After our article of June 26 (p. 189) it is needless to go much further into detail, but it may be pointed out that hitherto certain subjects, of which Science is one, have had 2000 marks each, whilst three others have had 3000, and free choice of four subjects among these has been allowed. Owing to the great influence which marks have naturally exercised upon the choice of candidates and to other circumstances, the position of Science has been modest enough. In future, however, it will have (to put it numerically) about one-fourth of its previous chance, since, instead of the candidates being free to select four subjects, they will now only be free to select one. In short the final arrangements, though undoubtedly improvements upon those announced a few months since, and in one particular considerable improvements, are in their main features retrograde and unsound. They will hamper those of our schools which make it their aim to widen the basis of education in this country by the introduction of science into their regular work, and they will further discourage those who have hitherto hesitated from following their example. They will be a check on freedom and progress in education. The new regulations no doubt will encourage the study of modern languages. They do this, however, at too great a cost to other subjects of at least equal importance. We regret very much that the War Office authorities have not adopted some plan by which, whilst securing a knowledge of those subjects which they regard as professionally essential, they would have left a fairer field to such studies as Higher Mathematics, Natural Science, Greek, &c. Whilst we regret so much the blow to science teaching in our schools which is given by the final adoption of the scheme before us, and that young men of scientific capacity should stand so poor a chance of employment in our military services, we must acknowledge that in their revised regulations the War Office authorities have effected a distinct improvement by the new grouping of the experimental sciences and the addition of a practical examination to each group; although, from the greatly inferior position which science will hold in future, we fear that the practical advantage in the Sandhurst examinations will be very small. If, however, such a grouping of the natural sciences could be extended to the Woolwich examination also, it would be a great gain. The allotment of marks at present in force in the Woolwich competitions deals very fairly with the various subjects. Mathematics and Drawing, which are of special importance, are duly encouraged, whilst a fair liberty of choice among other subjects is left to the candidates. It has, however, been a complaint that, owing to the unequal difficulties presented by the present groups, some science subjects are unduly encouraged at the cost of others. This complaint ought practically to cease under such a classification as is now introduced into the Sandhurst scheme, for it ought no longer to be difficult for the examiners to set papers of fairly equal difficulty and range. We hope this change of detail may be extended to the science of the Woolwich examinations. We believe it would be welcomed by all those who have the interests of science teaching at heart.

THE BRITISH ASSOCIATION

THE fifty-fourth annual meeting of the British Association commenced yesterday at Montreal. About 800 members have arrived in Montreal from England, and the interest taken in the meeting both in Canada and the United States is evidenced by the great number of visitors which it has attracted to that city. The reception accorded to the Association both by the city and the Dominion is all that could be wished. Montreal itself has raised a fund of 40,000 dollars for expenses, and over 300 members have been received as guests into private houses. McGill College, where the Association meets, has been specially prepared for the purpose, and there is every probability that the meeting will be a success in all respects.

The proceedings began last night with the address of the President, Lord Rayleigh, who was to be introduced by Sir William Thomson. To-day the Sectional proceedings began, and it will be seen from what follows that the addresses are quite up to the average. To-morrow evening Prof. Lodge delivers his lecture on "Dust"; on Saturday Prof. R. S. Ball gives the popular lecture, his subject being "Comets"; and on Monday evening Dr. Dallinger gives a richly illustrated account of the lowest forms of life.

Extensive arrangements have been made for excursions of the members to Quebec, Ottawa, and other places of interest in the Dominion and the United States, with garden parties, *soirées*, and receptions, in the intervals of the meetings. The citizens of Quebec are arranging to entertain 600 members on Saturday.

Inaugural Address by the Right Hon. Lord Rayleigh, M.A., D.C.L., F.R.S., F.R.A.S., F.R.G.S., Professor of Experimental Physics in the University of Cambridge, President

It is no ordinary meeting of the British Association which I have now the honour of addressing. For more than fifty years the Association has held its autumn gathering in various towns of the United Kingdom, and within those limits there is, I suppose, no place of importance which we have not visited. And now, not satisfied with past successes, we are seeking new worlds to conquer. When it was first proposed to visit Canada, there were some who viewed the project with hesitation. For my own part, I never quite understood the grounds of their apprehension. Perhaps they feared the thin end of the wedge. When once the principle was admitted, there was no knowing to what it might lead. So rapid is the development of the British Empire, that the time might come when a visit to such out-of-the-way places as London or Manchester could no longer be claimed as a right, but only asked for as a concession to the susceptibilities of the English. But seriously, whatever objections may have at first been felt soon were outweighed by the consideration of the magnificent opportunities which your hospitality affords of extending the sphere of our influence and of becoming acquainted with a part of the Queen's dominion which, associated with splendid memories of the past, is advancing daily by leaps and bounds to a position of importance such as not long ago was scarcely dreamed of. For myself, I am not a stranger to your shores. I remember well the impression made upon me, seventeen years ago, by the wild rapids of the St. Lawrence, and the gloomy grandeur of the Saguenay. If anything impressed me more, it was the kindness with which I was received by yourselves, and which I doubt not will be again extended not merely to myself but to all the English members of the Association. I am confident that those who have made up their minds to cross the ocean will not repent their decision, and that, apart altogether from scientific interests, great advantage may be expected from this visit. We Englishmen ought to know more than we do of matters relating to the Colonies, and anything which tends to bring the various parts of the Empire into closer contact can hardly be over-valued. It is pleasant to think that this Association is the means of furthering an object which should be dear to the hearts of all of us; and I venture to say that a large proportion of the visitors to this country will be astonished by what they see, and will carry home an impression which time will not readily efface.

To be connected with this meeting is to me a great honour, but also a great responsibility. In one respect, especially, I feel that the Association might have done well to choose another President. My own tastes have led me to study mathematics and physics rather than geology and biology, to which naturally more attention turns in a new country, presenting as it does a fresh field for investigation. A chronicle of achievements in these departments by workers from among yourselves would have been suitable to the occasion, but could not come from me. If you would have preferred a different subject for this address, I hope at least that you will not hold me entirely responsible.

At annual gatherings like ours the pleasure with which friends meet friends again is sadly marred by the absence of those who can never more take their part in our proceedings. Last year my predecessor in this office had to lament the untimely loss of

Spottiswoode and Henry Smith, dear friends of many of us, and prominent members of our Association. And now, again, a well-known form is missing. For many years Sir W. Siemens has been a regular attendant at our meetings, and to few indeed have they been more indebted for success. Whatever the occasion, in his Presidential Address of two years ago, or in communications to the Physical and Mechanical Sections, he had always new and interesting ideas, put forward in language which a child could understand, so great a master was he of the art of lucid statement in his adopted tongue. Practice with Science was his motto. Deeply engaged in industry, and conversant all his life with engineering operations, his opinion was never that of a mere theorist. On the other hand, he abhorred rule of thumb, striving always to master the scientific principles which underlie rational design and invention.

It is not necessary that I should review in detail the work of Siemens. The part which he took, during recent years, in the development of the dynamo machine must be known to many of you. We owe to him the practical adoption of the method, first suggested by Wheatstone, of throwing into a shunt the coils of the field-magnets, by which a greatly improved steadiness of action is obtained. The same characteristics are observable throughout—a definite object in view and a well-directed perseverance in overcoming the difficulties by which the path is usually obstructed.

These are indeed the conditions of successful invention. The world knows little of such things, and regards the new machine or the new method as the immediate outcome of a happy idea. Probably, if the truth were known, we should see that, in nine cases out of ten, success depends as much upon good judgment and perseverance as upon fertility of imagination. The labours of our great inventors are not unappreciated, but I doubt whether we adequately realise the enormous obligations under which we lie. It is no exaggeration to say that the life of such a man as Siemens is spent in the public service; the advantages which he reaps for himself being as nothing in comparison with those which he confers upon the community at large.

As an example of this it will be sufficient to mention one of the most valuable achievements of his active life—his introduction, in conjunction with his brother, of the regenerative gas furnace, by which an immense economy of fuel (estimated at millions of tons annually) has been effected in the manufacture of steel and glass. The nature of this economy is easily explained. Whatever may be the work to be done by the burning of fuel, a certain *temperature* is necessary. For example, no amount of heat in the form of boiling water would be of any avail for the fusion of steel. When the products of combustion are cooled down to the point in question, the heat which they still contain is useless as regards the purpose in view. The importance of this consideration depends entirely upon the working temperature. If the object be the evaporation of water or the warming of a house, almost all the heat may be extracted from the fuel without special arrangements. But it is otherwise when the temperature required is not much below that of combustion itself, for then the escaping gases carry away with them the larger part of the whole heat developed. It was to meet this difficulty that the regenerative furnace was devised. The products of combustion, before dismissal into the chimney, are caused to pass through piles of loosely stacked fire-brick, to which they give up their heat. After a time the fire-brick, upon which the gases first impinge, becomes nearly as hot as the furnace itself. By suitable valves the burnt gases are then diverted through another stack of brickwork, which they heat up in like manner, while the heat stored up in the first stack is utilised to warm the unburnt gas and air on their way to the furnace. In this way almost all the heat developed at a high temperature during the combustion is made available for the work in hand.

As it is now several years since your presidential chair has been occupied by a professed physicist, it may naturally be expected that I should attempt some record of recent progress in that branch of science, if indeed such a term be applicable. For it is one of the difficulties of the task that subjects as distinct as mechanics, electricity, heat, optics, and acoustics, to say nothing of astronomy and meteorology, are included under physics. Any one of these may well occupy the life-long attention of a man of science, and to be thoroughly conversant with all of them is more than can be expected of any one individual, and is probably incompatible with the devotion of much time and energy to the actual advancement of knowledge. Not that I would complain of the association sanctioned by common parlance. A sound knowledge of at least the principles of general physics is necessary to the cultivation of any department. The predominance of the sense of sight as the medium of communication with the outer world, brings with it dependence upon the science of optics; and there is hardly a branch of science in which the effects of *temperature* have not (often without much success) to be reckoned with. Besides the neglected borderland between two branches of knowledge is often that which best repays cultivation, or, to use a metaphor of Maxwell's, the greatest benefits may be derived from a cross-fertilisation of the sciences. The wealth of material is an evil only from the point of view of one of whom too much may be expected. Another difficulty incident to the task, which must be faced, but cannot be overcome, is that of estimating rightly the value, and even the correctness, of recent work. It is not always that which seems at first the most important that proves in the end to be so. The history of science teems with examples of discoveries which attracted little notice at the time, but afterwards have taken root downwards and borne much fruit upwards.

One of the most striking advances of recent years is in the production and application of electricity upon a large scale—a subject to which I have already had occasion to allude in connection with the work of Sir W. Siemens. The dynamo machine is indeed founded upon discoveries of Faraday now more than half a century old; but it has required the protracted labours of many inventors to bring it to its present high degree of efficiency. Looking back at the matter, it seems strange that progress should have been so slow. I do not refer to details of design, the elaboration of which must always, I suppose, require the experience of actual work to indicate what parts are structurally weaker than they should be, or are exposed to undue wear and tear. But with regard to the main features of the problem it would almost seem as if the difficulty lay in want of faith. Long ago it was recognised that electricity derived from chemical action is (on a large scale) too expensive a source of mechanical power, notwithstanding the fact that (as proved by Joule in 1846) the conversion of electrical into mechanical work can be effected with great economy. From this it is an evident consequence that electricity may advantageously be obtained from mechanical power; and one cannot help thinking that if the fact had been borne steadily in mind, the development of the dynamo might have been much more rapid. But discoveries and inventions are apt to appear obvious when regarded from the standpoint of accomplished fact; and I draw attention to the matter only to point the moral that we do well to push the attack persistently when we can be sure beforehand that the obstacles to be overcome are only difficulties of contrivance, and that we are not vainly fighting unawares against a law of Nature.

The present development of electricity on a large scale depends, however, almost as much upon the incandescent lamp as upon the dynamo. The success of these lamps demands a very perfect vacuum—not more than about one-millionth of the normal quantity of air should remain—and it is interesting to recall that, twenty years ago, such vacua were rare even in the laboratory of the physicist. It is pretty safe to say that these wonderful results would never have been accomplished had practical applications alone been in view. The way was prepared by an army of scientific men whose main object was the advancement of knowledge, and who could scarcely have imagined that the processes which they elaborated would soon be in use on a commercial scale and intrusted to the hands of ordinary workmen.

When I speak in hopeful language of practical electricity, I do not forget the disappointment within the last year or two of many over-sanguine expectations. The enthusiasm of the inventor and promoter are necessary to progress, and it seems to be almost a law of nature that it should overpass the bounds marked out by reason and experience. What is most to be regretted is the advantage taken by speculators of the often uninstructed interest felt by the public in novel schemes by which its imagination is fired. But looking forward to the future of electric lighting, we have good ground for encouragement. Already the lighting of large passenger-ships is an assured success, and one which will be highly appreciated by those travellers who have experienced the tedium of long winter evenings unrelieved by adequate illumination. Here, no doubt, the conditions are in many respects especially favourable. As regards space, life on board ship is highly concentrated; while unity of management and the presence on the spot of skilled engineers obviate some of the difficulties that are met with under other circumstances. At present we have no experience of a house-to-house system of illumination on a great scale and in competition with cheap gas; but preparations are already far advanced for trial on an adequate scale in London. In large institutions, such as theatres and factories, we all know that electricity is in successful and daily extending operation.

When the necessary power can be obtained from the fall of water, instead of from the combustion of coal, the conditions of the problem are far more favourable. Possibly the severity of your winters may prove an obstacle, but it is impossible to regard your splendid river without the thought arising that the day may come when the vast powers now running to waste shall be bent into your service. Such a project demands of course the most careful consideration, but it is one worthy of an intelligent and enterprising community.

The requirements of practice react in the most healthy manner upon scientific electricity. Just as in former days the science received a stimulus from the application to telegraphy, under which everything relating to measurement on a small scale acquired an importance and development for which we might otherwise have had long to wait, so now the requirements of electric lighting are giving rise to a new development of the art of measurement upon a large scale, which cannot fail to prove of scientific as well as practical importance. Mere change of scale may not at first appear a very important matter, but it is surprising how much modification it entails in the instruments, and in the processes of measurement. For instance, the resistance coils on which the electrician relies in dealing with currents whose maximum is a fraction of an ampere fail altogether when it becomes a question of hundreds, not to say thousands, of amperes.

The powerful currents, which are now at command, constitute almost a new weapon in the hands of the physicist. Effects which in old days were rare and difficult of observation may now be produced at will on the most conspicuous scale. Consider for a moment Faraday's great discovery of the "Magnetisation of Light," which Tyndall likens to the Weisshorn among mountains, as high, beautiful, and alone. This judgment (in which I fully concur) relates to the scientific aspect of the discovery, for to the eye of sense nothing could have been more insignificant. It is even possible that it might have eluded altogether the penetration of Faraday, had he not been provided with a special quality of very heavy glass. At the present day these effects may be produced upon a scale that would have delighted their discoverer, a rotation of the plane of polarisation through 180° being perfectly feasible. With the aid of modern appliances, Kundt and Röntgen in Germany, and H. Becquerel in France, have detected the rotation in gases and vapours, where, on account of its extreme smallness, it had previously escaped notice.

Again, the question of the magnetic saturation of iron has now an importance entirely beyond what it possessed at the time of Joule's early observations. Then it required special arrangements purposely contrived to bring it into prominence. Now in every dynamo machine, the iron of the field-magnets approaches a state of saturation, and the very elements of an explanation of the action require us to take the fact into account. It is indeed probable that a better knowledge of this subject might lead to improvements in the design of these machines.

Notwithstanding the important work of Rowland and Stoletow, the whole theory of the behaviour of soft iron under varying magnetic conditions is still somewhat obscure. Much may be hoped from the induction balance of Hughes, by which the marvellous powers of the telephone are applied to the discrimination of the properties of metals, as regards magnetism and electric conductivity.

The introduction of powerful alternate-current in machines by Siemens, Gordon, Ferranti, and others, is likely also to have a salutary effect in educating those so-called practical electricians whose ideas do not easily rise above ohms and volts. It has long been known that when the changes are sufficiently rapid, the phenomena are governed much more by induction, or electric inertia, than by mere resistance. On this principle much may be explained that would otherwise seem paradoxical. To take a comparatively simple case, conceive an electro-magnet wound with two contiguous wires, upon which acts a given rapidly periodic electromotive force. If one wire only be used, a certain amount of heat is developed in the circuit. Suppose now that the second wire is brought into operation in parallel—a proceeding equivalent to doubling the section of the original wire. An electrician accustomed only to constant currents would be sure to think that the heating effect would be doubled by the change, as much heat being developed in each wire separately as was at first in the single wire. But such a conclusion would be entirely erroneous. The total current, being governed practically by the self-induction of the circuit, would not be augmented by the accession of the second wire, and the total heating effect, so far from being doubled, would, in virtue of the superior conductivity, be halved.

During the last few years much interest has been felt in the reduction to an absolute standard of measurements of electromotive force, current, resistance, &c., and to this end many laborious investigations have been undertaken. The subject is one that has engaged a good deal of my own attention, and I should naturally have felt inclined to dilate upon it, but that I feel it to be too abstruse and special to be dealt with in detail upon an occasion like the present. As regards resistance, I will merely remind you that the recent determinations have shown a so greatly improved agreement that the Conference of Electricians assembled at Paris in May have felt themselves justified in defining the ohm for practical use as the resistance of a column of mercury of 0° C., one square millimetre in section, and 106 cm. in length—a definition differing by a little more than 1 per cent. from that arrived at twenty years ago by a committee of this Association.

A standard of resistance once determined upon can be embodied in a "resistance coil," and copied without much trouble, and with great accuracy. But in order to complete the electrical system, a second standard of some kind is necessary, and this is not so easily embodied in a permanent form. It might conveniently consist of a standard galvanic cell, capable of being prepared in a definite manner, whose electromotive force is once for all determined. Unfortunately, most of the batteries in ordinary use are for one reason or another unsuitable for this purpose, but the cell introduced by Mr. Latimer Clark, in which the metals are zinc in contact with saturated zinc sulphate and pure mercury in contact with mercurous sulphate, appears to give satisfactory results. According to my measurements, the electromotive force of this cell is 1·435 theoretical volts.

