THURSDAY, JULY 7, 1887.

PROFESSOR TYNDALL AND THE SCIENTIFIC MOVEMENT.

THE complimentary banquet to Prof. Tyndall, to which reference has more than once been made in these columns, is described in detail elsewhere. We cannot, however, allow an event of so much interest, and which is, we believe, unique in the history of science in this country, to pass without comment.

Many notable gatherings have taken place in Willis's Rooms, but we question if English science has ever been more completely represented than at the "Tyndall Dinner." The President of the Royal Society was in the chair. The seven Vice-Chairmen were Presidents of the most important scientific Societies. The tables were crowded with men whose names are known wherever Nature is studied.

No every-day motive would suffice to bring together such an assembly, and it is not every day that we have an opportunity of doing honour to a life-work such as that of Prof. Tyndall. Others will rank beside or above him as investigators, but in the promotion of the great scientific movement of the last fifty years he has played a part second to none. The English people are a determined but somewhat slow-witted race, and it has been no easy task to convince them that a new era—that of science—was dawning. They have been content to pride themselves on industrial successes due for the most part to isolated efforts of genius, which was hampered by unnecessary difficulties, and which cannot be produced at will. They were long in seeing, they do not yet fully see, that our industrial position can only be maintained if armies of well-equipped followers are ready to seize the ground which the leaders win.

There is, however, a still harder lesson to learn. The industrial application of a scientific principle—vitally important to the well-being of the people as that application may be—requires nevertheless a lower form of intellectual energy than the discovery of the principle itself. The triumphs of applied science, of the physician, the engineer, the telegraphist, are readily "understanded of the people." The research laboratory, on the other hand, is open to few. The flash of genius which has wrung a fresh secret from Nature can only be fully appreciated by those who are intellectually competent to understand the difficulty and the success. And yet, if a widespread knowledge of science was to be, as it is, an essential condition of national well-being, it was absolutely necessary that the people should know something of, and be in some sort in sympathy with, the methods and conditions of scientific thought.

In supplying this need, Prof. Tyndall's greatest work has been done. Uniting scientific eminence of no ordinary kind with extraordinary gifts of exposition, he has, by his lectures and his books, brought the democracy into touch with scientific research. In dozens of lecture-rooms experiments devised by him are proving that a living science is a nobler instrument of education than a dead language. In hundreds of libraries his nervous English is convincing men of the value of a career like Faraday's, and teaching them to appreciate, if they cannot always in detail follow, the methods by which the victories of science are won. He has done perhaps more than any other living man to compel those who regard knowledge as valuable only in so far as it is immediately useful, to admit that the seed which is sown in the laboratory often produces the most abundant harvest in the workshop, and that a desire for knowledge is the mother of inventions which necessity could never have brought to the birth.

Such has been Prof. Tyndall's work; and yet we venture to think that among those who met in Willis's Rooms a deeper feeling was aroused than admiration for an eminent worker and a useful career.

Many of the greatest masters both of the moral and intellectual life have sought the attainment of their highest ideals in a more or less complete withdrawal from society, and it may well be that some natures can best achieve in seclusion the concentration which a supreme effort demands.

But although the scientific movement of to-day may receive its highest inspirations from men who, like Darwin and Joule, have worked in self-imposed retirement, its distinguishing characteristic is that it is sweeping along with it all classes and all opinions. It is a new habit of thought in the light of which the foundations of our educational, industrial, and political systems are being reconsidered. It is a new and deliberate attempt to put into practice the belief that "the sovereignty of man lieth hid in knowledge, wherein many things are reserved which kings with their treasures cannot buy, nor with their force command; their spials and intelligencers can give no news of them; their seamen and discoverers cannot sail where they grow."

Thus it has come to pass that science has gathered round it a crowd of workers, engaged in very various tasks, but all of whom would be ready to admit that the cardinal principle of the movement in which they take part is the investigation of truth for truth's sake alone. They may be professors or manufacturers, soldiers or physicians. If only they are imbued with the desire to penetrate a little further into the mysteries which surround us, if only they are willing and able to add something to the sum of human knowledge, they are scientific men.

In part this army is organized. There is in England no Academy of Literature. The Academy of Arts, it is admitted, needs reform. The principal scientific Societies, however, with the Royal Society at their head, perform the duties of an Academyof Science to the general satisfaction. No human institution is perfect, but it may be fairly said that they set in their Transactions a high standard of scientific work, and that their judgment, whether of men or of investigations, is seldom challenged.

In spite of this advantage, neither the outside world nor scientific men themselves have as yet sufficiently realized that these Societies constitute a great guild of that learning which is the most powerful and the most characteristic influence of our age.

On an occasion such as the Tyndall Dinner this realization is quickened. The curious magnetic influence of numbers is felt. Minor differences disappear in the knowledge that all are workers in the same cause. Men become more vividly conscious that though students of Nature are excluded from the State recognition which is extended to the Church, to medicine, and to the law, they too are members of a great profession. They realize that, though State rewards are given only to those who have applied their knowledge to some directly useful end, in a gathering of the profession of science the true leaders are those who have wrested the deepest secrets from Nature, careless whether they could be turned to gold or no.

A meeting held in great numbers and for a common purpose may have an influence which many an apparently more useful testimonial would lack. Prof. Tyndall has done service in the cause of science which merited the unique compliment he received. He would, we believe, be the first to rejoice if in the future the Tyndall Dinner was remembered not only as a tribute to his own work, but as marking the beginning of a period in which the ranks of science were drawn closer together, and in which the further organization of the investigation of Nature claimed and received the attention which its importance demands.

THE GEOLOGY OF ENGLAND AND WALES.

The Geology of England and Wales. With Notes on the Physical Features of the Country. By Horace B. Woodward, F.G.S. Second Edition. (London: Philip and Son, 1887.)

THE student of physical geology has at least two large English text-books, interesting, full, accurate, judicial, and written by masters of the science; but he who would build on this foundation a knowledge of historical and palæontological geology is in a harder case, and finds either a meagre outline containing little but a few meaningless names of formations and fossil lists, or else an ill-digested and formless mass of matter, derived from everywhere, but leading nowhere. Perhaps the time has not yet come when stratigraphy can be treated from the stand-point of inorganic evolution, so that fact may be joined to his fact and an organized whole result.

While, however, we wait for one who shall give us geology in the form of the inorganic and organic evolution of the globe, we must not omit to notice the labour of those whose "work is to record the facts from which the pleasanter deductions may be made." Mr. Woodward has done wisely in republishing by subscription and in an enlarged form his admirable book on the geology of England and Wales—a veritable mine of facts, well indexed and admirably supplied with references for the advanced reader, forming a base-line for further study and research, but complete in itself for the more elementary student and rendered interesting by the author's fresh style, by his capital and apt illustrations, and by his wonderful faculty of seizing upon the individuality of the rock group he is describing and skilfully tracing its variations from place to place. This new edition is improved by a larger and better map, undertaken by Mr. Goodchild, by more free use of sections, illustrations, and fossil lists, and by the employment of local names with tables of correlation.

