"Carnot was the first to assert the law *that the ratio of the maximum mechanical effect to the whole heat expended in an expansive machine is a function solely of the two temperatures at which the heat is respectively received and emitted, and is independent of the nature of the working substance.* But his investigations, not being based on the principle of the dynamical convertibility of heat, involve the fallacy that power can be produced out of nothing.

"The merit of combining Carnot's *law*, as it is termed, with that of the convertibility of heat and power belongs to Mr. Clausius and Prof. William Thomson; and in the shape into which they have brought it, it may be stated thus: *The maximum proportion of heat converted into expansive power by any machine is a function solely of the temperatures at which heat is received and emitted by the working substance, which function for each pair of temperatures is the same for all substances in Nature.*"

None will regret the loss of Prof. Clausius more keenly than the students of the University of Bonn, where he formed a centre of attraction not only as a great investigator, but as a teacher of almost unrivalled ability. The secret of his powers as a teacher may easily be guessed from the study of his published papers and treatises. Their great characteristic is the direct insight which they give into the very heart of the physical principles under discussion. The author, while showing himself a master of mathematical methods, ever keeps the physical meaning of the symbols before the eye of the reader, and never allows his analysis to carry him away into the regions of mere mathematical ingenuity. In this he was a worthy compeer of some of our own great mathematical physicists, like Thomson and Maxwell, and the greater part of his work has the additional advantage, for the majority of students, of being effected by the aid of comparatively simple analysis.

In 1868, Prof. Clausius was elected a Foreign Member of the Royal Society, and in 1879 he was presented with the Copley Medal, the highest distinction at the disposal of the Society. He was decorated with various civil Prussian and Bavarian orders; and after the Franco-German war, during which he had volunteered to serve as caretaker of the wounded, he received the German decoration of the Iron Cross, and the French decoration of the Legion of Honour.

G. W. DE TUNZELMANN.

THE BRITISH ASSOCIATION.

Wednesday Night.

THE meeting of the British Association which opens to-night, after twenty-four years' absence, in Bath, will be the fifty-eighth. At the meeting of 1864, the President was Sir Charles Lyell, and the occasion was rendered memorable by the presence at once of Dr. Livingstone and Bishop Colenso, both at the time filling a large space in the public eye. Though a vast majority of the members of the Association would prefer to visit Bath to either Birmingham or Manchester, the latter towns possess in Owens College and the Town Hall buildings which offer greater conveniences for the meeting of a scientific Congress. In Bath the Sections will be somewhat scattered. The Physical Science Section meets at the St. James's Hall; the Mechanical Section in the Masonic Hall; the Chemical Section in the Friends' Meeting-House; Geology and Biology are housed at the Mineral Water Hospital, with the Blue Coat School for the sub-sections; Geography at the Guildhall, and Anthropology at the Grammar School; while the President's address and some of the popular lectures, as well as the concluding general meeting, will be delivered at the Drill Hall. The Mayor gives a *conversazione* to-morrow in the Assembly Rooms, and the Chairman and Local Committee give another on Tuesday. A large number of foreign visitors, especially geologists for the International Geological Congress to be held in London on the 17th inst., are expected. Amongst those already arrived are Prince Roland Bonaparte; Profs. Dufont, Gilbert, Capellini, Stephenson, Lory, von Koenen, Frazer, Kalkowsky, and Waagen.

The retiring President, Sir Henry Roscoe, M.P., F.R.S., in introducing Sir Frederick Bramwell, the President-Elect, spoke as follows:—

"My Lords, Ladies, and Gentlemen,—Four-and-twenty eventful years in the history of science have passed away since the British Association last visited the city of Bath. Those of us who were present here in 1864 will not soon forget that memorable meeting. It was presided over, as you all will remember, by that veteran geologist, that great forerunner of a new science of life, Sir Charles Lyell, of beloved and venerated memory. Yes, ladies and gentlemen, it was he who prepared the way by his recognition of the true history of our globe for the even more illustrious Darwin. It was he who pointed out that the causes which have modified the earth's crust in the past are, for the most part, those which are now changing the face of Nature. Lyell was a typical example of the expositor of Nature's most secret processes. His work was that of an investigator of science pure and undefiled, and as such, his life and labours stand for ever as an example to all those who love Science for her own sake.

"But the far-seeing founders of this our British Association were as fully alive to the fact as we, in perhaps our more utilitarian age, can be, that, just as man does not live by bread alone, so it is not only by purely scientific discovery that the nations progress, or that science advances. They knew as well as we do that to benefit humanity the application of the results of scientific research to the great problems of every-day life is a necessity. Hence our founders, whilst acknowledging that the basis of our Association can only be securely laid upon the principles of pure science in its various branches, recognized the importance of the application of those principles in the establishment of a Section which should represent one of the most remarkable outcomes of the activity and force of the nation—a Section of Engineering. It is therefore meet and right that in due proportion this great department of our scientific edifice—a department which, perhaps, more than any other, has effected a revolution in our modern social system—should be represented in our Presidential chair.

"Twenty-four years ago it was pure science that we honoured in Sir Charles Lyell: to-day it is applied science to which we show our respect in the person of Sir Frederick Bramwell. It would ill become me, engaged as I have been in the study of subjects far removed from those which fill the life of an active and successful engineer, to venture on this occasion on a eulogium upon the work of my successor, still less is it in my mind to draw any comparison as to the relative importance to be attached to the work of the investigator, such as Lyell, and to that of him who applies the researches of others to the immediate wants of mankind. It is enough for me, as I am sure it will be for you, to remember that both classes of men are needed for the due advancement of science, and to rejoice that as in former years the names of Fairbairn, of Armstrong, and of Hawkshaw, have adorned our list of Presidents, so in the present instance, this branch of science, which represents lines of human activity rendered illustrious by the labours of many great Englishmen, is to-day represented by our eminent President.

"I have the honour of requesting Sir Frederick Bramwell to take the chair, and to favour us with the Presidential address."

INAUGURAL ADDRESS BY SIR FREDERICK BRAMWELL, D.C.L., F.R.S.; M.INST.C.E., PRESIDENT.

THE late Lord Iddesleigh delighted an audience, for a whole evening, by an address on "Nothing." Would that I had his talents, and could discourse to you as charmingly as he did to his audience, but I dare not try to talk about "Nothing." I do, however, propose, as one of the two sections of my address, to discourse to you on the importance of the "Next-to-Nothing." The other section is far removed from this microscopic quantity, as it will embrace the "Eulogy of the Civil Engineer," and will point out the value to science of his works."

I do not intend to follow any system in dealing with these two sections. I shall not even do as Mr. Dick, in "David Copperfield," did—have two papers, to one of which it was suggested he should confine his memorial and his observations as to King Charles's head. The result is, you will find, that the importance of the next-to-nothing, and the laudation of the civil engineer, will be mixed up in the most illogical and haphazard way, throughout my address. I will leave to such of you as are of orderly minds, the task of rearranging the subjects as you see fit, but I trust—arrangement or no arrangement—that by the time I have brought my address to a conclusion I shall have convinced you that there is no man who more thoroughly appreciates the high importance of the "next-to-nothing" than the civil engineer of the present day, the object of my eulogy this evening.

If I may be allowed to express the scheme of this address in modern musical language, I will say that the "next-to-nothing" "motive" will commonly usher in the "praise-song" of the civil engineer; and it seems to me will do this very fitly, for in many cases it is by the patient and discriminating attention paid to the effect of the "next-to-nothing" that the civil engineer of the present day has achieved some of the labours of which I now wish to speak to you.

An Association for the Advancement of Science is necessarily one of such broad scope in its objects, and is so thoroughly catholic as regards science, that the only possible way in which it can carry out those objects at all is to segregate its members into various subsidiary bodies, or sections, engaged on particular branches of science. Even when this division is resorted to, it is a hardy thing to say that every conceivable scientific subject can be dealt with by the eight Sections of the British Association. Nevertheless, as we know, for fifty-seven years the Association has carried on its labours under Sections, and has earned the right to say that it has done good service to all branches of science.

Composed, as the Association is, of a union of separate Sections, it is only right and according to the fitness of things that, as time goes on, your Presidents should be selected, in some sort of rotation, from the various Sections. This year it was felt, by the Council and the members, that the time had once more arrived when Section G—the Mechanical Section—might put forward its claim to be represented in the Presidency; the last time on which a purely engineering member filled the chair having been at Bristol in 1875, when that position was occupied by Sir John Hawkshaw. It is true that at Southampton, in 1882, our lamented friend, Sir William Siemens, was President, and it is also true that he was a most thorough engineer and representative of Section G; but all who knew his great scientific attainments will probably agree that on that occasion it was rather the Physical Section A which was represented, than the Mechanical Section G.

I am aware it is said Section G does not contribute much to pure science by original research, but that it devotes itself more to the application of science. There may be some foundation for this assertion, but I cannot refrain from the observation that, when engineers such as Siemens, Rankine, Sir William Thomson, Fairbairn, or Armstrong, make a scientific discovery, Section A says it is made, not in the capacity of an engineer, and therefore does not appertain to Section G, but in the capacity of a physicist, and therefore appertains to Section A—an illustration of the danger of a man's filling two positions, of which the composite Prince-Bishop is the well-known type. But I am not careful to labour this point, or even to dispute that Section G does not do much for *original* research. I do not agree it is a fact, but, for the purposes of this evening, I will concede it to be so. But what then? This Association is for the "Advancement of Science"—the *advancement*, be it remembered; and I wish to point out to you, and I trust I shall succeed in establishing, that for the *advancement* of science it is absolutely necessary there should be the *application* of science, and that, therefore, the Section, which as much as any other (or, to state the fact more truly, which more than any other) in the Association *applies* science, is doing a very large share of the work of *advancing* science, and is fully entitled to be periodically represented in the Presidency of the whole Association.

I trust also I shall prove to you that applications of science, and discoveries in pure science, act and react the one upon the other. I hope in this to carry the bulk of my audience with me, although there are some, I know, whose feelings, from a false notion of respect for science, would probably find vent in the "toast" which one has heard in another place—this "toast" being attributed to the pure scientist—"Here's to the latest scientific discovery: may it never do any good to anybody!"

To give an early illustration of this action and reaction, which I contend occurs, take the well-worn story of Galileo, Torricelli, and the pump-maker. It is recorded that Galileo first, and his pupil Torricelli afterwards, were led to investigate the question of atmospheric pressure, by observing the failure of a pump to raise water by "suction," above a certain level. Perhaps you will say the pump-maker was not applying science, but was working without science. I answer, he was unknowingly applying it, and it was from that which arose in this unconscious application that the mind of the pure scientist was led to investigate the subject, and thereupon to discover the primary fact of the pressure of the atmosphere, and the subsidiary facts which attend thereon. It may appear to many of you that the question of the exercise of pressure by the atmosphere should have been so very obvious, that but little merit ought to have accrued to the discoverer; and that the statement, once made, must have been accepted almost as a mere truism. This was, however, by no means the case. Sir Kenelm Digby, in his "Treatise on the Nature of Bodies," printed in 1658, disputes the proposition altogether, and says, in effect, he is quite sure the failure of the pump to raise water was due to imperfect workmanship of some kind or description, and had nothing to do with the pressure of the air; and that there is no reason why a pump should not suck up water to any height. He cites the boy's sucker, which, when applied to a smooth stone, will lift it, and he says the reason why the stone follows the sucker is this. Each body must have some other body in contact with it. Now, the stone being in contact with the sucker, there is no reason why that contact should be broken up, for the mere purpose of substituting the contact of another body, such as the air. It seems pretty clear, therefore, that even to an acute and well-trained mind, such as that of Sir Kenelm Digby, it was by no means a truism, and to be forthwith accepted, when once stated, that the rise of water on the "suction side" of a pump was due to atmospheric pressure. I hardly need point out that the pump-maker should have been a member of "G." Galileo and Torricelli, led to reflect by what they saw, should have been members of "A" of the then "Association for the Advancement of Science."