We may also conveniently express the second absolute electrical measurement necessary to the completion of the system by taking advantage of Faraday's law that the quantity of metal decomposed in an electrolytic cell is proportional to the whole quantity of electricity that passes. The best metal for the purpose is silver, deposited from a solution of the nitrate or of the chlorate. The results recently obtained by Prof. Kohlrausch and by myself are in very good agreement, and the conclusion that one ampere flowing for one hour decomposes 4·025 grains of silver, can hardly be in error by more than a thousandth part. This number being known, the silver voltameter gives a ready and very accurate method of measuring currents of intensity varying from one-tenth of an ampere to four or five amperes.

The beautiful and mysterious phenomena attending the discharge of electricity in nearly vacuous spaces have been investigated and in some degree explained by De La Rue, Crookes, Schuster, Moulton, and the lamented Spottiswoode, as well as by various able foreign experimenters. In a recent research Crookes has sought the origin of a bright citron-coloured band in the phosphorescent spectrum of certain earths, and after encountering difficulties and anomalies of a most bewildering kind, has succeeded in proving that it is due to yttrium, an element much more widely distributed than had been supposed. A conclusion like this is stated in a few words, but those only who have undergone similar experience are likely to appreciate the skill and perseverance of which it is the final reward.

A remarkable observation by Hall of Baltimore, from which it appeared that the flow of electricity in a conducting sheet was disturbed by magnetic force, has been the subject of much discussion. Mr. Shelford Bidwell has brought forward experiments tending to prove that the effect is of a secondary character, due in the first instance to the mechanical force operating upon the conductor of an electric current when situated in a powerful magnetic field. Mr. Bidwell's view agrees in the main with Mr. Hall's division of the metals into two groups according to the direction of the effect.

Without doubt the most important achievement of the older generation of scientific men has been the establishment and application of the great laws of thermo-dynamics, or, as it is often called, the mechanical theory of heat. The first law, which asserts that heat and mechanical work can be transformed one into the other at a certain fixed rate, is now well understood by every student of physics, and the number expressing the mechanical equivalent of heat resulting from the experiments of Joule has been confirmed by the researches of others, and especially of Rowland. But the second law, which practically is even more important than the first, is only now beginning to receive the full appreciation due to it. One reason of this may be found in a not unnatural confusion of ideas. Words do not always lend themselves readily to the demands that are made upon them by a growing science, and I think that the almost unavoidable use of the word equivalent in the statement of the first law is partly responsible for the little attention that is given to the second. For the second law so far contradicts the usual statement of the first, as to assert that equivalents of heat and work are not of equal value. While work can always be converted into heat, heat can only be converted into work under certain limitations. For every practical purpose the work is worth the most, and when we speak of equivalents, we use the word in the same sort of special sense as that in which chemists speak of equivalents of gold and iron. The second law teaches us that the real value of heat, as a source of mechanical power, depends upon the temperature of the body in which it resides; the hotter the body in relation to its surroundings, the more available the heat.

In order to see the relations which obtain between the first and the second law of thermo-dynamics, it is only necessary for us to glance at the theory of the steam-engine. Not many years ago calculations were plentiful demonstrating the inefficiency of the steam-engine on the basis of a comparison of the work actually got out of the engine with the mechanical equivalent of the heat supplied to the boiler. Such calculations took into account only the first law of thermo-dynamics, which deals with the equivalents of heat and work, and have very little bearing upon the practical question of efficiency, which requires us to have regard also to the second law. According to that law the fraction of the total energy which can be converted into work depends upon the relative temperatures of the boiler and condenser; and it is, therefore, manifest that, as the temperature of the boiler cannot be raised indefinitely, it is impossible to utilise all the energy which, according to the first law of thermo-dynamics, is resident in the coal.

On a sounder view of the matter, the efficiency of the steam-engine is found to be so high that there is no great margin remaining for improvement. The higher initial temperature possible in the gas-engine opens out much wider possibilities, and many good judges look forward to a time when the steam-engine will have to give way to its younger rival.

To return to the theoretical question, we may say with Sir W. Thomson that, though energy cannot be destroyed, it ever tends to be dissipated, or to pass from more available to less available forms. No one who has grasped this principle can fail to recognise its immense importance in the system of the universe. Every change, chemical, thermal, or mechanical—which takes place, or can take place, in Nature, does so, at the cost of a certain amount of available energy. If, therefore, we wish to inquire whether or not a proposed transformation can take place, the question to be considered is whether its occurrence would involve dissipation of energy. If not, the transformation is (under the circumstances of the case) absolutely excluded. Some years ago, in a lecture at the Royal Institution, I endeavoured to draw the attention of chemists to the importance of the principle of dissipation in relation to their science, pointing out the error of the usual assumption that a general criterion is to be found in respect of the development of heat. For example, the solution of a salt in water is, if I may be allowed the phrase, a downhill transformation. It involves dissipation of energy, and can therefore go forward; but in many cases it is associated with the absorption rather than with the development of heat. I am glad to take advantage of the present opportunity in order to repeat my recommendation, with an emphasis justified by actual achievement. The foundations laid by Thomson now bear an edifice of no mean proportions, thanks to the labours of several physicists, among whom must be especially mentioned Willard, Gibbs, and Helmholtz. The former has elaborated a theory of the equilibrium of heterogeneous substances, wide in its principles, and we cannot doubt far-reaching in its consequences. In a series of masterly papers Helmholtz has developed the conception of *free energy* with very important applications to the theory of the galvanic cell. He points out that the mere tendency to solution bears in some cases no small proportion to the affinities more usually reckoned chemical, and contributes largely to the total electromotive force. Also in our own country Dr. Alder Wright has published some valuable experiments relating to the subject.

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.

Under the head of scientific mechanics it is principally in relation to fluid motion that advances may be looked for. In speaking upon this subject I must limit myself almost entirely to experimental work. Theoretical hydrodynamics, however important and interesting to the mathematician, are eminently unsuited to oral exposition. All I can do to attenuate an injustice, to which theorists are pretty well accustomed, is to refer you to the admirable reports of Mr. Hicks, published under the auspices of this Association.

The important and highly practical work of the late Mr. Froude in relation to the propulsion of ships is doubtless known to most of you. Recognising the fallacy of views then widely held as to the nature of the resistance to be overcome, he showed to demonstration that, in the case of fair-shaped bodies, we have to deal almost entirely with resistance dependent upon skin friction, and at high speeds upon the generation of surface-waves by which energy is carried off. At speeds which are moderate in relation to the size of the ship, the resistance is practically dependent upon skin friction only. Although Prof. Stokes and other mathematicians had previously published calculations pointing to the same conclusion, there can be no doubt that the view generally entertained was very different. At the first meeting of the Association which I ever attended, as an intelligent listener, at Bath in 1864, I well remember the surprise which greeted a statement by Rankine that he regarded skin friction as the only legitimate resistance to the progress of a well-designed ship. Mr. Froude's experiments have set the question at rest in a manner satisfactory to those who had little confidence in theoretical prevision.

In speaking of an explanation as satisfactory in which skin friction is accepted as the cause of resistance, I must guard myself against being supposed to mean that the nature of skin friction is itself well understood. Although its magnitude varies with the smoothness of the surface, we have no reason to think that it would disappear at any degree of smoothness consistent with an ultimate molecular structure. That it is connected with fluid viscosity is evident enough, but the *modus operandi* is still obscure.

Some important work bearing upon the subject has recently been published by Prof. O. Reynolds, who has investigated the flow of water in tubes as dependent upon the velocity of motion and upon the size of the bore. The laws of motion in capillary tubes, discovered experimentally by Poiseuille, are in complete harmony with theory. The resistance varies as the velocity, and depends in a direct manner upon the constant of viscosity. But when we come to the larger pipes and higher velocities with which engineers usually have to deal, the theory which presupposes a regularly stratified motion evidently ceases to be applicable, and the problem becomes essentially identical with that of skin friction in relation to ship propulsion. Prof. Reynolds has traced with much success the passage from the one state of things to the other, and has proved the applicability under these complicated conditions of the general laws of dynamical similarity as adapted to viscous fluids by Prof. Stokes. In spite of the difficulties which beset both the theoretical and experimental treatment, we may hope to attain before long to a better understanding of a subject which is certainly second to none in scientific as well as practical interest.

As also closely connected with the mechanics of viscous fluids, I must not forget to mention an important series of experiments upon the friction of oiled surfaces, recently executed by Mr. Tower for the Institution of Mechanical Engineers. The results go far towards upsetting some ideas hitherto widely admitted. When the lubrication is adequate, the friction is found to be nearly independent of the load, and much smaller than is usually supposed, giving a coefficient as low as 1/1000. When the layer of oil is well formed, the pressure between the solid surfaces is really borne by the fluid, and the work lost is spent in shearing, that is, in causing one stratum of the oil to glide over another.

In order to maintain its position, the fluid must possess a certain degree of viscosity, proportionate to the pressure; and even when this condition is satisfied, it would appear to be necessary that the layer should be thicker on the ingoing than on the outgoing side. We may, I believe, expect from Prof. Stokes a further elucidation of the processes involved. In the meantime, it is obvious that the results already obtained are of the utmost value, and fully justify the action of the Institution in devoting a part of its resources to experimental work. We may hope indeed that the example thus wisely set may be followed by other public bodies associated with various departments of industry.

I can do little more than refer to the interesting observations of Prof. Darwin, Mr. Hunt, and M. Forel on ripplemark. The processes concerned would seem to be of a rather intricate character, and largely dependent upon fluid viscosity. It may be noted indeed that most of the still obscure phenomena of hydrodynamics require for their elucidation a better comprehension of the laws of viscous motion. The subject is one which offers peculiar difficulties. In some problems in which I have lately been interested, a circulating motion presents itself of the kind which the mathematician excludes from the first when he is treating of fluids destitute altogether of viscosity. The intensity of this motion proves, however, to be independent of the coefficient of viscosity, so that it cannot be correctly dismissed from consideration as a consequence of a supposition that the viscosity is infinitely small. The apparent breach of continuity can be explained, but it shows how much care is needful in dealing with the subject, and how easy it is to fall into error.

The nature of gaseous viscosity, as due to the diffusion of momentum, has been made clear by the theoretical and experimental researches of Maxwell. A flat disk moving in its own plane between two parallel solid surfaces is impeded by the necessity of shearing the intervening layers of gas, and the magnitude of the hindrance is proportional to the velocity of the motion and to the viscosity of the gas, so that under similar circumstances this effect may be taken as a measure, or rather definition, of the viscosity. From the dynamical theory of gases, to the development of which he contributed so much, Maxwell drew the startling conclusion that the viscosity of a gas should be independent of its density,—that within wide limits the resistance to the moving disk should be scarcely diminished by pumping out the gas, so as to form a partial vacuum. Experiment fully confirmed this theoretical anticipation—one of the most remarkable to be found in the whole history of science, and proved that the swinging disk was retarded by the gas, as much when the barometer stood at half an inch as when it stood at thirty inches. It was obvious, of course, that the law must have a limit, that at a certain point of exhaustion the gas must begin to lose its power; and I remember discussing with Maxwell, soon after the publication of his experiments, the whereabouts of the point at which the gas would cease to produce its ordinary effect. His apparatus, however, was quite unsuited for high degrees of exhaustion, and the failure of the law was first observed by Kundt and Warburg, at pressures below 1 mm. of mercury. Subsequently the matter has been thoroughly examined by Crookes, who extended his observations to the highest degrees of exhaustion as measured by MacLeod's gauge. Perhaps the most remarkable results relate to hydrogen. From the atmospheric pressure of 760 mm. down to about $\frac{1}{2}$ mm. of mercury the viscosity is sensibly constant. From this point to the highest vacua, in which less than one-millionth of the original gas remains, the coefficient of viscosity drops down gradually to a small fraction of its original value. In these vacua Mr. Crookes regards the gas as having assumed a different, ultra-gaseous condition; but we must remember that the phenomena have relation to the other circumstances of the case, especially the dimensions of the vessel, as well as to the condition of the gas.

Such an achievement as the prediction of Maxwell's law of viscosity has of course drawn increased attention to the dynamical theory of gases. The success which has attended the theory in the hands of Clausius, Maxwell, Boltzmann, and other mathematicians, not only in relation to viscosity, but over a large part of the entire field of our knowledge of gases, proves that some of its fundamental postulates are in harmony with the reality of Nature. At the same time it presents serious difficulties; and we cannot but feel that, while the electrical and optical properties of gases remain out of relation to the theory, no final judgment is possible. The growth of experimental knowledge may be trusted to clear up many doubtful points, and a younger generation of theorists will bring to bear improved mathematical weapons. In the meantime we may fairly congratulate ourselves on the possession of a guide which has already conducted us to a position which could hardly otherwise have been attained.

In optics attention has naturally centred upon the spectrum. The mystery attaching to the invisible rays lying beyond the red has been fathomed to an extent that, a few years ago, would have seemed almost impossible. By the use of special photographic methods Abney has mapped out the peculiarities of this region with such success that our knowledge of it begins to be comparable with that of the parts visible to the eye. Equally important work has been done by Langley, using a refined invention of his own based upon the principle of Siemens' pyrometer. This instrument measures the actual energy of the radiation, and thus expresses the effects of various parts of the spectrum upon a common scale, independent of the properties of the eye and of sensitive photographic preparations. Interesting results have also been obtained by Becquerel, whose method is founded upon a curious action of the ultra-red rays in enfeebling the light emitted by phosphorescent substances. One of the most startling of Langley's conclusions relates to the influence of the atmosphere in modifying the quality of solar light. By the comparison of observations made through varying thicknesses of air he shows that the atmospheric absorption tells most upon the light of high refrangibility; so that to an eye situated outside the atmosphere the sun would present a decidedly bluish tint. It would be interesting to compare the experimental numbers with the law of scattering of light by small particles given some years ago as the result of theory. The demonstration by Langley of the inadequacy of Cauchy's law of dispersion to represent the relation between refrangibility and wave-length in the lower part of the spectrum must have an important bearing upon optical theory.

The investigation of the relation of the visible and ultra-violet spectrum to various forms of matter has occupied the attention of a host of able workers, among whom none have been more successful than my colleagues at Cambridge, Profs. Liveing and Dewar. The subject is too large both for the occasion and for the individual, and I must pass it by. But, as more closely related to optics proper, I cannot resist recalling to your notice a beautiful application of the idea of Doppler to the discrimination of the origin of certain lines observed in the solar spectrum. If a vibrating body have a general motion of approach or recession, the waves emitted from it reach the observer with a frequency which in the first case exceeds, and in the second case falls short of, the real frequency of the vibrations themselves. The consequence is that, if a glowing gas be in motion in the line of sight, the spectral lines are thereby displaced from the position that they would occupy were the gas at rest—a principle which, in the hands of Huggins and others, has led to a determination of the motion of certain fixed stars relatively to the solar system. But the sun is itself in rotation, and thus the position of a solar spectral line is slightly different according as the light comes from the advancing or from the retreating limb. This displacement was, I believe, first observed by Thollon; but what I desire now to draw attention to is the application of it by Cornu to determine whether a line is of solar or atmospheric origin. For this purpose a small image of the sun is thrown upon the slit of the spectroscope, and caused to vibrate two or three times a second, in such a manner that the light entering the instrument comes alternately from the advancing and retreating limbs. Under these circumstances a line due to absorption within the sun appears to tremble, as the result of slight alternately opposite displacements. But if the seat of the absorption be in the atmosphere it is a matter of indifference from what part of the sun the light originally proceeds, and the line maintains its position in spite of the oscillation of the image upon the slit of the spectroscope. In this way Cornu was able to make a discrimination which can only otherwise be effected by a difficult comparison of appearances under various solar altitudes.

The instrumental weapon of investigation, the spectroscope itself, has made important advances. On the theoretical side, we have for our guidance the law that the optical power in gratings is proportional to the total number of lines accurately ruled, without regard to the degree of closeness, and in prisms that it is proportional to the thickness of glass traversed. The magnificent gratings of Rowland are a new power in the hands of the spectroscopist, and as triumphs of mechanical art seem to be little short of perfection. In our own report for 1882 Mr. Mallock has described a machine, constructed by him, for ruling large diffraction gratings, similar in some respects to that of Rowland.

The great optical constant, the velocity of light, has been the subject of three distinct investigations by Cornu, Michelson, and Forbes. As may be supposed, the matter is of no ordinary difficulty, and it is therefore not surprising that the agreement should be less decided than could be wished. From their observations, which were made by a modification of Fizeau's method of the toothed wheel, Young and Forbes drew the conclusion that the velocity of light *in vacuo* varies from colour to colour, to such an extent that the velocity of blue light is nearly 2 per cent. greater than that of red light. Such a variation is quite opposed to existing theoretical notions, and could only be accepted on the strongest evidence. Mr. Michelson, whose method (that of Foucault) is well suited to bring into prominence a variation of velocity with wave-length, informs me that he has recently repeated his experiments with special reference to the point in question, and has arrived at the conclusion that no variation exists comparable with that asserted by Young and Forbes. The actual velocity differs little from that found from his first series of experiments, and may be taken to be 299,800 km. per second.