The author works his way upwards from the lowest rocks, but combines a geographical with a chronological arrangement, and varies his method from system to system in order to adapt it better to the rocks under consideration. Just occasionally one meets with a slip in method, as in the case of the Rhætic rocks, where for no apparent reason he has reversed his usual order and treated the White Lias first. Where the mass of facts is unusually great and somewhat barren of interest, the author has introduced little helps and alleviations for which the student will be truly grateful,—the character of the hero of a system sketched in one graphic touch, the origin of the name of a system or a fossil, or the discussion of the origin of some bed of palæontological or economic value (*vide* pp. 24, 47, 84, 266, 670).

It seems hard to criticise any points of detail in such well-intentioned and well-executed work, but the indication of a few lines for improvement will perhaps show better than anything else how little the author has left for others to suggest. First, with regard to the map. This is clearly engraved, and coloured with light but well-contrasted tints; every name on it suggests some fact interesting from a geologist's point of view, and the effect of the whole is pleasing. There is no special colour for the Permian (not an unmixed advantage), and, oddly enough, the Yorkshire coal-field is left uncoloured; the boundary is engraved, however, and the student can easily fill in the colour for himself. A point has been gained in using a distinctive colour for beds below the Bala, but one lost in not using still another for the lowest Cambrians. The igneous rock colours should have been used less sparingly, and surely the Arenigs and Snowdon deserve a volcanic tint as much as the Borrowdales and the Cheviot rocks. We miss, too, the north of England dikes and the Whinsill.

The book opens with an introduction containing a little history, a little cosmogony, and a few definitions. The latter are hardly needed, and might have made room for the accounts of the geology of different lines of railway, which found a place in the first edition but have been crowded out of this. A few words on the Palæozoic group are followed by an account of the Archæan system, in which too little is said of the new class of work amongst these rocks instituted by Prof. Lapworth, while Prof. Bonney's papers on the Bangor area are almost passed over. The table on page 52 hardly makes it quite clear that the Harlech group of St. David's is divided into the Caerfai and Solva groups, of which the former constitutes the *Annelidian* of Lapworth, and the latter, together with the Menevian beds, the *Paradoxidian*. On page 58 we find the time-worn section across that part of the Longmynd which teaches nothing of the succession of the Longmynd rocks; this and several other sections should have been orientated. On page 60 the Hollybush sandstone is omitted from the table of Shropshire Cambrians, and awkwardly placed on page 65, while the Shineton shales are correlated with the Dolgelly beds and Malvern black shales in the table, though afterwards correctly placed with the Dictyonema shales and Lower Tremadoc. A deceptive appearance of unconformity in sections on page 89 might easily have been removed, even if present in the original woodcut. It is good to see Mr. Lewis's name brought up with

Murchison's on page 103, in connexion with the Aymestry limestone.

The author mentions, but does not definitely accept, Prof. Hull's correlation of the Devonian rocks. Throughout the work, and particularly in the Carboniferous section, great care has been taken to show and where possible give the origin of the economic value of the rocks. A little more stress should have been laid on the relations of the Coal-measures to the underlying rocks, and one might notice here the absence of the Dudley, Sedgeley, and other inliers from the South Staffordshire coal-field on the map. An important feature consists in the description of Palæozoic rocks from all the deep borings (a list of these forms the first appendix); and a good section to express the present state of knowledge on the deep-seated geology of the London Basin is given on page 202.

The Permian and Lias form a single system, the Poikilitic, which is included in the Mesozoic, the author being guided by the widespread discordance between it and the older rocks. It is not quite easy to understand all the tables (pp. 286, 470), but these only echo the difficulties which exist in the rocks themselves. It would have been as well if the Yorkshire Cornbrash had found a place after the Upper Estuarine on page 321. A good opportunity was missed of discussing the anomalous beds of Faringdon and Blackdown, particularly in relation to Mr. Starkie Gardner's recent papers on kindred questions; and we should have liked to see the grit phases in the Jurassic clays of the eastern counties more accurately defined. A section might have been introduced to show the thinning of the Gault and growth of the Cambridge nodule beds; and Mr. Sollas's work on flints ought not to have been omitted.

The Upper Eocene beds are classed as Oligocene, but the Brockenhurst bed is put in its true place in the Headon.

There are some very suggestive remarks on the connexion between health and geology, between villages and springs and consequently the outcrop of porous rocks, and on the effects of percolation of spring and sea water through rocks. The section on igneous rocks is of necessity somewhat vague and unsystematic from its brevity, but room has been found to treat the volcanic rocks historically; the Nuneaton diorites are intrusive in pre-Carboniferous rocks only. There are concluding chapters on metalliferous deposits, and on scenery and geology, the latter containing a useful list of hills, valleys, plains, and forests.

A little more space might with advantage have been spent in indicating with greater fulness what is known of the physical geography of the different periods, and epochs of earth movements, their dates, directions, and effects should have been more fully dealt with in the last chapter. A capital synopsis of the animal kingdom is furnished in an appendix by Mr. Edwin T. Newton; and a grand index, occupying 45 pages of three columns each, and giving the dates of the birth and death of authors referred to, closes the volume, which is an excellent summary of the present state of our knowledge of British geology. The author has worked conscientiously and well, and that we have been able to suggest so few additions clearly shows that his labour has not been in vain.

W. W. W.

A TREATISE ON GEOMETRICAL OPTICS.

A Treatise on Geometrical Optics. By R. S. Heath, M.A., D.Sc., Fellow of Trinity College, Cambridge, Professor of Mathematics in the Mason College, Birmingham. Demy 8vo, pp. xvii. 356. (Cambridge: University Press, 1887.)

THIS treatise is based on the conception of a beam of light as consisting of a system of rays, which obey the laws of reflexion and refraction. The transformations of such a system and the construction and properties of optical instruments are deduced, so far as the latter are capable of explanation from this point of view.

In confining himself to geometrical optics in this sense, the author follows the mode of division of the science which has been usually adopted in text-books in this country, through the succession of Cambridge treatises by Coddington, Griffin, and Parkinson, and Lloyd's "Treatise on Light and Vision." The subject then splits up naturally into the theory of reflexion and refraction of systems of rays, which is in fact a department of geometry; and the more special discussion of the nature of optical instruments and the forms and positions to be given to their refracting surfaces to diminish spherical and chromatic aberration, which allies itself with the technical science of optical construction.

