THE DOVER MEETING OF THE BRITISH ASSOCIATION.

IT is hardly yet possible to give anything like an accurate estimate of the number of members likely to be present at the meeting of the Association, but it is probable that there will be at least 1500 visitors. The foreign members of the Association will be well represented. Prof. Chappuis, of Paris, will discuss Thermometry in Section A. It is likely that Prof. Remsen will pay a flying visit to the meeting. Amongst other Americans who have promised to attend are Prof. Rotch, of the Blue Hill Observatory, Prof. Bauer, of the Magnetic and Geodetic Survey, Profs. Barker, Carl Barus, Campbell, Thurston and Scott. Prof. Calmette, of the Pasteur Institute, may possibly attend, but he is at present engaged on the Plague Commission at Oporto : Prof. Kossel, of Marburg, is with him, but hopes to come to Dover if possible. Prof. Kronecker, of Berne, will be also present, so that the foreign physiologists are well represented. Of foreign chemists, besides those mentioned, are Profs. Ladenburg and Fittig and Georges Lemoine. Geography will be represented by Prof. Hjort, of Christiania ; Dr. Gerhard Schott, of the Deutsche Seewarte, who will speak on the results of the '*Valdivia* Deep Sea Exploration ; H. Arctowski, who will read a paper on Arctic Exploration ; Admiral Markaroff, of the Russian Navy, will also attend. Abbé Renard, of Ghent, Dr. van Rijckevosel, Prof. Julin, of Liége, Prof. Cyon are a few of the other celebrated foreign men of science who are expected.

The question of accommodation of the visitors has reached a very acute stage, if we may judge from the letters which have appeared in various London papers. Dover lodging-house keepers and their agents persist in thinking that the meeting of the Association will permit Ascot week charges to be made. There are lodgings of all kinds to be found at really moderate charges without difficulty, and the local secretaries are willing to do all in their power to assist members to obtain such accommodation. The strongest representations have been made to the agents on the subject, and it is probable that there will be no further difficulties ; but of course it is hard to remove an impression which has got abroad.

The installation of the Marconi system of wireless telegraphy has now been made in the Town Hall, and a sufficiently lofty pole has been erected to permit of the direct transmission of messages to Wimereux. It is intended that Prof. Fleming should transmit a message of congratulation on the evening of his lecture to the meeting held at Rome on the same day, and that the reply will reach Boulogne by the ordinary wire, and be transmitted by the wireless system before the meeting terminates. Demonstrations of the Marconi system of telegraphy will take place at the Mayor's conversazione on Thursday evening.

It is hoped that about 400 of the French men of science will attend the luncheon to members of the French Association on Saturday. Some twenty Belgian geologists, who have been visiting London during the past ten days, will also be present.

On the occasion of the French visit there will be special facilities for the inspection of the Castle, which will be closed to the general public on the afternoon of Saturday, to permit the military authorities to devote themselves entirely to the members of the British Association and their guests.

The details of the foreign tour are all settled. Those who take part in this tour will have every occasion to look back with pleasure upon a very pleasant visit. At most of the towns visited there will be official receptions, with something of a special nature at Brussels on Sunday, September 24.

The following details of the work in the Sections, omitted from last week's article, have now been supplied.

In Section C (Geology) the address of the President, Sir Archibald Geikie, will, it is hoped, be delivered on Saturday, September 16, in order that the members of the French Association may be present. The address will deal with matters of equal interest to geologists and physicists. Reports will be presented by a committee appointed at the Toronto meeting to investigate the Pleistocene flora and fauna of Canada ; by a committee which has been securing photographic records of the disappearing drift section at Moel Tryfan ; by the three committees appointed a short time ago to investigate the ossiferous caves at Uphill, near Weston-super-Mare, the Ty-Newydd caves, and the Irish elk remains in the Isle of Man. The report of the committee which has been engaged for some years in collecting photographs of geological interest in the British Isles, will this year be accompanied by that of a similar committee appointed for the same purpose in Canada ; and reports may be expected from other committees on erratic blocks, on life-zones in British carboniferous rocks, and on the registration of type specimens.

The chief interest of this Section will, no doubt, centre round the explorations for coal in Kent, and communications on this subject from Prof. Boyd Dawkins and Mr. Robert Etheridge will be awaited with expectation : in connection with this subject Mr. Jukes-Browne promises a paper on a boring made through the chalk at Dieppe in 1898. Many French and Belgian geologists will, it is hoped, take part in the discussion on this and kindred subjects, especially as the Belgian Geological Society is holding a special meeting at Dover during that of the British Association. Among foreign visitors Prof. van den Broeck promises a paper on the Iguanodons of Bernissart, and Prof. Renard one on the origin of chondritic meteorites.

Among papers of local interest will be one by Prof. Boyd Dawkins on the geology of the Channel Tunnel ; one by Dr. Rowe on the Dover chalk ; and one by Captain McDakin on coast erosion. Among other papers promised in this Section may be mentioned Prof. Sollas on homotaxy and on contemporaneity, and also on the origin of flint ; Mr. Vaughan Cornish on photographs of wave phenomena ; Dr. F. Moreno on Neomylodon ; Prof. Watts on the Mount Sorel granite ; Mr. G. Abbott on water zones and their influence on concretions ; Mr. Plunkett on the Fermanagh Caves ; and Dr. Tempest Anderson on the 1898 eruption of Vesuvius.

In Section G (Mechanical Science) the programme of papers to be read and discussed is as follows :—Thursday, Presidential Address by Sir William White, K.C.B., F.R.S. ; the Dover Admiralty Harbour Works, by W. Mathews ; non-inflammable wood and its use in warships, by E. Marshall Fox. Friday, a short history of the engineering works of the Suez Canal to the present time, by Sir Charles Hartley, K.C.M.G ; fast cross-Channel steamers, by Hon. C. A. Parsons, F.R.S. ; the Niclausse water-tube boiler, by M. Robinson ; the discharge of torpedoes below water, by Captain Lloyd. Saturday, the erection of Alexander III. Bridge in Paris, by A. Alby (of Paris). Monday, electrical machinery on board ship, by A. Siemens ; earth currents from electric tramways, by J. Swinburne ; some recent applications of electro-metallurgy to chemical engineering, by Sherard Cowper Coles ; signalling without contact, a new system of railway signalling, by Wilfrid S. Boult. Tuesday, recent experiences with steam on common roads, by J. I. Thornycroft, F.R.S. ; the Dymchurch wall and the reclamation of Romney Marsh, by E. Case ; an instrument for gauging the circularity of boiler furnaces and producing a diagram, by T. Messenger ; and the sea lights of the south and south-east coasts of England, including the Channel and Scilly Islands, by T. Kenward.

INAUGURAL ADDRESS BY PROF. SIR MICHAEL FOSTER, K.C.B., SEC.R.S., PRESIDENT OF THE ASSOCIATION.

HE who until a few minutes ago was your President said somewhere at the meeting at Bristol, and said with truth, that among the qualifications needed for the high honour of Presidency of the British Association for the Advancement of Science, that of being old was becoming more and more dominant. He who is now attempting to speak to you feels that he is rapidly earning that distinction. But the Association itself is older than its President; it has seen pass away the men who, wise in their generation, met at York on September 27, 1831, to found it: it has seen other great men who in bygone years served it as Presidents, or otherwise helped it on, sink one after another into the grave. Each year, indeed, when it plants its flag as a signal of its yearly meeting, that flag floats half-mast high in token of the great losses which the passing year has brought. This year is no exception; the losses, indeed, are perhaps unwontedly heavy. I will not attempt to call over the sad roll-call; but I must say a word about one who was above most others a faithful and zealous friend of the Association. Sir Douglas Galton joined the Association in 1860. From 1871 to 1895, as one of the General Secretaries, he bore, and bore to the great good of the Association, a large share of the burden of the Association's work. How great that share was is perhaps especially known to the many men, among whom I am proud to count myself, who during his long term of office served in succession with him as brother General Secretary. In 1895, at Ipswich, he left the post of General Secretary, but only to become President. So long and so constantly did he labour for the good of the Association that he seemed to be an integral part of it, and meeting as we do to-day, and as we henceforward must do, without Douglas Galton, we feel something greatly missing. This year, perhaps even more than in other years, we could have wished him to be among us; for to-day the Association may look with joy, not unmixed with pride, on the realisation of a project in forwarding which it has had a conspicuous share, on the commencement of an undertaking which is not only a great thing in itself, but which, we trust, is the beginning of still greater things to come. And the share which the Association has had in this was largely Sir Douglas Galton's doing. In his Address as President of Section A, at the meeting of the Association at Cardiff in 1891, Prof. Oliver Lodge expounded with pregnant words how urgently, not pure science only, but industry and the constructive arts—for the interests of these are ever at bottom the same—needed the aid of some national establishment for the prosecution of prolonged and costly physical researches, which private enterprise could carry out in a lame fashion only, if at all. Lodge's words found an echo in many men's minds; but the response was for a long while in men's minds only. In 1895 Sir Douglas Galton, having previously made a personal study of an institution analogous to the one desired—namely, the Reichsanstalt at Berlin—seized the opportunity offered to him as President of the Association at Ipswich to insist, with the authority not only of the head for the time being of a great scientific body, but also of one who himself knew the ways and wants at once of science and of practical life, that the thing which Lodge and others had hoped for was a thing which could be done, and ought to be done at once. And now to-day we can say it has been done. The National Physical Laboratory has been founded. The Address at Ipswich marked the beginning of an organised effort which has at last been crowned with success. A feeling of sadness cannot but come over us when we think that Sir Douglas Galton was not spared to see the formal completion of the scheme whose birth he did so much to help, and which, to his last days, he aided in more ways than one. It is the old story—the good which men do lives after them.

Still older than the Association is this nineteenth century, now swiftly drawing to its close. Though the century itself has yet some sixteen months to run, this is the last meeting of the British Association which will use the numbers eighteen hundred to mark its date.

The eyes of the young look ever forward; they take little heed of the short though ever-lengthening fragment of life which lies behind them; they are wholly bent on that which is to come. The eyes of the aged turn wistfully again and again to the past; as the old glide down the inevitable slope their present becomes a living over again the life which has gone before, and the future takes on the shape of a brief lengthening of the past. May I this evening venture to give rein to the impulses of advancing years? May I, at this last meeting of the Association in the eighteen hundreds, dare to dwell for a while upon the past, and to call to mind a few of the changes which have taken place in the world since those autumn days in which men were saying to each other that the last of the seventeen hundreds was drawing towards its end?

Dover in the year of our Lord seventeen hundred and ninety-nine was in many ways unlike the Dover of to-day. On moonless nights men groped their way in its narrow streets by the help of swinging lanterns and smoky torches, for no lamps lit the ways. By day the light of the sun struggled into the houses through narrow panes of blurred glass. Though the town then, as now, was one of the chief portals to and from the countries beyond the seas, the means of travel were scanty and dear, available for the most part to the rich alone, and, for all, beset with discomfort and risk. Slow and uncertain was the carriage of goods, and the news of the world outside came to the town—though it from its position learnt more than most towns—tardily, fitfully, and often falsely. The people of Dover sat then much in dimness, if not in darkness, and lived in large measure on themselves. They who study the phenomena of living beings tell us that light is the great stimulus of life and that the fulness of the life of a being or of any of its members may be measured by the variety, the swiftness, and the certainty of the means by which it is in touch with its surroundings. Judged from this standpoint then life at Dover, as indeed elsewhere, must have fallen far short of the life of to-day.

The same study of living beings, however, teaches us that while from one point of view the environment seems to mould the organism, from another point the organism seems to be master of its environment. Going behind the change of circumstances, we may raise the question, the old question, Was life in its essence worth more then than now? Has there been a real advance?

Let me at once relieve your minds by saying that I propose to leave this question in the main unanswered. It may be, or it may not be, that man's grasp of the beautiful and of the good, if not looser, is not firmer than it was a hundred years ago. It may be, or it may not be, that man is no nearer to absolute truth, to seeing things as they really are, than he was then. I will merely ask you to consider with me for a few minutes how far, and in what ways, man's laying hold of that aspect of or part of truth which we call natural knowledge, or sometimes science, differed in 1799 from what it is to-day, and whether that change must not be accounted a real advance, a real improvement in man.

I do not propose to weary you by what in my hands would be the rash effort of attempting a survey of all the scientific results of the nineteenth century. It will be enough if for a little while I dwell on some few of the salient features distinguishing the way in which we nowadays look upon, and during the coming week shall speak of, the works of nature around us—though those works themselves, save for the slight shifting involved in a secular change, remain exactly the same—from the way in which they were looked upon and might have been spoken of at a gathering of philosophers at Dover in 1799. And I ask your leave to do so.

In the philosophy of the ancients, earth, fire, air, and water were called "the elements." It was thought, and rightly thought, that a knowledge of them and of their attributes was a necessary basis of a knowledge of the ways of nature. Translated into modern language, a knowledge of these "elements" of old means a knowledge of the composition of the atmosphere, of water, and of all the other things which we call matter, as well as a knowledge of the general properties of gases, liquids, and solids, and of the nature and effects of combustion. Of all these things our knowledge to-day is large and exact, and, though ever enlarging, in some respects complete. When did that knowledge begin to become exact?

To-day the children in our schools know that the air which wraps round the globe is not a single thing, but is made up of two things, oxygen and nitrogen,[1] mingled together. They know, again, that water is not a single thing, but the product of two things, oxygen and hydrogen, joined together. They know that when the air makes the fire burn and gives the animal life, it is the oxygen in it which does the work. They know that all

[1] Some may already know that there is at least a third thing, argon.

round them things are undergoing that union with oxygen which we call oxidation, and that oxidation is the ordinary source of heat and light. Let me ask you to picture to yourselves what confusion there would be to-morrow, not only in the discussions at the sectional meetings of our Association, but in the world at large, if it should happen that in the coming night some destroying touch should wither up certain tender structures in all our brains, and wipe out from our memories all traces of the ideas which cluster in our minds around the verbal tokens, oxygen and oxidation. How could any of us, not the so-called man of science alone, but even the man of business and the man of pleasure, go about his ways lacking those ideas? Yet those ideas were in 1799 lacking to all but a few.

Although in the third quarter of the seventeenth century the light of truth about oxidation and combustion had flashed out in the writings of John Mayow, it came as a flash only, and died away as soon as it had come. For the rest of that century, and for the greater part of the next, philosophers stumbled about in darkness, misled for the most of the time by the phantom conception which they called phlogiston. It was not until the end of the third quarter of the eighteenth century that the new light, which has burned steadily ever since, lit up the minds of the men of science. The light came at nearly the same time from England and from France. Rounding off the sharp corners of controversy, and joining, as we may fitly do to-day, the two countries as twin bearers of a common crown, we may say that we owe the truth to Priestley, to Lavoisier, and Cavendish. If it was Priestley who was the first to demonstrate the existence of what we now call oxygen, it is to Lavoisier we owe the true conception of the nature of oxidation and the clear exposition of the full meaning of Priestley's discovery, while the knowledge of the composition of water, the necessary complement of the knowledge of oxygen, came to us through Cavendish and, we may perhaps add, through Watt.

The date of Priestley's discovery of oxygen is 1774, Lavoisier's classic memoir "on the nature of the principle which enters into combination with metals during calcination" appeared in 1775, and Cavendish's paper on the composition of water did not see the light until 1784.

During the last quarter of the eighteenth century this new idea of oxygen and oxidation was struggling into existence. How new was the idea is illustrated by the fact that Lavoisier himself at first spoke of that which he was afterwards, namely in 1778, led to call oxygen, the name by which it has since been known, as "the principle which enters into combination." What difficulties its acceptance met with is illustrated by the fact that Priestley himself refused to the end of his life to grasp the true bearings of the discovery which he had made. In the year 1799 the knowledge of oxygen, of the nature of water and of air, and indeed the true conception of chemical composition and chemical change, was hardly more than beginning to be, and the century had to pass wholly away before the next great chemical idea, which we know by the name of the Atomic Theory of John Dalton, was made known. We have only to read the scientific literature of the time to recognise that a truth which is now not only woven as a master-thread into all our scientific conceptions, but even enters largely into the everyday talk and thoughts of educated people, was a hundred years ago struggling into existence among the philosophers themselves. It was all but absolutely unknown to the large world outside those select few.

If there be one word of science which is writ large on the life of the present time, it is the word "electricity"; it is, I take it, writ larger than any other word. The knowledge which it denotes has carried its practical results far and wide into our daily life, while the theoretical conceptions which it signifies pierce deep into the nature of things. We are to-day proud, and justly proud, both of the material triumphs and of the intellectual gains which it has brought us, and we are full of even larger hopes of it in the future.

At what time did this bright child of the nineteenth century have its birth?

He who listened to the small group of philosophers of Dover, who in 1799 might have discoursed of natural knowledge, would perhaps have heard much of electric machines, of electric sparks, of the electric fluid, and even of positive and negative electricity; for frictional electricity had long been known and even carefully studied. Probably one or more of the group, dwelling on the observations which Galvani, an Italian, had made known some twenty years before, developed views on the connection of electricity with the phenomena of living bodies. Possibly one of them was exciting the rest by telling how he had just heard that a professor at Pavia, one Volta, had discovered that electricity could be produced not only by rubbing together particular bodies, but by the simple contact of two metals, and had thereby explained Galvani's remarkable results. For, indeed, as we shall hear from Prof. Fleming, it was in that very year, 1799, that electricity as we now know it took its birth. It was then that Volta brought to light the apparently simple truths out of which so much has sprung. The world, it is true, had to wait for yet some twenty years before both the practical and the theoretic worth of Volta's discovery became truly pregnant, under the fertilising influence of another discovery. The loadstone and magnetic virtues had, like the electrifying power of rubbed amber, long been an old story. But, save for the compass, not much had come from it. And even Volta's discovery might have long remained relatively barren had it been left to itself. When, however, in 1819, Oersted made known his remarkable observations on the relations of electricity to magnetism, he made the contact needed for the flow of a new current of ideas. And it is perhaps not too much to say that those ideas, developing during the years of the rest of the century with an ever-accelerating swiftness, have wholly changed man's material relations to the circumstances of life, and at the same time carried him far in his knowledge of the nature of things.