It is remarkable how many of the playthings of our childhood give rise to questions of the deepest scientific interest. The top is, or may be, understood, but a complete comprehension of the kite and of the soap-bubble would carry us far beyond our present stage of knowledge. In spite of the admirable investigations of Plateau, it still remains a mystery why soapy water stands almost alone among fluids as a material for bubbles. The beautiful development of colour was long ago ascribed to the interference of light, called into play by the gradual thinning of the film. In accordance with this view the tint is determined solely by the thickness of the film, and the refractive index of the fluid. Some of the phenomena are, however, so curious as to have led excellent observers like Brewster to reject the theory of thin plates, and to assume the secretion of various kinds of colouring matter. If the rim of a wine-glass be dipped in soapy water, and then held in a vertical position, horizontal bands soon begin to show at the top of the film, and extend themselves gradually downwards. According to Brewster these bands are not formed by the "subsidence and gradual thinning of the film," because they maintain their horizontal position when the glass is turned round its axis. The experiment is both easy and interesting; but the conclusion drawn from it cannot be accepted. The fact is that the various parts of the film cannot quickly alter their thickness, and hence when the glass is rotated they rearrange themselves in order of superficial density, the thinner parts floating up over, or through, the thicker parts. Only thus can the tendency be satisfied for the centre of gravity to assume the lowest possible position.

When the thickness of a film falls below a small fraction of the length of a wave of light, the colour disappears and is replaced by an intense blackness. Profs. Reinold and Rücker have recently made the remarkable observation that the whole of the black region, soon after its formation, is of uniform thickness, the passage from the black to the coloured portions being exceedingly abrupt. By two independent methods they have determined the thickness of the black film to lie between seven and fourteen millionths of a millimetre; so that the thinnest films correspond to about one-seventieth of a wave-length of light. The importance of these results in regard to molecular theory is too obvious to be insisted upon.

The beautiful inventions of the telephone and the phonograph, although in the main dependent upon principles long since established, have imparted a new interest to the study of acoustics. The former, apart from its uses in every-day life, has become in the hands of its inventor, Graham Bell, and of Hughes, an instrument of first-class scientific importance. The theory of its action is still in some respects obscure, as is shown by the comparative failure of the many attempts to improve it. In connection with some explanations that have been offered, we do well to remember that molecular changes in solid masses are inaudible in themselves, and can only be manifested to our ears by the generation of a to-and-fro motion of the external surface extending over a sensible area. If the surface of a solid remains undisturbed, our ears can tell us nothing of what goes on in the interior.

In theoretical acoustics progress has been steadily maintained, and many phenomena which were obscure twenty or thirty years ago, have since received adequate explanation. If some important practical questions remain unsolved, one reason is that they have not yet been definitely stated. Almost everything in connection with the ordinary use of our senses presents peculiar difficulties to scientific investigation. Some kinds of information with regard to their surroundings are of such paramount importance to successive generations of living beings, that they have learned to interpret indications which, from a physical point of view, are of the slenderest character. Every day we are in the habit of recognising, without much difficulty, the quarter from which a sound proceeds, but by what steps we attain that end has not yet been satisfactorily explained. It has been proved that when proper precautions are taken we are unable to distinguish whether a pure tone (as from a vibrating tuning-fork held over a suitable resonator) comes to us from in front or from behind. This is what might have been expected from an *a priori* point of view; but what would not have been expected is that with almost any other sort of sound, from a clap of the hands to the clearest vowel sound, the discrimination is not only possible, but easy and instinctive. In these cases it does not appear how the possession of two ears helps us, though there is some evidence that it does; and even when sounds come to us from the right or left, the explanation of the ready discrimination which is then possible with pure tones is not so easy as might at first appear. We should be inclined to think that the sound was heard much more loudly with the ear that is turned towards than with the ear that is turned from it, and that in this way the direction was recognised. But if we try the experiment we find that, at any rate with notes near the middle of the musical scale, the difference of loudness is by no means so very great. The wave-lengths of such notes are long enough in relation to the dimensions of the head to forbid the formation of anything like a sound shadow in which the averted ear might be sheltered.

In concluding this imperfect survey of recent progress in physics, I must warn you emphatically that much of great importance has been passed over altogether. I should have liked to speak to you of those far-reaching speculations, especially associated with the name of Maxwell, in which light is regarded as a disturbance in an electro-magnetic medium. Indeed, at one time I had thought of taking the scientific work of Maxwell as the principal theme of this address. But, like most men of genius, Maxwell delighted in questions too obscure and difficult for hasty treatment, and thus much of his work could hardly be considered upon such an occasion as the present. His biography has recently been published, and should be read by all who are interested in science and in scientific men. His many-sided character, the quaintness of his humour, the penetration of his intellect, his simple but deep religious feeling, the affection between son and father, the devotion of husband to wife, all combine to form a rare and fascinating picture. To estimate rightly his influence upon the present state of science, we must regard not only the work that he executed himself, important as that was, but also the ideas and the spirit which he communicated to others. Speaking for myself as one who in a special sense entered into his labours, I should find it difficult to express adequately my feeling of obligation. The impress of his thoughts may be recognised in much of the best work of the present time. As a teacher and examiner he was well acquainted with the almost universal tendency of uninstructed minds to elevate phrases above things: to refer, for example, to the principle of the conservation of energy for an explanation of the persistent rotation of a fly-wheel, almost in the style of the doctor in "Le Malade Imaginaire," who explains the fact that opium sends you to sleep by its soporific virtue. Maxwell's endeavour was always to keep the facts in the foreground, and to his influence, in conjunction with that of Thomson and Helmholtz, is largely due that elimination of unnecessary hypothesis which is one of the distinguishing characteristics of the science of the present day.

In speaking unfavourably of superfluous hypothesis let me not be misunderstood. Science is nothing without generalisations. Detached and ill-assorted facts are only raw material, and in the absence of a theoretical solvent have but little nutritive value. At the present time and in some departments the accumulation of material is so rapid that there is danger of indigestion. By a fiction as remarkable as any to be found in law, what has once been published, even though it be in the Russian language, is usually spoken of as "known," and it is often forgotten that the rediscovery in the library may be a more difficult and uncertain process than the first discovery in the laboratory. In this matter we are greatly dependent upon annual reports and abstracts, issued principally in Germany, without which the search for the discoveries of a little-known author would be well-nigh hopeless. Much useful work has been done in this direction in connection with our Association. Such critical reports as those upon hydrodynamics, upon tides, and upon spectroscopy, guide the investigator to the points most requiring attention, and in discussing past achievements contribute in no small degree to future progress. But, though good work has been done, much yet remains to do.

If, as is sometimes supposed, science consisted in nothing but the laborious accumulation of facts, it would soon come to a standstill, crushed, as it were, under its own weight. The suggestion of a new idea, or the detection of a law, supersedes much that had previously been a burden upon the memory, and by introducing order and coherence facilitates the retention of the remainder in an available form. Those who are acquainted with the writings of the older electricians will understand my meaning when I instance the discovery of Ohm's law as a step by which the science was rendered easier to understand and to remember. Two processes are thus at work side by side, the reception of new material and the digestion and assimilation of the old; and as both are essential, we may spare ourselves the discussion of their relative importance. One remark, however, should be made. The work which deserves, but I am afraid does not always receive, the most credit, is that in which discovery and explanation go hand in hand, in which not only are new facts presented, but their relation to old ones is pointed out.

In making one's self acquainted with what has been done in any subject, it is good policy to consult first the writers of highest general reputation. Although in scientific matters we should aim at independent judgment, and not rely too much upon authority, it remains true that a good deal must often be taken upon trust. Occasionally an observation is so simple and easily repeated, that it scarcely matters from whom it proceeds; but as a rule it can hardly carry full weight when put forward by a novice whose care and judgment there has been no opportunity of testing, and whose irresponsibility may tempt him to "take shots," as it is called. Those who have had experience in accurate work know how easy it would be to save time and trouble by omitting precautions and passing over discrepancies, and yet, even without dishonest intention, to convey the impression of conscientious attention to details. Although the most careful and experienced cannot hope to escape occasional mistakes, the effective value of this kind of work depends much upon the reputation of the individual responsible for it.

In estimating the present position and prospects of experimental science, there is good ground for encouragement. The multiplication of laboratories gives to the younger generation opportunities such as have never existed before, and which excite the envy of those who have had to learn in middle life much that now forms part of an undergraduate course. As to the management of such institutions, there is room for a healthy difference of opinion. For many kinds of original work, especially in connection with accurate measurement, there is need of expensive apparatus; and it is often difficult to persuade a student to do his best with imperfect appliances when he knows that by other means a better result could be attained with greater facility. Nevertheless it seems to me important to discourage too great reliance upon the instrument-maker. Much of the best original work has been done with the homeliest appliances; and the endeavour to turn to the best account the means that may be at hand develops ingenuity and resource more than the most elaborate determinations with ready-made instruments. There is danger otherwise that the experimental education of a plodding student should be too mechanical and artificial, so that he is puzzled by small changes of apparatus much as many school-boys are puzzled by a transposition of the letters in a diagram of Euclid.

From the general spread of a more scientific education we are warranted in expecting important results. Just as there are some brilliant literary men with an inability, or at least a distaste practically amounting to inability, for scientific ideas, so there are a few with scientific tastes whose imaginations are never touched by merely literary studies. To save these from intellectual stagnation during several important years of their lives is something gained; but the thoroughgoing advocates of scientific education aim at much more. To them it appears strange, and almost monstrous, that the dead languages should hold the place they do in general education; and it can hardly be denied that their supremacy is the result of routine rather than of argument. I do not myself take up the extreme position. I doubt whether an exclusively scientific training would be satisfactory; and where there is plenty of time and a literary aptitude I can believe that Latin and Greek may make a good foundation. But it is useless to discuss the question upon the supposition that the majority of boys attain either to a knowledge of the languages or to an appreciation of the writings of the ancient authors. The contrary is notoriously the truth; and the defenders of the existing system usually take their stand upon the excellence of its discipline. From this point of view there is something to be said. The laziest boy must exert himself a little in puzzling out a sentence with grammar and dictionary, while instruction and supervision are easy to organise and not too costly. But when the case is stated plainly, few will agree that we can afford so entirely to disregard results. In after life the intellectual energies are usually engrossed with business, and no further opportunity is found for attacking the difficulties which block the gateways of knowledge. Mathematics, especially, if not learned young, are likely to remain unlearned. I will not further insist upon the educational importance of mathematics and science, because with respect to them I shall probably be supposed to be prejudiced. But of modern languages I am ignorant enough to give value to my advocacy. I believe that French and German, if properly taught, which I admit they rarely are at present, would go far to replace Latin and Greek from a disciplinary point of view, while the actual value of the acquisition would, in the majority of cases, be incomparably greater. In half the time usually devoted without success to the classical languages, most boys could acquire a really serviceable knowledge of French and German. History and the serious study of English literature, now shamefully neglected, would also find a place in such a scheme.

There is one objection often felt to a modernised education, as to which a word may not be without use. Many excellent people are afraid of science as tending towards materialism. That such apprehension should exist is not surprising, for unfortunately there are writers, speaking in the name of science, who have set themselves to foster it. It is true that among scientific men, as in other classes, crude views are to be met with as to the deeper things of Nature; but that the life-long beliefs of Newton, of Faraday, and of Maxwell are inconsistent with the scientific habit of mind is surely a proposition which I need not pause to refute. It would be easy, however, to lay too much stress upon the opinions of even such distinguished workers as these. Men who devote their lives to investigation cultivate a love of truth for its own sake, and endeavour instinctively to clear up, and not, as is too often the object in business and politics, to obscure, a difficult question. So far the opinion of a scientific worker may have a special value; but I do not think that he has a claim, superior to that of other educated men, to assume the attitude of a prophet. In his heart he knows that underneath the theories that he constructs there lie contradictions which he cannot reconcile. The higher mysteries of being, if penetrable at all by human intellect, require other weapons than those of calculation and experiment.

Without encroaching upon grounds appertaining to the theologian and the philosopher, the domain of natural science is surely broad enough to satisfy the wildest ambition of its devotees. In other departments of human life and interest, true progress is rather an article of faith than a rational belief; but in science a retrograde movement is, from the nature of the case, almost impossible. Increasing knowledge brings with it increasing power, and great as are the triumphs of the present century, we may well believe that they are but a foretaste of what discovery and invention have yet in store for mankind. Encouraged by the thought that our labours cannot be thrown away, let us redouble our efforts in the noble struggle. In the

Old World and in the New, recruits must be enlisted to fill the place of those whose work is done. Happy should I be if, through this visit of the Association, or by any words of mine, a larger measure of the youthful activity of the West could be drawn into this service. The work may be hard, and the discipline severe, but the interest never fails, and great is the privilege of achievement.

SECTION A

MATHEMATICAL AND PHYSICAL SCIENCE

OPENING ADDRESS BY PROF. SIR WILLIAM THOMSON, M.A., LL.D., D.C.L., F.R.SS.L. & E., F.R.A.S., PRESIDENT OF THE SECTION

Steps towards a Kinetic Theory of Matter

THE now well-known kinetic theory of gases is a step so important in the way of explaining seemingly static properties of matter by motion, that it is scarcely possible to help anticipating in idea the arrival at a complete theory of matter, in which all its properties will be seen to be merely attributes of motion. If we are to look for the origin of this idea, we must go back to Democritus, Epicurus, and Lucretius. We may then, I believe, without missing a single step, skip 1800 years. Early last century we find in Malebranche's "Recherche de la Verité," the statement that "La dureté de corps" depends on "petits tourbillons."[1] These words, embedded in a hopeless mass of unintelligible statements of the physical, metaphysical, and theological philosophies of the day, and unsupported by any explanation, elucidation, or illustration throughout the rest of the three volumes, and only marred by any other single sentence or word to be found in the great book, still do express a distinct conception, which forms a most remarkable step towards the kinetic theory of matter. A little later we have Daniel Bernoulli's promulgation of what we now accept as a surest article of scientific faith—the kinetic theory of gases. He, so far as I know, thought only of the Boyle's and Marriot's law of the "spring of air," as Boyle called it, without reference to change of temperature or the augmentation of its pressure if not allowed to expand for elevation of temperature, a phenomenon which perhaps he scarcely knew, still less the elevation of temperature produced by compression, and the lowering of temperature by dilatation, and the consequent necessity of waiting for a fraction of a second or a few seconds of time (with apparatus of ordinary experimental magnitude), to see a subsidence from a larger change of pressure, down to the amount of change that verifies Boyle's law. The consideration of these phenomena forty years ago by Joule, in connection with Bernoulli's original conception, formed the foundation of the kinetic theory of gases as we now have it. But what a splendid and useful building has been placed on this foundation by Clausius and Maxwell, and what a beautiful ornament we see on the top of it in the radiometer of Crookes, securely attached to it by the happy discovery of Tait and Dewar,[2] that the length of the free path of the residual molecules of air in a good modern vacuum may amount to several inches. Clausius' and Maxwell's explanations of the diffusion of gases, and of thermal conduction in gases, their charmingly intelligible conclusion that in gases the diffusion of heat is just a little more rapid than the diffusion of molecules, because of the interchange of energy in collisions between molecules,[3] while the chief transference of heat is by actual transport of the molecules themselves, and Maxwell's explanation of the viscosity of gases, with the absolute numerical relations which the work of those two great discoverers found among the three properties of diffusion, thermal conduction, and viscosity, have annexed to the domain of science a vast and ever-growing province.

[1] "Preuve de la supposition que j'ay faite : Que la matière subtile ou éthérée est nécessairement composée de PETITS TOURBILLONS ; et qu'ils sont les causes naturelles de tous les changements qui arrivent à la matière ; ce que je confirme par l'explication des effets le plus généraux de la Physique, tels que sont la dureté des corps, leur fluidité, leur pesanteur, leur légèreté, la lumière et la réfraction et réflexion de ses rayons."—Malebranche, "Recherche de la Vérité," 1712.

[2] *Proc.* R. S. E., March 2, 1874, and July 5, 1875.

[3] On the other hand, in liquids, on account of the crowdedness of the molecules, the diffusion of heat must be chiefly by interchange of energies between the molecules, and should be, as experiment proves it is, enormously more rapid than the diffusion of the molecules themselves, and this again ought to be much less rapid than either the material or thermal diffusivities of gases. Thus the diffusivity of common salt through water was found by Fick to be as small as ·0000112 square centimetres per second ; nearly 200 times as great as this is the diffusivity of heat through water, which was found by J. T. Bottomley to be about ·002 square centimetres per second. The material diffusivities of gases, according to Loschmidt's experiments, range from ·098 (the interdiffusivity of carbonic acid and nitrous oxide) to ·642 (the interdiffusivity of carbonic oxide and hydrogen), while the thermal diffusivities of gases, calculated according to Clausius' and Maxwell's kinetic theory of gases, are ·089 for carbonic acid, ·16 for common air or other gases of nearly the same density, and 1·12 for hydrogen (all, both material and thermal, being reckoned in square centimetres per second).

Rich as it is in practical results, the kinetic theory of gases, as hitherto developed, stops absolutely short at the atom or molecule, and gives not even a suggestion towards explaining the properties in virtue of which the atoms or molecules mutually influence one another. For some guidance towards a deeper and more comprehensive theory of matter, we may look back with advantage to the end of last century, and the beginning of this century, and find Rumford's conclusion regarding the heat generated in boring a brass gun: "It appears to me to be extremely difficult, if not quite impossible, to form any distinct idea of anything capable of being excited and communicated in the manner the heat was excited and communicated in these experiments, except it be MOTION," and Davy's still more suggestive statements: "The phenomena of repulsion are not dependent on a peculiar elastic fluid for their existence. . . ." "Heat may be defined as a peculiar motion, probably a vibration, of the corpuscles of bodies, tending to separate them. . . ." "To distinguish this motion from others, and to signify the causes of our sensations of heat, &c., the name *repulsive* motion has been adopted." Here we have a most important idea. It would be somewhat a bold figure of speech to say the earth and moon are kept apart by a repulsive motion; and yet, after all, what is centrifugal force but a repulsive motion, and may it not be that there is no such thing as repulsion, and that it is solely by inertia that what seems to be repulsion is produced? Two bodies fly together, and, accelerated by mutual attraction, if they do not precisely hit one another, they cannot but separate in virtue of the inertia of their masses. So, after dashing past one another in sharply concave curves round their common centre of gravity, they fly asunder again. A careless onlooker might imagine they had repelled one another, and might not notice the difference between what he actually sees and what he would see if the two bodies had been projected with great velocity towards one another, and either colliding and rebounding, or repelling one another into sharply convex continuous curves, fly asunder again.

