his own work, large though that has been (for no one builder can lay many bricks in an edifice where every brick requires such careful laying), but also for his influence on national life.

At one time I confess that I was disposed to underrate the value of popularising science;—now I see the power of exposition to be a great power for good. We have an example of the good that it effects in the history of this Association. We have another in that of the Royal Institution, which has lately been made familiar to us by the accounts which have been given of that great and good man who for so many years was its life. Faraday, the greatest physicist of his time, was equally master of the art of exposition. Of the influence which his mind thereby exercised on the minds of men, women, and children, there can be no doubt. Nor do I think that he lost by it himself, for although we cannot suppose that he taught without some exhaustion of his energies, I cannot believe that the effort was a useless one even to himself.

One would not venture to say of such a man that, in explaining to children the fundamental conceptions which in his mind were already so clear, these became still clearer; but I think it may be so.

I pass at once to the second part of my position, that which relates to the teaching of science, and particularly physiology, in schools. This I may deal with very shortly.

The teaching must necessarily be elementary. If it is thorough and genuine, it is good.

To wedge a little bit of Bowdlerised physiology, something about the structure and functions of the human body, into the ordinary course of a school education, may be an ornamental addition to it, but can scarcely be really useful. Our reform, if it is to be attempted at all, must be much more complete and radical. It must consist, not in adding natural science to the system of instruction in which we ourselves and our predecessors were brought up, but in substituting for some of the old drudgeries something better and more substantial.

As regards that higher education which may be defined as introductory to the studies of the University, most people are now disposed to recognise that there exists at the present day a tendency to increase its extent at the expense of its thoroughness. On the one hand, a powerful effort is made by the *laudatores temporis acti* to maintain the old discipline; while on the other a general though somewhat vague notion prevails that no system of education can be regarded as complete from which science is excluded. To reconcile these antagonistic tendencies, the only method which has been found is that of addition and accumulation. Instead of displacing some of the old requirements, an additional load of new subjects has been imposed on the unfortunate examinee in the form of chemistry, physics, animal physiology, &c. No wonder that to the victim who has just passed through one of our modern ordeals the very names of these sciences are sickening; for in addition to the disagreeable task of getting them up from text-books (text-books, however excellent, are at best but very poor reading), the competitor, whether successful or not, has the consoling reflection that he has been doing treadmill work after all—learning a number of facts and laws of great value to the man who is able to possess himself of them, but to him rendered absolutely useless from the mode of study to which the present system of examinations has compelled him.

The way to obviate this I have already hinted at. Let it be clearly understood that if natural science is to be made a part of our educational system, it cannot be introduced as an ornamental addition or accomplishment, but as part of the groundwork. To serve as a groundwork, we must admit that physiology and anatomy are not adapted.

The corner-stone must, of course, be mathematics. Side by side with mathematics the subjects which ought to claim preference are physics and chemistry. The latter, when taught and studied experimentally, is specially fitted to cultivate that certainty, that convincedness of mind, that clear realisation of facts seen not by the bodily but by the intellectual eye, which constitute the scientific spirit. A boy who has learnt to feel the certainty of the laws of chemical combination, of the relations between density and combining weight, and between both and specific heat, can never, so long as he retains his mental soundness, relapse into that state of vague indifference about facts which characterises many uneducated persons, or lose the habit of exactitude of conception and statement to which he is compelled by practice in chemical reasoning.

It is clear that anatomy and physiology cannot be recommended on the same ground, yet I believe that it may be wisely included in ordinary education, not as a discipline, and not as a subject of examination, but on the ground that it is so usefully applicable to the common affairs of life. It is undoubtedly useful that every one should know something of the structure and functions of his own body, and this for several reasons—first, because he is enabled thereby to take better care of himself, and to understand how to preserve himself by reasonable precautions against some of the well-recognised causes of disease. Another reason is, he would not be so likely as he would otherwise be to become the dupe of the many quackeries which are afloat—more ready to take the advice of the doctor as regards the regulation of his mode of life, less credulous about the efficacy of drugs.

Let us now, in conclusion, say one word as to the influences which the general adoption of a system based upon scientific training would exercise on scientific progress, and particularly on the progress of the science in which we are interested.

I can illustrate this best by taking the medical student as an example. We teachers of physiology to medical students know that when we begin first to talk to them about the principles of the subject, *e.g.*, about chemical change as the essential condition of all vital phenomena, about the relation between the production of heat and external motion, about the exchange of gases in respiration, and many other fundamental subjects, the great difficulty is that our auditors are utterly at fault for want of those conceptions about matter and its powers, which are expressed by the words we are constantly using, such as solid, liquid, gas, vapour, weight, density, volume, &c., all of which to the average-finished schoolboy are perfectly meaningless. The result is that these fundamental conceptions, not having been mastered at first, are not mastered at all, and the student begins to build the superstructure without having had any opportunity of laying the foundation. If the *Vorbildung* were different, if students were to come to their work with the scientific habit of mind already formed, it would not only make them better students, but would retain its influence on him through life. The details might fade from the memory, but the spirit would remain.

I trust that it will not appear to the members of the Section that I have, in any of the observations I have made, forgotten that the object for which we are assembled here is the promotion of the science of anatomy and physiology. Although I cannot claim for our science a more direct interest in scientific training than for others, there are reasons (as I have endeavoured to show) why it suffers more from the want of it than others. The chief one being that, as compared with what we feel and know to be its real importance to the future welfare of humanity, the practical benefits which immediately arise from it are not so obvious.

I have said very little indeed of another pressing difficulty which we have now, and, I believe, will have for many years, to contend with—the want of pecuniary resources—because I know that in this country if educated public opinion can be interested on behalf of any scientific object, and particularly if the intelligent classes of the community can be shown on good ground, that the furtherance of abstract science is a matter of vital importance to our national existence, no one believes that now the really trifling public expenditure which would be required to enable us to compete at least on equal terms with Germany, Austria, Bavaria, and Russia, would at once be forthcoming.

In the meantime it is the function and duty of all who have the means and are interested in scientific progress, and especially of us, the members of this section of the British Association, to afford such aid as we can to those who, supported by their own enthusiasm rather than by the prospect of honour or emolument, are willing to devote their lives to physiological and anatomical researches.

### SUB-SECTION ANTHROPOLOGY

### Opening Address by the President, Colonel A. Lane Fox

(*Concluded from page* 324)

Amongst the earliest improvements upon the primitive arts of man would be the substitution of the throwing-stick by the bow as a means of accelerating the flight and force of the javelin. So decided an advance in the employment of missile force would lead to the discontinuance of the throwing-stick for ordinary purposes wherever the bow was introduced. The throwing-stick is now found only in distant and unconnected regions, viz., in Australia, and amongst the Esquimaux, and the Furus Furus Indians of South America; and it has been assumed, on account of the isolated positions in which it is found, that it must be indigenous. On the other hand, the use of the bow is almost universal; and it has equally been assumed, on account of its world-wide distribution, that it must be indigenous in different localities, and not derived from a common centre. Geographical distribution, however, although affording the best evidence obtainable, cannot be relied upon with certainty in the case of so early an invention as the bow appears to have been. I cannot concur in thinking that we have any sure evidence that the bow originated in different places; on the contrary, what evidence we have appears to me to be of a contrary tendency.

As by degrees the use of the bow spread over the world, that of the throwing-stick would tend to disappear. We have some grounds for supposing that the latter instrument was formerly in use in the Pelew Islands, and Mr. Franks has found it amongst some Mexican relics probably preserved in a tomb. May it not also have existed formerly in other localities where it has not been preserved in tombs, and where no trace of it now exists? If this were the case, where should we now expect to find it retained? In such localities as the Arctic Seas, where lack of suitable materials still renders the construction of the bow a work of great difficulty, as is shown by the manner in which several pieces of hard bone are sometimes fastened together to form one, or in Australia, where the knowledge of the use of the bow has never penetrated.

