stellar point situated near the central region of the nebula. It is quite free from any blurred appearance or any aspect of indefiniteness other than that introduced by the nebula on which it is projected.

On later nights the star seemed to have slightly decreased; its light was feebler and less sparkling, but I made no exact comparisons for tracing the decline of brilliancy, if any.

During many years the naked eye appearance of this conspicuous nebula has been familiar to me, and I have been accustomed to notice it particularly while engaged in prolonged watches for shooting stars. No sharply-defined nucleus was ever perceptible, but now the involved star is distinctly visible by slightly averting the vision. When the air is very clear the glowing out of the star now and then is very obvious, and I mention the fact in proof that the variation of the nebula by this new phenomenon is sufficiently great to affect its naked-eye aspect. W. F. DENNING

Bristol, September 13

LETTERS TO THE EDITOR

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

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

Red Rays after Sunset

THERE have lately been seen here some remarkable examples of rose-coloured streamers radiating from the sun at an interval of from 20 to 30 minutes after sunset, particularly on the 3rd, 5th, and 6th of this month. On the 3rd the appearance was especially striking, the contrast of colour between one very broad, vertical ray and the greenish-gray sky which separated it from its neighbours being most marked.

That these rose-coloured rays are essentially identical with the diffused rose-tint observed on other occasions is evident, not only from the similarity of colour and of interval after sunset at which they appear, but also from the occurrence of intermediate examples, in which the rays are so far and so broad that the radiate character is almost lost.

It is, however, by no means so clear why the coloured tract of sky should be sometimes split into rays, and it is with a view to ventilate this question that I desire to call attention to the subject.

I believe it is generally supposed that the dark spaces between the rays are due to masses of cloud intercepting the sun's light, but there are difficulties in the way of this explanation which I have never seen met.

It need hardly be pointed out that the matter (whatever it be) which reflects the red light must be at an altitude far above any such masses of cloud as could intercept the sun's rays; it could not otherwise receive and reflect those rays half an hour after the sun had set to the observer. But although above the level of the clouds, the reflecting matter would still be subject to interception of the sun's rays by cloud at sunset, and in order to judge whether the phenomenon can be so accounted for it is necessary to consider what kind of horizon that would be behind which the sun would set to an observer at the altitude supposed. My impression is that the horizon as seen from such a height would be so distant that whatever the irregularities of cloud-surface forming it, it would be practically a level line, and that the most mountainous masses of cumulus-cloud would be insufficient to cast at that distance the enormous shadows which would be necessary to account for the rifts between the rays.

Clifton, September 8 GEORGE F. BURDER

Fireball

A LARGE fireball was visible at Bristol and other places on September 11, at about 9h. 25m. p.m. It was described to me by several observers who approximately assigned its path as from *Altair* towards the western horizon. The sky was much clouded here at the time, with only 1st magnitude stars visible, but the light of the meteor appears to have been something astonishing.

Mr. G. T. Davis, of Theale, near Reading, writes me that, when first seen there, the meteor was near β Ophiuchi, and seemed to describe a slightly curved path to the horizon, which it touched apparently under β Serpentis. It exhibited a greenish tinted disk with bright, white aureole around it, and left no train. The aureole was at least 16′ in diameter.

It will be desirable to collect further accounts of this fine meteor. The direction of its path suggests that it may belong to the same system as that of the detonating fireball of September 14, 1875, which had a radiant point at α 348°, δ 0° ± (Tupman). During the past fortnight I have observed a considerable number of shooting-stars, and one of the best radiant points is at α 346, δ 0° ±, or 2° W. of that of Col. Tupman's fireball of September 14, 1875. W. F. DENNING

Bristol, September 13

Pulsation in the Veins

IF Mr. Hippisley will refer to Landois' text-book, vol. i. p. 196, he will find it there stated, on the authority of Quincke, that a venous pulse occurs on rare occasions, normally, in the veins on the back of the hand and foot, when the peripheral ends of the arteries become dilated and relaxed. But it is to be remembered that the very same phenomenon may obtain *abnormally*, owing to some pathological condition of the heart, as stenosis of the mitral orifice, or insufficiency in action of the mitral valve. Mr. Hippisley does not state in his letter whether the heart was in a healthy condition, or whether any lesion of that organ was present in those on whom his experiment was tried. J. W. WILLIAMS

Middlesex Hospital

"Furculum" or "Furcula"

Is there any authority for the use of furc*ulum* for the *os furculatorium* of birds? I am told by a contributor to the *Proceedings* of this Society, whose phraseology I have ventured to interfere with, that "furc*ulum*" has been employed by Balfour, Huxley, and Rolleston. Such may be the case, but it is possible that even these great anatomical writers may have erred in the use of a Latin termination. No dictionary that I have been able to refer to contains the word "furculum."

The Zoological Society of London P. L. SCLATER

THE BRITISH ASSOCIATION

Aberdeen, Monday

THERE have been few meetings of the British Association so crowded with papers in nearly all the sections. On Saturday several sections met which, unless under the greatest pressure, never meet on that day. Section D has been compelled to split up into three subsections, and probably most of the sections will have to meet on Wednesday morning. The social distractions have been much more numerous than usual, and we suspect have somewhat seriously interfered with the legitimate work of the meeting. As might be expected, the Music Hall was crowded on Wednesday evening last to hear the President's address, which seems to have produced a great impression on the audience.

It is being more and more strongly recognised that such pre-arrangements as those of Sections A and B ought to become general throughout the sections. The discussions in the two great sections, of which the programmes have appeared in NATURE, have certainly excited great interest among real workers in physics and chemistry. It is to be hoped that a full abstract of these discussions will be placed on record, as otherwise they cannot have any great permanent results. Perhaps the most popular feature in the regular sectional work has been the reading of Sir John Lubbock's paper on ants, in Section D, on Friday.

The number of entertainments, afternoon parties, excursions, and *conversaziones* is almost without precedent. The *conversazione* in the Art Galleries on Thursday last was in every way successful, though the place was overcrowded. The flower and fruit show and the illuminations outside reminded many of the South Kensington displays. It was satisfactory to notice that, thanks to

Prof. Traill, the nucleus of a valuable local natural history collection has been formed. Prof. Osborne Reynolds's illustrations of compression of solids was one of the most attractive features of the evening. The collection of pictures was large and highly creditable, while the precious collections of old manuscripts and books lent by the Earl of Crawford had many admirers. One of the most successful afternoon parties was given the same day at Tollshill Wood by Mr. David Stewart. Of course, of the numerous Saturday excursions, that to Balmoral was the most popular. In spite of the wretched weather 200 people must have left Aberdeen for Ballater at 1 p.m. and happily by the time the end of the railway journey was reached the weather greatly improved. The drive from Ballater to Balmoral evidently gave great enjoyment to the occupants of the long cavalcade of miscellaneous "machines" which wound along the banks of the Dee, and no less, we may be sure, did the sumptuous five o'clock dinner ("lunch," it was called) which was provided in the ball-room of Balmoral. Gen. Gairdner presided at the table, and, after proposing the Queen's health, drank, by command of Her Majesty, prosperity to the British Association. Under the guidance of Dr. Profeit the guests made a round of the fine grounds of Balmoral, and on driving back to Ballater, passed Her Majesty on her return from a day's outing. The excursion to Dunecht was also a great success, the arrangements at Lord Crawford's observatory exciting much interest.

A deputation from Birmingham is here to make arrangements for the visit to that town next year. It is evident that the Birmingham people mean to make the 1886 meeting a success, though, so far as social arrangements go, it will be difficult to surpass that of Aberdeen. It is expected that Manchester will be the place of meeting in 1887, and for 1888 or 1889 several enterprising members hope to secure the selection of London, in order to have a meeting in common with the American Association. Against this choice, however, there will probably be a strong protest, though of course the American Association will be sure to receive an enthusiastic welcome whenever it chooses to visit the old country.

Prof. Adams's lecture on Friday attracted a large audience, and on Saturday evening the Music Hall was filled with an enthusiastic audience of genuine working men to listen to Mr. H. B. Dixon's lecture and admire his experiments. Mr. Murray's lecture to-night will certainly be of popular interest, but, summing up as it does the present position of oceanography, it will also be of the highest scientific value. The diagrams are very striking, and certainly original. A full report will no doubt appear in NATURE.

The regret at the resignation of the secretaryship of the Association by Prof. Bonney is universal, though it is confidently expected that Mr. Atchison will be a thoroughly competent successor.

The additional arrivals up to this morning will bring the total number present at the meeting up to 2500.

SECTION B

CHEMICAL SCIENCE

OPENING ADDRESS BY PROF. HENRY E. ARMSTRONG, PH.D., F.R.S., SEC. C.S., PRESIDENT OF THE SECTION [1]

I NOW pass to the consideration of a subject of special interest in this section, which I think requires the immediate earnest attention of chemists and physicists combined—that of *Chemical Action*. In his Presidential Address to the Association last year Prof. Lord Rayleigh made only a brief reference to chemistry, but many of us must have felt that his few remarks were pregnant with meaning, especially his reference to the importance of the principle of the dissipation of energy in relation to chemical change. A year's reflection has led me to think them of peculiar weightiness and full of prophecy. I would especially draw attention to the closing paragraph of this portion of his address: "From the further study of electrolysis we may expect to gain improved views as to the nature of the chemical reactions, and of the forces concerned in bringing them about. I am not qualified—I wish I were—to speak to you on recent progress in general chemistry. Perhaps my feelings towards a first love may blind me, but I cannot help thinking that the next great advance, of which we have already some foreshadowing, will come on this side. And if I might, without presumption, venture a word of recommendation, it would be in favour of a more minute study of the simpler chemical phenomena."

Chemical action may be defined as being any action of which the consequence is an alteration in molecular constitution or composition; the action may concern molecules which are of only one kind—cases of mere decomposition, of isomeric change and of polymerisation; or it may take place between dissimilar molecules—cases of combination and of interchange. Hitherto it appears to have been commonly assumed and almost universally taught *by chemists* that action takes place directly between A and B, producing AB, or between AB and CD, producing AC and BD, for example. This, at all events, is the impression which the ordinary average student gains. Our text-books do not, in fact, as a rule, deign to notice observations of such fundamental importance as those of De La Rive on the behaviour of nearly pure zinc with dilute sulphuric acid, or the later ones of Faraday ("Exp. Researches," Series vii., 1834, 863, *et seq.*) on the insolubility of amalgamated zinc in this acid. Belief in the equation $Zn + H_2SO_4 = H_4 + ZnSO_4$ hence becomes a part of the chemist's creed, and it is generally interpreted to mean that zinc *will* dissolve in sulphuric acid, forming zinc sulphate, not, as should be the case, that *when* zinc dissolves in sulphuric acid it produces zinc sulphate, &c. In studying the chemistry of carbon compounds we become acquainted with a large number of instances in which a more or less minute quantity of a substance is capable of inducing change in the body or bodies with which it is associated without apparently itself being altered. The polymerisation of a number of cyanogen compounds and of aldehydes, the "condensation" of ketonic compounds and the hydrolysis of carbohydrates are cases in point; but so little has been done to ascertain the nature of the influence of the contact-substance, or *catalyst*, as I would term it, the main object in view being the study of the product of the reaction, that the importance of the catalyst is not duly appreciated. Recent discoveries, however—more particularly Mr. H. B. Dixon's invaluable investigation on conditions of chemical change in gases, and the experiments of Mr. Cowper with chlorine and various metals, and of Mr. Baker on the combustion of carbon and phosphorus—must have given a rude shock, from which it can never recover, to the belief in the assumed simplicity of chemical change. The inference which I think may fairly be drawn from Mr. Baker's observations—that *pure* carbon and phosphorus are incombustible in *pure* oxygen—is indeed startling, and his experiments must do much to favour that "more minute study of the simpler chemical phenomena" so pertinently advocated by Lord Rayleigh.

But if it be a logical conclusion from the cases now known to us that chemical action is not possible between any two substances other than elementary atoms, and that the presence of a third is necessary, what is the function of the third body—the catalyst—and what must be its character with reference to one or both of the two primary agents? In the discussion which took place at the Chemical Society after the reading of Mr. Baker's paper, I ventured to define chemical action as *reversed electrolysis*, stating that in any case in which chemical action was to take place it was essential that the system operated upon should contain a material of the nature of an electrolyte (Chem. Soc. *Proc.*, 1885, p. 40). In short, I believe that the conditions which obtain in any voltaic element are those which must be fulfilled in every case of chemical action. There is nothing new in this; in fact, it practically was stated by Faraday in 1834 ("Experimental Researches in Electricity," series vii. §§ 858, 859 [1]); and had due heed been given to Faraday's teachings we

[1] Continued from p. 453.

[1] "Those bodies which, being interposed between the metals of the voltaic pile, render it active, *are all of them electrolytes*, and it cannot but press upon the attention of every one engaged in considering this subject, that in those bodies (so essential to the pile) decomposition and the transmission of a current are so intimately connected that one cannot happen without the other. If, then, a voltaic trough have its extremities connected by a body capable of being decomposed, as water, we shall have a continuous current through the apparatus; and whilst it remains in this state we may look at the part where the acid is acting upon the plates and that where the current

should scarcely now be so ignorant as we are of the conditions of chemical change.

The questions—What is Electrolysis? What is an Electrolyte? are all-important to the chemist, if my contention be accepted. Moreover, the consideration of chemical action from this point of view almost of necessity obliges us also to consider what it is that constitutes chemical affinity. I will not presume to offer any opinion on this subject; but I would recall attention to the prominence which so great an authority as Helmholtz gave in the last Faraday Lecture (Chem. Soc. *Trans.*, 1881, 277) to the view held by Faraday, and which is so definitely stated in a passage in his "Experimental Researches"[1] (series viii. 918, also 850 and 869).

Helmholtz used the words: "I think the facts leave no doubt that the very mightiest among the chemical forces are of electric origin. The atoms cling to their electric charges, and opposite electric charges cling to each other; but I do not suppose that other molecular forces are excluded, working directly from atom to atom." In the passages which immediately follow, this physicist then makes several statements of extreme importance, which directly bear upon the subject I desire to discuss, and which, therefore, I quote[2]

The interpretation of Faraday's law of electrolysis, which Helmholtz has brought under the notice of chemists, is of the most definite and far-reaching character. Does it, however, at all events in the form in which he has put it forward, accord sufficiently with the facts as these present themselves to the chemist's mind? All will recognise that the chemical changes effected by a current in a series of electrolytic cells are equivalent to those which take place within the voltaic cells wherein the current is generated; but in neither case is the action of a simple character: in both a variety of chemical changes takes place, the precise character of which is but imperfectly understood, and we are unable to assign numerical values, either in terms of heat or electrical units, to most of the *separate* changes. Moreover, many compounds are not electrolytes, while others which are regarded by the chemist as their analogues are very readily decomposed by a current of low E.M.F., although no great difference is to be observed in their "heats of formation;" liquid hydrogen chloride on the one hand, and fused silver chloride on the other, may be cited as examples. Again, how are we to interpret on this theory such changes as that involved in the conversion of stannic into stannous chloride? The former, I suppose, is to be regarded as consisting of an atom of quadrivalent tin charged with four units of, say, positive electricity, and of four atoms of univalent chlorine, each carrying a unit charge of negative electricity; on withdrawal of two of the chlorine atoms, the residual $SnCl_2$ will have two free unit charges of positive electricity. We know that when the temperature is sufficiently lowered two such residues unite, forming Sn_2Cl_4, and it is not improbable that crystalline stannous chloride represents a still later stage of condensation. Is this compatible with the theory? That cases of this kind are contemplated would appear from the reference to "unsaturated compounds with an even number of unconnected units of affinity," which we are told may be charged with equal equivalents of opposite electricity; and also from the allusion to the existence of molecules of elementary substances composed of two atoms. It is more than probable that these anomalies would disappear on fuller statement of his views by the author of the theory: I have ventured to call attention to them in the hope of eliciting such statement.

Helmholtz tells us that electrolytes belong to the class of typical compounds, the constituents of which are united by "atomic affinities," not to the class of "molecular aggregates." Is this the fact? Before chemists can accept this conclusion many difficulties must be removed which appear to surround the question. In the first place, it is in the highest degree remarkable that, with the one single exception of *liquefied* ammonia, *no known binary hydride is in the liquid state an electrolyte*: liquid hydrogen chloride, bromide and iodide, for example, withstanding an E.M.F. of over 8,000 volts (8,040 De la Rue cells: Bleekrode). Water, again, according to Kohlrausch's most recent determinations, has an almost infinite resistance. Yet a mixture of hydrogen chloride and water readily conducts, and is electrolysed; an aqueous solution of sulphuric acid behaves similarly, although the acid itself has a very high resistance.[1] Very many similar examples might be quoted, but it is well known that aqueous solutions generally conduct more or less perfectly, and are electrolysed.[2]

The current belief among physicists would appear to be that the dissolved electrolyte—the acid or the salt—is almost exclusively primarily decomposed (Wiedemann, "Elektricität," 1883, ii. 924). We are commonly told that sulphuric acid is added to water to *make it conduct*, but the chemist desires to know why the solution becomes conducting. It may be that in all cases the "typical compound" is the actual electrolyte—*i.e.* the body decomposed by the electric current—*but the action only takes place when the typical compounds are conjoined and form the molecular aggregate*, for it is an undoubted fact that HCl and H_2SO_4 dissolve in water, forming "hydrates." This production of an "electrolytical system" from dielectrics is, I venture to think, *the* important question for chemists to consider. I do not

is acting upon the water as the reciprocals of each other. In both parts we have the two conditions, *inseparable in such bodies as these*, namely, the passing of a current and decomposition; *and this is as true of the cells in the battery as of the water-cell*; for no voltaic battery has as yet been constructed in which the chemical action is only that of combination: *decomposition is always included*, and is, I believe, an essential chemical part

"But the difference in the two parts of the connected battery—that is, the decomposition or acting cells—is simply this: in the former we urge the current through, but it, apparently of necessity, is accompanied by decomposition; in the latter we cause decompositions by ordinary chemical actions (*which are, however, themselves electrical*), and, as a consequence, have the electrical current; and as the decomposition dependent upon the current is definite in the former case, so is the current associated with the decomposition also definite in the latter."

[1] "All the facts show us that that power commonly called chemical affinity can be communicated to a distance through the metals and certain forms of carbon; that the electric current is only another form of the forces of chemical affinity; that its power is in proportion to the chemical affinities producing it; that when it is deficient in force it may be helped by calling in chemical aid, the want in the former being made up by an equivalent of the latter; that, in other words, *the forces termed chemical affinity and electricity are one and the same*."

[2] "Several of our leading chemists have lately begun to distinguish two classes of compounds—viz. molecular aggregates and typical compounds, the latter being united by atomic affinities, the former not. Electrolytes belong to the latter class. If we conclude from the facts that every unit of affinity is charged with one equivalent, either of positive or of negative electricity, they can form compounds, being electrically neutral, only if every unit charged positively unites under the influence of a mighty electric attraction with another unit charged negatively. You see that this ought to produce compounds in which every unit of affinity of every atom is connected with one, and only one, other unit of another atom. This, as you will see immediately, is the modern chemical theory of quantivalence, comprising all the saturated compounds. The fact that even elementary substances, with few exceptions, have molecules composed of two atoms makes it probable that even in these cases electric neutralisation is produced by the combination of two atoms, each charged with its full electric equivalent, not by neutralisation of every single unit of affinity. Unsaturated compounds with an even number of unconnected units of affinity offer no objection to such an hypothesis: they may be charged with equal equivalents of opposite electricity. Unsaturated compounds with one unconnected unit, existing only at high temperatures, may be explained as dissociated by intense molecular motion of heat, in spite of their electric attractions. But there remains one single instance of a compound which, according to the law of Avogadro, must be considered as unsaturated even at the lowest temperature—namely, nitric oxide (NO), a substance offering several very uncommon peculiarities, the behaviour of which will be perhaps explained by future researches." The popular mistake is here made of assuming that elementary substances, with few exceptions, have molecules composed of two atoms. We now know considerably over seventy elements, but of these the molecular weights in the *gaseous* state of only thirteen have been satisfactorily determined. The gaseous elements hydrogen, oxygen, nitrogen and chlorine, and also bromine, iodine and tellurium, have diatomic molecules; phosphorus and arsenic have tetratomic molecules; those of sulphur are hexatomic, and selenium molecules are probably of similar constitution, but more readily broken down than those of sulphur; lastly, cadmium and mercury molecules are monatomic. It is more than probable that carbon, and also silicon and boron, form highly complex molecules. Of the remaining undetermined elements, the greater number are metals, and it is not unreasonable to assume that many of these will be found to resemble cadmium and mercury in molecular composition. It is clear, however, that at present we have no right to say that the elementary molecules are, as a rule, diatomic. It would assist in removing this error if chemists would consistently place *after* the symbol the numeral indicating the "atomicity" of the elementary molecule—thus, Hg_1, Cd_1, O_2; and if in all cases when a numeral is absent, or is placed *before* the symbol, it were understood that advisedly no indication of the molecular state is afforded.

[1] It is more than probable that the most nearly pure sulphuric acid which can be obtained is not homogeneous, but is at least a mixture of H_2SO_4, $H_2S_2O_7$ and "hydrated compounds" in proportions depending on the temperature, and hence that (pure) sulphuric acid, H_2SO_4, like water, would behave as a dielectric.

[2] On the other hand, it is remarkable that, whereas liquefied ammonia may be electrolysed, an aqueous solution of ammonia is a most imperfect conductor (Faraday, F. Kohlrausch), although solutions of ammonium salts compare favourably in conductivity with corresponding sodium and potassium salts. This fact serves *somewhat* to allay the suspicion that Bleekrode did not take sufficient precautions to dry the ammonia; but his result cannot, I think, be accepted as final, on account of the relatively high E.M.F. required, and the repetition of the experiment with every precaution to ensure purity of the gas is most important. Faraday regarded the decomposition of ammonia on electrolysis of its solution as merely the result of secondary action.

believe that we shall be able to state the exact conditions under which chemical change will take place until a satisfactory solution has been found.

F. Kohlrausch (*Pogg. Ann.* 1876, 159, 233) has shown that, on adding sulphuric acid to water, the electric conductivity increases very rapidly until when about 30 per cent. of acid is present a maximum (6,914) is attained; conductivity then diminishes almost as rapidly, and a minimum (913) is reached when the concentration corresponds with that of a monohydrate (H_2SO_4,OH_2); from this point conductivity increases somewhat (to 1,031 at 92·1 per cent. H_2SO_4), and then again falls, and is probably zero for the pure acid; on adding sulphuric anhydride to the acid conductivity again increases. Solutions of other acids and of a number of salts—chiefly deliquescent and very soluble salts—also exhibit maximum conductivity at particular degrees of concentration. In no other case has the existence of two maxima, such as are observed in solutions of sulphuric acid, been established; but probably this is because the experiments either have not been, or cannot well be, carried out with pure substances or very concentrated solutions. Solutions of less soluble salts increase in conductivity as the amount of salt dissolved increases.

Kohlrausch has suggested, as an explanation of the influence of the "solvent" on the conductivity of an "electrolyte," that in a solution the ions which are being transferred electrolytically come less frequently into collision than would be the case in the pure substance. There is therefore less opportunity for the formation of new molecules, and the ions are able to travel farther before entering into combination.

Regarding the question from a chemist's point of view, however, I cannot help thinking that this explanation is scarcely satisfactory or sufficient; but I cannot resist the feeling that the production of electrolytically conducting solutions from dielectrics is in some measure dependent upon the occurrence of chemical action. If the composition of the solutions of maximum conductivity be calculated,[1] it will be seen that they contain but a limited number of water molecules; thus the solution of sulphuric acid of maximum conductivity (at 18°) contains 30·4 per cent. of acid, and therefore has the composition $H_2SO_4 : 12{\cdot}4\ H_2O$ (approximately); for nitric acid the ratio is 1 : 8; for acetic acid it is about 1 : 17. Now, it is highly remarkable that the solutions of maximum electric conductivity are also very nearly those in the formation of which nearly the maximum amount of heat is developed; this will at once be obvious on comparison of the curves given by Thomsen ("Thermochemische Untersuchungen," vol. iii.) and by Kohlrausch. In the chemist's experience, the point of maximum heat development is usually near to the point of maximum chemical change, and I think, therefore, that we are justified in concluding that, even if electrical conductivity be not a maximum at a particular concentration on account of the presence of a particular hydrate (belonging to the class of molecular aggregates) in maximum amount, at all events the "structure" of the system is especially favourable, and the "chemical influence" exerted by the one set of molecules upon the other is at a maximum at the point of maximum conductivity. The fact that the amount of sulphuric acid required to form a solution of maximum conductivity increases with temperature—

Temp.	0°	10°	20°	30°	40°	50°	60°	70°
Per cent.	30·2	30·9	30·7	32·5	33·5	34·1	34·5	35·4

and also the fact that the maxima and minima of conductivity tend to become obliterated with rise of temperature (Kohlrausch), are both in accordance with the view that conductivity is in some way dependent upon chemical composition, as the effect of rise of temperature would be to cause the dissociation of hydrates such as I have referred to. The increase in conductivity of aqueous solutions with rise of temperature would appear to be against the view here put forward; but it is probable that this may be largely due to diminution in viscosity and increase in the rate of diffusion.

Our knowledge of the binary metallic compounds, which are generally admitted to be electrolytes *per se*, also affords evidence, I think, of an intimate relation between chemical constitution and "electrolysability." It has been pointed out (comp. L. Meyer, "Theorien d. mod. Chemie," 4th ed. p. 554) that, whereas all the metallic chlorides and analogous compounds which cannot be electrolysed are easily-volatile bodies, the electrolysable metallic chlorides, &c., are fusible only at high temperatures. A careful discussion of the various known cases does not, however, justify the conclusion that decomposition takes place, or not, according as the temperature at which the body assumes the liquid state—and at which, therefore, there is full opportunity given for electrolysis to take place—is high or low, especially as recent observations show that electrolysis may take place prior to fusion. But it is especially noteworthy that many of the chlorides, &c., which are electrolytes undoubtedly contain more than a single atom of metal in their molecules; indeed, after careful consideration of the evidence, I am inclined to go so far as to put forward the hypothesis *that among metallic compounds only those are electrolytes which contain more than a single atom of metal in their molecules.* No difficulty will be felt in granting this of cuprous and stannous chlorides, and even of cadmium, lead, silver, and zinc chlorides; but opinions will differ as regards the metals of the alkalies and the alkaline earths.[1] Assuming the constitution of metallic electrolytes to be such as I have suggested it is not improbable that on electrolysis a part only of the metal is determined to the one pole, the remainder being transferred along with the negative radical to the opposite pole. Hittorf, indeed, has already put forward this view in explanation of the remarkable results he obtained on determining the extent of transfer of the ions in aqueous and alcoholic solutions of the chloride and iodide of cadmium and zinc.