Joule, Clausius, and Maxwell, and no doubt Daniel Bernoulli himself, and I believe every one who has hitherto written or done anything very explicit in the kinetic theory of gases, has taken the mutual action of molecules in collision as repulsive. May it not after all be attractive? This idea has never left my mind since I first read Davy's "Repulsive Motion," about thirty-five years ago, but I never made anything of it, at all events have not done so until to-day (June 16, 1884)—if this can be said to be making anything of it—when, in endeavouring to prepare the present address, I notice that Joule's and my own old experiments[1] on the thermal effect of gases expanding from a high-pressure vessel through a porous plug, proves the less dense gas to have greater intrinsic *potential* energy than the denser gas, if we assume the ordinary hypothesis regarding the temperature of a gas, according to which two gases are of equal temperatures[2] when the kinetic energies of their constituent molecules are of equal average amounts per molecule.

Think of the thing thus. Imagine a great multitude of particles inclosed by a boundary which may be pushed inwards in any part all round at pleasure. Now station an engineer corps of Maxwell's army of sorting demons all round the inclosure, with orders to push in the boundary diligently everywhere, when none of the besieged troops are near, and to do nothing when any of them are seen approaching, and until after they have turned again inwards. The result will be that, with exactly the same sum of kinetic and potential energies of the same inclosed multitude of particles, the throng has been caused to be denser. Now Joule's and my own old experiments on the efflux of air prove that if the crowd be common air, or

[1] Republished in Sir W. Thomson's "Mathematical and Physical Papers," vol. i. Article xlix. p. 381.

[2] That this is a mere hypothesis has been scarcely remarked by the founders themselves, nor by almost any writer on the kinetic theory of gases. No one has yet examined the question: What is the condition as regards average distribution of kinetic energy, which is ultimately fulfilled by two portions of gaseous matter, separated by a thin elastic septum which absolutely prevents interdiffusion of matter, while it allows interchange of kinetic energy by collisions against itself? Indeed I do not know but that the present is the very first statement which has ever been published of this condition of the problem of equal temperatures between two gaseous masses.

oxygen, or nitrogen, or carbonic acid, the temperature is a little higher in the denser than in the rarer condition when the energies are the same. By the hypothesis, equality of temperature between two different gases or two portions of the same gas at different densities means equality of kinetic energies in the same number of molecules of the two. From our observations proving the temperature to be higher, it therefore follows that the potential energy is smaller in the condensed crowd. This—always, however, under protest as to the temperature hypothesis—proves some degree of attraction among the molecules, but it does not prove ultimate attraction between two molecules in collision, or at distances much less than the average mutual distance of nearest neighbours in the multitude. The collisional force might be repulsive, as generally supposed hitherto, and yet attraction might predominate in the whole reckoning of difference between the intrinsic potential energies of the more dense and less dense multitudes. It is, however, remarkable that the explanation of the propagation of sound through gases, and even of the positive fluid pressure of a gas against the sides of the containing vessel, according to the kinetic theory of gases, is quite independent of the question whether the ultimate collisional force is attractive or repulsive. Of course it must be understood that, if it is attractive, the particles must be so small that they hardly ever meet—they would have to be infinitely small to *never* meet—that, in fact, they meet so seldom, in comparison with the number of times their courses are turned through large angles by attraction, that the influence of these purely attractive collisions is preponderant over that of the comparatively very rare impacts from actual contact. Thus, after all, the train of speculation suggested by Davy's "Repul ive Motion" does not allow us to escape from the idea of true repulsion, does not do more than let us say it is of no consequence, nor even say this with truth, because, if there are impacts at all, the nature of the force during the impact and the effects of the mutual impacts, however rare, cannot be evaded in any attempt to realise a conception of the kinetic theory of gases. And in fact, unless we are satisfied to imagine the atoms of a gas as mathematical points endowed with inertia, and, as according to Boscovich, endowed with forces of mutual positive and negative attraction, varying according to some definite function of the distance, we cannot avoid the question of impacts, and of vibrations and rotations of the molecules resulting from impacts, and we must look distinctly on each molecule as being either a little elastic solid or a configuration of motion in a continuous all-pervading liquid. I do not myself see how we can ever permanently rest anywhere short of this last view; but it would be a very pleasant temporary resting-place on the way to it if we could, as it were, make a mechanical model of a gas out of little pieces of round perfectly elastic solid matter, flying about through the space occupied by the gas, and colliding with one another and against the sides of the containing vessel. This is, in fact, all we have of the kinetic theory of gases up to the present time, and this has done for us, in the hands of Clausius and Maxwell, the great things which constitute our first step towards a molecular theory of matter. Of course from it we should have to go on to find an explanation of the elasticity and all the other properties of the molecules themselves, a subject vastly more complex and difficult than the gaseous properties, for the explanation of which we assume the elastic molecule; but without any explanation of the properties of the molecule itself, with merely the assumption that the molecule has the requisite properties, we might rest happy for a while in the contemplation of the kinetic theory of gases, and its explanation of the gaseous properties, which is not only stupendously important as a step towards a more thoroughgoing theory of matter, but is undoubtedly the expression of a perfectly intelligible and definite set of facts in Nature. But alas for our mechanical model consisting of the cloud of little elastic solids flying about amongst one another. Though each particle have absolutely perfect elasticity, the end must be pretty much the same as if it were but imperfectly elastic. The average effect of repeated and repeated mutual collisions must be to gradually convert all the translational energy into energy of shriller and shriller vibrations of the molecule. It seems certain that each collision must have something more of energy in vibrations of very finely divided nodal parts than there was of energy in such vibrations before the impact. The more minute this nodal subdivision, the less must be the tendency to give up part of the vibrational energy into the shape of translational energy in the course of a collision, and I think it is rigorously demonstrable that the whole translational energy must ultimately become transformed into vibrational energy of higher and higher nodal subdivisions if each molecule is a continuous elastic solid. Let us, then, leave the kinetic theory of gases for a time with this difficulty unsolved, in the hope that we or others after us may return to it, armed with more knowledge of the properties of matter, and with sharper mathematical weapons to cut through the barrier which at present hides from us any view of the molecule itself, and of the effects other than mere change of translational motion which it experiences in collision.

To explain the elasticity of a gas was the primary object of the kinetic theory of gases. This object is only attainable by the assumption of an elasticity more complex in character, and more difficult of explanation, than the elasticity of gases—the elasticity of a solid. Thus, even if the fatal fault in the theory, to which I have alluded, did not exist, and if we could be perfectly satisfied with the kinetic theory of gases founded on the collisions of elastic solid molecules, there would still be beyond it a grander theory which need not be considered a chimerical object of scientific ambition—to explain the elasticity of solids. But we may be stopped when we commence to look in the direction of such a theory with the cynical question: What do you mean by explaining a property of matter? As to being stopped by any such question, all I can say is that if engineering were to be all and to end all physical science, we should perforce be content with merely finding properties of matter by observation, and using them for practical purposes. But I am sure very few, if any, engineers are practically satisfied with so narrow a view of their noble profession. They must and do patiently observe, and discover by observation, properties of matter, and results of material combinations. But deeper questions are always present, and always fraught with interest to the true engineer, and he will be the last to give weight to any other objection to any attempt to see below the surface of things than the practical question: Is it likely to prove wholly futile? But now, instead of imagining the question: What do you mean by explaining a property of matter? to be put cynically, and letting ourselves be irritated by it, suppose we give to the questioner credit for being sympathetic, and condescend to try and answer his question. We find it not very easy to do so. All the properties of matter are so connected that we can scarcely imagine one *thoroughly explained* without our seeing its relation to all the others, without in fact having the explanation of all, and till we have this we cannot tell what we mean by "explaining a property," or "explaining the properties" of matter. But though this consummation may never be reached by man, the progress of science may be, I believe will be, step by step towards it, on many different roads converging towards it from all sides. The kinetic theory of gases is, as I have said, a true step on one of the roads. On the very distinct road of chemical science, St. Clair Deville arrived at his grand theory of dissociation without the slightest aid from the kinetic theory of gases. The fact that he worked it out solely from chemical observation and experiment, and expounded it to the world without any hypothesis whatever, and seemingly even without consciousness of the beautiful explanation it has in the kinetic theory of gases, secured for it immediately an independent solidity and importance as a chemical theory when he first promulgated it, to which it might even by this time scarcely have attained if it had first been suggested as a probability indicated by the kinetic theory of gases, and been only afterwards confirmed by observation. Now, however, guided by the views which Clausius and Williamson have given us of the continuous interchange of partners between the compound molecules constituting chemical compounds in the gaseous state, we see in Deville's theory of dissociation a point of contact of the most transcendent interest between the chemical and physical lines of scientific progress.

To return to elasticity: if we could make out of matter devoid of elasticity a combined system of relatively moving parts which, in virtue of motion, has the essential characteristics of an elastic body, this would surely be, if not positively a step in the kinetic theory of matter, at least a finger-post pointing a way which we may hope will lead to a kinetic theory of matter. Now this, as I have already shown,[1] we can do in several ways. In the case of the last of the communications referred to, of which only the title has hitherto been published, I showed that, from the

[1] Paper on "Vortex Atoms," *Proc.* R. S. E. February 1867; abstract of a lecture before the Royal Institution of Great Britain, March 4, 1881, on "Elasticity Viewed as possibly a Mode of Motion"; Thomson and Tait's "Natural Philosophy," second edition, part 1, §§ 345 viii. to 345 xxvii.; "On Oscillation and Waves in an Adynamic Gyrostatic System" (title only), *Proc.* R. S. E. March 1883.

mathematical investigation of a gyrostatically dominated combination contained in the passage of Thomson and Tait's "Natural Philosophy" referred to, it follows that any ideal system of material particles, acting on one another mutually through massless connecting springs, may be perfectly imitated in a model consisting of rigid links jointed together, and having rapidly rotating fly-wheels pivoted on some or on all of the links. The imitation is not confined to cases of equilibrium. It holds also for vibration produced by disturbing the system infinitesimally from a position of stable equilibrium and leaving it to itself. Thus we may make a gyrostatic system such that it is in equilibrium under the influence of certain positive forces applied to different points of this system; all the forces being precisely the same as, and the points of application similarly situated to, those of the stable system with springs. Then, provided proper masses (that is to say, proper amounts and distributions of inertia) be attributed to the links, we may remove the external forces from each system, and the consequent vibration of the points of application of the forces will be identical. Or we may act upon the systems of material points and springs with any given forces for any given time, and leave it to itself, and do the same thing for the gyrostatic system; the consequent motion will be the same in the two cases. If in the one case the springs are made more and more stiff, and in the other case the

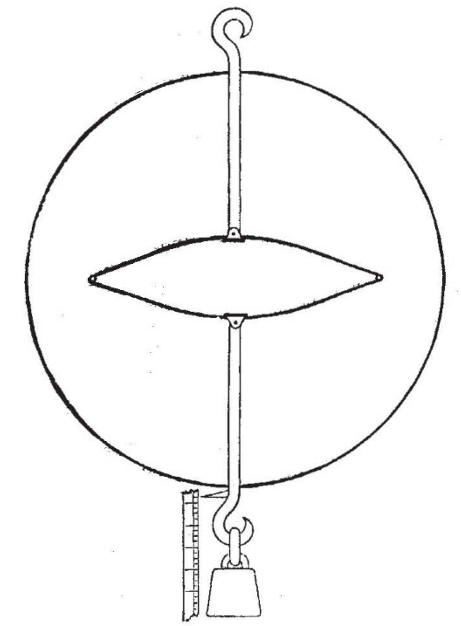

FIG. 1.

angular velocities of the fly-wheels are made greater and greater, the periods of the vibrational constituents of the motion will become shorter and shorter, and the amplitudes smaller and smaller, and the motions will approach more and more nearly those of two perfectly rigid groups of material points, moving through space and rotating according to the well-known mode of rotation of a rigid body having unequal moments of inertia about its three principal axes. In one case the ideal nearly rigid connection between the particles is produced by massless exceedingly stiff springs; in the other case it is produced by the exceedingly rapid rotation of the fly-wheels in a system which, when the fly-wheels are deprived of their rotation, is perfectly limp.

The drawings (Figs. 1 and 2) before you illustrate two such material systems.[1] The directions of rotation of the fly-wheels in the gyrostatic system (Fig. 2) are indicated by directional ellipses, which show in perspective the direction of rotation of the fly-wheel of each gyrostat. The gyrostatic system (Fig. 2) might have been constituted of two gyrostatic members, but four are shown for symmetry. The inclosing circle represents in each case in section an inclosing spherical shell to prevent the interior from being seen. In the inside of one there are fly-wheels, in the inside of the other a massless spring. The projecting hooked rods seem as if they are connected by a spring in each case. If we hang any one of the systems up by the hook on one of its projecting rods, and hang a weight to the hook of the other projecting rod, the weight, when first put on, will oscillate up and down, and will go on doing so for ever if the system be absolutely unfrictional. If we check the vibration by hand, the weight will hang down at rest, the pin drawn out to a certain degree; and the distance drawn out will be simply proportional to the weight hung on, as in an ordinary spring balance.

Here, then, out of matter possessing rigidity, but absolutely devoid of elasticity, we have made a perfect model of a spring in the form of a spring balance. Connect millions of millions of particles by pairs of rods such as these of this spring balance, and we have a group of particles constituting an elastic solid; exactly fulfilling the mathematical ideal worked out by Navier, Poisson, and Cauchy, and many other mathematicians who, fol-

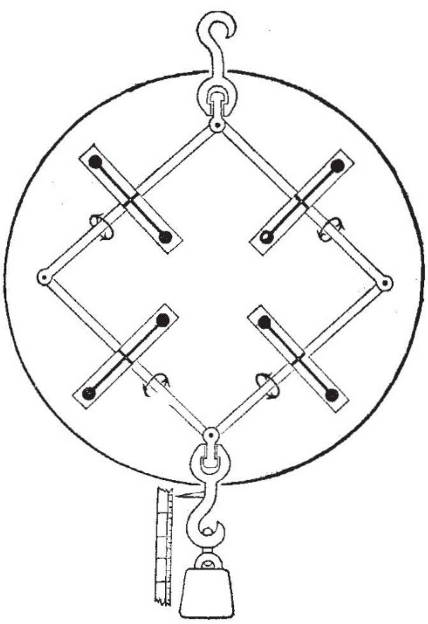

FIG. 2.

lowing their example, have endeavoured to found a theory of the elasticity of solids on mutual attraction and repulsion between a group of material particles. All that can possibly be done by this theory, with its assumption of forces acting according to any assumed law of relation to distance, *is* done by the gyrostatic system. But the gyrostatic system does, besides, what the system of naturally acting material particles cannot do: it constitutes an elastic solid which can have the Faraday magneto-optic rotation of the plane of polarisation of light; supposing the application of our solid to be a model of the luminiferous ether for illustrating the undulatory theory of light. The gyrostatic model spring balance is arranged to have zero moment of momentum as a whole, and therefore to contribute nothing to the Faraday rotation; with this arrangement the model illustrates the luminiferous ether in a field unaffected by magnetic force. But now let there be a different rotational velocity imparted to the jointed square round the axis of the two projecting hooked rods, such as to give a resultant moment of momentum round any given line through the centre of inertia of the system, and let pairs of the hooked rods in the model thus altered, which is no longer a model of a mere spring balance, be applied as connections between millions of pairs of particles as before; with the lines of resultant moment of momentum all similarly directed. We now have a model elastic

[1] In Fig. 1 the two hooked rods seen projecting from the sphere are connected by an elastic coach-spring. In Fig. 2 the hooked rods are connected one to each of two opposite corners of a four-sided jointed frame, each member of which carries a gyrostat so that the axis of rotation of the fly-wheel is in the axis of the member of the frame which bears it. Each of the hooked rods in Fig. 2 is connected to the framework through a swivel joint, so that the whole gyrostatic framework may be rotated about the axis of the hooked rods in order to annul the moment of momentum of the framework about this axis due to rotation of the fly-wheels in the gyrostat.

solid which will have the property that the direction of vibration in waves of rectilinear vibrations propagated through it shall turn round the line of propagation of the waves, just as Faraday's observation proves to be done by the line of vibration of light in a dense medium between the poles of a powerful magnet. The case of wave front perpendicular to the lines of resultant moment of momentum (that is to say, the direction of propagation being parallel to these lines) corresponds, in our mechanical model, to the case of light travelling in the direction of the lines of force in a magnetic field.