Closely connected with the bow, the harpoon may be instanced as an example of early origin and wide distribution. The harpoon is found in some of the French caves, amongst the earliest bone relics of human workmanship that have been brought to light. Its present distribution is almost universal, being found in Australia, North and South Africa, North and South America, and in all regions where its use has not been superseded by more suitable contrivances.

In proportion as our investigations are carried into the higher phases of civilisation, we find our areas of distribution more limited, and of more and more value to us in tracing the continuity of culture; and when we come to the distribution of the metallurgic arts we find them defined by marked geographical boundaries which are not the boundaries of the great primæval races of mankind.

If we draw a line across the globe from Behring Strait in a south-westerly direction through Wallace's line, leaving Australia on the east, and take for our period the date of the first discovery of America, we shall find that—putting aside the metallurgic culture of Mexico and Peru, which, it may be observed, is grouped round a single centre—this line separates the area of stone culture on the east from the area of metallurgic culture on the west, but it passes straight through the primæval racial boundaries.

If we take what we may call the metallurgic area more in detail, and endeavour to trace the distribution of the implements of the bronze period, we find that the same class of weapons and tools extends over a continuous area, including the whole of the northern, western, and central parts of Europe, as far as Siberia on the east; these implements, including palstaves, leaf-shaped swords, and socket celts, with the moulds for casting them, are of a character to prove that the diffusion of the bronze culture throughout this area must have been connected and continuous. In Egypt, Assyria, India, and China, we have also bronze, but the forms of the implements do not, as a rule, correspond to those of the area above mentioned; our knowledge of the bronze weapons of India and China is, however, extremely limited as yet. I have elsewhere given my reasons for believing that the knowledge of the use of iron in Africa must have been derived from a common centre; not only is the mode of working it the same throughout that continent and in India, but the forms of the weapons fabricated in this metal, and especially the corrugated blades, are the same in every part, and appear to have been copied and retained through habit, wherever the use of iron has penetrated. I have lately traced this peculiar form of blade in several parts of the Indian Peninsula and Burmah, and I have no doubt it will eventually be found further to the north, so as to connect the area of its distribution continuously with those of the same identical construction that are found in the Saxon and Frankish graves.

I have thus briefly alluded to the distribution of some of the arts associated with early culture, with the view of showing that as our knowledge increases we may expect to be able to trace many connections of which we are now ignorant, and that we should be careful how we too readily assume, in accordance with the theory which appears popular among anthropologists at the present time, that coincidences in the culture of people in distant regions must invariably have originated independently, because no evidence of communication is observable at the present time. Owing, perhaps, to a praiseworthy desire to refute the arguments of Archbishop Whately, and others who have erroneously, as I think, assumed that because no race of existing savages has been known to elevate itself in the scale of civilisation, therefore, the first steps in culture must have resulted from supernatural revelation, we have now had a run upon the theory of what may be called the spontaneous generation of culture, and the pages of travel have been ransacked to find examples of independent origin and progress in the arts and customs of savage tribes.

Owing to this cause we have, I think, lost sight in a great measure of the important fact which history reveals to us, that, account for it as we may—and it is one of the great problems of Anthropology to account for it if we can—the civilisation of the world has always advanced by means of a leading shoot, and though constantly shifting its area, it has within historic times invariably grouped itself round a single centre, from which the arts have been disseminated into distant lands, or handed down to posterity. In all cases a continuous development must be traced before the problem of origin can be considered solved; the development may have been slow, or it may have been rapid, but the sequence of ideas must have been continuous, and until that sequence is established our knowledge is at fault. As with the distribution of plants, certain soils are favourable to the growth of certain plants, but we do not on that account assume them to be spontaneous offspring of the soil; so certain arts and phases of culture may flourish amongst certain races, or under certain conditions of life. But it is as certain that each art, custom, and institution had its history of natural growth, as it is that each seed which sprouts in the soil once fell from a parent stem. The human intellect is the soil in which the arts and sciences may be said to grow; and this is the only condition of things compatible with the existence of minds capable of adapting external nature, but possessing no power of originality.

If I am right in supposing that it is one of the primary objects of Anthropological Science to trace out the history and sources of human culture, a consideration of the relative value of the various classes of evidence on which we rely for this purpose, will be admitted to be a question of no slight importance in connection with our subject. We must distinguish between those branches of study which we are apt to look upon as intrinsically the highest, and on that account the most attractive, and those which are of most value as evidence of man in a low condition of culture. To the religions, myths, institutions, and language of a people we are naturally drawn, as affording the best indications of their mental endowments; but it is evident that these carry us no farther back in time than the historic period, and however necessary to be studied as branches of our science, they fail to afford us any direct evidence of those vast ages during which our species appears to have gradually taken upon itself the characteristics of humanity. Every age has, however, left us the relics of its material arts, which, when studied comprehensively in connection with the geological record, may be taken as evidence of mental development from the earliest period of time. Nor is it in point of time alone, but also by reason of their stability, that the material arts afford us the surest evidence on which to reconstruct our social edifice. The tendency to constant variation within narrow limits is a psychical characteristic of the uncultivated man; but the material arts are not subject to those comparatively abrupt changes to which, prior to the introduction of writing, all branches of culture are liable which are dependent for their transmission on the memory, and are communicated by word of mouth.

Few who have read the works of Prof. Max Müller or Mr. Farrar can fail to have been struck with the value of the evidence, as far as it goes; but, on the other hand, the very short distance to which it carries us back in investigating the origin of speech; nor is this surprising when it is considered how constant must have been the changes to which language was subject in prehistoric times. Amongst the one hundred islands occupied by the Melanesian race, the Bishop of Wellington informs us there are no less than two hundred languages differing from each other as much as Dutch and German, and this diversity of languages and dialects is confirmed by Mr. Turner in his account of his nineteen years' residence in Polynesia. Amongst the Penons, or savage tribes of Cambodia, Mr. Muhot speaks of the great number of dialects spoken by tribes whose manners and customs are the same. Amongst the Musgu of Central Africa, Barth tells us that, owing to the absence of friendly intercourse between the several tribes and families, such a number of dialects had sprung up as to render communication between them difficult. Upon the River Amazon Mr. Bates mentions that in a single canoe he found several individuals speaking languages so different as to be unintelligible to the others. In a state of culture in which such diversity of tongues existed, what could have been the chance of preserving unchanged the myths, religions, and all those manifestations of intellect which are dependent on tradition?

The greater stability of the material arts as compared with the fluctuations in the language of a people in a state of primæval savagery, is well shown by a consideration of the weapons of the Australians and the names by which they are known in the several parts of that continent. As I have already mentioned, these people, from the simplicity of their arts, afford us the only living examples of what we may presume to have been the characteristics of a primitive people. Their weapons, respecting the distribution of which we have more accurate information than we have of their vocabularies, are the same throughout the continent; the shield, the throwing-stick, the spear, the boomerang, and their other weapons, differ only in being thicker, broader, flatter, or longer in different localities, but whether seen on the east or the west coast, each of these classes of weapons is easily recognised by its form and uses. On the other hand, amongst the innumerable languages and dialects spoken by these people, it would appear that almost every tribe has a different name for the same weapon.

If, then, it is evident that much of the history of our prehistoric ancestors has been for ever lost to us, we may console ourselves with the reflection that in their tools and weapons and other relics of their material arts, the most reliable source of evidence as to their intellectual condition has continued to our time. As to the myths, religions, superstitions, and languages, with which they were associated, we may content ourselves by devoutly thanking Providence that they have not been preserved. As it is, anthropological studies are said to have their fair share in the creation of lunatics, and we can easily believe that no sane intellect would have survived the attempt to unravel such a complex and tangled web of difficulty as the study of these subjects would have presented to our minds.