Again, an argument in favour of a connection between chemical constitution and electrical conductivity is the fact that carbon, sulphur, selenium and *phosphorus* each exist in conducting and non-conducting modifications, as it can scarcely be doubted that the so-called allotropic modifications of these elements are differently constituted.

It appears, as I have already said, to be the current belief that when aqueous solutions are submitted to electrolysis, as a rule the dissolved substance, and not the water, is the actual electrolyte. Without reference to the question I have raised as to the constitution of an electrolyte, it appears at least doubtful whether this view can be justified by appeal to known facts; at all events, I have failed to find satisfactory evidence that such is the case. Moreover, as sulphuric anhydride dissolves in water with considerable development of heat, it would appear that more work has to be done to separate hydrogen from sulphuric acid than to separate it from water; on this account we might expect that the water rather than the acid would be decomposed. Are not perhaps both affected according to the proportions in which they are present? The marked variation in the extent to which the negative ion is transferred to the positive pole, as observed by Hittorf, when solutions of different degrees of concentration are electrolysed, would appear to support this view. The difference in the products, according as dilute or very concentrated solutions of sulphuric acid are used, may also be cited as an argument that the chemical changes effected vary with the concentration; but, on the other hand, it is quite possible that the observed differences may result from the occurrence of purely secondary changes. Ostwald has recently put forward the view that one *or more* of the hydrogen atoms of certain acids are split off according to the concentration of the solution.

I call attention to this because I conceive that it has a most

[1]

Formula	Formula weight	Per cent. in solution of max. cond.	Composition in approximate mol. ratios	Conductivity
HNO_3	63	29·7	1 : 8	7330
HCl	36·4	18·3	1 : 9	7174
H_2SO_4	98	30·4	1 : 12·4	6914
H_3PO_4	98	46·8	1 : 6	1962
$C_2H_4O_2$	60	16·6	1 : 17	15·2
KOH	56	28·1	1 : 8	5995
$NaOH$	40	15·2	1 : 12·7	3276

[1] We may regard as evidence in support of this explanation the fact that neither beryllium chloride, which fuses at 600°, nor mercuric chloride, is an electrolyte, as both of these, at temperatures not far removed from their boiling-points, exhibit the simplest possible molecular composition. It should be pointed out, however, that Nilson and Patterson found it possible to determine the density of beryllium and chloride gas at a temperature 100°—150° below the melting-point found by Carnelly; but they were not able to say that fusion took place. Clarke's recent interesting observations on mercuric chloride and iodide do not, I think, suffice to prove that these compounds are electrolytes; it is more than probable that electrolysis is preceded by the formation of mercurous compounds. Even an aqueous solution of mercuric chloride does not conduct appreciably better than water (Buff). I should perhaps add that the mere presence of more than a single atom of metal in the molecule does not, I believe, alone constitute the compound an electrolyte; much depends probably both on the nature of the metal and on the structure of the molecule.

important bearing on the discussion of the nature of the chemical changes which occur during the dissolution of metals. Formerly it was said that when zinc acts upon dilute sulphuric acid, the zinc displaces the hydrogen of the water and the resulting zinc oxide dissolves in the acid, forming zinc sulphate; the modern explanation advocated by most chemists has been that the metal directly displaces the hydrogen of the acid: in fact, that this is the nature of the change whenever an acid is acted upon by a metal. If in a solution of sulphuric acid, of whatever strength, the acid be the actual electrolyte, I imagine that we are right in accepting this modern view; but if the water be the electrolyte, we must, to be consistent, return to the view that the oxide—more probably in most cases the hydroxide—is the primary product. And if it can be shown that during electrolysis both water and acid, according to circumstances—concentration, E. M. F., &c.—undergo change, it will be necessary to teach that in a similar manner the action of metals on acids is no less complex. Our views on the action of metals on concentrated sulphuric acid, and on solutions of nitric acid of various strength, must also materially depend on the interpretation of the behaviour of these acids on electrolysis with varying electromotive forces.

Having thus fully explained why I venture to think that Helmholtz's definition that "electrolytes belong to the class of typical compounds, not to that of molecular aggregates," is somewhat open to question, it now becomes necessary to make some slight reference to the constitution of these so-called molecular aggregates. Although opinions differ widely as to the definition to be given of a typical or atomic compound, and of a molecular compound or aggregate, the majority of chemists appear to agree that we must recognise the existence of two distinct classes of compounds. Prof. Williamson, in his address to this Section at the York meeting (1881), entered at length into the discussion of this question, and in very forcible terms objected to the recognition of molecular combinations as something different from atomic combinations; in this I, in the main, agree most fully with him. He further said that he had been led to doubt whether we have any grounds for assigning any limits whatever to atomic values, and he adduced a number of cases which, in his opinion, afforded illustration of a capability of elements to assume greater atomic values by combining with both negative and positive atoms than with atoms of one kind only; for example, he cited the compounds K_2CuCl_4 and K_2HgCl_4 as proof that copper and mercury may assume hexad functions; the compound K_2AgI_3 as an illustration that silver may act as a pentad; and the compounds $KAsF_6$ and K_2AsF_7 were regarded by him as evidence of the heptadicity and nonadicity of arsenic.

I have long been of opinion that the experimental investigation of this question is of great importance, and I believe that it must ere long attract the attention it deserves. The problem will be solved, not by discussions on the fertile theme of valency, but by determining the structure—the constitution—of bodies such as were referred to by Prof. Williamson.

My own view on the question is a very decided one. So far as the mere definition of valency is concerned, I entirely agree with Lossen; and, as I have said, I hold with Prof. Williamson that in all compounds the constituents are held together by atomic affinities, and atomic affinities only, but I believe that the formation of so-called molecular compounds is mainly due to peculiarities inherent more especially in the negative elements—*i.e.* the non-metals and metalloids and not in the positive elements—the metals; in other word, to the fact that, as was first pointed out, I believe, by Lothar Meyer, the negative elements tend to exhibit a higher valency towards each other than towards positive elements. The view I take, then, is, that in the majority of so-called molecular compounds the parent molecules are preserved intact in the sense in which a hydrocarbon radical, such as ethyl, is preserved intact in an ethyl compound, being held together by the "surplus affinity" of the negative elements. Thus I would represent the compounds K_2CuCl_4 and K_2HgCl_4 as containing copper and mercury of the same valency as the metal in the parent chloride, and regard them as compounds of the radicals $(CuCl_2)$, $(HgCl_2)$ and (KCl); a view which may be expressed by the formulæ

$$Cu\begin{matrix}Cl\,.\,ClK\\Cl\,.\,ClK\end{matrix} \qquad Hg\begin{matrix}Cl\,.\,ClK\\Cl\,.\,ClK\end{matrix}$$

The arsenic compounds referred to may be similarly represented

$$F_4AsF\,.\,FK \qquad F_3As\begin{matrix}F\,.\,FK\\F\,.\,FK\end{matrix}$$

We do not hesitate to attribute to the so-called double cyanides this order of structure, without in any way supposing that the metal changes in valency. Evidence that the "constituent radicals exist unchanged in molecular compounds" is afforded by facts such as that ferrous and potassium chlorides, for example, form a compound which obviously is still ferrous, being of a green colour, which would hardly be the case if the valency of the iron were increased; and that in like manner the compounds formed from stannous chloride manifest all the properties of stannous derivatives.

Whatever be the nature of chemical affinity, it is difficult to resist the conclusion that the "charge" of a negative radical especially is rarely, if ever, given up all at once, that its affinity is at once exhausted. It would also appear that the amount of residual charge—of surplus affinity—possessed by a radical after combination with others depends both on its own nature and that of the radical or radicals with which it becomes associated. Differences such as are observed in the composition and stability of the hydrates of the salts of an acid—the sulphates, for example—clearly point to this. Other illustrations are afforded by the manner in which chlorhydric acid yield chlorhydrates of some metals and chlorides of others.

It is noteworthy, however, that often those elements which from the ordinary point of view are regarded as possessed of feeble affinities are those which manifest the greatest tendency to form molecular compounds. Thus it is commonly held that, of the three elements, chlorine, bromine and iodine, chlorine has the highest and iodine the lowest affinity, and this views accords well with the recent observations of V. Meyer on the relative stability of their diatomic molecules at high temperatures; but nevertheless we find that the compound which HI forms with PH_3 is far more stable than that of HBr or HCl with this gas; and it is well known that mercuric *iodide* has a much greater affinity for other iodides than have mercuric bromide and chloride for the corresponding bromides and chlorides.[2]

The recognition of the peculiarity in the negative elements to which I would attribute the formation of molecular compounds must, I think, exercise an important influence in stimulating and directing the investigation of these compounds and of compounds other than those of carbon; in the near future the determination of the structure of such compounds should occupy an important share of the chemist's attention. It will perhaps afford a clue in not a few cases which are not altogether satisfactorily interpreted in accordance with the popular view of valency. I may instance the formation of (?) polymeric metaphosphates, of complex series of silicates and tungstates, and of compounds of hydrocarbons with trinitrophenol. It may even serve to explain some of the peculiarities of the more complex carbohydrates.

It is one of the most clearly established of the "laws of substitution" in carbon compounds that negative radicals tend to accumulate: numerous instances are afforded by the behaviour of paraffinoid compounds with chlorine, bromine and oxidising agents, and by that of unsaturated paraffinoid compounds when combining with hydrogen bromide and iodine. The special affinity of negative elements for negative is not improbably the cause of this accumulation. A similar explanation may perhaps be given of some of the peculiarities which are manifested by benzenoid compounds.

I would even venture to suggest that in electrolysing solutions the friction arising from the attraction of the ions for each other is perhaps diminished, not by the mere mechanical interposition of the *neutral* molecules of the solvent—in the manner suggested by Kohlrausch—but by the actual attraction exercised by these molecules upon the negative ion in virtue of the affinities of the negative radicals.

One result of increased attention being paid to the investigation

[1] The name chlorhydric acid is here applied to the compound $HCl(OH_2)_x$—probably $x = 1$—which, according to Thomsen, is present in an aqueous solution of hydrogen chloride. It would be an advantage if we ceased to speak of HF, HCl, HBr, HI, as acids, and always termed them hydrogen fluoride, chloride, bromide and iodide respectively. The names hydric chloride, bromide. &c., might with equal advantage be altogether abandoned; hydrochloric acid is objectionable, as suggesting a relation to chloric acid. The names fluor-, chlor-, brom-, and iodhydric, as applied to the *acids* present in aqueous solutions of the hydrides, are especially appropriate as indicating that they are compounds containing the radical water—that they are hydrates: indeed, it would be well to restrict the use of hydric and hydro-to bodies of this kind, and to speak of hydrides as hydri-, not as hydro-, derivatives. It would then be possible to give comparatively simple names even to complex hydrates.

[2] Thomsen gives the values in heat units as—

$$HGCl_2,2KClAq = 1380$$
$$HgBr_2,2KBrAq = 1640$$
$$HgI_2,2KIAq = 3450$$
$$HgCy_2,2KCyAq = 8830$$

of problems such as I have indicated will probably be that we shall be called upon to abandon some even of our most cherished notions. I would suggest, for example, that it may become necessary to regard nitrogen peroxide not as a mixed anhydride of nitrous and nitric acids, but as a compound of two NO_2 groups; its conversion into nitrite and nitrate affords no proof of its constitution, as chlorine peroxide, ClO_2, which exhibits no tendency whatever to combine with itself, also yields both chlorite and chlorate. A greater shock may result from a conviction arising that not only carbon dioxide, but sulphur dioxide, and perhaps even sulphur trioxide, dissolve in water, forming *hydrates*—$SO_2 \cdot OH_2$, $SO_3 \cdot OH_2$—not *hydroxides*. In recent times, in discussing questions of this kind, we have perhaps often been led to attach too much importance to the argument from analogy; it is not improbable that, especially in the case of compounds other than those of carbon, chemical change involves change in structure more frequently than we are apt to believe.

It is possible that a precise estimate of what, for want of a better name, I have spoken of as residual affinity, may sooner or later be obtained, if the view Prof. Lodge has propounded in his paper "On the Seat of the Electromotive Forces in a Voltaic Cell" be correct, that the cause of the volta effect is the *tendency to chemical action* between the bodies in contact; that, for example, chemical strain at the air-contacts is the real cause of the apparent contact-force at the junction of two metals in air. Prof. Lodge, if I understand his argument, appears to assume that the air effects are in some way dependent on the presence of "dissociated oxygen atoms." I think this is probably an entirely unnecessary assumption: of late years, no doubt, it has been the fashion to attribute the occurrence of changes of various kinds to the presence of products of dissociation, but probably to a very unnecessary extent. Recent investigations to which I have alluded show that there are other factors of extreme importance: for example, that water must be present in order to render a mixture of carbonic oxide and oxygen explosive. Again, the observations of V. Meyer and Langer have shown that, whereas chlorine *violently attacks* platinum at low temperatures, it is *without action* upon it at temperatures between about 300° and 1300°, but then *again begins to act* upon it, the action becoming violent at 1600° to 1700°. I have little doubt that the action at low temperatures is dependent upon the presence of moisture; if it were due to dissociated chlorine atoms, the action should increase with rise of temperature without break. In short, I see no reason to assume that oxygen at ordinary temperatures consists of other than diatomic molecules.[1] Assuming Prof. Lodge's view to be correct, the strain exists in virtue of the attraction which the oxygen molecules exert upon the metal molecules. On this assumption I can well understand that the method of calculation followed by Prof. Lodge will not uniformly lead to satisfactory results. The "heat of combination" is not necessarily a measure of "affinity." The values are in all cases algebraic sums of a series of values, scarcely one of which is known, and, as I have already pointed out, the affinities of the molecules are by no means always of the same order as the affinities of the constituent atoms; for example, in all probability, oxygen stuff has a higher absolute affinity than sulphur stuff; chlorine stuff a higher absolute affinity than iodine stuff; yet iodine and sulphur compounds, more often than not, seem to exhibit more residual affinity than chlorine and oxygen compounds. So that, from Prof. Lodge's point of view, chlorine would have the higher and iodine the lower contact values; whereas from my point of view the reverse might often be the case. I point this out because it appears to me that we here have an opportunity of testing the question experimentally, and seeing that it is possible practically to prevent chlorine from attacking metals by excluding moisture, I do not take the hopeless view that Prof. Lodge and others seem to hold regarding the possibility of settling the important question of pure contact *versus* chemical action by appeal to experiment. I may also point out that according to my hypothesis it is possible that the metals may exert a considerable attraction for each other, especially those having monatomic molecules:[2] many alloys are undoubtedly compounds; possibly not a few are compounds of the "molecular aggregate" class.[1]

To return now for but a few moments to the subject of chemical change and its intimate connection with electrical phenomena. One application I would make of the views here put forward would be to explain the superior activity of bodies in the *nascent state*, and in particular of nascent hydrogen. Briefly stated, I believe it to consist in the fact that nascent hydrogen is hydrogen in circuit—hydrogen in electrical contact with the substance to be acted upon. The experiments of Faraday and of Grove afford the clearest evidence that in order to bring about action between hydrogen and oxygen at ordinary temperatures it is merely necessary to make them elements in a voltaic circuit. The difference in the effects produced by "nascent hydrogen" from different sources is, I imagine, attributable to the variations in E.M.F., which necessarily attend variations in the constituent elements of the circuit.

It is not so easy, however, as yet to explain some of the changes which take place at high temperatures. Mr. Dixon's experiments have proved that a mixture of carbonic oxide and oxygen is non-explosive, but that explosion takes place if moisture be present, the velocity of the explosive wave depending upon the amount of water present. When the mixture of the two gases is "sparked," change takes place, but only in the path of the discharge. Mr. Dixon considers "that the carbonic oxide becomes oxidised at the expense of the water, the hydrogen *set free* then becoming reoxidised. M. Traube, who in a series of papers has called attention to the importance of water in promoting oxidation, has suggested that the oxygen and carbonic oxide together act on the water, forming hydrogen peroxide and carbonic acid: $CO + 2OH_2 + O_2 = CO(OH)_2 + H_2O_2$; and that the peroxide then reacts with carbonic oxide to form carbonic acid: $CO + O_2H_2 = CO(OH)_2$. The carbonic acid, of course, is resolved into carbon dioxide and water (*Berichte*, 1885, p. 1890). Traube actually shows that traces of hydrogen peroxide are formed during the combustion. It appears to me that the water may exercise the same kind of action as it (or rather dilute sulphuric acid) exercises in a Grove's gas battery, and that its hydrogen does not become free in any ordinary sense. The production of hydrogen peroxide is not improbably due to a secondary simultaneous change.

Unlike a mixture of carbonic oxide and oxygen, a mixture of hydrogen and oxygen is violently explosive. If we assume that in both cases the reacting molecules are electrolysed by the very high E.M.F. employed, and that the atoms then combine, it is difficult to explain the difference in the results. Does it arise from the fact that hydrogen is an altogether peculiar element? Or are we to attribute it to an influence which water itself exercises upon the formation of water from hydrogen and oxygen—as in the Grove gas battery? It is noteworthy that the velocity of the explosive wave in electrolytic gas, according to Berthelot and Vielle, is a close approximation to the mean velocity of translation of the molecules in the gaseous products of combustion calculated from the formula of Clausius (H. B. Dixon, *Phil. Trans.*, 1884, p. 636). And this is also true of mixtures of carbonic oxide and oxygen, and of nitrous oxide and oxygen with hydrogen. May we therefore assume, as the velocity corresponds with that of the products, that the water exercises the important office of inducing change throughout the mass, and not that the hydrogen is peculiar? I am tempted here to suggest that perhaps the "induction" observed by Bunsen and Roscoe in a mixture of chlorine and hydrogen is due to the occurrence of a change in which a something is produced which then promotes reaction between the two gases. I here assume that there would he no action between the pure gases.

If I have allowed myself to flounder in among these difficult questions, it is not because I feel that I am justified in speaking

[1] This conclusion would also lead me to disbelieve entirely in the explanation which Clausius has given of electrolysis.

[2] Assuming that the heat absorbed in raising the temperature of a solid is mainly expended in overcoming intermolecular attraction, the high "atomic heat" of metals may be regarded as evidence that their molecules powerfully attract each other, and hence that their molecular composition is relatively simple; and on this view the "atomic heat" of carbon and of a number of other non-metals and of some metalloids is low owing to the extent to which the "affinity" of the atoms is, as it were, exhausted in the formation of their molecules. Comparison of the "molecular heats" of chlorides and similar compounds with those of the oxides lends much support to this view, as we have reason to believe that the chlorides—which have high "molecular heats"—are of relatively simple molecular composition, and that the oxides—which have low "molecular heats"—are of relatively complex molecular composition.

[1] The study of alloys from this point of view will probably furnish interesting results. It is noteworthy that the contact difference of potential of brass is less than that of copper, and much less than that of zinc, with the same solution, in all the cases quoted by Ayrton and Perry; thus—

	Zinc	Copper	Brass
Alum	−·536 volt. ...	−·127 ...	−·014
Sea salt	−·565 ,, ...	−·475 ...	−·435
Sal ammoniac ...	−·637 ,, ...	−·596 ...	−·348

It is especially important to examine the copper-tin alloys, which vary in electrical conductivity in so remarkable a manner.

with authority, but in the hope that I may be the "fool," and that the "angels" who are well able to discuss them will be led to do so without delay: for chemists are anxiously awaiting guidance on matters such as I have referred to.

Attention must, however, be directed to the study of electrical phenomena by the recent publications of Arrhenius and of Ostwald (*Journal für praktische Chemie*," 1884, 30, 93, 225; 1885, 31, 219, 433), and especially by the statement put forward by the latter that the rate of change under the influence of acids (in hydrolytic changes) is strictly proportional to the electrical conductivities of the acids. There cannot be a doubt that these investigations are of the very highest importance.

I trust that in the discussions which we are to have on molecular weights of liquids and solids, and on electrolysis, there may be a free exchange of opinion on some of the points here raised. My reason for selecting these subjects for discussion in this Section will have been made sufficiently clear, I imagine. Last year in the Physical Section the idea assumed shape which had long been latent in the minds of many members of the Association, that it is unadvisable, as a rule, to encourage the reading of abstract papers, which rarely are, or can be, discussed. Two important discussions were *introduced* by Profs. Lodge and Schuster. We must all cordially agree with Prof. Lodge's remarks on the importance of discussing subjects of general interest at these meetings. It appears to me, however, that even a more important work may often be accomplished if the discussion consist of a series of papers which together form a monograph of the subject. I have endeavoured to carry this idea into practice on the present occasion, and a number of friends have most kindly consented to assist. Unexpected difficulties have arisen, and probably we shall none o us succeed in doing all we might wish. I trust, however, that the Section will approve of this first attempt sufficiently to justify my successors in this chair in adopting a similar course.

I much regret that it is impossible for me to attempt any review of recent work in chemistry. Not a few really important discoveries might be chronicled, and the patient industry of many who have toiled long to win results apparently insignificant should have been mentioned with high approval. A few remarks I will crave permission for, as regarding the general character of the work being done by chemists, and regarding that which has to be done.

Complaints are not unfrequently made in this country that a large proportion of the published work is of little value, and that chemists are devoting themselves too exclusively to the study of carbon compounds, and especially of synthetical chemistry. We are told that investigation is running too much in a few grooves, and it is said that we are gross worshippers of formulæ. Most of these outbursts are attributable to that pardonable selfishness which consists in assigning a higher value to the particular class of work with which one happens to be engaged or interested in than to any other line of investigation; too frequently they result from want of sympathy with, if not absolute ignorance of, the scope and character of the work complained of. It must not be forgotten that chemical investigation, like other investigation, is to a large extent the work of genius; the rank and file must necessarily follow in the order of their abilities and opportunities; hence it is that we work in grooves. The attention paid to the study of carbon compounds may be more than justified both by reference to the results obtained and to the nature of the work before us: the inorganic kingdom refuses any longer to yield up her secrets—new elements—except after severe compulsion; the organic kingdom, both animal and vegetable, stands ever ready before us; little wonder, then, if problems directly bearing upon life prove the more attractive to the living. The physiologist complains that probably 95 per cent. of the solid matters of living structures are pure unknowns to us, and that the fundamental chemical changes which occur during life are entirely enshrouded in mystery. It is in order that this may no longer be the case that the study of carbon compounds is being so vigorously prosecuted: our weapons—the knowledge of synthetical processes and of chemical function—are now rapidly being sharpened, but we are yet far from ready for the attack. As to the value of this work, I believe that every fact honestly recorded is of value; an infinite number of examples might be quoted to prove this. No unprejudiced reader can but be struck also with the improvement in quality which is manifest in the majority of the investigations now published; at no time was more attention given to the discovery of all the products of the reactions studied, and to the determination of the influence of changes in the conditions. As regards our formulæ, those who look upon the outward visible form without proper knowledge of the facts symbolised, and who take no pains to appreciate the spirit in which they are conceived, are undoubtedly misled by them. The great outcome of the labours of carbon-chemists has been, however, the establishment of the doctrine of structure;[1] that doctrine has received the most powerful support from the investigation of physical properties, and it may almost, without exaggeration, be said to have been rendered visible in Abney and Festing's infra-red spectrum photographs. Some of us look forward to the extension of the doctrine of structure not only to compounds generally, but even to the "elements." The relationships between these are in so many cases so exactly similar to those which obtain between carbon compounds, which we are persuaded differ merely in structure, that it is almost impossible to avoid such a conclusion, even in the absence of all laboratory evidence.[2]

As the field of view opens out before us, so does the vastness of the work to be accomplished become more and more apparent; and Faraday's words of 1834 may be quoted as even more appropriate than half a century ago.

"Indeed, it is the great beauty of our science, Chemistry, that advancement in it, whether in a degree great or small, instead of exhausting the subjects of research, opens the door to further and more abundant knowledge, overflowing with beauty and utility, to those who will be at the easy personal pains of undertaking its experimental investigation.

SECTION C

GEOLOGY

Opening Address by Prof. J. W. Judd, F.R.S., Sec. G.S., President of the Section[3]

Concerning the overlying formation of quartzites and limestones, much yet remains to be made out. Nicol, Lapworth, and the officers of the Geological Survey, have shown it to be made up of three principal members—the identity of which cannot be mistaken although different names have been assigned to them. While Nicol estimated the total thickness of this formation at from 300 to 800 feet, however, and Lapworth places it at the smaller of these amounts, the officers of the Survey believe it to be no less than 2,000 feet thick.

Even greater uncertainty still exists as to the exact geological age of this important formation. Murchison, who in his later years made "Silurian" a mere synonym for Lower Palæozoic, was no doubt right in regarding these rocks as being of that age. I have no intention of attempting to flog that dead horse—the controversy concerning the names which should be applied to the great systems containing the three faunas which Barrande so well showed to be present in the Lower Palæozoic rocks. That controversy, commencing, it must be confessed, with some tragic elements, has long since passed into the sphere of comedy, and now bids fair, if still persisted in, to degenerate into farce. Little, if anything, has been added to the work of Salter in connection with these fossils of the Durness limestone. With their abundance of that remarkable and aberrant mollusc, *Maclurea*, they can be paralleled with no other British or even European deposit, unless it be the Stinchar limestone of the Girvan district. Salter thought that this remarkable Scotch formation had its nearest analogues in the Calciferous sandstone and the Chazy limestone of North America. As those rocks contain "Primordial" forms of Trilobites, they must probably be regarded as either of Cambrian age, or as constituting a link between the rocks containing Barrande's first and second faunas respectively. Under these circumstances, it is a piece of welcome intelligence that the officers of the Geological Survey have succeeded in obtaining a rich and varied collection of organic remains from the beds of Sutherland; and the results of the examination and discussion of these fossils will be awaited by all geologists with the greatest interest.