But, passing away from the question of the value of the application of science of a date some two and a half centuries ago, let us come a little nearer to our own times.

Electricity (known in its simplest form to the Greeks by the results arising from the friction of amber, and named therefrom; afterwards produced from glass cylinder machines, or from plate machines; and produced a century ago by the "influence" machine) remained, as did the discoveries of Volta and Galvani, the pursuit of but a few, and even the brilliant experiments of Davy did not suffice to give very great impetus to this branch of physical science.

Ronalds, in 1823, constructed an electric telegraph. In 1837 the first commercial use was made of the telegraph, and from that time electrical science received an impulse such as it had never before experienced. Further scientific facts were discovered; fresh applications were made of these discoveries. These fresh applications led to renewed vigour in research, and there was the action and reaction of which I have spoken. In the year 1871 the Society of Telegraph-Engineers was established. In the year 1861 our own Association had appointed a Committee to settle the question of electrical standards of resistance, which Committee, with enlarged functions, continued its labours for twenty years, and of this Committee I had the honour of being a member. The results of the labours of that Committee endure (somewhat modified, it is true), and may be pointed to as one of the evidences of the value of the work done by the British Association. Since Ronalds's time, how vast are the advances which have been made in electrical communication of intelligence, by land lines, by submarine cables all over the world, and by the telephone! Few will be prepared to deny the statement, that pure electrical science has received an enormous impulse, and has been advanced by the commercial application of electricity to the foregoing, and to purposes of lighting. Since this latter application, scores, I may say hundreds, of acute minds have been devoted to electrical science, stimulated thereto by the possibilities and probabilities of this application.

In this country, no doubt, still more would have been done if the lighting of districts from a central source of electricity had not been, since 1882, practically forbidden by the Act passed in that year. This Act had in its title the facetious statement that it was "to facilitate electrical lighting"—although it is an Act which, even modified as it has been this year, is still a great discouragement of free enterprise, and a bar to progress. The other day a member of the House of Commons was saying to me: "I think it is very much to our discredit in England that we should have allowed ourselves to be outrun in the distribution of electric lighting to houses, by the inhabitants of the United States, and by those of other countries." Looking upon him as being one of the authors of the "facetious" Act, I thought it pertinent to quote the case of the French parricide, who, being asked what he has to say in mitigation of punishment, pleads, "Pity a poor orphan"—the parricide and the legislator being both of them authors of conditions of things which they affect to deplore. I will say no more on this subject, for I feel that it would not be right to take advantage of my position here to-night to urge political economy views, which should be reserved for Section F. I will merely, and as illustrative of my views of the value of the application of science to science itself, say there is no branch of physics pursued with more zeal and with more happy results than that of electricity, with its allies, and there is no branch of science towards which the public look with greater hope of practical benefits; a hope that, I doubt not, will be strengthened after we have had the advantage of hearing one of the ablest followers of that science, Prof. Ayrton, who, on Friday next, has been good enough to promise to discourse on "The Electrical Transmission of Power."

One of the subjects which, as much as (or probably more than) any other, occupies the attention of the engineer, and therefore of Section G, is that of (the so-called) prime movers, and I will say boldly that, since the introduction of printing by the use of movable type, nothing has done so much for civilization as the development of these machines. Let us consider these prime movers—and, first, in the comparatively humble function of replacing that labour which might be performed by the muscular exertion of human beings, a function which at one time was looked upon by many kindly but short-sighted men as taking the bread out of the mouth of the labourer (as it was called), and as being therefore undesirable. I remember revisiting my old schoolmaster, and his saying to me, shaking his head: "So you have gone the way I always feared you would, and are making things of iron and brass, to do the work of men's hands."

It must be agreed that all honest and useful labour is honourable, but when that labour can be carried out without the exercise of any intelligence, one cannot help feeling that the result is likely to be intellectually lowering. Thus it is a sorry thing to see unintelligent labour, even although that labour be useful. It is but one remove from unintelligent labour which is not useful; that kind of labour generally appointed (by means of the tread-wheel or the crank) as a punishment for crime. Consider even the honourable labour (for it is useful, and it is honest) of the man who earns his livelihood by turning the handle of a crane, and compare this with the labour of a smith, who, while probably developing more energy by the use of his muscles, than is developed by the man turning the crane-handle, exercises at the same time the powers of judgment, of eye, and of hand in a manner which I never see without my admiration being excited. I say that the introduction of prime movers as a mere substitute for unintelligent manual labour is in itself a great aid to civilization and to the raising of humanity, by rendering it very difficult, if not impossible, for a human being to obtain a livelihood by unintelligent work—the work of the horse in the mill, or of the turnspit.

But there are prime movers and prime movers—those of small dimensions, and employed for purposes where animal power or human power might be substituted, and those which attain ends that by no conceivable possibility could be attained at all by the exertion of muscular power.

Compare a galley, a vessel propelled by oars, with the modern Atlantic liner; and first let us assume that prime movers are non-existent, and that this vessel is to be propelled galley-fashion. Take her length as some 600 feet, and assume that place be found for as many as 400 oars on each side, each oar worked by three men, or 2400 men; and allow that six men under these conditions could develop work equal to one horse-power: we should have 400 horse-power. Double the number of men, and we should have 800 horse-power, with 4800 men at work, and at least the same number in reserve, if the journey is to be carried on continuously. Contrast the puny result thus obtained with the 19,500 horse-power given forth by a large prime mover of the present day, such a power requiring, on the above mode of calculation, 117,000 men at work, and 117,000 in reserve; and these to be carried in a vessel less than 600 feet in length. Even if it were possible to carry this number of men in such a vessel, by no conceivable means could their power be utilized so as to impart to it a speed of twenty knots an hour.

This illustrates how a prime mover may not only be a mere substitute for muscular work, but may afford the means of attaining an end that could not by any possibility be attained by muscular exertion, no matter what money was expended or what galley-slave suffering was inflicted.

Take again the case of a railway locomotive. From 400 to 600 horse-power developed in an implement which, even including its tender, does not occupy an area of more than fifty square yards, and that draws us at sixty miles an hour. Here again, the prime mover succeeds in doing that which no expenditure of money or of life could enable us to obtain from muscular effort.

To what, and to whom, are these meritorious prime movers due? I answer: To the application of science, and to the labours of the civil engineer, using that term in its full and proper sense, as embracing all engineering other than military. I am, as you know, a civil engineer, and I desire to laud my profession and to magnify mine office; and I know of no better means of doing this than by quoting to you the definition of "civil engineering," given in the Charter of the Institution of Civil Engineers—namely, that it is "the art of directing the great sources of power in Nature for the use and convenience of man." These words are taken from a definition or description of engineering given by one of our earliest scientific writers on the subject, Thomas Tredgold, who commences that description by the words above quoted, and who, having given various illustrations of the civil engineer's pursuits, introduces this pregnant sentence:—

"This is, however, only a brief sketch of the objects of civil engineering, the real extent to which it may be applied is limited only by the progress of science; its scope and utility will be increased with every discovery in philosophy, and its resources with every invention in mechanical or chemical art, since its bounds are unlimited, and equally so must be the researches of its professors."

"The art of directing the great sources of power in Nature for the use and convenience of man." Among all secular pursuits, can there be imagined one more vast in its scope, more beneficent, and therefore more honourable, than this? There are those, I know—hundreds, thousands—who say that such pursuits are not to be named as on a par with those of literature; that there is nothing ennobling in them; nothing elevating; that they are of the earth earthy; are mechanical, and are unintellectual, and that even the mere bookworm, who, content with storing his own mind, neither distributes those stores to others nor himself originates, is more worthily occupied than is the civil engineer.

I deny this altogether, and, while acknowledging, with gratitude, that, in literature, the masterpieces of master minds have afforded, and will afford, instruction, delight, and solace for all generations so long as civilization endures, I say that the pursuits of civil engineering are worthy of occupying the highest intelligence, and that they are elevating and ennobling in their character.

Remember the kindly words of Sir Thomas Browne, who said, when condemning the uncharitable conduct of the mere bookworm, "I make not, therefore, my head a grave, but a treasure of knowledge, and study not for mine own sake only, but for those who study not for themselves." The engineer of the present day finds that he must not make his "head a grave," but that, if he wishes to succeed, he must have, and must exercise, scientific knowledge; and he realizes daily the truth that those who are to come after him must be trained in science, so that they may readily appreciate the full value of each scientific discovery as it is made. Thus the application of science by the engineer not only stimulates those who pursue science, but adds him to their number.

Holding, as I have said I do, the view that he who displaces unintelligent labour is doing good to mankind, I claim for the unknown engineer who, in Pontus, established the first water-wheel of which we have a record, and for the equally unknown engineer who first made use of wind for a motor, the title of pioneers in the raising of the dignity of labour, by compelling the change from the non-intelligent to the intelligent.

With respect to these motors—wind and water—we have two proverbs which discredit them: "Fickle as the wind," "Unstable as water."

Something more trustworthy was needed—something that we were sure of having under our hands at all times. As a result, science was applied, and the "fire" engine, as it was first called, the "steam" engine, as it was re-named, a form of "heat" engine, as we now know it to be, was invented.

Think of the early days of the steam-engine—the pre-Watt days. The days of Papin, Savory, Newcomen, Smeaton! Great effects were produced, no doubt, as compared with no fire engine at all; effects so very marked as to extort from the French writer, Belidor, the tribute of admiration he paid to the "fire" engine erected at the Fresnes Colliery by English engineers. A similar engine worked the pumps in York Place (now the Adelphi) for the supply of water to portions of London. We have in his work one of the very clearest accounts, illustrated by the best engravings (absolute working drawings), of the engine which had excited his admiration. These drawings show the open-topped cylinder, with condensation taking place below the piston, but with the valves worked automatically.

It need hardly be said that, noteworthy as such a machine was, as compared with animal power, or with wind or water motors, it was of necessity a most wasteful instrument as regards fuel. It is difficult to conceive in these days how, for years, it could have been endured that at each stroke of the engine the chamber that was to receive the steam at the next stroke was carefully cooled down beforehand by a water injection.

Watt, as we know, was the first to perceive, or, at all events, to cure, this fundamental error which existed prior to his time in the "fire" engine. To him we owe condensation in a separate vessel, the doing away with the open-topped cylinder, and the making the engine double-acting; the parallel motion; the governor; and the engine-indicator, by which we have depicted for us the way in which the work is being performed within the cylinder. To Watt, also, we owe that great source of economic working—the knowledge of the expansive force of steam; and to his prescience we owe the steam-jacket, without which expansion, beyond certain limits, is practically worthless. I have said "prescience"—fore-knowledge—but I feel inclined to say that, in this case, prescience may be rendered "pre-Science," for I think that Watt *felt* the utility of the steam-jacket, without being able to say on what ground that utility was based.

I have already spoken in laudatory terms of Tredgold, as being one of the earliest of our scientific engineering writers, but, as regards the question of steam-jacketing, Watt's prescience was better than Tredgold's science, for the latter condemns the steam-jacket, as being a means whereby the cooling surfaces are enlarged, and whereby, therefore, the condensation is increased.