The consideration of the value of evidence naturally leads us to the third part of my subject, namely, the mode of collecting it and of digesting it after it has been brought together; and as this is, I believe, the most defective part of our organisation—or to speak more properly, the part of our existing institutions in which our want of organisation is most conspicuous—I had intended to have spoken at greater length on this subject; but as I have already trespassed upon your time so long, I am under the necessity of curtailing what I had proposed to say on the subject of organisation. If I am wrong, as I have heard it suggested by some anthropologists, in supposing that the greatest difficulties under which we labour are attributable to the absence of reliable evidence, and if we already possess as much information about savages and about prehistoric men as we require—and we have nothing to do but to read the books in our libraries, and write papers calculated to promote discussions, and fill journals with interesting controversies and speculations—if, as I gravely heard it asserted not long ago at a public meeting, it would be a pity to explore Stonehenge for fear so remarkable a monument should be divested of that mystery which has always attached to it, owing to our entire ignorance as to its origin and uses, then to those who entertain such views the few remarks I shall venture to offer on this subject must appear not only superfluous but mischievous. But if, on the other hand, I am right in supposing that our existing evidence is lamentably deficient, and in many cases false; that it has been collected by travellers, many of whom have had but little knowledge what to look for and observe; and if, this being the state of our knowledge, the evidence which we desire to obtain is now rapidly disappearing from off the face of the earth. The Tasmanians have been swept away before we know anything about them; the New Zealanders and all the Polynesian Islanders are fast changing their habits; and it is now difficult to find a North American Indian in a state of unadulterated savagery; whilst at home our prehistoric monuments are broken up and ploughed down day by day in the construction of buildings and railroads; it is evident that a set of societies which provide no organisation whatever for promoting exploration at home or abroad can only be regarded as fulfilling very imperfectly the functions which institutions established for the purpose of anthropological investigation might reasonably be expected to serve. Beyond the limits of this Association, there is but one society in this country which has the funds necessary for promoting explorations, and that is the Geographical Society. Every expedition which goes out under the auspices of that Society is necessarily brought in contact with the races inhabiting the districts which are explored; but it can hardly be expected that the Geographical Society should do as much as could be desired in the way of promoting anthropological investigation, as long as Anthropology and Ethnology are excluded from the functions of that Society. A Geographical Society should be regarded as the eyes and ears of an Anthropological Society abroad, in the same way that the archæological societies should fulfil the functions of eyes and ears directed to the past history of man, and the most intimate alliance ought to exist between them. A step in the right direction has lately been taken, at the suggestion of Mr. Clements Markham, by the establishment of a joint committee of the Geographical Society and Anthropological Institute, to draw up questions for travellers whom it is proposed to send to the Arctic Seas; and this, it is to be hoped, will be the first step towards a more intimate alliance in the future. As to the Archæological Societies, whose name is legion, and the functions of which are necessarily anthropological in a great degree, they are, as a rule, the most impotent and unprogressive bodies; living from hand to mouth, with funds barely sufficient to maintain a secretary, and to produce a small volume of transactions annually; without the means of promoting exploration, they are dependent entirely upon the casual communications of members, the substance of which is sometimes repeated over and over again in the different societies. If we inquire what useful purpose is served by these divisions of the metropolitan societies, we are told that one is a society, another is an association, and a third is an institute; and yet it does not appear that any one of these societies, associations, or institutes, perform any special function which cannot equally well be served by the others. They constitute divisions of persons rather than divisions of subjects; instead of promoting division of labour they serve only to promote repetition of labour; and so ill do any of them answer the expectations of those who devote themselves to the close study of any one branch of archæology or anthropology, that it has lately become necessary to establish an additional metropolitan society for promoting protohistoric archæology under the title of the Society of Biblical Archæology, embracing subjects which fall mainly within in the domain of anthropology. Much as I should feel disposed to condemn the multiplication of societies under existing circumstances, I cannot but think that by promoting the close study of a particular branch, the establishment of this society is a step in the right direction; and I therefore trust that it may be found to flourish at the expense of those which appear to have no special function to perform. As a prehistoric archæologist, I can only add my humble testimony to that of others who think that this branch of anthropology is very unsatisfactorly dealt with by the metropolitan societies in which it is discussed. Quite recently this happy family has been increased by the birth of a fine child under the title of an Historic Society, and I observe that by way of specialising the functions of this society, it commenced life with a paper on Prehistoric Man. But there are no signs of any limitation to this improvident childbearing; it is announced that a Psychological Society is confidently expected. No one would be more disposed than myself to welcome psychology as a special branch of study if this family of gutter children is to go on increasing *ad libitum*; but it will be admitted that a Psychological Society, of all others, is liable to grow up scatterbrained, if completely severed from the influence of its more experienced kinsfolk.

I trust that I have made it apparent that Anthropology in its various branches includes some of the most popular and widely-disseminated scientific interests of the country; that the loss of power is enormous; not only is there no means of organised exploration, but the information which is published is either repeated over and over again in the different societies, or it is so scattered as to be beyond the reach of the majority of the students. They labour also under the disadvantage of being supported chiefly by men of small means, for the well-to-do classes in this country do not, as a rule, take any interest in either scientific or anthropological investigations. During the past year, a single American has done more in the way of anthropological exploration than the whole of the English societies, institutes, and associations together.

I will now briefly state my views as to the remedies for the evils of which I have spoken. I am averse to the principle of amalgamation; narrow views are often the most pronounced, and if they become dominant are liable to bring down the standard of an amalgamated society instead of enlarging its sphere of usefulness; besides, this amalgamation necessarily entails a certain loss of income by the loss of double subscriptions.

If my experience as a member of the council of most of the societies of which I speak does not deceive me, it should be the object of those who have the progress of anthropological studies at heart to induce the metropolitan societies to specialise their functions. The following might then become the titles of the various societies included under the term Anthropology, and they would represent not only the natural divisions of the science, but practically the divisions which are most consonant with the organisation of the existing societies. Setting history and historic archæology aside as beyond our province, we should have (1) Protohistoric Archæology; I adopt the term proposed by Mr. Hyde Clarke for this branch, which practically includes all that comes under the head of Biblical Archæology at present; (2) Prehistoric Archæology; (3) Philology; (4) Biology, including Psychology and Comparative Anatomy, in so far as it relates to man; (5) Descriptive Ethnology, viz., original reports of travellers on the races of man, conducted in association with geographical exploration. Under these heads we should, I believe, include all the various classes of special workers. These should constitute independent but associated societies; that is to say, the members of one should be privileged to attend the meetings and take part in the discussions of the others, but not to receive the publications of any but their own society. By this means each would profit by the experience of the other societies, but the funds necessary for the maintenance of each would be secured. As branch sections of anthropology they would be under the control of a general elected council, only in so far as would be necessary to prevent their clashing with each other, and for the control of any measures which it might be necessary for the several sections to undertake in concert; under the auspices of the general council might also be held the anthropological meetings devoted to such general subjects as either embraced the whole, or were not included in the sections. By these means the standard of anthropological science as a comprehensive study of the science of man in all its branches would be secured, and the possibility of its becoming narrowed under the influence of any dominant party would be obviated. It is hardly necessary to say that the chief advantage of such an arrangement as I suggest would consist in the employment of a single theatre and library for these cognate societies; they would employ a single printer, and the arrangements might include one or more artists, lithographers, and map-drawers, by which a great increase, and at the same time economy, would be effected in the illustrations. The saving effected by the union of these societies in a single establishment might be applied to conducting explorations either at home or abroad in connection with the Geographical Society. The question of the utilisation of apartments is one which commends itself especially to the notice of Government in regard to those societies, for which apartments are provided at the public cost. It should be made a *sine quâ non* that the societies so favoured should fairly represent all the branches of their subject.