Of all the various branches of science, none perhaps is to-day, none for these many years past has been, so well known to, even if not understanded by, most people as that of geology. Its practical lessons have brought wealth to many; its fairy tales have brought delight to more; and round it hovers the charm of danger, for the conclusions to which it leads touch on the nature of man's beginning.

In 1799 the science of geology, as we know it, was struggling into birth. There had been from of old cosmogonies, theories as to how the world had taken shape out of primæval chaos. In that fresh spirit which marked the zealous search after natural knowledge pursued in the middle and latter part of the seventeenth century, the brilliant Stenson, in Italy, and Hooke, in our own country, had laid hold of some of the problems presented by fossil remains; and Woodward, with others, had laboured in the same field. In the eighteenth century, especially in its latter half, men's minds were busy about the physical agencies determining or modifying the features of the earth's crust; water and fire, subsidence from a primæval ocean and transformation by outbursts of the central heat, Neptune and Pluto, were being appealed to, by Werner on the one hand, and by Desmarest on the other, in explanation of the earth's phenomena. The way was being prepared, theories and views were abundant, and many sound observations had been made; and yet the science of geology, properly so called, the exact and proved knowledge of the successive phases of the world's life, may be said to date from the closing years of the eighteenth century.

In 1873 James Hutton put forward in a brief memoir his "Theory of the Earth," which in 1795, two years before his death, he expanded into a book; but his ideas failed to lay hold of men's minds until the century had passed away, when in 1802 they found an able expositor in John Playfair. The very same year that Hutton published his theory, Cuvier came to Paris and almost forthwith began, with Brongniart, his immortal researches into the fossils of Paris and its neighbourhood. And four years later, in the year 1799 itself, William Smith's tabular list of strata and fossils saw the light. It is, I believe, not too much to say that out of these geology, as we now know it, sprang. It was thus in the closing years of the eighteenth century that was begun the work which the nineteenth century has carried forward to such great results. But at that time only the select few had grasped the truth, and even they only the beginning of it. Outside a narrow circle the thoughts, even of the educated, about the history of the globe were bounded by the story of the Deluge—though the story was often told in a strange fashion—or were guided by fantastic views of the plastic forces of a sportive nature.

In another branch of science, in that which deals with the problems presented by living beings, the thoughts of men in 1799 were also very different from the thoughts of men to-day.

It is a very old quest, the quest after the knowledge of the nature of living beings, one of the earliest on which man set out; for it promised to lead him to a knowledge of himself, a promise which perhaps is still before us, but the fulfilment of which is as yet far off. As time has gone on, the pursuit of natural knowledge has seemed to lead man away from himself into the furthermost parts of the universe, and into secret workings of nature in which he appears to be of little or no account; and his knowledge of the nature of living things and so of his own nature, has advanced slowly, waiting till the progress of other branches of natural knowledge can bring it aid. Yet in the past hundred years, the biologic sciences, as we now call them, have marched rapidly onward.

We may look upon a living body as a machine doing work in accordance with certain laws, and may seek to trace out the working of the inner wheels, how these raise up the lifeless dust into living matter, and let the living matter fall away again into dust, giving out movement and heat. Or we may look upon the individual life as a link in a long chain, joining something which went before to something about to come, a chain whose beginning lies hid in the farthest past, and may seek to know the ties which bind one life to another. As we call up to view the long series of living forms, living now or flitting like shadows on the screen of the past, we may strive to lay hold of the influences which fashion the garment of life. Whether the problems of life are looked upon from the one point of view or the other, we to-day, not biologists only, but all of us, have gained a knowledge hidden even from the philosophers a hundred years ago.

Of the problems presented by the living body viewed as a machine, some may be spoken of as mechanical, others as physical, and yet others as chemical. while some are, apparently at least, none of these. In the seventeenth century William Harvey, laying hold of the central mechanism of the blood stream, opened up a path of inquiry which his own age and the century which followed trod with marked success. The knowledge of the mechanics of the animal and of the plant advanced apace; but the physical and chemical problems had yet to wait. The eighteenth century, it is true, had its physics and its chemistry; but, in relation at least to the problems of the living being, a chemistry which knew not oxygen and a physics which knew not the electricity of chemical action were of little avail. The philosopher of 1799, when he discussed the functions of the animal or of the plant involving chemical changes, was fain for the most part, as were his predecessors in the century before, to have recourse to such vague terms as "fermentation" and the like; to-day our treatises on physiology are largely made up of precise and exact expositions of the play of physical agencies and chemical bodies in the living organism. He made use of the words "vital force" or "vital principle," not as an occasional, but as a common explanation of the phenomena of the living body. During the present century, especially during its latter half, the idea embodied in those words has been driven away from one seat after another; if we use it now when we are dealing with the chemical and physical events of life we use it with reluctance, as a *deus ex machina* to be appealed to only when everything else has failed.

Some of the problems—and those, perhaps, the chief problems—of the living body have to be solved neither by physical nor by chemical methods, but by methods of their own. Such are the problems of the nervous system. In respect to these the men of 1799 were on the threshold of a pregnant discovery. During the latter part of the present century, and especially during its last quarter, the analysis of the mysterious processes in the nervous system which issue as feeling, thought, and power to move, has been pushed forward with a success conspicuous in its practical, and full of promise in its theoretical, gains. That analysis may be briefly described as a following up of threads. We now know that what takes place along a tiny thread which we call a nerve-fibre differs from that which takes place along its fellow-threads, that differing nervous impulses travel along different nerve-fibres, and that nervous and psychical events are the outcome of the clashing of nervous impulses as they sweep along the closely-woven web of living threads of which the brain is made. We have learnt by experiment and by observation that the pattern of the web determines the play of the impulses, and we can already explain many of the obscure problems, not only of nervous disease, but of nervous life, by an analysis which is a tracking out the devious and linked paths of nervous threads. The very beginning of this analysis was known in 1799. Men knew that nerves were the agents of feeling and of the movements of muscles; they had learnt much about what this part or that part of the brain could do; but they did not know that one nerve-fibre differed from another in the very essence of its work. It was just about the end of the past century, or the beginning of the present one, that an English surgeon began to ponder over a conception which, however, he did not make known until some years later, and which did not gain complete demonstration and full acceptance until still more years had passed away. It was in 1811, in a tiny pamphlet published privately, that Charles Bell put forward his "New Idea" that the nervous system was constructed on the principle that "the nerves are not single nerves possessing various powers, but bundles of different nerves, whose filaments are united for the convenience of distribution, but which are distinct in office as they are in origin from the brain."

Our present knowledge of the nervous system is to a large extent only an exemplification and expansion of Charles Bell's "New Idea," and has its origin in that.

If we pass from the problems of the living organism viewed as a machine to those presented by the varied features of the different creatures who have lived or who still live on the earth, we at once call to mind that the middle years of the present century mark an epoch in biologic thought such as never came before, for it was then that Charles Darwin gave to the world the "Origin of Species."

That work, however, with all the far-reaching effects which it has had, could have had little or no effect, or, rather, could not have come into existence, had not the earlier half of the century been in travail preparing for its coming. For the germinal idea of Darwin appeals, as to witnesses, to the results of two lines of biologic investigation which were almost unknown to the men of the eighteenth century.

To one of these lines I have already referred. Darwin, as we know, appealed to the geological record; and we also know how that record, imperfect as it was then, and imperfect as it must always remain, has since his time yielded the most striking proofs of at least one part of his general conception. In 1799 there was, as we have seen, no geological record at all.

Of the other line I must say a few words.

To-day the merest beginner in biologic study, or even that exemplar of acquaintance without knowledge, the general reader, is aware that every living being, even man himself, begins its independent existence as a tiny ball, of which we can, even acknowledging to the full the limits of the optical analysis at our command, assert with confidence that in structure, using that word in its ordinary sense, it is in all cases absolutely simple. It is equally well known that the features of form which supply the characters of a grown-up living being, all the many and varied features of even the most complex organism, are reached as the goal of a road, at times a long road, of successive changes; that the life of every being, from the ovum to its full estate, is a series of shifting scenes, which come and go, sometimes changing abruptly, sometimes melting the one into the other, like dissolving views, all so ordained that often the final shape with which the creature seems to begin, or is said to begin, its life in the world is the outcome of many shapes, clothed with which it has in turn lived many lives before its seeming birth.

All or nearly all the exact knowledge of the laboured way in which each living creature puts on its proper shape and structure is the heritage of the present century. Although the way in which the chick is moulded in the egg was not wholly unknown even to the ancients, and in later years had been told, first in the sixteenth century by Fabricius, then in the seventeenth century in a more clear and striking manner by the great Italian naturalist Malpighi, the teaching thus offered had been neglected or misinterpreted. At the close of the eighteenth century the dominant view was that in the making of a creature out of the egg there was no putting on of wholly new parts, no epigenesis. It was taught that the entire creature lay hidden in the egg, hidden by reason of the very transparency of its substance, lay ready-made but folded up, as it were, and that the process of development within the egg or within the womb was a mere unfolding, a simple evolution. Nor did men shrink from accepting the logical outcome of such a view—namely, that within the unborn creature itself lay in like manner, hidden and folded up, its offspring also, and within that again its offspring in turn, after the fashion of a cluster of ivory balls carved by Chinese hands, one within the other. This was no fantastic view put forward by an imaginative dreamer; it was seriously held by sober men, even by men like the illustrious Haller, in spite of their recognising that as the chick grew in the egg some changes of form took place. Though so early as the middle of the eighteenth century Friedrich Caspar Wolff and, later on, others had strenuously opposed such a view, it held its own, not only to the close of the century, but far on into the next. It was not until a quarter of the present century had been added to the past that Von Baer made known the results of researches which once and for all swept away the old view. He and others working after him made it clear that each individual puts on its final form and structure, not by an unfolding of pre-existing hidden features, but by the formation of new parts through the continued differentiation of a primitively simple material. It was also made clear that the successive changes which the embryo undergoes in its progress from the ovum to maturity are the expression of morphologic laws, that the progress is one from the general to the special, and that the shifting scenes of embryonic life are hints and tokens of lives lived by ancestors in times long past.

If we wish to measure how far off in biologic thought the end of the last century stands, not only from the end but even from the middle of this one, we may imagine Darwin striving to write the "Origin of Species" in 1799. We may fancy him being told by philosophers that one group of living beings differed from another group because all its members and all their ancestors came into existence at one stroke when the firstborn progenitor of the race, within which all the rest were folded up, stood forth as the result of a creative act. We may fancy him listening to a debate between the philosopher who maintained that all the fossils strewn in the earth were the remains of animals or plants churned up in the turmoil of a violent universal flood, and dropped in their places as the waters went away, and him who argued that such were not really the "spoils of living creatures," but the products of some playful plastic power which out of the superabundance of its energy fashioned here and there the lifeless earth into forms which imitated, but only imitated, those of living things. Could he amid such surroundings by any flight of genius have beat his way to the conception for which his name will ever be known?

Here I may well turn away from the past. It is not my purpose, nor, as I have said, am I fitted, nor is this perhaps the place, to tell even in outline the tale of the work of science in the nineteenth century. I am content to have pointed out that the two great sciences of chemistry and geology took their birth, or at least began to stand alone, at the close of the last century, and have grown to be what we know them now within about a hundred years, and that the study of living beings has within the same time been so transformed as to be to-day something wholly different from what it was in 1799. And, indeed, to say more would be to repeat almost the same story about other things. If our present knowledge of electricity is essentially the child of the nineteenth century, so also is our present knowledge of many other branches of physics. And those most ancient forms of exact knowledge, the knowledge of numbers and of the heavens, whose beginning is lost in the remote past, have, with all other kinds of natural knowledge, moved onward during the whole of the hundred years with a speed which is ever increasing. I have said, I trust, enough to justify the statement that in respect to natural knowledge a great gulf lies between 1799 and 1899. That gulf, moreover, is a twofold one: not only has natural knowledge been increased, but men have run to and fro spreading it as they go. Not only have the few driven far back round the full circle of natural knowledge the dark clouds of the unknown which wrap us all about, but also the many walk in the zone of light thus increasingly gained. If it be true that the few to day are, in respect to natural knowledge, far removed from the few of those days, it is also true that nearly all which the few alone knew then, and much which they did not know, has now become the common knowledge of the many.

What, however, I may venture to insist upon here is that the difference in respect to natural knowledge, whatever be the case with other differences between then and now, is undoubtedly a difference which means progress. The span between the science of that time and the science of to-day is beyond all question a great stride onwards.

We may say this, but we must say it without boasting. For the very story of the past which tells of the triumphs of science bids the man of science put away from him all thoughts of vain-glory. And that by many tokens.

Whoever, working at any scientific problem, has occasion to study the inquiries into the same problem made by some fellow-worker in the years long gone by, comes away from that study humbled by one or other of two different thoughts. On the one hand, he may find, when he has translated the language of the past into the phraseology of to-day, how near was his forerunner of old to the conception which he thought, with pride, was all his own, not only so true but so new. On the other hand, if the ideas of the investigator of old, viewed in the light of modern knowledge, are found to be so wide of the mark as to seem absurd, the smile which begins to play upon the lips of the modern is checked by the thought, Will the ideas which I am now putting forth, and which I think explain so clearly, so fully, the problem in hand, seem to some worker in the far future as wrong and as fantastic as do these of my forerunner to me? In either case his personal pride is checked. Further, there is written clearly on each page of the history of science, in characters which cannot be overlooked, the lesson that no scientific truth is born anew, coming by itself and of itself. Each new truth is always the offspring of something which has gone before, becoming in turn the parent of something coming after. In this aspect the man of science is unlike, or seems to be unlike, the poet and the artist. The poet is born, not made; he rises up, no man knowing his beginnings; when he goes away, though men after him may sing his songs for centuries, he himself goes away wholly, having taken with him his mantle, for this he can give to none other. The man of science is not thus creative; he is created. His work, however great it be, is not wholly his own; it is in part the outcome of the work of men who have gone before. Again and again a conception which has made a name great has come, not so much by the man's own effort as out of the fulness of time. Again and again we may read in the words of some man of old the outlines of an idea which in later days has shone forth as a great acknowledged truth. From the mouth of the man of old the idea dropped barren, fruitless; the world was not ready for it, and heeded it not; the concomitants and abutting truths which could give it power to work were wanting. Coming back again in later days, the same idea found the world awaiting it; things were in travail preparing for it; and some one, seizing the right moment to put it forth again, leapt into fame. It is not so much the men of science who make science, as some spirit which, born of the truths already won, drives the man of science onward, and uses him to win new truths in turn.

It is because each man of science is not his own master, but one of many obedient servants of an impulse which was at work long before him, and will work long after him, that in science there is no falling back. In respect to other things there may be times of darkness and times of light, there may be risings, decadences, and revivals. In science there is only progress. The path may not be always a straight line, there may be swerving to this side and to that, ideas may seem to return again and again to the same point of the intellectual compass; but it will always be found that they have reached a higher level—they have moved, not in a circle, but in a spiral. Moreover, science is not fashioned as is a house, by putting brick to brick, that which is once put remaining as it was put to the end. The growth of science is that of a living being. As in the embryo phase follows phase, and each member of the body puts on in succession different appearances, though all the while the same member, so a scientific conception of one age seems to differ from that of a following age, though it is the same one in the process of being made; and as the dim outlines of the early embryo, as the being grows, become more distinct and sharp, like a picture on a screen brought more and more into focus, so the dim gropings and searchings of the men of science of old are by repeated approximations wrought into the clear and exact conclusions of later times.

The story of natural knowledge, of science, in the nineteenth century, as, indeed, in preceding centuries, is, I repeat, a story of continued progress. There is in it not so much as a hint of falling back, not even of standing still. What is gained by scientific inquiry is gained for ever; it may be added to, it may seem to be covered up, but it can never be taken away. Confident that the progress will go on, we cannot help peering into the years to come and straining our eyes to foresee what science will become and what it will do as they roll on. While we do so, the thought must come to us, Will all the increasing knowledge of nature avail only to change the ways of man—will it have no effect on man himself?

The material good which mankind has gained and is gaining through the advance of science is so imposing as to be obvious to every one, and the praises of this aspect of science are to be found in the mouths of all. Beyond all doubt science has greatly lessened and has markedly narrowed hardship and suffering; beyond all doubt science has largely increased and has widely diffused ease and comfort. The appliances of science have, as it were, covered with a soft cushion the rough places of life, and that not for the rich only, but also for the poor. So abundant and so prominent are the material benefits of science that in the eyes of many these seem to be the only benefits which she brings. She is often spoken of as if she were useful and nothing more, as if her work were only to administer to the material wants of man.

Is this so?

We may begin to doubt it when we reflect that the triumphs of science which bring these material advantages are in their very nature intellectual triumphs. The increasing benefits brought by science are the results of man's increasing mastery over nature, and that mastery is increasingly a mastery of mind; it is an increasing power to use the forces of what we call inanimate nature in place of the force of his own or other creatures' bodies; it is an increasing use of mind in place of muscle.

Is it to be thought that that which has brought the mind so greatly into play has had no effect on the mind itself? Is that part of the mind which works out scientific truths a mere slavish machine producing results it knows not how, having no part in the good which in its working it brings forth?

What are the qualities, the features of that scientific mind which has wrought, and is working, such great changes in man's relation to nature? In seeking an answer to this question we have not to inquire into the attributes of genius. Though much of the progress of science seems to take on the form of a series of great steps, each made by some great man, the distinction in science between the great discoverer and the humble worker is one of degree only, not of kind. As I was urging just now, the greatness of many great names in science is often, in large part, the greatness of occasion, not of absolute power. The qualities which guide one man to a small truth silently taking its place among its fellows, as these go to make up progress, are at bottom the same as those by which another man is led to something of which the whole world rings.

The features of the fruitful scientific mind are in the main three.

In the first place, above all other things, his nature must be one which vibrates in unison with that of which he is in search; the seeker after truth must himself be truthful, truthful with the truthfulness of nature. For the truthfulness of nature is not wholly the same as that which man sometimes calls truthfulness. It is far more imperious, far more exacting. Man, unscientific man, is often content with "the nearly" and "the almost." Nature never is. It is not her way to call the same two things which differ, though the difference may be measured by less than the thousandth of a milligramme or of a millimetre, or by any other like standard of minuteness. And the man who, carrying the ways of the world into the domain of science, thinks that he may treat nature's differences in any other way than she treats them herself, will find that she resents his conduct; if he in carelessness or in disdain overlooks the minute difference which she holds out to him as a signal to guide him in his search, the projecting tip, as it were, of some buried treasure, he is bound to go astray, and the more strenuously he struggles on, the farther will he find himself from his true goal.