I think it is not too much to say that engineers who, since Watt's days, have produced machines of such marvellous power—and, compared with the engines of Watt's days, of so great economy—have, so far as principles are concerned, gone upon those laid down by Watt. Details of the most necessary character—necessary to enable those principles to be carried out—have, indeed, been devised since the days of Watt. Although it is still a very sad confession to have to make, that the very best of our steam-engines only utilizes about one-sixth of the work which resides (if the term may be used) in the fuel that is consumed, it is, nevertheless, a satisfaction to know that great economical progress has been made, and that the 6 or 7 pounds of fuel per horse-power per hour consumed by the very best engines of Watt's days, when working with the aid of condensation, is now brought down to about one-fourth of this consumption; and this in portable engines, for agricultural purposes, working without condensation—engines of small size, developing only 20 horse-power; in such engines the consumption has been reduced to as little as 1·85 pound per brake horse-power per hour, equal to 1·65 pound per indicated horse-power per hour, as was shown by the trials at the Royal Agricultural Society's meeting at Newcastle last year—trials in which I had the pleasure of participating.

In these trials Mr. William Anderson, one of the Vice-Presidents of Section G, and I were associated, and, in making our report of the results, we adopted the balance-sheet system, which I suggested and used so long ago as 1873 (see vol. lii., pp. 154 and 155, of the Minutes of Proceedings of the Institution of Civil Engineers"), and to which I alluded in my address as President of Section G at Montreal.

I have told you that the engineer of the present day appreciates the value of the "next-to-nothings." There is an old house-keeping proverb that, if you take care of the farthings and the pence, the shillings and the pounds will take care of themselves. Without the balance-sheet one knows that for the combustion of 1 pound of coal, the turning into steam of a given quantity of water at a given pressure is obtained. It is seen, at once, that the result is much below that which should be had, but to account for the deficiency is the difficulty. The balance-sheet, dealing with the most minute sources of loss—the farthings and the pence of economic working—brings you face to face with these, and you find that improvement must be sought in paying attention to the "next-to-nothings."

Just one illustration. The balance-sheet will enable you at a glance to answer this among many important questions: Has the fuel been properly burnt—with neither too much air, nor too little?

At the Newcastle trials our knowledge as to whether we had the right amount of air for perfect combustion was got by an analysis of the waste gases, taken continuously throughout the whole number of hours' run of each engine, affording, therefore, a fair average. The analysis of any required portion of gases thus obtained was made in a quarter of an hour's time by the aid of the admirable apparatus invented by Mr. Stead, and, on the occasion to which I refer, manipulated by him. In one instance an excess of air had been supplied, causing a percentage of loss of 6·34. In the instance of another engine there was a deficiency of air, resulting in the production of carbonic oxide, involving a loss of 4 per cent. The various percentages of loss, of which each one seems somewhat unimportant, in the aggregate amounted to 28 per cent., and this with one of the best boilers This is an admirable instance of the need of attention to apparently small things.

I have already said that we now know the steam-engine is really a heat engine. At the York meeting of our Association I ventured to predict that, unless some substantive improvement were made in the steam-engine (of which improvement, as yet, we have no notion), I believed its days, for small powers, were numbered, and that those who attended the centenary of the British Association in 1931 would see the present steam-engines in museums, treated as things to be respected, and of antiquarian interest to the engineers of those days, such as are the open-topped steam cylinders of Newcomen and of Smeaton to ourselves. I must say I see no reason, after the seven years which have elapsed since the York meeting, to regret having made that prophecy, or to desire to withdraw it.

The working of heat engines, without the intervention of the vapour of water, by the combustion of the gases arising from coal, or from coal and from water, is now not merely an established fact, but a recognized and undoubted, commercially economical means of obtaining motive power. Such engines, developing from 1 to 40 horse-power, and worked by the ordinary gas supplied by the gas mains, are in most extensive use in printing-works, hotels, clubs, theatres, and even in large private houses, for the working of dynamos to supply electric light. Such engines are also in use in factories, being sometimes driven by the gas obtained from "culm" and steam, and are giving forth a horse-power for, it is stated, as small a consumption as 1 pound of fuel per hour.

It is hardly necessary to remind you—but let me do it—that, although the saving of half a pound of fuel per horse-power appears to be insignificant, when stated in that bald way, one realizes that it is of the highest importance when that half-pound turns out to be 33 per cent. of the whole previous consumption of one of those economical engines to which I have referred.

The gas-engine is no new thing. As long ago as 1807 a M. de Rivaz proposed its use for driving a carriage on ordinary roads. For anything I know, he may not have been the first proposer. It need hardly be said that in those days he had not illuminating gas to resort to, and he proposed to employ hydrogen. A few years later a writer in *Nicholson's Journal*, in an article on "Flying Machines," having given the correct statement that all that is needed to make a successful machine of this description is to find a sufficiently light motor, suggests that the direction in which this may be sought is the employment of illuminating gas, to operate by its explosion on the piston of an engine. The idea of the gas-engine was revived, and formed the subject of a patent by Barnett in the year 1838. It is true this gentleman did not know very much about the subject, and that he suggested many things which, if carried out, would have resulted in the production of an engine which could not have worked; but he had an alternative proposition which would have worked.

Again, in the year 1861, the matter was revived by Lenoir, and in the year 1865 by Hugon, both French inventors. Their engines obtained some considerable amount of success and notoriety, and many of them were made and used; but in the majority of cases they were discarded as wasteful and uncertain. The Institution of Civil Engineers, for example, erected a Lenoir in the year 1868, to work the ventilating fan, but after a short time they were compelled to abandon it and to substitute an hydraulic engine.

At the present time, as I have said, gas-engines are a great commercial success, and they have become so by the attention given to small things, in popular estimation—to important things, in fact, with which, however, I must not trouble you. Messrs. Crossley Brothers, who have done so much to make the gas-engine the commercial success that it is, inform me that they are prosecuting improvements in the direction of attention to detail, from which they are obtaining greatly improved results.

But, looking at the wonderful petroleum industry, and at the multifarious products which are obtained from the crude material, is it too much to say that there is a future for motor engines, worked by the vapour of some of the more highly volatile of these products—true vapour—not a gas, but a condensable body, capable of being worked over and over again? Numbers of such engines, some of as much as 4 horse-power, made by Mr. Yarrow, are now running, and are apparently giving good results; certainly excellent results as regards the compactness and lightness of the machinery. For boat purposes they possess the great advantage of being rapidly under way. I have seen one go to work within two minutes of the striking of the match to light the burner.

Again, as we know, the vapour of this material has been used as a gas in gas-engines, the motive power having been obtained by direct combustion.

Having regard to these considerations, was I wrong in predicting that the heat engine of the future will probably be one independent of the vapour of water? And, further, in these days of electrical advancement, is it too much to hope for the direct production of electricity from the combustion of fuel?

As the world has become familiar with prime movers, the desire for their employment has increased. Many a householder could find useful occupation for a prime mover of ¼ or ½ horse-power, working one or two hours a day; but the economical establishment of a steam-engine is not possible until houses of very large dimensions are reached, where space exists for the engine, and where, having regard to the amount of work to be done, the incidental expenses can be borne. Where this cannot be, either the prime mover, with the advantages of its use, must be given up as a thing to be wished for, but not to be procured, or recourse must be had to some other contrivance—say to the laying on of power, in some form or another, from a central source.

I have already incidentally touched upon one mode of doing this—namely, the employment of illuminating gas, as the working agent in the gas-engine; but there are various other modes, possessing their respective merits and demerits—all ingenious, all involving science in their application, and all more or less in practical use—such as the laying on of special high-pressure water, as is now being extensively practised in London, in Hull, and elsewhere. Water at 700 pounds pressure per inch is a most convenient mode of laying on a large amount of power, through comparatively small pipes. Like electricity, where, when a high electromotive force is used, a large amount of energy may be sent through a small conductor, so with water, under high pressure, the mains may be kept of reasonable diameters, without rendering them too small to transmit the power required through them

Power is also transmitted by means of compressed air, an agent which, on the score of its ability to ventilate, and of its cleanliness, has much to recommend it. On the other hand, it is an agent which, having regard to the probability of the deposition of moisture in the form of "snow," requires to be worked with judgment.

Again, there is an alternative mode for the conveyance of power by the exhaustion of air—a mode which has been in practical use for over sixty years.

We have also the curious system pursued at Schaffhausen, where quick-running ropes are driven by turbines, these being worked by the current of the River Rhine; and at New York, and in other cities of the United States, steam is laid on under the streets, so as to enable domestic steam-engines to be worked, without the necessity of a boiler, a stoker, or a chimney, the steam affording also means of heating the house when needed.

Lastly, there is the system of transmitting power by electricity, to which I have already adverted. I was glad to learn, only the other day, that there was every hope of this power being applied to the working of an important subterranean tramway.

These distributions from central sources need, as a rule, statutory powers to enable the pipes or wires to be placed under the roads; and, following the deplorable example of the Electrical Facilities Act, it is now the habit of the enlightened Corporation and the enterprising town clerk of most boroughs to say to capitalists who are willing to embark their capital in the plant for the distribution of power from a central source—for their own profit no doubt, but also, no doubt, for the good of the community—"We will oppose you in Parliament, unless you will consent that, at the end of twenty-one years, we may acquire compulsorily your property, and may do so, if it turns out to be remunerative, without other payment than that for the mere buildings and plant at that time existing." This is the way English enterprise is met, and then English engineers are taunted, by Englishmen—often by the very men who have had a share in making this "boa-constrictor" of a "Facilities Act"—that their energy is not to be compared with that which is to be found in the United States and other countries. Again, however, I must remember that I am not addressing Section F.

There is one application of science, by engineers, which is of extreme beauty and interest, and that cannot be regarded with indifference by the agriculturists of this country. I allude to the heat-withdrawing engines (I should like to say, "cold-producers," but I presume, if I did, I should be criticized), which are now so very extensively used for the importation of fresh meat, and for its storage when received here. It need hardly be said, that that which will keep cool and sweet the carcasses of sheep will equally well preserve milk, and many other perishable articles of food. We have in these machines daily instances that, if you wish to make a ship's hold cold, you can do it by burning a certain quantity of coals—a paradox, if ever there was one.

In this climate of ours, where the summer has been said to consist of "three hot days and a thunderstorm," there is hardly need to make a provision for cooling our houses, although there is an undoubted need for making a provision to heat them. Nevertheless, those of us who have hot-water heating arrangements for use in the winter would be very glad indeed if, without much trouble or expense, they could turn these about, so as to utilize them for cooling their houses in summer. Mr. Loftus Perkins, so well known for his labours in the use of very high-pressure steam (600 to 1000 pounds on the inch), and also so well known for those most useful high-pressure warming arrangements which, without disfiguring our houses by the passage of large pipes, keep them in a state of warmth and comfort throughout the winter, has lately taken up the mode of, I will say it, producing "cold" by the evaporation of ammonia, and, by improvements in detail, has succeeded in making an apparatus which, without engine or pumps, produces "cold" for some hours in succession, and requires, to put it in action, the preliminary combustion of only a few pounds of coke or a few feet of gas.

As I have said, our climate gives us but little need to provide or employ apparatus to cool our houses, but one can well imagine that the Anglo-Indian will be glad to give up his punkah for some more certain, and less draughty, mode of cooling.

I now desire to point out how, as the work of the engineer grows, his needs increase. New material, or better material of the old kind, has to be found to enable him to carry out these works of greater magnitude. At the beginning of this century, stone, brick, and timber were practically the only materials employed for that which I may call standing engineering work—*i.e.* buildings, bridges, aqueducts, and so on—while timber, cast iron, and wrought iron were for many years the only available materials for the framing and principal parts of moving machines and engines, with the occasional use of lead for the pipes and of copper for pipes and for boilers.