As regards the local societies, it has been proposed to republish a selection of their papers under the auspices of this association. It is to be hoped that some arrangement, such as that proposed by the committee of which Sir Waller Elliot is secretary, may be carried out. I have only one suggestion to make on this point: re-publication is simply a repetition of cost and labour, if the desired object of bringing the papers together can be accomplished by other means. As to selection, I have no faith in it. If local and metropolitan societies could be induced to adopt a uniform size for their publications—not necessarily a uniform type—the papers relating to the same subjects might be brought together without the cost of reprinting. It would only be necessary to establish a classification of papers under various headings, such as, for example, those which constitute the sections of this Association. The societies might then print additional copies of their papers under each heading, in the same manner that additional copies are now struck off for the use of authors. A single metropolitan society might be recognised as the representative of each branch, and under its auspices the whole of the papers of the local and metropolitan societies relating to its branch might be brought together and printed in a single volume. Time does not allow me to enter into the details of the arrangements which would be necessary to carry out such a measure. I believe the difficulties would not be so great as might at first sight appear, especially as the evils of the existing arrangements are much complained of; but it should be a primary object of any arrangement that may hereafter be made that the independence of the several branches should not be sacrificed unnecessarily; it should be endeavoured to stimulate them and train them into useful channels, rather than to bring them too much under central control.

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## SECTION E

### GEOGRAPHY

### Opening Address by the President, Francis Galton, F.R.S.

The functions of the several Sections of the British Association differ from those of other Institutions which pursue corresponding branches of science. We, who compose this Section, are not simply a Geographical Society, meeting in a hospitable and important provincial town, but we have a distinct individuality of our own. We have purposes to fulfil which are not easily to be fulfilled elsewhere; and, on the other hand, there are many functions performed by Geographical Societies which we could not attempt without certain failure. Our peculiarities lie in the brief duration of our existence, combined with extraordinary opportunities for ventilating new ideas and plans, and of promoting the success of those that deserve to succeed. We are constituents of a great scientific organisation, which enables us to secure the attention of representatives of all branches of science to any projects in which we are engaged; and if those projects have enough merit to earn their deliberate approval, they are sure of the hearty and powerful support of the whole British Association.

These considerations indicate the class of subjects to which our brief existence may be devoted with most profit. They are such as may lead to a definite proposal being made by the Committee of our Section for the aid of the Association generally; and there are others, of high popular interest, which cannot be thoroughly discussed except by a mixed assemblage, which includes persons who are keen critics, though not pure geographers, and who have some wholesome irreverence to what Lord Bacon would have called "the idols of the Geographical den."

We may congratulate ourselves that many excellent memoirs will be submitted to us, which fulfil one or other of these conditions. They will come before us in due order, and it is needless that I should occupy your attention by imperfect anticipations of them. But I must say that their variety testifies to the abundance of the objects of geographical pursuit, other than exploration. There is no reason to fear that the most interesting occupation of geographers will be gone, when the main features of all the world are known. On the contrary, it is to be desired, in the interests of the living pursuit of our science, that the primary facts should be well ascertained, in order that geographers may have adequate materials, and more leisure to devote themselves to principles and relations. I look forward with eagerness to the growth of Geography as a science, in the usually accepted sense of that word; for its problems are as numerous, as interesting, and as intricate as those of any other. The configuration of every land, its soil, its vegetable covering, its rivers, its climate, its animal and human inhabitants, act and re-act upon one another. It is the highest problem of Geography to analyse their correlations, and to sift the casual from the essential The more accurately the crude facts are known, the more surely will induction proceed, the further will it go, and, as the analogy of other sciences assures us, the interest of its results will in no way diminish.

As a comparatively simple instance of this, I would mention the mutual effects of climate and vegetation, on which we are at present very imperfectly informed, though I hope we shall learn much that is new and valuable during this meeting. Certain general facts are familiar to us: namely, that rain falling upon a barren country drains away immediately. It ravages the hill-slopes, rushes in torrents over the plains, and rapidly finds its way to the sea, either by rivers or by subterranean water courses, leaving the land unrefreshed and unproductive. On the other hand, if a mantle of forest be nursed into existence, the effects of each rainfall are far less sudden and transient. The water has to soak through much vegetation and humus before it is free to run over the surface; and, when it does so, the rapidity of its course is checked by the stems of the vegetation. Consequently, the rain supplies are held back and stored by the action of the forest, and the climate among the trees becomes more equable and humid. We also are familiar with the large differences between the heat-radiating power of the forest and of the desert, also between the amount of their evaporation; but we have no accurate knowledge of any of these data. Still less do we know about the influences of forest and desert on the rate of passage, or upon the horizontality, of the water-laden winds from the sea over the surface of the land: indeed, I am not aware that this subject has ever been considered, although it is an essential element in our problem. If we were thoroughly well informed on the matters about which I have been speaking, we might attempt to calculate the precise difference of climate under such and such conditions of desert and of forest, and the class of experiences whence our data were derived would themselves furnish tests of the correctness of our computations. This will serve as an example of what I consider to be the geographical problems of the future; it is also an instance of the power of man over the phenomena of nature. He is not always a mere looker-on, and a passive recipient of her favours and slights; but he has power, in some degree, to control her processes, even when they are working on the largest scale. The effects of human agency on the aspect of the earth would be noticeable to an observer far removed from it. Even were he as distant as the moon is, he could see them; for the colour of the surface of the land would have greatly varied during historic times, and in some places the quantity and the drift of cloud would have perceptibly changed. It is no trifling fact in the physical geography of the globe, that vast regions to the east of the Mediterranean, and broad tracts to the south of it, should have been changed from a state of verdure to one of aridity, and that immense European forests should have been felled.

We are beginning to look on our heritage of the earth much as a youth might look upon a large ancestral possession, long allowed to run waste, visited recently by him for the first time, whose boundaries he was learning, and whose capabilities he was beginning to appreciate. There are tracts in Africa, Australia, and at the Poles, not yet accessible to geographers, and wonders may be contained in them; but the region of the absolutely unknown is narrowing, and the career of the explorer, though still brilliant, is inevitably coming to an end. The geographical work of the future is to obtain a truer knowledge of the world. I do not mean by accumulating masses of petty details, which subserve no common end, but by just and clear generalisations. We want to know all that constitutes the individuality, so to speak, of every geographical district, and to define and illustrate it in a way easily to be understood; and we have to use that knowledge to show how the efforts of our human race may best conform to the geographical conditions of the stage on which we live and labour.

I trust it will not be thought unprofitable, on an occasion like this, to have paused for a while, looking earnestly towards the future of our science, in order to refresh our eyes with a sight of the distant land to which we are bound, and to satisfy ourselves that our present efforts lead in a right direction.

The work immediately before us is full of details, and now claims your attention. There is much to be done and discussed in this room, and I am chary of wasting time by an address on general topics. It will be more profitable that I should lay before you two projects of my own about certain maps, which it is desirable that others than pure geographers should consider, and on which I shall hope to hear the opinions of my colleagues in the Committee-room of this Section.

They both refer to the Ordnance Maps of this country, and the first of them to the complete series well known to geographers, that are published on the scale of one inch to a mile. It is on these alone that I am about to speak; for, though many of my remarks will be applicable more or less to the other Government map publications, I think it better not to allude to them in direct terms, to avoid distracting attention by qualifications and exceptions.

English geographers are justly proud of these Ordnance Maps of their country, whose accuracy and hill-shading are unsurpassed elsewhere, though the maps do not fulfil, in all particulars, our legitimate desires. I shall not speak here of the absence from the coast-maps of the sea *data*, such as the depth and character of the bed of the sea, its currents and its tides (although these are determined and published by another Department of the Government—namely, the Admiralty), neither shall I speak of the want of a more frequent revision of the sheets, but shall confine myself to what appear to be serious, though easily remediable, defects in the form and manner of their publication. It is much to be regretted that these beautiful and cheap maps are not more accessible. They are rarely to be found even in the principal bookseller's shops of important country towns, and I have never observed one on the bookstall of a railway station. Many educated persons seldom, if ever, see them; they are almost unknown to the middle and lower classes; and thus an important work, made at the expense of the public, is practically unavailable to a large majority of those interested in it, who, when they want a local map, are driven to use a common and inferior one out of those which have the command of the market. I am bound to add that this evil is not peculiar to our country, but is felt almost as strongly abroad, especially in respect to the Government of France. I account for it by two principal reasons. The first is, that the maps are always printed on stiff paper, which makes them cumbrous and unfit for immediate use; it requires large portfolios or drawers to keep them smooth, clean, and in separate sets, and an unusually large table to lay them out side by side, to examine them comfortably, and to select what is wanted. These conditions do not exist on the crowded counter of an ordinary bookseller's shop, where it is impossible to handle them without risk of injury, and without the certainty of incommoding other customers. Moreover, their stiffness and size, even when published in quarter-sheets, make them most inconvenient to the purchaser. Either he has to send them to be mounted in a substantial and therefore costly manner, or he must carry a roll home with him, and cut off the broad ornamental borders, and divide the sheet into compartments suitable for the pocket, which, to say the least, is a troublesome operation to perform with neatness. The other of the two reasons why the maps are rarely offered for sale, is that the agents for their publication are themselves map-makers, and therefore competitors, and it is not to be expected of human nature that they should push the sale of maps adversely, in however small a degree, to their own interests.