In the second place, he must be alert of mind. Nature is ever making signs to us, she is ever whispering to us the beginnings of her secrets; the scientific man must be ever on the watch, ready at once to lay hold of nature's hint however small, to listen to her whisper however low.

In the third place, scientific inquiry, though it be pre-eminently an intellectual effort, has need of the moral quality of courage—not so much the courage which helps a man to face a sudden difficulty as the courage of steadfast endurance. Almost every inquiry, certainly every prolonged inquiry, sooner or later goes wrong. The path, at first so straight and clear, grows crooked and gets blocked; the hope and enthusiasm, or even the jaunty ease, with which the inquirer set out leave him, and he falls into a slough of despond. That is the critical moment calling for courage. Struggling through the slough he will find on the other side the wicket-gate opening up the real path; losing heart he will turn back and add one more stone to the great cairn of the unaccomplished.

But, I hear some one say, these qualities are not the peculiar attributes of the man of science; they may be recognised as belonging to almost every one who has commanded or deserved success, whatever may have been his walk of life. That is so. That is exactly what I would desire to insist, that the men of science have no peculiar virtues, no special powers. They are ordinary men, their characters are common, even commonplace. Science, as Huxley said, is organised common sense, and men of science are common men, drilled in the ways of common sense.

For their life has this feature. Though in themselves they are no stronger, no better than other men, they possess a strength which, as I just now urged, is not their own, but is that of the science whose servants they are. Even in his apprenticeship, the scientific inquirer, while learning what has been done before his time, if he learns it aright, so learns it that what is known may serve him, not only as a vantage ground whence to push off into the unknown, but also as a compass to guide him in his course. And when fitted for his work he enters on inquiry itself, what a zealous anxious guide, what a strict and, because strict, helpful schoolmistress does nature make herself to him. Under her care every inquiry, whether it bring the inquirer to a happy issue or seem to end in nought, trains him for the next effort. She so orders her ways that each act of obedience to her makes the next act easier for him, and step by step she leads him on towards that perfect obedience which is complete mastery.

Indeed, when we reflect on the potency of the discipline of scientific inquiry we cease to wonder at the progress of scientific knowledge. The results actually gained seem to fall so far short of what under such guidance might have been expected to have been gathered in that we are fain to conclude that science has called to follow her, for the most part, the poor in intellect and the wayward in spirit. Had she called to her service the many acute minds who have wasted their strength struggling in vain to solve hopeless problems, or who have turned their energies to things other than the increase of knowledge; had she called to her service the many just men who have walked straight without the need of a rod to guide them, how much greater than it has been would have been the progress of science, and how many false teachings would the world have been spared! To men of science themselves, when they consider their favoured lot, the achievements of the past should serve, not as a boast, but as a reproach.

If there be any truth in what I have been urging, that the pursuit of scientific inquiry is itself a training of special potency, giving strength to the feeble and keeping in the path those who are inclined to stray, it is obvious that the material gains of science, great as they may be, do not make up all the good which science brings or may bring to man. We especially, perhaps, in these later days, through the rapid development of the physical sciences, are too apt to dwell on the material gains alone. As a child in its infancy looks upon its mother only as a giver of good things, and does not learn till in after days how she was also showing her love by carefully training it in the way it should go, so we, too, have thought too much of the gifts of science, overlooking her power to guide.

Man does not live by bread alone, and science brings him more than bread. It is a great thing to make two blades of grass grow where before one alone grew; but it is no less great a thing to help a man to come to a just conclusion on the questions with which he has to deal. We may claim for science that while she is doing the one she may be so used as to do the other also. The dictum just quoted, that science is organised common sense, may be read as meaning that the common problems of life which common people have to solve are to be solved by the same methods by which the man of science solves his special problems. It follows that the training which does so much for him may be looked to as promising to do much for them. Such aid can come from science on two conditions only. In the first place, this her influence must be acknowledged; she must be duly recognised as a teacher no less than as a hewer of wood and a drawer of water. And the pursuit of science must be followed, not by the professional few only, but, at least in such measure as will ensure the influence of example, by the many. But this latter point I need not urge before this great Association, whose chief object during more than half a century has been to bring within the fold of science all who would answer to the call. In the second place, it must be understood that the training to be looked for from science is the outcome, not of the accumulation of scientific knowledge, but of the practice of scientific inquiry. Man may have at his fingers' ends all the accomplished results and all the current opinions of any one or of all the branches of science, and yet remain wholly unscientific in mind; but no one can have carried out even the humblest research without the spirit of science in some measure resting upon him. And that spirit may in part be caught even without entering upon an actual investigation in search of a new truth. The learner may be led to old truths, even the oldest, in more ways than one. He may be brought abruptly to a truth in its finished form, coming straight to it like a thief climbing over the wall; and the hurry and press of modern life tempt many to adopt this quicker way. Or he may be more slowly guided along the path by which the truth was reached by him who first laid hold of it. It is by this latter way of learning the truth, and by this alone, that the learner may hope to catch something at least of the spirit of the scientific inquirer.

This is not the place, nor have I the wish, to plunge into the turmoil of controversy; but, if there be any truth in what I have been urging, then they are wrong who think that in the schooling of the young science can be used with profit only to train those for whom science will be the means of earning their bread. It may be that from the point of view of the pedagogic art the experience of generations has fashioned out of the older studies of literature an instrument of discipline of unusual power, and that the teaching of science is as yet but a rough tool in unpractised hands. That, however, is not an adequate reason why scope should not be given for science to show the value which we claim for it as an intellectual training fitted for all sorts and conditions of men. Nor need the studies of humanity and literature fear her presence in the schools, for if her friends maintain that that teaching is one-sided, and therefore misleading, which deals with the doings of man only, and is silent about the works of nature, in the sight of which he and his doings shrink almost to nothing, she herself would be the first to admit that that teaching is equally wrong which deals only with the works of nature and says nothing about the doings of man, who is, to us at least, nature's centre.

There is yet another general aspect of science on which I would crave leave to say a word. In that broad field of human life which we call politics, in the struggle, not of man with man, but of race with race, science works for good. If we look only on the surface it may at first sight seem otherwise. In no branch of science has there during these later years been greater activity and more rapid progress than in that which furnishes the means by which man brings death, suffering, and disaster on his fellow-men. If the healer can look with pride on the increased power which science has given him to alleviate human suffering and ward off the miseries of disease, the destroyer can look with still greater pride on the power which science has given him to sweep away lives and to work desolation and ruin; while the one has slowly been learning to save units, the other has quickly learnt to slay thousands. But, happily, the very greatness of the modern power of destruction is already becoming a bar to its use, and bids fair—may we hope before long?—wholly to put an end to it; in the words of Tacitus, though in another sense, the very preparations for war, through the character which science gives them, make for peace.

Moreover, not in one branch of science only, but in all, there is a deep undercurrent of influence sapping the very foundations of all war. As I have already urged, no feature of scientific inquiry is more marked than the dependence of each step forward on other steps which have been made before. The man of science cannot sit by himself in his own cave weaving out results by his own efforts, unaided by others, heedless of what others have done and are doing. He is but a bit of a great system, a joint in a great machine, and he can only work aright when he is in due touch with his fellow-workers. If his labour is to be what it ought to be, and is to have the weight which it ought to have, he must know what is being done, not by himself, but by others, and by others not of his own land and speaking his tongue only, but also of other lands and of other speech. Hence it comes about that to the man of science the barriers of manners and of speech which pen men into nations become more and more unreal and indistinct. He recognises his fellow-worker, wherever he may live and whatever tongue he may speak, as one who is pushing forward shoulder to shoulder with him towards a common goal, as one whom he is helping and who is helping him. The touch of science makes the whole world kin.

The history of the past gives us many examples of this brotherhood of science. In the revival of learning throughout the sixteenth and seventeenth centuries, and some way on into the eighteenth century, the common use of the Latin tongue made intercourse easy. In some respects, in those earlier days science was more cosmopolitan than it afterwards became. In spite of the difficulties and hardships of travel, the men of science of different lands again and again met each other face to face, heard with their ears, and saw with their eyes what their brethren had to say or to show. The Englishman took the long journey to Italy to study there; the Italian, the Frenchman, and the German wandered from one seat of learning to another; and many a man held a chair in a country not his own. There was help, too, as well as intercourse. The Royal Society of London took upon itself the task of publishing nearly all the works of the great Italian Malpighi, and the brilliant Lavoisier, two years before his own countrymen in their blind fury slew him, received from the same body the highest token which it could give of its esteem.

In these closing years of the nineteenth century this great need of mutual knowledge and of common action felt by men of science of different lands is being manifested in a special way. Though nowadays what is done anywhere is soon known everywhere, the news of a discovery being often flashed over the globe by telegraph, there is an increasing activity in the direction of organisation to promote international meetings and international co-operation. In almost every science inquirers from many lands now gather together at stated intervals in international congresses to discuss matters which they have in common at heart, and go away each one feeling strengthened by having met his brother. The desire that in the struggle to lay bare the secrets of Nature the least waste of human energy should be incurred is leading more and more to the concerted action of nations combining to attack problems the solution of which is difficult and costly. The determination of standards of measurement, magnetic surveys, the solution of great geodetic problems, the mapping of the heavens and of the earth—all these are being carried on by international organisations.

In this and in other countries men's minds have this long while past been greatly moved by the desire to make fresh efforts to pierce the dark secrets of the forbidding Antarctic regions. Belgium has just made a brave single-handed attempt; a private enterprise sailing from these shores is struggling there now, lost for the present to our view; and this year we in England and our brethren in Germany are, thanks to the promised aid of the respective Governments, and no less to private liberality, in which this Association takes its share, able to begin the preparation of carefully organised expeditions. That international amity of which I am speaking is illustrated by the fact that in this country and in that there is not only a great desire, but a firm purpose, to secure the fullest co-operation between the expeditions which will leave the two shores. If in this momentous attempt any rivalry be shown between the two nations, it will be for each a rivalry, not in forestalling, but in assisting the other. May I add that if the story of the past may seem to give our nation some claim to the seas as more peculiarly our own, that claim bespeaks a duty likewise peculiarly our own to leave no effort untried by which we may plumb the seas' yet unknown depths and trace their yet unknown shores? That claim, if it means anything, means that when nations are joining hands in the dangerous work of exploring the unknown South, the larger burden of the task should fall to Britain's share; it means that we in this country should see to it, and see to it at once, that the concerted Antarctic expedition, which in some two years or so will leave the shores of Germany, of England, and, perhaps, of other lands, should, so far as we are concerned, be so equipped and so sustained that the risk of failure and disaster may be made as small, and the hope of being able, not merely to snatch a hurried glimpse of lands not yet seen, but to gather in with full hands a rich harvest of the facts which men not of one science only, but of many, long to know, as great as possible.

Another international scientific effort demands a word of notice. The need which every inquirer in science feels to know, and to know quickly, what his fellow-worker, wherever on the globe he may be carrying on his work or making known his results, has done or is doing, led some four years back to a proposal for carrying out by international co-operation a complete current index, issued promptly, of the scientific literature of the world. Though much labour in many lands has been spent upon the undertaking, the project is not yet an accomplished fact. Nor can this, perhaps, be wondered at, when the difficulties of the task are weighed. Difficulties of language, difficulties of driving in one team all the several sciences, which, like young horses, wish each to have its head free with leave to go its own way, difficulties mechanical and financial of press and post, difficulties raised by existing interests—these and yet other difficulties are obstacles not easy to be overcome. The most striking and the most encouraging features of the deliberations which have now been going on for three years have been the repeated expressions, coming not from this or that quarter only, but from almost all quarters, of an earnest desire that the effort should succeed, of a sincere belief in the good of international co-operation, and of a willingness to sink as far as possible individual interests for the sake of the common cause. In the face of such a spirit we may surely hope that the many difficulties will ultimately pass out of sight.

Perhaps, however, not the least notable fact of international co-operation in science is the proposal which has been made within the last two years that the leading academies of the world should, by representatives, meet at intervals to discuss questions in which the learned of all lands are interested. A month hence a preliminary meeting of this kind will be held at Wiesbaden; and it is at least probable that the closing year of that nineteenth century in which science has played so great a part may at Paris during the great World's Fair—which every friend, not of science only, but of humanity, trusts may not be put aside or even injured through any untoward event, and which promises to be an occasion, not of pleasurable sight-seeing only, but also, by its many international congresses, of international communing in the search for truth—witness the first select Witenagemote of the science of the world.

I make no apology for having thus touched on international co-operation. I should have been wanting, had I not done so, to the memorable occasion of this meeting. A hundred years ago two great nations were grappling with each other in a fierce struggle, which had lasted, with pauses, for many years, and was to last for many years to come; war was on every lip and in almost every heart. To-day this meeting has, by a common wish, been so arranged that those two nations should, in the persons of their men of science draw as near together as they can, with nothing but the narrow streak of the Channel between them, in order that they may take counsel together on matters in which they have one interest and a common hope. May we not look upon this brotherly meeting as one of many signs that science, though she works in a silent manner and in ways unseen by many, is steadily making for peace?

Looking back, then, in this last year of the eighteen hundreds, on the century which is drawing to its close, while we may see in the history of scientific inquiry much which, telling the man of science of his shortcomings and his weakness, bids him be humble, we see also much, perhaps more, which gives him hope. Hope is indeed one of the watchwords of science. In the latter-day writings of some who know not science, much may be read which shows that the writer is losing or has lost hope in the future of mankind. There are not a few of these; their repeated utterances make a sign of the times. Seeing in matters lying outside science few marks of progress and many tokens of decline or of decay, recognising in science its material benefits only, such men have thoughts of despair when they look forward to the times to come. But if there be any truth in what I have attempted to urge to-night, if the intellectual, if the moral influences of science are no less marked than her material benefits, if, moreover, that which she has done is but the earnest of that which she shall do, such men may pluck up courage and gather strength by laying hold of her garment. We men of science at least need not share their views or their fears. Our feet are set, not on the shifting sands of the opinions and of the fancies of the day, but on a solid foundation of verified truth, which by the labours of each succeeding age is made broader and more firm. To us the past is a thing to look back upon, not with regret, not as something which has been lost, never to be regained, but with content, as something whose influence is with us still, helping us on our further way. With us, indeed, the past points not to itself, but to the future; the golden age is in front of us, not behind us; that which we do know is a lamp whose brightest beams are shed into the unknown before us, showing us how much there is ahead and lighting up the way to reach it. We are confident in the advance because, as each one of us feels that any step forward which he may make is not ordered by himself alone and is not the result of his own sole efforts in the present, but is, and that in large measure, the outcome of the labours of others in the past, so each one of us has the sure and certain hope that as the past has helped him, so his efforts, be they great or be they small, will be a help to those to come.

SECTION A.

MATHEMATICS AND PHYSICS.

OPENING ADDRESS BY PROF. J. H. POYNTING, F.R.S., PRESIDENT OF THE SECTION.

THE members of this Section will, I am sure, desire me to give expression to the gratification that we all feel in the realisation of the scheme first proposed from this chair by Dr. Lodge, the scheme for the establishment of a National Physical Laboratory. It would be useless here to attempt to point out the importance of the step taken in the definite foundation of the Laboratory, for we all recognise that it was absolutely necessary for the due progress of physical research in this country. It is matter for congratulation that the initial guidance of the work of the Laboratory has been placed in such able hands.

While the investigation of nature is ever increasing our knowledge, and while each new discovery is a positive addition never again to be lost, the range of the investigation and the nature of the knowledge gained form the theme of endless discussion. And in this discussion, so different are the views of different schools of thought, that it might appear hopeless to look for general agreement, or to attempt to mark progress.

Nevertheless, I believe that in some directions there has been real progress, and that physicists, at least, are tending towards a general agreement as to the nature of the laws in which they embody their discoveries, of the explanations which they seek to give, and of the hypotheses they make in their search for explanations.

I propose to ask you to consider the terms of this agreement, and the form in which, as it appears to me, they should be drawn up.

The range of the physicist's study consists in the visible motions and other sensible changes of matter. The experiences with which he deals are the impressions on his senses, and his aim is to describe in the shortest possible way how his various senses have been, will be, or would be affected.

His method consists in finding out all likenesses, in classing together all similar events, and so giving an account as concise as possible of the motions and changes observed. His success in the search for likenesses and his striving after conciseness of description lead him to imagine such a constitution of things that likenesses exist even where they elude his observation, and he is thus enabled to simplify his classification on the assumption that the constitution thus imagined is a reality. He is enabled to predict on the assumption that the likenesses of the future will be the likenesses of the past.

His account of nature, then, is, as it is often termed, a descriptive account.

Were there no similarities in events, our account of them could not rise above a mere directory, with each individual event entered up separately with its address. But the similarities observed enable us to class large numbers of events together, to give general descriptions, and indeed to make, instead of a directory, a readable book of science, with laws as the headings of the chapters.

These laws are, I believe, in all cases brief descriptions of observed similarities. By way of illustration let us take two or three examples.

The law of gravitation states that to each portion of matter we can assign a constant—its mass—such that there is an acceleration towards it of other matter proportional to that mass divided by the square of its distance away. Or all bodies resemble each other in having this acceleration towards each other.

Hooke's law for the case of a stretched wire states that each successive equal small load produces an equal stretch, or states that the behaviour of the wire is similar for all equal small pulls.

Joule's law for the heat appearing when a current flows in a wire states that the rate of heat development is proportional to the square of the current multiplied by the resistance, or states that all the different cases resemble each other in having $H \div C^2Rt$ constant.

And, generally, when a law is expressed by an equation, that equation is a statement that two different sets of measurements are made, represented by the terms on the two sides of the equation, and that all the different cases resemble each other in that the two sets have the constant relation expressed by the equation. Accurate prediction is based on the assumption that when we have made the measurements on the one side of the equation we can tell the result of the measurements implied on the other side.

If this is a true account of the nature of physical laws, they have, we must confess, greatly fallen off in dignity. No long time ago they were quite commonly described as the Fixed Laws of Nature, and were supposed sufficient in themselves to govern the universe. Now we can only assign to them the humble rank of mere descriptions, often tentative, often erroneous, of similarities which we believe we have observed.