As regards the cast iron, little was known of the science involved (or that ought to be involved) in its manufacture. It was judged of by results. It was judged of largely by the eye. It was "white," it was "mottled," it was "gray." It was known to be "fit for refining," fit for "strong castings," or fit for castings in which great fluidity in the molten metal was judged to be of more importance than strength in the finished casting. With respect to wrought iron, it was judged of by its results also. It was judged of by the place of its manufacture—but when the works of the district were unknown, the iron, on being tested, was classed as "good fibrous," although some of the very best was "steel-like," or "bad," "hot-short," or "cold-short." A particular district would produce one kind of iron, another district another kind of iron. The ore, the flux, and the fuel were all known to have influence, but to what extent was but little realized; and if there came in a new ore, or a new flux, it might well be that for months the turn-out of the works into which these novelties had been introduced would be prejudiced. Steel again—that luxury of the days of my youth—was judged by the eye. The wrought bars, made into "blister" steel by "cementation," were broken, examined, and grouped accordingly. Steel was known, no doubt, to be a compound of iron and carbon, but the importance of exactness in the percentage was but little understood, nor was it at all understood how the presence of comparatively small quantities of foreign matter might necessitate the variation of the proportions of carbon. The consequence was that anomalous results every now and then arose to confound the person who had used the steel, and, falsifying the proverb "true as steel," steel became an object of distrust. Is it too much to say that Bessemer's great invention of steel made by the "converter," and that Siemens's invention of the open-hearth process, reacted on pure science, and set scientific men to investigate the laws which regulate the union of metals and of metalloids? and that the labours of these scientific men have improved the manufacture, so that steel is now thoroughly and entirely trusted? By its aid engineering works are accomplished which, without that aid, would have been simply impossible. The Forth Bridge, the big gun, the compound armour of the ironclad with its steel face, the projectile to pierce that steel face—all equally depend upon the "truth" of steel as much as does the barely visible hair-spring of the chronometer which enables the longitude of the ship in which it is carried to be ascertained. Now, what makes the difference between trustworthy and untrustworthy steel for each particular purpose? Something which, until our better sense comes to our aid, we are inclined to look upon as ridiculously insignificant—a "next-to-nothing." Setting extraneous ingredients aside, and considering only the union of iron and carbon, the question whether there shall be added or deducted one-tenth of 1 per cent. (pardon my clumsy way of using the decimal system) of carbon is a matter of great importance in the resulting quality of the steel. This is a striking practical instance of how apparently insignificant things may be of the highest importance. The variation of this fraction of a percentage may render your boiler steel untrustworthy, may make the difference between safety in a gun and danger in a gun, and may render your armour-piercing projectile unable to pierce even the thinnest wrought-iron armour.

While thus brought incidentally to the subject of guns, let me derive from it another instance of the value of small things. I have in my hand a piece of steel ribbon. It is probable that only those who are near to me can see it. Its dimensions are one-fourth by one-sixteenth of an English inch, equal to an area of one sixty-fourth of a square inch. This mode of stating the dimensions I use for the information of the ladies. To make it intelligible to my scientific friends, I must tell them that it is approximately ·00637 of a metre by approximately ·00159 of a metre, and that its sectional area is ·0000101283 (also approximately) of a square metre. This insignificant (and speaking in reference to the greater number of my audience), practically invisible piece of material—that I can bend with my hand, and even tie into knots—is, nevertheless, not to be despised. By it one reinforces the massive and important-looking A-tube of a 9·2-inch gun, so that from that tube can be projected with safety a projectile weighing 380 pounds at a velocity, when leaving the muzzle, of between one-third and one-half of a mile in a second, and competent to traverse nearly 12½ miles before it touches the ground. It may be said, "What is the use of being able to fire a projectile to a distance which commonly is invisible (from some obstacle or another) to the person directing the gun?" I will suggest to you a use. Imagine a gun of this kind placed by some enemy who, unfortunately, had invaded us, and had reached Richmond. He has the range-table for his gun; he, of course, is provided with our Ordnance maps, and he lays and elevates the gun at Richmond, with the object of striking, say, the Royal Exchange. Suppose he does not succeed in his exact aim. The projectile goes 100 yards to one side or to the other; or it falls 250 yards short, or passes 250 yards over; and it would be "bad shooting" indeed, in these days, if nearly every projectile which was fired did not fall somewhere within an area such as this. In this suggested parallelogram of 100,000 square yards, or some 20 acres, there is some rather valuable property; and the transactions which are carried on are not unimportant. It seems to me that business would not be conducted with that calmness and coolness which are necessary for success, if, say, every five minutes, a 380-pound shell fell within this area, vomiting fire, and scattering its walls in in hundreds of pieces, with terrific violence, in all directions. Do not suppose I am saying that similar effects cannot be obtained from a gun where wire is not employed. They can be. But my point is, that they can also be obtained by the aid of the insignificant thing which I am holding up at this moment—this piece of steel ribbon, which looks more suitable for the framework of an umbrella.

I have already spoken to you, when considering steel as a mere alloy of iron and carbon, as to the value of even a fraction of 1 per cent. of the latter; but we know that in actual practice steel almost always contains other ingredients. One of the most prominent of these is manganese. It had for years been used, in quantities varying from a fraction of 1 per cent. up to 2·5 per cent., with advantages as regards ductility, and as regards its ability to withstand forging. A further increase was found not to augment the advantage: a still further increase was found to diminish it; and here the manufacturer stopped, and, so far as I know, the pure scientist stopped, on the very reasonable ground that the point of increased benefit appeared to have been well ascertained, and that there could be no advantage in pursuing an investigation which appeared only to result in decadence. But this is another instance of how the application of science reacts in the interests of pure science itself. One of our steel manufacturers, Mr. Hadfield, determined to pursue this apparently barren subject, and in doing so discovered this fact—that, while with the addition of manganese in excess of the limit before stated, and up to as much as 7 per cent., deterioration continued, after this latter percentage was passed improvement again set in.

Again, the effects of the addition of even the very smallest percentages of aluminium upon the steel with which it may be alloyed are very striking and very peculiar, giving to the steel alloy thus produced a very much greater hardness, and enabling it to take a much brighter and more silver-like polish. Further, the one-twentieth part of 1 per cent. of aluminium, when added to molten wrought iron, will reduce the fusing-point of the whole mass some 500°, and will render it extremely fluid, and thus enable wrought iron (or what are commercially known as "mitis" castings of the most intricate character) to be produced.

No one has worked more assiduously at the question of the effect of the presence of minute quantities, even traces, of alloys with metals than Prof. Roberts-Austen, and he appears, by his experiments, to be discovering a general law, governing the effect produced by the mixture of particular metals, so that, in future, it is to be hoped, when an alloy is, for the first time, to be attempted, it will be possible to predict with reasonable certainty what the result will be, instead of that result remaining to be discovered by experiment.

I have just, incidentally, mentioned aluminium. May I say that we engineers look forward, with much interest, to all processes tending to bring this metal, or its alloys, within possible commercial use?

One more instance of the effect of impurities in metals. The engineer engaged in electrical matters is compelled, in the course of his daily work, frequently to realize the importance of the "next to-nothing." One striking instance of this is afforded by the influence which an extremely minute percentage of impurity has on the electrical conductivity of copper wire; this conductivity being in some cases reduced by as much as 50 per cent., in consequence of the admixture of that which, under other circumstances, would be looked upon as insignificant.

Reverting to the question of big guns. According to the present mode of manufacture, after we have rough-bored and turned the A-tube (and perhaps I ought to have mentioned that by the A-tube is meant the main piece of the gun, the innermost layer, if I may so call it, that portion which is the full length of the gun, and upon which the remainder of the gun is built up)—after, as I have said, we have rough-bored and turned this A-tube, we heat it to a temperature lying between certain specified limits, but actually determined by the behaviour of samples previously taken, and then suddenly immerse it perpendicularly into a well some 60 feet deep, full of oil, the oil in this well being kept in a state of change by the running into it, at the bottom, of cold oil conveyed by a pipe proceeding from an elevated oil tank. In this way the steel is oil-hardened, with the result of increasing its ultimate tensile strength, and also with the result of raising its so-called elastic limit. In performing this operation it is almost certain that injurious internal strains will be set up—strains tending to produce self-rupture of the material. Experiments have been carried out in England, by Captain Andrew Noble, and by General Maitland of the Royal Gun Factory, by General Kalakoutsky, in Russia, and also in the United States, to gauge what is the value, as represented by dimensions, of these strains, and we find that they have to be recorded in the most minute fractions of an inch, and yet, if the steel be of too "high" a quality (as it is technically called), or if there has been any want of uniformity in the oil-hardening process, these strains, unless got rid of or ameliorated by annealing, may, as I have said, result in the self-rupture of the steel.

I have spoken of the getting rid of these strains by annealing, a process requiring to be conducted with great care, so as not to prejudice the effects of the oil-hardening. But take the case of a hardened steel projectile, hardened so that it will penetrate the steel face of compound armour. In that case annealing cannot be resorted to, for the extreme hardness of the projectile must not be in the least impaired. The internal strains in these projectiles are so very grave that for months after they are made there is no security that they will not spontaneously fracture. I have here the point of an 8-inch projectile, which projectile weighs 210 pounds; this with others was received from the makers as long ago as March of this year, and remained an apparently perfect and sound projectile until about the middle of August—some five months after delivery, and, of course, a somewhat longer time since manufacture—and between August 6 and 8 this piece which I hold in my hand, measuring 3¾ inches by 3½ inches, spontaneously flew off from the rest of the projectile, and has done so upon a surface of separation which, whether having regard to its beautiful regularity, or to the conclusions to be drawn from it as to the nature of the strains existing, is of the very highest scientific interest. Many other cases of self-rupture of similar projectiles have been recorded.

Another instance of the effect of the "next-to-nothing" in the hardening and tempering or annealing of steel. As we know, the iron and the carbon (leaving other matters out of consideration) are there. The carbon is (even in tool-steel) a very small proportion of the whole. The steel may be bent, and will retain the form given to it. You heat it and plunge it in cold water; you attempt to bend it and it breaks; but if, after the plunging in cold water, you temper it by carefully reheating it, you may bring it to the condition fit either for the cutting-tool for metal, or for the cutting-tool for wood, or for the watch-spring; and these important variations of condition which are thus obtained depend upon the "next-to-nothing" in the temperature to which it is reheated, and therefore in the nature of the resulting combination of the ingredients of which the steel is composed.

Some admirable experiments were carried out on this subject by the Institution of Mechanical Engineers, with the assistance of one of our Vice-Presidents, Sir Frederick Abel, and the subject has also been dealt with by an eminent Russian writer.

There is, to my mind, another and very striking popular instance (if I may use the phrase) of the importance of attention to detail—that is, to the "next-to-nothing." Consider the bicycles and tricycles of the present day—machines which afford the means of healthful exercise to thousands, and which will, probably within a very short time, prove of the very greatest possible use for military purposes. The perfection to which these machines have been brought is almost entirely due to strict attention to detail; in the selection of the material of which the machines are made; in the application of pure science (in its strictest sense) to the form and to the proportioning of the parts, and also in the arrangement of these various parts in relation the one to the other. The result is that the greatest possible strength is afforded with only the least possible weight, and that friction in working has been reduced to a minimum. All of us who remember the hobby-horse of former years, and who contrast that machine with the bicycle or tricycle of the present day, realize how thoroughly satisfactory is the result of this attention to detail—this appreciation of the "next-to-nothing."