The remedy I shall propose for the consideration of the Committee of this Section is, to memorialise Government to cause an issue of the maps to be made in quarter-sheets on thin paper, and to be sold, folded in a pocket-size, like the county maps seen at every railway station, each having a portion of an index map impressed on its outside, to show its contents and those of the neighbouring sheets, as well as their distinguishing numbers. Also, I would ask that they should be sold at every "Head Post-office" in the United Kingdom. There are about seven hundred of these offices, and each might keep nine adjacent quarter-sheets in stock, the one in which it was situated being the centre of the nine. An index-map of the whole survey might be procurable at each of these post-offices, and, by prepayment, any map not kept in stock might be ordered at any one of them, and received in the ordinary course of the post. This is no large undertaking that I have proposed. The price of a quarter-sheet in its present form, which is more costly than what I ask for, is only sixpence; therefore the single complete set of nine sheets for each office has a value of not more than four shillings and sixpence, and for all the seven hundred Head Post-offices, of not more than 160*l*.

I believe that these simple reforms would be an immense public boon, by enabling any one to buy a beautiful and accurate pocket-map of the district in which he resides, for only sixpence, and without any trouble. They would certainly increase the sale of Government maps to a great extent, and they would cause the sympathies of the people and of their representatives in Parliament to be enlisted on the side of the Survey, and they probably be imitated by Continental nations.

It has often been objected to any attempt to increase the sale of Government maps, that the State ought not to interfere with private enterprise. I confess myself unable to see the applicability of that saying. It would be an argument against making Ordnance maps at all; but the nation has deliberately chosen to undertake that work, on the ground that no private enterprise could accomplish it satisfactorily; and, having done so, I cannot understand why it should restrict the sale of its own work in order to give a fictitious protection to certain individuals, against the interests of the public. It seems to me to be a backward step in political economy, and one that has resulted in our getting, not the beautiful maps for which we, as taxpayers, have paid, but copies or reductions of them, not cheaper than the original, and of very inferior workmanship and accuracy.

So much for the first of the two projects which I propose to bring before the consideration of the committee of this section. It is convenient that I should preface my second one with a few remarks on colour-printing, its bearing on the so-called "bird's-eye views," and on its recent application to cartography. Colour-printing is an art which has made great advances in recent years, as may be seen by the specimens struck off in the presence of visitors to the present International Exhibition. One of these receives no less than twenty-four consecutive impressions, each of a different colour from a different stone. This facility of multiplying coloured drawing will probably lead to a closer union than heretofore between geography and art. There is no reason now why "bird's-eye views" of large tracts of country should not be delicately drawn, accurately coloured, and cheaply produced. We all know what a geographical revelation is contained in a clear view from a mountain top, and we also know that there was an immense demand for the curiously coarse bird's-eye views which were published during recent wars, because even such as they are capable of furnishing a more pictorial idea of the geography of a country than any map. It is therefore to be hoped that the art of designing the so-called "bird's-eye views" may become studied, and that real artists should engage in it. Such views of the environs of London would form very interesting, and it might be, very artistic pictures.

The advance of colour-printing has already influenced cartography in foreign countries, and it is right that it should do so, for a black and white map is but a symbol—it can never be a representation—of the many-coloured aspects of Nature. The Governments of Belgium, Russia, Austria, and many other countries, have already issued coloured maps; but none have made further advance than the Dutch, whose maps of Java are printed with apparently more than ten different colours, and succeed in giving a vivid idea of the state of cultivation in that country.

I now beg to direct your attention to the following point:—It is found that the practice of printing maps in more than one colour has an incidental advantage of a most welcome kind, namely, that it admits of easy revision, even of the most beautifully executed maps, for the following reason. The hill-work in which the delicacy of execution lies, is drawn on a separate plate, having perhaps been photographically reduced; this has never to be touched, because the hills are permanent. But it is in the plate which contains nothing else but the road-work where the corrections have to be made, and that is a very simple matter. I understand that the Ordnance Survey Office has favourably considered the propriety of printing at some future time an edition of the one-inch maps on this principle, and at least in two colours—the one for the hills and the other for the roads.

This being stated, I will now proceed to mention my second proposal

Recollecting what I have urged about the feasibility of largely increasing the accessibility and the sale of Government maps, by publishing them in a pocket form and selling them at the Head Post-offices, it seems to me a reasonable question for the committee of this section to consider whether Government might not be memoralised to consider the propriety of undertaking a reduced ordnance map of the country, to serve as an accurate route-map and to fulfil the demand to which the coarse country maps, which are so largely sold, are a sufficient testimony. The scale of the reduced Government map of France corresponds to what I have in view; it is one of five miles to an inch, within a trifle ($\frac{1}{320000}$ of Nature), which is just large enough to show every lane and footpath. Of course it would be a somewhat costly undertaking to make such a map, but much less so than it might, at first sight, appear. Its area would be only twenty-fifth that of the ordinary ordnance map, and the hill-work of the latter might perhaps be photographically reduced and rendered available at once. The desirability of maps such as these, accurately executed and periodically revised, is undoubted, while it seems impossible that private enterprise should supply them except at a prohibitive cost, because private publishers are necessarily saddled with the cost of re-obtaining much of what the Ordnance Survey Office has already in hand for existing purposes. A Government department has unrivalled facilities for obtaining a knowledge of every alteration in roads, paths, and boundaries of commons, and Government also possesses an organised system in the post-offices, fitted to undertake their sale. The production of an accurate route-map seems a natural corollary to that of the larger Ordnance maps, and has been considered so by many Continental Governments.

I therefore intend to propose to the committee of this section to consider the propriety of memorialising Government to cause inquiries to be made as to the cost of construction, and the probability of a remunerative sale, of maps such as those I have described; and, if the results are satisfactory, to undertake the construction of a reduced Ordnance Map, on the same scale as that of France, to be printed in colours, and frequently revised.

These, then, are the two projects to which I alluded—the one to secure the sale of one-inch Ordnance Maps, on paper folded into a pocket form, to be sold at the Head Post-offices of the United Kingdom—700 or thereabouts in number, each office keeping in stock the maps of the district in which it is situated; and the other to obtain a reduced Ordnance Map of the kingdom, on the scale of about five miles to an inch, to fulfil all the purposes of a road map, and to be sold throughout the country at the post-offices, in the way I have just described.

I will now conclude my Address, having sufficiently taxed your patience, and beg you to join with me in welcoming, with your best attention, the eminent geographers whose communications are about to be submitted to your notice.

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SECTIONAL PROCEEDINGS—FRIDAY, AUGUST 16

*Discoveries at the Northern End of Lake Tanganyika*, by H. M. Stanley.

THE President, Mr. Galton, in announcing the programme of proceedings for the day, explained the circumstances connected with Dr. Livingstone's discoveries previous to Mr. Stanley's expedition.