The book begins with a short chapter on the nature and properties of light, in which the theory of illumination is worked out as a consequence of the experimental fact that self-luminous surfaces appear equally bright in all directions and at all distances. The second and third chapters contain the statement, in geometrical and analytical form, of the laws of reflexion and refraction, and the investigation of conjugate foci for direct pencils.

In Chapter IV. the subject of refraction through lenses and systems of lenses is treated, use being made of the symmetrical analysis, by means of the convergents of continued fractions, to determine the principal points of a system whose refracting surfaces are specified. Free use is also made of the cardinal points of the system in the semi-geometrical manner introduced by Möbius. The following chapter is devoted to an account of the general analytical investigation by means of which Gauss placed the whole theory on an independent basis. The notion of the equivalent lens is here introduced to some practical purpose, for the investigations of this and the preceding chapter enable the author to specify the exact character of the equivalence that can be secured by a single lens or a single refracting surface: viz. that if the lens or surface occupied the position of one of the principal planes of the system, it would refract any beam incident along its axis into the same configuration as it actually possesses when it emerges through the other principal plane of the instrument; so that, neglecting aberrations, the equivalence holds in every sense except as regards the displacement along the axis, and is therefore complete for most practical purposes.

The theory of caustics is treated, chiefly by analytical methods; and the existence of wave-surfaces, which cut the system of rays at right angles in an isotropic medium, is established geometrically.

Chapter VII. is devoted to the discussion of the spherical aberration of direct pencils, which is perhaps one of the most difficult parts of the subject to present in an elegant manner, on account of the non-symmetrical character of the necessary approximations. The treatment here given seems to leave nothing to be desired.

Chapter VIII. begins with an exposition of the properties of a general system of rays: this with the cardinal result that the rays are all bi-tangents to a focal surface is ascribed to Kummer. They had, however, been previously given by Hamilton in his memoir on "Systems of Rays," in the discussion of ray-systems in a crystalline medium where the wave-surface no longer cuts the rays at right angles; and he in turn refers back to the same papers of Malus which contain the theorem of orthogonality in isotropic media.

The theory of the characteristic function is next applied to the solution of the general problem of the refraction of a narrow beam at a surface of double curvature; and to the analytical determination of the relation between the forms of such beams before and after passing through a general optical instrument whose internal structure is not specified. In these discussions the author has closely followed a series of papers by Clerk Maxwell which appeared about fifteen years ago in the Proceedings of the London Mathematical Society, and which presumably were to find a place in a book on optics then contemplated by their lamented author. It does not seem to have been much noticed in this country that the same formulæ for oblique refraction were developed a long time ago by Sturm and others, in a direct geometrical manner, from Malus's theorem; but the conciseness and precision which arise from defining a beam by means of its characteristic function give them an enhanced importance in optical theory. Their application is here given to some cases which we do not remember having seen published before: thus the modification impressed on a beam by refraction centrically through a single thin lens is expressed by means of very simple formulæ, from which several properties of considerable elegance and some practical value might be directly drawn.

The theory of dispersion and achromatism is treated in the ordinary way. In the chapter on vision are introduced discussions, chiefly from Helmholtz, of the mechanism of accommodation and the principles of binocular vision. Then follows a clear and valuable chapter on telescopes and microscopes, a chapter on miscellaneous optical instruments, and a brief account of atmospheric refraction, mirage, rainbows, and halos.

It may seem ungracious to expect more where so much is given, but we could have wished that the theory of refraction through general systems had been treated more from an historical standpoint. A difficulty often felt in this part of the subject arises from the way in which the geometrical and analytical methods of different writers are liable to be intermixed. The book was probably in the press before a recent note by Lord Rayleigh had brought again into prominence the large share taken by the English opticions of last century, notably Cotes and Smith, in the development of the general theory of this branch of the subject.

The list of treatises and memoirs might be improved by consulting the bibliographies given by Helmholtz and Verdet.

It is a misfortune incident on the scheme of the book that it is seldom able to say the last word in relation to the more delicate arrangements of telescopes and microscopes, where diffraction plays an important part. This becomes very patent, for example, in the account of immersion objectives. The theory of diffraction as applied to optical construction is for the most part purely geometrical, and it would much increase the value and interest of books on geometrical optics if that theory were explicitly included, and the subject introduced by the consideration of light as wave-motion, instead of the artificial conception of the reflexion and refraction of rays.

As is usual in English text-books, selections of problems have been added at the ends of the chapters. In this case, Cambridge examination-papers of recent years have been largely drawn upon for questions, with the result that some are included which are not of much value as illustrations of the subject, though they may be very useful as tests of mathematical power. Indeed it seems open to question whether the practice of adding large collections of examples is not now overdone in this country; it certainly in some cases tends to unfit the books which contain them for the use of students who do not possess the advantage of tuition, or some guidance in selecting the few that will be of value for them.

The treatise is, on the whole, a most welcome addition to our optical text-books. Much of its contents, though fundamental and elementary, has only hitherto been accessible in English through Mr. Pendlebury's treatise on "Systems of Lenses"; and there is more that now appears in a text-book for the first time. The printing and general appearance of the book reflect great credit on all concerned with it. J. LARMOR.

OUR BOOK SHELF.

Shores and Alps of Alaska. By H. W. Seton Karr, F.R.G.S. (London: Sampson Low, 1887).

THIS is a very interesting account of a journey of exploration in a country which, as the author says, is probably destined soon to become better known. The most important part of the book is that which relates to the attempt made by Mr. Seton Karr and his companions upon Mount St. Elias. When this attempt was made, the combined "alpinism" of the climbers was "insignificant." Nevertheless, they achieved considerable success, and the writer has been able to present a vivid and striking record of their observations. The height of Mount St. Elias was differently estimated by the old navigators, and Mr. Seton Karr points out that it is the only mountain the real height of which has exceeded the first estimates made of it. The latest determination taken from Yakatat and from the United States Coast Survey schooner *Yukon*, gives 19,500 and possibly 20,000 feet. From its massive shape the mountain does not convey the impression of being quite so high as this, although "its whole altitude is presented to the eye, from its sharp summit down to the ocean at its foot." Of the scenery of which Mount St. Elias is the most prominent feature, Mr. Seton Karr writes most enthusiastically. He even goes so far as to say that "without a doubt the scenery at Yakatat is the most wonderful of its kind in the whole world." Seen early in the morning, when the air is remarkably transparent, the mountains seem "too ethereal to have any actual existence." The observer feels that "they cannot be anything except some unholy illusion that must dissolve and disperse when the sun rises."

LETTERS TO THE EDITOR.

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

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

Relation of Coal-Dust to Explosions in Mines.