Whether, as in the case of Scandinavia, other fossiliferous

[1] I venture here to direct attention to an extension of the acknowledged theory of structure suggested (by myself, I may say) at the close of the discussion of the van 't Hoff-Le Bel hypothesis of isomerism in Miller's "Chemistry," vol. iii., 1880 edition, p. 993. The same view was soon afterwards independently put forward by Dr. Perkin.

[2] F. Exner, in a recent paper (*Monatshefte für Chemie*, 1885, p. 249), "On a New Method of Determining the Size of Molecules," actually puts forward an hypothesis as to the structure of elements.

[3] Continued from p. 458.

deposits of Silurian age will be found to be represented in a highly metamorphosed condition in our Scottish Highlands, remains to be discovered. There is such a perfect parallelism between the several members of the Silurian in Scania and in the Scottish Borderland, so well shown by the researches of Linnarson and Lapworth, that, as Nicol always anticipated, we may not improbably find a portion of the rocks of the Highlands to be altered forms of those of the Borderland.

Since the last meeting of the British Association in the Highlands, much progress has been made in the study of that preeminently British formation—the Old Red Sandstone. Dr. Archibald Geikie has thrown much new light, by his valuable researches, on the relations of the several members of the vast series of deposits which go by that name; while Dr. Traquair, bringing to bear on the subject great anatomical knowledge, has re-examined the collections of fossil-fish made by that indefatigable explorer, Hugh Miller. The Old Red Sandstone is the only great system of strata which we possess, while it is either wholly absent, or very imperfectly represented, in Scandinavia.

In the year 1876, I was able to announce that a vestige—a small but highly interesting vestige—of the great Carboniferous system exists within the limits of the Scottish Highlands. Well do I recall the deep, the ineffaceable impression made upon my mind when, standing at the Innimore of Ardtornish, I beheld for the first time this relic of a great formation, preserved by such a wonderful series of accidents. What the inscribed stone of Rosetta or the papyri of Herculaneum are to the archæologist, this little patch of sandstone is to the geologist. Overwhelmed by successive lava-streams that were piled upon one another to the depth of many hundreds of feet, and then carried down by a fault which buried it at least two thousand feet in the bowels of the earth, this fragment has remained while every other trace of the formation has been swept from the Highlands by the besom of denudation.

Highly interesting and important in these northern areas are the Mesozoic deposits, which in places attain a vertical thickness of several miles, and which must have originally covered enormous tracts of country. Now, judged by that very fallacious test, the space which they cover upon our geological maps, they appear in the Scottish Highlands to be absolutely insignificant.

The correspondence in characters between the several Secondary formations on the two sides of the North Sea is of a most striking kind. I have had the good fortune to study the Secondary rocks of Scania under the guidance and with the assistance of Professor Lundgren, of the University of Lund, who has made so many important discoveries in connection with them. While doing so, I have again and again felt almost constrained to pause and rub my eyes, to convince myself that I was not back again in Scotland—so complete is the correspondence between the mineral characters, the fossils, and the geognostic relations of these strata in the two areas.

The Triassic rocks of Scandinavia, consisting of variegated sandstones and conglomerates, containing much calcareous material, are absolutely undistinguishable from those of the Western Highlands. In both countries the thickness of the deposits of this age varies within very short distances, their development being local and inconstant. The formation which in places exceeds a thousand feet in thickness, at other points is reduced to an insignificant band of conglomerate. On the eastern flank of our Highlands, yellow sandstones belonging to this formation have yielded to Mr. Duff, Dr. Gordon, Mr. Grant, and others that interesting series of reptilian remains which, in the hands of Professor Huxley, have been made to throw such important light on the forms of life which existed at that remote geological period. In the very similar deposits which occur in Scandinavia, however, reptilian remains have not as yet been obtained. The abundance and variety in form and size of the footprints which occur in our Scottish rocks of this age indicate the richness of the vertebrate fauna which must have existed at that distant epoch.

On both sides of the North Sea, the Triassic rocks are found passing up insensibly into the great formation known as the Rhætic and Infralias—a formation imperfectly represented in England and Central Europe by a few thin and insignificant strata, but in our Highland districts attaining a vast thickness and exhibiting a magnificent development. This system of strata consists of alternation of marine and estuarine deposits, the latter containing in both areas thin seams of coal. In Scania, the working of the coal and fire-clays of these deposits has brought to light vast numbers of fossil plants, which have been so well described by Nathorst. Several very distinct floras, occurring at different horizons, have been made out, and the relations of the beds containing these floras to one another, and to the marine strata with which they are intercalated, have been clearly demonstrated by the researches of Hébert, Erdmann, and Lundgren. That similar rich stores of fossil plants would reward a search as skilful and persevering as that made by our Scandinavian brethren, if carried on in the equivalent strata of Scotland, there can be little doubt.

The whole of the vast Jurassic system in these northern latitudes, attaining a thickness of 3,000 or 4,000 feet, appears to be similarly made up of alternations of marine and estuarine strata. Time would fail me to indicate even in the briefest manner the numerous problems of the highest interest suggested by the study of these vast deposits. At many different horizons, beds of coal and the relics of a rich terrestrial vegetation abound. Most of these await careful study and description. So far as they are yet known, the Ferns, the Cycads, and the Conifers of the Jurassic rocks of the Highlands present wonderful resemblances with those described by Heer from strata of the same age in Norway, in Russia, in Siberia, and even far away in the Arctic regions. The marine forms occurring in the associated strata seem to indicate that they belong to an ancient life-province, distinct from those in which the Jurassic rocks of Central and of Southern Europe were deposited. In the Upper Jurassic, so well represented in Sutherland by strata not less than 1,000 feet in thickness, we find evidence of the existence of mighty rivers, the banks of which, though clothed with tree-ferns, Cycads, and gigantic pines, yet at certain seasons must have borne down ice-buoyed blocks of vast dimensions.

That the succeeding Neocomian period was for Scandinavia and Scotland an epoch of elevation and of the prevalence of terrestrial conditions is indicated by the total absence of any trace of marine deposits of this age, no less than by the enormous denudation which can be shown to have followed the Jurassic and preceded the Cretaceous period. Our now ruined mountain-chain then probably formed the lofty watershed of a great continent, through which flowed the mighty rivers that formed the deltas known as the English and German Wealdens.

How powerful and prolonged were the agencies of sub-aërial waste during this period is shown by the fact that the relics of the Cretaceous formation are found resting in turn on every member of the Jurassic, the Rhætic, the Trias, and all the different Palæozoic and Archæan rocks. A great portion, indeed, of the thick and widespread Rhætic and Jurassic strata seems to have been removed by denudation before the commencement of the Cretaceous period.

That thick strata of chalk once covered large areas of the Scottish Highlands and of Scandinavia we have the clearest proofs. In Scania and the adjoining parts of Denmark deposits of this age are found let down by tremendous faults, and these include even younger members of the series than are anywhere found in England. In the West of Scotland I have shown that thin deposits of Cretaceous age, preserved to us by a wonderful series of accidents, still survive the tremendous denudation of the Tertiary periods. It is true that in Scandinavia and Scotland alike, the chalk alternates with sandstones and even with strata of estuarine origin, but the pure foraminiferal rock that occurs in both areas could have been formed in no very shallow sea. That before the commencement of the great Tertiary denudation large areas, in Scandinavia and Scotland alike, must have been swathed in winding sheets of chalky rock there cannot be the smallest doubt. That considerable portions of these winding-sheets remained to so late a period as the glacial is shown by the fact that the indestructible flints of the chalk with the rocks and fossils of the upper greensand abound in your boulder-clays of Aberdeenshire and Banffshire.

Of the vast periods of the Tertiary we have left to us, either in the Highlands or Scandinavia, but few and insignificant relics in the form of stratified deposits. In our beautiful Western Isles and in Antrim the lava poured out in successive streams, during enormous periods of time, from the lofty volcanic cones of the earlier Tertiary epoch, has here and there buried patches of lake-mud, or river-gravel, or ancient soils. But everywhere, alike in the Highlands and in Scandinavia, we behold the most impressive evidences of the sub-aërial waste, and of the elevation that promoted this waste during the Tertiary epoch. Among such evidences we may reckon the circumstance that all traces of the vast deposits of the Secondary periods have been relentlessly stripped away from the country, except where buried deeply by gigantic earth-throes, or sealed up under massive lava-streams.

Down to post-glacial times Scotland, and what are now its outlying islands, remained united with Scandinavia. I need not remind you how, during the glacial period, they were the scene of a similar succession of events; while from their then far more elevated mountain summits streams of glacier-ice flowed down and relieved the mantle of snow which enveloped them.

But at a very recent geological period, and indeed since the appearance of man in this part of our globe, the separation of the two areas, so long united, was brought about. In the district now constituting the North Sea, which separates the two countries, great faults, originating in the Tertiary epoch, appear to have let down wide tracts of the softer Secondary strata among the harder crystalline rock-masses. The numerous changes of level, of which we find such abundant evidence around the shores of this sea, facilitated the wearing away of the whole of these softer Secondary deposits, except the slight fringes that remain along the shores of Sutherland, Ross, and Cromarty, on the one hand, and the isolated patches forming Scania, Jutland, and the surrounding islands on the other. Little could the Vikings, as they sailed over this shallow sea, have imagined that their predecessors in these regions were able to roam on foot from Norroway to Suderey!

It is almost impossible to over-estimate the effects produced by the several denudations to which Scandinavia and the Scottish Highlands have been successively subjected. In that which occurred during the later Tertiary periods, almost every portion of the non-crystalline rocks that rose above the sea-level was either entirely removed or converted into level plains, which, covered with drift deposits, now form districts like Scania and Denmark. Where, as in the great central valley of Scotland, hard volcanic masses are associated with the softer sedimentary rocks, the former are left rising as picturesque crags, standing boldly up above the general level, while the latter are worn down and buried under drift. In the west of Scotland a chain of volcanic mountains, with summits towering to the height of from ten to fifteen thousand feet, have been reduced by this same denudation to basal-wrecks, the highest portions of which attain to but little more than 3,000 feet above the sea-level!

During the grea elevation and denudation which marked the Neocomian period, thousands of feet of strata must have been removed over wide areas, as is proved by the wonderful overlap of the Cretaceous beds on all the older strata.

Of the enormous sub-aërial waste which went on in these Northern Alps during the Newer Palæozoic periods we have impressive evidence in the vast masses of the Old Red Sandstone and Carboniferous rocks—themselves only a series of fragments that have survived the later denudations—for these rocks are built up of the materials derived from our Northern Alps.

The Torridon Sandstone is the monument, and a very striking monument too, of another and still earlier period of enormous denudation. The thousands of feet of conglomerate and sandstone of which it is made up consist of the disintegrated crystals of granites and gneisses that have been swept away.

When we penetrate towards the axis of this eroded mountain chain, the proofs of the magnitude of these denudations become even more striking and impressive. Here we see, towering aloft, the ruined buttresses of vast rocky arches, that when complete must have risen miles above the present surface; there we find, lying side by side, rock-masses that could only have been brought together by displacements of tens of thousands of feet; yet so complete has been the planing down of the surface since, that it requires the most careful study even to detect the almost obliterated traces of these grand movements. The Alps and the Himalayas, during their elevation, have suffered enormous waste and denudation; but if the elevation were to cease and the waste to go on till these magnificent mountain-chains were reduced to masses of diminutive peaks, ranging from 2,000 to 8,000 feet in height, we should then have the counterpart of this stupendous ruin of the mountain-chain of the north.

The history of the series of successive movements to which the rock-masses of our Highlands have been subjected is one well worthy of the most attentive study. When the evidence bearing upon the subject is carefully sifted and weighed, we become convinced of the fact that many of these movements—including some on a prodigious scale—must have taken place during what we are commonly accustomed to regard as comparatively recent geological periods.

On the eastern coast of Sutherland, a mass of Secondary rocks, including several thousands of feet of Triassic, Rhætic, and Jurassic strata, has been let down by a gigantic fault, so as to be placed in juxtaposition with the Old Red Sandstone and the crystalline rocks. Now, taking the very lowest estimates of the thicknesses of the several strata affected, the vertical "throw" of this fault must have exceeded a mile! It may not improbably, indeed, have been at least double or treble that amount! Yet this great dislocation was certainly produced at a later date than the Upper-Jurassic period, for rocks of that age are found to be affected by it.

Along the coasts of the Black Isle, strata of Middle and Upper Jurassic age are similarly found faulted against the "Old Red" and the crystalline rocks.

On the other side of the North Sea, in Andö, one of the Lofoten Isles, a patch of Lower-Oolite strata, consisting of marine and estuarine strata, and including beds of coal like that of Brora, is found let down by gigantic faults into the very heart of the crystalline rocks of the district. In Scania, the whole of the Secondary rock-masses owe their preservation in the same way to a plexus of tremendous faults, by which they have been entangled among the harder rocks. These faults have affected not only the Jurassic strata, but even the very youngest members of the Cretaceous series.

Nor are we without evidence that some of the great faults are of post-Cretaceous age, in this country, for in the Western Highlands displacements of several thousands of feet have been detected, which affect not only the Upper Cretaceous, but also the Older Tertiary rocks.

The effects produced by these great dislocations, which have a generally parallel direction in our Highlands, from north-east to south-west, are of the most startling character. Great strips of Triassic and Old Red Sandstone strata, like those of Elgin, and Turriff, and Tomintoul, and of the line of the Caledonian Canal, are found let down among the crystalline rocks by these gigantic faults.

The great central valley of Scotland itself consists of masses of Newer Palæozoic strata, faulted down between the harder Archæan and Lower Palæozoic rocks, which form the Highlands on the one hand, and the Borderland on the other.

The evidences of the existence of these great faults were collected by many of the older Scottish geologists, like Landale, Bald, Chalmers, Milne-Home, and Nicol; and the accurate mapping of the country by the officers of the Geological Survey has, on the whole, tended to confirm their results. With regard to the age of these great dislocations of Central Scotland, it can only be *certainly* affirmed that they are of more recent date than the youngest Carboniferous strata; but I have long believed that, like many similar dislocations both in our own Highlands and in Scandinavia, they are really post-Cretaceous.

Less difficulty perhaps will be found in accepting this apparently startling conclusion, when we remember that a complicated series of fractures injected by the lavas of the Great Tertiary volcanic foci of the West, extend right across the Highlands, the central valley, and the Borderlands of Scotland, and even traverse the whole series of the Secondary rocks in the North of England.

The indications of the tremendous manifestations of subterranean energy, to which these great dislocations owe their origin, are sometimes of a very striking kind. For hundreds of yards on either side of the faults, the two sets of strata are found bent and crumpled, and not unfrequently crushed into the finest dust ("fault-rock"). In the case of the great Sutherland-fault, to which I have previously alluded, we have a beautiful illustration of the way in which mineral veins may originate along such lines of fissure, for in the interstices of the granite of the Ord, where it has been broken up along this certainly post-Jurassic, and probably Tertiary fault, fluor-spar and pyrites have been deposited in large quantities.

It is impossible to study the tremendous movements and dislocations, and the enormous amount of denudation which have taken place in the Highlands and surrounding districts during Tertiary times, without being convinced that all the existing surface-features of the country must date from a comparatively recent period. The vast movements which have placed soft and hard masses in opposition along certain parallel lines—generally ranging in a north-east and south-west direction—and the denudation which has worn away the former, while it has left the latter standing in relief, must, I believe, both be referred to the Tertiary period; though the disposition of rock-masses brought about by earlier movements would of course exercise a certain though subordinate influence in producing the existing forms of the surface of the country.

At the close of the Jurassic period, and before the commencement of the Cretaceous, during the vast epoch marked by the deposition of the Neocomian of Southern Europe, a series of disturbances similar to those of the Tertiary, and scarcely inferior in their consequences, can be shown to have taken place.

If the movements of the Scandinavian and Scottish rock-masses which took place in the Tertiary and Mesozoic periods respectively were so startling in their magnitude and so vast in their effects, what shall we say concerning those far greater disturbances which affected the same area towards the close of the Older Palæozoic and the beginning of the Newer Palæozoic, when this Northern Alps was still a living and growing mountain-chain?

These movements, in which both the Archæan and the Older-Palæozoic rocks are found to be involved, have resulted in the production, through enormous lateral pressure, of those reversed faults, caused by the disruption along their axial planes of greatly inclined and compressed folds, as so well described by Rogers.

Dr. Archibald Geikie assures us that the studies of the geological surveyors in North-West Sutherland led to the conclusion that certain masses of rock have thus been carried almost horizontally over others, along these "thrust-planes" for a distance of at least ten miles. As the result of these tremendous lateral compressions, thin beds of limestone and quartzite, which have sufficiently definite characters to permit of their recognition, may be seen in Assynt, and in other parts of the Western Highlands, to be so repeated again and again by crumpling and faulting, that they have been regarded as deposits of enormous thickness; while, on the other hand, massive formations have been crushed and rolled out, thereby acquiring a laminated structure like so much pie-crust. Great portions of rock-masses, which, like the much-discussed "Logan-rock," have been nipped between gigantic faults, show evidence under the microscope of having been crushed to powder and subsequently reconsolidated, while the surfaces of the "thrust-planes" sometimes exhibit the phenomena known as "slickensides" on the most gigantic scale.

As we pass away from the central axis of this old mountain-chain, however, these complicated puckerings and dislocations pass gradually into more ordinary folds and faults, just as is the case with the Appalachians. The oft-repeated undulations of the Lower Palæozoic strata of the Borderland, so admirably described by Professor Lapworth, bear the same relation to the far more involved disturbances of rocks of the same age in the Highlands, which the foldings of the strata in the Jura do to the intense crumplings of those of the Alps; and these in turn pass insensibly into the slightly undulating or horizontal strata of the southern half of this island.

We may perhaps add another comparison between the existing mountain-chain of Southern Europe and the "basal wreck" of Northern Europe, one which I find has been already suggested by Professor Bonney. The Miocene Conglomerates, which in the Rigi and other flanking mountain masses of the Alpine chain are found piled to the depth of many thousands of feet, seem to be exactly represented in its prototype by the vast masses of the "Old-Red" Conglomerate.

Vast as were the three series of movements to which I have been referring, I believe that the Scandinavian and Highland rocks bear the impress of a still grander series of di turbances than either of these—one at the same time of older date and far more universal in its effects.

Many writers have treated of the great divisional planes, almost everywhere conspicuous in the Highland rock-masses, as being necessarily coincident with planes of sedimentation. It is manifest, indeed, that the tracing of sequences and unconformities among such rocks must proceed upon the assumption that the planes of foliation and stratification *are* coincident. Murchison and Geikie so fully recognised the fact that this proposition lay at the very root of their arguments concerning a Highland succession, that they added a supplement to their paper to illustrate and enforce it.

It must not be forgotten, however, that the truth of this proposition has not only been doubted, but has been stoutly contested by many of the most profound thinkers on geological questions.

As long ago as 1822, Professor Henslow, in a very remarkable paper, showed that the rocks of Anglesea are traversed by a system of divisional planes, which intersect the bedding at a very high angle, and must have been produced long subsequently to the latter; and in 1835 Professor Sedgwick extended the observations and enforced the arguments of Henslow.

At an even earlier date, Poulett Scrope had shown, by his study of viscous lavas, that the planes along which crystalline action takes place are determined by pressure and strain; and he insisted that the foliation of metamorphic masses was a phenomenon strictly analogous to the banding of rhyolitic lavas.

Charles Darwin, the pupil of Henslow and the friend of Poulett Scrope—whose labours in the geological field would perhaps have met with fuller recognition had they not been overshadowed by his still greater achievements in the world of biological thought—strongly maintained the truth of these views. He added the important observation that, in the South American continent, the planes of foliation are seen everywhere, over enormous areas, to be parallel to those of cleavage; and that these latter are of secondary origin and due to lateral pressure, the observations of Sharpe and the experiments of Sorby have convincingly demonstrated.

That the schists and gneisses of our Highlands and of Scandinavia have resulted from crystallising forces, acting upon strata of sandstone, clay, and limestone, or upon igneous materials constituting lava-currents, or intrusive sheets, dykes, and bosses, I see every reason for believing. That these re-crystallised and highly-foliated masses in the great majority of cases maintain their original positions and relations, or indeed anything approaching their original positions and relations, I greatly doubt; and my doubt on this point has increased the more I have studied the Highland rocks.

Thin bands of quartzite may be the rolled-out representatives of massive beds of sandstone or conglomerate; wide-spreading schists may consist of the crystallised materials of clays and shales, crumpled, pleated, and kneaded together in endless convolutions; vast sheets of gneiss may have originally been intrusive bosses of granite or thick strata of arkose. How, then, are we to apply the ordinary principles that regulate questions concerning dip and strike, and unconformity in the case of sedimentary deposits, to highly altered rocks like these?

The observations of Jukes, Allport, and Phillips on some of the simpler and more easily explicable examples of the production of foliation in rocks require to be cautiously extended, by patient study in the field and in the laboratory, to cases of a more complex and difficult character. Especially in this connection do we welcome such contributions to our knowledge as that made by Mr. Teall in his description of the remarkable foliated dyke of Scourie.

Very significant indeed is the fact that the phenomena of foliation appears to be confined to regions which have been the scene of the most violent subterranean movement and disturbance. That solid rock-masses, subjected to the tremendous earth-strains to which they are liable during mountain-making, are capable of internal movement and flow—like the ice of a glacier—we have the clearest evidence. Many illustrations might be adduced in support of the view that crystallisation is influenced and controlled by mechanical forces—pressures, stresses, and strains. May it not also be true, as long ago suggested by Vose, that the heat which must be generated in the great shearing movements taking place in rocks have also had much to do in giving rise to that re-crystallisation which is the essence of foliation? Rock-masses, in the throes of mountain-birth, have, like glaciers, behaved substantially as viscous bodies; may not the former have undergone molecular changes analogous to regelation in the latter?

That many of the stupendous earth-movements which produced the foliation of the rocks of Scandinavia and the Scottish Highlands must be referred to Archæan times, there is not the smallest room for doubt. That similar effects have resulted from the same agencies during subsequent periods, our fellow-geologists in Scandinavia believe they have found incontrovertible proof. For my own part I look forward confidently to the establishment of the same conclusion from the study of our own Highland rocks.

But here I am conscious that I am venturing on topics upon which great and allowable differences of opinion still exist. The debates in this Geological Section during the first meeting of the British Association in Aberdeen ought, I think, to have marked the practical close of one great series of controversies. The discussions of the present meeting will, I trust, result in the recognition and clear statement of a number of other equally important problems of Highland geology which still await solution. And I am sanguine enough to hope that when this Association next gathers here, my successor in this chair will have to congratulate his audience upon a very brilliant retrospect of work actually accomplished in the interval.

I am encouraged in this optimism by the fact that in the period which has elapsed since our last meeting here, great and important improvements have been made in the methods of geological investigation. We have seen how the discovery of a few fragmentary shells in the limestone of Durness, and of sundry casts of bones in the sandstone of Elgin, have been the means of profoundly modifying our ideas concerning the age of vast tracts of rock in the Highlands. The development of modern methods of petrographical research is destined, I believe, to lead to a similar revolutionising of our views concerning the wonderful series of changes which have taken place within rock-masses, subsequently to their original accumulation.

Especially does the application of the microscope to the study of rocks, when employed in due subordination to, and illustration of, work done in the field, promise to be the source of valuable and fruitful discoveries in the field of Highland geology.

In connection with this subject I cannot refrain from reminding you that while the initiative in the application of the palæontological method of research was taken by an English land-surveyor, we are indebted to a Scotchman in an equally lowly station of life, for overcoming some of the first difficulties in connection with petrographical study. Many microscopists had employed their instruments, and sometimes with useful results, in the study of the powders and the polished surfaces of rocks; but it is to William Nicol, of Edinburgh, the inventor of the well-known polarising prism which bears his name, that we owe the discovery of the method of preparing transparent sections of fossils, crystals, and rocks, whereby their internal structure may be examined by transmitted light. Nicol bequeathed his preparations to his friend Alexander Bryson, and some of them are now preserved in the British Museum. It is interesting, too, to recall the circumstance that it was a thin section of the granite of Aberdeen in the collection of Bryson which exhibited to Sorby that wondrous assemblage of minute cavities containing liquids, and led him, shortly before our previous meeting here, to write his paper "On the Microscopical study of Crystals, indicating the origin of Minerals and Rocks"—a paper which has indeed proved epoch-making in the history of geology.

Before concluding the remarks which by your kindness I have been permitted to offer you to-day, I cannot forbear from indulging in a pleasant reminiscence of a personal character. Nearly fifteen years have passed away since I first visited the Highlands for the purpose of geological study; it was at that time I first found myself at liberty to put into practice a scheme cherished by me from boyhood, that of studying those Secondary rocks and fossils of the Highlands among which such valuable pioneer work had been done by John Macculloch, Roderick Murchison, and Hugh Miller. I had endeavoured to prepare myself for a somewhat difficult task, by a training partly unofficial and partly official—I will not employ the terms "amateur" and "professional," for of late they have been so sadly misused—but when I came a stranger among you, I could not have deserved, and I certainly did not anticipate, that cordial welcome, that kindly aid and that generous appreciation, of which I accept my position here to-day as the crowning manifestation.

While I continue to occupy myself with the glorious problems of Highland geology—and hitherto I have found that each difficulty surmounted has resulted, like the sown teeth of the slaughtered dragon, in a plentiful crop of new ones—the many acts of kindness of my numerous friends here can never cease to be present in my mind. For not only am I indebted to those who, like your own Dr. Gordon, of Birnie, and Dr. Joass, of Golspie, have been able out of stores of their knowledge to furnish me with "things new and old," and who have been unfailing in their aid and sympathy, but to those also who have pitied, but nevertheless helped, the "daft callant that speers after the chucky stanes."