Mr. Stanley then read his paper, in which the following are the most important items, omitting everything of merely personal interest:—

"If you will glance at the south eastern shore of the Tanganyika, you will find it a blank; but I must now be permitted to fill it with rivers and streams and marshes and mountain ranges. I must people it with powerful tribes, the Wafipa, Wakawendi, Wakonongo, and Wanyamwezi, more to the south with ferocious Watula and predatory Waiori, and to the north with Mara, Msengi, Wangondo, and Waluriba. Before coming to the Malagarazi, I had to pass through Southern Wavinza. Crossing that river, and after a day's march north, I entered Ubha, a broad plain country, extending from Uvinza north to Urundi and the lands inhabited by the Northern Watuta. Three long marches through Ubha brought me to the beautiful country of Ukararga, and a steady tramp of twenty miles farther westward brought me to the divisional line between Ukaranga and Ujiji, the Liuche Valley, or Ruche, as Burton has it. Five miles farther westward brought us to the summit of a smooth hilly ridge, and the town of Ujiji embowered in the palms lay at our feet, and beyond was the silver lake, the Tanganyika, and beyond the broad belt of water towered the darkly-purpled mountains of Ugoma and Ukaramba.

"The connection between the Tanganyika and the Albert Nyanza was a subject of interest to all geographers before I went to Central Africa. Livingstone even was almost sure that the Albert Nyanza was no more than a lower Tanganyika, and indeed he had very good reason for believing so. He had perceived a constant flow northward. All the Arabs and natives persisted in declaring that the Rusizi ran out of the Lake Tanganyika. Before I arrived at Ujiji he had never been to the north end of the Tanganyika. As we hugged the coast of Ujiji and Urundi, looking sharply to every little inlet and creek for the outlet that was said to be somewhere in a day's pulling, we would pass by from fifteen to twenty miles of country. It took us ten days' hard pulling to reach the head of the lake, a distance of nearly 100 geographical miles from Ujiji. Two days sufficed for the coast of Ujiji, the remaining eight we were coasting along the bold shores of Urundi, which gradually inclined to the eastward, the western ranges, ever bold and high, looking like a huge blue-black barrier some thirty miles west of us, to all appearances impenetrable and impassable. Only two miles from shore I sounded, and though I let down 620 feet of line I found no bottom. Livingstone sounded when crossing the Tanganyika from the westward, and found no bottom with 1,800 feet of line. The evening before we saw the Rusizi a freedman of Zanzibar was asked which way the river ran—out of the lake or into it? The man swore that he had been on the river but the day before, and that it ran out of the lake. I thought the news too good to be true. I should certainly have preferred that the river ran out of the lake into either the Victoria or the Albert. Livingstone and I resolved, if it flowed into the Victoria Nyanza, to proceed with it to that lake, and then strike south to Unyamyembe, and, if it flowed into the Albert Lake, to proceed into the Albert and cruise all round it, in the hope of meeting Baker. Just after dark we started, and in the morning we arrived at Muighewa, and started for the mouth of the river. In about fifteen minutes we were entering a little bay about a mile wide, and saw before us to the north a dense brake of papyrus and matete cane. Until we were close to this brake we could not detect the slightest opening for a river such as we imagined the Rusizi to be. We followed some canoes which were disappearing mysteriously and suspiciously through some gaps in the dense brake. Pulling boldly up, we found ourselves in what afterwards proved to be the central mouth of the river. All doubt as to what the Rusizi was vanished at once and for ever before that strong brown flood, which tasked our exertions to the utmost as we pulled up. I once doubted, as I seized an oar, that we should ever be able to ascend; but, after a hard quarter of an hour's pulling, the river broadened, and a little higher up we saw it widen into lagoons on either side. The alluvial plain through which the river makes its exit into the lake is about twelve miles wide, and narrows into a point after a length of fifteen miles, or a narrow valley folded in by the eastern and western ranges, which here meet at a distance of a couple of miles. The western range, which inclines to the eastward, halts abruptly, and a portion of it runs sharply north-westward, while the eastern range inclines westward, and after over-lapping the western range shoots off north-westward, where it is lost amid a perfect jumble of mountains. The chief Rubinga, living at Mugihewa, said that the Rusizi rose from the Lake Kivo, a lake fifteen miles in length and about eight in breadth. Kwansibura was the chief of the district in North-eastern Urundi, which gives its name to the lake. Through a gap in a mountain the river Rusizi escaped out of Lake Kivo. On leaving Kivo Lake it is called Kwangeregere; it then runs through the district of Unyambungu, and becomes known as the Rusizi or Lusizi. A day's march from Mugihewa, or say twenty miles north of the mouth, it is joined by the Luanda, or Ruanda, flowing from a north-westerly direction, from which I gather that the river Luanda is called after the name of the country—Ruanda, said to be famous for its copper mines. Besides the Luanda, there are seventeen other streams which contributed to the Rusizi; these are the Mpanda, Karindwa, Wa Kanigi, Kaginissi, Kaburan, Mohira, Niamagana, Nya Kagunda, Ruviro, Rofuba, Kavimvira, Mujove, Ruhuhha, Mukindu, Sange, Rubirizi, Kiriba. Usige, a district of Urundi occupying the head of the lake, extends two marches into the north, or thirty miles; after which comes what is called Urundi Proper for another two days' march; and directly north of that is Ruanda, a very large country, almost equal in size to Urundi. Rubinga had been six days to the northward. There were some in his tribe who had gone farther; but from no one could we obtain any intelligence of a lake or a large body of water, such as the Albert Nyanza, being to the north. Sir Samuel Baker has sketched the lake as being within one degree north of the Tanganyika; but it is obvious that its length is not so great as it is represented, though it might extend thirty or forty miles south of Vacovia. Ruanda, as represented to us by Rubinga, Mokamba, chiefs of Usige, and their elders, is an exceedingly mountainous country with extensive copper mines. It occupies that whole district north of Urundi Proper between Mutumbi on the west and Urundi on the east, and Itara north-east. Of the countries lying north of Ruanda we could obtain no information. West of Urundi is the extreme frontier of Manyuema, which even here has been heard of. In returning to Ujiji, after the satisfactory solution of the river Rusizi, we coasted down the western shore of the Tanganyika, and came to Uvira at noon on the following day. We were shown the sandy beach on which the canoes of Burton and Speke had rested. Above, a little south of this, rises the lofty peak of Sumburizi, fully 4,500 feet above the level of the lake. Mruti, the chief of Uvira, still lives in the village he occupied when Burton and Speke visited his dominions. A day's march, or fifteen miles south of this, Uvira narrows down to the alluvial plains formed by the numerous streams which dash down the slopes of the western range; while the mountainous country is known as Uhembe, the land of the cannibals, who seldom visit the canoes of the traders. South of Uvira is Usansi, peopled by a race extremely cannibalistic in its taste. From Usansi we struck off across the lake, and, rowing all night, at dawn we arrived at a port in Southern Urundi. Three days afterwards we were welcomed by the Arab traders of Ujiji, as we once more set foot on the beach near that bunder. We have thus coasted around the northern half of the Tanganyika, and I might inform you of other tribes who dwell on its shores; but the principal subject of my paper was to show you how we settled that vexed question, "Was the Rusizi an effluent or an influent?" There is, then, nothing more to be said on that point.—In reply to a question from the President, Mr. Stanley said that Burton and Speke halted on a sandy beach just thirteen miles from the extreme end of Tanganyika. Had they gone but half way up the mountain, to the village where resided Amruta, the King of Uveri, they must have seen the northern head of Tanganyika plainly. But in drawing up at this point they simply took the point where the eastern and western ranges meet. The western range halts abruptly; the eastern ranges overlap it. I would not wish for sweeter water than the water of Lake Tanganyika.

*Dr. Livingstone's Recent Discoveries*, by Colonel J. A. Grant.