The old conception of laws as self-sufficing governors of nature was, no doubt, a survival of a much older conception of the scope of physical science, a mode of regarding physical phenomena which had itself passed away.

I imagine that originally man looked on himself and the result of his action in the motions and changes which he produced in matter, as the one type in terms of which he should seek to describe all motions and changes. Knowing that his purpose and will were followed by motions and changes in the matter about him, he thought of similar purpose and will behind all the motions and changes which he observed, however they occurred; and he believed, too, that it was necessary to think thus in giving any consistent account of his observations. Taking this anthropomorphic—or, shall we say, psychical—view, the laws he formulated were not merely descriptions of similarities of behaviour, but they were also expressions of fixed purpose and the resulting constancy of action. They were commands given to matter which it must obey.

The psychical method, the introduction of purpose and will, is still appropriate when we are concerned with living beings. Indeed, it is the only method which we attempt to follow when we are describing the motions of our fellow-creatures. No one seeks to describe the motions and actions of himself and of his fellow-men, and to classify them without any reference to the similarity of purpose when the actions are similar. But as the study of nature progressed, it was found to be quite futile to bring in the ideas of purpose and will when merely describing and classifying the motions and changes of non-living matter. Purpose and will could be entirely left out of sight, and yet the observed motions and changes could be described, and predictions could be made as to future motions and changes. Limiting the aim of physical science to such description and prediction, it gradually became clear that the method was adequate for the purpose, and over the range of non-living matter, at least, the psychical yielded to the physical. Laws ceased to be commands analogous to legal enactments, and became mere descriptions. But during the passage from one position to the other, by a confusion of thought which may appear strange to us now that we have finished the journey, though no doubt it was inevitable, the purpose and will of which the laws had been the expression were put into the laws themselves; they were personified and made to will and act.

Even now these early stages in the history of thought can be traced by survivals in our language, survivals due to the ascription of moral qualities to matter. Thus gases are still sometimes said to obey or to disobey Boyle's Law as if it were an enactment for their guidance, and as if it set forth an ideal, the perfect gas, for their imitation. We still hear language which seems to imply that real gases are wanting in perfection, in that they fail to observe the exact letter of the law. I suppose on this view we should have to say that hydrogen is nearest to perfection; but then we should have to regard it as righteous over-much, a sort of Pharisee among gases which overshoots the mark in its endeavour to obey the law. Oxygen and nitrogen we may regard as good enough in the affairs of everyday life. But carbon dioxide and chlorine and the like are poor sinners which yield to temptation and liquefy whenever circumstances press at all hardly on them.

There is a similar ascription of moral qualities when we judge bodies according to their fulfilment of the purpose for which we use them, when we describe them as good or bad radiators, good or bad insulators, as if it were a duty on their part to radiate well, or insulate well, and as if there were failures on the part of nature to come up to the proper standard.

These are of course mere trivialities, but the reaction of language on thought is so subtle and far-reaching that, risking the accusation of pedantry, I would urge the abolition of all such picturesque terms. In our quantitative estimates let us be content with "high" or "low," "great" or "small," and let us remember that there is no such thing as a failure to obey a physical law. A broken law is merely a false description.

Concurrently with the change in our conception of physical law has come a change in our conception of physical explanation. We have not to go very far back to find such a statement as this—that we have explained anything when we know the cause of it, or when we have found out the reason why—a statement which is only appropriate on the psychical view. Without entering into any discussion of the meaning of cause, we can at least assert that that meaning will only have true content when it is concerned with purpose and will. On the purely physical or descriptive view, the idea of cause is quite out of place. In description we are solely concerned with the "how" of things, and their "why" we purposely leave out of account. We explain an event, not when we know "why" it happened. but when we show "how" it is like something else happening elsewhere or otherwhen—when, in fact, we can include it as a case described by some law already set forth. In explanation, we do not account *for* the event, but we improve our account *of* it by likening it to what we already knew.

For instance, Newton explained the falling of a stone when he showed that its acceleration towards the earth was similar to and could be expressed by the same law as the acceleration of the moon towards the earth.

He explained the air disturbance we call "sound" when he showed that the motions and forces in the pressure waves were like motions and forces already studied.

Franklin explained lightning when and so far as he showed that it was similar in its behaviour to other electric discharges.

Here I do not fear any accusation of pedantry in joining those who urge that we should adapt our language to the modern view. It would be a very real gain, a great assistance to clear thinking, if we could entirely abolish the word "cause," in physical description, cease to say "why" things happen unless we wish to signify an antecedent purpose, and be content to own that our laws are but expressions of "how" they occur.

The aim of explanation, then, is to reduce the number of laws as far as possible, by showing that laws, at first separated, may be merged in one; to reduce the number of chapters in the book of science by showing that some are truly mere sub-sections of chapters already written.

To take an old but never-worn-out metaphor, the physicist is examining the garment of nature, learning of how many, or rather of how few, different kinds of thread it is woven, finding how each separate thread enters into the pattern, and seeking from the pattern woven in the past to know the pattern yet to come.

How many different kinds of thread does nature use?

So far, we have recognised some eight or nine, the number of different forms of energy which we are still obliged to count as distinct. But this distinction we cannot believe to be real. The relations between the different forms of energy, and the fixed rate of exchange when one form gives place to another, encourage us to suppose that if we could only sharpen our senses, or change our point of view, we could effect a still further reduction. We stand in front of nature's loom as we watch the weaving of the garment; while we follow a particular thread in the pattern it suddenly disappears, and a thread of another colour takes its place. Is this a new thread, or is it merely the old thread turned round and presenting a new face to us? We can do little more than guess. We cannot get to the other side of the pattern, and our minutest watching will not tell us all the working of the loom.

Leaving the metaphor, were we true physicists, and physicists alone, we should, I suppose, be content to describe merely what we observe in the changes of energy. We should say, for instance, that so much kinetic energy ceases, and that so much heat appears, or that so much light comes to a surface, and that so much chemical energy takes its place. But we have to take ourselves as we are, and reckon with the fact that though our material is physical, we ourselves are psychical. And, as a mere matter of fact, we are not content with such discontinuous descriptions. We dislike the discontinuity and we think of an underlying identity. We think of the heat as being that which a moment before was energy of visible motion, we think of the light as changing its form alone and becoming itself the chemical energy. Then to our passive dislike to discontinuity we join our active desire to form a mental picture of what may be going on, a picture like something which we already know. Coming on these discontinuities our ordinary method of explanation fails, for they are not obviously like those series of events in which we can trace every step. We then imagine a constitution of matter and modifications of it corresponding to the different kinds of energy, such that the discontinuities vanish, and such that we can picture one form of energy passing into another and yet keeping the same in kind throughout. We are no longer content to describe what we actually see or feel, but we describe what we imagine we should see or feel if our senses were on quite another scale of magnitude and sensibility. We cease to be physicists of the real and become physicists of the ideal.

To form such mental pictures we naturally choose the sense which makes such pictures most definite, the sense of sight, and think of a constitution of matter which shall enable us to explain all the various changes in terms of visible motions and accelerations. We imagine a mechanical constitution of the universe.

We are encouraged in this attempt by the fact that the relations in this mechanical conception can be so exactly stated, that the equations of motion are so very definite. We have, too, examples of mechanical systems, of which we can give accounts far exceeding in accuracy the accounts of other physical systems. Compare, for instance, the accuracy with which we can describe and foretell the path of a planet with our ignorance of the movements of the atmosphere as dependent on the heat of the sun. The planet keeps to the astronomer's time-table, but the wind still bloweth almost where it listeth.

The only foundation which has yet been imagined for this mechanical explanation—if we may use "explanation" to denote the likening of our imaginings to that which we actually observe—is the atomic and molecular hypothesis of matter. This hypothesis arose so early in the history of science that we are almost tempted to suppose that it is a necessity of thought, and that it has a warrant of some higher order than any other hypothesis which could be imagined. But I suspect that if we could trace its early development we should find that it arose in an attempt to explain the phenomena of expansion and contraction, evaporation and solution. Were matter a continuum we should have to admit all these as simple facts, inexplicable in that they are like nothing else. But imagine matter to consist of a crowd of separate particles with interspaces. Contraction and expansion are then merely a drawing in and a widening out of the crowd. Solution is merely the mingling of two crowds, and evaporation merely a dispersal from the outskirts. The most evident properties of matter are then similar to what may be observed in any public meeting.

For ages the molecular hypothesis hardly went further than this. The first step onward was the ascription of vibratory motion to the atoms to explain heat. Then definite qualities were ascribed, definite mutual forces were called into play to explain elasticity and other properties or qualities of matter. But I imagine its first really great achievement was its success in explaining the law of combining proportions, and next to that we should put its success in explaining many of the properties of gases.

While light was regarded as corpuscular—in fact molecular, and while direct action at a distance presented no difficulty, the molecular hypothesis served as the one foundation for the mechanical representation of phenomena. But when it was shown that infinitely the best account of the phenomena of light could be given on the supposition that it consisted of waves, something was needed, as Lord Salisbury has said, to wave, both in the interstellar and in the intermolecular spaces. So the hypothesis of an ether was developed, a necessary complement of that form of the molecular hypothesis in which matter consists of discrete particles with matter-free intervening spaces.

Then Faraday's discovery of the influence of the dielectric medium in electric actions led to the general abandonment of the idea of action at a distance, and the ether was called in to aid matter in the explanation of electric and magnetic phenomena. The discovery that the velocity of electro-magnetic waves is the same as that of light-waves is at least circumstantial evidence that the same medium transmits both.

I suppose we all hope that some time we shall succeed in attributing to this medium such further qualities that it will be able to enlarge its scope and take in the work of gravitation.

The mechanical hypothesis has not always taken this dualistic form of material atoms and molecules, floating in a quite distinct ether. I think we may regard Boscovich's theory of point-centres surrounded by infinitely extending atmospheres of force as really an attempt to get rid of the dualism, and Faraday's theory of point centres with radiating lines of force is only Boscovich's theory in another form. But Lord Kelvin's vortex-atom theory gives us a simplification more easily thought of. Here all space is filled with continuous fluid—shall we say a fluid ether?—and the atoms are mere loci of a particular type of motion of this frictionless fluid. The sole differences in the atoms are differences of position and motion. Where there are whirls, we call the fluid matter; where there are no whirls, we call it ether. All energy is energy of motion. Our visible kinetic energy, $MV^2/2$, is energy in and round the central whirls; our visible energy of position, our potential energy, is energy of motion in the outlying regions.

A similar simplification is given by Dr. Larmor's hypothesis, in which, again, all space is filled with continuous substance all of one kind, but this time solid rather than fluid. The atoms are loci of strain instead of whirls, and the ether is that which is strained.

So, as we watch the weaving of the garment of nature, we resolve it in imagination into threads of ether spangled over with beads of matter. We look still closer, and the beads of matter vanish; they are mere knots and loops in the threads of ether.

The question now faces us—How are we to regard these hypotheses as to the constitution of matter and the connecting ether? How are we to look upon the explanations they afford? Are we to put atoms and ether on an equal footing with the phenomena observed by our senses, as truths to be investigated for their own sake? Or are they mere tools in the search for truth, liable to be worn out or superseded?

That matter is grained in structure is hardly more than an expression of the fact that in very thin layers it ceases to behave as in thicker layers. But when we pass on from this general statement and give definite form to the granules, or assume definite qualities to the intergranular cement, we are dealing with pure hypotheses.

It is hardly possible to think that we shall ever see an atom or handle the ether. We make no attempt whatever to render them evident to the senses. We connect observed conditions and changes in gross visible matter by invisible molecular and ethereal machinery. The changes at each end of the machinery of which we seek to give an account are in gross matter, and this gross matter is our only instrument of detection, and we never receive direct sense-impressions of the imagined atoms or the intervening ether. To strictly descriptive physicists their only use and interest would lie in their service in prediction of the changes which are to take place in gross matter.

It appears quite possible that various types of machinery might be devised to produce the known effects. The type we have adopted is undergoing constant minor changes, as new discoveries suggest new arrangements of the parts. Is it utterly beyond possibility that the type itself should change?

The special molecular and ethereal machinery which we have designed, and which we now generally use, has been designed because our most highly developed sense is our sense of sight. Were we otherwise, had we a sense more delicate than sight, one affording us material for more definite mental presentation, we might quite possibly have constructed very different hypotheses. Though, as we are, we cannot conceive any higher type than that founded on the sense of sight, we can imagine a lower type, and by way of illustration of the point let us take the sense of which my predecessor spoke last year—the sense of smell. In us it is very undeveloped. But let us imagine a being in whom it is highly cultivated, say a very intellectual and very hypothetical dog. Let us suppose that he tries to frame an hypothesis as to light. Having found that his sense of smell is excited by surface exhalations, will he not naturally make and be content with a corpuscular theory of light? When he has discovered the facts of dispersion, will he not think of the different colours as different kinds of smell—insensible, perhaps, to him, but sensible to a still more highly gifted, still more hypothetical, dog?

Of course, with our superior intellect and sensibility, we can see where his hypothesis would break down; but unless we are to assume that we have reached finality in sense development, the illustration, grotesque as it may be, will serve to show that our hypotheses are in terms of ourselves rather than in terms of nature itself, they are ejective rather than objective, and so they are to be regarded as instruments, tools, apparatus only to aid us in the search for truth.

To use an old analogy—and here we can hardly go except upon analogy—while the building of nature is growing spontaneously from within, the model of it, which we seek to construct in our descriptive science, can only be constructed by means of scaffolding from without, a scaffolding of hypotheses. While in the real building all is continuous, in our model there are detached parts which must be connected with the rest by temporary ladders and passages, or which must be supported till we can see how to fill in the understructure. To give the hypotheses equal validity with facts is to confuse the temporary scaffolding with the building itself.

But even if we take this view of the temporary nature of our molecular and ethereal imaginings, it does not lessen their value, their necessity to us.

It is merely a true description of ourselves to say that we must believe in the continuity of physical processes, and that we must attempt to form mental pictures of those processes the details of which elude our observation. For such pictures we must frame hypotheses, and we have to use the best material at command in framing them. At present there is only one fundamental hypothesis—the molecular and ethereal hypothesis—in some such form as is generally accepted.

Even if we take the position that the form of the hypothesis may change as our knowledge extends, that we may be able to devise new machinery—nay, even that we may be able to design some quite new type to bring about the same ends—that does not appear to me to lessen the present value of the hypothesis. We can recognise to the full how well it enables us to group together large masses of facts which, without it, would be scattered apart, how it serves to give working explanations, and continually enables investigators to think out new questions for research. We can recognise that it is the symbolical form in which much actual knowledge is cast. We might almost as well quarrel with the use of the letters of the alphabet, inasmuch as they are not the sounds themselves, but mere arbitrary symbols of the sounds.

In this country there is no need for any defence of the use of the molecular hypothesis. But abroad the movement from the position in which hypothesis is confounded with observed truth has carried many through the position of equilibrium equally far on the other side, and a party has been formed which totally abstains from molecules as a protest against immoderate indulgence in their use. Time will show whether these protesters can do without any hypothesis, whether they can build without scaffolding or ladders. I fear that it is only an attempt to build from balloons.

But the protest will have value if it will put us on our guard against using molecules and the ether everywhere and everywhere. There is, I think, some danger that we may get so accustomed to picturing everything in terms of these hypotheses that we may come to suppose that we have no firm basis for the facts of observation until we have given a molecular account of them, that a molecular basis is a firmer foundation than direct experience.

Let me illustrate this kind of danger. The phenomena of capillarity can, for the most part, be explained on the assumption of a liquid surface tension. But if the subject is treated merely from this point of view, it stands alone—it is a portion of the building of science hanging in the air. The molecular hypothesis then comes in to give some explanation of the surface tension, gives, as it were, a supporting understructure connecting capillarity with other classes of phenomena. But here, I think, the hypothesis should stop, and such phenomena as can be explained by the surface tension should be so explained without reference to molecules. They should not be brought in again till the surface-tension explanation fails. It is necessary to bear in mind what part is scaffolding, and what is the building itself, already firm and complete.

Or, as another illustration, take the Second Law of Thermodynamics. I suspect that it is sometimes supposed that a molecular theory from which the Second Law could be deduced would be a better basis for it than the direct experience on which it was founded by Clausius and Kelvin, or that the mere imagining of a Maxwell's sorting demon has already disproved the universality of the law; whereas he is a mere hypothesis grafted on a hypothesis, and nothing corresponding to his action has yet been found.

There is more serious danger of confusion of hypothesis with fact in the use of the ether: more risk of failure to see what is accomplished by its aid. In giving an account of light, for instance, the right course, it appears to me, is to describe the phenomena and lay down the laws under which they are grouped, leaving it an open question what it is that waves, until the phenomena oblige us to introduce something more than matter, until we see what properties we must assign to the ether, properties not possessed by matter, in order that it may be competent to afford the explanations we seek. We should then realise more clearly that it is the constitution of matter which we have imagined, the hypothesis of discrete particles which obliges us to assume an intervening medium to carry on the disturbance from particle to particle. But the vortex-atom hypothesis and Dr. Larmor's strain-atom hypothesis both seem to indicate that we are moving in the direction of the abolition of the distinction between matter and ether, that we shall come to regard the luminiferous medium, not as an attenuated substance here and there encumbered with detached blocks—the molecules of matter—but as something which in certain places exhibits modifications which we term matter. Or starting rather from matter, we may come to think of matter as no longer consisting of separated granules, but as a continuum with properties grouped round the centres, which we regard as atoms or molecules.

Perhaps I may illustrate the danger in the use of the conception of the ether by considering the common way of describing the electro-magnetic waves, which are all about us here, as ether waves. Now in all cases with which we are acquainted, these waves start from matter; their energy before starting was, as far as we can guess, energy of the matter between the different parts of the source, and they manifest themselves in the receiver as energy of matter. As they travel through the air, I believe that it is quite possible that the electric energy can be expressed in terms of the molecules of air in their path, that they are effecting atomic separations as they go. If so, then the air is quite as much concerned in their propagation as the ether between its molecules. In any case, to term them ether waves is to prejudge the question before we have sufficient evidence.