Let me give you another illustration of the importance of small things, drawn from gunnery practice.

At first sight one would be tempted to say that the density of the air on the under side of a shot must, notwithstanding its motion of descent, be so nearly the same as that of the air upon the upper side as to cause the difference to be unworthy of consideration, but we know that the projectiles from rifled guns tend to travel sideways as they pass through the air, and that the direction of their motion, whether to the right or to the left, depends on the 'hand' of the rifling. We know also, that the friction against liquid or against gaseous bodies varies with the densities of these bodies, and it is believed that, minute as is the difference in density to which I have referred, it is sufficient to determine the lateral movement of the projectile. This lateral tendency must be allowed for, in these days of long ranges, in the sighting and laying of guns, if we desire accuracy of aim, at those distances at which it is to be expected our naval engagements will have to be commenced, and perhaps concluded. We can no longer afford to treat the subject as Nelson is said to have treated it, in one of his letters to the Secretary of the Admiralty, who had requested that an invention for laying guns more accurately should be tried. Nelson said he would be glad to try the invention, but that, as his mode of fighting consisted in placing his ship close alongside that of the enemy, he did not think the invention, even if it were successful, would be of much use to him.

While upon the question of guns, I am tempted to remark upon that which is by no means a small thing (for it is no less than the rotation of the earth), which in long-distance firing may demand attention, and that to an extent little suspected by the civilian.

Place the gun north and south, say in the latitude of London, and fire a 12-mile round such as I have mentioned, and it will be found that, assuming the shot were passing through a vacuum, a lateral allowance of more than 200 feet must be made to compensate for the different velocity of the circumference of the earth at 12 miles north or south of the place where the gun was fired, as compared with the velocity of the circumference of the earth at that place itself—the time of flight being in round numbers one minute.

At the risk of exciting a smile, I am about to assert that engineering has even its poetical side. I will ask you to consider with me whether there may not be true poetry in the feelings of the engineer who solves a problem such as this:—Consider this rock, never visible above the surface of the tide, but making its presence known by the waves which rise around it: it has been the cause of destruction to many a noble vessel which had completed, in safety, its thousands of leagues of journey, and was, within a few score miles of port, then dashed to pieces upon it! Here is this rock. On it build a lighthouse. Lay your foundations through the water, in the midst of the turmoil of the sea; make your preparations; appear to be attaining success, and find the elements are against you, and that the whole of your preliminary works are ruined or destroyed in one night; but again commence, and then go on and go on until at last you conquer; your works rise above ordinary tide-level; then upon these sure foundations, obtained it may be after years of toil, erect a fair shaft, graceful as a palm and sturdy as an oak; surmount it with a light, itself the produce of the highest application of science; direct that light by the built-up lens, again involving the highest application of science; apply mechanism, so arranged that the lighthouse shall from minute to minute reveal to the anxious mariner its exact name and its position on the coast. When you have done all this, will you not be entitled to say to yourself, "It is I who have for ever rendered innocuous this rock which has been hitherto a dread source of peril"? Is there no feeling, do you think, of a poetical nature excited in the breast of the engineer who has successfully grappled with a problem such as this?

Another instance: the mouth of a broad river, or, more properly speaking, the inlet of the sea, has to be crossed at such a level as not to impede the passage of the largest ships. Except in one or two places the depth is profound, so that multiple foundations for supporting a bridge become commercially impossible, and the solution of the problem must be found by making, high in the air, a flight of span previously deemed unattainable. Is there no poetry here? Again, although the results do not strike the eye in the same manner, is there nothing of poetry in the work that has to be thought out and achieved when a wide river or an ocean channel has to be crossed by a subterranean passage? Works of great magnitude of this character have been performed with success, and to the benefit of those for whose use they were intended. One of the greatest and most noble of such works, encouraged, in years gone by, by the Governments of our own country and of France, has lately fallen into disfavour with an unreasoning public, who have not taken the pains to ascertain the true state of the case.

Surely it will be agreed that the promotion of ready intercourse and communication between nations constitutes the very best and most satisfactory guarantee for the preservation of peace: when the peoples of two countries come to know each other intimately, and when they, therefore, enter into closer business relations, they are less liable to be led away by panic or by anger, and they hesitate to go to war the one with the other. It is in the interests of both that questions of difference which may arise between them should be amicably settled, and having an intimate knowledge of each other, they are less liable to misunderstand, and the mode of determination of their differences is more readily arranged. Remember, the means of ready intercourse and of communication, and the means of easy travel, are all due to the application of science by the engineer. Is not therefore his profession a beneficent one?

Further, do you not think poetical feeling will be excited in the breast of that engineer who will in the near future solve the problem (and it certainly will be solved when a sufficiently light motor is obtained) of travelling in the air—whether this solution be effected by enabling the self-suspended balloon to be propelled and directed, or perhaps, better still, by enabling not only the propulsion to be effected and the direction to be controlled, but by enabling the suspension in the air itself to be attained by mechanical means?

Take other functions of the civil engineer—functions which, after all, are of the most important character, for they contribute directly to the prevention of disease, and thereby not only prolong life, but do that which is probably more important—afford to the population a healthier life while lived.

In one town, about which I have full means of knowing, the report has just been made that in the year following the completion of a comprehensive system of sewerage, the deaths from zymotic diseases had fallen from a total of 740 per annum to a total of 372—practically one-half. Has the engineer no inward satisfaction who knows such results as these have accrued from his work?

Again, consider the magnitude and completeness of the water-supply of a large town, especially a town that has to depend upon the storing up of rain water: the prevision which takes into account, not merely the variation of the different seasons of the year, but the variation of one year from another; that, having collated all the stored-up information, determines what must be the magnitude of the reservoirs to allow for at least three consecutive dry years, such as may happen; and that finds the sites where these huge reservoirs may be safely built.

All these—and many other illustrations which I could put before you if time allowed—appear to me to afford conclusive evidence that, whether it be in the erection of the lighthouse on the lonely rock at sea; whether it be in the crossing of rivers, or seas, or arms of seas, by bridges or by tunnels; whether it be the cleansing of our towns from that which is foul; whether it be the supply of pure water to every dwelling, or the distribution of light or of motive power; or whether it be in the production of the mighty ocean steamer, or in the spanning of valleys, the piercing of mountains, and affording the firm, secure road for the express train; or whether it be the encircling of the world with telegraphs—the work of the civil engineer is not of the earth earthy, is not mechanical to the exclusion of science, is not unintellectual; but is of a most beneficent nature, is consistent with true poetical feeling, and is worthy of the highest order of intellect.

SECTION A.

MATHEMATICAL AND PHYSICAL SCIENCE.

OPENING ADDRESS BY PROF. G. F. FITZGERALD, M.A., F.R.S., PRESIDENT OF THE SECTION.

THE British Association in Bath, and especially we here in Section A, have to deplore a very great loss. We confidently anticipated profit and pleasure from the presence in this chair of one of the leading spirits of English science, Dr. Schuster. We deplore the loss, and we deplore the cause of it. It is always sad when want of strength makes the independent dependent, and it is doubly sad when a life's work is thereby delayed; and to selfish humanity it is trebly sad when, as in this case, we ourselves are involved in the loss. And our loss is great. Dr. Schuster has been investigating some very important questions. He has been studying electric discharges in gases, and he has been investigating the probably allied question of the variations of terrestrial magnetism. We anticipated his matured pronouncements upon these subjects, and also the advantage of his very wide general information upon physical questions, and the benefit of his judicial mind while presiding here.

As to myself, his substitute, I cannot express how much gratified I feel at the distinguished honour done me in asking me to preside. It has been one of the ambitions of my life to be worthy of it, and I will do my best to deserve your confidence; man can do no more, and upon such a subject "the less said the soonest mended."

I suppose most former occupants of this chair have looked over the addresses of their predecessors to see what sort of a thing was expected from them. I find that very few had the courage to deliver no address. Most have devoted themselves to broad general questions, such as the relations of mathematics to physics, or more generally deductive to inductive science. On the other hand, several have dealt each with his own specialty. On looking back over these addresses my attention was specially arrested by the first two past Presidents of this Section whose bodily presence we cannot have here. They were Presidents of Section A in consecutive years. In 1874, Provost Jellett occupied this chair; and in 1875, Prof. Balfour Stewart occupied it. Both have gone from us since the last meeting of this Association. Each gave a characteristic address. The Provost, with the clearness and brilliancy that distinguished his great intellect, plunged through the deep and broad questions surrounding the mechanism of the universe, and with impassioned earnestness claimed on behalf of science the right to prosecute its investigations until it attains, if it ever does attain, to a mechanical explanation of all things. This intrepid honesty, to carry to their utmost the principles of whose truth he was convinced, the utter abhorrence of the shadow of double-dealing with truth, was eminently characteristic of one whom all, but especially we of Trinity College, Dublin, will long miss as a lofty example of the highest intellectual keenness and honesty, and mourn as the truest-hearted friend, full of sympathy and Christian charity. In 1875, Prof. Stewart gave us a striking example of the other class of address in a splendid exposition of the subject he did so much to advance—namely, solar physics. He brought together from the two great storehouses of his information and speculation a brilliant store, and displayed them here for the advancement of science. Him, too, all science mourns. Though, from want of personal acquaintance, I am unequal to the task of bringing before you his many abilities and great character, you can each compose a fitting epitaph for this well-known great one of British science. In this connection I am only expressing what we all feel when I say how well timed was the Royal bounty recently extended to his widow. At the same time, the niggardly recognition of science by the public is a disgrace to the enlightenment of the nineteenth century. What Chancellor or General with his tens of thousands has done that for his country and mankind that Faraday, Darwin, and Pasteur have done? The "public" now are but the children of those who murdered Socrates, tolerated the persecution of Galileo, and deserted Columbus.

In a Presidential address on the borderlands of the known delivered from this chair the great Clerk Maxwell spoke of it as an undecided question whether electro-magnetic phenomena are due to a direct action at a distance or are due to the action of an intevening medium. The year 1888 will be ever memorable as the year in which this great question has been experimentally decided by Hertz in Germany, and, I hope, by others in England.

It has been decided in favour of the hypothesis that these actions take place by means of an intervening medium. Although there is nothing new about the question, and although most workers at it have long been practically satisfied that electro-magnetic actions are due to an intervening medium, I have thought it worth while to try and explain to others who may not have considered the problem, what the problem is and how it has been solved. A Presidential address such as this is not for specialists—it is for the whole Section; and I would not have thought of dealing with this subject, only that its immediate consequences reach to all the bounds of physical science, and are of interest to all its students.