In these illustrations and models we have different portions of ideal rigid matter acting upon one another, by normal pressure at mathematical points of contact—of course no forces of friction are supposed. It is exceedingly interesting to see how thus, with no other postulates than inertia, rigidity, and mutual impenetrability, we can thoroughly model not only an elastic solid, and any combination of elastic solids, but so complex and recondite a phenomenon as the passage of polarised light through a magnetic field. But now, with the view of ultimately discarding the postulate of rigidity from all our materials, let us suppose some to be absolutely destitute of rigidity, and to possess merely inertia and incompressibility, and mutual impenetrability with reference to the still remaining rigid matter. With these postulates we can produce a perfect model of mutual action at a distance between solid particles, fulfilling the condition, so keenly desired by Newton and Faraday, of being explained by continuous action through an intervening medium. The law of the mutual force in our model, however, is not the simple Newtonian law, but the much more complex law of the mutual action between electro-magnets—with this difference, that in the hydro-kinetic model in every case the force is opposite in direction to the corresponding force in the electro-magnetic analogue. Imagine a solid bored through with a hole and placed in our ideal perfect liquid. For a moment let the hole be stopped by a diaphragm, and let an impulsive pressure be applied for an instant uniformly over the whole membrane, and then instantly let the membrane be dissolved into liquid. This action originates a motion of the liquid relatively to the solid, of a kind to which I have given the name of "irrotational circulation," which remains absolutely constant however the solid be moved through the liquid. Thus, at any time the actual motion of the liquid at any point in the neighbourhood of the solid will be the resultant of the motion it would have in virtue of the circulation alone, were the solid at rest, and the motion it would have in virtue of the motion of the solid itself, had there been no circulation established through the aperture. It is interesting and important to remark in passing that the whole kinetic energy of the liquid is the sum of the kinetic energies which it would have in the two cases separately. Now, imagine the whole liquid to be inclosed in an infinitely large, rigid, containing vessel, and in the liquid, at an infinite distance from any part of the containing vessel, let two perforated solids, with irrotational circulation through each, be placed at rest near one another. The resultant fluid motion due to the two circulations will give rise to fluid pressure on the two bodies, which, if unbalanced, will cause them to move. The force systems—force-and-torques, or pairs of forces—required to prevent them from moving will be mutual and opposite, and will be the same as, but opposite in direction to, the mutual force systems required to hold at rest two electro-magnets fulfilling the following specification. The two electro-magnets are to be of the same shape and size as the two bodies, and to be placed in the same relative positions, and to consist of infinitely thin layers of electric currents in the surfaces of solids possessing extreme diamagnetic quality—in other words, infinitely small permeability. The distribution of electric current on each body may be any whatever which fulfils the condition that the total current across any closed line drawn on the surface once through the aperture is equal to $1/4\pi$ of the circulation[1] through the aperture in the hydro-kinetic analogue.

It might be imagined that the action at a distance thus provided for by fluid motion could serve as a foundation for a theory of the equilibrium, and the vibrations, of elastic solids, and the transmission of waves like those of light through an extended quasi-elastic solid medium. But unfortunately for this idea the equilibrium is essentially unstable, both in the case of magnets, and, notwithstanding the fact that the forces are oppositely directed, in the hydro-kinetic analogue also, when the several movable bodies (two or any greater number) are so placed relatively as to be in equilibrium. If, however, we connect the perforated bodies with circulation through them in the hydro-kinetic system, by jointed rigid connecting links, we may arrange for configurations of stable equilibrium. Thus without fly-wheels, but with fluid circulations through apertures, we may make a model spring balance, or a model luminiferous ether, either without or with the rotational quality corresponding to that of the true luminiferous ether in the magnetic fluid —in short, do all by the perforated solids with circulations through them that we saw we could do by means of linked gyrostats. But something that we cannot do by linked gyrostats we can do by the perforated bodies with fluid circulation: we can make a model gas. The mutual action at a distance, repulsive or attractive according to the mutual aspect of the two bodies when passing within collisional distance[1] of one another, suffices to produce the change of direction of motion in collision, which essentially constitutes the foundation of the kinetic theory of gases, and which, as we have seen before, may as well be due to attraction as to repulsion, so far as we know from any investigation hitherto made in this theory.

There remains, however, as we have seen before, the difficulty of providing for the case of actual impacts between the solids, which must be done by giving them massless spring buffers, or, which amounts to the same thing, attributing to them repulsive forces sufficiently powerful at very short distances to absolutely prevent impacts between solid and solid; unless we adopt the equally repugnant idea of infinitely small perforated solids, with infinitely great fluid circulations through them. Were it not for this fundamental difficulty, the hydro-kinetic model gas would be exceedingly interesting; and, though we could scarcely adopt it as conceivably a true representation of what gases really are, it might still have some importance as a model configuration of solid and liquid matter, by which without elasticity the elasticity of a true gas might be represented.

But lastly, since the hydro-kinetic model gas with perforated solids and fluid circulations through them fails because of the impacts between the solids, let us annul the solids and leave the liquid performing irrotational circulation round vacancy,[2] in the place of the solid cores which we have hitherto supposed; or let us annul the rigidity of the solid cores of the rings and give them molecular rotation according to Helmholtz's theory of vortex motion. For stability the molecular rotation must be such as to give the same velocity at the boundary of the rotational fluid core as that of the irrotationally circulating liquid in contact with it, because, as I have proved, frictional slip between two portions of liquid in contact is inconsistent with stability. There is a further condition, upon which I cannot enter into detail just now, but which may be understood in a general way when I say that it is a condition of either uniform or of increasing molecular rotation from the surface inwards, analogous to the condition that the density of a liquid, resting for example under the influence of gravity, must either be uniform or must be greater below than above for stability of equilibrium. All that I have said in favour of the model vortex gas composed of perforated solids with fluid circulations through them holds without modification for the purely hydro-kinetic model, composed of either Helmholtz cored vortex-rings or of coreless vortices, and we are now troubled with no such difficulty as that of the impacts between solids. Whether, however, when the vortex theory of gases is thoroughly worked out, it will or will not be found to fail in a manner analogous to the failure which I have already pointed out in connection with the kinetic theory of gases composed of little elastic solid molecules, I cannot at present undertake to speak with certainty. It seems to me most probable that the vortex theory cannot fail in any such way, because all I have been able to find out hitherto regarding the vibration of

[1] The integral of tangential component velocity all round any closed curve, passing once through the aperture, is defined as the "cyclic constant," or the "circulation" ("Vortex Motion," § 60 (*a*), *Trans.* R. S. E. April 29, 1867). It has the same value for all closed curves passing just once through the aperture, and it remains constant through all time, whether the solid body be in motion or at rest.

[1] According to this view there is no precise distance, or definite condition respecting the distance, between two molecules, at which apparently they come to be in collision, or when receding from one another they cease to be in collision. It is convenient, however, in the kinetic theory of gases, to adopt arbitrarily a precise definition of collision, according to which two bodies or particles mutually acting at a distance may be said to be in collision when their mutual action exceeds some definite arbitrarily assigned limit, as, for example, when the radius of curvature of the path of either body is less than a stated fraction (1/100, for instance) of the distance between them.

[2] Investigations respecting coreless vortices will be found in a paper by the author, "Vibrations of a Columnar Vortex," *Proc.* R. S. E., March 1 1880; and a paper by Hicks, recently read before the Royal Society.

vortices,[1] whether cored or coreless, does not seem to imply the liability of translational or impulsive energies of the individual vortices becoming lost in energy of smaller and smaller vibrations.

As a step towards kinetic theory of matter it is certainly most interesting to remark that in the quasi-elasticity, elasticity looking like that of an india-rubber band, which we see in a vibrating smoke-ring launched from an elliptic aperture, or in two smoke-rings which were circular, but which have become deformed from circularity by mutual collision, we have in reality a virtual elasticity in matter devoid of elasticity, and even devoid of rigidity, the virtual elasticity being due to motion, and generated by the generation of motion.

SECTION B

CHEMICAL SCIENCE

OPENING ADDRESS BY PROF. SIR HENRY ENFIELD ROSCOE, PH.D., LL.D., F.R.S., F.C.S., PRESIDENT OF THE SECTION

WITH the death of Berzelius in 1848 ended a well-marked epoch in the history of our science; with that of Dumas—and, alas! that of Wurtz also—in 1884 closes a second. It may not perhaps be unprofitable on the present occasion to glance at some few points in the general progress which chemistry has made during this period, and thus to contrast the position of the science in the "sturm und drang" year of 1848, with that in the present, perhaps quieter, period.

The differences between what may probably be termed the Berzelian era and that with which the name of Dumas will for ever be associated show themselves in many ways, but in none more markedly than by the distinct views entertained as to the nature of a chemical compound.

According to the older notions, the properties of compounds are essentially governed by a qualitative nature of their constituent atoms, which were supposed to be so arranged as to form a binary system. Under the new ideas, on the other hand, it is mainly the number and arrangement of the atoms within the molecule, which regulate the characteristics of the compound which is to be looked on not as built up of two constituent groups of atoms, but as forming one group.

Amongst those who successfully worked to secure this important change of view on a fundamental question of chemical theory, the name of Dumas himself must first be mentioned, and, following upon him, the great chemical twin-brethren Laurent and Gerhardt, who, using both the arguments of test-tube and of pen in opposition to the prevailing views, gradually succeeded, though scarcely during the lifetime of the first, in convincing chemists that the condition of things could hardly be a healthy one when chemistry was truly defined "as the science of bodies which do not exist." For Berzelius, adhering to his preconceived notions, had been forced by the pressure of new discovery into the adoption of formulæ which gradually became more and more complicated, and led to more and more doubtful hypotheses, until his followers at last could barely succeed in building up the original radical from its numerous supposed component parts. Such a state of things naturally brought about its own cure, and the unitary formulæ of Gerhardt began to be generally adopted.

It was not, however, merely as an expression of the nature of the single chemical compound that this change was beneficial, but, more particularly, because it laid open the general analogies of similarly constituted compounds, and placed fact as the touchstone by which the constitution of these allied bodies should be ascertained. Indeed, Gerhardt, in 1852, gave evidence of the truth of this in his well-known theory of type, according to which, organic compounds of ascertained constitution can be arranged under the four types of hydrogen, hydrochloric acid, water, and ammonia, and of which it is, perhaps, not too much to say that it has, more than any other of its time, contributed to the clearer understanding of the relations existing amongst chemical compounds.

Another striking difference of view between the chemistry of the Berzelian era and that of what we sometimes term the modern epoch is illustrated by the so-called substitution theory. Dumas, to whom we owe this theory, showed that chlorine can take the place of hydrogen in many compounds, and that the resulting body possesses characters similar to the original. Berzelius opposed this view, insisting that the essential differences between these two elements rendered the idea of a substitution impossible, and notwithstanding the powerful advocacy of Liebig, and the discovery by Melsens of reverse substitutions (that is, the reformation of the original compound from its substitution-product), Berzelius remained to the end unconvinced, and that which was in reality a confirmation of his own theory of compound radicals, which, as Liebig says, "illumined many a dark chapter in organic chemistry," was looked upon by him as an error of the deepest dye. This inability of many minds to see in the discoveries of others confirmation of their own views is not uncommon; thus Dalton, we may remember, could never bring himself to admit the truth of Gay-Lussac's laws of gaseous volume-combination, although, as Berzelius very truly says, if we write *atom* for *volume* and consider the substance in the solid state in place of the state of gas, the discovery of Gay-Lussac is seen to be one of the most powerful arguments in favour of Dalton's hypothesis.

But there is another change of view, dating from the commencement of the Dumas epoch, which has exerted an influence equal, if not superior, to those already named on the progress of our science. The relative weights of the ultimate particles, to use Dalton's own words, which up to this time had been generally adopted by chemists, were the equivalent weights of Dalton and Wollaston, representing, in the case of oxygen and hydrogen, the proportions in which these elements combine, viz. as 8 to 1. The great Swedish chemist at this time stood almost alone in supporting another hypothesis; for, founding his argument on the simple laws of volume-combination enunciated by Gay-Lussac, he asserted that the true atomic weights are to be represented by the relations existing between equal volumes of the two gases, viz. as 16 to 1. Still these views found no favour in the eyes of chemists until Gerhardt, in 1843, proposed to double the equivalent weights of oxygen, sulphur, and carbon, and then the opposition which this suggestion met with was most intense, Berzelius himself not even deigning to mention it in his annual account of the progress of the science, thus proving the truth of his own words: "That to hold an opinion habitually often leads to such an absolute conviction of its truth that its weak points are unregarded, and all proofs against it ignored." Nor were these views generally adopted by chemists until Cannizaro, in 1858, placed the whole subject on its present firm basis by clearly distinguishing between equivalent and molecular weights, showing how the atomic weights of the constituent elements are derived from the molecular weights of their volatile compounds based upon the law of Avogadro and Ampère, or where, as is the case with many metals, no compounds of known vapour-density exist, how the same result may be ascertained by the help of the specific heat of the element itself. Remarkable as it may appear, it is nevertheless true that it is in the country of their birth that Gerhardt's atomic weights and the consequent atomic nomenclature have met with most opposition, so much so that within a year or two of the present time there was not a single course of lectures delivered in Paris in which these were used.

The theory of organic radicals, developed by Liebig so long ago as 1834, received numerous experimental confirmations in succeeding years. Bunsen's classical research on cacodyl, proving the possibility of the existence of metallo-organic radicals capable of playing the part of a metal, and the isolation of the hydrocarbon ethyl by Frankland in 1849, laid what the supporters of the theory deemed the final stone in the structure.

The fusion of the radical and type theories, chiefly effected by the discovery in 1849 of the compound ammonias by Wurtz, brings us to the dawn of modern chemistry. Henceforward organic compounds were seen to be capable of comparison with simple inorganic bodies, and hydrogen not only capable of replacement by chlorine, or by a metal, but by an organic group or radical.

To this period my memory takes me back. Liebig at Giessen, Wöhler in Gottingen, Bunsen in Marburg, Dumas, Wurtz, and Laurent and Gerhardt in Paris, were the active spirits in Continental chemistry. In our own country, Graham, whose memorable researches on the phosphates had enabled Liebig to found his theory of polybasic acids, was working and lecturing at University College, London; and Williamson, imbued with the new doctrines and views of the twin French chemists, had just

[1] See papers by the author "On Vortex Motion," *Trans.* R. S. E., April 1867, and "Vortex Statics," *Proc.* R. S. E., December 1875; also a paper by J. J. Thomson, B.A., "On the Vibrations of a Vortex Ring," *Trans.* R. S., December 1881, and his valuable book on "Vortex Motion."

been appointed to the Chair of Practical Chemistry in the same College, vacant by the death of poor Fownes. At the same time, Hofmann, in whom Liebig found a spirit as enthusiastic in the cause of scientific progress as his own, bringing to England a good share of the Giessen fire, founded the most successful school of chemistry which this country has yet seen.

At the Edinburgh meeting of this Association in 1850, Williamson read a paper on "Results of a Research on Ætherification," which included not only a satisfactory solution of an interesting and hitherto unexplained problem, but was destined to exert a most important influence on the development of our theoretical views. For he proved, contrary to the then prevailing ideas, that ether contains twice as much carbon as alcohol, and that it is not formed from the latter by a mere separation of the elements of water, but by an exchange of hydrogen for ethyl, and this fact, being in accordance with Avogadro's law of molecular volumes, could only be represented by regarding the molecule of water as containing two atoms of hydrogen to one of oxygen, one of the former being replaced by one of ethyl to form alcohol, and the two of hydrogen by two of ethyl to form ether. Then Williamson introduced the type of water (subsequently adopted by Gerhardt) into organic chemistry, and extended our views of the analogies between alcohols and acids, by pointing out that these latter are also referable to the water-type, predicting that bodies bearing the same relations to the ordinary acids as the ethers do to the alcohols must exist, a prediction shortly afterwards (1852) verified by Gerhardt's discovery of the anhydrides. Other results followed in rapid succession, all tending to knit together the framework of modern theoretical chemistry. Of these the most important was the adoption of condensed types, of compounds constructed on the type of two and three molecules of water, with which the names of Williamson and Odling are connected, culminating in the researches of Brodie on the higher alcohols, of Berthelot on glycerine, and of Wurtz on the dibasic alcohols or glycols; whilst, in another direction, the researches of Hofmann on the compound amines and amides opened out an entirely new field, showing that either a part or the whole of the hydrogen in ammonia can be replaced by other elements or elementary groups without the type losing its characteristic properties.

Again, in 1852, we note the first germs of a theory which was destined to play an all-important part in the progress of the science, viz., the doctrine of valency or atomicity, and to Frankland it is that we owe this new departure. Singularly enough, whilst considering the symmetry of construction visible amongst the inorganic compounds of nitrogen, phosphorus, arsenic, and antimony, and whilst putting forward the fact that the combining power of the attracting element is always satisfied by the same number of atoms, he does not point out the characteristic tetrad nature of carbon; and it was not until 1858 that Couper initiated, and Kekulé, in the same year, thoroughly established, the doctrine of the linking of the tetrad carbon atoms, a doctrine to which, more than to any other, is due the extraordinary progress which organic chemistry has made during the last twenty years, a progress so vast that it is already found impossible for one individual, even though he devote his whole time and energies to the task, to master all the details, or make himself at home with the increasing mass of new facts which the busy workers in this field are daily bringing forth.

The subject of the valency of the elements is one which, since the year above referred to, has given chemists much food for discussion, as well as opportunity for experimental work. But whether we range ourselves with Kekulé, who supports the unalterable character of the valency of each element, or with Frankland, who insists on its variability, it is now clear to most chemists that the hard and fast lines upon which this theory was supposed to stand cannot be held to be secure. For if the progress of investigation has shown that it is impossible in many instances to affix one valency to an element which forms a large number of different compounds, it is also equally impossible to look on the opposite view as tending towards progress, inasmuch as to ascribe to an element as many valencies as it possesses compounds with some other element, is only expressing by circuitous methods what the old Daltonian law of combination in multiple proportion states in simple terms. Still we may note certain generally-accepted conclusions: in the first place, that of the existence of non-saturated compounds both inorganic and organic, as carbon-monoxide on the one hand, and malic and citraconic acids on the other. Secondly, that the valency of an element is not only dependent upon the nature of the element with which it combines, but that this valency is a periodic function of the atomic weight of the other component. Thus the elements of the chlorine group are always monads when combined with positive elements or radicals, but triad, pentad, and heptad with negative ones. Again, the elements of the sulphur group are dyads in the first case, but tetrad and hexad in the second. The periodicity of this property of the atoms, increasing and again diminishing, is clearly seen in such a series as

$$AgCl_1, CdCl_2, InCl_3, SnCl_4, SbH_3, TeH_2, IH,$$

as well as in the series of oxides. The difficulties which beset this subject may be judged of by the mention of a case or two:—Is vanadium a tetrad because its highest chloride contains four atoms of chlorine? What are we to say is the valency of lead when one atom unites with four methyls to form a volatile product, and yet the vapour-density of the chloride shows that the molecule contains one of metal to two of chlorine? Or, how can our method be said to determine the valency of tungsten when the hexachloride decomposes in the state of vapour, and the pentachloride is the highest volatile stable compound? How again are we to define the point at which a body is volatile without decomposition?—thus sulphur tetrachloride, one of the most unstable of compounds, can be vaporised without decomposition at all temperatures below $-22°$, whilst water, one of the most stable of known compounds, is dissociated into its elements at the temperature of melting platinum.