Unless we bear in mind the hypothetical character of our mechanical conception of things, we may run some risk of another danger—the danger of supposing that we have something more real in mechanical than in other measurements. For instance, there is some risk that the work measure of specific heat should be regarded as more fundamental than the heat measure, in that heat is truly a "mode of motion." On the molecular hypothesis, heat is no doubt a mixture of kinetic energy and potential energy of the molecules and their constituents, and may even be entirely kinetic energy; and we may conceivably in the future make the hypothesis so definite that, when we heat a gramme of water 1°, we can assign such a fraction of an erg to each atom. But look how much pure hypothesis is here. The real superiority of the work measure of specific heat lies in the fact that it is independent of any particular substance, and there is nothing whatever hypothetical about it.[1]

[1] This risk of imagining one particular kind of measure more real than another, more in accordance with the truth of things, may be further illustrated by the common idea that mass-acceleration is the only way to measure a force. We stand apart from our mechanical system and watch the motions and the accelerations of the various parts, and we find that mass-accelerations have a certain significance in our system. If we keep ourselves outside the system and only use our sense of sight, then mass-acceleration is the only way of describing that behaviour of one body in the presence of others which we term force on it. But if we go about in the system and pull and push bodies, we find that there is another conception of force, in which another sense than sight is concerned—another mode

Another illustration of the illegitimate use of our hypothesis, as it appears to me, is in the attempt to find in the ether a fixed datum for the measurement of material velocities and accelerations, a something in which we can draw our coordinate axes so that they will never turn or bend. But this is as if, discontented with the movement of the earth's pole, we should seek to find our zero lines of latitude and longitude in the Atlantic Ocean. Leaving out of sight the possibility of ethereal currents which we cannot detect, and the motions due to every ray of light which traverses space, we could only fix positions and directions in the ether by buoying them with matter. We know nothing of the ether, except by its effects on matter, and, after all, it would be the material buoys which would fix the positions and not the ether in which they float.

The discussion of the physical method, with its descriptive laws and explanations, and its hypothetical extension of description, leads us on to the consideration of the limitation of its range. The method was developed in the study of matter which we describe as non-living, and with non-living matter the method has sufficed for the particular purposes of the physicist. Of course only a little corner of the universe has been explored, but in the study of non-living matter we have come to no impassable gulfs, no chasms across which we cannot throw bridges of hypothesis. Does the method equally suffice when it is applied to living matter? Can we give a purely physical account of such matter, likening its motions and changes to other motions and changes already observed, and so explaining them? Can we group them in laws which will enable us to predict future conditions and positions? The ancient question never answered, but never ceasing to press for an answer.

Having faith in our descriptive method, let us use it to describe our real attitude on the question. Do we, or do we not, as a matter of fact, make any attempt to apply the physical method to describe and explain those motions of matter which on the psychical view we term voluntary?

Any commonplace example, and the more commonplace the more is it to the point, will at once tell us our practice, whatever may be our theory. For instance, a steamer is going across the Channel. We can give a fairly good physical account of the motion of the steamer. We can describe how the energy stored in the coal passes out through the boiler into the machinery, and how it is ultimately absorbed by the sea. And the machinery once started, we can give an account of the actions and reactions between its various parts and the water, and if only the crew will not interfere, we can predict with some approach to correctness how the vessel will run. All these processes can be likened to processes already studied—perhaps on another scale—in our laboratories, and from the similarities prediction is possible. But now think of a passenger on board who has received an invitation to take the journey. It is simply a matter of fact that we make no attempt at a complete physical account and explanation of those actions which he takes to accomplish his purpose. We trace no lines of induction in the ether connecting him with his friends acrcss the Channel, we seek no law of force under which he moves In practice the strictest physicist abandons the physical view, and replaces it by the psychical. He admits the study of purpose as well as the study of motion.

He has to admit that here his physical method of prediction fails. In physical observations one set of measurements may lead to the prediction of the results of another set of measurements. The equations expressing the laws imply different observations with some definite relation between their results, and if we know one set of observations and that definite relation we can predict the result of the other set. But if we take the psychical view of actions, we can only measure the actions. We have no independent means of studying and measuring the motions which preceded the actions, we can only estimate their value by the consequent actions. If we formed equations, they would be mere identities with the same terms on either side.

The consistent and persistent physicist, finding the door closed against him, finding that he has hardly a sphere of influence left to him in the psychical region, seeks to apply his methods in another way by assuming that if he knew all about the molecular positions and motions in the living matter, then the ordinary physical laws could be applied and the physical conditions at any future time could be predicted. He would say, I suppose, with regard to the Channel passenger, that it is absurd to begin with the most complicated mechanism, and seek to give a physical account of that. He would urge that we should take some lower form of life where the structure and motions are simpler, and apply the physical methods to that.

of measurement much more ancient and still far more extensively used —the measurement by weight supported. Each method has its own range; each is fundamental in that range. It is one of the great practical problems n physics to make the pendulum give us the exact ratio of the units in the wo systems.

Well, then, let us look for the physical explanation of any motion which we are entitled from its likeness to our own action to call a voluntary motion. Must we not own that even the very beginning of such explanation is as yet non-existent? It appears to me that the assumption that our methods do apply, and that purely physical explanation will suffice to predict all motions and changes, voluntary and involuntary, is at present simply a gigantic extra-polation, which we should unhesitatingly reject if it were merely a case of ordinary physical investigation. The physicist when thus extending his range is ceasing to be a physicist, ceasing to be content with his descriptive methods in his intense desire to show that he is a physicist throughout.

Of course we may describe the motions and changes of any type of matter after the event, and in a purely physical manner. And as Prof. Ward has suggested, in a most important contribution to this subject which he has made in his recently published "Gifford Lectures" ("Naturalism and Agnosticism," *The Gifford Lectures*, 1896–98, vol. ii. p. 71), where ordinary physical explanations fail to give an account of the motions, we might imagine some structure in the ether, and such stresses between the ether and matter that our physical explanations should still hold. But, as Prof. Ward says, such ethereal constructions would present no warrant for their reality or consistency. Indeed they would be mere images in the surface of things to account for what goes on in front of the surface, and would have no more reality than the images of objects in a glass.

If we have full confidence in the descriptive method, as applied to living and non-living matter, it appears to me that up to the present it teaches us that while in non-living matter we can always find similarities, that, while each event is like other events, actual or imagined, in a living being there are always dissimilarities. Taking the psychical view—the only view which we really do at present take—in the living being there is always some individuality, something different from any other living being, and full prediction in the physical sense, and by physical methods, is impossible. If this be true, the loom of nature is weaving a pattern with no mere geometrical design. The threads of life, coming in we know not where, now twining together, now dividing, are weaving patterns of their own, ever increasing in intricacy, ever gaining in beauty.

SECTION B.

CHEMISTRY.

OPENING ADDRESS BY DR. HORACE T. BROWN, F.R.S., PRESIDENT OF THE SECTION.

THE subject which I have chosen for my Address is the fixation of carbon by plants, one which is the common meeting ground of chemistry, physics and biology. I must, however, confine myself only to certain aspects of the question, since it is manifestly impossible to fully discuss the whole of a subject of such magnitude and importance within the time at my disposal.

We have become so accustomed to the idea that the higher plants derive *the whole* of their carbon from atmospheric sources that we are apt to forget how very indirect is the nature of much of the experimental evidence on which this belief is founded. There can, of course, be no doubt that the primary source of the organic carbon of the soil, and of the plants growing on it, is the atmosphere; but of late years there has been such an accumulation of evidence tending to show that the higher plants are capable of being nourished by the direct application of a great variety of ready-formed organic compounds, that we are justified in demanding further proof that the stores of organic substances in the soil must necessarily be oxidised down to the lowest possible point before their carbon is once more in a fit state to be assimilated.

It was the hope of gaining more direct evidence on this important question which led me some time ago to attack the problem experimentally in conjunction with Mr. F. Escombe, the resources of the Jodrell Laboratory at Kew having been kindly put at our disposal by Sir W. Thiselton-Dyer and Dr. D. H. Scott. Up to the present time our experiments have not been carried far enough to enable us to give a positive answer to the main question, but they have already suggested a new method of attack which will enable us in the future to determine, with a fair amount of certainty, whether any particular plant, growing under perfectly natural conditions, derives any appreciable portion of its carbon from any other source than the gaseous carbon dioxide of the atmosphere.

During the course of the inquiry, many interesting side issues have been raised which we believe to be of some importance in their bearing on the processes of plant nutrition, and it is to a consideration of these that I intend to devote the greater part of my Address.

I must, however, in the first place indulge in a little historical retrospect, and am the more tempted to do this, as far as the early pioneers in this branch of knowledge are concerned, since a critical study of their writings has shown me very clearly that the relative merits of some of these older workers, and the respective parts which they took in founding the true theory of assimilation, have in our own time been much misrepresented by more than one historian of science whose name carries great weight.

There is no chapter in the history of scientific discovery of greater abiding interest than that which was opened by Priestley in 1771, when he commenced his work on the influence of plants on the composition of the air around them. It has often been assumed that these experiments of Priestley, which were unquestionably the starting-point for all succeeding workers, were the result of some haphazard method of working, and of one of those happy chances to which he is in the habit of attributing some of his most important discoveries. However much the element of chance entered into some of his work, and in this respect I think Priestley often does himself injustice, the discovery of the amelioration of vitiated air by plants was certainly not a case of this kind. Of all his contemporaries belonging to the old school of chemistry, Priestley had the clearest conception of the processes of animal respiration and of their identity with the process of combustion. This is clearly shown by his "Observations on Respiration and the Use of the Blood," which he presented to the Royal Society in 1776 This memoir, written of course from the phlogistic point of view, only requires translating into the language of the newer chemistry to be an accurate statement of the main facts of animal respiration. We have it on Priestley's own authority that it was these studies which produced in his mind a conviction that there must be some provision in nature for dephlogisticating the air which was constantly being vitiated by the processes of respiration, combustion and putrefaction, and for rendering it once more fit for maintaining animal life. In his search for this compensating influence, which he justly regarded as one of the most important problems of natural philosophy, he made many attempts to bring back the vitiated air to its original state by agitating it with water, and by submitting it to the continued action of light and heat, and it was in the course of these systematic attempts that he was led to study the influence of plants in this direction.

It was in the month of August 1771 that he made the memorable experiments at Leeds of immersing sprigs of mint in air which had been vitiated by the burning of a candle or by animal respiration. To quote his own words, this observation led him "to conclude that plants, instead of affecting the air in the same manner with animal respiration, reverse the effects of breathing, and tend to keep the atmosphere sweet and wholesome when it is become noxious in consequence of animals either living or breathing, or dying and putrefying in it." That he was fully convinced that these observations, which he repeated and amplified in the following year, presented the true key to the problem, is sufficiently shown by another passage in which he says: "These proofs of the partial restoration of air by plants in a state of vegetation, though in a confined and unnatural situation, cannot but render it highly probable that the injury which is continually done to the atmosphere by the respiration of such a number of animals, and the putrefaction of such masses of both vegetable and animal matter, is, in part at least, repaired by the vegetable creation; and notwithstanding the prodigious mass of air that is corrupted daily by the above causes, yet if we consider the immense profusion of vegetables upon the face of the earth growing in places suited to their nature, and consequently at full liberty to exert all their powers, both inhaling and exhaling, it can hardly be thought but that it may be a sufficient counterbalance to it, and that the remedy is adequate to the evil."

Between the time of Priestley temporarily relinquishing his experiments in this direction in 1772 and his resumption of them in 1778, owing to the adverse criticism of Scheele and others, he had discovered dephlogisticated air or oxygen, and had elaborated his method for ascertaining the purity of air, or its richness in oxygen, by determining its diminution in volume after mixing with an excess of nitric oxide over water.[1] This method gave, of course, a much greater degree of precision to his results than was attainable in his earlier work, where the purity of the air at the end of an experiment was only determined by ascertaining if it would support the combustion of a candle or allow a small animal to live in it.

The results of his later work were published in 1779, and were not altogether confirmatory of those arrived at six years before. It is true that he generally found evidence of an evolution of oxygen by the plants, but occasionally the air was less "pure" at the end of an experiment than it was at the beginning, and this occurred in a sufficient number of cases to Dr. Priestley to doubt to some extent the accuracy of his previous conclusions. On the whole, however, he still thinks it *probable* that the vegetation of healthy plants has a salutary effect on the air in which they grow.

The reason for this want of complete consistency in these later experiments was, of course, his failure at that time to recognise the important influence of *light* in bringing about the evolution of oxygen, an explanation which was given shortly afterwards by Ingen-Housz.

Priestley's attention was now taken up with another observation, which led him within a very short distance indeed of the discovery that the evolution of oxygen by plants is conditioned, not only by a sufficient degree of illumination, but also by the pre-existence of carbon dioxide. It is the more necessary to treat of this point somewhat in detail, since it is a part of his work which has received but scanty justice at the hands of recent writers, who have apparently failed to see how much our modern conceptions of plant nutrition really owe to the initiative of Priestley. In his "History of Botany," Sachs deals very unfairly with Priestley in this respect, owing to a want of intimate knowledge of his writings, and to the lack of anything like perspective in estimating the relative merits of his contemporaries Ingen-Housz and Senebier, whose position can only be completely understood after a careful study of their numerous original memoirs, some of which are by no means readily accessible.

In the course of his experiments on plants partially immersed in water more or less fully impregnated with "fixed air," Priestley had observed a fact which had not escaped the notice of Bonnet at an earlier date, that bubbles of gas arose spontaneously from the leaves and stems, and it occurred to him that an examination of the nature of this gas by means of his new eudiometric process ought to settle the question whether plants really do contribute in any way to the purification of ordinary air. It was in June 1778 that he put this to the test, and he found that the air thus liberated was much richer in oxygen than ordinary air. On removing the plants, he found to his astonishment that the water in which they had been placed, and which had a considerable amount of "green matter" adhering to the sides of the phials, still continued to evolve a gas which increased in amount when the vessels were placed in sunlight. On testing this gas with his eudiometric process, he found that it consisted to a great extent of "dephlogisticated air" or oxygen; in fact, from the experimental results which he gives it is evident that the gas contained from 74 to 85 per cent. of oxygen. Having observed that the "green matter" appeared much more readily in pump water than in rain or river water, and knowing that pump water contained considerable amounts of "fixed air," he was led to make a series of experiments with water artificially impregnated with carbon dioxide, which left no doubt in his mind that the production of the "green matter," and the evolution of the dephlogisticated air were in some way due to the presence of "fixed air." Up to this point Priestley was following a path which seemed about to lead him to a complete solution of his previous difficulties. He had beyond all question succeeded in showing that the evolution of oxygen was not only dependent on the pre-existence of carbon dioxide, but that light was also required

1 Nitric oxide was discovered by Priestley in 1772, and was described by him under the name of "nitrous air."

for the process. It only wanted, in fact, the recognition of the vegetable nature of the alga which constituted his "green substance" to bring these observations into line with his previous work, and to complete a discovery which would have eclipsed in importance all the others with which Priestley's name is associated. It was just this one step which he most provokingly failed to take. It is true that he examined the "green substance" under the microscope, but owing to want of skill in the use of the instrument, and also to his defective eyesight, he was unable to determine its true nature, and unfortunately adopted the view that it had merely a mechanical action in separating the oxygen from the water, and, to use his own words, that "it was only a circumstance preceding the spontaneous emission of the air from water." He was, in fact, now inclined to regard the process as a purely chemical one, due to the direct action of light on the carbon dioxide dissolved in the water.

But this was by no means Priestley's final view, as shown by a further description of his experiments on plants set forth in the new edition of his works published in 1790, where he clearly recognised the error into which he had been led.[1] Meanwhile the subject had been taken up by two other observers, Ingen-Housz and Senebier, and in order to thoroughly understand the respective shares which these men took in advancing our knowledge of the assimilatory process, it is necessary to consult, not only their books, but also the numerous scattered memoirs which appeared at intervals between the years 1779 and 1800.

To Ingen-Housz must unquestionably be awarded the merit of having experimentally demonstrated that the amelioration of the surrounding air by plants is not, as Priestley at first believed, due to vegetative action *per se*, but is dependent on the access of light of a sufficient degree of intensity, and, moreover, that the power is confined to the green parts of the plants. At the same time, whilst recognising, as Priestley had done before him, that the combined action of plants and light on the air was a dephlogisticating process, he did not know, until after its demonstration by Senebier, that the particular form of phlogisticated air which was essential to plants was "fixed air" or carbon dioxide. In fact, Ingen-Housz had but a slender knowledge of the chemistry of his day, so much so indeed that he constantly confuses "phlogisticated air" or nitrogen with "fixed air," and attributes the source of the evolved oxygen either to air imprisoned within the leaf, or, in the case of submerged plants, to a metamorphosis of the water itself. I must, however, recall the fact that Ingen-Housz was the first to show that the green parts of plants in the dark, and the roots both in the light and in darkness, vitiate the air in the same way as animals do. On the strength of these experiments, he is generally given credit for having first observed the true respiration of plants, but I cannot avoid the conclusion that, in the controversy which ensued on this point between Ingen-Housz and Senebier, the adverse criticisms of the latter were well-founded. Whilst not denying that plants in the dark have some mephitic influence on the air around them, Senebier maintained that the greater part of the observed effect was due to a fermentative action set up in the large bulk of leaves which Ingen-Housz employed. Certainly some of the results appear to be largely in excess of those we should now expect to obtain from respiratory processes only.[2]

Senebier's work falls between the years 1782 and 1800. The fact that he was an early convert to the new ideas and generalisations of Lavoisier gives his views on plant nutrition far greater precision than those of Priestley and Ingen-Housz. His experiments, for the most part well devised, proved beyond all doubt that the oxygen disengaged from submerged and isolated plants could not be derived from air contained in the leaf parenchyma, but that it depended on the pre-existence of carbon dioxide, and that its evolution was strictly proportional to the amount of carbon dioxide which the water contained.

Although positive experimental proof was still wanting that aërial plants also derive their carbon from carbon dioxide, Senebier regarded this as extremely probable; but, taking into consideration the small amount of this gas present in the atmosphere, he concluded that it must reach the plant by the roots and leaves entirely in a state of solution in water.

The work of Priestley, Senebier, and Ingen-Housz fortunately attracted the attention of a young chemist of high attainments, who, within a period of less than ten years, did more for the advancement of vegetable physiology than any single observer before or since his time. Théodore de Saussure, the second of that illustrious name, and the son of the famous explorer and natural philosopher, commenced his researches about the year 1796, and in 1804 published his "Recherches Chimiques sur la Végétation," a modest little octavo volume of some 300 pages which must certainly take rank as one of the great classics of scientific literature, and one of the most remarkable books of the century.