We are all familiar with this, that when we do not know all about something there are generally a variety of explanations of what we do know. Whether there is anything of which there are in reality a variety of explanations is a deep question, which some have connected with the freedom of the will, but which I am not concerned with here. A notable example of the possibility of a variety of explanations for us is recorded in connection with an incident said to have occurred in the neighbouring town of Clifton, where a remarkable meteorological phenomenon, as it appeared to an observing scientist, was explained by others as a bull's-eye lantern in the hands of Mr. Pickwick. Another kind of example is the old explanation of water rising in a pump, that "Nature abhors a vacuum," as compared with the modern one. Nowadays, when we know as little about anything, we say, "It is the property of electricity to attract." This is really little or no advance on the old form, and is merely a way of stating that we know a fact but not its explanation. There are plenty of cases still where a variety of explanations are possible. For example, we know of no *experimentum crucis* to decide whether the people I see around me are conscious or are only automata. There are other questions which have existed, but which have been experimentally decided. The most celebrated of these are the questions between the caloric and kinetic theories of heat, and between the emission and undulatory theories of light. The classical experiments by which the case has been decided in favour of the kinetic theory of heat and the undulatory theory of light are some of the most important experiments that have ever been performed. When it was shown that heat disappeared whenever work appeared, and *vice versâ*, and so the caloric hypothesis was disproved; when it was shown that light was propagated more slowly in a dense medium than in a rare, the sciences of light and heat were revolutionized. Not but that most who studied the subject had given their adhesion to the true theory before it was finally decided in general estimation. In fact, Rumford's and Davy's experiments on heat, and Young and Fresnel's experiments on light, had really decided these questions long before the erroneous views were finally abandoned. I hope that science will not be so slow in accepting the results of experiment in respect of electro-magnetism as it was in the case of light and heat, and that no Carnot will throw back science by giving plausible explanations on a wrong hypothesis. Rowland's experiment proving an electro-magnetic action between electric charges depending on their absolute and not relative velocities has already proved the existence of a medium relative to which the motion must take place, but the connection is rather metaphysical, and is too indirect to attract general attention. The importance of these striking experiments was that they put the language of the wrong hypothesis out of fashion. Elementary text-books that halted between two opinions, and, after the manner of text-books, leant towards that enunciated in preceding text-books, had all perforce to give prominence to the true theory, and the whole rising generation began their researches from a firm and true stand-point. I anticipate the same results to follow Hertz's experimental demonstration of a medium by which electro-magnetic actions are produced. Text-books which have gradually been invoking lines of force, in some respects to the aid of learners and in others to their bewilderment, will now fearlessly discourse of the stresses in the ether that cause electric and magnetic force. The younger generation will see clearly in electro-magnetic phenomena the working of the all-pervading ether, and this will give them a firm and true stand-point for further advances.

And now I want to spend a short time in explaining to you how the question has been decided. An illustrative example may make the question itself clearer, and so lead you to understand the answer better. In colloquial language we say that balloons, hot air, &c., rise because they are light. In old times this was stated more explicitly, and therefore much more clearly. It was said that they possessed a quality called "levity." "Levity" was opposed to "heaviness." Heaviness made things tend downwards, levity made things tend upwards. It was a sort of action at a distance. At least, it would have required such an hypothesis if it had survived until it was known that heaviness was due to the action of the earth. I expect levity would have been attributed to the direct action of heaven. It was comparatively recently in the history of mankind that the rising of hot air, flames, &c., was attributed to the air. Everybody knew that there was air, but it was not supposed that the upward motion of flames was due to it. We now know that this and the rising of balloons are due to the difference of pressure at different levels in the air. In a similar way we have long known that there is an ether, an all-pervading medium, occupying all known space. Its existence is a necessary consequence of the undulatory theory of light. People who think a little, but not much, sometimes ask me, "Why do you believe in the ether? What's the good of it?" I ask them, "What becomes of light for the eight minutes after it has left the sun and before it reaches the earth?" When they consider that, they observe how necessary the ether is. If light took no time to come from the sun, there would be no need of the ether. That it is a vibratory phenomenon, that it is affected by matter it acts through—these could be explained by action at a distance very well. The phenomena of interference would, however, require such complicated and curious laws of action at a distance as practically to put such an hypothesis out of court, or else be purely mathematical expressions for wave propagation. In fact, anything except propagation in time is explicable by action at a distance. It is the same in the case of electro-magnetic actions. There were two hypotheses as to the causes of electro-magnetic actions. One attributed electric attraction to a property of a thing called electricity to attract at a distance, the other attributed it to a pull exerted by means of the ether, somewhat in the way that air pushes balloons up. We do not know what the structure of the ether is by means of which it can pull, but neither do we know what the structure of a piece of india-rubber is by means of which it can pull; and we might as well ignore the india-rubber, though we know a lot about the laws of its action, because we do not know its structure, as to ignore the ether because we do not know its structure. Anyway, what was wanted was an experiment to decide between the hypothesis of direct action at a distance and of action by means of a medium. At the time that Clerk Maxwell delivered his address no experiment was known that could decide between the two hypotheses. Specific inductive capacity, the action of intervening matter, the delay in telegraphing, the time propagation of electro-magnetic actions by means of conducting material—these were known, but he knew that they could be explained by means of action at a distance, and had been so explained. Waves in a conductor do not necessarily postulate action through a medium such as the ether. When we are dealing with a conductor and a thing called electricity running over its surface, we are, of course, postulating a medium on or in the conductor, but not outside it, which is the special point at issue. Clerk Maxwell *believed* that just as the same air that transmits sound is able by differences of pressure—*i.e.* by means of its energy per unit volume—to move bodies immersed in it, so the same ether that transmits light causes electrified bodies to move by means of its energy per unit volume. He believed this, but there was no experiment known then to decide between this hypothesis and that of direct action at a distance. As I have endeavoured to impress upon you, no *experimentum crucis* between the hypotheses is possible except an experiment proving propagation in time, either directly, or indirectly by an experiment exhibiting phenomena like those of the interference of light. A theorist may speak of propagation of actions in time without talking of a medium. This is all very well in mathematical formulæ, but, as in the case of light we must consider what becomes of it after it has left the sun and before it reaches the earth, so every hypothesis assuming action in time really postulates a medium whether we talk about it or not. There are some difficulties surrounding the complete interpretation of some of Hertz's experiments. The conditions are complicated, but I confidently expect that they will lead to a decision on most of the outstanding questions on the theory of electro-magnetic action. However, there is no doubt that he has observed the interference of electro-magnetic waves quite analogous to those of light, and that he has proved that electro-magnetic actions are propagated in air with the velocity of light. By a beautiful device Hertz has produced rapidly alternating currents of such frequency that their wave-length is only about 2 metres. I may pause for a minute to call your attention to what that means. These waves are propagated three hundred thousand kilometres in a second. If they vibrated three hundred thousand times a second, the waves would be each a kilometre long. This rate of vibration is much higher than the highest audible note, and yet the waves are much too long to be manageable. We want a vibration about a thousand times as fast again with waves about a metre long. Hertz produced such vibrations, vibrating more than a hundred million times a second. That is, there are as many vibrations in one second as there are seconds—in a day? No, far more. In a week? No, more even than that. The pendulum of a clock ticking seconds would have to vibrate for four months before it would vibrate as often as one of Hertz's vibrators vibrates in one second. And how did he detect the vibrations and their interference? He could not see them; they are much too slow for that; they should go about a million times as fast again to be visible. He could not hear them; they are much too quick for that. If they went a million times more slowly they would be well heard. He made use of the principle of resonance. You all understand how by a succession of well-timed small impulses a large vibration may be set up. It explains many things, from speech to spectrum analysis. It is related that a former Marquess of Waterford used the principle to overturn lamp-posts—his ambition soared above knocker-wrenching. So that it is a principle known to others besides scientific men. Hertz constructed a circuit whose period of vibration for electric currents was the same as that of his generating vibrator, and he was able to see sparks, due to the induced vibration, leaping across a small air-space in this resonant circuit. The well-timed electrical impulses broke down the air-resistance just as those of my Lord of Waterford broke down the lamp-post. The combination of a vibrating generating circuit with a resonant receiving circuit is one that I spoke of at the meeting of the British Association at Southport as one by which this very question might be studied. At the time I did not see any feasible way of detecting the induced resonance: I did not anticipate that it could produce sparks. By its means, however, Hertz has been able to observe the interference between waves incident on a wall and the reflected waves. He placed his generating vibrator several wave-lengths away from a wall, and placed the receiving resonant circuit between the generator and the wall, and in this air-space he was able to observe that at some points there were hardly any induced sparks, but at other and greater distances from his generator they reappeared, to disappear again in regular succession at equal intervals between his generator and the wall. It is exactly the same phenomenon as what are known as Lloyd's bands in optics, which are due to the interference between a direct and a reflected wave. It follows hence that, just as Young's and Fresnel's researches on the interference of light prove the undulatory theory of optics, so Hertz's experiment proves the ethereal theory of electro-magnetism. It is a splendid result. Henceforth I hope no learner will fail to be impressed with the theory—hypothesis no longer—that electro-magnetic actions are due to a medium pervading all known space, and that it is the same medium as the one by which light is propagated, that non-conductors can, and probably always do, as Prof. Poynting has taught us, transmit electro-magnetic energy. By means of variable currents energy *is* propagated into space with the velocity of light. The rotation of the earth is being slowly stopped by the diurnal rotation of its magnetic poles. This seems a hopeful direction in which to look for an explanation of the secular precession of terrestrial magnetism. It is quite different from Edlund's curious hypothesis that free space is a perfect conductor. If this were true, there would be a pair of great antipoles outside the air, and terrestrial magnetism would not be much like what it is, and I think the earth would have stopped rotating long ago. With alternating currents we *do* propagate energy through non-conductors. It seems almost as if our future telegraph-cables would be pipes. Just as the long sound-waves in speaking-tubes go round corners, so these electro-magnetic waves go round corners if they are not too sharp. Prof. Lodge will probably have something to tell us on this point in connection with lightning-conductors. The silvered glass-bars used by surgeons to conduct light are exactly what I am describing. They are a glass, a non-conducting, and therefore transparent, bar surrounded by a conducting, and therefore opaque, silver sheath, and they transmit the rapidly alternating currents we call light. There would not be the same difficulty in utilizing the energy of these electro-magnetic waves as in utilizing radiant heat. Having all the vibrations of the same period we might utilize Hertz's resonating circuits, and in any case the second law of thermodynamics would not trouble us when we could practically attain to the absolute zero of these, as compared with heat, long-period vibrations.

We seem to be approaching a theory as to the structure of the ether. There are difficulties from diffusion in the simple theory that it is a fluid full of motion, a sort of vortex-sponge. There were similar difficulties in the wave theory of light owing to wave propagation round corners, and there is as great a difficulty in the jelly theory of the ether arising from the freedom of motion of matter through it. It may be found that there is diffusion, or it may be found that there are polarized distributions of fluid kinetic energy which are not unstable when the surfaces are fixed: more than one such is known. Osborne Reynolds has pointed out another, though in my opinion less hopeful, direction in which to look for a theory of the ether. Hard particles are abominations. Perhaps the impenetrability of a vortex would suffice. Oliver Lodge speaks confidently of a sort of chemical union of two opposite kinds of elements forming the ether. The opposite sides of a vortex-ring might perchance suit, or maybe the ether, after all, is but an atmosphere of some infra-hydrogen element: these two latter hypotheses may both come to the same thing. Anyway we are learning daily what sort of properties the ether must have. It must be the means of propagation of light; it must be the means by which electric and magnetic forces exist; it should explain chemical actions, and, if possible, gravity.

On the vortex-sponge theory of the ether there is no real difficulty by reason of complexity why it should not explain chemical actions. In fact, there is every reason to expect that very much more complex actions would take place at distances comparable with the size of the vortices than at the distances at which we study the simple phenomena of electro-magnetism. Indeed, if vortices can make a small piece of a strong elastic solid, we can make watches and build steam-engines and any amount of complex machinery, so that complexity can be no essential difficulty. Similarly the instantaneous propagation of gravity, if it exists, is not an essential difficulty, for vortices each occupy all space, and they act on one another simultaneously everywhere. The theory that material atoms are simple vortex-rings in a perfect liquid otherwise unmoving is insufficient, but with the innumerable possibilities of fluid motion it seems almost impossible but that an explanation of the properties of the universe will be found in this conception. Anything purporting to be an explanation founded on such ideas as "an inherent property of matter to attract," or building up big elastic solids out of little ones, is not of the nature of an ultimate explanation at all; it can only be a temporary stopping-place. There are metaphysical grounds, too, for reducing matter to motion and potential to kinetic energy.