I know of no higher pleasure than that which the geologist experiences in visiting regions of great scientific interest which are new to him, and of grasping the hands of fellow-workers, whose labours and teachings he has learned to admire and to appreciate. Whatever may be my lot in this way in future years, however rich the country visited may be in objects of profound instructiveness or of surpassing interest, I can anticipate or desire nothing more valuable than the lessons, or kinder than the reception which I have met with here.

> "I'll ask na mair, when I get there,
> Than just a *Hielan* welcome."

SECTION D

BIOLOGY

OPENING ADDRESS BY PROF. W. C. McINTOSH, M.D., LL.D., F.R.SS.L. & E., F.L.S., COR. M.Z.S., PRESIDENT OF THE SECTION

I HAVE selected the subject of the phosphorescence of marine animals for a few remarks on the present occasion—the theme, perhaps, being the more appropriate from its congenial local surroundings; for, like St. Andrews, Aberdeen is an

> "Old University town
> Looking out on the cold North Sea."

A phenomenon so striking as the emission of light by marine organisms could not fail to have attracted notice from very early times, both in the case of navigators and those who gave their attention in a more systematic manner to the study of nature. Accordingly we find that the literature of the subject is both varied and extensive—so much so, indeed, that it is impossible on the present occasion to give more than a very brief outline of its leading features. This is a subject of less moment, however, since the great microscopist, Ehrenberg, in his treatise, "Das Leuchten des Meeres," published by the Berlin Academy in 1835, has given a very full account of the early literature on phosphorescence, both in marine and terrestrial animals, no less than 436 authors being quoted. The limitation just mentioned is therefore sufficiently warranted.

Though it is in the warmer seas of the globe that phosphorescence is observed in its most remarkable forms—as for instance the sheets of white light caused by *Noctiluca*, and the vividly luminous bars of *Pyrosoma*—yet it is a feature which the British zoologist need not leave his native waters to see both in beauty and perfection. Many luminous animals occur between tide-marks, and even the stunted sea-weeds near the line of high water everywhere sparkle with a multitude of brilliant points. As a ship or boat passes through the calm surface of the sea in summer and autumn, the wavelets gleam with phosphorescent points, or are crested with light; while the observer, leaning over the stern, can watch the long trail of luminous water behind the ship, from the brightly sparkling and seething mass at the screw, to the faint glimmer in the distance. On the southern and western shores, again, every stroke of the oar causes a luminous eddy, and some of the smaller forms are lifted by the blade and scintillate brightly as they roll into the water. The dredge and trawl likewise produce, both in the shallower and deeper parts of our seas, many luminous types of great interest and beauty.

I shall, in the first instance, glance at the various groups of marine animals which possess the property of phosphorescence, and thereafter make some general remarks on the subject. It is found then that this feature is possessed by certain members of the Protozoa, and by the following groups of the Metozoa,—viz. Cœlenterates, Echinoderms, Worms, Rotifers, Crustaceans, Molluscoids, Mollusks, and Fishes.

About the middle of last century Baster found that at least three species of what he called microscopic animalcula ("Opuscula Subseciva," vol. i. p. 31, table 4, Fig. 1), apparently infusoria, were phosphorescent; and fully half a century later, Pfaff noticed that the luminosity of the sea at Kiel was due to certain members of the group just mentioned. Subsequently both Michaelis and Ehrenberg met with phosphorescent infusoria in the Baltic, the latter describing them as species of *Peridinium* (now *Ceratium*) and *Prorocentrum*. The same fact, associated with the absence of *Noctiluca* at Kiel, has again more recently been brought forward by Stein. In our own seas I have been especially struck with this feature in July and August, the whole surface of the sea along the eastern shores of England and Scotland teeming with *Ceratium* and *Peridinium*, besides other Infusoria, which form a greenish scum on the interior of tow-nets in inshore water, and for many miles seaward. As the waves curl from the sides of the boat in quiet water, the crest of each sparkles with multitudes of luminous points, which gleam for a moment as the ripple stretches outward, and then disappear; or, still more vividly, when the plunging vessel sends the sparkling spray all around the bow. If, on removing the tow-net from such water at night it is suddenly jerked, the whole interior is beautifully lit up with a luminous lining, which glows brightly for a few seconds and then fades. I have been unable, nevertheless, to satisfy myself as to the phosphorescence of isolated examples of *Ceratium*, and Mr. Murray (who is inclined to follow Klebs in considering them algæ), tells me that he has not been more successful.

The most conspicuous member of the first group (viz. the Protozoa), however, is *Noctiluca*, which for a long time has been associated with luminosity in many seas. The minute size of this little transparent gelatinous sphere, which ranges from $\frac{1}{8}$ to $\frac{1}{3}$ of a millimetre, probably gave origin to some of the ancient views that the phosphorescence of the sea originated from the water, and not from any visible organisms. Amongst the first who clearly made known the relationship of this minute body to the phenomenon we are examining was M. Rigaut, a French naval surgeon, who examined it off various parts of the French coasts as well as off the Antilles, and pointed out in a memoir communicated to the Academy that the luminosity of the sea was caused by an immense number of what he termed little spherical polyps, about a quarter of a line in diameter (*Journal des Savants*, tome xliii., February, 1770, pp. 554–61). The observations of this acute French surgeon were followed up by many subsequent authors, amongst whom may be mentioned Baker, Martin Slabber, Abbé Dicquemare, Suriray, Macartney, and Baird; while in more recent times Verhaege, De Quatrefages, and Giglioli have specially studied the phosphorescence of the sea caused by *Noctiluca*. The light given out by this form is occasionally spread over a large area, and is often evident along the margin of the beach, where the broad belts of Noctilucæ gleam in the broken water. It is not uncommon in summer on the southern shores of Britain, while it is rare in the northern; but it stretches into most of the great oceans, and is the cause of that diffused and silvery phosphorescence so well known to voyagers in the warmer seas. At Ostend, Verhaege found the maximum number in a given quantity of water in the warm months, few or none appearing in the winter. The observations of De Quatrefages ("Observations sur les Noctiluques," *Ann. des Sc. Nat.*, 3e Série, Zool., tom. xiv. p. 226) were made on the shores of France as well as those of Sicily, for he accompanied the distinguished Prof. Henri Milne-Edwards (whose loss science has had so recently to deplore), on his celebrated "Voyage en Sicile," and they were more extensive than those of the previous author. He attributes the emission of the clear bluish light in quiet water, or the white light with greenish or bluish touches in broken water, to any physical agent which produces contraction, the scintillations arising from the rupture and rapid contraction of the protoplasmic filaments in the interior. Thus, like Verhaege and others, he found no special luminous organ. Moreover, Ehrenberg and De Quatrefages observed that the light emitted by *Noctiluca*, though apparently uniform under a lens, was broken up into a number of minute scintillations when highly magnified. Mr. Sorby, in examining the light of this form, has been unable to obtain satisfactory spectroscopic results, apparently from its feebleness.

Besides *Noctiluca*, which was chiefly met with in inshore water, Mr. Murray, of the *Challenger*, describes various species of *Pyrocystis* (*Proc. Roy. Soc.*, vol. xxiv., p. 553, pl. xxi.; and *Narrative*, Zool., vols. i. and ii., pp. 935–38), a closely-allied form, and indeed some of which have been thought to be identical with the former. They abound in the open sea, and are the chief causes of its phosphorescence in the tropical and subtropical oceans. The light is stated to proceed from the nucleus, and in this respect diverges from that observed by De Quatrefages in *Noctiluca*. When shaken in a glass they give out, Sir Wyville Thomson observes ("Atlantic," vol. ii. p. 87), the uniform soft light of an illuminated ground-glass globe.

Dr. Giglioli, during the voyage of the Italian frigate *Magenta*, mentions (*Atti della R. Accad. delle Sc. di Torino*, vol. v., 1869-70, p. 492) that another group of the Protozoa, viz. the Radiolaria, show phosphorescent properties. In the Pacific the genera *Thalassicolla*, *Collozoum*, and *Sphærozoum* shone with an intermittent greenish light. It is possible that Dr. Baird (Loudon's *Mag. Nat. Hist.*, vol. iii. p. 312, Fig. 81, *c*, *d*), in his earlier paper, refers to the same group when describing an unknown phosphorescent pelagic organism.

No group of marine animals is more prominent in regard to phosphorescence than the Cælenterates. The Hydroida are familiar examples (even after many days and in impure water some of these retain this property, a shock to the stem sending off a crowd of luminous points from the trophosome), and, as Mr. Hincks observes, none excels the common *Obelia geniculata*, which forms pigmy forests on the broad blades of Laminariæ all around our shores. In the fresh specimen a touch during summer causes a large number of luminous points to appear on the zoophytes, the stems most irritated emitting beautiful flashes, which glitter like faintly-dotted lines of fire, the points not being harshly separated, but blending into each other, while the shock imparted by the instrument detaches the minute medusoids, which scintillate upward from the parent stem to the summit of the water. Mere blowing on the surface in July, where Laminariæ abound, suffices to produce the emission of light from the pelagic buds. Moreover, these minute bodies, along with the various species of *Ceratium* and minute larval forms of diverse kinds, are sometimes swept by the gales landward, and cause phosphorescence where least expected. In the same manner Vaughan Thompson ("Zoological Researches," vol. i. part i. mem. iii. p. 48, 1829) found luminous patches on the masts and windward yardarms on board ship, and they gradually mounted upward as the gale increased. Many of the free gonosomes of the Hydroids are as luminous as the polypites, and indeed have been described by some of the older naturalists as one of the main causes of the luminosity of the ocean. The light in these (*e.g. Thaumantias*) gleams around the margin and along the four radii.

The Ascraspedote Medusæ have also been signalised as factors in producing the phosphorescence of the sea, such forms as *Pelagia noctiluca* and *Pelagia cyanella* being especially prominent. Spallanzani, indeed, made an elaborate series of experiments on the luminosity of the Medusæ in his voyage to the Two Sicilies. Some of these, as *Dactylometra* (*Pelagia*) *quinquecirra*, Agass., are nocturnal in their habits. They are only occasionally found floating at the surface during the day, while at night, in the same localities, the bottom swarms with these large masses of dull phosphorescence, moving about with the greatest rapidity (Agassiz, "North American Acalephæ," p. 49, Cambridge, 1865). Species of *Rhizostoma* were likewise observed by Giglioli to have a pale bluish luminosity. The two most abundant Medusæ of our eastern shores, viz. *Aurelia aurita* and *Cyanea capillata* (both in its young purple and adult brown condition), so far as I can make out, exhibit no luminosity. This agrees with the views expressed long ago by Ehrenberg.

The oceanic Hydrozoa (*Siphonophora*) are likewise characterised by their phosphorescence. Thus Giglioli met with luminosity in *Abyla*, *Diphyes*, *Eudoxia*, *Praya* and *Aglaismoides*. Dr. Bennett ("Gatherings of a Naturalist," p. 69, 1860) has also observed luminosity amongst the Coralligenous Actinozoa, the grazing of a boat on a coral reef causing a vivid stream of phosphoric light. Similar observations were made on Madrepores by Giglioli (*Atti della R. Accad. d. Sc. di Torino*, vol. v. p. 502), the light in this case being bright greenish and enduring some minutes.

Amongst the Alcyonarians the luminosity of the common Sea-Pen (*Pennatula phosphorea*) has been long known, and was studied by Gesner, Bartholin, Adler, and others. In the earlier part of this century Grant made the well-known and oft-quoted description (*Brewster's Edin. Journ.* vol. vii. p. 330, 1827), in which he pictures a *Pennatula* "with all its delicate transparent polypi expanded and emitting their usual brilliant phosphorescent light, sailing through the still and dark abysses by the regular and synchronous pulsations of the minute fringed arms of the whole polypi." But it ought to be balanced by his concluding statement, that the sea-pens are probably stationary, or "lie at the bottom, and move languidly like Spatangi, Asteriæ or Actiniæ" (certainly the specimens in the St. Andrew's Marine Laboratory were very helpless). Edward Forbes again observed that the light proceeded from the irritated point to the extremity of the polypiferous portion, and never in the opposite direction. As Dr. George Johnston tells us, Forbes induced Dr. George Wilson to test, along with Professor Swan, the polyps during phosphorescence by a delicate galvanometer, but without result. He thought the luminosity was due to a spontaneously inflammable substance.

More recently a series of interesting observations were made by Panceri on the structure and physiology of the luminous organs of this form. His conclusions are (1) that the light emanates from the polyps and zoids; (2) that the phosphorescent organs are the eight white cords adhering to the outer surface of the stomach, and that these are chiefly composed of cells containing a substance of a fatty nature, the oxidation of which causes the light. Panceri's conclusions further considerably modify Forbes's views about the direction of the waves or points of light. He supposes that the elements which stand in the place of nerves are capable of producing in the luminous batteries of the polyps a momentary oxidation—more rapid and more intense—accompanied by phosphorescence. Like those examined by Professor Milnes Marshall ("Report on the Oban Pennatulidæ," p. 49, Birmingham, 1882), the specimens at St. Andrews, after irritation, show a series of brilliant coruscations which flash along the rows of polyps in a somewhat irregular manner.

Two other Alcyonarians, *Funiculina* and *Umbellularia*, are equally phosphorescent. Though the former is familiar enough to some of the long liners of the outer Hebrides and west coast, it is rare that either is procured for scientific investigation. *Funiculina quadrangularis*, according to Forbes (Johnston's *Brit. Zooph.* vol. i. p. 166), gives out a vivid bluish light, which comes from the bases of the polyps, and appears to be connected with the reproductive system. Wyville Thomson ("Depths of the Sea," p. 149) describes the specimens procured in the *Porcupine* as resplendent with a steady pale lilac phosphorescence like the flame of cyanogen; and always sufficiently bright to make every portion of a stem caught in the tangles distinctly visible. The same zoologist mentions that the stem and polyps of *Umbellularia* are so brightly phosphorescent, that Captain Maclear found it easy to determine the character of the light by the spectroscope. It gave a restricted spectrum sharply included between the lines *b* and D ("Atlantic," vol. i. p. 151).

Besides the foregoing Alcyonarians, *Isis* and *Gorgonia* have been indicated as likewise phosphorescent. Dr. Merle Norman and Dr. Gwyn Jeffreys (whose death since the last meeting of the British Association is a serious loss to science) mention a beautifully luminous *Isis* on board the French ship *Le Travailleur;* and Sir Wyville Thomson ("Atlantic," vol. i. p. 119), with the facile and genial pen which characterised the lamented naturalist, gives a fascinating picture of a long, delicate, simple Gorgonian which came up in immense numbers in the trawl from 600 fathoms off the Spanish coast. He conjures up this Gorgonian forest as an animated cornfield waving gently in the slow tidal current, and glowing with a soft diffused phosphorescence, scintillating and sparkling on the slightest touch, and now and again breaking into long avenues of vivid light, indicating the paths of fishes or other wandering denizens of these enchanted regions. Prof. Moseley thinks that this brilliant phosphorescence of the Alcyonarians may be regarded as an accidental production, but that it may be of occasional service. Further, that the deep sea is at any rate lighted up by these Alcyonarians, which would thus form luminous oases round which animals with eyes might possibly congregate ("Notes of a Naturalist on the *Challenger*," p. 590).

The last group of the Cœlenterates, the *Ctenophora*, are even more conspicuous than the foregoing in regard to luminosity. It is indeed long since the Abbé Dicquemare descanted on *Cydippe* (*Pleurobrachia*) and Suriray on *Beroë*, while subsequent authors have made it clear that the majority of this group are phosphorescent. In our own seas, as Prof. Allman observes, *Beroë* at various stages is one of the most prominent luminous forms during certain seasons. Their enormous numbers make their effects more striking, though the intensity of the phosphorescence is less than that of the Medusæ. Quiet seas like Bressay Sound and the Firth of Forth are occasionally covered by a dense layer of these animals. Prof. Allman found that *Beroë* did not phosphoresce if suddenly taken from light into darkness, but that after they had remained about twenty minutes in obscurity they became luminous. Considerable variety exists in this respect at St. Andrews, some emitting light at once, others showing none. It is probable that this uncertainty is connected with the hygienic condition of the individuals.

In foreign seas many brightly luminous species are met with. Thus Prof. A. Agassiz ("North American Acalephæ," p. 20, Cambridge, 1865) describes *Mnemiopsis Leidyi* as "exceedingly phosphorescent, and when passing through shoals of these Medusæ, varying in size from a pin's head to several inches in length, the whole water becomes so brilliantly luminous that an oar dipped up to the handle can plainly be seen on dark nights by the light so produced; the seat of the phosphorescence is confined to the locomotive rows, and so exceedingly sensitive are they that the slightest shock is sufficient to make them plainly visible by the light emitted from the eight phosphorescent ambulacra." The same author (*Op. cit.* p. 24) mentions that *Lesueuria* has a very peculiar bluish light of an exceedingly pale steel colour, but very intense. Giglioli, again, found that the beautiful riband-like *Cestus* shone with a reddish yellow light, but in *Eucharis* the latter was intensely blue (*Op. cit.* p. 495, 496).

While many of the preceding group are pelagic at all periods of their existence, the luminous star-fishes are in their adult condition members of the bottom fauna. The larval stages of the brittle-stars, however, are passed at the surface of the water, where it is probable they add their quota to swell the ranks of the phosphorescent types. Amongst the first to note this property in the brittle-stars was Prof. Viviani, who found on the shores of Genoa a little brittle-star which he termed *Asterias noctiluca*,[1] and which probably is identical with the *Amphiura elegans* of Leach. Péron likewise mentions the phosphorescence of his *Ophiura phosphorea*. Sir Wyville Thomson observed in the *Porcupine* that the light from *Ophiacantha spinulosa* was of a brilliant green, coruscating from the centre of the disk along the rays and illuminating the whole outline of the starfish ("Depths of the Sea," p. 98). More recently Prof. Panceri of Naples has re-examined the phosphorescence of the species described by Viviani, and he finds that though with the first momentary glow the whole ray is lit up with a greenish light, that the luminous points correspond with the bases of the pedicels and are ranged in pairs along the arms (*Atti della R. Accad. d. Sc. Fisiche e Mathem. di Napoli*, 1875, p. 17, pl. iv. figs. 1, 3). In deep water (between twenty and forty fathoms) off our eastern shores, *Ophiothrix* gleams all over the trawl-net with a pale greenish light; but the adults of the same form between tide-marks give no trace of luminosity.

The older authors were familiar with certain luminous annelids which they termed *Nereides*, such as *Nereis phosphorans*. Ehrenberg paid considerable attention to this group, specially referring to *Polynoë fulgurans* from the North Sea, *Nereis noctiluca*[2] and *Nereis* (*Photocharis*) *cirrigera*, the latter species having a photogenic structure in its cirri like the electric organ of the Torpedo. The latter form is probably related to the ubiquitous *Eusyllis*, which, under various names, has been noticed by many observers. Thus it is very likely the same species that is mentioned by Harmer, in Baker's "Employment for the Microscope," p. 400, as having been found on oyster shells; and also by Vianelli, who describes it as a caterpillar-like form amongst seaweeds. Indeed the Syllideans have been conspicuous in the literature of phosphorescence from the time of De la Voie (1666, *fide* Panceri), and Vianelli ("Nuove Scoperte intorno le Luci dell' Acqua Marina," Venezia, 1749), to the recent period of Claparède ("Glanures Zootomiques," p. 95) and Panceri (*Op. cit.* p. 8). The structure of the cirri of the phosphorescent forms, however, gives no support to the opinion of Ehrenberg that they possess a special photogenic structure.

The luminous annelids group themselves under five families, viz. the Polynoidæ, Syllidæ, Chætopteridæ, Terebellidæ, and Tomopteridæ, and the number may yet be extended to include other pelagic types.

In the first family one of the most abundant is *Harmothoë imbricata*, which lives both between tide-marks and deep water, and is cosmopolitan in geographical distribution. It discharges bright greenish scintillations from the point of attachment of each dorsal scale; and thus, under irritation, the flashes are arranged in pairs along the body, or in a double moniliform line. If severely pinched the worm wriggles through the water, emitting sparks of green light from the bases of the feet. The separated scales also continue to gleam for some time, chiefly at the surfaces of attachment (scars), near which, in each, a ganglion exists. The same phenomenon is readily produced in a fragment either of the anterior or posterior end of the body. No mucous secretion is emitted, but the light is clearly produced by the will of the animal, and by the agency of its nervous system. A recent writer, Dr. Jourdan (*Zoologischer Anzeiger*, March, 2 1885, No. 189, p. 133), has endeavoured to prove that this luminosity in another member of the Polynoidæ (viz., *Polynoë torquata*) is produced by cells secreting a phosphorescent mucus, but this view is by no means applicable in all cases.

Besides the species mentioned, various other forms in this family are equally luminous, such as *Polynoë scolopendrina*, *Achloë astericola*, *Polynoë lunulata*, and a Zetlandic *Eunoa*.

[1] "Phosphorescentia Maris," Genoa, 1805, p. 5, tab. i. figs. 1, 2. He observes: "Species hæc radiatæ instar stellæ scintillas in marinis aquis excitasse, quas electrico fluido adscripserunt, admodum probabile est."

[2] Supposed by some to refer to *Noctiluca miliaris*.

As an example of the Syllidæ, the common *Eusyllis*, so often mentioned by previous authors, may be taken. Under irritation a fine green light is emitted from the ventral aspect of each foot, and the scintillations seem to issue from many points at each space, flash along both sides of the worm posterior to the point of stimulation, and then disappear. Under severe irritation the animal remains luminous behind the injured part for nearly half a minute, while the surface of granular light on each segment is larger than usual, and in some instances those of opposite sides are connected on the ventral aspect by a few phosphorescent points. The body behind the irritated region has a paler pinkish hue immediately after the emission of light showing that the luminosity is diffused.

In the Chætopteridæ the phosphorescence is remarkably beautiful, bright flashes being emitted from the posterior feet; but the most vivid luminosity is at a point on the dorsum between the lateral wings of the tenth segment. Here the abundant mucus exuded by the animal can be drawn out as bluish-purple fire of great intensity, which, besides, now and then gleams along the edges of the wing-like processes, and illuminates the surrounding water. A very characteristic odour, somewhat resembling that produced by phosphorus in combustion, is given out by the animal during such experiments. In this connection it may be observed that Quoy and Gaimard mention that an odour similar to that around an electric machine is given out by luminous marine annelids.

Amongst the Terebellidæ, as first shown by Grube none excel the genus *Polycirrus* in the brightness of the phosphorescence and the ease with which it is elicited. Mere blowing on the water of the dissecting-trough suffices to cause in the British *Polycirrus* the most vivid pale bluish luminosity, which gleams for a moment along every one of the independent mobile tentacles. Long before Grube, however, had discovered the phosphorescence of *Polycirrus*, our patient and laborious countryman, Sir J. Graham Dalyell, had noticed it in the group ("Powers of the Creator," vol. ii. p. 210), for he mentions that when irritated *Terebella figulus* gave out the most copious blue refulgence, intermingled with a reddish flame. Another member of this family, viz. *Thelepus*, is only faintly phosphorescent in life, but when decomposition has made progress it gleams in the vessel with a pale lambent light, somewhat like phosphorus in air.

In the pelagic Tomopteridæ certain peculiar structures on the parapodia, formerly supposed by some to be eyes, and by others simply glandular organs, were lately found by Professor Greeff (*Zoologischer Anzeiger*, 1882, p. 384—87) to be luminous organs, which, though glandular, have a considerable nervous supply, including a ganglion.

Panceri's observations on the luminous annelids of Naples, and the peculiar type *Balanoglossus* (Enteropneusta) have recently added considerably to our knowledge of the subject. He specially describes, in *Chætopterus*, the structure of the phosphorescent glands in the great pinnules and other parts, which produce the luminous mucus. With some reason he concludes that two kinds of phosphorescence are present in annelids, viz., one which is the result of purely nervous action, and another which is due to this *plus* a luminous secretion.

A Turbellarian, viz., *Planaria retusa*, was mentioned by Viviani (*Op. cit.* p. 13) as luminous, but this feature appears to be rare in the group; and the same may be observed of phosphorescent Rotifers, one of which (*Synchæta baltica*) was described by Ehrenberg (*Op. cit.* p. 128). Giglioli (*Op. cit.* p. 498) again, records a *Sagitta* which showed a feeble luminosity in the posterior region of the body.

The minute forms amongst the Crustacea (chiefly Copepoda) were recognised as phosphorescent by Athanasius Kircher in 1640, and have been mentioned by most authors who have alluded to the subject since that date. Thus Viviani gives seven species from the shores of Genoa, and Tilesius no less than nineteen luminous crustaceans from Krusenstern's voyage. Dr. Baird describes the light given out by those met with in his cruises as brilliant in the extreme, and Vaughan Thompson added considerably to our knowledge of *Sapphirina* and of the luminous schizopods, an example of which had been discovered by Sir Joseph Banks, and described by Macartney (*Phil. Trans.* 1810, as "Cancer fulgens"). Most authors agree that the minute forms, such as the Copepods, give a sparkling appearance to the surface of the water. The light in these, according to Lesson, proceeds from glands placed on the sides of the thorax; while Giglioli found the luminous organ of the cosmopolitan *Sapphirina* in the anterior part of the thorax. On the other hand, Captain Chimmo (*Euplectella*, &c., 1878) thought it was decomposing food in the stomach, and Prof. Moseley (*Op. cit.* p. 574.—Naturalist on the *Challenger*) in certain cases entertained a similar opinion. The phosphorescence of the Euphausiidæ was a prominent feature in the voyage of the *Challenger*, brilliant flashes being emitted on capture from a series of spots along the trunk and tail. Mr. Murray also met with a diffused light in the Faröe channel when dredging in the *Triton*, and he attributed this to the phosphorescent organs of *Nyctiphanes norvegica*, M. Sars, one of the same group. Prof. G. O. Sars describes these organs as composed of a series of coloured globules, the lens-like body of which acts as a condenser, and thus enables the animal to produce at will a bright flash of light in a given direction ("*Challenger* Narrative," Zoology, I. part ii. pp. 740—43).