The two letters from Dr. Livingstone to Mr. Gordon Bennett, of the *New York Herald*, inform us that he had traced the southern waters from 11° to 5° south latitude, and he supposed they must flow on to the Nile by the Bahr Gazal, at 9° north latitude. I must say that this is an extravagant idea which cannot be entertained, for there are many circumstances precluding such a thing. The distance still unexplored by Dr. Livingstone may be roughly stated as nearly one thousand miles between his most advanced position and the mouth of the Gazal. In this distance we have Speke's Mountains of the Moon, and the great bend (to the west) of the Nile at 7° to 8° north latitude as the principal obstructions to Dr. Livingstone's theory. We also have three hundred miles of longitude between the two positions, but the crowning objection to Dr. Livingstone's waters reaching the Nile is the fact that we already know that the source of the Gazal was visited and determined only a few years ago by the eminent botanist Schweinfurth, who fully satisfied all geographers that the source of the Gazal is about 5° north of the Equator, and not, as Dr. Livingstone supposes, 11° south of it. My observations on the Gazal, made in March 1863, when descending the Nile from Gondokoro with my late companion, show that it is insignificant when compared with the Nile; it seems to be a swamp with little current, for the Nile branch, along which we were sailing, was not increased in width by the water from the Bahr Gazal, the Nile maintained its width of one hundred yards till after the Giraffe and Sooba joined it, then the stream was increased to a breadth of five hundred yards. The Gazal had no perceptible stream; at the junction its waters were still, and looked like a backwater, half a mile across, and surrounded by rushes. Our boatman and others told us that no boats were able to ascend it that year, 1863, as its channel was choked with reeds and the ambatch tree. There is, therefore, no regular traffic on it by boats; some years it is completely blocked, a contrast to the Nile, which is navigable to large dahabieh, all the year round, between Gondokoro and Khartoom. If anything were wanted to prove that the Gazal has no connection with the southern waters of Livingstone, reference might be made to several men who have been in the Gazal country; but Dr. Schweinfurth, who is now in Europe, would be most able to give definite information. The narrative of Dr. Livingstone contains some curious incidents which are quite novel to me, for, in our journey from Zanzibar to Egypt, when travelling on the watershed of the Nile, we never saw any race of cannibals, any signs of gorilla—neither did we find that any race of natives ever kept pigs in a domesticated state; they eat one species of wild hog, but no race in the valley of the Nile was ever seen to keep pigs tame. Oysters must be a misprint. Taking into consideration these remarkable differences from the country we traversed, I cannot but think that Dr. Livingstone, having no chronometers to fix his longitude, has got farther to the west than he supposes, and that he had got amongst races similar in most respects to those on the west coast of Africa, described by M. Du Chaillu. In conclusion, this fresh discovery of lakes and rivers by Livingstone has defined a distinct new basin, and left clearer than ever the position given by Speke to the Nile in 1863. Besides the two despatches to Mr. Gordon Bennett, we have now seen Dr. Livingstone's letters to Lords Stanley, Clarendon, and Granville, dated between 1870 and 1872. It is much to be regretted that they contain so little mention of latitude, longitude, and altitude, for his vast discoveries of new country cannot be laid down by our map makers with any degree of certainty—indeed, no two men could make a similar map out of all the geography he has forwarded. He informs us that his drainage, from 12° south latitude, has been traced by him up to 4° south latitude, and that he believes these waters continue to flow north, and from the valley of the Nile by joining the Nile of Speke at 9° north latitude by the Bahr el Gazal. No such thing can happen, for we have ample evidence, from independent sources, against the doctor's theory, besides which there are curious circumstances in his letters forbidding any connection with the Valley of the Nile. Livingstone tells us that the natives keep pigs, and that he had met with the skull of a gorilla. This shows a region distinct from the Nile races and the Nile animals, for nowhere on our route did we meet with pigs domesticated, or gorilla in the forests. Though this is but circumstantial evidence, it appears strong to me, and it also appears that he must have been farther west in his longitude than he supposed.

Sir Samuel Baker states that the Nile receives the following rivers from the west, namely, "The Yé, third class, full from 15th April to 15th November, also another smaller river, third class, full from 15th April till 15th November," and "the Bahr el Gazal, little or no water supplied by this river." Having seen this river in March of the same year, I can testify to the correctness of Sir Samuel's description, from the notes made by me upon it. These notes state the Bahr Gazal gave little or no water to the Nile, which was not increased much in size after its junction. The Bahr Gazal was still water, and the Nile flowed past it at the rate of about two miles per hour. It must be remembered also that this was only one branch of the Nile; the other, the Bahr Giraffe, supplied half as much again of water, and flowed at the rate of four miles an hour where it joined the Nile. Another fact worth noting is, that the water of the Bahr Gazal is described as being clear by Sir Samuel—this implies that the water had settled, that it was still water, and that there can be no current, but if additional proof was necessary to show that Dr. Livingstone's waters from 12° south latitude do not join the Nile here, we have the crowning evidence—which Dr. Livingstone is not aware of—of the German traveller, Dr. Schweinfurth, who determined the source of this Bahr Gazal at 3° or 5° north latitude. Dr. Livingstone has been informed by natives that Speke's Victoria Nyanza consists of three or four lakes, the Okara, Kavirondo, the Naibash, and the Baringo; but we know from Speke's map and from his writings, that the Okaro is the Ukereweh lake, the Naibash is the Naivasha, on the east of his lake, and that the Baringo is to the north-east of his lake. Captain Speke tells us that he had seen quite half of the lake, as laid down by him, his longitudes and latitudes and altitudes gave him its general outline, and where he could not obtain those he had to content himself with native information. We, therefore, think that his information is perfectly reliable, and that Dr. Livingstone has been misinformed—at all events, the Victoria Nyanza discharges quite sufficient water to form a navigable river from the Ripon Falls to the Mediterranean, and no stream which joins it in its course can for a moment be compared to it in size. Dr. Livingstone makes a mistake as to its size; he calls it eighty to ninety yards wide, writes of it as the "little river," and I cannot conceive where he got his information. Speke's estimate makes it a width of 150 yards—not feet—across the actual waterfall, and immediately above this, he tells us that it is 300 yards wide. From this we turn to the Tanganyika Lake. Unfortunately, its altitude is not mentioned, so we must take it for granted that Speke's measurement of 1,800 feet was correct; but Dr. Livingstone tells us that it has some influence on the Nile. It is tantalising to be told this bare statement, which leads us to conclude that it runs into his line of drainage, and not into the Indian Ocean—as I suppose it does. However, if life and health be spared to the doctor he will determine this point when he has visited the southern end of this lake; and it is to be hoped that he will send us despatches with more frequency, as there is nothing to prevent his doing so by the hands of the numerous traders travelling between Zanzibar and Ujiji.

Consul Petherick was then called on by the President to give an account of the Bahr el Gazal river, the great tributary of the Nile discovered by him.

Dr. Beke then said it was by no means pleasant for him to have to recant the opinions which he had so long maintained, but he was perfectly convinced that Livingstone had not discovered the sources of the Nile. Capt. Speke made Tanganyika 1,700 ft., Baker made the Albert Nyanza 2,700 ft., or, as it had been corrected, 2,500 ft. Even making an allowance of 200 ft. or 300 ft., it seemed impossible, on account of the levels, that the river Lualaba should flow into either of those lakes. He concluded that the Lualaba must either go into the Ulle or into some lake, or turn round to the Congo. He did not, however, think it went to the Congo, owing to the ascertained levels. It was a mortifying thing to have to acknowledge that what he had so long contended for was wrong, but the facts which had previously been made known led to the inevitable conclusion that what Livingstone had discovered was not the source of the Nile.

Sir H. Rawlinson said he was glad to have this opportunity of bearing testimony to the great value which the Royal Geographical Society attached to Mr. Stanley's services, and also of expressing the high opinion they entertained of his merit as a traveller. Livingstone had, no doubt, achieved a great geographical success in discovering the great interior system of river beds, but from his letters it was evident that almost to the very last he had strong misgivings about his being upon the Nile basin. Over and over again he said it had occurred to him that he might have been on the Congo. What did really become of this great river system which he had discovered it was impossible to say authoritatively. All that could be said was, that it was a completely new discovery, but he trusted that Livingstone himself would be the discoverer of where those great central rivers run, for he should be sorry if he did not carry out to a successful issue the great work upon which he had been so long, and so honourably, and so conscientiously engaged. In a matter of this sort all must be conjecture, but putting all the arguments side by side, he confessed to the supposition that this great river system fell into a large central inland lake. He should very much doubt its ever reaching Lake Chad, but there was a very large space in the interior of the continent which might very well be occupied by such a river-stream draining all the surrounding mountains. The discovery of the lake (if there be such a lake) into which the central waters ran, would, he trusted, be the crowning success of Livingstone's African travels. He had announced that he was going to the source of those waters, and when he had satisfied himself of that, he would return northward, and with the supplies furnished him in a great measure by Mr. Stanley, he would be then in a position to follow the system beyond a point where he was previously arrested. His great difficulty on former occasions was owing to the incapacity and hostility of his attendants, but it was most gratifying to know that he had now at his command a faithful and efficient body of followers.