THE suggestion in my former letter on this subject (vol. xxxiv. p. 595) that "keeping the ventilating air-current saturated with aqueous vapour" might prove the most effective way of rendering the dust in coal-mines innocuous, has, I am glad to see, been since shown to be practicable, in a South Wales colliery. Since the above date, I have considerably extended my research, with results that confirm the conviction therein expressed that many of the most disastrous colliery explosions during the last seven years in this northern district have been practically *dust explosions*, and therefore preventable; that the rough method of watering the floors only, or floors and sides, of the mines is delusive, since it leaves the most dangerous dust undisturbed, the upper and flocculent dust; and last, that probably the reasons why dust in dry pits does not explode more frequently are now within grasp. To this latter conclusion, with your permission, I will now briefly address myself. That every firing of a shot that is accompanied by flame in a dry and dusty pit does not produce an explosion is well known; that *sometimes* such firing of a shot does is unhappily also well known. That the local presence of gas, even in small amount, is sometimes the reason of this is universally acknowledged. That the amount and condition of the dust present (even in the practical absence of gas) is at other times the reason is now believed by many. Setting aside the *amount* of dust, which every one will allow must be an essential factor, and also the varying energy which the shot, blown out or not, develops, let us look at the other conditions. The temperature and hygroscopic state of the air-current is one most important factor, and consequently the concomitant temperature and hygroscopic state of the dust traversed by such current. Beyond this, the *degree of fineness* and the *constituents* of the dust will have much to say in the matter. The finer the particles the more readily will they ignite, and the more completely will they place their substance under the influences present. Thus ordinary screen coal-dust will not ignite when a common match is lighted and applied to it, but it will when finely pounded in a mortar. Now the dust resting on the baulks and upper portions generally of the ways will invariably so light and burn when dry, although the constituents vary greatly in different pits and in different seams of the same pit.

What are the ordinary *constituents* of coal-dust? Two, perhaps three, important substances, and others unimportant: important, as being inflammable in varying degrees; unimportant, either from their uninflammability or from their excessively small amount. The three important are mother of coal, or *dant; coal;* and certain coloured bodies, probably *spores*. The unimportant are shale or other stone dust, iron pyrites, lime flakes, and incidentals, as animal and vegetable matters, and the results of the wear and tear of the haulage and winning apparatus, &c. Dismiss these last, as only one needs any attention, the shale; and that special, not general.

Dant lights most readily; the red end of a used match is often sufficient to fire it, and then it burns itself out whether resting on wood or stone. Burned in a retort, it loses little weight, and the fumes it gives off will not ignite. Now, this dant is largely present in upper and flocculent dust, reaching in some specimens even 70 or 80 per cent. Dant clearly therefore is not itself dangerously explosive, yet is admirably fitted to act the part that tinder used to do, when it handed on the spark from the flint and steel to the old-fashioned brimstone match.

Coal forms a considerable part of all upper and flocculent dust, and constitutes the great mass of the bottom dust along intake haulage roads. Coal-dust (got as free from dant as possible) when pounded very fine ignites with some difficulty, burns at first somewhat fiercely and with considerable smoke, but generally goes out leaving a portion of the heap unburned. Placed on an iron plate and burned by heating the plate, it threw off scintillations, its fumes readily took fire, and forty grains of dust were reduced to one grain of ash. In a retort it gave off first much smoke which would not light; soon, however, the smoke lessened, when its fumes lit and burned with a long bright flame. Such coal-dust is manifestly capable of producing an explosion. Under favourable conditions it can produce a considerable amount of ordinary illuminating coal-gas, whose presence would convert the air-current into an explosive mixture. Therefore, adopting the former simile, as the dant is the *tinder*, so this coal is the *sulphur match*, as the shot flame or other initial cause is the *spark* struck from the flint and steel.

Spores.—Nearly all dusts (and I have examined many) have shown under the microscope few or many orange, brown, or reddish flakes, very often triangular in shape and with concoidal fractures. I have not yet examined thin sections of these coals, but the fragments present much the appearance presented by the spores in the well-known spore coals of the Bradford "Better Bed," and Leicestershire "Moira." If these coloured bodies originate in Lycopodian and other microspores or macrospores, they may play an important part, for the resinous nature of the microspores of the *Selaginella selaginoides*, &c., of our northern hills is so well known that they were formerly used in theatres to produce artificial lightning. As my experiments and inquiries in this direction are yet incomplete, I will only suggest that their presence may account for some dusts being so much more dangerous (as the German experiments have conclusively shown) than others, and add the hope that these words may lead others to pursue this inquiry. ARTHUR WATTS.

Bede College, Durham, May 26.

Science for Artists.

OF the various optical errors in this year's pictures, certainly that in the elegant scene (624) of the Queen's Accession, in the morning small hours of June 20, 1837, is largest and most hopeless. Neither a source of light at 93,000,000 miles, nor one at 93 inches, could cast the bar-shadows. It is impossible to say whether they are meant to be aërial in the dust or mist, or cast on the walls and wainscot. But for either they are equally preternatural, though not by diverging perspectively. If cast on the solids they would, instead of being straight, be crooking in and out over the mouldings. But if they are in aërial mist or dust, the error is in supposing the same eye can see more than one of such shadows at a time. The eye requires to be very nearly in the plane of the shadow seen, so that, of those cast by parallel things, as window-bars, only one could be seen by any single eye, and only as continuing the line of the bar itself. The bar and its mist-shadow could never meet at an angle, as they all do in this picture. Another error (now common) is in there being no more penumbra than if the sun were a star, or a small electric arc-light. EDWD. L. GARBETT.

Weight, Mass, and Force.

WITH reference to the extract, as to the language employed in which Prof. Greenhill invites my criticism, I have no doubt that to an engineer it would convey perfectly definite and intelligible information, and that one who has mastered the fundamental notions of dynamics as a science would be able to divine its meaning, but Prof. Greenhill would hardly maintain that the language is scientifically accurate, and that, however sufficient as a shorthand for the trained engineer addressing engineers, it is not full of pitfalls for the tyro.

There is no need to object to the statement that "the *weight* is 137,000 pounds," though it is just as easy to say, "the *mass* is 137,000 pounds." But that "the boiler carries 160 pounds of steam," I find, means that the pressure of the steam is 160 pounds (weight) *per square inch*, while "a 96-feet grade" means "a gradient of 96 feet *per mile*." Surely, except as a recognized shorthand for experts, the suppression of the words in italics is unjustifiable and liable to lead into error.

It is more important, however, to observe that (as in a great majority of the cases an engineer has to deal with) the question here discussed is essentially a *statical* one. The motion of the train considered is uniform (30 miles per hour), and the variations in pressure in the cylinders, &c., are avoided by taking the "mean effective pressure," so that there are no *accelerations* to be considered, and only, in fact, a balancing of forces. The question of *mass* therefore, (a purely *kinetic* notion), can hardly arise, and there is no room for confusion between mass and weight. R. B. HAYWARD.