Marine phosphorescence has some of its most striking examples amongst the Tunicates. One of the best known instances is that of *Pyrosoma*, the light from which has been so graphically described by M. Péron, Prof. Huxley, and other naturalists who have had an opportunity of observing it. It proceeds in each member of the compound organism from two small patches of cells at the base of each inhalent tube. These cells contain a substance resembling fat. *Salpa* has frequently been mentioned as a luminous form by many authors, but Delle Chiaje found that in the Mediterranean *Salpa pinnata* was not phosphorescent; and amongst the multitudes of Salpæ which for some weeks abounded at Lochmaddy in North Uist, neither the former nor the *Salpa spinosa* of Otto exhibited this property, though a spark was occasionally seen in the nucleus in some specimens, probably from the food. Giglioli likewise is doubtful concerning them, but in one instance a brilliant rose-coloured light appeared in the nucleus. *Doliolum*, on the other hand, shone with a greenish tint, while examples of *Appendicularia* which he encountered in various seas were chameleon-like in their luminosity, and often gleamed with great brightness.

Various mollusks exhibit the property of phosphorescence. Fabricius ab Aquapendente mentions *Sepia*, Panceri *Eledone*, Adler *Chama* and "*Dactylus*." The best known, however, is *Pholas dactylus*, which possesses two wavy bands and triangular organs of ciliated epithelium on the inner surface of the mantle. These secrete a luminous substance, soluble in ether and alcohol, which light up the excurrent water. The light is also maintained for a long time during putrefaction, as in the case of *Thelepus*. Panceri found that carbonic acid extinguished the light, but that air re-illuminated it, just as Johannes Müller had previously observed in a vacuum and in air. The light is monochromatic, the bands having a constant place in connection with the solar spectrum (from line E to line F).

Several Pteropods likewise contribute to the phosphorescence of the sea. Thus Giglioli noticed that a *Cleodora* gave out a very reddish light, while a *Criseis* and a *Hyalæa* were luminous at the base of the shell. He mentions also a large unknown Heteropod (*Op. cit.* p. 497) in the Indian Ocean, which glowed with a reddish phosphorescence. Amongst the Dermatobranchs, *Phyllirrhoë* has the same property, Giglioli further found that *Loligo sagittatus* and a small *Octopus* gleamed all over with a whitish luminosity.

Phosphorescence in living fishes appears to have been accurately observed within a comparatively recent date, though the luminosity of dead fishes has been known from very early times, and has been the subject of many interesting experiments such as those of Robert Boyle on dead whitings (*Phil. Trans.* 1667, pp. 591-93), and Dr. Hulme on herrings (*Phil. Trans.* 1800, p. 161). I do not mean to say that the literature of the so-called phosphorescent fishes is scanty, for it extends from the days of Aristotle and Pliny to modern times, but that the writers have had little reliable evidence in regard to living fishes to bring forward. Thus of upwards of fifty fishes entered by Ehrenberg in his list it is hard to say that one is really luminous during life. In many cases it is probable that the supposed phosphorescence of large forms, such as sword-fishes and sharks, has arisen from the presence of multitudes of minute phosphorescent animals in the water, just as the herring causes a gleam when it darts from the side of a ship. Prof. Moseley, for instance, observed in the *Challenger* that when large fishes, porpoises, and penguins dashed through phosphorescent water, that it was brilliantly lit up, and their track marked by a trail of light. The same feature is observed in hooked fishes, and it is known that fishermen are doubtful of success when the sea is very phosphorescent, for the presence of the net in the water excites the luminosity and scares the herring.

One of the most striking instances of phosphorescence in living fishes is that of the luminous shark (*Squalus fulgens*) found by Dr. Bennett. This is a small dark-coloured shark, which was captured on two or three occasions at the surface of the sea. It emitted without irritation a vivid greenish luminosity as it swam about at night, and it shone for some hours after death. The phosphorescence appears to be due to a peculiar secretion of the skin. The eyes of the shark were more prominent than usual in such forms. (The Danish naturalist Reinwardt describes a phosphorescent fish (*Hemiramphus lucens*) from the Moluccas. *Fide* Giglioli, *Op. cit.* p. 503.) Little is known with regard to the luminosity of the "Pearl-sides" (*Maurolicus pennantii*, Cuv. and Val.) of our own shores, though from its wide distribution this lack of information seems to be remediable.

In recent times phosphorescence has generally been associated with deep-sea fishes. Thus in a narrative of the early part of the voyage of the *Challenger* (NATURE, August 28, 1873) Sir Wyville Thomson mentions ranges of spots or glands producing a phosphorescent secretion on the body of a fish pertaining to the Sternoptychidæ, a species of which is included by Dr. F. Day in the British list. Of a new *Echiostoma* (one of the Stomiatidæ) it is also noted that the two rows of probably phosphorescent dots along the body were red, surrounded by a circle of pale violet ("*Challenger* Narrative, Zoology," I. vol. ii. p. 42). Dr. Günther ("*Challenger* Narrative, Zoology, I. part ii. p. 905) observes that many deep-sea fishes have round, shining, mother-of-pearl bodies embedded in the skin. These are supposed to be producers of light, and they have been observed to be phosphorescent in two species of Sternoptychidæ. He further states that the whole muciferous system is dilated in deep-sea fishes, that is, fishes inhabiting 1000 fathoms or more, and that the entire body seems to be covered with a layer of mucus, the physiological use of which is unknown; it has been noticed to have phosphorescent properties in perfectly fresh specimens.

Having thus briefly reviewed the leading features of phosphorescence in marine animals, a glance may now be taken at the supposed causes and purposes of this provision.

I do not deem it necessary to go into detail with regard to the numerous views which have been advanced to account for the phosphorescence of marine organisms, for these range over a very wide area—from its production by electricity, the constant agitation of the water, by putrefaction, by luminous imbibition, to its manifestation as a vital action in the animals, or a secretion of a phosphorescent substance. Ehrenberg considered it a vital act similar to the development of electricity, and sometimes accompanied by the secretion of a mucilaginous humour which is diffused around; while others, such as Meyen, thought it only a superficial oxidation of the mucous coat, or a luminour secretion from certain glands. Some believed that a liquid containing phosphorus was secreted, and that this underwent slow combustion; while others explained that it was a nervous fluid modified by certain organs to appear as light. Coldstream thought it was due to an imponderable agent, and that phosphorus or an analogous substance might enter into the organs producing it. De Quatrefages, again, clearly affirms that it is produced in two ways: (1) by the secretion of a peculiar substance exuding from the entire body or a special organ; and (2) by a vital action independent of all material secretion. Panceri was strongly impressed with the importance of fatty matter in the forms he examined—such as *Pennatula*, the Medusæ, Beroïdes, Pholades, *Chætopteri*, and *Noctilucæ*—the phosphorescence arising from the slow oxidation of this substance; the nervous system of the living animal, however, being capable of producing a momentary oxidation more rapid and more intense, accompanied by light.

It will be observed that in the Protozoa the structure of the minute but often very abundant animals which furnish the luminosity clearly proves that the presence of a well-defined nervous system is not required for its manifestation, the protoplasm of their bodies alone sufficing for its development. There are neither glands for secreting it, and in some apparently no fatty matter for slow combustion. In the Cœlenterates the phenomena appear to be more nearly related to nervous manifestations, though in certain cases the luminous matter possesses inherent properties of its own. While in some annelids, such as *Chætopterus* and *Polyirrus*, there are glands which may be charged with the secretion of a luminous substance, it is otherwise with certain Polynoidæ, in which the emission of light appears to be an inherent property of the nervous system. The irritability in the phosphorescent examples of the latter family, however, varies considerably, some, *e.g. Polynoë scolopendrina*, being sluggish, while others, like *Harmothoë*, are extremely irritable. In the Crustaceans the luminosity seems to have the nature of a secretion, probably under the control of the nervous system. In *Pyrosoma* and *Pholas dactylus* a luminous secretion is also a prominent feature, and in both the latter and the annelids decay excites its appearance, as also is the case, to a limited extent, in fishes.

It is evident, therefore, that the causation of phosphorescence is complex. In the one group of animals it is due to the production of a substance which can be left behind as a luminous trail. The ease, for instance, with which in *Pennatula* and other Cœlenterates the phosphorescence can be repeatedly produced by friction on a surface having a minute trace of the material, clearly points to other causes than nervous agency. The action, moreover, clearly affects the organic chemical affinities of the tissues engaged. On the other hand again, as in certain annelids, it is purely a nervous action, probably resembling that which gives rise to heat.

With the exception of such as Macartney, the older authors, who in some cases took an imaginative view of the question, connected the emission of light with the special economy of the deep sea. The speculations to this effect are fairly summarised in "Brewster's Edinburgh Encyclopædia," published in 1830 (Chiefly the views of Dr. Macculloch). Thus it is supposed that total darkness exists at the depth of 1000 feet, and that the phosphorescence of marine animals is a substitute for the light of the sun. Moreover, that by these lights the animals on the one hand are guided for attack, and on the other their power of extinguishing them enables them to escape destruction. Fishes are known to prey chiefly at night, and the writer supposes that the phosphorescence of their prey guides them; for, he says, this luminosity is particularly brilliant in those inferior animals which from their astonishing powers of reproduction, and from a state of feeling little superior to that of vegetables, appear to have been in a great measure created for the food of the more perfect kinds. Dr. Coldstream at a later period (1847) reproduced the same views in his article on animal luminosity (Todd's "Cyclop. of Anat. and Phys.").

The same notion was brought forward in the "Report of the Cruise of the *Porcupine*" (*Proc. Roy. Soc.*, No. 121, 1870, p. 432), and special reference was made to the young of certain starfishes, which are stated to be more luminous than the adults, that being part of the general plan which provides an excess of the young of many species, apparently as a supply of food, their wholesale destruction being nece sary for the due restriction of the multiplication of the species, while the parent individuals, on the other hand, are provided with special appliances for escape or defence. Thus phosphorescence, it is further asserted ("Depths of the Sea," p. 149), in very young Ophiacanthæ just rid of their plutei, in a sea swarming with predaceous crustaceans, such as *Dorynchus* and *Munida*, with great bright eyes, must be a fatal gift. Some naturalists still appear to hold a similar, though perhaps modified view. Much caution, however, is necessary in theorising on this head.

In the first place, phosphorescent animals do not appear to be more abundant in the depths of the sea than between tide-marks or on the surface, the latter perhaps presenting the maximum development of those exhibiting this phenomenon. Very many of the young that have been indicated as so brilliantly luminous become surface-forms soon after leaving the egg, and thus at their several stages more or less affect the three regions—of surface, mid-water, and bottom.

A survey of the life-histories of the several phosphorescent groups affords at present no reliable data for the foundation of a theory as to the functions of luminosity, especially in relation to food. No phosphorescent form is more generally devoured by fishes or other animals than that which is not; and, on the other hand, the possessor of luminosity, if otherwise palatable, does not seem to escape capture. An examination of the stomachs of fishes makes this clear, except perhaps in the case of the herring, which, however, is chiefly a surface-fish. Further, it is not evident that such animals are luminous at all times, for it is only under stimulation that many exhibit the phenomenon.

Moreover, the irregularity of its occurrence in animals possessing the same structure and habits in every respect, strengthens the view just expressed. Thus, while *Pholas dactylus* has been known from the days of Pliny to be luminous, the common *Pholas crispata* is not so endowed. Two annelids abound between tide-marks (*Harmothoë imbricata* and *Polynoë floccosa*), and closely resemble each other in habits and appearance; yet one is brightly luminous, while the other shows no trace. Instead of luring animals for prey, or affording facilities for being easily preyed upon, the possessors of phosphorescence in the annelids are often the inhabitants of tubes, or are commensalistic on starfishes. Indeed, every variety of condition accompanies the presence of phosphorescence in the several groups, so that the greatest care is necessary in making deductions, especially if these are to have a wide application.

In the foregoing brief outline of the remarkable phenomenon of phosphorescence as it affects marine animals, it is apparent that, though a considerable increase in our knowledge has taken place during the last quarter of a century, much more yet remains to be done. I, however, confidently look forward for further advances, in this as well as in other departments, to the marine laboratories of the country—I mean such institutions as those now in working order at Granton, St. Andrews, and Tarbet, as well as the larger establishment proposed to be erected by the Biological Association at Plymouth. These laboratories, it is true, have been tardily instituted, but it is satisfactory to think that at last the zeal and methods of the workers have, and will have, a better field for their exercise than formerly, and that the zoology of the fisheries will obtain that attention which its importance to the country necessitates.

SECTION E.

GEOGRAPHY.

Opening Address by General J. T. Walker, C.B., LL.D., F.R.S., F.R.G.S., President of the Section.

My predecessors in this chair have claimed for geography a range of science which may be said to be practically unlimited; for it comprehends the history of the earth itself, and of all the life to be met with on the surface of the earth, from the first beginnings of things, and through their subsequent development onwards to their present conditional status; it is associated in a greater or less degree with every other department of knowledge and is a remarkable exemplification of the mutual interdependence and correlation of the physical sciences, for while all other branches of science are incomplete without some knowledge of geography, it is incomplete without some knowledge of each and all of them.

Such claims on behalf of geography would, not many years ago, have been considered extravagant and exaggerated; a popular encyclopædia which is still of some note defines geography to be simply the science which describes the surface of the earth, and somewhat querulously complains that geographical treatises contain matter not unfrequently taken from statistics, natural philosophy, and history which it declares to be irrelevant and not properly admissible into such treatises. And in a popular sense geography is still commonly suggestive only of such a knowledge of locality as may be acquired from maps and charts, with their graphical delineations of whatever exists on the surface of the earth, and of the various natural or artificial boundary lines of the peoples and states between whom the surface is divided. But the British Association and the Royal Geographical Society have successfully maintained that scientific geography is not restricted in its scope to a mere knowledge of locality—though that in itself is a very important factor in whatever appertains to the intercourse and mutual relations of mankind—but embraces all that relates to the structure and existing configuration of the earth, and takes cognisance of the varied conditions of all the life, both animal and vegetable, which is nurtured and supported by the earth; it studies the side lights which the general configuration of surface throws on the character of each locality as a home and support of life, and it examines with special interest the influence which that character has exerted on the social and political conditions of different races and peoples.

And geography does not merely devote its attention to the existing order of things as now displayed to our gaze; in alliance with geology it studies the history of a distant past, when the features of the earth's surface were not precisely as now, and lands which we see high above our horizon lay deep beneath the ocean, and life existed in other forms, whose mute records we possess in the fossils—the *likha-káni* or written stones as they are significantly called by the people of Afghanistan—which, after long lying entombed among the rocks, are presented to modern sight as revelations of life's early dawn; it investigates what Baron Richtofen describes as the reciprocal causal relations of the three kingdoms—land, water, and atmosphere; it seeks to determine the processes by which in some parts of the globe continents were built up with their varied sculpture of mountain and valley, of highly elevated plateau and low lying plain, of lakes and inland seas, and great river systems,—while in other parts land was depressed below the sea level, or broken up into the islands which are now dotting the surface of the ocean; and it endeavours to trace a process of continuous evolution of life from the primary and simplest types which perished in the early ages of the earth's history, to the latest and most highly developed types which are now flourishing around us. Going back still further it searches for evidence of the first beginnings of the material universe; it looks beyond the orbit of the most distant planet of the solar system, and scrutinises the boundless regions of stellar space to find, in the widely scattered particles of the nebulæ, the beginnings of new solar systems and new worlds such as ours; there it may be said to behold as in a mirror the formation of our own planet as a fluid igneous mass thrown off with great velocity from its sun, and rapidly revolving, and then becoming spheroidal, and slowly cooling and solidifying, and finally acquiring the crust which was to become an abode for life, the stage whereon man was to play out the drama of his planetary existence, and be held all the while fast imprisoned and out of touch with the surrounding universe.

More than this we would seek to know, but in vain; in passing from the early dawn of matter to that of life, science finds its career of wonderful achievement in the one direction exchanged for failure and disappointment in the other; it cannot discover the origin of life in any of its existing material forms, nor trace to its birthplace the spiritual life which exerts such an influence on what is material; it cannot ascertain whether man had a prior existence as different from his present existence as the first beginnings of his planet home differed from its present condition; it cannot gauge the truth of the poet's prescient conception that

> "Our birth is but a sleep and a forgetting;
> The soul that rises with us, our life's star,
> Hath had elsewhere its setting
> And cometh from afar."

It whispers faint suggestions regarding the possible future of the planet; but when questioned as to what is to follow the coming soul's setting of man, the planet's chief glory and dignity, it has nothing to reply, but is hopelessly dumb and inarticulate.

Scientific geography embraces a wide range of subjects, wider than can be claimed for any other department of science. Thus the President of this Section has a vast field from which to gather subjects for his opening address. I shall, however, restrict my address to the subject with which I am most familiar, and give you some account of the Survey of India, and more particularly of the labours of the trigonometrical or geodetic branch of that survey, in which the best years of my life have been passed.

I must begin by pointing out that the survey operations in India have been very varied in nature, and constitute a blending together of many diverse ingredients. Their origin was purely European, nothing in the shape of a general survey having been executed under the previous Asiatic Governments; lands had been measured in certain localities, but merely with a view to acquiring some idea of the relative areas of properties, in assessing on individuals the share of the revenue levied on a community; but other factors than area—such as richness or poverty of soil, and proximity or absence of water—influenced the assessment, and often in a greater degree, so that very exact measurements of area were not wanted for revenue purposes, and no other reason then suggested itself why lands should be accurately measured. The value of accurate maps of individual properties, with every boundary clearly and exactly laid down, was not thought of in India in those days, and indeed has only of late years begun to be recognised by even the British Government. The idea of a general geographical survey never suggested itself to the Asiatic mind. Thus when Englishmen came to settle in India, one of their first acts was to make surveys of the tracts of country over which their influence was extending; and as that influence increased, so the survey became developed from a rude and rapid primary delineation of the broad facts of general geography, to an elaborately executed and artistic delineation of the topography of the country, and in some provinces to the mapping of every field and individual property. Thus there have been three orders or classes of survey, and these may be respectively designated geographical, topographical, and cadastral; all three have frequently been carried on *pari passu*, but in different regions, demanding more or less elaborate survey according as they happened to be more or less under British influence. There is also the Great Trigonometrical or Geodetic Survey, by which the graphical surveys are controlled, collated, and co-ordinated, as I will presently explain.

Survey operations in India began along the coast-lines before the commencement of the seventeenth century, the sailors preceding the land surveyors by upwards of a century. The Directors of the East India Company, recognising the importance of correct geographical information for their mercantile enterprises, appointed Richard Hakluyt, Archdeacon of Westminster, their historiographer and custodian of the journals of East Indian voyages, in the year 1601, within a few weeks of the establishment of the company by Royal Charter. Hakluyt gave lectures to the students at Oxford, and is said by Fuller to have been the first to exhibit the old and imperfect maps and the new and revised maps for comparison in the common schools, "to the singular pleasure and great contentment of his auditory." The first general map of India was published in 1752 by the celebrated French geographer D'Anville, and was a meritorious compilation from the existing charts of coast-lines and itineraries of travellers. But the Father of Indian Geography, as he has been called, was Major Rennell, who landed in India as a midshipman of the Royal Navy in 1760, distinguished himself in the blockade of Pondicherry, was employed for a time in making surveys of the coast between the Paumben Passage and Calcutta, was appointed Surveyor of the East India Company's dominions in Bengal in 1764, was one of the first officers to receive a commission in the Bengal Engineers on its formation, and in 1767 was raised to the position of Surveyor-General. Bengal was not in those days the tranquil country we have known it for so many years, but was infested by numerous bands of brigands who professed to be religious devotees, and with whom Rennell came into collision in the course of one of his surveying expeditions, and was desperately wounded; he had to be taken 300 miles in an open boat for medical assistance, the natives meanwhile applying onions to his wounds as a cataplasm. His labours in the survey of Bengal lasted over a period of nineteen years, and embraced an area of about 300,000 square miles, extending from the eastern boundaries of Lower Bengal to Agra, and from the Himalayas to the borders of Bandelkand and Chota Nagpur. Ill-health then compelled him to retire from the service on a small pension and return to England; but not caring, as he said, to eat the bread of idleness, he immediately set himself to the utilisation of the large mass of geographical materials laid up and perishing in what was then called the India House; he published numerous charts and maps, and eventually brought out his great work on Indian Geography, the "Memoir of a map of Hindostan," which went through several editions; this was followed by his Geographical System of Herodotus, and various other works of interest and importance. His labours in England extended over a period of thirty-five years, and their great merits have been universally acknowledged.

Rennell's system of field-work in Bengal was a survey of routes checked and combined by astronomical determinations of the latitude and the longitude, and a similar system was adopted in all other parts of India until the commencement of the present century. But in course of time the astronomical basis was found to be inadequate to the requirements of a general survey of all India, as the errors in the astronomical observations were liable materially to exceed those of the survey, if executed with fairly good instruments and moderate care. Now this was no new discovery, for already early in the eighteenth century the French Jesuits who were making a survey of China—with the hope of securing the protection of the Emperor, which they considered necessary to favour the progress of Christianity—had deliberately abandoned the astronomical method and employed triangulation instead. Writing in the name of the missionaries who were associated with him in the survey, Père Regis enters fully into the relative advantages of the two methods, and gives the trigonometrical the preference, as best suited to enable the work to be executed in a manner worthy the trust reposed in them by a wise prince, who judged it of the greatest importance to his State. "Thus," he says, "we flatter ourselves we have followed the surest course, and even the only one practicable in prosecuting the greatest geographical work that was ever performed according to the rules of art."

What was true in those days is true still; points whose relative positions have been fixed by any triangulation of moderate accuracy present a more satisfactory and reliable basis for topographical survey than points fixed astronomically. Though the lunar theory has been greatly developed since those days by the labours of eminent mathematicians, and the accuracy of the lunar tables and star catalogues is much increased, absolute longitudes are still not susceptible of ready determination with great exactitude; moreover, all astronomical observations, whether of latitude or longitude, are liable to other than intrinsic errors, which arise from deflection of the plumb-line under the influence of local attractions, and which of themselves materially exceed the errors that would be generated in any fairly executed triangulation of a not excessive length, say not exceeding 500 miles.

Thus at the close of the last century Major Lambton, of the 33rd Regiment, drew up a project for a general triangulation of Southern India. It was strongly supported by his commanding officer—Colonel Wellesley, afterwards the Duke of Wellington—and was readily sanctioned by the Madras Government; for a large accession of territory in the centre of the peninsula had been recently acquired, as the result of the Mysore campaign, by which free communication had been opened between the east and west coasts of Coromandel and Malabar; and the proposed triangulation would not merely furnish a basis for new surveys, but connect together various isolated surveys which had already been completed or were then in progress. The Great Trigonometrical Survey of India owes its origin as such, and its simultaneous inception as a geodetic survey, to Major Lambton, who pointed out that the trigonometrical stations must needs have their latitudes and longitudes determined for future reference just as the discarded astronomical stations, not however by direct observation, but by processes of calculation requiring a knowledge of the earth's figure and dimensions. But at that time the elements of the earth's figure were not known with much exactitude, for all the best geodetic arcs had been measured in high latitudes, the single short and somewhat questionable arc of Peru being the only one situated in the vicinity of the equator. Thus additional arcs in low latitudes, as those of India, were greatly needed and might be furnished by Lambton. He took care to set this forth very distinctly in the programme which he drew up for the consideration of the Madras Government, remarking that there was thus something still left as a desideratum for the science of geodesy, which his operations might supply, and that he would rejoice indeed should it come within his province "to make observations tending to elucidate so sublime a subject."

Lambton commenced operations by measuring a base line and a small meridional arc near Madras, and then, casting a set of triangles over the southern peninsula, he converted the triangles on the central meridian into a portion of what is now known as the Great Arc of India, measuring its angles with extreme care, and checking the triangulation by base lines measured at distances of two to three degrees apart in latitude. His principal instruments were a steel measuring chain, a great theodolite, and a zenith sector, each of which had a history of its own before coming into his hands. The chain and zenith sector were sent from England with Lord Macartney's Embassy to the Emperor of China, as gifts for presentation to that potentate, who unfortunately did not appreciate their value and declined to accept them; they were then made over to Dr. Dinwiddie, the astronomer to the embassy, who took them to India for sale. The theodolite was constructed in England for Lambton, on the model of one in use on the Ordnance Survey; on its passage to India it was captured by the French frigate, the *Piemontaise*, and landed at Mauritius, but eventually it was forwarded to its destination by the chivalrous French Governor, De Caen, with a complimentary letter to the Governor of Madras.

Lambton was assisted for a short time by Captain Kater, whose name is now best known in connection with pendulum experiments and the employment of the seconds' pendulum as a standard of length; but for many years afterwards he had no officer to assist him. At first he met with much opposition from advocates of the discarded astronomical method, who insisted on its being sufficiently accurate and more economical than the trigonometrical. But he was warmly supported by Maskelyne, the Astronomer-Royal in England; and soon had an opportunity of demonstrating the astronomical method to be fallacious, for its determination of the breadth of the peninsula in the latitude of Madras was proved by the triangulation to be forty miles in error. Still, for several years he never received a word of sympathy, encouragement, or advice either from the Government or from the Royal Society. A foreign nation was the first to recognise the importance of his services to science, the French Institute electing him a corresponding member in 1817. After this, honours and applause quickly followed from his own countrymen. In 1818 the Governor-General of India—then the Marquis of Hastings—decided that the survey should be withdrawn from the supervision of a local Government and placed under the Supreme Government, with a view to its extension over all India, remarking at the same time that he was "not aware that with minds of a certain order he might lay himself open to the idle imputation of vainly seeking to partake the gale of public favour and applause which the labours of Colonel Lambton had recently attracted;" but as the survey had reached the northern limits of the Madras Presidency, its transfer to the Supreme Government, if it was to be further extended, had become a necessity. He directed the transfer to be made, and the survey to be called in future the Great Trigonometrical Survey of India. Noticing that the intense mental and bodily labour of conducting it was being performed by Lambton alone, that his rank and advancing age demanded some relief from such severe fatigue, and farther, that it was not right that an undertaking of such importance should hang on the life of a single individual, the Governor-General appointed two officers to assist him—Captain Everest, as chief assistant in the geodetic operations; and Dr. Voysey, as surgeon and geologist. Five years afterwards Lambton died, at the age of seventy. The happy possessor of an unusually robust and energetic constitution and a genial temperament, he seems to have scarcely known a day's illness, though he never spared himself nor shrank from subjecting himself to privations and exposure which even Everest thought reckless and unjustifiable. These he accepted as a matter of course, saying little about them, and devoting his life calmly and unostentatiously to the interests of science and the service of his country.