Mr. Clements Markham asked Mr. Stanley whether the country of Balegga, which Livingstone alluded to, was the same as that laid down in Sir Samuel Baker's map as Malegga. Baker gave the name of Malegga to a range of mountains, and Livingstone spoke of it as a mountainous country.

Mr. Stanley said he believed the two names referred to the same region. No importance could be attached to difference in spelling, as it was purely a matter of hearing. To one traveller the word might appear to be pronounced one way, and to another traveller another. It was not even a difference of dialect. It was simply a difference in the spelling owing to the difference in the hearing.

The President, in closing the discussion, said that there was not the slightest doubt that Dr. Livingstone could only draw inferences as to where the waters he saw found an outlet. It was entirely a matter of theory. Geographers dealt with the facts that he had made known to them, and from those facts they drew their conclusions, just as Dr. Livingstone, from the same facts, drew his conclusions.

## SCIENTIFIC SERIALS

THE *Monatsbericht der k. preussichen Akademie der Wissenschaften zu Berlin*, from January to March, 1872. The most numerous scientific papers in these three numbers of the Berlin *Monatsbericht* are of a chemical nature, consisting of notes by Prof. Hofmann on aromatic phosphines, on the products of oxidation of methylphosphine and ethylphosphine, and on derivatives of the ethylene-bases, and a paper by Prof. Rammelsberg on the chemical composition of ambygonite. Prof. von Rath gives an account of a meteorite which fell at Ibbenbiehren, in Westphalia, on June 17, 1870, and consists chiefly of silica, protoxide of iron, and magnesia, with small quantities of protoxide of manganese, lime, and allumina. Structurally it consists of a granular mass, containing imbedded crystaline grains, the composition of both being mainly identical. Prof. H. A. Schwarz, of Zurich, communicated a continuation of his investigations upon special mineral surfaces. Prof. Riess has a paper on the reaction of the induced currents in an unaltered circuit upon the main current of the Leyden battery. Prof. Helmholtz communicates a summary of the results obtained by Dr. W. Dolerowolsky, of St. Petersburg, by experiments upon the sensibility of the eye to differences in the luminous intensity of various spectral colours The natural history papers consist of a notice by Prof. Ehrenberg of Prof. Whitney's recent investigations of the Californian 'Bacillarian rocks, a paper by Dr. Roth on the geology of the Philippian islands, and two geographical memoirs by Prof. W. Peters, one giving the synonyms of the species of the chiropterous genus *Megaderma*, and describing a new form (*M. cor* Peters, from Abyssinia), the other discussing in considerable detail the species of Batrachia, collected by Spix in Brazil, after an inspection of the original specimens in the museum at Munich.

*Journal of the Chemical Society*, June 1872.—This number opens with a lecture on the "Chemistry of the Hydrocarbons" by C. Schorlemmer, which is a condensed summary of the history of this subject. In the opening part of this paper the definitions of organic chemistry are discussed, Mr. Schorlemmer preferring to define it as "the chemistry of the hydrocarbons and their derivatives." The lecturer then proceeds to show how far our knowledge of the constitution of the hydrocarbons has advanced. The chemistry of the paraffin series of hydrocarbons is first entered upon; these perhaps have been more completely studied than any of the succeeding series. All the paraffins of known structure may be divided into four groups; the first or normal paraffins have been to a great extent worked out by the author, and are those in which each carbon atom is directly combined with at least two other carbon atoms forming a symmetrical chain. The other three groups have not been studied completely, and are not here discussed. By abstracting two atoms of hydrogen from the paraffins a series of hydrocarbons is obtained called the olefines. The probable constitution is not, as was at one time supposed, that they have carbon atoms with free combining units, thus—$CH_2 - CH_2$—; but that one carbon atom is linked by two combining units to another carbon atom, thus $CH_2 = CH_2$. The hydrocarbons of the acetylene series were next introduced; these are formed by again abstracting two other atoms of hydrogen from the olefines; in acetylene, for instance, it is probable that the carbon atoms may be linked together by three combining units of each atom, thus $CH \equiv CH$. The aromatic hydrocarbons have been very much worked on during the last few years. The present theory of their constitution is due to Kekulé, who supposes that the six carbon atoms are united together in a sort of chain or hexagon by one and two combining units alternately, which would then leave six combining units unsaturated. These when combined or saturated with hydrogen will yield the hydrocarbon benzol, which may be considered as the starting point of the aromatic series. The differences observed in certain groups of isomeric aromatic compounds may be accounted for by the supposition that the different relative positions of certain elements or radicals as attached to the carbon nucleus cause a difference in the physical condition of the substance. The constitution of various other hydrocarbons, such as naphthaline, anthracene, &c., is discussed; but they are too complex to be here described in detail.—E. A. Letts has prepared benzyl isocyanate by the action of the benzyl chloride on argentic cyanate, benzyl isocyanurate being also formed during the reaction. By the action of ammonia on the former body monobenzyl urea is formed, which by treatment with water yields dibenzyl urea. By substituting aniline in the place of ammonia in the last-named reaction phenyl benzyl urea is produced.—T. E. Thorpe contributes some laboratory notes on various subjects' and Prof. Himly a paper "On the estimation of carbonic acid in sea water," the method recommended is to precipitate the carbonic acid by baryta water, and to estimate the baric carbonate produced, it having been found that simply boiling the water is insufficient to drive off the whole of the carbonic anhydride. A detailed description of an apparatus for the collection of sea water below the surface is promised, which is also to be provided with the means of adding the reagent below the surface of the sea so as to avoid any loss of carbonic anhydride during and after the collection of the sample. The abstracts which follow the paper are as usual full of interest, containing as they do the pith and substance of many important papers.

## BOOKS RECEIVED

ENGLISH.—The Graft Theory of Disease: Jas Ross (J. and A. Churchill). Magnetism and Deviation of the Compass: Merrifield (Longmans).—A Handbook of Chemical Technology: R. Wagner (Churchill).

FOREIGN.—Forstzoologie: Dr. B. Altum.—Nomenclatur Botanicus, Vol. i., Mos. 1—6: L. Pfeiffer.—Anatomische Untersuchungen: G. Retzius.—Aufgaben aus der analytischen Mechanik, Nos. 1, 2: Dr. A. Zuhrmann.—Theorie der Bewegung der Kräfte; ein Lehrbuch der theoretischen Mechanik: Dr. W. Schell —Faune des Vertébrés de la Suisse: Dr. V. Fatio.

## CONTENTS



ERRATA.—Vol. vi., p. 300, 1st col., line 2 from bottom, for "this" read "the;" 2nd col., line 10 from bottom, for "Fig 4" read "Fig. 1;" last line, for "Fig. 5" read "Fig. 2;" p. 301, 1st col., line 12 from bottom, for "Fig. 6" read "Fig. 3;" p. 302, 1st col., line 28, for "Fig. 1" read "Fig. 4;" line 39, for "Fig. 2" read "Fig. 5;" line 45, for "Fig. 3" read "Fig. 6;" 2nd col, line 22 from bottom, for "modified" read "modifying:" p. 303, 2nd col., line 13, for "the" read "this."

THE BRITISH ASSOCIATION.—*Authors of papers are requested to favour the Editor of* NATURE *with copies or abstracts of their communications as soon as possible, as by these means alone can an accurate and early notice be insured. The Editor appeals to men of science to aid him in his attempt to give an account of the results of their investigations to their brethren throughout the world.*