Upper Cloud Movements in the Equatorial Regions of the Atlantic.

I AM sorry that the observations of so good an observer as Capt. D. W. Barker should not agree with my own, but I certainly never confounded what he calls high low-level clouds with the true high clouds.

When clouds are being propagated in a different direction from that in which they are being blown—as sometimes happens—it is exceedingly difficult to ascertain the real direction; but that would not account for the discrepancy between our observations.

My own researches were specially directed to the doldrums, and the history of the Krakatão dust entirely confirms my observations; but in some low latitudes—as in Cuba—the highest clouds are usually from about south-west. This, however, does not affect the doldrum districts. RALPH ABERCROMBY.

21 Chapel Street.

Fish Dying.

IN a large pool in this county, well stocked with fish, especially trout and roach, a considerable number of the roach have been found dead every day during the last week. They are in fair condition, and show no evidence of poison or of parasitic disease. There is a certain amount of current through the centre of the pool, but the ingress of water has been, of course, much reduced by the drought. The pool, however, covers many acres, and there are twenty feet of water in the deepest parts. Can any of your readers suggest a cause for the death of the roach, and a remedy? No other species appears to have suffered. F. T. MOTT.

Birstal Hill, Leicester, July 4.

THE DINNER TO PROFESSOR TYNDALL.

THE dinner to Prof. Tyndall, as we stated last week, was going on at Willis's Rooms on Wednesday evening as we went to press. It was attended by as large and distinguished a company as ever assembled to do honour to a man of science. The chair was taken by Prof. Stokes, President of the Royal Society, who had acted as Chairman of the Organizing Committee. Among those who had consented to serve on the Committee were the Marquis of Salisbury, the Duke of Devonshire, the Duke of Argyll, the Right Hon. J. Inglis, the Earl of Rosse, Earl Granville, Sir F. Abel, Prof. Adams, and many others holding high positions in connexion with scientific and learned Societies, and Mr. J. Norman Lockyer and Mr. A. W. Rücker had acted as honorary secretaries to the Committee. Among those who attended the dinner were the Earl of Derby, Earl Bathurst, the Earl of Lytton, Sir F. Leighton, Lord Rayleigh, Lord Thurlow, Sir J. Lubbock, M.P., Sir W. Bowman, Sir F. Bramwell, Sir I. Lowthian Bell, M.P., Sir J. Lister, Sir H. Roscoe, M.P., Sir G. Richards, Lord A. Russell, Sir F. Pollock, Sir Lyon Playfair, M.P., Sir Prescott Hewett, Prof. J. C. Adams, Colonel Donnelly, Sir J. Hooker, Prof. Asa Gray, Prof. Flower, Dr. A. Geikie, Dr. Hirst, Mr. W. Crookes (President of the Chemical Society), Mr. G. B. Bruce (President of the Institution of Civil Engineers), Mr. D. Adamson (President of the Iron and Steel Institute), Dr. J. Evans (President of the Society of Antiquaries), Prof. B. Stewart (President of the Physical Society), Prof. Judd (President of the Geological Society), General Strachey (President of the Royal Geographical Society), Sir J. Fayrer, Sir H. Wilde, Sir H. Doulton, Sir J. Caird, Sir P. Magnus, the President of the Alpine Club, Profs. Frankland, Debus, Tilden, Ray Lankester, Liversedge, G. Darwin, Dewar, M. Foster, Carey Foster, Odling, Gamgee, W. G. Adams, Clifton, Humphry, and Dallinger, Messrs. Warren de la Rue, Gill, Kempe, J. Hopkinson, H. Pollock, E. Wood, Brudenell Carter, Romanes, Pengelly, Preece, Ellis, Vernon Harcourt, R. H. Scott, and others.

At the close of the dinner Mr. Norman Lockyer, at the request of the Chairman, read a list of absentees, from most of whom had been received letters expressing strong sympathy with the object of the banquet, and admiration of the career of Prof. Tyndall. Among the writers were the Marquis of Salisbury, Mr. Goschen, Mr. W. H. Smith, Lord Cranbrook, the Marquis of Ripon, the Earl of Rosse, Lord Monk Bretton, Profs. Max Müller, J. R. Seeley, T. H. Huxley, Sir F. Abel, and about thirty others identified with science and literature.

The first toast was "The Queen," and

The Chairman in proposing it said that the recent celebration of the Jubilee diminished the necessity for saying many words in commendation of the toast. All hearts were affected by the Queen's letter, in which she so touchingly acknowledged the manner in which she had been received. Those who were present at the scene in the Abbey were touched by the exhibition of family devotion and affection which took place at the conclusion of the service, when the Royal Family saluted her who was at the same time Sovereign and mother, and received from her the kiss of affection. And as on that occasion the Royal Family was united with the Sovereign, so on the present occasion, in drinking the health of Her Majesty, they would mentally include the health of the Prince and Princess of Wales and the rest of the Royal Family.

The toast was drunk with all the honours.