These ideas are not new, but it is well to enunciate them from time to time, and a Presidential address in Section A is a fitting time. Besides all this, it has become the fashion to indulge in quaint cosmical theories and to dilate upon them before learned Societies and in learned journals. I would suggest, as one who has been bogged in this quagmire, that a successor in this chair might well devote himself to a review of the cosmical theories propounded within the last few years. The opportunities for piquant criticism would be splendid.

Returning to the sure ground of experimental research, let us for a moment contemplate what is betokened by this theory that in electro-magnetic engines we are using as our mechanism the ether, the medium that fills all known space. It was a great step in human progress when man learnt to make material machines, when he used the elasticity of his bow and the rigidity of his arrow to provide food and defeat his enemies. It was a great advance when he learnt to use the chemical action of fire; when he learnt to use water to float his boats and air to drive them; when he used artificial selection to provide himself with food and domestic animals. For two hundred years he has made heat his slave to drive his machinery. Fire, water, earth, and air have long been his slaves, but it is only within the last few years that man has won the battle lost by the giants of old, has snatched the thunderbolt from Jove himself, and enslaved the all-pervading ether.

SECTION C.

GEOLOGY.

OPENING ADDRESS BY W. BOYD DAWKINS, M.A., F.R.S., F.G.S., F.S.A., PROFESSOR OF GEOLOGY AND PALÆONTOLOGY IN OWENS COLLEGE, PRESIDENT OF THE SECTION.

IN taking the chair occupied twenty-four years ago in this place by my honoured master, Prof. Phillips, I have been much perplexed as to the most fitting lines on which to mould my address. It was open to me to deal with the contributions to our knowledge since our last meeting in Manchester in such a manner as to place before you an outline of our progress during the last twelve months. But this task, difficult in itself, is rendered still more so by the special circumstances of this meeting, attended, as it is, by so large a number of distinguished geologists, assembled from nearly every part of the world for the purposes of the Geological Congress. It would be presumptuous of me, in the presence of so many specialists, to attempt to summarize and co-ordinate their work. Indeed, we stand too near to it to be able to see the true proportions of the various parts. I will merely take this opportunity of offering to our visitors, in the name of this Section and of English geologists in general, a hearty welcome to our shores, feeling that not only will our science be benefited enormously by the simplification of geological nomenclature, but that we ourselves shall derive great advantage by a closer personal contact than we have enjoyed hitherto.

Our science has made great strides during the last twenty-four years, and she has profited much from the development of her sisters. The microscopic analysis of the rocks has opened out a new field of research, in which physics and chemistry are in friendly rivalry, and in which fascinating discoveries are being made almost day by day as to metamorphism, and the crushing and shearing forces brought to bear upon the cooling and contracting crust while the earth was young. The deep-sea explorations have revealed the structure and the deposits of the ocean abysses ; and the depths supposed to be without life, like the fabled deserts in the interior of Africa, are now known to teem with varied forms glowing with the richest colours. From a comparison of these deposits with the stratified rocks we may conclude that the latter are marginal, and deposited in depths not greater than 1000 fathoms, or the shore end of the Globigerina ooze, and most of them at a very much less depth, and that consequently there is no proof in the geological record of the ocean depths having ever been in any other than their present places.

In North America the geological survey of the Western States has brought to light an almost unbroken series of animal remains, ranging from the Eocene down to the Pleistocene age. In these we find the missing links in the pedigree of the horse, and sufficient evidence of transitional forms to cause Prof. Flower to restore to its place in classification the order Ungulata of Cuvier. These may be expected to occupy the energies of our kinsmen on the other side of the Atlantic for many years, and to yield further proof of the truth of the doctrine of evolution. The use of this word reminds me how much we have grown since 1864, when evolution was under discussion, and when biological, physical, and geological laboratories could scarcely be said to have existed in this country. Truly may the scientific youth of to-day make the boast—

> Ἡμεῖς μὲν πατέρων μέγ' ἀμείνονες εὐχόμεθ' εἶναι—

"We are much better off than our fathers were ;" while we, the fathers, have the poor consolation of knowing that when they are fathers their children will say the same of them. There is reason to suppose that our science will advance more swiftly in the future than it has in the past, because it has more delicate and precise methods of research than it ever had before, and because its votaries are more numerous than they ever were.

In 1864 the attention of geologists was mainly given to the investigations of the later stages of the Tertiary period. The bent of my pursuits inclines me to revert to this portion of geological inquiry, and to discuss certain points which have arisen during the last few years in connection with the classificatory value of fossils, and the mode in which they may be best used for the co-ordination of strata in various parts of the world.

The principle of homotaxy, first clearly defined by Prof. Huxley, has been fully accepted as a guiding principle in place of synchronism or contemporaneity, and the fact of certain groups of plants and animals succeeding one another in a definite order, in countries remote from each other, is no longer taken to imply that each was living in the various regions at the same time, but rather, unless there be evidence to the contrary, that they were not. While, however, there is a universal agreement on this point among geologists, the classificatory value of the various divisions of the vegetable and animal kingdoms is still under discussion, and, as has been very well put by my predecessor in this chair at Montreal, sometimes the evidence of one class of organic remains points in one direction, while the evidence of another class points in another and wholly different direction, as to the geological horizon of the same rocks. The flora, put into the witness-box by the botanist, says one thing, while the Mollusca or the Vertebrata say another thing in the hands of their respective counsel. There seems to be a tacit assumption that the various divisions of the organic world present the same amount of variation in the rocks, and that consequently the evidence of every part of it is of equal value.

It will not be unprofitable to devote a few minutes to this question, premising that each case must be decided on its own merits, without prejudice, and that the whole of the evidence of the flora and fauna must be considered. We will take the flora first.

The Cryptogamic flora of the later Primary rocks shows but slight evidence of change. The forests of Britain and of Europe generally, and of North America, were composed practically of the same elements—Sigillaria, Calamites, and conifers allied to the Ginkho—throughout the whole of the Carboniferous (16,336 feet in thickness in Lancashire and Yorkshire) and Devonian rocks, and do not present greater differences than those which are to be seen in the existing forests of France and Germany. They evidently were continuous both in space and time, from their beginning in the Upper Silurian to their decay and ultimate disappearance in the Permian age. This disappearance was probably due to geographical and climatic changes, following the altered relations of land to sea at the close of the Carboniferous age, by which Secondary plants, such as *Voltzia* and *Walchia*, were able to find their way by migration from an area hitherto isolated. The Devonian formation is mapped off from the Carboniferous, and this from the Permian, but to a slight degree by the flora, and nearly altogether by the fauna. While the fauna exhibits great and important changes, the flora remained on the whole the same.

The forests of the Secondary period, consisting of various conifers and cycads, also present slight differences as they are traced upwards through the Triassic and Jurassic rocks, while remarkable and striking changes took place in the fauna, which mark the division of the formations into smaller groups. As the evidence stands at present, the cycads of the Lias do not differ in any important character from those of the Oolites or the Wealden, and the *Salisburia* in Yorkshire in the Liassic age is very similar to that of the Island of Mull in the Early Tertiary, and to that (*Salisburia adiantifolia*) now living in the open air in Kew Gardens.

Nor do we find evidence of greater variation in the Dicotyledonous forests, from their first appearance in the Cenomanian stage of the Cretaceous rocks of Europe and America, through the whole of the Tertiary period down to the present time. In North America, the flora of the Dakota series so closely resembles the Miocene of Switzerland, that Dr. Heer had no hesitation in assigning it in the first instance to the Miocene age. It consists of more than a hundred species, of which about one-half are closely allied to those now living in the forests of North America—sassafras, tulip, plane, willow, oak, poplar, maple, beech, together with *Sequoia*, the ancestor of the giant redwood of California. The first palms also appear in both continents at this place in the geological record.

In the Tertiary period there is an unbroken sequence in the floras, as Mr. Starkie Gardner has proved, when they are traced over many latitudes, and most of the types still survive at the present day, but slightly altered. If, however, Tertiary floras of different ages are met with in one area, considerable differences are to be seen, due to progressive alterations in the climate and altered distribution of the land. As the temperature of the northern hemisphere became lowered, the tropical forests were pushed nearer and nearer to the equator, and were replaced by plants of colder habit from the northern regions, until ultimately, in the Pleistocene age, the Arctic plants were pushed far to the south of their present habitat. In consequence of this, Mr. Gardner concludes that "it is useless to seek in the Arctic regions for Eocene floras as we know them in our latitudes, for during the Tertiary period the climatic conditions of the earth did not permit their growth there. Arctic fossil floras of temperate and therefore Miocene aspect are, in all probability, of Eocene age, and what has been recognized in them as a newer or Miocene facies is due to their having been first studied in Europe in latitudes which only became fitted for them in Miocene times. When stratigraphical evidence is absent or inconclusive, this unexpected persistence of plant types or species throughout the Tertiaries should be remembered, and the degrees of latitude in which they are found should be well considered before conclusions are published respecting their relative age."

This view is consistent with that held by the leaders in botany —Hooker, Dyer, Saporta, Dawson, and Asa Gray (whose recent loss we so deeply deplore)—that the North Polar region is the centre of dispersal, from which the Dicotyledons spread over the northern hemisphere. If it be true—and I, for one, am prepared to accept it—it will follow that for the co-ordination of the subdivisions of the Tertiary strata in various parts of the world the plants are uncertain guides, as they have been shown to be in the case of the Primary and Secondary rocks. In all cases where there is a clash of evidence, such as in the Laramie lignites, in which a Tertiary flora is associated with a Cretaceous fauna, the verdict, in my opinion, must go to the fauna. They are probably of the same geological age as the deposit at Aix-la-Chapelle.

I would remark, further, before we leave the floras behind us, that the migration of new forms of plants into Europe and America took place before the arrival of the higher types in the fauna, after the break-up of the land at the close of the Carboniferous period, and after the great change in geography at the close of the Neocomian. The Secondary plants preceded the Secondary vetebrates by the length of time necessary for the deposit of the Permian rocks, and the Tertiary plants preceded the Tertiary vertebrates by the whole period of the Upper Cretaceous.

Let us now turn to the fauna.

Prof. Huxley, in one of his many addresses which have left their mark upon our science, has called attention to the persistence of types revealed by the study of palæontology, or, to put it in other words, to the singularly little change which the ordinal groups of life have undergone since the appearance of life on the earth. The species, genera, and families present an almost endless series of changes, but the existing orders are for the most part sufficiently wide, and include the vast series of fossils without the necessity of framing new divisions for their reception. The number of these extinct orders is not equally distributed through the animal kingdom. Taking the total number of orders at 108, the number of extinct orders in the Invertebrata amounts only to 6 out of 88, or about 7 per cent., while in the Vertebrates it is not less than 12 out of 40, or 30 per cent. These figures imply that the amount of ordinal change in the fossil Vertebrates stands to that in the Invertebrata in the ratio of 30 to 7. This disproportion becomes still more marked when we take into account that the former had less time for variation than the latter, which had the start by the Cambrian and Ordovician periods. It follows also that as a whole they have changed faster.