De Saussure was a past master in the art of experiment, and the methods which he devised for demonstrating the influence of water, air and soil on vegetation have been the models on which all such investigations have been conducted ever since. It is indeed very difficult, when reading this masterly essay, to bear in mind that it was not written fifty or sixty years later than the date on its title-page, so essentially modern are its modes of expression and reasoning, and so far is the author in advance of his contemporaries. It is to this work we must refer for the first experimental proof that plants derive at any rate the greater part of their carbon from the surrounding atmosphere. This was shown by De Saussure by a variety of quantitative experiments of a sufficient degree of accuracy to bring out the great leading facts. By making known mixtures of carbon dioxide and air, and submitting them to the action of plants in sunlight, he was able, not only to show that the gaseous carbon dioxide was decomposed and the carbon assimilated, but also that the volume of oxygen disengaged was approximately equal to that of the carbon dioxide decomposed.[1] He also showed that plants growing in the open in moist sand, or in distilled water, and therefore under conditions in which they could not derive any carbon from other than atmospheric sources, not only materially increased in dry weight, but contained much more carbon at the close of the experiment than at the beginning, and had also fixed an appreciable amount of water in the process. That atmospheric carbon dioxide is not only beneficial to plants in sunlight, but is also essential to their very existence, De Saussure proved by introducing an absorbent of this gas into the vessel containing a plant or the branch of a tree rooted naturally in the soil. Under these conditions, the portions of the plant enclosed always died. He also ascertained by experiment the increase in dry weight of a sunflower plant during four months of natural growth; and knowing approximately the amount of water transpired during that period, and the maximum amount of solids which this transpired water could possibly introduce into the plant, he calculated that these solids, and the carbon dioxide in solution in the transpiration water, fell far short of accounting for the observed increase in the dry weight of the plant. This increase must, therefore, be mainly due to the fixation of atmospheric carbon dioxide and water.

It is certainly a remarkable fact that the rigid experimental proofs which De Saussure brought forward in support of his views did not carry conviction to the minds of every one. His book, however, suffered the fate of many others which have appeared in advance of their time. It is true that De Saussure's doctrines were always kept alive by the advanced physiologists of the French school, such as De Candolle and Dutrochet, but when Liebig first turned his attention to the subject he found the field in possession of the humus theory of Treviranus, a theory which no longer took any account of the decomposition of carbon dioxide by the leaves, but which de-

[1] The view which was taken by Priestley's contemporaries of his position with regard to the discovery of the fundamental facts is well exemplified by the following remarks taken from a paper published by Ingen-Housz in 1784 (*Annales de Physique*, xxiv. 44). "C'est à M. Priestley seul que nous devons la grande découverte que les végétaux possèdent le pouvoir de corriger l'air mauvais et d'ameliorer l'air commun : c'est lui qui nous en a ouvert la porte. J'ai été assez constamment attaché à ce beau système, dans le temps que lui même, par trop peu de prédilection pour ses propres opinions, paroissoit chanceler."

[2] It is by no means uncommon to find Ingen-Housz put forward as the discoverer of the fixation of carbon by plants from carbon dioxide. This claim is generally based on certain statements made in his essay on the "Food Plants and the Renovation of the Soil," published in 1796 as an appendix to the outlines of the fifteenth chapter of the "Proposed General Report from the Board of Agriculture." All that is good and sound in this essay is taken from Senebier's papers without any acknowledgment, but, in appropriating ideas which he evidently understands very imperfectly, he has built up a system of plant economy which is almost unintelligible.

[1] Although clearly indicating that no change of volume occurred in the mixture of air and carbon dioxide so treated, his final analytical results show a small apparent evolution of nitrogen. This was due to the eudiometric methods he employed, methods, it is true, far superior in point of accuracy to those of his predecessors, but still necessarily imperfect.

rived the whole of the elements of the growing plant from a solution of the soil extract taken up by the roots. We may well say with Sachs, "nothing can be conceived more deplorable than this theory of nutrition; it would have been bad at the end of the seventeenth century, it is difficult to believe that it could have been published thirty years after De Saussure's work." It is well known how by the cogency of his reasoning and the force of his genius Liebig successfully overthrew this heresy, and once more established the doctrine of carbon assimilation as taught by De Saussure; and the accurate work of Boussingault, who, whilst elaborating far more delicate analytical processes than were possessed by chemists in the early days of the century, still in the main used De Saussure's methods, gave the final death-blow to the humus theory, at any rate in the crude form in which it was presented by its originators. No one since that time has questioned the fact that green plants owe the greater part of their carbon to atmospheric sources, and the accumulated experience of two succeeding generations of workers has added proof on proof of the correctness of this great generalisation.

But whilst it cannot be doubted that green plants devoid of parasitic or saprophytic habit derive the principal part of their carbon from the air, is the experimental evidence at present so complete as to exclude all other sources of supply? De Saussure himself certainly left the door open to such a possibility, and although Boussingault held a different view, we find Sachs as late as 1865 maintaining that it is not contrary to the generally accepted theory of assimilation to suppose that there are chlorophyllous plants which decompose carbon dioxide and at the same time absorb ready-formed organic substances whose carbon they utilise in the formation of new organs.

Up to comparatively recently there was little or no experimental evidence to justify this supposition, for the early experiments of De Saussure on the influence of solutions of sugar, and of other organic substances, on growing plants, although very suggestive, were not of a sufficiently precise nature to lead to any conclusions, and we must come down to within fifteen years of the present time for anything like a demonstration that the green organs of plants can, under favourable conditions, build up their tissue from already elaborated carbon compounds just as do the fungi and the non-chlorophyllous plants generally.

The active centres of the decomposition of carbon dioxide in green leaves are the chlorophyll corpuscles or chloroplastids, and the first visible indication of this decomposition is the formation within these chloroplastids of minute granules of starch whose presence can be shown by suitable micro-chemical means. I have elsewhere discussed the question of how far the appearance of this starch is dependent on the pre-existence of other carbohydrates of a simpler constitution, and also the probability that the whole of the products of assimilation do not necessarily pass through the form of starch: this is a subject which need scarcely concern us at the present moment; it is sufficient to draw attention to the main fact that in an assimilating cell the chloroplastids, in the vast majority of cases, give rise to these minute starch granules, which once more disappear when the plant is placed in darkness, or when the air around it is deprived of carbon dioxide. Now in 1883 Böhm made the interesting discovery that when green leaves are placed in the dark until the starch of their chloroplastids has completely disappeared, there is a reappearance of starch when the cut end of the leaf-stalk is immersed in a solution of cane sugar and of dextrose, or when the leaf is brought directly in contact with solutions of these substances. He found, in fact, that the elements of the cell which, under ordinary circumstances, manufacture their materials for plant growth by the reduction of carbon dioxide under the influence of sunlight, can, under other conditions, supply their requirements from suitable ready-formed organic substances. These observations of Böhm were fully confirmed two years later by Schimper, and were subsequently much extended by A. Meyer and E. Laurent, who found that fructose, maltose, mannitol, dulcitol, and glycerol could also contribute directly to the nutrition of leaves.

Bokorny, working with *Spirogyra* immersed in dilute solutions, found that starch production in the chlorophyll bodies could be induced by a large number of organic substances, including, amongst many others, asparagin, citric, tartaric, and lactic acids, leucine, tyrosine, and peptone.[1]

Very much more to the point are the experiments of Acton, made in 1889, and the still more recent work of J. Laurent and of Mazé.

In his experiments on terrestrial plants, Acton, after depleting them of starch, immersed the cut branches or roots, as the case might be, in culture fluids containing certain organic substances, and took precautions to prevent any normal assimilation from taking place by depriving the air around the plant of any trace of carbon dioxide. He was not able to show the direct nutritive influence of so large a range of substances as Bokorny had done for *Spirogyra*, but his results leave no room for doubt that several of the carbohydrates, and even glycerine, can be absorbed by the roots, and can contribute to the nutrition of the green parts. Acton tried, amongst other substances, an "extract of natural humus," which was an aqueous solution of the extractives of a light soil which are soluble in dilute alcohol. This extract was found to be effective in producing a small quantity of starch in the leaves, and it evidently contained some substance or substances directly assimilable by the plant.

Apparently without knowing anything of this work of Acton, J. Laurent has recently made a series of experiments on the culture of the maize plant in mineral solutions containing saccharose, glucose, or invert-sugar, and in this way has not only obtained, as Acton had done before him, evidence of the active formation of starch in the leaves, but has also found a very notable increase in the dry weight of the plant. Although assimilation of the carbohydrate may under these circumstances go on in darkness, Laurent found that the process was much enhanced when light had access to the plant. Mazé, within the last few months, has obtained even more pronounced effects of this kind.

When all these new facts are taken into consideration, I think they justify what I have already said, that we ought to demand more direct evidence than is at present available before we accept the view that the majority of chlorophyllous plants take in *the whole* of their carbon from the atmosphere. In the cycle of change which the organic matter of the soil is constantly undergoing under the influence of micro-organisms, it seems by no means improbable that intermediate substances may be formed which in some measure directly contribute to the nutrition of the higher plants, and we must also by no means lose sight of the possible effect, in the same direction, of the symbiotic union of certain fungi with the root extremities of many plants, the Mycorhizæ, whose functions are still so imperfectly understood. Then, again, we must remember that we have another possible extra-atmospheric source of carbon dioxide in the transpiration water of the plant, which is derived from a soil whose gases may contain 5 per cent. or more of carbon dioxide. From the amount of water transpired in a given time, and an application of the law of partial pressures, it may be readily shown that the supply of carbon dioxide to the aërial organs of a plant from this source is by no means negligible.

Before these problems can be attacked for a particular plant with any hope of success, it is clear that we must have some means of establishing an accurate debtor and creditor account as between the plant and the surrounding atmosphere, and this account must extend over a sufficiently long period, and allow of an accurate balance being struck with the amount of carbon found in the plant at the end of the experiment.

Up to within a few years ago we had no means of even approximately determining the actual rate at which the assimilatory process goes on in a plant other than that afforded by its increase in weight in a given time. Such experiments, necessarily extending over weeks or months, can, at the best, only give us certain average results, and consequently afford no measure of the activity of assimilation under fixed conditions of insolation. In the year 1884, Sachs, who had for some time been at work on the formation of starch in leaves under the action of sunlight, found that the accumulation of freshly assimilated material in a leaf may, under favourable conditions, go on so rapidly as to give rise to a very appreciable increase of weight in the leaf lamina within the short space of a few hours. By observing at

[1] By far the most interesting and important result of Bokorny is the proof he gives that formaldehyde is directly assimilable by *Spirogyra*. His early attempts to show this had been rendered abortive by the highly poisonous nature of this substance. The difficulty was surmounted by using a dilute solution of sodium oxymethylsulphonate, which on warming with water splits up into formaldehyde and acid sodium sulphite. To prevent the unfavourable action of the acid sodium sulphite, dipotassium or disodium phosphate was added to the plant cultures. In such a solution, with rigid exclusion of carbon dioxide, *Spirogyra majuscula* forms starch in its chlorophyll bodies, but the access of light appears to be necessary.

The importance of this experiment is very great in connection with Baeyer's well-known hypothesis that the first act of assimilation is the reduction of carbon dioxide and water to the state of formaldehyde.

different times of the day the varying dry weight of equal areas of large leaves, Sachs obtained an approximate measure of the rate of the assimilatory process which he could express in terms of actual number of grams of substance assimilated by a unit area of leaf in unit of time. In this manner he was able to show, for instance, that a sunflower leaf, whilst still attached to the plant, increases in weight when exposed to bright sunshine at the hourly rate of about one gram per square metre of leaf area. In the case of similar leaves detached from the plant, and of course under conditions in which the products of assimilation were entirely accumulated in the leaf, he found an increase in weight of rather more than 1½ grams per square metre per hour.

I was able to confirm this work of Sachs in the course of an investigation on the Chemistry of Leaves which I made with Dr. G. H. Morris in 1892–93, and there can be no doubt that the variations in the weight of leaves can be used as a fair index of the activity of a leaf in assimilating, but it is not a method which admits of much refinement of accuracy, owing, amongst other things, to the want of perfect symmetry in the leaves as regards thickness and density of the laminæ and to the possible migration of the assimilated material into the larger ribs, which of course cannot be included in the weighings.

It is evident that a far better plan of measuring the rate of assimilation under varying conditions would be the estimation of the actual amount of carbon dioxide entering a given area of the leaf in a certain time, and it was to the perfection of a method of this kind that Mr. Escombe and I first turned our attention.

In all previous attempts to measure the rate of ingress of carbon dioxide, such as those of Corenwinder, and more recently still of Mr. F. F. Blackman, it has been necessary to use air containing comparatively large quantities of carbon dioxide, amounting to 4 per cent. and upwards. Interesting and useful as such experiments undoubtedly are from the point of view from which they were undertaken, we must not lose sight of the fact that such conditions are highly artificial, and very far removed from those under which a plant finds itself in the natural state, where its leaves are bathed with air containing, not 4 or 5 per cent., but only ·03 per cent. of carbon dioxide. I shall have occasion later on to show how remarkably the rate of intake of carbon dioxide into a plant is influenced by extremely small variations in the tension of that gas, and that on this account no deduction can be drawn as to the rate of assimilation under natural conditions from any experiments in which the air contains even so small an amount of carbon dioxide as 1 per cent.

Before proceeding further in this direction, however, it will be well to consider the amount of carbon dioxide which must enter a leaf in a given time in order to produce an influence on its weight comparable with that indicated by the Sachs method of weighing definite areas. For this purpose I will consider a leaf with which we have made many experiments—that of *Catalpa bignonioides*. It is a very symmetrical leaf and a good assimilator, and since the intake of carbon dioxide takes place only on the under side, the question to which I wish to draw your attention can be stated in a simple manner. When such a leaf is subjected to a modified form of the half-leaf weighing method of Sachs, into the details of which I cannot here enter, it may, under favourable conditions, show an increase in dry weight equal to about one gram per square metre per hour. Since this increase in weight is due almost entirely to the formation of carbohydrates, we can calculate with a close approximation to accuracy the corresponding amount of carbon dioxide. This will of course depend, within certain narrow limits, on the nature of the carbohydrate formed. The formation of a gram of starch requires 1·628 grams of carbon dioxide, whilst an equal amount of a $C_6H_{12}O_6$ or a $C_{12}H_{22}O_{11}$ sugar require 1·466 and 1·543 grams respectively. From the knowledge we possess of the nature of the carbohydrates of the leaf, we are quite sure that the mean of these values, that is 1·545 grams, must be very near the truth. This amount corresponds to 784 c.c. of carbon dioxide at normal temperature and pressure, which must represent the volume abstracted by the square metre of leaf surface in one hour from air containing only three parts of carbon dioxide in 10,000, supposing the method of leaf weighing to give correct results. We shall see later on that this intake can be verified by direct estimations; it is equivalent to the total amount of carbon dioxide in a column of air of a cross section equal to that of the leaf, and of a height of 26 decimetres.

The extraordinary power which an assimilating leaf possesses of abstracting carbon dioxide from the air is best shown by comparing it with an equal area of a freely exposed solution of caustic alkali. We have made a very large number of experiments on the rate at which atmospheric carbon dioxide can be taken up by a solution of caustic soda under varying conditions, and have been surprised to find how constant the absorption is. In a moderately still air a square metre of surface of such a freely exposed solution will absorb about 1200 c.c. of carbon dioxide per hour, and this can only be increased to about 1500 c.c. even if the dish is exposed to the full influence of a strong wind out in the open. When the surface of the liquid is constantly renewed during the experiment by means of a mechanical stirrer, the rate of absorption is not sensibly affected, providing the agitation does not appreciably increase the surface area, and considerable variations in the strength of the alkaline solution are also without any effect. On the other hand, slight variations in the tension of the carbon dioxide of the air have a marked influence on the rate of absorption, and in order to study this point we have constructed an apparatus which allows us to pass over an absorptive surface of liquid a current of air in a stratum of known thickness, and with a known average velocity.

By introducing definite amounts of carbon dioxide into this stream of air we have been able to determine the influence of its tension on the rate of absorption. At present we have only employed air containing amounts varying from 0·8 to 13 parts per 10,000, that is to say, from about one-quarter to a little more than four times the amount contained in normal air. Within these limits, and probably beyond them, the rate of absorption by the alkaline surface is strictly proportional to the tension of the carbon dioxide in the air current. I shall have occasion to show later on that the same rule holds good with regard to an assimilating leaf, and that in this case also, within certain limits, the intake of the gas is proportional to its tension.

The fact which I wish more particularly to bring out in these comparisons is that a leaf surface which is assimilating at the rate of one gram of carbohydrate per square metre per hour is absorbing atmospheric carbon dioxide *more than half as fast as the same surface would do if wetted with a constantly renewed film of a strong solution of caustic alkali.*

From what I have just said about the influence of tension on the absorption of carbon dioxide by an assimilating leaf, it is clear that any attempts to determine by direct means the natural intake of that gas during assimilation must be made with *ordinary* air, and that such experiments can only be carried out on a comparatively large scale. We had in the first instance to devise an apparatus which would rapidly and completely absorb the whole of the carbon dioxide from a stream of air passing through it at the rate of from 100 to 200 litres per hour, and at the same time admit of an extremely accurate determination of the absorbed carbon dioxide.

The absorbing apparatus which we finally adopted is a modification of one used by Reiset in his estimations of the carbon dioxide of the atmosphere. It consists essentially of a glass tube 50 cm. long, fixed vertically in a wide-mouthed glass vessel furnished with a second aperture and tubulure. The height of the vertical tube is invariable, but its width is regulated according to the amount of air required to be drawn through the apparatus in a given time. The bottom of this tube is closed with a platinum or silver plate pierced with a large number of very small holes, and two other similar perforated plates are inserted in the tube at certain intervals. The upper part of the tube is put in connection with an aspirating water-pump, and the absorbing liquid is placed in the lower glass vessel, whose second tubulure is connected with the supply of air in which the carbon dioxide has to be determined. When the aspirator is started the liquid is first drawn up into the vertical tube, and the air then follows through the perforated plates which act as "scrubbers." Reiset, in his work, used baryta water as the absorbent, an aliquot part of which was titrated before and after the experiment, the changes in the volume of the liquid being corrected for by certain devices which I need not describe.