The Chairman in proposing the toast of the evening said:—My Lords and Gentlemen,—I now come to the toast of the evening, "The Health of Dr. Tyndall," and may he long enjoy the leisure which he has so well earned. A social gathering like the present is not an occasion on which it is desirable to enter into detail as to the scientific labours of a man, however eminent. Yet the circumstances of the present meeting seem to demand that I should say a few words on some of Dr. Tyndall's researches. Some of his earliest scientific work related to diamagnetism and magnecrystallic action, and in part of this he was associated with the well known German physicist Knoblauch. But I cannot dwell on these now. And I will even dismiss with this brief mention his researches on the properties of ice and his application of them to the theory of glaciers and the observations which he made in common with his friend and colleague Prof. Huxley, whose necessary absence from among us to-night we so much regret. If I be not trespassing too much on the patience of those who listen to me, I would wish to say a little more on that elaborate series of researches, forming no less than six separate papers in the Philosophical Transactions, in which Dr. Tyndall investigated the relation of simple and compound gases and of vapours to radiant heat, especially radiant heat from sources at a moderate temperature. According to his researches, while the main constituents of the earth's atmosphere, nitrogen and oxygen, are practically diathermous, at least with regard to radiations which can traverse rock-salt, as we know that by far the greater part of those that we have to deal with can, such is far from being the case with other gases equally transparent with regard to light. Dr. Tyndall found that as a rule the more complex the composition of a gas the greater is its defect of diathermancy. To confine ourselves to the two gases which occur in the atmosphere mixed with its main constituents—I allude of course to carbonic acid and to water in the gaseous state of vapour—he found that both, especially the latter, which likewise is present in by far the larger quantity, are very distinctly defective in diathermancy, and he concluded that the main part of the absorption of solar heat in passing through the atmosphere, absorption as distinguished from scattering, is due to the watery vapour which it contains. From this result he drew important inferences as to atmospheric temperature and climatological conditions. Dr. Tyndall's researches on the relation of gases to radiant heat came naturally before me during my long tenure of office as one of the Secretaries of the Royal Society; and for my own part I may say that it seemed to me all along that the results were established on so firm a basis, and the conclusions regarding the invisible radiations were so perfectly analogous to what we know to be true regarding the visible ones, where the investigation is comparatively easy, that the work bore on it the stamp of truth. The conclusions were not, however, accepted without opposition. In the late Prof. Magnus Dr. Tyndall met a foeman worthy of his steel; a foeman, however, only in the sense of an intellectual athlete; for socially I doubt not they were the firmest friends, and their friendship was even cemented by the fact that they were both alike seeking after truth in a similar subject. But truth only gains by opposition: its defenders are led to engage in fresh researches, which end in strengthening its foundations. I think that the validity of Dr. Tyndall's results is now generally admitted. If some hesitation is still felt, it arises mainly, I think, from misconception; from imagining that assertions which were meant to apply only to heat-rays of such refrangibilities as to be absorbed by water were meant to be affirmed of the invisible radiations generally which lie beyond the extreme red. The time reminds me that I must only very briefly refer to another investigation in which Dr. Tyndall has more recently been engaged, and of which the interest is biological while the means of investigation are physical; I allude, of course, to the question of abiogenesis. Here, again, Dr. Tyndall was working on contested ground, and the objections of opponents stimulated him to fresh inquiries, which resulted in the continual strengthening of his negative conclusions. In the course of his work he was led, for instance, to the discovery of the great difference which exists between the germs of microscopic creatures and the creatures themselves, in relation to their power of resisting the destructive influence of a high temperature. This discovery not only detected a source of error in some experiments which had seemed to favour the hypothesis of abiogenesis, but threw important light on the conditions which must be fulfilled in order to secure complete sterility. But original research is not the only way in which a man can advance the cause of science. All-important though it is, it nevertheless often happens that an original investigation is too abstruse to be followed by more than a few experts; nor is it by any means necessarily the case that an eminent investigator is equally successful in expounding to others, especially to a mixed audience, the results at which he himself or other investigators may have arrived. The general diffusion of science depends largely on the clearness with which its leading principles and results are expounded, whether by lectures or by treatises, in which, while they are scientifically sound, popularity of style and general readableness are not sacrificed to the dry exactness of scientific detail. Most of us have had opportunities, whether at the Royal Institution, with which the name of Tyndall has so long been connected, or elsewhere, of being impressed with the singularly lucid style and graphic expression with which he expounded to his audience the salient points of the scientific subject which he brought before them. Nor was it only in clearness of verbal exposition that he excelled; the manipulative skill with which his original investigations were carried on served him in good stead in his more popular expositions; and by the aid of that "domestic sun," which even the murky atmosphere of a London winter could not obscure, he was enabled in very many cases to exhibit to the audience the actual results of experiments which had first been carried out in the quiet of the laboratory. Nor is it our own countrymen alone who have had the benefit of Dr. Tyndall's lucidity of exposition. Our friends across the ocean have flocked to hear and have appreciated the lectures which he has there delivered as a free gift to Transatlantic science. But oral lectures, after all—the lectures at least of one individual—can only reach a fraction of the community; nor do they admit of that pause for thought which the learner requires in endeavouring to make himself master of a new subject. But the same qualities of mind which enable a man to be a clear and interesting lecturer fit him also to be the author of eminently readable books; and for the general diffusion of science which is taking place we owe much to the writings of Dr. Tyndall. My lords and gentlemen, I fear that I have trespassed too long upon your time, and I will therefore now conclude by asking you once more to drink to the health of Dr. Tyndall. (The toast was drunk with great enthusiasm, the company rising.)

Professor Tyndall, on rising to respond, was received with loud cheers, the company rising. He said:—Mr. President, my Lords, and Gentlemen,—When the project of a dinner was first mentioned to me by a very old and steadfast friend of mine, who, to my regret and his, is not here to-night, had any dream, or vision, of the assembly now before me risen on my mind's eye, I should have declined the risk of standing in my present position; for I should have doubted, as I still continue to doubt, my ability to rise to the level of the occasion. Gratitude, however, is possible to all men; and I would offer you, Sir, my grateful thanks for the manner in which you have proposed this toast; I would thank with equal warmth an assembly which, in intellectual measure, is, probably, as distinguished as any of the same size ever addressed by man, for the way in which they have received it; and I would extend my thanks to my friends of the Department of Science and Art, for their spontaneous kindness to an old colleague, who for many years lent his humble aid to the Department in diffusing sound scientific knowledge among the masses of the people. My own scientific education began late. It had, of necessity, to be postponed until after I had reached the age of seven or eight and twenty. Notwithstanding this drawback, in learning, teaching, and working in the laboratory, I have been permitted to enjoy a spell of thirty-nine years. In 1850, during a flying visit from Germany to England, I stood, for the first time, in the bright presence of Faraday. In February 1853, I gave my first Friday evening lecture in the Royal Institution; and three months afterwards, on the motion of Faraday, the old Chair of Natural Philosophy, which had been filled at the beginning of the century by Thomas Young, was restored, and to it I was elected. It causes me genuine pleasure to think that I shall be succeeded in that Chair by so true and so eminent a man of science as Lord Rayleigh.

It is not my intention to overburden you with egotism to-night; but, casting an earnest glance back upon the past, a few words seem due from me to the memory of one or two of the group of good men, no longer with us, with whom I was so intimately associated. Regarding Faraday I will confine myself to stating that years have not altered my estimate of the beauty and the nobleness of his character. He was the prince of experimental philosophers; but he was more than this—in every fibre of his mind he was a gentleman. It is, however, of two of our honorary secretaries that I wish now to speak; premising that, for the first seven years of my life in the Royal Institution, the post of honorary secretary was held by a cultivated and very worthy gentleman, the Rev. John Barlow. From 1860 to 1873—that is, for a stretch of thirteen memorable years—I had the happiness of working hand in hand with Dr. Bence Jones. Never in my experience have I met a man more entirely and unselfishly devoted to the furtherance of scientific work. I hardly like to mention the following incident, because it furnishes but a scanty measure of his devotion. On one occasion I was in need of funds to carry out some experiments of a delicate and costly character. Bence Jones came to me, and after some hesitation—for he knew that money was likely to raise a difficulty between us—he said, with earnestness: "Dear Tyndall, behave as my friend; do me the favour and the honour of devoting this to your investigation. There is more, if you need it, where that came from." He handed me a cheque for £100. Had I asked for £1000, he would have given it to me, and the world, as far as he was concerned, would have been none the wiser. Bence Jones was a strong man, and liked to have his own way. At first, as was natural, we sometimes surged against each other; but these little oppositions were rapidly adjusted, and for many years before his death the tie of brother to brother was not truer or tenderer than that which united myself and Bence Jones. On my return from the United States I found him dying. In fact, the knowledge of his condition caused me to take leave, earlier than I otherwise should have done, of a people that I had learnt to trust and love. Soon after my return I saw him lowered into the grave.