Everest's career in the survey commenced disastrously. He was deputed by Lambton to carry a triangulation from Hydrabad, in the Nizam's territory, eastwards to the coast, crossing the forest-clad and fever-haunted basin of the Godavery river, a region which he described as "a dreadful wilderness, than which no part of the earth was more dreary, desolate, and fatal." Indignant at being taken there, his escort, a detachment of the Nizam's troops, mutinied, and soon afterwards he and his assistants, and almost all the men of his native establishment, were stricken down by a malignant fever; many died on the spot, and the survivors had to be carried into Hydrabad, whence litters and vehicles of all descriptions, and the whole of the public elephants, were despatched to their succour. To recover his health Everest was compelled to leave India for a while and proceed to the Cape of Good Hope, where he remained for three years. He availed himself of the opportunity to inspect Lacaille's meridional arc, which, when compared with the arcs north of the equator, indicated that the opposite hemispheres of the globe were seemingly of different ellipticities. He succeeded in tracing this anomaly to an error in the astronomical amplitude of the arc, which had been caused by deflection of the plumb-line at the ends of the arc, under the influence of the attraction of neighbouring mountains. Thus he became aware of the necessity of placing the astronomical stations of the Indian arcs at points where the plumb-line would not be liable to material deflection by the attraction of neighbouring mountain ranges. Shortly after his return to India Lambton died, and Everest succeeded him, and immediately concentrated his energies on the extension of the Great Arc northwards. He soon came to the conclusion that his instrumental equipment, though good for the time when it was procured, and amply sufficient for ordinary geographical purposes, was inadequate for the requirements of geodesy, and generally inferior to the equipments of the geodetic surveys then in progress in Europe. He therefore proceeded to Europe to study the procedure of the English and French surveys, and also to obtain a supply of new instruments of the latest and most improved forms. The Court of Directors of the Honourable East India Company accorded a most liberal assent to all his proposals, and gave him *carte blanche* to provide himself with whatever he considered desirable to satisfy all the requirements of science.

Everest returned to India with his new instrumental equipment in 1830, a year that marks the transition of the character of the operations from an order of accuracy which was sufficient as a basis for the graphical delineation of a comparatively small portion of the earth's surface, to the higher precision and refinement which modern geodesists have deemed essentially necessary for the determination of the figure and dimensions of the earth as a whole. He immediately introduced an important modification of the general design of the principal triangulation, which up to that time had been thrown as a network over the country on either side of the Great Arc, as in the English survey and many others; but he abandoned this method, and, adopting that of the French survey instead, he devised a system of meridianal chains to be carried at intervals of about 1° apart, and tied together by longitudinal chains at intervals of about 5°, the whole forming, from its resemblance to the homely culinary utensil with which we are all familiar, what has been called the gridiron system in contradistinction to the network. The entire triangulation was to rest on base-lines to be measured with the new Colby apparatus of compensation bars and microscopes which had been constructed to supersede the measuring chain the Emperor of China had rejected; the base-lines were to be placed at the intersections of the longitudinal chains of triangles with the central meridional or axial chain, and also at the further angles of the gridirons on each side. Latitudes were to be measured at certain of the stations of the central chain, with new astronomical circles in place of the old zenith sector, to give the required meridional arcs of amplitude. Two radical improvements on all previous procedure were introduced in the measurement of the principal angles, one affecting the observations, the other the objects observed. The great theodolites were manipulated in such a manner as not merely to reduce the effects of accidental errors by numerous repetitions in the usual way, but absolutely to eliminate all periodic errors of graduation by systematic changes of the position of the azimuthal circle relatively to the telescope, in the course of the complete series of measures of every angle. The objects formerly observed had been cairns of stones or other opaque signals; for these Everest substituted luminous signals, lamps by night, and, by day, heliotropes which were manipulated to reflect the sun's rays through diaphragms of small aperture, in pencils appearing like bright stars, and capable of penetrating a dense atmosphere through which distant opaque objects could not be seen.

Everest's programme of procedure furnished the guiding principles on which the operations were carried out during the period of half a century which intervened between their commencement under his superintendence and the completion of the principal triangulation under myself. The external chains have necessarily been taken along the winding course of the frontier and coast lines instead of the direct and more symmetrical lines of the meridians and the parallels of latitude. The number of the internal meridional chains has latterly been diminished by widening the spaces between them, and in two instances a principal chain has been dispensed with because, before it could be taken in hand, a good secondary triangulation had been carried over the area for which it was intended to provide. But these are departures from the letter rather than the spirit of Everest's programme which has been faithfully followed throughout, first by his immediate successor, Sir Andrew Waugh, and afterwards by myself, thus affording an instance of the impress of a single mind on the work of half a century which is probably unique in the annals of India; for there, as is well known, changes of personal administration are frequent, and are not uncommonly followed by changes of procedure.

The physical features of a country necessarily exercise a considerable influence on the operations of any survey that may be carried over it, and more particularly on those of a geodetic survey, of which no portion is allowed to fall below a certain standard of precision. Every variety of feature, of scenery, and of climate that is to be met with anywhere on the earth's surface between the equator and the arctic regions has its analogue between the highlands of Central Asia and the ocean, which define the limits of the area covered by the Indian survey. Thus in some parts the operations were accomplished with ease, celerity, and enjoyment, while in others they were very difficult and slow of progress, always entailing great exposure, and at times very deadly. In an open country, dotted with hills and commanding eminences, they advanced as on velvet; in close country, forest-clad or covered with other obstacles to distant vision, they were greatly retarded, for there it became necessary either to raise the stations to a sufficient height to overlook all surrounding obstacles, or to render them mutually visible by clearing the lines between them; and both these processes are more or less tedious and costly. There are many tracts of forest and jungle which greatly impeded the operations, not merely because of the physical difficulties they presented, but because they teemed with malaria, and were very deadly during the greater portion of the year, and more particularly immediately after the rainy seasons, when the atmosphere is usually clearest and most favourable for distant observations. At first tracts of forest, covering extensive plains, were considered impracticable; thus Lambton carried his network over the open country, and stopped it whenever it reached a great plain covered with forest and devoid of hills; but Everest's system would not permit of any break of continuity, nor the abandonment of any chain which was required to complete a gridiron; it has been carried out in all its integrity, often with much sacrifice of life, but never with any shrinking on the part of the survey officers from carrying out what it had become a point of honour with them to accomplish, and the accomplishment of which the Government had come to regard as a matter of course. We have already seen how the progress of Everest's first chain of triangles was suddenly arrested because he and all his people were struck down by malaria in the pestilential regions of the Godavery basin. That chain remained untouched for fifty years; it was then resumed and completed, but with the loss of the executive officer, Mr. George Shelverton, who succumbed when he had not yet reached, but was within sight of, the east coast line, the goal towards which his labours were directed. Many regions, as the ba in of the Mahanaddi, the valley of Assam, the hill ranges of Tipperah, Chittagong, Arracan, and Burma, and those to the east of Moulmein and Tennasserim, which form the boundary between the British and the Siamese territories, are covered with dense forest, up to the summits of the peaks which had to be adopted as the sites of the survey stations. As a rule the reaks were far from the nearest habitation, and they could not be reached until pathways to them had been cut through forests tangled with a dense undergrowth of tropical jungle; not unfrequently large areas had to be cleared on the summits to open out the view of the surrounding country. Here the physical difficulties to be overcome were very considerable, and they were increased by the necessity that arose, in almost every instance, of importing labourers from a great distance to perform the necessary clearances. But the broad belt of forest tract known as the Terai, which is situated in the plains at the feet of the Nepalese Himalayas, was the most formidable region of all, because the climate was very deadly for a great portion of the year, and more particularly during the season when the atmosphere was most favourable for the observations, though the physical difficulties were not so great as in the hill tracts just mentioned, and labour was more easily procurable. Lying on the British frontier, at the northern extremities of no less than ten of the meridional chains of triangles, it had necessarily to be operated in to some extent, and Everest wished to carry the several chains across it, on to the outer Himalayan range, and then to connect them together by a longitudinal chain running along the range from east to west, completing the gridiron in this quarter. But the range was a portion of the Nepalese territories, and all Europeans—excepting those attached to the British embassy at Khatmandu—were debarred from entering any part of Nepal, by treaty with the British Government. Everest hoped that the rulers of Nepal might make an exception in his favour for the prosecution of a scientific survey; and when he found they would not, he urged the Government to compel them to give his surveyors access, at least, to their outlying hills; but he urged in vain, for the Government would not run the risk of embarking in a war with Nepal for purely scientific purposes. Thus the connecting chain of triangles—now known as the N.E. Longitudinal Series—had to be carried through the whole length of the Terai, a distance of about 500 miles, which involved the construction of over 100 towers—raised to a height of about 30 feet to overlook the earth's curvature—and the clearance of about 2,000 miles of line through forest and jungle to render the towers mutually visible. It required no small courage on Everest's part to plunge his surveyors into this region; he endeavoured to minimise the risks as much as possible by taking up the longitudinal chain in sections, bit by bit, on the completion of the successive meridional chains, and thus apportioning it between several survey parties, each operating in the Terai for a short time, instead of assigning it to a single party to execute continuously from end to end, as all the other chains of triangles. But notwithstanding these precautions, the peril was great, and the mortality among both officers and men was very considerable; greater than in many a famous battle, says Mr. Clements Markham, in an eloquent passage in his Memoir of the Indian Surveys, in which he claims for the surveyors who were employed on these operations—with no hope of reward other than the favourable notice of their immediate chief and colleagues—merit for more perilous and honourable achievement than much of the military service which is plentifully rewarded by the praises of men and prizes of all kinds.

Everest retired in 1843, and was succeeded by Waugh, who applied himself energetically to the completion of the several chains of triangles exterior to the Great Arc, for which he obtained a substantial addition to the existing equipment of great theodolites. It was under him that the formidable longitudinal series through the Terai, which had been begun by Everest, was chiefly carried out. He personally initiated the determination of the positions and heights of the principal snow peaks of the Himalayan ranges; and he did much for the advancement of the general topography of India, which had somewhat languished under his predecessor, who had devoted himself chiefly to the geodetic operations. He retired in 1861, and I succeeded to the charge of the Great Trigonometrical Survey. The last chain of the principal triangulation was completed in 1882, shortly before my own retirement.

Of the general character of the operations, it may be asserted without hesitation that a degree of accuracy and precision has been attained which has been reached by few and surpassed by none of the great national surveys carried out in other parts of the world, and which leaves nothing to be desired even for the requirements of geodesy; a very considerable majority of the principal angles have been measured with the great 24-inch and 36-inch theodolite, and their theoretical probable error averages about a quarter of a second; of the linear measurements the probable error, so far as calculable, may be taken as not exceeding the two-millionth part of any measured length. And as regards the extent of the triangulation, if we ignore the primary network in Southern India, and all secondary triangulation, however valuable for geographical purposes, we still have a number of principal chains—meridional, longitudinal, and oblique—of which the aggregate length is 17,300 miles, which contain 9,230 first-class angles all observed, and rest on eleven base-lines measured with the Colby apparatus of compensation bars and microscopes. This prodigious amount of field-work furnishes an enormous mass of interdependent angular and linear measures; and each of these is fallible in some degree, for, great as was the accuracy and care with which they had severally been executed, perfect accuracy of measurement is as yet beyond human achievement; thus every circuit of triangles, every chain closing on a base-line, and even every single triangle, presented discrepancies the magnitude of which was greater or less according as derived from a combination of many, or only of a few, of the fallible facts of observation. Thus, when the field operations were approaching their termination, the question arose as to how these facts were to be harmonised and rendered consistent throughout, which was a very serious matter considering their great number. The strict application of mathematical theory to a problem of this nature requires the adjustment to be effected by the application of a correction to every fact of observation, not arbitrarily, but in such a manner as to give it its proper weight, neither more nor less, in the final investigation, and in this the whole of the facts must be treated simultaneously. That would have involved the simultaneous solution of upwards of 4,000 equations between 9,230 unknown quantities, by what is called the method of minimum squares, and I need scarcely say that it is practically impossible to solve such a number of equations between so many unknown quantities by any method at all. Thus a compromise had to be made between the theoretically desirable and the practically possible. It would be out of place here to attempt to describe the method of treatment which was eventually adopted, after much thought and deliberation; I will merely say that the bulk of the triangulation was divided into five sections, each of which was treated in succession with as close approximation to the mathematically rigorous method as was practically possible; but even then the mass of simultaneous interdependent calculation to be performed in each instance was enormous, I believe greatly exceeding anything of the kind as yet attempted in any other survey. But the happy result of all this labour was that the final corrections of the angles were for the most part very minute, less than the theoretical probable errors of the angles, and thus fairly applicable without taking any liberties with the facts of observation. If the attribute of beauty may ever be bestowed on such things as small numerical quantities, it may surely be accorded to these notable results of very laborious calculations, which, while in themselves so small, were so admirably effective in introducing harmony and precision throughout the entire triangulation.

If now we turn once more to what Lambton calls "the sublime science of geodesy," which was held in such high regard by both him and Everest, we shall find that the great meridional arc between Cape Comorin and the Himalayas, on which they laboured with so much energy and devotion, is not the only contribution to that science to which the Indian triangulation is subservient, but every chain of triangles—meridional, longitudinal, or oblique—may be made to throw light either on geodesy, the science of the figure of the earth, or on geognosy, the science of the earth's interior structure, when combined with corresponding astronomical arcs of amplitude. Thus each of the several meridional chains of triangles may be utilised in this way, as their prototype has been, by having latitude observations taken at certain of their stations to give meridional arcs; and the several longitudinal chains of triangles may also be utilised—in combination with the main lines of telegraph—by electro-telegraphic determinations of differential longitudes to give arcs of parallel. When the stations of the triangulation which are resorted to for the astronomical observations are situated in localities where the normal to the surface coincides fairly with the corresponding normal to the earth's figure, the result is valuable as a contribution to geodesy; when the normal to the surface is sensibly deflected by local attraction, the result gives a measure of the deflection which is valuable as a contribution to geognosy.

Having regard to these circumstances, I moved the Government to supply the Trigonometrical Survey with the necessary instruments for the measurement of the supplemental astronomical arcs; and as officers became available on the gradual completion of the successive chains of triangles, I employed some of them in the required determinations of latitude and differential longitude. It so happened that about the same time geodesists in Europe began to recognise the advantages to science to be acquired by connecting the triangulations of the different nationalities together, and supplementing them with arcs of amplitude. The "International Geodetic Association for the Measurement of Degrees in Europe" was formed in consequence, and it has been, and is still, actively employed in carrying out this object; in India, however, the triangulation was complete and connected throughout, so that only the astronomical amplitudes were wanting. They are still in progress, but already meridional chains, aggregating 1,840 miles in length, and lying to the west of the Great Arc, have been converted into meridional arcs; and the three longitudinal chains, from Madras to Mangalore, from Bombay to Vizagapatam, and from Kurrachee *via* Calcutta to Chittagong, of which the aggregate length is 2,600 miles, have been converted into arcs of parallel. In the former the operations follow the meridional course of the chains of triangles; in the latter they follow the principal lines of the electric telegraph, which sometimes diverge greatly from the direction of the longitudinal chains of triangles, the two only intersecting at occasional points; the astronomical stations are therefore placed at the trigonometrical points which may happen to be nearest the telegraph lines, whether on the meridional or on the longitudinal chains, and their positions are invariably so selected as to form self-verificatory circuits which are usually of a triangular form, presenting three differential arcs of longitude; each of these arcs is measured independently as regards the astronomical work—though for the third arc there is usually no independent telegraph line, but only a coupling of the lines for the first and second arcs—and this has been proved to give such an excellent check on the accuracy of the operations, that it is not too much to say that no telegraphic longitude operations are entirely reliable which have not been verified in some such manner.

Through the courtesy of Colonel Stotherd, Director-General of the Ordnance Survey, I am enabled to exhibit two charts, one of the triangulation of India, the other of that of Europe, which have recently been enlarged to the same scale in the Ordnance Survey Office at Southampton for purposes of comparison. The first is taken from the official chart of the Indian Survey, and shows the great meridional and longitudinal chains and Lambton's network of principal triangles, the positions of the base-lines measured with the Colby apparatus, the latitude and the differential longitude stations, the triangular circuits of the longitudinal arcs, the stations of the pendulum and the tidal operations which will be noticed presently, and the secondary triangulations to fix the peaks of the Himalayan and Sulimani ranges, and the positions of Bangkok in Siam and Kandahar in Afghanistan, the extreme eastern and western points yet reached. The chart of the European triangulation has been enlarged from one published by the International Geodetic Association of Europe; in it special prominence is given to the Russian meridional arc, which extends from the Danube to the Arctic Ocean, and is 25° 20′ in length, and to the combined English and French meridional arc, 22° 10′ in length, which extends from the Balearic Island of Formentera in the Mediterranean, to Saxavord in the Shetland Islands. The aggregate length of the meridional arcs already completed in India is about equal to that of the English, French, and Russian arcs combined; but the longest in India is about $1\frac{1}{2}$° shorter than the Russian. As regards longitudinal arcs, I believe the two which were first measured in India, and were employed shortly afterwards by Colonel Clarke in his last investigation of the figure of the Earth, are the only ones which have as yet been deemed sufficiently accurate to be made use of in such investigations, though arcs of much greater length have been measured in Europe. It would be interesting, if time permitted, to set forth the salient points of divergence between the systems of the Indian and the European surveys; I will only mention that in the southern part of the Russian arc, for a space of about 8° from the Duna to the Dneister, a vast plain, covered with immense and almost impenetrable forests, presented great obstacles to the prosecution of the work; the difficulty was overcome by the erection of a large number of lofty stations of observation, wooden scaffoldings which were 120 and even as much as 146 feet high, to overlook the forests. In Indian forests, as the Terai on the borders between British and Nepalese territories, the stations were rarely raised to a greater height than 30 feet, or just sufficient to overtop the curvature, and all trees and other obstacles were cleared away on the lines between them; this was found the most expeditious and economical process. The stations were very substantial, with a central masonry pillar, for the support of a great theodolite, which was isolated from the surrounding platform for the support of the observer. The lofty Russian scaffoldings only sufficed for small theodolites, and they were so liable to shake and vibration, that the theodolites had to be fitted with two telescopes to be pointed simultaneously by two observers at the pair of stations, the angle between which was being measured.

All the modern geodetic data of the Indian survey that were available up to the year 1880 were utilised by Colonel A. R. Clarke, C.B., of the Ordnance Survey, in the last of the very valuable investigations of the Figure of the Earth which he has undertaken from time to time. It will be obvious that new data tend to modify in some degree the conclusions derived from previous data, for the figure of so large a globe as our earth is not to be exactly determined from measurements carried over a few narrow belts of its superficies. Thus thirty years ago it was inferred that the equator was sensibly elliptic—and not circular, as had been generally assumed—with its major axis in longitude 15° 34′ east of Greenwich; but later investigations indicate a far smaller ellipticity, and place the major axis in west longitude 8° 15′. More significant evidence of the influence of new facts of observation in modifying previous conclusions is furnished by the French national standard of length, the metre, which was fixed at the ten-millionth part of the length of the earth's meridional quadrant, as deduced from the best geodetic data available up to the end of the last century; but it is now found to be nearly $\frac{1}{5000}$th part less than the magnitude which it is supposed to represent, the difference being about a hundred times greater than what would now be considered an allowable error in an important national standard of measure.

The Indian survey has also made valuable contributions to geodesy and geognosy in an elaborate series of pendulum observations for determining variations of gravity, which throws light both on the grand variation from the poles to the equator that governs the ellipticity, and on the local and irregular variations depending on the constitution of the interior of the earth's crust. They were commenced in 1865 by Captain J. P. Basevi, on the recommendation of General Sabine and the Council of the Royal Society, with two pendulums, one of which the General had swung in his notable operations which extend from a little below the equator to within 10° of the pole. Captain Basevi had nearly completed the operations in India, and had taken swings at a number of the stations of the Great Arc and at various other points near mountain ranges and coast lines, when he died of exposure in 1871 at a station on the high table-lands of the Himalayas, while investigating the force of gravity under mountain ranges. Major Heaviside swung the pendulums at the remaining Indian stations, then at Aden and Ismailia on the way back to England, and finally at the base station, the Kew Observatory. Afterwards they and a third pendulum were swung at Kew and Greenwich by Lieutenant-Colonel Herschel, who took all three to America, swung them at Washington, and then handed them over to officers of the United States Coast Survey, by whom they have been swung at San Francisco, Auckland, Sydney, Singapore, and in Japan.

The pendulum operations in India have been successful in removing from the geodetic operations the reproach which had latterly been cast on them, that their value has become much diminished since the discovery that the attraction of the Himalayan mountains is so much greater than had previously been suspected, that it may have materially deflected the plumb-line at a large number of the astronomical stations of the Great Arc, and injuriously influenced the observations. Everest considered the effects of the Himalayan attraction to be immaterial at any distance exceeding sixty miles from the feet of the mountains; but in his days the full extent and elevation of the mountain masses was unknown, and their magnitude was greatly underestimated. Afterwards, when the magnitude became better known, Archdeacon Pratt of Calcutta, a mathematician of great eminence, calculated that they would materially attract the plumb-line at points many hundred miles distant; he also found that everywhere between the Himalayas and the ocean, the excess of density of the land of the continent as compared with the water of the ocean would combine with the Himalayan attraction and increase the deflection of the plumb-line northwards, towards the great mountain ranges, and that under the joint influence of the Himalayas and the ocean the level of the sea at Kurrachee would be raised 560 feet above the level at Cape Comorin.

But as a matter of fact the Indian arc gave a value of the earth's ellipticity which agreed sufficiently closely with the values derived from the arcs measured in all other quarters of the globe, to show that it could not have been largely distorted by deflections of the plumb-line; thus it appeared that whereas Everest might have slightly underestimated the Himalayan attraction, Pratt must have greatly overestimated it. His calculations were however based on reliable data, and were indubitably correct. For some time the contradiction remained unexplained, but eventually Sir George Airy put forward the hypothesis that the influence of the Himalayan masses must be counteracted by some compensatory disposition of the matter of the earth's crust immediately below them, and in which they are rooted; he suggested that the bases of the mountains had sunk to some depth into a fluid lava which he conceived to exist below the earth's crust, and that the sinking had caused a displacement of dense matter by lighter matter below, which would tend to compensate for the excess of matter above. Now Pratt's calculations had reference only to the visible mountain and oceanic masses, and their attractive influences—the former positive, the latter negative—in a horizontal direction; he had no data for investigating the density of the crust of the earth below either the mountains on the one hand, or the bed of the ocean on the other. The pendulum observations furnished the first direct measures of the vertical force of gravity in different localities which were obtained, and these measures revealed two broad facts regarding the disposition of the invisible matter below; first, that the force of gravity diminishes as the mountains are approached, and is very much less on the summit of the highly elevated Himalayan table-lands than can be accounted for otherwise than by a deficiency of matter below; secondly, that it increases as the ocean is approached, and is greater on islands than can be accounted for otherwise than by an excess of matter below. Assuming gravity to be normal on the coast lines, the mean observed increase at the island stations was such as to cause a seconds' pendulum to gain three seconds daily, and the mean observed decrease in the interior of the Continent would have caused the pendulum to lose $2\frac{1}{2}$ seconds daily at stations averaging 1,200 feet above the sea level, 5 seconds at 3,800 feet, and about 22 seconds at 15,400 feet—the highest elevation reached—in excess of the normal loss of rate due to height above the sea.

Pratt was strongly opposed to the hypothesis of a substratum, or magma, of fluid igneous rock beneath the mountains; he assumed the earth to be solid throughout, and regarded the mountains as an expansion of the invisible matter below, which thus becomes attenuated and lighter than it is under regions of less elevation, and more particularly in the depressions and contractions below the bed of the ocean. And certainly we seem to have more reason to conclude that the mountains emanate from the subjacent matter of the earth's crust than that they are as wholly independent of it as if they were formed of stuff shot from passing meteors and asteroids; any severance of continuity and association between the visible above and the invisible below appears, on the face of it, to be decidedly improbable.

The hypothesis of sub-continental attenuation and sub-oceanic condensation of matter is supported by the two arcs of longitude on the parallels of Madras and Bombay; for at the extreme points of these arcs, which are situated on the opposite coast lines, the horizontal attraction has been found to be not landwards, as might have been anticipated, but seawards, showing that the deficient density of the sea as compared with the land is more than compensated by the greater density of the matter under the ocean than of that under the land.

While on the subject of the constitution of the earth's crust, I may draw attention to the circumstance that the tidal observations which have been carried on at a number of points on the coasts of India, as a part of the operations of the Survey, tend to show that the earth is solid to its core, and that the geological hypothesis of a fluid interior is untenable. They have been analysed by Prof. G. H. Darwin, with a view to the determination of a numerical estimate of the rigidity of the earth, and he has ascertained that whilst there is some evidence of a tidal yielding of the earth's mass, that yielding is certainly small, and the effective rigidity is very considerable, not so great as that of steel, as was at first surmised, but sufficient to afford an important confirmation of the justice of Sir William Thomson's conclusion as to the great rigidity.

The Indian pendulum observations have been employed by Colonel Clarke, in combination with those taken in other parts of the globe, to determine the earth's ellipticity. Formerly there was wont to be a material difference between the ellipticities which were respectively derived from pendulum observations and direct geodetic measurements, the former being somewhat greater than $\frac{1}{290}$, the latter somewhat less than $\frac{1}{300}$; but as new and more exact data became available, the values derived from these two essentially independent sources became more and more accordant, and they now nearly agree in the value $\frac{1}{293}$.