But, however many doubts may have been raised in special instances against a thorough application of the law of valency, it cannot be denied that the general relations of the elements which this question of valency has been the means of bringing to light are of the highest importance, and point to the existence of laws of Nature of the widest significance; I allude to the periodic law of the elements first foreshadowed by Newlands, but fully developed by Mendeléeff and Lothar Meyer. Guided by the principle that the chemical properties of the elements are a periodic function of their atomic weights, or that matter becomes endowed with analogous properties when the atomic weight of an element is increased by the same or nearly the same number, we find ourselves for the first time in possession of a key which enables us to arrange the hitherto *disjecta membra* of our chemical household in something like order, and thus gives us means of indicating the family resemblances by which these elements are characterised.

And here we may congratulate ourselves on the fact that, by the recent experiments of Brauner, and of Nilson and Pettersen respectively, tellurium and beryllium, two of the hitherto outstanding members, have been induced to join the ranks, so that at the present time osmium is the only important defaulter amongst the sixty-four elements, and few persons will doubt that a little careful attention to this case will remove the stigma which yet attaches to its name. But this periodic law makes it possible for us to do more; for as the astronomer, by the perturbations of known planets, can predict the existence of hitherto unknown ones, so the chemist, though, of course, with much less satisfactory means, has been able to predict with precision the properties, physical and chemical, of certain missing links amongst the elements, such as ekaluminium and ekaboron, then unborn, but which shortly afterwards became well known to us in the flesh as gallium and scandium. We must, however, take care that success in a few cases does not blind us to the fact that the law of Nature which expresses the relation between the properties of the elements and the value of the atomic weights is as yet unknown; that many of the groupings are not due to any well-ascertained analogy of properties of the elements, and that it is only because the values of their atomic weights exhibit certain regularities that such a grouping is rendered possible. So, to quote Lothar Meyer, we shall do well in this, as indeed in all similar cases in science, to remember the danger pointed out in Bacon's aphorism, that "The mind delights in springing up to the most general axioms, that it may find rest, but after a short stay here it disdains experience," and to bear in mind that it is only the lawful union of hypothesis with experiment which will prove a fruitful one in the establishment of a systematic inorganic chemistry which need not fear comparison with the order which reigns in the organic branch of our science. And here it is well to be reminded that complexity of constitution is not the sole prerogative of the carbon compounds, and that before this systematisation of inorganic chemistry can be effected we shall have to come to terms with many compounds concerning whose constitution we are at present wholly in ignorance. As instances of such I would refer to the finely crystalline phospho-molybdates, containing several hundred atoms in the molecule, lately prepared by Wolcott Gibbs.

Arising out of Kekulé's theory of the tetrad nature of the carbon atom, came the questions which have caused much debate among chemists: (1) Are the four combining units of the carbon atom of equal value or not? and (2) Is the assumption of a dyad carbon atom in the so-called non-saturated compounds justifiable or not? The answer to the first of these, a favourite view of Kolbe's, is given in the now well-ascertained laws of isomerism; and from the year 1862, when Schorlemmer proved the identity of the hydrides of the alcohol radicals with the so-called radicals themselves, this question may be said to have been set at rest; for Lossen *himself admits* that the existence of his singular isomeric hydroxylamine derivatives can be explained otherwise than by the assumption of a difference between each of the combining units of nitrogen, and the differences supposed by Schreiner to hold good between the methyl-ethyl carbonic ethers have been shown to have no existence in fact. With respect to the second point the reply is no less definite, and is recorded in the fact, amongst others, that ethylene chlorhydrin yields on oxidation chloracetic acid, a reaction which cannot be explained on the hypothesis of the existence in ethylene of a dyad carbon atom.

Passing from this subject, we arrive, by a process of natural selection, at more complicated cases of chemical orientation—that is, given certain compounds which possess the same composition and molecular formulæ but varying properties, to find the difference in molecular structure by which such variation of properties is determined. Problems of this nature can now be satisfactorily solved, the number of possible isomers foretold, and this prediction confirmed by experiment. The general method adopted in such an experimental inquiry into the molecular arrangement or chemical constitution of a given compound is either to build up the structure from less complicated ones of known constitution, or to resolve it into such component parts. Thus, for example, if we wish to discriminate between several isomeric alcohols, distinguishing the ordinary or primary class from the secondary or tertiary class, the existence of which was predicted by Kolbe in 1862, and of which the first member was prepared by Friedel in 1864, we have to study their products of oxidation. If one yields an acid having the same number of carbon atoms as the alcohol, it belongs to the first class and possesses a definite molecular structure; if it splits up into two distinct carbon compounds, it is a secondary alcohol; and if three carbon compounds result from its oxidation, it must be classed in the third category, and to it belongs a definite molecular structure, different from that of the other two.

In a similar way orientation in the much more complicated aromatic hydrocarbons can be effected. This class of bodies forms the nucleus of an enormous number of carbon compounds which, both from a theoretical and a practical point of view, are of the highest interest. For these bodies exhibit characters and possess a constitution totally different from those of the so-called fatty substances, the carbon atoms being linked together more intimately than is the case in the latter-named group of bodies. Amongst them are found all the artificial colouring matters, and some of the most valuable pharmaceutical and therapeutical agents.

The discovery of the aniline colours by Perkin, their elaboration by Hofmann, the synthesis of alizarin by Graebe and Liebermann, being the first vegetable colouring matter which has been artificially obtained, the artificial production of indigo by Baeyer, and lastly the preparation, by Fischer, of kairin, a febrifuge as potent as quinine, are some of the well-known recent triumphs of modern synthetical chemistry. And these triumphs, let us remember, have not been obtained by any such "random haphazarding" as yielded results in Priestley's time. In the virgin soil of a century ago, the ground only required to be scratched and the seed thrown in to yield a fruitful crop; now the surface soil has long been exhausted, and the successful cultivator can only obtain results by a deep and thorough preparation, and by a systematic and scientific treatment of his material.

In no department of our science has the progress made been more important than in that concerned with the accurate determination of the numerical, physical, and chemical constants upon the exactitude of which every quantitative chemical operation depends. For the foundation of an accurate knowledge of the first of these constants, viz., the atomic weights of the elements, science is indebted to the indefatigable labours of Berzelius. But "humanum est errare," and even Berzelius's accurate hand and delicate conscientiousness did not preserve him from mistakes, since corrected by other workers. In such determinations it it difficult, if not impossible, always to ascertain the limits of error attaching to the number. The errors may be due in the firss place to manipulative faults, in the second to inaccuracy of the methods, or lastly to mistaken views as to the composition of the material operated upon; and hence the uniformity of any series of similar determinations gives no guarantee of their truth, the only safe guide being the agreement of determinations made by altogether different methods. The work commenced by Berzelius has been worthily continued by many chemists. Stas and Marignac, bringing work of an almost astronomical accuracy into our science, have ascertained the atomic weights of silver and iodine to within one hundred-thousandth of their value, whilst the numbers for chlorine, bromine, potassium, sodium, nitrogen, sulphur, and oxygen may now be considered correct to within a unit in the fourth figure. Few of the elements, however, boast numbers approaching this degree of accuracy, and many may even still be erroneous from half to a whole unit of hydrogen. And, as Lothar Meyer says, until the greater number of the atomic weights are determined to within one or two tenths of the unit, we cannot expect to be able to ascertain the laws which certainly govern these numbers, or to recognise the relations which undoubtedly exist between them and the general chemical and physical properties of the elements. Amongst the most interesting recent additions to our knowledge made in this department we may note the classical experiments, in 1880, of J. W. Mallet on aluminium, and in the same year of J. P. Cooke on antimony, and those, in the present year, of Thorpe on titanium.

Since the date of Berzelius's death to the present day, no discovery in our science has been so far-reaching, or led to such unforeseen and remarkable conclusions, as the foundation of Spectrum Analysis by Bunsen and Kirchhoff in 1860.

Independently altogether of the knowledge which has been gained concerning the distribution of the elementary bodies in terrestrial matter, and of the discovery of half a dozen new elements by its means, and putting aside for a moment the revelation of a chemistry not bounded by this world, but limitless as the heavens, we find that over and above all these results spectrum analysis offers the means, not otherwise open to us, of obtaining knowledge concerning the atomic and molecular condition of matter.

Let me recall some of the more remarkable conclusions to which the researches of Lockyer, Schuster, Liveing and Dewar, Wüllner, and others in this direction have led. In the first place it is well to bear in mind that a difference of a very marked kind, first distinctly pointed out by Alex. Mitscherlich, is to be observed between the spectrum of an element and that of its compounds, the latter only being seen in cases in which the compound is not dissociated at temperatures necessary to give rise to a glowing gas. Secondly, that these compound spectra—as, for instance, those of the halogen compounds of the alkaline-earth metals—exhibit a certain family likeness, and show signs of systematic variation in the position of the lines, corresponding to changes in the molecular weight of the vibrating system. Still this important subject of the relation of the spectra of different elements is far from being placed on a satisfactory basis, and in spite of the researches of Lecoq de Boisbaudran, Ditte, Troost and Hautefeuille, Ciamician, and others, it cannot be said that as yet definite proof has been given in support of the theory that a causal connection is to be found between the emission spectra of the several elements belonging to allied groups and their atomic weights or other chemical or physical properties. In certain of the single elements, however, the connection between the spectra and the molecular constitution can be traced. In the case of sulphur, for example, three distinct spectra are known. The first of these, a continuous one, is exhibited at temperatures below 500°, when, as we know from Dumas' experiments, the density of the vapour is three times the normal, showing that at this temperature the molecule consists of six atoms. The second spectrum is seen when the temperature is raised to above 1000°, when, as Deville and Troost have shown, the vapour reaches its normal density, and the molecule of sulphur, as with most other gases, contains two atoms, and this is a band spectrum, or one characterised by channelled spaces. Together with this band spectrum, and especially round the negative pole, a spectrum of bright lines is observed. This latter is doubtless due to the vibrations of the single atoms of the dissociated molecule, the existence of traces of a band spectrum demonstrating the fact that in some parts of the discharge the tension of dissociation is insufficient to prevent the reunion of the atoms to form the molecule.

To this instance of the light thrown on molecular relations by changes in the spectra, others may be added. Thus the low-temperature spectrum of channelled spaces, mapped by Schuster and myself, in the case of potassium, corresponds to the molecule of two atoms and to the vapour-density of seventy-nine, as observed by Dewar and Dittmar. Again, both oxygen and nitrogen exhibit two, if not three, distinct spectra: of these the line spectrum seen at the highest temperatures corresponds to the atom; the band spectrum seen at intermediate temperatures represents the molecule of two atoms; whilst that observed at a still lower point would, as in the case of sulphur, indicate the existence of a more complicated molecule, known to us in one instance as ozone.

That this explanation of the cause of these different spectra of an element is the true one, can be verified in a remarkable way. Contrary to the general rule amongst those elements which can readily be volatilised, and with which, therefore, low-temperature spectra can be studied, mercury exhibits but one spectrum, and that one of bright lines, or, according to the preceding theory, a spectrum of atoms. So that, judging from spectroscopic evidence, we infer that the atoms of mercury do not unite to form a molecule, and we should predict that the vapour-density of mercury is only half its atomic weight. Such we know, from chemical evidence, is really the case, the molecule of mercury being identical in weight with its atom.

The cases of cadmium and iodine require further elucidation. The molecule of gaseous cadmium, like that of mercury, consists of one atom; probably, therefore, the cadmium spectrum is also distinguished by one set of lines. Again, the molecule of iodine at 1200° separates, as we know from Victor Meyer's researches, into single atoms. Here spectrum analysis may come again to our aid; but, as Schuster remarks, in his report on the spectra of the non-metallic elements, a more extensive series of experiments than those already made by Ciamician is required before any definite opinion as to the connection of the different iodine spectra with the molecular condition of the gas can be expressed.

It is not to be wondered at that these relations are only exhibited in the case of a few elements. For most of the metals the vapour-density remains, and probably will remain, an unknown quantity, and therefore the connection between any observed changes in the spectra and the molecular weights must also remain unknown. The remarkable changes which the emission spectrum of a single element—iron, for instance—exhibits have been the subject of much discussion, experimental and otherwise. Of these, the phenomenon of long and short lines is one of the most striking, and the explanation that the long lines are those of low temperature appears to meet the fact satisfactorily, although the effect of dilution, that is, a reduction of the quantity of material undergoing volatilisation, is, remarkably enough, the same as that of diminution of temperature. Thus it is possible, by the examination of a spectrum by Lockyer's method, to predict the changes which it will undergo, either on alteration of temperature, or by an increase or decrease of quantity. There appears to be no theoretical difficulty in assuming that the relative intensity of the lines may vary when the temperature is altered, and the molecular theory of gases furnishes us with a plausible explanation of the corresponding change when the relative quantities of the luminous elements in a mixture are altered. Lockyer has proposed a different explanation of the facts. According to him, every change of relative intensity means a corresponding change of molecular complexity, and the lines which we see strong near the poles would bear the same relation to those which are visible throughout the field, as a line spectrum bears to a band spectrum; but then almost every line must be due to a different molecular grouping, a conclusion which is scarcely capable of being upheld without very cogent proof.

The examination of the absorption-spectra of salts, saline and organic liquids, first by Gladstone, and afterwards by Bunsen, and by Russell, as well as by Hartley for the ultra-violet, and by Abney and Festing for the infra-red region, have led to interesting results in relation to molecular chemistry. Thus Hartley finds that, in some of the more complicated aromatic compounds, definite absorption-bands in the more refrangible region are only produced by substances in which three pairs of carbon atoms are doubly linked, as in the benzene ring, and thus the means of ascertaining this double linkage is given. The most remarkable results obtained by Abney and Festing show that the radical of an organic body is always represented by certain well-marked absorption-bands, differing, however, in position, according as it is linked with hydrogen, a halogen, or with carbon, oxygen, or nitrogen. Indeed, these experimenters go so far as to say that it is highly probable that by this delicate mode of analysis the hypothetical position of any hydrogen which is replaced may be identified, thus pointing out a method of physical orientation of which, if confirmed by other observers, chemists will not be slow to avail themselves. This result, it is interesting to learn, has been rendered more than probable by the recent important researches of Perkin on the connection between the constitution and the optical properties of chemical compound.

One of the noteworthy features of chemical progress is the interest taken by physicists in fundamental questions of our science. We all remember, in the first place, Sir William Thomson's interesting speculations, founded upon physical phenomena, respecting the probable size of the atom, viz. "that if a drop of water were magnified to the size of the earth, the constituent atoms would be larger than small shot, but smaller than cricket balls." Again, Helmholtz, in the Faraday Lecture, delivered in 1881, discusses the relation of electricity and chemical energy, and points out that Faraday's law of electrolysis, and the modern theory of valency, are both expressions of the fact that, when the same quantity of electricity passes through an electrolyte, it always either sets free, or transfers to other combinations, the same number of units of affinity at both electrodes. Helmholtz further argues that, if we accept the Daltonian atomic hypothesis, we cannot avoid the conclusion that electricity, both positive and negative, is divided into elementary portions which behave like atoms of electricity. He also shows that these charges of atomic electricity are enormously large as compared, for example, with the attraction of gravitation between the same atoms; in the case of oxygen and hydrogen, 71,000 billion times larger.

A further subject of interest to chemists is the theory of the vortex-ring constitution of matter thrown out by Sir William Thomson, and lately worked out from a chemical point of view by J. J. Thomson, of Cambridge. He finds that if one such ring be supposed to constitute the most simple form of matter, say the monad hydrogen atom, then two such rings must, on coming into contact with nearly the same velocity, remain enchained together, constituting what we know as the molecule of free hydrogen. So, in like manner, systems containing two, three, and four such rings constitute the dyad, tryad, and tetrad atoms. How far this mathematical expression of chemical theory may prove consistent with fact remains to be seen.

Another branch of our science which has recently attracted much experimental attention is that of thermo-chemistry, a subject upon which in the future the foundation of dynamical chemistry must rest, and one which already proclaims the truth of the great principle of the conservation of energy in all cases of chemical as well as of physical change. But here, although the materials hitherto collected are of very considerable amount and value, the time has not yet arrived for expressing these results in general terms, and we must, therefore, be content to note progress in special lines and wait for the expansion into wider areas. Reference may, however, be properly made to one interesting observation of general significance. It is well known that, while, in most instances, the act of combination is accompanied by evolution of heat—that is, whilst the potential energy of most combining bodies is greater than that of most compounds—cases occur in which the reverse of this is true, and heat is absorbed in combination. In such cases the compound readily undergoes decomposition, frequently suddenly and with explosion. Acetylene and cyanogen seem to be exceptions to this rule, inasmuch as, whilst their component elements require to have energy added to them in order to enable them to combine, the compounds appear to be very stable bodies. Berthelot has explained this enigma by showing that, just as we may ignite a mass of dynamite without danger, whilst explosion takes place if we agitate the molecules by a detonator, so acetylene and cyanogen burn, as we know, quietly when ignited, but when their molecules are shaken by the detonation of even a minute quantity of fulminate, the constituents fly apart with explosive violence, carbon and hydrogen, or carbon and nitrogen, being set free, and the quantity of heat absorbed in the act of combination being suddenly liberated.

In conclusion, whilst far from proposing even to mention all the important steps by which our science has advanced since the year 1848, I cannot refrain from referring to two more. In the first place, to that discovery, more than foreshadowed by Faraday, of the liquefaction of the so-called permanent gases by Pictet and Cailletet; and secondly, to that of the laws of dissociation as investigated by Deville. The former, including Andrews's discovery of the critical point, indicates a connection, long unseen, between the liquid and the gaseous states of matter; the latter has opened out entirely fresh fields for research, and has given us new views concerning the stability of chemical compounds of great importance and interest.