The death of Bence Jones, whose steadfast loyalty to the Institution he loved so well, showed itself to the last, was a sore calamity to be met. At that time one man only seemed fitted to supply his place. That man was the beloved and lamented William Spottiswoode. To him I appealed to stand by the Institution at a critical hour of its fortunes. He had his own mathematical work on hand, and he was too well acquainted with the duties of our honorary secretaryship to accept them lightly. After much reflection, he wrote me a letter regretfully but distinctly declining the office. But he reflected a second time. He knew that his refusal would cause me pain, and his affection for me prevailed. When, therefore, the letter of refusal—for he sent it to me—came, it was accompanied by a second letter, cancelling the refusal and accepting the post. With William Spottiswoode I had the happiness of working in close companionship for six years. The diligence, wisdom, and success with which he discharged his onerous duties—the princely hospitality which shed a glow upon the office while he held it—are well remembered. Of the dignity with which he afterwards filled the high position now occupied by the illustrious man who presides here this evening it is needless to speak. Him also we have seen lowered to his rest, amid the grief of friends assembled to do honour to his memory. Such were the men who served the Royal Institution in the past; and their example has been worthily followed by other men of eminence, still happily amongst us. Never was an institution better served than the Royal Institution, and not by its honorary secretaries alone. With singleness of purpose and purity of aim, its successive Presidents, Boards of Managers, and honorary treasurers have unswervingly promoted the noble work of investigation and discovery. May they never lower the flag which, for well-nigh a century, they have kept victoriously unfurled.

The year after my appointment I was called upon to deliver, in conjunction with Dr. Whewell, Faraday, Sir James Paget, and some other eminent men, one of a series of lectures on scientific education. I then referred with serious emphasis to the workers in our coal-mines, and to the terrible perils of their occupation. I pointed to the intellectual Samsons toiling with closed eyes in the mills and forges of Manchester and Birmingham, and I said: "Give these toilers sight by the teachings of science, and you diminish the causes of calamity, multiply the chances of discovery, and widen the prospect of national advancement." Thus early, you will see, I was alive to the importance of technical education; and I am no less alive to it now. You will not, therefore, misunderstand me when I say that to keep technical education from withering, and to preserve the applications of science from decay, the roots of both of them must be well embedded in the soil of original investigation. And here let it be emphatically added, that in such investigation practical results may enter as incidents, but must never usurp the place of aims. The true son of science will pursue his inquiries irrespective of practical considerations. He will ever regard the acquisition and expansion of natural knowledge—the unravelling of the complex web of nature by the disciplined intellect of man—as his noblest end, and not as a means to any other end. And what has been the upshot of science thus pursued? Why, that the investigator has over and over again tapped springs of practical power which otherwise he would never have reached. Illustrations are here manifold. I might point to the industries which affiliate themselves with Faraday's discovery of benzol, and with his discovery of the laws of electrolysis. But I need not go further than the fact that in this our day a noble and powerful profession has been called into existence by his discovery of magneto-electricity. The electric lamps which mildly illuminate our rooms, the foci which flood with light of solar brilliancy our railway-stations and public halls, can all be traced back to an ancestral spark so small as to be barely visible. With impatient ardour Faraday refused to pause in his quest of principles to intensify his spark. That work he deliberately left to others, confidently predicting that it would be accomplished. And, prompted by motives both natural and laudable, but which had never the slightest influence on Faraday, others have developed his spark into the splendours which now shine in our midst.

It would be a handsome Jubilee present, if it were a possible one, to roll up the career of Faraday into portable form, and to offer it to the Queen as the achievement of one of Her Majesty's most devoted subjects during her own reign. Faraday's series of great discoveries, however, began in 1831, which throws his work five or six years too far back. During the rest of his fruitful life he was a loyal son of the Victorian epoch. But, passing beyond the limitations of the individual, what is science, as a whole, able to offer, on the golden wedding of the Queen with her people? A present of the principle of gravitation—a handing over to Her Majesty of the bit and bridle whereby the compelling intellect of Newton brought the solar system under the yoke of physical laws—would surely be a handsome offering. I mention this case of known and conspicuous grandeur, in order to fix the value of another generalization which the science of her reign *can* proudly offer to the Queen. Quite fit to take rank with the principle of Gravitation—more momentous if that be possible—is that law of Conservation which combines the energies of the material universe into an organic whole; that law which enables the eye of science to follow the flying shuttles of the universal power, as it weaves what the Earth Spirit in "Faust" calls "the living garment of God." This, then, is the largest flower of the garland which the science of the last fifty years is able to offer to the Queen.

The second generalization is like unto the first in point of importance, though very unlike as regards its reception by the world. For whereas the principle of Conservation, with all its far-reaching, and, from some points of view, tremendous implications, slid quietly into acceptance, its successor evoked the thunder-peals which it is said always accompany the marriage of thought and fact. For a long time the scent of danger was in the air. But the evil odour has passed away; the air is fresher than before; it fills our lungs and purifies our blood, and science, in its Jubilee offering to the Queen, is able to add to the law of Conservation the principle of Evolution.

In connexion with these victories of the scientific intellect, I have mentioned neither persons nor nationalities, holding, as Davy expressed it, when the Copley Medal was awarded to Arago, that "science, like Nature, to which it belongs, is neither limited by time nor space. It belongs to the world, and is of no country and no age."

Still, it will not be counted Chauvinism if I say that in the establishment of these two great generalizations Her Majesty's subjects have quitted themselves like men. With regard to a third generalization, neither England nor Germany has been idle. Omitting the name of many a noble worker in both countries, the antiseptic system of surgery assuredly counts for something in the civilized world. And yet it is but a branch of a larger generalization, of momentous import, which in our day has been extended and consolidated to an amazing degree by a Gallic investigator. To some, however, any flower culled in this garden will be without odour. Let me therefore add a sweet-scented violet under the name of spectrum analysis which, besides revealing new elements in matter, enables the human worker to stretch forth his hand to sun and stars, to bring samples of them, as it were, into his laboratory, and to tell us, with certainty, whereof they are composed. Surely all these, and other discoveries of high importance, taken and bound together, form an intellectual wreath, not unworthy of Her Majesty's acceptance in her Jubilee year.