As a part of the pendulum operations, a determination of the length of the seconds' pendulum was made at Kew by Major Heaviside, with the pendulum which had been employed for the same purpose by Kater early in the present century, when leading men of science in England believed that in the event of the national standard yard being destroyed or lost, the length might be reproduced at any time with the aid of a reversible pendulum. In consequence of this belief an Act of Parliament was passed in 1824 which defined the relations between the imperial and the seconds' pendulum, the length of the former being to that of the latter—swung in the latitude of London, in a vacuum and at the level of the sea—in the proportion of 36 inches to 39·1393 inches. Thus, while the French took for their unit of length the ten-millionth part of the earths' meridional quadrant, the English took the pendulum swinging seconds in the latitude of London. In case of loss the yard is obviously recoverable more readily and inexpensively by reference to the pendulum than the metre by reference to the quadrant; it is also recoverable with greater accuracy; still the accuracy is not nearly what would now be deemed indispensable for the determination of a national standard of length, and it is now generally admitted that every pendulum has certain latent defects, the influence of which cannot be exactly ascertained. Thus the instrument cannot be relied on as a suitable one for determinations of absolute length; but, on the other hand, so long as its condition remains unaltered, it is the most reliable instrument yet discovered for differential determinations of the variations of gravity. In truth, however, the pendulum is a very wearisome instrument to employ even for this purpose, for it has to be swung many days and with constant care and attention to give a single satisfactory determination; thus if such a thing can be invented and perfected as a good differential gravity meter, light and portable, with which satisfactory results can be obtained in a few hours, instead of many days, the boon to science will be very great.

The trigonometrical operations fix with extreme accuracy two of the co-ordinates—the latitude and longitude—which define the positions of the principal stations; but the third co-ordinate, the height, is not susceptible of being determined by such operations with anything like the same degree of accuracy, because of the variations of refraction to which rays of light passing through the lower strata of the atmosphere are liable, as the temperature of the surface of the ground changes in the course of the day. In the plains the apparent height of a station ten to twelve miles from the observer has been found to be upwards of 100 feet greater in the cool of the night than in the heat of the day, the refraction being always positive when the lower atmospheric strata are chilled and laden with dew, and negative when they are rarefied by the heat radiated from the surface of the ground. At hill stations the rays of light usually pass high above the surface of the ground, and the diurnal variations of refraction are comparatively immaterial, and very good results are obtained by the expedient of taking the vertical observations between reciprocating stations at the same hour of the day, and as nearly as possible at the time of minimum refraction; but in the plains this expedient does not usually suffice to give reliable results. The hill ranges of central and those of northern India are separated by a broad belt of plains, which embraces the greater portion of Sind, the Punjab, Rajputana, and the valley of the Ganges, and is crossed by a very large number of the principal chains of triangles, on the lines where the chart shows stretches of comparatively small triangles, which are in most instances of considerable length. Thus it became necessary to run lines of spirit levels over these plains, from sea to sea, to check the trigonometrical heights. The opportunity was taken advantage of to connect all the levels which had been executed for irrigation and other public works, and reduce them to a common datum; and eventually lines of level were carried along the coast and from sea to sea to connect the tidal stations. The aggregate length of the standard lines of level executed up to the present time is nearly 10,000 miles, and an extensive series of charts of the levels derived from other departments of the public service and reduced to the survey datum has already been published.

The survey datum which has been adopted for all heights, whether deduced trigonometrically or by spirit-levelling, is the mean sea level as determined, either for initiation or verification, by tidal observations at several points on the coast lines. At first the observations were restricted to what was necessary for the requirements of the survey, and their duration was limited to a lunar month at each station. In 1872 more exact determinations were called for, to ascertain whether gradual changes in the relative level of land and sea were taking place at the head of the Gulf of Cutch, as had been surmised by the geological surveyors, and observations were taken for over a year at three tidal stations on the coasts of the gulf, to be repeated hereafter when a sufficient period had elapsed to permit of a measurable change of level having taken place. Finally, in 1875, the Government intimated that as "the great scientific advantages of a systematic record of tidal observations on Indian coasts had been frequently urged and admitted," such observations should be taken at all the principal ports and at such points on the coast lines as were best suited for investigations of the laws of the tides. In accordance with these instructions, five years' observations have been made at several points, and new stations are taken up as the operations at the first ones are completed.

The initiation of the later and more elaborate operations is due in great measure to the recommendations of the Tidal Committee of the British Association, of which Sir William Thomson was President. The tidal observations have been treated by the method of harmonic analysis advocated by the Committee. The constants for amplitude and epoch are determined for every tidal component, both of long and of short periods, and with their aid tide-tables are now prepared and published annually for each of the principal ports; and further, it is with them that Prof. G. H. Darwin made the investigations of the effective rigidity of the earth, which I have already mentioned. The very remarkable waves which were caused by the earthquake on December 31, 1881, in the Bay of Bengal, and by the notable volcanic eruptions in the island of Krakatoa and the Straits of Sunda on August 27 and 28, 1883, were registered at several of the tidal stations, and thus valuable evidence has been furnished of the velocities of both the earth-wave and the ocean-wave which are generated by such disturbances of the ordinarily quiescent condition of the earth's crust.

I must not close this account of the non-graphical, or more purely scientific, operations of the great Trigonometrical Survey of India without saying something of the officers who were employed thereon, under the successive superintendence of Everest, Waugh, and myself. A considerable majority were military, from all branches of the army—the cavalry and infantry, as well as the corps of engineers and artillery; the remainder were civilians, mostly promoted from the subordinate grades. Prominent shares in the operations were taken by Lieutenant Renny, Bengal Engineers, afterwards well known in this neighbourhood as Colonel Renny Tailyour, of Borrowfield in Forfarshire, of whom and his contemporary, Lieutenant Waugh, Everest, retiring, reported in terms of the highest commendation; by Reginald Walker, of the Bengal Engineers, George Logan, George Shelverton, and Henry Beverley, all of whom fell victims to jungle fever; by Strange, F.R.S., of the Madras Cavalry, whose name is associated with the construction of the modern geodetic instruments of the Survey; by Jacob—afterwards Government Astronomer at Madras—Rivers and Haig, all of the Bombay Engineers; Tennant, C.I.E., F.R.S., Bengal Engineers, afterwards Master of the Mint in Calcutta; Montgomerie, F.R.S., of the Bengal Engineers, whose name is best remembered in connection with the Trans-Himalayan geographical operations; James Basevi, of the Bengal Engineers, who so sadly died of exposure while engaged on the pendulum operations in the higher Himalayas; Branfill, of the Bengal Cavalry; Thuillier, Carter, Campbell, Trotter, Heaviside, Rogers, Hill, and Baird, F.R.S., all engineer officers; also Hennessey, C.I.E., F.R.S., M.A., Herschel, F.R.S., and Cole, M.A., whose names are intimately associated with the collateral mathematical investigations and the final reduction of the principal triangulation.

The Trigonometrical Survey owes very much to the liberal and even generous support which it has invariably received from the Supreme Government, with the sanction and approval, first of the Directors of the East India Company, and afterwards of the Secretary of State for India. In times of war and financial embarrassment the scope of the operations has been curtailed, the establishments have been reduced, and some of the military officers sent to join the armies in the field; but whatever the crisis, the operations have never been wholly suspended. Even during the troubles of 1857-58, following the mutiny of the native army, they were carried on in some parts of the country, though arrested in others; and the then Viceroy, Lord Canning, on receiving the reports of the progress of the operations during that eventful period, immediately acknowledged them to the Surveyor-General, Colonel Waugh, in a letter from which the following extract is taken:

"I cannot resist telling you at once with how much satisfaction I have seen these papers. It is a pleasure to turn from the troubles and anxieties with which India is still beset, and to find that a gigantic work, of permanent peaceful usefulness, and one which will assuredly take the highest rank as a work of scientific labour and skill, has been steadily and rapidly progressing through all the turmoil of the last two years."

The operations have been uninfluenced by changes of *personnel* in the administration of the Indian Empire, as Governor-Generals and Viceroys succeeded each other, but have met with uniform and consistent support and encouragement. It may well be doubted whether any similar undertaking, in any other part of the world, has been equally favoured and as munificently maintained.

In conclusion I must state that I have purposely said nothing of the graphical operations executed in the Trigonometrical and other branches of the Survey of India, because they are more generally known, their results appear in maps which speak for themselves, and time would not permit of my attempting to describe them also. They comprise, *first*, the general topography of all India, mostly on the standard scale of 1 inch to the mile; *secondly*, geographical surveys and explorations of regions beyond the British frontier, notably such as are being carried on at the present time on the Russo-Afghan frontier, by Major Holdich and other officers of the Survey; *thirdly*, the so-called Revenue Survey of the British districts in the Bengal Presidency, which is simply a topographical survey on an enlarged scale—4 inches to the mile—showing the boundaries and areas of villages for fiscal requirements; and *fourthly*, the Cadastral Survey of certain of the British districts in the Bengal Presidency, showing fields and the boundaries of all properties, on scales of 16 to 32 inches to the mile. There are also certain large scale surveys of portions of British districts in the Madras and Bombay Presidencies, which, though undertaken originally for purely fiscal purposes by revenue and settlement officers working independently of the professional survey, have latterly been required to contribute their quota to the general topography of the country. And of late years a survey branch has been added to the Forest Department, to provide it with working maps constructed for its own requirements on a larger scale than the standard topographical scale, but on a trigonometrical basis, and in co-operation with the Survey Department. But this brief capitulation gives no sort of idea of the vast amount of valuable topographical and other work for the requirements of the local Administrations and the public at large—always toilsome, often perilous—which has been accomplished, quite apart from and in quantity far exceeding the non-graphical and more purely scientific work which I have been describing. Its magnitude and variety are such that a mere list of the officers who have taken prominent shares in it, from first to last, would be too long to read to you. Three names, however, I must mention: *first*, that of General Sir Henry Thuillier, who became Surveyor-General on the same day that I succeeded to the superintendence of the Great Trigonometrical Survey, and with whom I had the honour of co-operating for many years; under his administration a much larger amount of topography was executed than under any of his predecessors, and a great impetus was given to the lithographic, photographic, engraving and other offices in which the maps of the survey are published; *secondly*, that of Colonel Sconce, who became Deputy Surveyor-General soon after my accession in 1878 to the Survey-Generalship, and with whom I was associated for some years, much to my gratification and advantage, in various matters, but more particularly in the establishment of cadastral surveys on a professional basis at a moderate cost, to render them more generally feasible, which was a matter of the utmost importance for the administration of the more highly populated portions of the British provinces; and *thirdly*, that of Lieutenant-Colonel Waterhouse, who has for many years superintended the offices in which photography is employed, in combination with zincography and lithography, for the speedy reproduction *en masse* of the maps of the Survey, and has done much to develop the art of photogravure, whereby drawings in brushwork and mezzotint may be reproduced with a degree of excellence rivalling the best copperplate engraving, and almost as speedily and cheaply as drawings in pen and ink work are reproduced by photo-zincography.

Mr. Clements Markham's Memoir on the Indian Surveys gives the best account yet published of the several graphical surveys up to the year 1878. In that year the Trigonometrical, Topographical, and the Revenue branches, which up to that time had constituted three separate and almost independent departments, were amalgamated together into what is now officially designated "the Survey of India." In the same year the chronicle so well commenced by Mr. Markham came to an end on his retirement from the India office—unfortunately, for it is a work of excellence in object and in execution, and most encouraging to Indian surveyors, who find their labours recorded in it with intelligent appreciation and kindly recognition.

During the present meeting, several papers by officers of the Survey will be read—one by Colonel Barron, in person, on the cadastral surveys in the organisation of which he has taken a leading share; by Major Baird, on the work of spirit-levelling, which he superintends conjointly with the tidal observations; by Colonel Godwin Austen, on Lieutenant-Colonel Woodthorpe's recent journey from Upper Assam to the Irawadi river; by Colonel Branfill, on the physical geography of Southern India; and by Colonel Tanner, on portions of the Himalayas, and on recent explorations in Southern Tibet. Major Bailey will also read a paper or the forest surveys.

SECTION G

MECHANICAL SCIENCE

Opening Address by B. Baker, M.Inst.C.E., President of the Section

Two hundred and fifty-seven Presidential Addresses of one kind and another have been delivered at meetings of the British Association since the members last mustered at Aberdeen. I need hardly say that the candid friend who informed me of this interesting fact most effectually dispelled any illusion I may have previously entertained as to the possibility of preparing an address of sufficient novelty and suggestiveness to be worthy of your attention, and I can only hope that any shortcomings will be dealt with leniently by you. One compensating advantage obviously belongs to my late appearance in the field—I have 257 models of style upon which to frame my address. My distinguished predecessor, Sir Frederick Bramwell, has a style of his own, in which wit and wisdom are combined in palatable proportions; but were I to attempt this style I should doubtless incur the rebuke which a dramatic critic of Charles the Second's time administered to a too ambitious imitator of a popular favourite: "He's got his fiddle, but not his hands to play on't." I must search further back than last year, therefore, for a model of style, and the search reminds me that I labour under a double disadvantage: firstly, that only two addresses intervene between the present one and that of my partner, Mr. John Fowler, with whom I have so long had the honour of being associated, and whose professional experiences, as set forth in his address, are necessarily so largely identical with my own; and, secondly, that within the same period I have read before this Section two somewhat lengthy papers on the work which is at present chiefly engaging the attention of Mr. Fowler and myself—the great Forth Bridge.

Although, for the reasons aforesaid, I am conscious that my address may fail in novelty, I cannot honestly profess to feel a difficulty in preparing an address of some kind, for the subjects embraced under the head of "Mechanical Science" are so inexhaustible that even the youngest student might safely accept the responsibility of speaking for an hour on some of them. Prof. Rankine, addressing you thirty years ago, said it was well understood that questions of pure or abstract mechanics form no part of the subjects dealt with in this Section. With characteristic clearness of conception and precision of language he told you what the term "mechanical science" meant, and, after thirty years' interval, his words may be recalled with advantage to every one proposing to prepare an address or report for this Section. "Mechanical science," said Prof. Rankine, "enables its possessor to plan a structure or machine for a given purpose without the necessity of copying some existent example; to compute the theoretical limit of the strength and stability of a structure or the efficiency of a machine of a particular kind; to ascertain how far an actual structure or machine fails to attain that limit, and to discover the cause and the remedy of such shortcoming; to determine to what extent, in laying down principles for practical use, it is advantageous for the sake of simplicity to deviate from the exactness required by pure science; and to judge how far an existing practical rule is founded on reason, how far on custom, and how far on error." There is thus an ample text for many discourses; but, as I am not writing a treatise on engineering, but merely delivering a brief address, I will confine my attention at present to a particular case of the branch of mechanical science referred to in the last clause of Prof. Rankine's definition, and will ask you to consider how far the existing practical rules respecting the strength of metallic bridges are "founded on reason, how far on custom, and how far on error."

The first question obviously is, What are the rules adopted by engineers and Government departments at the present time?—and it is one not easily answered. I have for some time past been receiving communications from leading Continental and American engineers, asking me what is my practice as regards the admissible intensity of stress on iron and steel bridges, and in replying I have invited similar communications from themselves. As a result I am able to say that at the present time absolute chaos prevails. The old foundations are shaken, and engineers have not come to any agreement respecting the rebuilding of the structure. The variance in the strength of existing bridges is such as to be apparent to the educated eye without any calculation. If the wheels of a miniature brougham were fitted to a heavy cart the incident would excite the derision even of our street boys, and yet equal want of reason and method is to be found in hundreds of bridges in all countries. It is an open secret that nearly all the large railway companies are strengthening their bridges, and necessarily so, for I could cite cases where the working stress on the iron has exceeded by 250 per cent. that considered admissible by leading American and German bridge-builders in similar structures.

In the case of old bridges the variance in strength is often partly due to errors in hypothesis and miscalculation of stresses. In the present day engineers of all countries are in accord as to the principles of estimating the magnitude of the stresses on the different members of a structure, but not so in proportioning the members to resist those stresses. The practical result is that a bridge which would be passed by the English Board of Trade would require to be strengthened 5 per cent. in some parts and 60 per cent. in others before it would be accepted by the German Government or by any of the leading railway companies in America. This undesirable state of affairs arises from the fact that in our own and some other countries many engineers still persistently ignore the fact that a bar of iron may be broken in two ways—namely, by the single application of a heavy stress or by the repeated application of a comparatively light stress. An athlete's muscles have often been likened to a bar of iron, but, if "fatigue" be in question, the simile is very wide of the truth. Intermittent action—the alternative pull and thrust of the rower, or of the labourer turning a winch—is what the muscle likes and the bar of iron abhors. Troopers dismount to rest their horses, but to relieve a bar of iron temporarily of load only serves to fatigue it. Half a century ago Braithwaite correctly attributed the failure of some girders, carrying a large brewery vat, to the vessel being sometimes full and sometimes empty, the repeated deflection, although imperceptibly slow and wholly free from vibration, deteriorating the metal, until, in the course of years, the girders broke. These girders were of cast-iron; but it was equally well known that wrought-iron was similarly affected, for in 1842 Nasmyth called the attention of this Section to the fact that the "alternate strain" in axles rendered them weak and brittle, and suggested annealing as a remedy, he having found that an axle which would snap with one blow when worn would bear eighteen blows when new or after being annealed.

So important a matter as the action of intermittent stresses could not escape the attention of the Royal Commissioners appointed in 1849 to consider the application of iron to railway structures, and some significant and sufficiently conclusive experiments were made by Capt. Douglas Dalton and others. Cast-iron bars 3 inches square and 13 feet 6 inches span between the supports were deflected, both by the slow action of a cam and the percussive action of a swinging pendulum weight. When the deflection was that due to one-third of the breaking weight, about 50,000 successive bendings by the cam broke one of the bars, and about 1000 blows from the pendulum another. When the deflection was increased from one-third to one-half, about 500 applications of the cam, and 100 blows, sufficed to rupture two of the specimens. Slow-moving weights on bars and on a small wrought-iron box girder gave analogous results; and the deduction drawn by the experimenters at the time was that "iron bars scarcely bear the reiterated application of one-third the breaking weight without injury, hence the prudence of always making beams capable of bearing six times the greatest weight that could be laid upon them."

Although these experiments were entirely confirmatory of all previous experience, they would appear to have little influenced the practice of engineers, since Fairbairn, more than ten years later, in a communication to this Section, said that opinions were still much divided upon the question whether the continuous change of load which many wrought-iron structures undergo has any permanent effect upon their ultimate powers of resistance. To assist in settling the question he communicated to the Association the results of some experiments carried out by himself and Prof. Unwin on a little riveted girder 20 feet span and 16 inches deep. Once more the same important but disregarded facts were enforced on the attention of engineers. About 5000 applications of a load equal to four-tenths of the calculated breaking load fractured the beam with the small ultimate deflection of three-eighths of an inch, and subsequently, when repaired, the beam broke with one-third of the load and a deflection of but a quarter of an inch, which sufficiently indicated how small a margin the factor of safety of four, when currently adopted, allowed for defective manufacture, inferior material, and errors in calculation. Still nothing was done, and the general practice of engineers and the Board of Trade regulations continued unaltered.

Soon after the introduction of wrought-iron bridges on railways, the testimony of practical working was added to that of experiments. In 1848 several girder bridges of unduly light proportions were erected in America, and one of 66 feet span broke down under the action of the rolling load in the same manner as Fairbairn's little experimental girder. Again, in early American timber bridges the vertical tie-rods were often subject to stresses oscillating between 1 ton and 10 tons per square inch and upwards. Many of these broke, as did also the suspension bolts in platforms subjected to similar stresses. In my own experience, dozens of broken flange-plates and angle-bars, and hundreds of sheared rivets, have been the silent witnesses of the destructive action of a live load. Like evidence was afforded by early constructed iron ships deficient in girder strength. Under the alternating stresses due to the action of the waves weaknesses not at first apparent would, in the course of time be developed, and additional strength, in the way of stringers and otherwise, become imperative.

If none of the preceding evidence had been forthcoming, the results of the historical series of experiments carried out by Wöhler for the Prussian Ministry of Commerce would alone be conclusive. For the first time a truly scientific method of investigation was followed, and an attempt was made to determine the laws governing the already proved destructive action of intermittent stresses. In previous experiments the bar or girder was alternately fully loaded and wholly relieved of load. Wöhler was not satisfied with this, but tested also the result of a partial relief of load. The striking fact was soon evidenced on testing specimens under varying tensions, that the amount of the variation was as necessary to be considered as that of the maximum stress. Thus, an iron bar having a tensile strength of 24 tons per square inch broke with about 100,000 applications of a stress varying from *nil* to 21 tons, but resisted 4,000,000 applications of the 21 tons when the minimum stress was varied from *nil* to $11\frac{1}{2}$ tons. The alternations of stress in the case of some test pieces numbered no less than 132,000,000; and too much credit cannot be bestowed by engineers upon Wöhler for the ingenuity and patience which characterised his researches. As a result, it is proved beyond all further question that any bar or beam of cast iron, wrought iron, or steel may be fractured by the continued repetition of comparatively small stresses, and that, as the differences of stress increase, the maximum stress capable of being sustained diminishes.

Various formulæ based upon the preceding experiments have been proposed for the determination of the proper sectional area of the members of metallic structures. These formulæ differ in some essential respects, and doubtless many experiments are still required before any universally accepted rules can be laid down. Probably at the present time the engineers who have given the most attention to the subject are fairly in accord in holding that the admissible stress per square inch in a wrought-iron girder subject to a steady dead load would be one and a half times as great as that in a girder subject to a wholly live load, and three times that allowable in members subject to alternate tensile and compressive stresses of equal intensity, such as the piston-rod of a steam-engine or the central web-bracing of a lattice girder. If the alternations of stress to be guarded against are not assumably infinite in number, but only occasional —as in wind bracing for hurricane pressures, or in a vessel amongst exceptionally high waves—then the aforesaid ratio of 3, 2, and 1 would not apply, but would more nearly approach the ratios 6, 5, and 4.

Hundreds of existing railway bridges which carry twenty trains a day with perfect safety would break down quickly under twenty trains per hour. This fact was forced on my attention nearly twenty years ago by the fracture of a number of iron girders of ordinary strength under a five-minute train service. Similarly, when in New York last year I noticed, in the case of some hundreds of girders on the "Elevated Railway," that the alternate thrust and pull on the central diagonals from trains passing every two or three minutes had developed weaknesses which necessitated the bars being replaced by stronger ones after a very short service. Somewhat the same thing had to be done recently in this country with a bridge over the Trent, but the train service being small the life of the bars was measured by years instead of months. If ships were always amongst great waves the number going to the bottom would be largely increased, for, according to Mr. John, late of Lloyd's, "many large merchant steamers afloat are so deficient in longitudinal strength that they are liable under certain conditions of sea to be strained in the upper works to a tension of from 8 to 9 tons per square inch, and to a compression of from 6 to 7 tons—stresses which the experiments already referred to proved would cause failure after a definite number of repetitions. Similarly, on taking ground or being dry-docked with a heavy cargo on board, it has been shown that vessels are liable to stresses of over 11 tons per square inch on the reverse frames, but no permanent injury results from such high stresses, because the number of repetitions is necessarily very limited.

It appears natural enough to every one that a piece even of the toughest wire should be quickly broken if bent backward and forward to a sharp angle; but, perhaps, only to locomotive and marine engineers does it appear equally natural that the same result would follow in time if the bending were so small as to be quite imperceptible to the eye. A locomotive crank axle bends but 1-34th of an inch, and a straight driving axle the still smaller amount of 1-64th of an inch under the heaviest bending stresses to which they are subject, and yet their life is limited. During the year 1883 one iron axle in fifty broke in running, and one in fifteen was renewed in consequence of defects. Taking iron and steel axles together, the number then in use on the railways of the United Kingdom was 14,848, and of these, 911 required renewal during the year. Similarly, during the past three years no less than 228 ocean steamers were disabled by broken shafts, the average safe life of which is said to be about three or four years. In other words, experience has proved that a very moderate stress alternating from tension to compression, if repeated about one hundred million times, will cause fracture as surely as a sharp bending to an angle repeated perhaps only ten times.

I have myself made many experiments with a view to elucidate the laws affecting the strength of iron- and steel-work subject to frequent alternations of stress. Perhaps the most suggestive series was one in which I subjected flat steel bars about 3 feet long, in pairs, to repeated bendings until one bar broke, and then testing the surviving bar under direct tensile and compressive stresses to ascertain to what extent the metal had deteriorated. It had come under my notice, as a practical engineer, that if the compression members of a structure were unduly weak the fact became quickly evident, perhaps under the test load; but if, on the other hand, the tension members were weak, no evidence might appear of the fact until frequent repetition of stresses during several years had caused them to fracture without any measurable elongation of the metal. In the case of crank-shafts, also, the fracture is invariably due to a tearing and not a crushing action. It appeared to me, therefore, eminently probable that repetition of stresses might be far more prejudicial to tension than to compression members, and, if so, the fact ought to be taken account of in proportioning a structure.

This proved to be the case in my experiments. For example, the companion bars to those which had broken with 18,000 reversals of a stress less than half the original breaking weight behaved, when tested as columns thirty diameters in length, precisely the same as similar bars which had done no work at all, whereas when tested in tension the elongation was reduced from the original 25 per cent. to 2·5 per cent., and the fracture appeared to indicate that the bars had been made of three different kinds of steel imperfectly welded together. With a stress reduced by one-fourth the number of bendings required to break the bars was increased to 1,200,000. In this instance the calculated maximum working stress on the extreme fibres was 43 per cent. of the direct ultimate tensile resistance of the steel, and about 30 per cent. of the stress the bar was capable of sustaining as a beam under the single application of a load. Of course, the bars failed by tension, and the extreme fibres had thus deteriorated as regards tensile stresses to the extent indicated by the above percentages. Tested as a column, however, the injury the bar had received from the 1,200,000 bendings was inappreciable. The ductility was of course very largely reduced, but ductility is a quality of comparatively little importance when a material is in compression. There is no ductility in the slender Gothic stone columns of our cathedrals, which, though heavily stressed, have carried their loads for centuries. As I found repeated bendings raised the limit of elasticity, I rather anticipated finding an increased resistance from this cause in long columns. This did not prove to be the case, nor did I find any difference in short columns four diameters in length.