Turning for a moment to another topic, we feel that, although science knows no nationalities, it is impossible for those who, like ourselves, exhibit strong national traits, to avoid asking whether we Anglo-Saxons hold our own, as compared with other nations, in the part we have played and are playing in the development of our science. With regard to the past, the names of Boyle, Cavendish, Priestley, Dalton, Black, Davy, are sufficient guarantees that the English have, to say the least, occupied a position second to none in the early annals of chemistry. How has it been in the era which I have attempted to describe? What is the present position of English chemistry, and what its look-out for the future? In endeavouring to make this estimate, I would take the widest ground, including not only the efforts made to extend the boundaries of our science by new discovery, both in the theoretical and applied branches, but also those which have the no less important aims of spreading the knowledge of the subject amongst the people, and of establishing industries dependent on chemical principles by which the human race is benefited. Taking this wide view, I think we may, without hesitation, affirm that the progress which chemistry has made through the energies of the Anglo-Saxon race is not less than that made by any other nation.

In so far as pure science is concerned, I have already given evidence of the not inconsiderable part which English chemists have played in the progress since 1848. We must, however, acknowledge that the number of original chemical papers now published in our language is much smaller than that appearing in the German tongue, and that the activity and devotion displayed in this direction by the heads of German laboratories may well be laid to heart by some of us in England; yet, on the other hand, it must be remembered that the circumstances of different countries are so different that it is by no means clear that we should follow the same lines. Indeed our national characteristics forbid us to do so, and it may be that the bent of the Germanic lies in the assiduous collection of facts, whilst their subsequent elaboration and connection is the natural work of our own race.

As regards the publication of so-called original work by students, and speaking now only for myself as the director of an English chemical laboratory, I feel I am doing the best for the young men who, wishing to become either scientific or industrial chemists, are placed under my charge, in giving them as sound and extensive a foundation in the theory and practice of chemical science as their time and abilities will allow, rather than forcing them prematurely into the preparation of a new series of homologous compounds or the investigation of some special reaction, or of some possible new colouring matter, though such work might doubtless lead to publication. My aim has been to prepare a young man, by a careful and fairly complete general training, to fill with intelligence and success a post either as teacher or industrial chemist, rather than to turn out mere specialists, who, placed under other conditions than those to which they have been accustomed, are unable to get out of the narrow groove in which they have been trained. And this seems a reasonable course, for whilst the market for the pure specialist, as the colour chemist for example, may easily be overstocked, the man of all-round intelligence will always find opportunity for the exercise of his powers. Far, however, from underrating the educational advantages of working at original subjects, I consider this sort of training to be of the highest and best kind, but only useful when founded upon a sound and general basis.

The difficulty which the English teacher of chemistry—and in this I may include Canada and the United States—has to contend against is that, whilst in Germany the value of this high and thorough training is generally admitted, in England a belief in its efficacy is as yet not generally entertained. "The Englishman," to quote from the recent Report of the Royal Commission on Technical Instruction, "is accustomed to seek for an immediate return, and has yet to learn that an extended and systematic education, up to and including the methods of original research, is now a necessary preliminary to the fullest development of industry, and it is to the gradual but sure growth of public opinion in this direction that we must look for the means of securing to this country in the future, as in the past, the highest position as an industrial nation."

If, in the second place, we consider the influence which Englishmen have exerted on the teaching of our science, we shall feel reason for satisfaction; many of our text-books are translated into every European language and largely used abroad; often to the exclusion of those written by Continental chemists.

Again, science teaching, both practical and theoretical, in our elementary and many secondary schools, is certainly not inferior to that in schools of similar grade abroad, and the interest in and desire for scientific training is rapidly spreading throughout our working population, and is even now as great as, if not greater than, abroad. The universities and higher colleges are also moving to take their share of the work which has hitherto been far less completely done in our country than on the continent of Europe, especially in Germany, where the healthful spirit of competition, fostered by the numerous State-supported institutions, is much more common than with us, and, being of equal value in educational as in professional or commercial matters, has had its due effect.

Turning lastly to the practical applications of our science, in what department does England not excel? and in which has she not made the most important new departures? Even in colour chemistry, concerning which we have heard, with truth, much of German supremacy, we must remember that the industry is originally an English one, as the names of Perkin and of Maule, Simpson and Nicholson, testify; and if we have hitherto been beaten hollow in the development of this branch, signs are not wanting that this may not always be the case. But take any other branch of applied chemistry, the alkali trade for instance, what names but English, with the two great exceptions of Leblanc and Solvay, do we find in connection with real discoveries? In the application of chemistry to metallurgical processes, too, the names of Darby, Cort, Neilson, and Bell in iron, of Bessemer, Thomas, Gilchrist, and Snelus in steel, of Elkington and Matthey in the noble metals, show that in these branches the discoveries which have revolutionised processes have been made by Englishmen; whilst Young, the father of paraffin, Spence the alum-maker, and Abel of gun-cotton fame, are some amongst many of our countrymen whose names may be honourably mentioned as having founded new chemical industries.

Hence, whilst there is much to stimulate us to action in the energy and zeal shown by our Continental brethren in the pursuit both of pure and applied chemistry, there is nothing to lead us to think that that the chemistry of the English-speaking nations in the next fifty years will be less worthy than that of the past half-century of standing side by side with that of her friendly rivals elsewhere.

SECTION D

BIOLOGY

Opening Address by H. N. Moseley, M.A., F.R.S., Linacre Professor of Human and Comparative Anatomy in the University of Oxford, President of the Section

In appointing the phenomena of pelagic and deep-sea life as one of the subjects specially selected for consideration at the present meeting of this Section, the Organising Committee have, I think, done wisely. Our knowledge of the subject is at present in most active progress. It is one of the widest and deepest interest to the physiologist as well as the zoologist, and in some features claims a share of attention from the botanist. And the proximity here of the United States, to which science is indebted for so many important discoveries on deep-sea matters, is a strong argument in favour of the subject being brought forward at a British Association meeting on this side of the Atlantic. I have naturally been led to choose the consideration of some deep-sea biological questions as the subject of my address by the special interest which I have been led to take in deep-sea phenomena generally, owing to my long participation in actual deep-sea research during the voyage of H.M.S. *Challenger*.

Unfortunately, the physiology of deep-sea life has until lately received but little attention from professed physiologists. No physiologist has, as far as I am aware, as yet set forth comprehensively and dwelt upon the numerous difficulties which are encountered when the attempt is made to comprehend the mode in which the ordinary physiological processes of Vertebrata and other animals are carried on under the peculiar physical conditions which exist at great depths.

Whilst I was on the *Challenger* voyage, absorbed principally in the zoological discoveries daily resulting from the dredging operations, I received a letter from my revered teacher, Prof. Ludwig, of Leipzig, which brought deep-sea phenomena before me in a very different light. The Professor naturally regarded deep-sea questions mainly from a physiological point of view, and asked a series of most suggestive questions bearing on it. I am much indebted to him for this and recent letters on the same subject. One of the first questions he asked was, naturally, as to the amount of oxygen present in deep-sea water. A knowledge of the conditions under which gases occur in a state of absorption in the ocean-waters is of primary importance to the physiologist. With regard to this subject, most valuable information is contained in the report by the distinguished chemist, Prof. Dittmar, on "Researches into the Composition of the Ocean-Water collected by H.M.S. *Challenger*," which has appeared during the present year, and which embody Mr. J. Y. Buchanan's results.[1] It appears from his results that, contrary to what was before suspected, the presence of free carbonic acid in sea-water is an exception. What carbonic acid is present occurs as a bicarbonate, in general more or less incompletely saturated. In surface-waters the proportion of carbonic acid increases when the temperature falls, and *vice versâ*. Deep-sea water does not contain an abnormal proportion of loose or free carbonic acid.

Hence, with regard to Mr. John Murray's interesting discovery that after certain depths are reached *Pteropod* shells are dissolved and disappear from the sea-bottom, and at certain further depths *Globigerina* shells suffer the same fate, Prof. Dittmar holds the opinion that the solution is not due to the presence of free acid, but to the solvent action of the sea-water itself, which will, even when alkaline, take up additional carbonate of lime if sufficient time be given. Thus the amount of carbonic acid normally present throughout the ocean cannot be inimical to life; but, according to the Professor, there must be in the depths of the ocean numerous bodies of richly carbonated water, for he regards the principal supply of carbonic acid to the sea-water as derived from volcanic springs and discharges issuing from the ocean-bed, the quantity derived from the decay of marine plants and animals being insignificant in comparison with this. Possibly the *Challenger*, when it dredged from deep water off the Azores immense quantities of dead and blackened coral, encountered an area which had thus been visited by a carbonic acid discharge.

With regard to the absorbed oxygen and nitrogen, the theoretical maximum quantity of oxygen absorbed at normal surface-pressure by a litre of sea-water should range, according to Prof. Dittmar's experiments and calculations, from 8·18 c.c. in cold regions at 0° C. to 4·50 c.c. in the tropics, with a temperature of 30° C. The result experimentally obtained from samples of surface-water collected during the voyage differ considerably in detail from the calculated estimates, from various causes explained, and especially because of the reduction of the amount of oxygen by oxidation and respiration. The main and almost sole source of the nitrogen and oxygen present in deep-sea water lies in the atmosphere, and is absorbed there, its quantity being thus dependent on surface conditions of temperature and pressure, and not those of the depths. A given quantity of water, having absorbed its oxygen and nitrogen at the surface, may be supposed to sink unmixed with surrounding water to the depths. During the process its amount of contained nitrogen remains constant, whilst its oxygen-supply becomes gradually diminished, owing to the process of oxidation, which in the depths goes on without compensation. That the amount of absorbed oxygen present in sea-water diminishes with the depth has been shown already by Dr. Lant Carpenter's experiments. It is not yet possible to formulate in any precise terms the relation between the depth and the diminution of the oxygen present, but Mr. J. Y. Buchanan's previous conclusion that a minimum of oxygen is attained at a depth of about 800 fathoms is not confirmed by the summing-up of the whole of the evidence now available. This result is not without biological significance, since the existence of this supposed zone with a minimum of oxygen has been used as an argument in favour of the occurrence of especially abundant life at this depth below the ocean-surface.

Prof. Dittmar finds that there is nothing characteristic of bottom-waters as such in regard to their absorbed gases, nothing to distinguish them from waters from intermediate depths. This, it seems to me, is not quite what might have been expected, as the concentration of the food-supply, and consequently of life, on the actual bottom might have led to a different result.

If there were absolute stagnation of the water at great depths, the oxygen might be reduced there to zero, but the fact that in no case has oxygen been entirely absent from any sample of deep-sea water examined proves that a certain motion and change must occur. The smallest amount of oxygen found at all was in a sample of water from a depth of 2,875 fathoms, and amounted to 0·65 c.c. per litre only, a result long ago published by Mr. Buchanan. Even this, however, may well be sufficient to support life, since Humboldt and Provencal[1] found that certain fish could breathe in water containing only one-third of that quantity of oxygen per litre. In another sample, from 1645 fathoms, it was 2·04 c.c. On the other hand, as much as 4·055 c.c. was found in a sample from 4575 fathoms, and 4·39 c.c. in one from 3025. Most remarkable, in one instance water from a depth of only 300 fathoms yielded only 1·65 c.c. of oxygen. Prof. Dittmar admits that there was no lack of anomalous results, some, no doubt, due to some extent to imperfection in the apparatus employed in collecting the water.

In connection with the valuable investigations carried on in the *Travailleur* and the *Talisman* by Prof. Milne-Edwards and his associates, French physiologists have lately commenced researches on some of the problems of deep-sea life.

Experiments have been made by M. Regnard[2] with a view of determining the effects of high pressures, corresponding with those of the deep sea, on various organisms. Yeast, after being exposed to a pressure of 1000 atmospheres, equal to a depth of about 6500 fathoms of sea-water, for an hour, was mixed with a solution of sugar. An hour elapsed before any signs of fermentation appeared, and a mixture of yeast and sugar solution did not ferment at all whilst under a pressure of 600 atmospheres, equal to a depth of about 3900 fathoms. Algæ, seeds of phanerogamic plants, Infusoria, and even Mollusca and leeches, were found to be thrown into a sort of state of sleep or latency by exposure to similar pressures, recovering from this condition after a shorter or longer period of return to normal conditions. A fish without a swimming bladder, or one with the bladder emptied of air, may be submitted to a pressure of 100 atmospheres, equivalent to a depth of 650 fathoms, without injurious effect. At 200 atmospheres, equivalent to a depth of 1300 fathoms, it becomes torpid, but soon revives when the pressure is removed. At 300 atmospheres, equivalent to a depth of about 2000 fathoms, the fish dies.

These experiments are of the highest interest. The pressure made use of was obtained by means of water in the absence of air other than that absorbed at the normal atmosphere pressure, and thus the physical conditions produced were closely similar to those actually existent in the deep sea. They are the first of their kind.

Prof. Paul Bert's[3] somewhat similar experiments related to a different question altogether—namely, the effect on aquatic organisms of water subjected to the pressure of compressed air. He found that young eels were rapidly killed when subjected to a pressure of only 15 atmospheres, and could not survive one of even 7 atmospheres for any considerable time.[4] He pointed out the essential difference between the conditions produced in such experiments and those existing in the deep sea, where the charge of oxygen contained by the water has been taken up at the surface at a pressure of one atmosphere only.

In the experiments on animals made by M. Regnard's method there is the obvious difficulty that the supply of oxygen in the water compressed cannot be renewed during the experiment but must be gradually reduced by respiration, and for this reason it would probably be useless, unless a large quantity of water would

[1] "Official Report on the Scientific Results of the Voyage of H.M.S. *Challenger*: *Physics and Chemistry*," vol. i.

[1] "Sur la Respiration des Poissons," *Journ. de Physique, de Chimie, et d'Histoire Naturelle*, t. lxix. October 1809, p. 268.

[2] P. Regnard, "Recherches Expérimentales sur l'Influence des très-hautes Pressions sur les Organismes vivants," *Comptes Rendus*, No. 12, 24 mars 1884, p. 745.

[3] *La Pression barométrique*, Paris, 1878, p. 814. [4] *Ibid.* p. 1151.

be employed, to try the effect on a fish of a very gradual application of pressure, extending over many hours. It is probable that the results would be greatly modified if plenty of time could be given for the fish to accommodate itself to the change of pressure, and the conditions in which it moves in nature slowly from one depth to another be imitated. The results of M. Regnard's further experiments will be looked forward to with great interest.

A question of the utmost moment, and one that has received a good deal of attention, is that as to the source of food of the deep-sea animals. Certainly a large proportion of this food is derived from the life on the ocean-surface. The debris of pelagic animals sinks slowly downwards, forming on its passage a sparsely scattered supply of food for any animals possibly living at intermediate depths, but becoming concentrated as it were on the bottom. The pelagic animals depend for their ultimate source of food, no doubt, largely on the various pelagic plants, the range of which in depth is limited by the penetration of sea-water by the sunlight, and probably to an important extent is dependent on the symbiotic combinations of radiolarians with zooxanthella. But a large part of their food-supply is also constituted by animal and vegetable debris derived from the coasts, either directly from the littoral zone or by rivers and the action of the tides from terrestrial life. Immense quantities of shore-debris have been dredged from deep water near coasts, and deep-sea life appears to diminish in abundance as coasts are receded from. Unfortunately our knowledge of pelagic vegetable life is very imperfect, and it is to be hoped that botanists may be led to take up the subject and bring together what is known with regard to the geological ranges and abundance of the various larger sea-weeds, trichodesmium, diatoms, and other Algæ by which the sea-surface is inhabited. It will, then, be possible to form a nearer estimate of the extent to which these plants are capable of forming a sufficient ultimate food-source for the greater part of the pelagic fauna, and through it of deep-sea life. The question is of importance, because if the deep sea, having no ultimate source of food in itself, derived its main supply from the coasts and land-surfaces in the early history of the habitation of the globe by animals, there can have existed scarcely any deep-sea fauna until the littoral and terrestrial faunas and floras had become well established.

Whether the littoral and terrestrial plants or the pelagic be proved to have the larger share in composing the ultimate food-source of the deep sea, it seems certain that the food as it reaches the deep sea is mostly in the form of dead matter, and I imagine that the long but slender backwardly-directed teeth of many deep-sea fish, resembling those of snakes, are used rather as aids for swallowing whole other fishes which have fallen from above dead, and thus making the best of an occasional opportunity of a meal, than for catching and killing living prey. In a lecture on "Life in the Deep Sea," delivered in 1880,[1] I suggested that putrefaction of organic matter, such as ordinarily occurs elsewhere, may possibly be entirely absent in the deep sea, the Bacteria and other microphytes which cause it being possibly absent. Some interesting experiments with regard to this question have lately been made by M. A. Certes.[2] He added to sterilised solutions of hay-extract, milk, broth, and other organic nutrient fluids mixed with sea-water, with the usual necessary precautions, small quantities of deep-sea mud, or deep-sea water, procured by the *Travailleur* and *Talisman*. In some experiments air was present; others were made *in vacuo*. In nearly all the former putrefaction occurred after some time, especially after application of warmth, and micro-organisms were developed, whilst the latter remained without exception sterile, apparently indicating that the microbes which live where air is absent are not present in the deep sea. The others, which developed in the presence of oxygen, may possibly have sunk from the surface to the bottom, and have retained their vitality, although it is not improbable that they may be incapable of active existence and multiplication under the physical conditions there existing. M. Certes is to make further experiments on this question under conditions of pressure and temperature as nearly resembling those of the deep sea as possible. In the deep sea the ordinary cycle of chemical changes of matter produced by life is incomplete, there being no plants to work up the decomposition-products. These, therefore, in the absence of any rapid change of the deep-sea waters, must accumulate there, and can only be turned to account when they reach the surface-waters on the littoral regions.