The efficiency of the apparatus as a complete absorber of atmospheric carbon dioxide leaves nothing to be desired, but in dealing with large quantities of baryta solution, amounting to 400 c.c. or more, the errors due to inaccurate titrations, or to over or under estimation of the volume changes, are all thrown on the final result, of which they may form a considerable part. We have consequently altogether discarded the use of baryta as an absorbent in favour of caustic soda. The carbonate is estimated by a double titration process, suggested a few years ago by Hart, and we have succeeded in so far improving this method that there is no difficulty in determining in 100 c.c. of the alkaline solution an amount of carbonate corresponding to $\frac{1}{10}$ c.c. of carbon dioxide.

There is practically no limit to the amount of air which can be passed through an absorbing apparatus such as I have described, and one of very moderate dimensions will allow from 100 to 150 litres per hour to pass with perfect safety. Larger amounts can be dealt with either by increasing the size of the apparatus or by using several smaller ones arranged in parallel.

With proper precautions, determinations can certainly be made to within ·02 part of carbon dioxide in 10,000 of air, so that with an apparatus of this kind it is possible to estimate the intake of carbon dioxide into a leaf or plant from ordinary atmospheric air, and to keep a sufficiently rapid stream of air passing over the leaf to maintain the tension of the carbon dioxide only slightly below the normal amount.

The air is measured by carefully standardised meters, reading to about 20 c.c.; and since the amounts of air aspirated vary from 100 to 900 litres or more, there are practically no errors of measurement. The tension at which the air passes through the absorption apparatus is measured by a manometer, and all the volumes are reduced to standard temperature and pressure.

All such experiments of course necessitate, not only a determination of the carbon dioxide in the air which has passed over the leaf or plant, but also a simultaneous determination of the carbon dioxide in the ordinary air used. The accumulation of these air determinations clearly shows that the ordinary statements of our text-books as to the amount of carbon dioxide and its limits of variation are altogether misleading.

In our experiments the air was in all cases taken from a height of four feet six inches from the ground, the amounts aspirated varying from 100 to 500 litres.

In the month of July 1898, the minimum amount of carbon dioxide found was 2·71 parts per 10,000 of air, and the maximum 2·86. During the winter months, when the ground was almost bare of vegetation, it rose to from 3·00 to 3·23 parts per 10,000; and on one foggy day, March 16, 1899, after a whole week of similar weather, we found the very exceptional amount of 3·62. As a rule, we may take it that the amount of carbon dioxide in the atmosphere during the period of greatest plant growth rarely falls short of 2·7 parts per 10,000, and rarely exceeds 3·0 parts, with an average of about 2·85. These numbers come very close to the determinations of Reiset, and of Müntz and Aubin, and agree also fairly well with the Montsouris determinations.

If instead of taking the air from a height of three or four feet from the ground, we examine the stratum only one or two centimetres above the surface of a soil free from vegetation, we find, as might be expected, a very large increase in the amount of carbon dioxide, which may exceed, under these circumstances, 12 or 13 parts per 10,000 of air. Such a soil, in fact, gives off by diffusion into the surrounding air an amount of carbon dioxide which is comparable to that evolved by a respiring leaf, that is to say, about 50 c.c. per square metre per hour. This is probably a factor which has to be taken into account in considering the assimilative power of vegetation of very low growing habit, but in all other cases we may assume with safety that aërial plants have to take in their carbon dioxide from air in which its tension does not exceed $\frac{3}{10000}$ of an atmosphere.

The actual intake of carbon dioxide is determined by enclosing the entire leaf in specially constructed air-tight, glazed cases, through which a sufficiently rapid air stream is passed. These cases are so arranged that the leaf can be enclosed whilst still attached to a plant which is growing out in the open under perfectly natural conditions, and some of them are sufficiently large to take the entire leaf of a sunflower.

The carbon dioxide content of the air is determined both before and after its passage through the apparatus, and since the amount of air passed is known we have all the data requisite for determining the actual amount retained by the leaf.

An experiment generally lasts from five to six hours, and the carbon dioxide fixed in this time may amount to 150 c.c. or more, the actual error of determination being very small indeed.

For purposes of comparison the volumes are reduced to the actual number of cubic centimetres of the gas absorbed by a square metre of leaf in one hour, which of course necessitates an exact determination of the area of the leaf. This is most conveniently effected by printing the leaf on sensitised paper, and tracing round its outline with a planimeter set to read off square centimetres—a far more accurate and expeditious plan than that of cutting out a fac-simile of the leaf from paper of a known weight per unit of area.

If it is desired to estimate the assimilative power of a leaf in an atmosphere artificially enriched with carbon dioxide, the air stream before entering the leaf case is passed through a small tower containing fragments of marble, over which there drops a very slow stream of dilute acid, whose rate of flow is so proportioned to the air stream as to give about the desired enrichment with carbon dioxide. The stream of air is then divided, one part going on directly to the leaf case, whilst the other passes through a separate absorption apparatus and meter for the accurate determination of its carbon dioxide content.

In order to show the kind of results obtained in this manner, I will give one or two examples.

A leaf of the sunflower, having an area of 617·5 sq. cm., was enclosed in its case whilst still attached to the plant, and was exposed to the strong diffuse light of a clouded sky for five and a half hours, air being passed over it at the rate of nearly 150 litres per hour. The content of the air in carbon dioxide as it entered the apparatus was 2·80 parts per 10,000, and this was reduced to 1·74 parts per 10,000 during its passage over the leaf. This corresponds to a total absorption of 139·95 c.c. of carbon dioxide, or to an intake of 412 c.c. per square metre per hour. If we assume that the average composition of the carbohydrates formed is that of a $C_6H_{12}O_6$ sugar, the above amount of carbon dioxide corresponds to the formation of 0·55 gram of carbohydrate per square metre per hour. But we must bear in mind that the average tension of the carbon dioxide in the leaf case was only equal to 1·93 parts per 10,000—that is, only about seven-tenths of its tension in the normal air. A correction has therefore to be made if we wish to know how much the leaf would have taken in, under similar conditions of insolation, if it had been bathed with a current of air of sufficient rapidity to practically keep the amount of carbon dioxide constant at its normal amount of 2·8 per 10,000. We shall see later on that, well within the limits of this experiment, the intake is proportional to the tension, so that applying this correction we may conclude that under identical conditions of insolation and temperature this leaf would have taken in an amount of carbon dioxide from the free air at a rate sufficient to produce 0·8 gram of carbohydrate per square metre per hour. This is almost exactly equal to the assimilation rate of the sunflower which I deduced in 1892 from the indirect process of weighing equal areas of the leaf lamina before and after insolation, and it also agrees fairly well with some of Sachs' original experiments of a similar nature.

In another experiment made with the leaf of *Catalpa bignonioides* in full sunlight, the amount of carbon dioxide in the air passing over the leaf fell from 2·80 to 1·79 parts per 10,000, the total hourly intake for the square metre being 344·8 c.c. When this is corrected for tension, it corresponds to an assimilation in free air of 0·55 gram of carbohydrate per square metre per hour.

An increase in the intensity of the daylight, as might be expected, influences to some extent the rate of intake of atmospheric carbon dioxide; but providing the illumination has reached a certain minimum amount, a further increase in the radiant energy incident on the leaf does not result in anything like a proportional amount of assimilation. We have found, for instance, that the rate of assimilation of a sunflower leaf, exposed to the clear northern sky on a warm summer's day, was about one-half of what it was when the leaf was turned round so as to receive the direct rays of the sun almost normal to its surface. Now in this latter case the actual radiant energy received by the leaf was at least twelve times greater than was received from the northern sky, but the assimilation was only doubled.

These differences in the effect of great variation of illumination become stil less marked when we use air which has been artificially enriched with carbon dioxide. In one instance of this kind, for example, we found the assimilation in the full diffuse light of the northern sky to be 87 per cent. of what it was in direct sunshine.

This brings me to another interesting point on which I have already touched slightly—the enormous influence which slight changes in the carbon dioxide content of the air exert on the rate of its ingress into the assimilating leaf.

With a constant illumination, either in direct sunlight or diffuse light, the assimilatory process responds to the least variation in the carbon dioxide, and within certain limits, not yet clearly defined, the intake of that gas into the leaf follows the same rule as the one which holds good with regard to the absorption of carbon dioxide by a freely exposed surface of a solution of caustic alkali; that is to say, from air containing small but variable quantities of carbon dioxide *the intake is directly proportional to the tension of that gas.*

A single experiment will be sufficient to illustrate this.

A large sunflower leaf, still attached to the plant and exposed to a clear northern sky, gave an assimilation rate equal to 0·331 gram of carbohydrate per square metre per hour, when air was passed containing an average amount of 2·22 parts of carbon dioxide per 10,000. When the experiment was repeated under similar conditions of illumination, but with air containing 14·82 parts of CO_2 per 10,000, the intake corresponded to an hourly formation of 2·409 grams of carbohydrate per square metre. The ratio of the tensions of the carbon dioxide in the two experiments is 1 to 6·7, and the assimilatory ratio is 1 to 7·2, so that the increased assimilation is practically proportional to the increase in tension of the carbon dioxide.

Since an increase of carbon dioxide in the atmosphere surrounding a leaf is followed by increased assimilation even in diffuse daylight, it is clear that, under all ordinary conditions of illumination, the rays of the right degree of refrangibility for producing decomposition of carbon dioxide are largely in excess of the power of the leaf to utilise them. Under natural conditions this excess of radiant energy of the right wave-length must, from the point of view of the assimilatory process, be wasted, owing to the limitation imposed by the high degree of dilution of atmospheric carbon dioxide. But although the actual manufacture of new material within the leaf lamina is so largely influenced by small variations in the carbon dioxide of the air, we are not justified in concluding that the plant *as a whole* will necessarily respond to such changes in atmospheric environment, since the complex physiological changes involved in metabolism and growth may have become so intimately correlated that the perfect working of the mechanism of the entire plant may now only be possible in an atmosphere containing about three parts of carbon dioxide in 10,000.

We have commenced a series of experiments which will, I hope, throw considerable light on this point, but the work is not at present in a sufficiently advanced state for me to make more than a passing allusion to it.

The penetration of the highly diluted carbon dioxide of the atmosphere into the interior air-spaces of the leaf on its way to the active centres of assimilation must, in the first instance, be a purely physical process, and no explanation of this can be accepted which does not conform to the physical properties of the gases involved.

Since there is no mechanism in the leaf capable of producing an ebb and flow of gases within the air spaces of the mesophyll in any way comparable with the movements of the tidal air in the lungs of animals; and since also the arrangement of the stomatic openings is such as to effect a rapid equalisation of pressure within and without the leaf, we must search for the cause of the gaseous exchange, not in any mass movement, but in some form of diffusion. This may take place in the form of *open diffusion* through the minute stomatic apertures, which are in communication both with the outer air and the intercellular spaces, or the gaseous exchange may take place through the cuticle and epidermis by a process of *gaseous osmosis*, similar to that which Graham investigated in connection with certain colloid septa.

For many years there has been much controversy as to which form of gaseous diffusion is the more active in producing the natural interchanges of gases in the leaf. The present occasion is not one in which full justice can be done to the large amount of experimental work which has from time to time been carried out in this direction. Up to comparatively recently the theory of cuticular osmosis has been the one which has been more commonly accepted, free diffusion through the open stomata being considered quite subsidiary. In 1895, however, Mr. F. F. Blackman brought forward two remarkable papers which opened up an entirely new aspect of the subject. After showing the fallacy underlying certain experiments of Boussingault, which had been generally supposed to prove the osmotic theory of exchange, Mr. Blackman gave the results of his own experiments with a new and beautifulty constructed apparatus, which enabled him to measure very minute quantities of carbon dioxide given off from small areas of the upper and under sides of a respiring leaf, and also to determine the relative intake of carbon dioxide by the two surfaces during assimilation in air artificially charged with that gas. The conclusions drawn are that respiratory egress, and assimilatory ingress of carbon dioxide, do not occur in the upper side of a leaf if this is devoid of stomatic openings, and that when these openings exist on both the upper and under sides the gaseous exchanges of both physiological processes are directly proportional to the number of stomata on equal areas, hence in all probability the exchanges take place only through the stomata.[1]

These observations of Mr. Blackman are of such far-reaching importance, and lead, as we shall see presently, to such remarkable conclusions with regard to the rate of diffusion of atmospheric carbon dioxide, that we felt constrained to inquire into the matter further, and for this purpose we employed a modified form of the apparatus which we have used throughout our work on assimilation. This was so arranged that a current of ordinary air could be passed, just as in Mr. Blackman's experiments, over the upper and lower surface of a leaf separately, the increase or decrease in the carbon dioxide content of the air being determined by absorption and titration in the manner I have already alluded to.

In this way we were able to employ comparatively large leaf areas, and to continue an experiment for several hours, so that we had relatively large amounts of carbon dioxide to deal with, and the ratios of gaseous exchange of the two sides of the leaf could consequently be determined with considerable accuracy.

Our results, on the whole, are decidedly confirmatory of Mr. Blackman's observations. The side of a leaf which is devoid of stomatic openings certainly neither allows any carbon dioxide to escape during respiration, nor does it permit the ingress of that gas when the conditions are favourable for assimilation. On the other hand, when stomata exists on both the upper and under sides of a leaf, gaseous exchanges take place through both surfaces, and, as a rule, in some sort of rough proportion to the distribution of the openings. There is, however, under strong illumination, a greater intake of carbon dioxide through the upper surface than would be expected from a mere consideration of the ratio of distribution of the stomata.[2] Nevertheless, the general connection between gaseous exchange and distribution of stomata is so well brought out that we must regard it as highly probable that these minute openings are the true paths by which the carbon dioxide enters and leaves the leaf.

We must now look at certain physical consequences which proceed from this assumption, and see how far they can be justified by the known or ascertainable properties of carbon dioxide at very low tensions.

The leaf of *Catalpa bignonioides* is hypostomatic, and therefore takes in carbon dioxide only by its lower surface. Under

1 There is one important fact to be borne in mind when considering how far these observations exclude the possibility of cuticular osmosis. In the many leaves we have examined, Mr. Escombe and I have found that the occurrence of stomata on the upper surface of the leaf is always correlated with a much less dense palisade parenchyma. The cuticle and epidermis under these conditions are in a much more favourable state to allow carbon dioxide to pass into the leaf by osmosis than when the closely-packed palisade cells abut against the epidermis, as they do when this is imperforate.

2 Granted that the stomata constitute the paths of gaseous exchange, it is clear that the amount of diffusion through them, other things being equal, must depend very largely on the extent to which they are opened. The delicate self-regulating apparatus which governs the size of the openings is so readily influenced, amongst other things, by differences of illumination, that *a priori* we should not expect the stomata on the upper surface of an insolated leaf to be in the same condition as those of the more shaded lower surface. This may very well account for the stomatic ratio of the two sides not being in closer correspondence with the assimilatory ratios, as found in most of our experiments carried out in bright sunlight. In light of lesser intensity there is always a closer correspondence of the two ratios.

There is also another possible explanation of the fact. Since we have good reason to believe that the principal part of the assimilatory work is carried on by the palisade parenchyma, which occurs in the upper side of the leaf, the tension of the carbon dioxide in the air spaces of that part of the mesophyll is probably less than it is in the spongy parenchyma. There will, therefore, be a higher "diffusion gradient" between the carbon dioxide of the outer and inner air in the former case than in the latter, and this would certainly tend to a more rapid diffusion through the openings in the upper side of the leaf.

favourable conditions it is quite possible, during assimilation, to obtain an intake of atmospheric carbon dioxide into this leaf at the rate of 700 c.c. per square metre per hour (measured at 0° and 760 mm.), corresponding to an average linear velocity of the carbon dioxide molecules of 3·8 *centimetres per minute*, supposing the intake to be distributed evenly over the whole of the lower leaf surface. This velocity is almost exactly one-half of that at which carbon dioxide will enter a freely exposed surface of a solution of caustic alkali. But if the intake of the gas is confined to the stomatic openings of the leaf, its velocity of ingress must be very much greater than this.

We have carefully determined the number of stomata occurring on a given area of this particular leaf, and also the dimensions of the openings, and find that the total area of the openings, supposing them to be dilated to the fullest possible extent, amounts to just under *one per cent.* of the leaf surface. It follows from this that the average velocity of the atmospheric carbon dioxide in passing through these openings must be 380 *centimetres per minute*, that is to say, just *fifty* times greater than into a freely exposed absorbent surface of alkali. In other words, supposing every one of the stomatic openings of this leaf could be filled up with a solution of caustic alkali, the absorbent power of the leaf as a whole would only be $\frac{1}{50}$ of what it actually is when assimilating.

These are some of the consequences which flow from an acceptance of the hypothesis of stomatic exchange, and it appeared to be impossible to accept that hypothesis unreservedly without some collateral evidence that these comparatively high velocities of diffusion are physically possible when dealing with such low gradients of tension as must necessarily exist when the highest amount of carbon dioxide does not exceed ·03 per cent.

The well-known general law expressing the rate of the spontaneous intermixture of two gases when there is no intervening septum was, as every one knows, established by Graham, and the more elaborate investigations of Loschmidt many years later served to confirm the general accuracy of this law, and to show that, within very narrow limits, the diffusion constant varies in different gases inversely as the square roots of their densities.

But a mere knowledge of the diffusion constants of air and carbon dioxide does not, as far as I can see, materially assist us in the particular case we have under consideration. In order to gain some idea of what is actually possible in the way of stomatic diffusion in an assimilating leaf, we must know something of the *actual rate* at which atmospheric carbon dioxide can be made to pass into a small chamber containing air at the outside tension, but in which the carbon dioxide is kept down almost to the vanishing point by some rapid process of absorption; and we must also determine the influence of varying the size of the aperture through which the diffusion takes place.

Our attempts to answer these questions experimentally have led us into a long investigation, which promises to be of wider interest than we had first imagined. I only propose to give on this occasion a general account of the results so far as they affect the physical question of the intake of carbon dioxide into the plant.