In addition to the preceding experiments with rectangular bars, I have tested the endurance of many revolving shafts of cast iron, wrought iron, and steel, with similar results. About 5000 reversals of a stress equal to one-half the static breaking weight sufficed generally to cause the snapping of a shaft of any of the above materials. When the stress was reduced and the number of applications increased, I found the relative endurance of solid beams to be more nearly proportional to the tensile strength of the metal than to the breaking weight of the beam, a distinction of great importance where axles, springs, and similar things are concerned. Many of my experiments were singularly suggestive. Thus, it was instructive to see a bar of cast iron loaded with a weight which, according to Fairbairn's experiments, it should have carried for a long series of years, broken in two minutes when set gently rotating. Also to find a bar of the finest mild steel so changed in constitution by some months of rotation as to offer no advantages either in strength or toughness over a new cast-iron bar of the same section.

Although, as already stated, many more experiments are required before universally acceptable rules can be laid down, I have thoroughly convinced myself that, where stresses of varying intensity occur, tension and compression members should be treated on an entirely different basis. If, in the case of a tension member, the sectional area be increased 50 per cent. because the stress, instead of being constant, ranges from *nil* to the maximum, then I think 20 per cent. increase would be a liberal allowance in the case of a compression member. I have also satisfied myself that if a metallic railway bridge is to be built at a minimum first cost, and be free from all future charges for structural maintenance, it is essential to vary the working stress upon the metal within very wide limits, regard being had not merely to the effect of intermittent stresses, but also to the relative limits of elasticity in tension and compression members even under a steady load.

Why an originally strong and ductile metal should become weak and brittle under the frequent repetition of a moderate stress has not yet been explained. Lord Bacon touched upon the subject two or three centuries ago, but you may consider his explanation not wholly satisfactory. He said, "Of bodies, some are fragile, and some are tough and not fragile. Of fragility, the cause is an impotency to be extended, and the cause of this inaptness is the small quantity of spirits." I am sorry to have no better explanation to offer, but whatever may be the immediate cause of fragility, no doubt exists that it is induced in metals by frequent bendings, such as a railway bridge undergoes. This fact, however, is not recognised in our Board of Trade Regulations, which remain as they were in the dark ages, as do those of the Ministry of Public Works of France and other countries. With us it is simply provided that the stress on an iron bridge must not exceed 5 tons per square inch on the effective section of the metal. In France it is still worse, as the limiting stress of rather under 4 tons per square inch is estimated upon the gross section, regardless of the extent to which the plates may be perforated by rivet holes. In neither case is any regard had in the rules to intermittent stresses or the flexure of compression members. In Austria the regulations make a small provision for these elements; and American specifications make a large one, the limiting stresses, instead of being constant at 5 tons, as with us, ranging from about $2\frac{1}{2}$ tons to $6\frac{3}{4}$ tons per square inch, according to circumstances. It is hardly necessary that I should say more to justify my statement that, as regards the admissible intensity of stress on metallic bridges, absolute chaos prevails.

Engineers must remember that if satisfactory rules are to be framed, they, and not Governmental departments, must take the initiative. In former days the British Association did much to direct the attention of engineers to this important matter, but, so far as I know, the subject has been dropped for the past twenty years, and I have ventured, therefore, to bring it before you again in some detail. We are here avowedly for the advancement of science, and I have not been deterred by the dryness of the subject from soliciting your attention to a branch of science which is sadly in need of advancement.

Had I been addressing a less scientific audience I might have been tempted rather to boast of the achievements of engineers than to point out their shortcomings. The progress in many branches of mechanical science during the past fifty years has exceeded the anticipation of the most far-seeing. Fifty years ago the chairman of the Stockton and Darlington Railway, when asked by a Parliamentary committee if he thought any further improvements would be possible on railways, replied that he understood in future all new railways would have a high earth-work bank on each side to prevent engines toppling over the embankments, and to arrest hot ashes, which continually set fire to neighbouring stacks, but in other respects he appeared to think perfection was attained. Shortly before the introduction of locomotives it was also thought perfection was attained when low trucks were attached to the trains to carry the horses over the portions of the line where descending grades prevailed, and all the newspapers announced, with a great flourish of trumpets, that a year's experience showed the saving in horseflesh to be fully 33 per cent.

Although these views seem childlike enough from our present standpoint, I have no doubt that as able and enterprising engineers existed prior to the age of steam and steel as exist now, and their work was as beneficial to mankind, though different in direction. In the important matter of water supply to towns, indeed, I doubt whether, having reference to facility of execution, even greater works were not done 2000 years ago than now. Herodotus speaks of a tunnel 8 feet square, and nearly a mile long, driven through a mountain in order to supply the city of Samos with water; and his statement, though long doubted, was verified in 1882 through the abbot of a neighbouring cloister accidentally unearthing some stone slabs. The German Archæological Society sent out Ernst Fabricius to make a complete survey of the work, and the record reads like that of a modern engineering undertaking. Thus, from a covered reservoir in the hills proceeded an arched conduit about 1000 yards long, partly driven as a tunnel and partly executed on the "cut and cover" system adopted on the London underground railway. The tunnel proper, more than 1100 yards in length, was hewn by hammer and chisel through the solid limestone rock. It was driven from the two ends like the great Alpine tunnels, without intermediate shafts, and the engineers of 2400 years ago might well be congratulated for getting only some dozen feet out of level and little more out of line. From the lower end of the tunnel branches were constructed to supply the city mains and fountains, and the explorers found ventilating shafts and side entrances, earthenware socket-pipes with cement joints, and other interesting details connected with the water-supply of towns.

In the matter of masonry bridges, also, as great works were undertaken some centuries ago as in recent times. Sir John Rennie stated, in his presidential address at the Institute of Civil Engineers, that the bridge across the Dee at Chester was the "largest stone arch on record." That is not so. The Dee Bridge consists of a single segmental arch 200 feet span and 42 feet rise; but across the Adda, in Northern Italy, was built, in the year 1377—more than 500 years ago—a similar segmental arch bridge of no less than 237 feet span and 68 feet rise. Ferario not long since published an account of this, for the period, colossal work, from which it would appear that its life was but thirty-nine years, the bridge having been destroyed for military reasons on December 21, 1416. I believe our American cousins claim to have built the biggest existing stone arch bridge in the world—that across the Cabin Johns Creek; but the span, after all, is only 215 feet, or 10 per cent. smaller than the 500-year-old bridge. In timber bridges, doubtless, the Americans will ever head the list, for the bridge of 340 feet span built across the Schuylkill three-quarters of a century ago will probably never be surpassed. Our ancestors were splendid workers in stone and timber, and, if they had been in possession of an unlimited supply of iron and steel I fear there would have been little left for modern bridge-builders to originate.

The labours of the present generation of engineers are lightened beyond all estimate by labour-saving appliances. To prove how much the world is indebted to students of this branch of mechanical science, and how rapid is the development of a really good mechanical notion, it is only necessary to refer to the numerous hydraulic appliances of the kind first introduced forty years ago by a distinguished past-President, Sir W. G. Armstrong. Addressing you in 1854, Sir William Armstrong explained that the object he had in view from the first was "to provide, in substitution of manual labour, a method of working a multiplicity of machines, intermittent in their action and extending over a large area, by means of transmitted power, produced by a steam-engine and accumulated at one central point." The number of cases in which this method of working is a desideratum, or even indispensable, would appear to be limitless. I should be sorry, indeed, to have anything to do with building the Forth Bridge if hydraulic appliances were not at hand to do a giant's work. Let me shortly describe to you what we are doing there at the present time. More than 42,000 tons of steel plates and bars have to be bent, planed, drilled, and riveted together before or after erection, and hydraulic appliances are used throughout. The plates are handled in the shops by numerous little hydraulic cranes of special design, without any complication of multiplying sheaves, the whole arm being raised with the load by a 4-inch direct-acting ram of 6 feet stroke. A total length of no less than 60 miles of steel plates, ranging in thickness from $1\frac{1}{4}$ inches to $\frac{3}{8}$ inch, have to be bent to radii of from 6 feet to 9 inches, which is done in heavy cast-iron dies squeezed together by four rams of 24 inches in diameter, and the same stroke. With the ordinary working pressure of 1000 lbs. per square inch, the power of the press is thus about 1750 tons. Some 3000 pieces, shaped like the lid of a box, 15 inches by 12 inches wide, with a 3-inch deep rim all round, were required to be made of $\frac{1}{2}$-inch steel plate, and this was easily effected in two heats by a couple of strokes of a 14-inch ram. In numberless other instances steady hydraulic pressure has been substituted by Mr. Arrol, our able contractor, for the usual cutting and welding under the blacksmith's hammer.

Hydraulic appliances are also an indispensable part of the scheme for erecting the great 1700 feet spans. Massive girders will be put together at a low level, and be hoisted as high as the top of St. Paul's Cathedral by hydraulic power. Continuous girders, nearly a third of a mile in length, will be similarly raised. Not only the girders, but workmen, their sheds, cranes, and appliances will be carried up steadily and imperceptibly as the work of erection proceeds, on platforms weighing in some instances more than 1000 tons. It is hardly necessary to say that every rivet in the bridge will be closed up by hydraulic power, the machines being in many instances of novel design, specially adapted to the work. Thus the bed-plates, which in ordinary bridges are simple castings, in the Forth Bridge are necessarily built up of numerous steel plates, the size of each bed-plate being 37 feet long by 17 feet 6 inches wide. To grip together the 47 separate plates into a solid mass, 3800 rivets $1\frac{1}{8}$ inches in diameter with countersunk heads on both sides are required, and, remembering that the least dimension of the bed-plate is 17 feet 6 inches, it will be seen that the ordinary "gap"-riveter would not be applicable. A special machine was therefore designed by Mr. Arrol, consisting of a pair of girders and a pair of rams, between which the bed-plate to be riveted together lies. A double ram machine had for like reasons to be devised for riveting up the great tubular struts of the bridge.

Not merely in the superstructure, but in the construction of the foundations, were hydraulic appliances of a novel character indispensable at the Forth Bridge. Huge wrought-iron caissons or cylinders, 70 feet diameter and 72 feet high, were taken up and set down as readily as a man would handle a bucket. In sinking these caissons through the mud and clay of the Forth compressed air was used. When the boulder-clay was reached the labour of excavating the extremely hard and tenacious material in the compressed-air chamber proved too exhausting, pick-axes were of little avail, and the Italian labourers who were chiefly employed lost heart over the job altogether. But a giant power was at hand, and only required tools fit for the work. Spades with hydraulic rams in the hollow handles were made, and, with the roof of the compressed air-chamber to thrust against, the workmen had merely to hold the handle vertically, turn a little tap, and down went the spade with a force of three tons into the hitherto impracticable clay as sweetly as a knife into butter. Probably, when addressing you thirty years ago, Sir William Armstrong never anticipated that a number of hydraulic spades would be digging away in an electrically lighted chamber or diving-bell, 70 feet diameter and 7 feet high, 90 feet below the waves of the sea; but still the spades come strictly within the definition of the class of machines, intermittent in their action and extending over a large area, which it was his aim to introduce. It would be possible, indeed, with the appliances at the Forth Bridge, to arrange that the simple opening of a valve should start digging at the bottom of the sea, riveting at a height of nearly 400 feet above the sea, and all the multifarious operations of bending, forging, and hoisting, extending over a site a mile and a half in length.

It would not only be impossible to build a Forth Bridge, but it would be equally impossible to fight a modern ironclad without the aid of hydraulic appliances. Most of the Presidents of this Section have referred in the course of their addresses to our navy, and certainly the subject is a tempting one, for the progress of mechanical science in recent years could not be better illustrated than by a description of the innumerable appliances which go to the making and working of a modern ironclad. Let me quote a single passage from a pamphlet by a naval officer, which caused a great stir a few years before the Crimean war, that I may recall to your minds what was the speed and what the armament of our fleet at that comparatively recent period. "Conceive," said Capt. Plunkett, R.N., "a British and French fleet issuing simultaneously from Spithead and

Cherbourg; seven hours' steaming at the rate of six miles an hour will bring them together. A single glance at the heavy and well-appointed tiers of a line-of-battle ship's guns will satisfy any one that they are no toys to be placed in the hands of novices. Formidable batteries of the heaviest ordnance are there—not a gun under a 32-pounder, and many 68-pounder shell guns." In little more than a quarter of a century engineers have changed all that, and advanced to 20-knot vessels and 120-ton guns. Archæologists tell us that our predecessors in mechanical science of the Stone Age were apparently a thousand or more years in finding out that the best way of fitting an axe was to slip the handle through the axe and not the axe through the handle. Engineers of the present day may be excused, therefore, for occasionally illustrating the rapidity of the advance of their science by contrasting the ships of thirty years ago with our modern ironclads.

The latest type of battle-ship weighs, fully equipped, about 10,000 tons. There are about 3400 tons of steel in her hull, apart from armour, which, with its backing, will weigh a further 2800 tons. The machinery, largely of steel, is about 1400 tons; the armament, including ammunition, 1100 tons; the coals, 1100 tons; and general equipment, 270 tons. A detailed description bristles with the word "steel," and enthusiastic newspaper reporters sent down to Chatham Dockyard can no more "spin out their copy" with Cowper's oft-quoted lines on the "Launch of a First-Rate":—

> "Giant oaks of bold expansion
> O'er seven hundred acres fell,
> All to build thy noble mansion,
> Where our hearts of oak do dwell."

A latter-day poet might boast of 700 acres being exhausted by a single vessel, but it would be a coal-field and not a forest. Accepting Prof. Phillips's estimate of the average rate of formation of coal, it may be shown that a hard-worked American liner during her lifetime burns as much coal as would be produced on the area of 700 acres in a period of 2000 years. We are thus with our steel ships using up our primeval forests at a far more extravagant rate than that at which our immediate forefathers cleared the oak forests. Coal is the great stimulant of the modern engineer. Pope Pius the Second has left on record an expression of the astonishment he felt when visiting Scotland, in the fifteenth century, on seeing poor people in rags begging at church doors, and receiving for alms pieces of black stone, with which they went away contented. To such early familiarity with coal may, however, be due the fact that Scotland has ever led the way in the development of the steam-engine, and that at the date of the battle of Waterloo she had built and registered seven steam-vessels, whilst England could boast of none.

Probably none but a poet or a painter would wish for a return to our old oak sailing ships. Some few people still entertain the illusion that the picturesque old tubs were better sea-boats than our razor-ended steamers; but, speaking of them in 1846, Admiral Napier said: "The ships look very charming in harbour, but to judge of them properly you should see them in a gale of wind, when it would be found they would roll 45° leeward and 43° windward." Even our first ironclads were not so bad as that, for although, according to the *Times*, when the squadron was on trial in the Bay of Biscay, the ships rocked wildly to the rising swell and the sea broke in great hills of surf, yet the maximum roll signalled by the worst roller of the lot—the *Lord Warden*—was but 35° leeward and 27° windward—a total range of 62° as compared with 88° in the old line-of-battle ships.

We have heard much about the state of the navy during the past twelve months. A dip into the publications of the British Association—which in this, as in other respects, afford a fair indication of what is uppermost in people's minds—will show that similar discussions have recurred periodically, at any rate since 1830. If we consult Hansard, as I had occasion to do recently, we find the same remark applies to periods long antecedent to 1830.

It amounts almost to a religious conviction in the mind of a Briton that Providence will not be on his side unless his fleet is at least equal to that of France and Russia united. What would be said now of a Minister who met an attack on the administration of the navy by demonstrating that we had *half* as many line-of-battle ships as Russia: and yet that was literally done less than fifty years ago. Speaking in the House of Commons on March 4, 1839, the Secretary of the Admiralty said:

"For the last six months unceasing attacks have been made upon our naval administration, describing our navy as in a state of the utmost decrepitude, and Tory papers say that shameful reductions have been made in the navy by the present Government. It will be a consolation to my honourable friends to be assured that we have for years lived unharmed through dangers as great as that to which we are now exposed. In 1817 we had 15 sail-of-the-line in commission, and Russia had 30; in 1823 we had 12, and Russia 37; in 1832 we had 11, and Russia 36; and now we have 20, and the Russians 43, having raised our ships to nearly half the number of those of Russia.

Now as to our guns. The past twelve months is by no means the first occasion on which the armament of our navy has been attacked. Three years subsequent to the speech of the Secretary of the Admiralty just referred to, Sir Charles Napier made a statement from his place in Parliament of so extraordinary a character that I make no apology for quoting his exact words, as a reminder of the past and a warning for the future: "At the end of the last war the guns were in such a bad state that, when fired, they would scarcely hit an enemy, and during the latter period of the American war a secret order was issued that British ships of war should not engage American frigates, because the former were in such an inefficient state." As for himself, said the plain-spoken old admiral, when he got the order he put it in "the only place fit to receive it, the quarter-galley."

Happily, from our insular position, the change which the progress of mechanical science has wrought in military operations has not been brought home to the people of this country in the same vivid manner that it has to the people of the continents of Europe and America. In the American war, the Franco-German war, and the Russo-Turkish war the construction and equipment of railway works by engineers was an essential part of all great movements. The Russians, in 1877, constructed a railway from Bender to Galatz, 180 miles in length, in fifty-eight working days, or at the rate or more than three miles per day. Altogether, in the three latter months of that year they laid out and built about 240 miles of railway, and purchased and stocked the line with 110 locomotives and 2200 waggons. They also built numerous trestle bridges, together with an opening bridge and a ferry across the Danube.

We have had recent experience of the slowness of primitive modes of transport in the tedious advance of Lord Wolseley's handful of men in whale-boats up the Nile. It was the intention of the late Khedive, partly from military and partly from commercial considerations, to construct a railway exactly on the line of advance subsequently followed by Wolseley. My partner, Mr. Fowler, had the railway sent out in 1873, and the works were shortly after commenced. The total length was 550 miles, and the estimated cost, including rolling-stock and repairing-shops, 4,000,000*l*. Owing to financial difficulties the works were abandoned, but the 64 miles constructed by Mr. Fowler, and the recent extensions of the same by the military, proved of great service to the expedition, even some of the steam-launches being taken by railway to save delays at the cataracts.

During the siege of Paris the German forces were dependent upon supplies drawn from their base, and the army requirements were fully met by one line of railway running twelve to fourteen trains per day. Military authorities state that a train load of about 250 tons is equal to two days' rations and corn for an army corps of 37,000 men and 10,000 horses. The military operations in Egypt have proved that, even in the heart of Africa, railways, steamboats, electric lights, machine guns, and other offspring of mechanical science, are essential ingredients of success.

Members of this Section who visited the United States last year not for the first time could hardly have failed to notice that American and European engineering practice are gradually presenting fewer points of difference. Early American iron railway bridges were little more than the ordinary type of timber bridge done into iron, and the characteristic features, therefore, were great depth of truss, forged links, pins, screw-bolts, round or rectangular struts, cast-iron junction pieces, and, in brief, an assemblage of a number of independent members more or less securely bolted together, and not, as in European bridges, a solidly riveted mass of plates and angle-bars. At the present moment the typical American bridge is distinctly derived from the grafting of German practice on the original parent stock. Pin connections are still generally used in bridges of any size, but the top members and connections are more European than

American in construction, whilst for girders of moderate span, such as those on the many miles of elevated railway in New York, riveted girders of purely European type are admittedly the cheapest and most durable. From my conversations with leading American bridge builders, I am satisfied that their future practice and our own will approach still more nearly. We should never think of building another Victoria tubular bridge across the St. Lawrence, or repeat the design of the fallen Tay Bridge, nor would they again imitate in iron an old timber bridge, or repeat the design of the fallen Ashtabula bridge. In one respect the practice in America tends to the production of better and cheaper bridges than does our own practice, and it is this: each of the great bridge-building firms adopts by preference a particular type design, and the works are laid out to produce bridges of this kind. It is an old adage that practice makes perfect, and by adhering to one type, and not vaguely wandering over the whole field of design, details are perfected and a really good bridge is the result. Engineers in America therefore need only specify the span of their bridge, and the rolling load to be provided for, with certain limiting stresses, and they can make sure of obtaining a number of tenders from different makers of bridges, varying somewhat in design, but complying with all the requirements. With us, on the other hand, it is too often the privilege of a pupil to try his 'prentice hand on the design for a bridge, and it is no wonder, therefore, that many curious bits of detail meet the eye of an observant foreigner inspecting our railways.

The magnificent steel wire rope suspension bridge of 1600 feet span built by Roebling across the East River at New York well marks the advanced state of mechanical science in America as regards bridge-building. It is worthy of note that, at the second meeting of the British Association, held so long back as 1832, there was a paper on suspension bridges, and the author entreated the attention of the scientific world, and particularly of civil engineers, to the serious consideration of the question: "How far ought iron to be hereafter used for suspension bridges, since a steel bridge of equal strength and superior durability could be built at much less cost?" "I earnestly call upon the ironmasters of the United Kingdom," said he, "to lose no time in endeavouring to solve this question." In this, as in many other engineering matters, America has given us a lead. America, is indeed, the paradise of mechanics. When the British Association was inaugurated, years ago, there was, I believe, no intention to have a section for the discussion of mechanical science. Possibly it may have been considered too mean a branch. Even the usually generous Shakespeare speaks contemptuously of "mechanic slaves, with greasy aprons, rules, and hammers;" and our old friend Dr. Johnson's definition of "mechanical" is "mean, servile." We have lived down this feeling of contempt, and the world admits that the "greasy apron" is as honourable a badge as the priest's cassock or the warrior's coat of mail, and has played as important a part in the great work of civilising humanity and turning bloodthirsty savages into law-abiding citizens.

As I have had occasion to refer to Canada and America in the course of my remarks, I cannot refrain from expressing the high appreciation which I am sure every member of this Section entertains of the cordiality and warmth of our reception on the other side of the Atlantic last year. Such incidents make us forget that differences have ever existed between the two countries. I was amused the other day, on reading in Dr. Doran's "Annals of the Stage," that, in the year 1777, the theatrical company from Edinburgh was captured on its voyage to Aberdeen by an American privateer, and taken off Heaven knows where, for it did not turn up again. This, you will say, was a long time ago; but, if you glance through the speeches of our present gracious Sovereign, you will find one in which her Majesty speaks with "deep concern" of insurrection in Lower Canada, and of hostile incursions into Upper Canada by certain "lawless inhabitants" of the United States of North America.

This is strange reading, after our last year's experience. Gentlemen, I may not have carried you with me in some things I have said, but I think you will all agree with me in this: that the statesman who should suffer any slight difference of opinion to develope into a serious breach between ourselves and our brethren in Canada and cousins in America would, to quote the words of Burke, "far from being qualified to be directors of the great movements of this empire, be not fit even to turn a wheel in the machine."

NOTES

THE new gallery of fishes at the Natural History Museum is now open to the public, and an addition has been made to the Osteological Gallery by throwing open the pavilion at the west end, in which are exhibited skeletons and skulls of elephants, the giraffe, &c.

A REPORT is current in Rome that the members of the Italian Expedition to Central Africa, under the leadership of Signor Alfredo Massari, have been massacred.

THE natural history collections made by the late Dr. Nachtigal, in the course of his tour of annexation on the west coast of Africa, have arrived at Berlin in twenty cases, and the greater part of their contents will be assigned to the new ethnological museum.

AN astronomical-mathematical section, under the presidency of Profs. Reye and Christoffel, of Strassburg, has been formed in the Scientific Congress at Strassburg.

M. BOUQUET, a mathematician of some eminence and a Sorbonne professor, died on the 10th instant at the age of sixty-six.

THE death is announced of Mr. W. A. Guy, M.B., F.R.S., on the 10th inst., in the seventy-sixth year of his age. He was for a number of years Dean of the Medical Department in King's College, and Professor of Hygiene. He was admitted a Fellow of the Royal College of Physicians in 1844, held office as censor in 1855, 1856, and 1866, and as examiner in 1861–3, and in 1861, 1868, and 1875 was appointed Croonian, Lumleian, and Harveian lecturer. Mr. Guy also held a number of other appointments, among which were—honorary secretary to the Statistical Society in 1845, and President in 1873, examiner in forensic medicine at the University of London in 1862, Swiney Prizeman, 1869, and Vice-President of the Royal Society in 1876-7. Mr. Guy devoted much attention for many years to questions of sanitary reform and social science, and in 1878 was appointed one of the Royal Commissioners to inquire into the working of the Penal Servitude Acts; also in 1879 a member of the Criminal Lunatic Commission. He was the author of many essays on physiology and kindred subjects, and also of works of a more general character. Among his principal publications may be mentioned "Principles of Forensic Medicine," "Public Health," "The Factors of the Unsound Mind," "John Howard's Winter's Journey," and his last work, "The Claims of Science on Public Recognition and Support." It may be added that Mr. Guy was likewise editor of Hooper's "Physician's Vade-Mecum."

COL. PRJEVALSKY has sent the following message, dated July 1, from his camp in Chinese Turkestan:—"It is impossible to penetrate into Tibet by the Keria Mountains, the passes through them being impracticable for our beasts of burden, and the Chinese having obstructed the paths with rocks, and having also destroyed the bridges. The native population has given us everywhere a good reception, and, despite the interference of the Chinese, their sympathies with the Russians are openly pronounced. We shall pass the present month among the snow-covered mountains between the rivers of Keria and Khoten. About the middle of August we shall go to Khoten, and then by the course of the river of the same name to Aksu. All is well."

THE inaugural address at the commencement of the medical session 1885-86 will be delivered at St. Thomas's Hospital on October 1, at 3 p.m., by A. O. MacKellar, M.Ch., F.R.C.S., in the theatre of the hospital.

AT the request of the Batavian Society of Arts and Sciences, the Government of the Netherlands' Indies has taken a step