Many interesting results may be expected to be obtained when the histology of animals from great depths comes to be worked out, and especially that of the special sense-organs. At present very little has been attempted in this direction, principally, no doubt, because deep-sea specimens are too precious to be used for the purpose. In a remarkable scopelid fish dredged by the *Challenger* from deep water, *Ipnops murrayi* of Dr. Günther, the eyes are curiously flattened out and occupy the whole upper surface of the mouth. They are devoid of any trace of lens or iris, and, as appears from observations by Mr. John Murray and my own examination of his preparations, the retina consists of a layer of long rods, with a very thin layer indeed of nerve-fibres in front of it, and apparently no intervening granular ganglionic or other layers. The rods are disposed in hexagonal bundles, the free ends of which rest on corresponding well-defined hexagonal areas, into which the choroid is divided. It is probable that aberrant structures may be found in the retinas of deep-sea fish, which may conceivably help towards physiological conclusions as to the functions of the various components. With regard to the all-important question of the nature of the light undoubtedly present in the deep sea, it is hardly possible to accept Prof. Verrill's recent startling suggestion that sunlight penetrates to the greatest depths with perhaps an intensity at from 2000 to 3000 fathoms equal to that of some of our partially moonlight nights. Such a conjecture is entirely at variance with the results of all experiments on the penetration of sea-water by sunlight as yet made by physicists, results which have prevented other naturalists from adopting this solution of the problem.

The progress of research by experts on the deep-sea fauna confirms the conclusions early formed that it is impossible to determine any successive zones of depth in the deep-sea regions characterised by the presence of special groups of animals. Within the deep-sea region the contents of a trawl brought up from the bottom give no evidence which can be relied on as to the depth at which the bottom lies within a range of at least 2500 fathoms. Some groups of animals appear to be characteristic of water of considerable depth, but representatives of them struggle up into much shallower regions. Thus of the remarkable order of Holothurian Elasipoda nearly all the representatives occur at very considerable depths, and their numbers diminish shorewards, but one has been found in only 100 fathoms. Again, the Pourtalesidæ range upwards into about 300 fathoms, and the Phormosomas, which Loven considers as eminently deep-sea forms, range up to a little over 100 fathoms depth, and are nearly represented in shallow water at a depth of only five fathoms by Asthenosoma. As has often been pointed out before, there are numerous genera, and even species, which range even from the shore-region to great depths.

The fact that zones of depth cannot thus be determined adds seriously to the difficulties encountered in the attempt to determine approximately the depths at which geological deposits have been found. Dr. Theodore Fuchs,[1] in an elaborate essay on all questions bearing on the subject, has attempted to determine what geological strata should be considered as of deep-sea formation, but, as he defines the deep-sea fauna as commencing at 100 fathoms and extending downwards to all depths, his results may be considered as merely determining whether certain deposits have been found in as great a depth as 100 fathoms or less, a result of little value as indicating the depths of ancient seas or the extent of upheaval or depression of their bottoms. Mr. John Murray has shown that the depths at which modern deep-sea deposits have been formed can be approximately ascertained by the examination of their microscopical composition and the condition of preservation of the contained pelagic and other shells and spicules.

The most important question with regard to life in the ocean, at present insufficiently answered, is that as to the conditions with regard to life of the intermediate waters between the surface and the bottom. It is most necessary that further investigations should be made in extension of those carried out by Mr. Alexander Agassiz with similar apparatus—a net, or vessel, which can be let down to a certain depth whilst completely closed,

[1] Lecture delivered at the Royal Institution, March 5, 1880, *Nature*, vol. xxi. p. 592.

[2] "Sur la Culture, à l'abri des Germes atmosphériques, des Eaux et des Sédiments rapportés par les Expéditions du *Travailleur* et du *Talisman*, 1882, 1883," *Comptes Rendus*, No. 11, 11 mars 1884, p. 690.

[1] "Welche Ablagerungen haben wir als Tiefseebildungen zu betrachten?" *Neues Jahrbuch für Mineralogie, Geologie, und Paläontologie*, 11 Beilage, Bd. 1882.

then opened, lowered for some distance, and again closed before it is drawn to the surface. The greatest uncertainty and difference of opinion exist as to whether the intermediate waters are inhabited at all by animals, and, if they are inhabited, to what extent; and these intermediate waters constitute by far the greatest part of the ocean. If we estimate roughly the depth of the surface-zone inhabited by an abundant pelagic fauna at 100 fathoms, and that of the zone inhabited by the bottom animals at 100 fathoms also, the average depth of the ocean being about 1880 fathoms, it results that the intermediate waters, concerning the conditions of life in which we are at present in the utmost uncertainty, really represent more than eight-ninths of the bulk of the entire ocean. Great care should be exercised in drawing conclusions from the depths ascribed to animals in some of the memoirs in the official work on the *Challenger* Expedition. The scientific staff of the Expedition merely recorded on each bottle containing a specimen the depth from which the net in which the specimen was found had been drawn up. In many instances, from the nature of the specimen, it is impossible that it can have come from anywhere but the bottom, but in many others it is quite possible that a particular specimen may have entered the net at any intermediate depth, or close to the surface, and this is a matter on which the author of the monograph in which the specimen is described can form the best conclusion, if one can be formed at all from his knowledge of the animal itself. In all doubtful cases the mere record of the depth must be received with caution.

Just as before the commencement of the present period of deep-sea research there was a strong tendency amongst naturalists, owing to the influence of the views of Edward Forbes, to refuse to accept the clearest evidences of the existence of starfish and other animal life on the sea-bottom at great depths, so there seems now to have sprung up in certain quarters an opposite tendency, leading to the assignment of animals possibly of surface origin to great depths on inconclusive evidence.

With regard to the constitution of the deep-sea fauna, one of its most remarkable features is the general absence from it of Palæozoic forms, excepting so far as representatives of the Mollusca and Brachiopoda are concerned, and it is remarkable that amongst the deep-sea Mollusca no representatives of the *Nautilidæ* and *Ammonitidæ*, so excessively abundant in ancient periods, occur, and that *Lingula*, the most ancient Brachiopod, should occur in shallow water only.

There are no representatives of the most characteristic of the Palæozoic corals, such as *Zaphrentis*, *Cystiphyllum*, *Stauria*, or *Goniophyllum*. Possible representatives of the *Cyathonanidæ* have indeed been obtained in *Gruyina*, described by Prof. Martin Duncan, and *Haplophyllia* and *Duncania*, described by the late Count Pourtales, but the *Cyathonanidæ* are the least observant and characteristic members of so-called *Rugosa*. Pourtales justly felt doubtful whether the arrangement of the septa in four systems instead of six could in itself be considered as a criterion of the *Rugosa*,[1] and in the cases of *Haplophyllia* and *Duncania* the septa may be described rather as devoid of any definite numerical arrangement than exhibiting any tetrameral grouping. Further, I have lately examined by means of sections the structure of the soft parts of *Duncania* in a specimen kindly given to me by Mr. Alexander Agassiz for the purpose, and find that with regard to the peculiar arrangement of the longitudinal septal muscles and the demarcation of the directive septa the coral agrees essentially with the henactinian *Caryophyllia* and all other modern *Madreporaria* the anatomy of which has been adequately investigated.

There are further no representatives of the ancient *Alcyonarians*, forming massive coralla, the *Helioporidæ* and their allies, in deep water, no *Palæocrinoids*, *Cystidea*, or *Blastoidea*, no *Palechinoidea*, no *Trilobites*, no allies of *Limulus*, no *Ganoids*. Further, other ancestral forms, certainly of great antiquity, although unrecorded geologically, such as *Amphioxus*, do not occur in deep water. It might well have been expected that, had the deep sea been fully colonised in the Palæozoic period, a considerable series of representative forms of that age might have survived there in the absence of most of the active physical agents of modification which characterise the coast regions.

From the results of present deep-sea research, it appears that almost all modern littoral forms are capable of adapting themselves to the conditions of deep-sea life, and there is no reason why Palæozoic forms should not have done so if the abyssal conditions were similar to those now existing, just as a considerable number of forms of the chalk period have survived there. In fact, however, most of the survivals of very ancient forms—*Heliopora*, *Limulus*, *Amphioxus*, *Dipnoi*, *Ganoids*—occur in shallow seas or fresh water.

With regard to the origin of the deep-sea fauna, there can be little doubt that it has been derived almost entirely from the littoral fauna, which also must have preceded, and possibly given rise to, the entire terrestrial fauna. Although the littoral, and even its offspring, the terrestrial faunas, have undoubtedly, during the progress of time, contributed to the pelagic fauna, and although it is very likely that the first traces of life may have come into existence in the shallow waters of the coast, it is not improbable that we should look to the pelagic conditions of existence as those under which most of the earliest types of animal life were developed. Nearly all the present inhabitants of the littoral zone revert to the pelagic free-swimming form of existence in their early developmental stages, or in cases where these stages have been lost can be shown to have once possessed it. And these pelagic larval forms are in many cases so closely alike in essential structure, though springing from parents allied but widely differentiated from one another in the adult form, that it is impossible to regard them as otherwise than ancestral. Had they been produced by independent modification of the early stages of the several adult forms as a means of aiding in the diffusion of the species, they must have become more widely differentiated from one another. The various early pelagic free-swimming forms, represented now mostly only by larvæ, gradually adapted themselves to coast life, and underwent various modifications to enable them to withstand the beating of the surf on the shores and the actual modifying alterations of the tides, which, together with other circumstances of coast life, acted as strong impulses to their further development and differentiation. Some developed hard shells and skeletons as protections; others secured their position by boring in the rocks or mud; others assumed an attached condition, and thus resisted the wash of the waves. A remarkable instance in point, about the circumstances of which there can be little doubt, is that of the *Cirripedia*. The *Cypris* larva of *Balanus*, evidently of pelagic origin, sprung from a *Nauplius*, fixes itself by its head to the rocks and develops a hard conical shell, by means of which it withstands the surf in places where nothing else can live. In the same way the *Planula* larva, the Palæozoic cœlenterate form, produces the reef coral and various other forms specially modified for and by the conditions of littoral existence. Similarly echinoderms, Mollusca, Polyzoa, Crustacea, recapitulate in their ontogeny their passage from a pelagic into a littoral form of existence.

It is because the ancestors of nearly all animals have passed through a littoral phase of existence, preceded mostly by a pelagic phase, that the investigations now being carried on on the coasts in marine laboratories throw floods of light on all the fundamental problems of zoology. From the littoral fauna a gradual migration must have taken place into the deep sea, but probably this did not occur till the littoral fauna was very fully established and considerable pressure was brought to bear on it by the struggle for existence. Further, since a large share of the present food of deep-sea animals is derived from coast-debris, life must have become abundant in the littoral zone before there could have been a sufficient food-supply in the deeper regions adjoining it. Not until the development of terrestrial vegetation and animal life can the supply have reached its present abundance. Such a condition was, however, certainly reached in the Carboniferous period. From what has been stated as to the general absence of representatives of Palæozoic forms from the deep sea, it is just possible that if deep oceans existed in Palæozoic periods they may not have been colonised at all, or to a very small extent, then, and that active migration into deep waters commenced in the secondary period. Very possibly the discharges of carbonic acid from the interior of the earth, which Prof. Dittmar believes may have been sufficient to account for the vast existing deposits of coal and limestone, may have been much more abundant than at present over the deep-sea beds in the Palæozoic period, and have rendered the deep waters more or less uninhabitable.

In his splendid monograph on the *Pourtalesia*,[1] which has recently appeared, Prof. Loven has dwelt on the peculiar importance of the littoral region, and of the infinity of agencies present in it "competent to call into play the tendencies to vary

[1] "Zoological Results of the Hassler Expedition." See *Cat. Mus. Comp. Zool. Harvard*, No. viii. 1874, p. 44.

[1] *On Pourtalesia, a Group of Echinoidea*, by Sven Loven. (Stockholm, 1883.)

which are embodied in each species." He treats of the origin of the deep-sea fauna from that of the littoral region. It is impossible here to follow him in his most valuable speculations. In one matter, however, I would venture to express a difference of opinion. He regards the littoral forms of invertebrates as migrating into the deep sea by the following process: Their free-swimming larvæ are supposed to be carried out by currents far from land, and then, having completed their development, to sink to the bottom, where a very few survive and thrive. It is hardly to be conceived that any animal, especially in a young and tender condition, could suddenly adapt itself to the vast change of conditions entailed in a move from littoral to deep-sea life. It seems to me much more likely that the move of animals from the shallow to the deep sea has been of the most gradual kind, and spread over long series of generations, which may have migrated downwards, perhaps a fathom or so in a century, partly by very slight migrations of the adults, partly by very short excursions of larvæ. Thus alone, by almost insensible steps, could animals, such as those under consideration, be enabled to survive an entire change of food, light, temperature, and surroundings.

NOTES

WE have received a box of plants from the Ben Nevis Observatory, including specimens of *Saxifraga stellaris*, from a height of 4400 feet, and *Armeria vulgaris*, from 4370 feet. The plants of these two species, with the numerous flowers which covered them, are as large and well grown as any we have ever seen at lower levels. There is also a specimen of *Gnaphalium supinum*, fairly well grown and in flower, from a height of 4370 feet, and a single plant of *Oxyria reniformis* from 4390 feet, also fairly well grown, but not in flower. The interest attached to the collection is the great height at which they have been found growing in full vigour, the heights being greater than those hitherto given in our "Floras" as the limits of growth of the species in the British Islands. In the case of *Armeria vulgaris* the height is considerably greater, 3800 feet being the limit assigned to this species in Hooker's "British Flora."

THE Hygienic Congress has voted a resolution requesting the Dutch Government to convoke an International Conference on Cholera, for the purpose of establishing a permanent International Epidemical Committee, and preparing an international penal sanitary code. The Congress has denounced the modern system of education and competitive examinations as injurious to health.

ON the afternoon of Tuesday, August 12, the foundation-stone of the new Meteorological Observatory at Falmouth was laid by the Right Hon. the Earl of Mount Edgcumbe, the President of the Royal Cornwall Polytechnic Society, in the presence of an assembly of over 400 persons. Some eighteen months ago the Meteorological Council of London gave notice that they intended to maintain only three first-class Observatories in the British Isles, and that the grants to four out of the seven then existing would cease on December 31, 1883. The three which they decided on continuing to subsidise were Kew, Valentia, and Aberdeen, but in view of the good work done and the value of the observations, the Council expressed the hope that local efforts would succeed in maintaining the Observatories at the other four stations. The Meteorological Committee of the Royal Cornwall Polytechnic Society, which has the local management of the Observatory, made strenuous efforts to retain the institution in Falmouth, and, after much negotiation, the Meteorological Office agreed to continue their grant of 250*l.* a year, provided a new building were erected on a site approved by them, at some distance from the harbour, so as to be free from the disturbing influences on the wind which the harbour and its surroundings were supposed to cause. The Polytechnic Society took the matter up with great zeal and determination, and the result has been that matters in connection with the new Observatory have become sufficiently matured to admit of the foundation-stone being laid. The new Observatory is situate at the top of Killigrew Street, opposite Belmont. It will not take the shape of the present one, with its high octagonal tower, but will be built in the form of a villa residence, one portion to be the dwelling-house of the superintendent (Mr. E. Kitto, F.R.Met.S.), the other to be specially constructed for the reception of the various instruments. The apparatus in the present Observatory will be transferred to the new one, and the observations now carried on will be continued without alteration, so that the Falmouth Observatory will still be one of the first-class Observatories of the United Kingdom, under the control of the Meteorological Office. The instruments are a barograph, for recording the changes of the barometer; a thermograph, for recording the variations of both the dry and the wet bulb thermometers; the anemograph, to measure the force and direction of the wind; a sunshine recorder; and a self-registering rain-gauge. In addition to these, the Royal Society have made a grant for the purchase of a complete set of magnetographs for recording the declination of the needle, and the force of the magnetic currents. Although these will entail an extra annual expense of 50*l.* a year, the Polytechnic Society has boldly undertaken the responsibility of this work. Seeing that the great value of magnetic observations depends to a large extent on their being taken simultaneously and continuously at stations far removed from one another, and that there is now no first-class magnetic Observatory in the British Isles west of Oxford, Falmouth Observatory may be expected to supply a valuable additional data to our knowledge of this comparatively little-known subject. The Observatory will be under the superintendence of Mr. E. Kitto, the Secretary of the Royal Cornwall Polytechnic Society.

PROF. C. V. RILEY, U.S. Entomologist, and Curator of Insects in the U.S. National Museum, left for home on the 23rd, expecting to arrive in time for the meeting of the American Association for the Advancement of Science in Philadelphia. During his two month's sojourn in Europe he has twice been on the Continent, and has visited correspondents and acquaintances both there and in England, examining the insect collections in various museums, and especially in our own at South Kensington. He speaks favourably of the lasting influence for good which the International Forestry Exhibition at Edinburgh will have, and of the Serrel serigraph—an American invention which has of late years been perfected in Lyons, and which he thinks is destined to revolutionise silk-reeling and profoundly influence silk-culture, which is just now attracting unusual attention in the States. He was also much interested with the investigations into the life-habits of the *Aphididæ* that are being carried on by Jules Lichtenstein at Montpellier, and with the thoroughness with which the French authorities encourage experimental research in advanced agriculture. He received a warm welcome at Montpellier, whither he went at the invitation of the French Minister of Agriculture to explain some new methods of dealing with the Phylloxera, and where he found his own recommendations of previous years so fully carried out. He was also surprised at the very extensive and successful experiments with American vines carried on at Pageset near Nîmes. At a meeting of the Société d'Agriculture d'Hérault, held on June 30, he read a paper entitled "Quelques Mots sur les Insecticides aux États-Unis, et proposition d'un nouveau remède," which appears in full, with an account of the discussion, &c., in *Le Messager Agricole* for July 10, 1884. The "new" remedy is kerosene emulsion, which has been successfully used, especially against *Coccidæ*, in the States. Its application against the Phylloxera is recommended in much the same manner as is used with regard to bisulphate of carbon; the proportions recommended are 300 or 400 grammes of the emulsion in 40 litres of water.