When a shallow vessel containing a solution of caustic alkali is completely covered, the air above the liquid is very speedily deprived of the whole of its carbon dioxide. If we now imagine a hole to be made in the cover of the vessel, carbon dioxide will enter the air space by free diffusion, and its amount can be very accurately determined by subsequent titration in the manner I have previously referred to. The time occupied by the experiment and the dimensions of the aperture being known, we can express the results in actual amounts of carbon dioxide passing through unit area of aperture in unit of time; or, since the tension of that gas in the outer air is known, we can express the average rate of the carbon dioxide molecules across the aperture in terms of actual measurement, say centimetres per minute.

We have made a very large number of experiments of this kind, using, in the first instance, dishes of about 9 cm. in diameter, and varying the size of the holes in the cover, the air space over the absorbent liquid being always the same.

The accompanying curve, Fig. 1, illustrates the effect which a gradually decreasing orifice has on the rate of diffusion of atmospheric carbon dioxide under these conditions. The diameters of the orifice in millimetres are given on the abscissa line, and the rates of diffusion through equal areas of the apertures are taken as ordinates, the rate of absorption in the open dish under similar conditions being taken as unity.

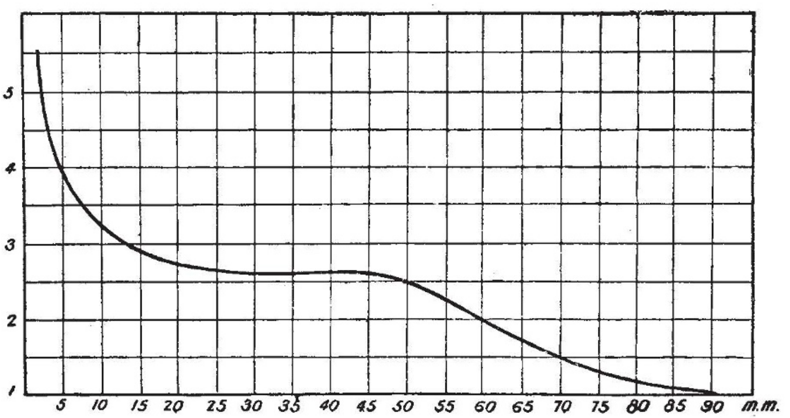

FIG. 1.

It will be seen that in the first instance a gradual reduction of the diameter of the opening is accompanied by a very regular increase in the rate of passage of the carbon dioxide until a diameter of about 50 mm. is reached; that is to say, up to a point at which about two-thirds of the area of the dish is covered. A further progressive diminution in the size of the aperture makes comparatively little difference in the diffusion rate until we reach about 20 mm., beyond which the curve again begins to rise, increasing rapidly in steepness as the apertures become smaller.

The experiments with open dishes are too crude for a study of the influence of very small apertures, so for this part of our work we constructed a special form of apparatus which has enabled us to determine the relative rates of diffusion through orifices in thin metal plates ranging down to 1 mm. in diameter.

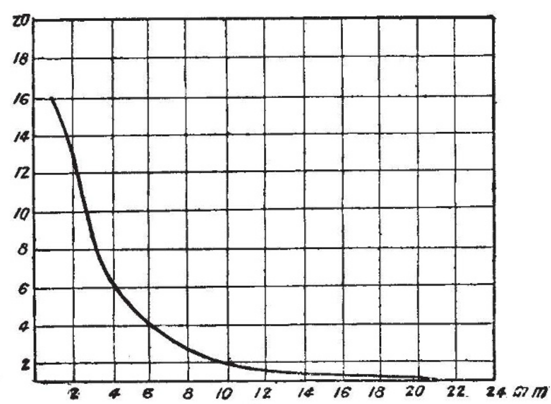

FIG. 2.

I have plotted the results of such a series of experiments (see Fig. 2), showing the relative rates of diffusion of atmospheric carbon dioxide through equal areas of apertures between 20 mm. and 1 mm. in diameter, under constant conditions, and it will be noticed how very steep the curve becomes after diameters of 5 or 6 mm. are reached.

The speed at which the diffusion of atmospheric carbon dioxide takes place through unit area of an orifice of 1 mm. in diameter is just sixteen times as fast as it is through unit area of an aperture of 20 mm.; and since we know that the rate of passage in the latter case is two and a half times greater than the absorption rate of an equal area of a freely exposed surface of a solution of caustic alkali, we arrive at the conclusion that, under the particular conditions of our experiment, the diffusion rate through an aperture of 1 mm. is *forty times* greater than the rate of absorption of a free alkaline surface of equal area.

This corresponds to an actual average rate of passage of the molecules of the atmospheric carbon dioxide of about 266 centimetres per minute.

Now, we have already seen, in the case of a Catalpa leaf, that if the gaseous exchange during assimilation goes on only through the stomatic openings, we require a minimum velocity of something like 380 centimetres per minute, a velocity which we are sensibly approaching in our experiments with apertures of about 1 mm. in diameter. But the effective area of a stomatic opening of the Catalpa leaf is equal to that of a circle with a diameter of less than 1/100 mm., and since our experiments indicate a very rapid increase in the velocity of diffusion as the aperture is diminished, it is clear that no difficulty, as regards the physics of the question, can be raised against the idea that atmospheric carbon dioxide reaches the active centres of assimilation by a process of free diffusion through the leaf stomata.

One of the most interesting problems connected with plant assimilation relates to the efficiency of a green leaf as an absorber and transformer of the radiant energy incident upon it.

It is already well known that the actual amount of energy stored up in the products of assimilation bears a very small proportion to the total amount reaching the leaf: in other words, the leaf, regarded from a thermo-dynamic point of view, is a machine with a very low "economic coefficient." We require, however, to know much more than this, and to ascertain, amongst other things, how the efficiency of the machine varies under different conditions of insolation, and in atmospheres containing varying amounts of carbon dioxide.

The measure of the two principal forms of work done within the leaf, the vaporisation of the transpiration water on the one hand, and the reduction of carbon dioxide and water to the form of carbohydrates on the other, can be ascertained by modifying our experiments in such a manner as to allow the transpiration water to be determined, as well as the intake of carbon dioxide.

For the actual measurement of the total energy incident on the leaf under various conditions we are now using one of Prof. Callendar's recording radiometers of specially delicate construction, which will be ultimately calibrated in calories. This instrument gives promise of excellent results, but up to the present time the work we have done with it is not sufficiently advanced for me to describe. We may, however, obtain a very fair idea of the variation in the efficiency of a leaf from one or two examples in which the amount of incident energy was deduced from other considerations.

In the case of a sunflower leaf exposed to the strong sunlight of a brilliant day in August, the average amount of radiant energy falling on the leaf during the five hours occupied by the experiment was estimated at 600,000 calories per square metre per hour. The averege hourly transpiration of water during that time was at the rate of 275 c c. per square metre, and the assimilated carbohydrate, estimated by the intake of carbon dioxide, was at the rate of 0·8 gram per square metre per hour.

The vaporisation of 275 c.c. of water must have required the expenditure of 166,800 calories, and the endothermic production of 0·8 gram of carbohydrate (taking the heat of combustion at 4000 gram calories) corresponds to the absorption of 3200 calories. Thus, as the final result under these particular conditions of experiment, we find that the leaf has absorbed and converted into internal work about 28 per cent. of the total radiant energy incident on it, 27·5 per cent. being used up in the vaporisation of water, and only *one-half per cent.* in the actual work of assimilation.

In strong diffuse light, such as that from a northern sky on a clear summer's day, the leaf has a higher "economic coefficient," using that term in relation to the permanent storage of energy in the assimilatory products. In one instance of this kind in which the total energy received by the leaf was approximately 60,000 calories per square metre per hour, it was found that 96 c.c. of water were evaporated and ·41 gram of carbohydrate was formed for the same area and time. This indicates an absorption and utilisation by the leaf of something like 95 per cent. of the incident energy, of which 2·7 per cent. has been made use of for actual work of assimilation as against 0·5 per cent. in brilliant sunshine.[1]

From what I have said previously about the effect of increased tension of carbon dioxide on the rate of assimilation, it must follow that the "efficiency" of a leaf as regards the permanent storage of energy must, *caeteris paribus*, be increased when small additions of that gas are made to the surrounding air.

In one such instance, in which the air had been enriched with carbon dioxide to the extent of about five-and-a-half times the normal amount, it was estimated that the "efficiency" of the leaf for bright sunshine was raised from 0·5 to 2·0 per cent.

Up to the present we have been regarding the efficiency of the assimilatory mechanism of a plant in reference to the *total* energy of all grades which falls upon the leaf. It is, of course, well known that the power of decomposing carbon dioxide is limited to rays of a certain refrangibility, and the researches of Timiriazeff, Engelmann and others leave little room to doubt that the rays of the spectrum which are instrumental in producing the reaction in the chloroplastids have a distinct relation to the absorption bands of the leaf-chlorophyll. By far the greater amount of the assimilatory work, probably more than 90 per cent. of it, is effected by the rays which correspond to the principal absorption band in the red, lying between wave-lengths 6500 and 6975.[2] If, therefore, we express the distribution of energy in a normal solar spectrum in the form of a curve, we have the means of approximately determining the *maximum theoretical efficiency* of a green leaf, that is to say, the maximum amount of assimilatory work which could be produced, supposing the conditions so favourable as to admit of the whole of the energy corresponding to this absorption band being stored up within the leaf.

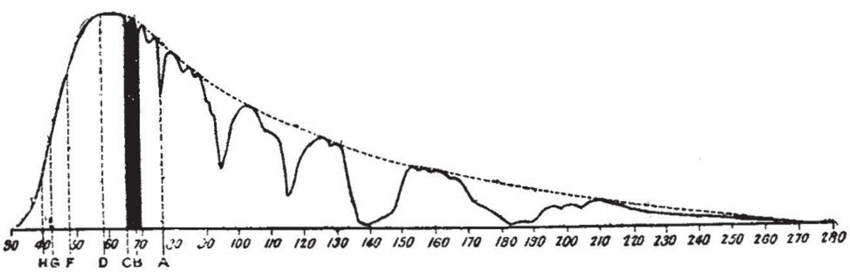

FIG. 3.

It is not without interest to get an approximate idea of this theoretical maximum.

For this purpose I have here reproduced a curve given by Prof. S. P. Langley representing the distribution of energy at the sea-level in the normal spectrum of a vertical sun shining in

[1] The principal factor which determines the amount of transpiration in a plant must be the amount of radiation falling on it. It is essential that the water-bearing mechanism should be able to keep up a good supply of water to the leaf lamina in order to prevent the temperature rising to a dangerously high point. This "safety valve" function of the transpiration current is not always sufficiently borne in mind, and we are too apt to think that the plant requires these enormous amounts of water in order to supply itself with the requisite mineral salts. The absolute necessity for the supply as a dissipator of energy will become evident by taking one or two facts into consideration. A square metre of the lamina of the leaf of a sunflower weighs about 250 grams, and its specific heat is about 0·9. We have seen that the hourly transpiration in bright sunshine may be as much as 275 c.c. per square metre, requiring the expenditure of 162,800 calories, and it therefore follows that, if the loss of water were stopped, the temperature of the leaf would rise at the rate of more than 12° C. *per minute*. In making our experiments in glazed cases it has sometimes been very interesting to watch the result of any accidental stoppage of the water-current in the leaf-stalk, and the almost instantaneous effect this has in destroying the leaf when the insolation is of high intensity.

[2] These limits are those of the band as measured by passing sunlight through the leaf itself. In an alcoholic solution of chlorophyll the band lies between λ 6400 and λ 6850. I must here express my thanks to Mr. Charles A. Schunck for having kindly undertaken to make these measurements for me.

a clear sky. The total amount of incident energy represented by the whole area of the curve is 1·7 calories per square centimetre per minute, or 1,020,000 calories per square metre per hour.

I have drawn a thick black vertical band in the red end of the spectrum corresponding in position and breadth with the principal absorption band of chlorophyll as seen in a green leaf. By integration it may be shown that the area of this part of the curve is about 6·5 per cent. of that of the whole curve, so that this value represents something like the theoretical maximum efficiency of a leaf in bright vertical sunshine, supposing the conditions could be made so favourable as to result in a complete filtering-out and utilisation of the whole of the rays of the right period for producing decomposition of carbon dioxide.

This maximum efficiency expressed in calories per square metre per hour is 66,300, corresponding to the heat of formation of about 16·5 grams of carbohydrate. Under the most favourable conditions we have employed up to the present we have not obtained a larger production than about 3·0 grams of carbohydrate per square metre per hour, or about 18 per cent. of the theoretical maximum; but this was in air containing only 16·4 parts of carbon dioxide per 10,000, which must be very far below the true optimum amount.

The brilliant discoveries of recent years on the constitution and synthesis of the carbohydrates have not brought us sensibly nearer to an explanation of the first processes of the reduction of carbon dioxide in the living plant. The hypothesis of Baeyer still occupies the position it did when it was first put forward nearly thirty years ago, although it has, it is true, received a certain amount of support from the observations of Bokorny, who found that formaldehyde can, under certain conditions, contribute to the building up of carbohydrates in the chloroplasts.

The changes which go on in the living cell are so rapid, and are of such a complex kind, that there seems little or no hope of ascertaining the nature of the first steps in the process unless we can artificially induce them under much simpler conditions.

The analogy which exists between the action of chlorophyll in the living plant and that of a *chromatic sensitiser* in a photographic plate, was, I believe, first pointed out by Captain Abney, and was more fully elaborated by Timiriazeff, who was inclined to regard chlorophyll as the sensitiser *par excellence*, since it absorbs and utilises for the assimilatory process the radiations corresponding approximately to the point of maximum energy in the normal spectrum. The view which Timiriazeff has put forward, that there is a mere physical transference of vibrations of the right period from the absorbing chlorophyll to the reacting carbon dioxide and water, is, I think, far too simple an explanation of the facts. Chromatic sensitisers have been shown to act by reason of their antecedent decomposition and not by direct transference of energy, and the same probably holds good with regard to chlorophyll, which is also decomposed by the rays which it absorbs. We must probably seek for the first and simplest stages of the assimilatory process in the interaction of the reduced constituents of the chlorophyll and the elements of carbon dioxide and water, the combinations so formed being again split up in another direction by access of energy from without.

The failure of all attempts to produce such a reaction under artificial conditions is, I think, to be accounted for by the neglect of one very important factor. We are dealing with a reaction of a highly endothermic nature, which is probably also highly *reversible*, and on this account we cannot expect any sensible accumulation of the products of change unless we employ some means for removing them from the sphere of action as fast as they are formed.

In the plant this removal is provided for by the living elements of the cell, by the chloroplast, assisted no doubt by the whole of the cytoplasm. We have here, in fact, the analogue of the *chemical sensitisers* of a photographic plate, which act as halogen absorbers, and so permit a sensible accumulation of effect on the silver salts.

When we have succeeded in finding some simple chemical means of fixing the initial products of the reduction of carbon dioxide, then, and then only, may we hopefully look forward to reproducing in the laboratory the first stages of the great synthetic process of nature on which the continuance of all life depends.

NOTES.

THE Allahabad *Pioneer Mail* understands that Mr. J. N. Tata, of Bombay, has determined to dissociate his offered endowment for a scientific research institute in India from the proposed family settlement, which was one of the original conditions, as the latter part of the scheme presented insuperable difficulties. With great generosity and public spirit Mr. Tata has declared his intention of making his offer, which amounts, it will be remembered, to some thirty lakhs of property, quite unconditional. He is now preparing, in consultation with the provisional committee, a revised scheme for submission to Government. In preparing it, he and the provisional committee will utilise all the information and advice they have received from all parts of India in response to the circulars issued some months ago, and there is good prospect of a practical plan being evolved.

THE forty-eighth annual meeting of the American Association for the Advancement of Science was held at Columbus, Ohio, on August 19–26, under the presidency of Dr. Edward Orton, of Ohio State University. There were 350 members and associates present, and 273 papers were communicated to the sections. The address of the retiring president, Prof. F. W. Putnam, of Harvard University, was published in last week's NATURE, and portions of the addresses delivered by presidents of the sections will appear at the earliest opportunity. The subjects of these addresses are:—Section of Mathematics and Astronomy, "The Fundamental Principles of Algebra," by Prof. A. Macfarlane; Section of Mechanical Science and Engineering, "Engineering Education as a Preliminary Training for Scientific Research Work," by Prof. Storm Bull; Section of Zoology, "The Importance and the Promise in the Study of the Domestic Animals," by Prof. Gage; Section of Geology and Geography, "The Devonian in Canada," by Mr. J. F. Whiteaves; Section of Physics, "The Field of Experimental Research," by Dr. Elihu Thomson; Section of Chemistry, "Definition of the Element," by Prof. F. P. Venable; Section of Botany, "The Progress and Problems of Plant Physiology," by Prof. Barnes; Section of Anthropology, "Beginnings of the Science of Prehistoric Anthropology," by Prof. Wilson. Prof. C. E. Munroe delivered a popular lecture on "Applications of Modern Electricity." New York was selected as the place of meeting next year, and Prof. R. S. Woodward, Columbia University, was nominated president.

A VERY successful congress of mining engineers was held at Teplitz, in Bohemia, on September 4–8. It was attended by 400 mining engineers from all parts of Austria and by a few representatives of other countries, Great Britain being represented by Mr. H. Bauerman, Mr. Bennett Brough and Mr. D. A. Louis. Mr. Gottfried Hüttemann, of Brüx, was elected president; Mr. J. Gleich, of Klagenfurt, and Prof. Clemens Winkler, of Freiberg, vice-presidents; and Prof. J. von Ehrenwerth, of the Przibram School of Mines, and Mr. M. Heinsius, secretaries. Papers were read by Prof. Clemens Winkler, on the history of combustion with reference to the duration of the world's coal supply; by Prof. Otto Frankl, of Prague, on suggested reforms in mining law; by Mr. H. Löcker, of Brüx, on water-inbursts in the Dux-Ossegg collieries and their influence on the Teplitz hot-springs; and by Mr. A. Bloemendal, of Vienna, on the electric transmission of power in mines. It was decided that the next mining congress should be held in Vienna in four years' time. In connection with the congress, excursions were arranged to the brown coal mines of the Brüx Coal Co. and of the Bruch Coal Co., to the Teplitz rolling mills, to the Aussig chemical works, to Edmundsklamm and other points of geological interest in the Bohemian Switzerland, and to the