The distribution of the extinct orders in the animal kingdom, taken along with their distribution in the rocks, proves further that some types have varied more than others, and at various places in the geological record. In the Protozoa, Porifera, and Vermes there are no extinct orders; among the Cœlenterates one—the Rugosa; in the Echinodermata three—Cystideans, Edriasterida, and Blastoidea; in the Arthropoda two—the Trilobita and Eurypterida. All these, with the solitary exception of the obscure order Rugosa, are found only in the Primary rocks. Among the Pisces there are none; in the Amphibia one; the Labyrinthodonts ranging from the Carboniferous to the Triassic age. Among the Reptilia there are at least six of Secondary age—Plesiosauria, Ichthyosauria, Dicynodontia, Pterosauria, Theriodontia, Deinosauria; in the Aves two—the Saururæ and Odontornithes, also Secondary. In the Mammalia the Amblypoda, Tillodontia, Condylarthra, and Toxodontia represent the extinct orders—the three first Early Tertiary, and the last Pleistocene. It is clear, therefore, that, while the maximum amount of ordinal variation is presented by the Secondary Reptilia and Aves, all the extinct orders in the Tertiary are Mammalian.

If we turn from the extinct orders to the extinct species, it will also be found that the maximum amount of variation is presented by the plants, and all the animals, excepting the Mammalia, in the Primary and Secondary periods.

The general impression left upon my mind by these facts is that, while all the rest of the animal kingdom had ceased to present important modifications at the close of the Secondary period, the Mammalia, which presented no great changes in the Secondary rocks, were, to quote a happy phrase of Prof. Gaudry, "en pleine évolution" in the Tertiary age. And when, further, the singular perfection of the record allows us to trace the successive and gradual modifications of the Mammalian types from the Eocene to the close of the Pleistocene age, it is obvious that they can be used to mark subdivisions of the Tertiary period, in the same way as the reigns of kings are used to mark periods in human history. In my opinion they mark the geological horizon with greater precision than the remains of the lower members of the animal kingdom, and in cases such as that of Pikermi, where typical Miocene forms, such as Deinotheria, are found in a stratum above an assemblage of marine shells of Pliocene age, it seems to me that the Mammalia are of greater value in classification than the Mollusca, some of the species of which have been living from the Eocene down to the present day.

Yet another important principle must be noted. The fossils are to be viewed in relation to those forms now living in their respective geographical regions. The depths of the ocean have been where they are now since the earliest geological times, although continual geographical changes have been going on at their margins. In other words, geographical provinces must have existed even in the earlier geological periods, although there is reason to believe that they did not differ so much from each other as at the present day. It follows from this that the only just standard for comparison in dealing with the fossils, and especially of the later rocks, is that which is offered by the fauna and flora of the geographical province in which they are found. The non-recognition of this principle has led to serious confusion. The fauna, for example, of the Upper Sivalik formation has been very generally viewed from the European stand-point and placed in the Miocene, while, judged by the stand-point of India, it is really Pliocene. A similar confusion has followed from taking the Miocene flora of Switzerland as a standard for the Tertiary flora of the whole of the northern hemisphere.

It now remains for us to see how these principles may be applied to the co-ordination of Tertiary strata in various parts of the world. In 1880 I proposed a classification of the European Tertiaries, in which, apart from the special characteristic fossils of each group, stress was laid on the gradual approximation of various groups to the living Mammalia. The definitions are the following:—

DIVISIONS.	CHARACTERISTICS.
1. Eocene, or that in which the higher Mammalia (Eutheria) now on the earth were represented by allied forms belonging to existing orders and families. Oligocene.	Extinct orders. Living orders and families. No living genera.
2. Miocene, in which the alliance between fossil and living Mammals is closer than before.	Living genera. No living species.
3. Pliocene, in which living species of Mammals appear.	Living species few. Extinct species predominant.
4. Plistocene, in which living species of Mammals preponderate.	Living species abundant. Extinct species present. Man present.
5. Prehistoric, or that period outside history in which Man has multiplied exceedingly on the earth and introduced the domestic animals.	Man abundant. Domestic animals present. Wild Mammals in retreat. One extinct Mammal.
6. Historic, in which the events are recorded in history.	Records.

These definitions are of more than European significance. The researches of Leidy, Marsh, and Cope prove that they apply equally to the Tertiary strata of North America. The

Wasatch Bridger and Uinta strata contain representatives of the orders Cheiroptera and Insectivora, the sub-orders Artio- and Perissodactyla, and the families Vespertilionidæ and Tapiridæ; but no living genera.[1] The Mammalia are obviously in the same stage of evolution as in the Eocenes of Europe, although there are but few genera, and no species common to the two.

The White River and Loup Fork groups present us with the living genera *Sciurus*, *Castor*, *Hystrix*, *Rhinoceros*, *Dicotyles*, and others; but no living species, as is the case with the Miocenes of Europe. In the Pliocenes of Oregon the first living species appear, such as the Beaver, the Prairie Wolf, and two Rodents (*Thomomys clusius* and *T. talpoides*), while in the Pleistocene river deposits and caves, from Eschscholtz Bay in the north to the Gulf of Mexico in the south, there is the same grouping of living with extinct species as in Europe, and the same evidence in the glaciated regions that the Mammalia occupied the land after the retreat of the ice.

If we analyze the rich and abundant fauna yielded by the caves and river deposits both of South America and of Australia, it will be seen that the Pleistocene group in each is marked by the presence of numerous living species in each, the first being remarkable for their gigantic extinct Edentata, and the second for their equally gigantic extinct Marsupials.

The admirable work of Mr. Lydekker allows us also to see how these definitions apply to the fossil Mammalia of India. The Miocene fauna of the Lower Sivaliks has yielded the living genera *Rhinoceros* and *Manis*, and no living species.

The fauna of the Upper Sivaliks, although it has only been shown, and that with some doubt, to contain one living Mammal, the Nilghai (*Boselaphus tragocamelus*), stands in the same relation to that of the Oriental Region as that of the Pliocenes of Europe to that of the Palæarctic Region, and is therefore Pliocene. And lastly, the Narbada formation presents us with the first traces of Palæolithic Man in India in association with the living one-horned Rhinoceros, the Nilghai, the Indian Buffalo, two extinct Hippopotami, Elephants, and others, and is Pleistocene.

It may be objected to the Prehistoric and Historic divisions of the Tertiary period that neither the one nor the other properly fall within the domain of geology. It will, however, be found that in tracing the fauna and flora from the Eocene downwards to the present day there is no break which renders it possible to stop short at the close of the Pleistocene. The living plants and animals were in existence in the Pleistocene age in every part of the world which has been investigated. The European Mollusca were in Europe in the Pliocene age. The only difference between the Pleistocene fauna, on the one hand, and the Prehistoric, on the other, consists in the extinction of certain of the Mammalia at the close of the Pleistocene age in the Old and New Worlds, and in Australia. The Prehistoric fauna in Europe is also characterized by the introduction of the ancestors of the present domestic animals, some of which, such as the Celtic shorthorn (*Bos longifrons*), sheep, goat, and domestic hog, reverted to a feral condition, and have left their remains in caves, alluvia, and peat-bogs over the whole of the British Isles and the Continent. These remains, along with those of Man in the Neolithic, Bronze, and Iron stages of culture, mark off the Prehistoric from the Pleistocene strata. There is surely no reason why a cave used by Palæolithic Man should be handed over to the geologist, while that used by men in the Prehistoric age should be taken out of his province, or why he should be asked to study the lower strata only in a given section, and leave the upper to be dealt with by the archæologist. In these cases the ground is common to geology and archæology, and the same things, if they are looked at from the stand-point of the history of the earth, belong to the first, and, if from the stand-point of the history of Man, to the second.

If, however, there be no break of continuity in the series of events from the Pleistocene to the Prehistoric ages, still less is there in those which connect the Prehistoric with the period embraced by history. The historic date of a cave or of a bed of alluvium is as clearly indicated by the occurrence of a coin as the geological position of a stratum is defined by an appeal to a characteristic fossil. The gradual unfolding of the present order of things from what went before compels me to recognize the fact that the Tertiary period extends down to the present day. The Historic period is being recorded in the strata now being formed, exactly in the same way as the other divisions of the Tertiary have left their mark in the crust of the earth, and history is incomplete without an appeal to the geological record. In the masterly outline of the destruction of Roman civilization in Britain the historian of the English Conquest was obliged to use the evidence, obtained from the upper strata, in caves which had been used by refugees from the cities and villas; and among the materials for the future history of this city there are, to my mind, none more striking than the proof, offered by the silt in the great Roman bath, that the resort of crowds had become so utterly desolate and lonely in the ages following the English Conquest as to allow of the nesting of the wild duck.

I turn now to the place of Man in the geological record, a question which has advanced but little since the year 1864. Then, as now, his relation to the glacial strata in Britain was in dispute. It must be confessed that the question is still without a satisfactory answer, and that it may well be put to "a suspense account." We may, however, console ourselves with the reflection that the River-drift Man appears in the Pleistocene strata of England, France, Spain, Italy, Greece, Algiers, Egypt, Palestine, and India along with Pleistocene animals, some of which were pre-glacial in Britain. He is also proved to have been post-glacial in Britain, and was probably living in happy, sunny, southern regions, where there was no ice, and therefore no Glacial period, throughout the Pleistocene age.

It may further be remarked that Man appears in the geological record where he might be expected to appear. In the Eocene the Primates were represented by various Lemuroids (*Adapis*, *Necrolemur*, and others) in the Old and New Worlds. In the Miocene the Simiadæ (*Dryopithecus*, *Pliopithecus*, *Oreopithecus*) appear in Europe, while Man himself appears, along with the living species of Mammalia, in the Pleistocene Age, both in Europe and in India.

The question of the antiquity of Man is inseparably connected with the further question: "Is it possible to measure the lapse of geological time in years?" Various attempts have been made, and all, as it seems to me, have ended in failure. Till we know the rate of causation in the past, and until we can be sure that it has been invariable and uninterrupted, I cannot see anything but failure in the future. Neither the rate of the erosion of the land by sub-aërial agencies, nor its destruction by oceanic currents, nor the rate of the deposit of stalagmite or of the movement of the glaciers, has as yet given us anything at all approaching a satisfactory date. We only have a sequence of events recorded in the rocks, with intervals the length of which we cannot measure. We do not know the exact duration of any one geological event. Till we know both, it is surely impossible to fix a date, in terms of years, either for the first appearance of Man or for any event outside the written record. We may draw cheques upon "the bank of force" as well as "on the bank of time."

Two of my predecessors in this chair, Dr. Woodward and Prof. Judd, have dealt with the position of our science in relation to biology and mineralogy. Prof. Phillips in 1864 pointed out that the later ages in geology and the earlier ages of mankind were fairly united together in one large field of inquiry. In these remarks I have set myself the task of examining that side of our science which looks towards history. My conception of the aim and results of geology is that it should present a universal history of the various phases through which the earth and its inhabitants have passed in the various periods, until ultimately the story of the earth, and how it came to be what it is, is merged in the story of Man and his works in the written records. Whatever the future of geology may be, it certainly does not seem likely to suffer in the struggle for existence in the scientific renascence of the nineteenth century.

NOTES.

MAJOR-GENERAL PRJEVALSKY started on Thursday last on his fifth journey of exploration in Tibet, with the intention of penetrating, if possible, into Lhassa, the capital. The General, with his officers and Cossacks, will this time take advantage of the new Central Asian railway as far as Samarcand, whence they will proceed to Semiretchinsk, and so to the Tibetan table-lands. General Prjevalsky will, it is thought, on this occasion have the best chance ever afforded him of entering the forbidden residence of the Dalai Lama.

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[1] The genus *Vesperugo* has not been satisfactorily determined.—Cope, "Report of Geol. Survey of the Territories: Tertiary Vertebrata," i., 1884.