A short time ago an illustrious party leader summed up the political progress of the Queen's reign. What I have said will, I trust, show that the intellectual world is not entirely compounded of party politics—that there is a band of workers scattered over the earth whose arena is the laboratory rather than the platform, and who noiselessly produce results as likely to endure, and as likely to influence for good the future of humanity, as the more clamorous performances of the politician.

One word more. On the continent of Europe, kings had been the nursing fathers, and queens the nursing mothers, of science; while Republican Governments were not a whit behind in the liberality of their subventions to scientific education. In England we had nothing of this kind, and to establish an equivalent state of things we had to appeal, not to the Government, but to the people. They have been roused by making the most recondite discoveries of science the property of the community at large. And as a result of this stirring of the national pulse—this development of self-reliance—we see schools, colleges, and universities now rising in our midst, which promise by and by to rival those of Germany in number and importance.

It is time that I should cease. But before doing so, I would ask—as they do in the House of Commons—permission to say a word in personal explanation. I have climbed some difficult mountains in my time, and after strenuous effort for a dozen hours or more, upon ice, rock, and snow, I have not unfrequently reached the top. I question whether there is a joy on earth more exhilarating than that of the mountaineer, who, having achieved his object, is able to afford himself, upon the summit, a foaming bumper of champagne. But, my lords and gentlemen, the hardest climb, by far, that I have ever accomplished, was that from the banks of the Barrow to the banks of the Thames—from the modest Irish roof under which I was born to Willis's Rooms. Here I have reached my mountain-top, and you—God bless you!—have given me a bumper which no scientific climber ever before enjoyed.

Sir Frederick Pollock, in proposing the toast of "Literature and Art," said that on most occasions similar to the present one this toast was a triple one, and included the three sisters—Science, Literature, and Art. But this evening they were assembled together to do homage to science, in the person of one of its most distinguished votaries, and for the time the room in which they had met became a temple of science. In such a temple the principal figure, standing upon the pedestal appropriated to the presiding goddess, must be that of Science, and to her due rites had been already rendered. But for the sisters Literature and Art room must be found also in the sacred edifice; they too must have their altars and their shrines. He pointed out that the highest powers of the imagination were required by the man of science, as well as by the poet and the painter, and instanced the prediction by Fresnel of the bright spot in the centre of the shadow of a disk; and the suggestion made to Goethe of his theory of the development of the vertebrate skeleton, by his accidental observation of the scattered fragments of the deer's skull lying in his path. He adduced the names of Aristotle, Bacon, and other great men who had connected literature with science; and instanced Leonardo da Vinci, and Sir Christopher Wren, one of the founders of the Royal Society, as linking together science and art. He accordingly had great pleasure in submitting for acceptance "Literature and Art," coupling with it the name of Lord Lytton, who was not only a distinguished representative of modern literature, but had also a distinct hereditary claim to represent that of the last generation; and Sir Frederick Leighton, the distinguished President of the Royal Academy.

The Earl of Lytton,—In returning thanks for "Literature" upon an occasion when we are all met to honour science in the person of one of its most illustrious adepts, I cannot but forcibly remember that we are living in an age when inquiry is more active and more widespread than conviction, and it is natural that in minds of the highest order under these conditions even the imaginative faculty should be more powerfully attracted to scientific research than to purely literary production. But inquiry, I think, would be very sterile if conviction in some form or another were not the ultimate fruit of it, and I think that for a period of really vigorous, creative, imaginative art we must look forward in the course of scientific research to some such general re-settlement of ideas upon the basis of a common conviction—which is not now, perhaps, altogether attainable—as may enable art, instead of representing, as it does now, merely the mental attitude of the individual poet or the individual painter, once more to become the universally spontaneous and universally recognized imaginative expression of ideas and emotions which are common to a whole generation or a whole community. If that is the case, if science is ultimately to render this great service to literature and art, surely in the meanwhile we cannot but gratefully appreciate the literary labours of those men of science who in our own and in other countries are promoting or have promoted this result, not only as original discoverers but also as popular and powerful interpreters of scientific fact, and who in this latter capacity have already enriched contemporary literature with writings of rare literary value. If, instead of returning thanks for literature, I were permitted to return thanks on behalf of literature to those writers who have powerfully influenced my own generation, not only by thoughts which stimulate and instruct the intellect, but also by words which stir and elevate the heart, then assuredly I should ask leave to mention some distinguished names which occupy in the field of literature a position only second to the high rank they hold in the hierarchy of science; and foremost among those names I should not hesitate to mention with a special personal gratitude the name of the illustrious man who is the honoured guest of this great assembly to-night. I cannot say it is as a student of science that I myself have studied the writings of Prof. Tyndall, but this I can say, and most truly, that those writings have been to me, from a very early period of my life, companions so cherished that I learnt to look upon their writer as a dear personal friend and benefactor long before it was my privilege to be admitted to his personal intimacy. I believe that scientific research has succeeded in establishing on a physiological basis certain evidences of intelligence even among oysters; and certainly there is, I think, one form of intelligence which is conspicuously displayed by the oyster which might perhaps be cultivated with advantage by after-dinner speakers in my position. The oyster knows when to shut up. Admonished by that very interesting and suggestive fact in natural history, what little else I have to say upon behalf of literature I shall confine to the expression of a hope that the well-deserved relaxation from his more systematic scientific labours in connexion with the Royal Institution may enable my valued and honoured friend Prof. Tyndall to enjoy an increased leisure for the continued cultivation of that department of literature which has already been so richly adorned by his admirable writings.

Sir F. Leighton, who was to have responded for "Art," had been obliged to leave before this stage of the proceedings in order to receive Royal visitors at the Academy.

Sir Lyon Playfair, M.P., proposed the next toast, "The Public Services in Relation to Science." He said that undoubtedly the public services were intimately connected with science and were profoundly affected by its progress, but, unfortunately, the truth was only beginning to be recognized in this country. In the United States scientific men were attached to all public offices, but in this country the attachment was of the loosest possible character. Nevertheless, science had undoubtedly affected our public services in the most profound way. The telegraph had altered the whole system of commerce and also the methods and the powers of government. There was to be a great naval review next month; it would be interesting to imagine Elizabeth's thirty small ships, which conquered the Armada, sailing through two miles of modern ironclads. The largest piece of ordnance used in the Crimean War cost less than a single shot fired from the huge guns of our ironclads. But it was in peace rather than in war that science rejoiced in aiding government. A strong feeling was arising that we must improve our intellectual position as a nation, and this at last was being recognized by the Government. A material index of progressive civilization had always been desired. Liebig contended that the best index of civilization was the quantity of soap consumed. When the Queen ascended the throne we consumed per head 7¾ pounds of soap, and now we use 10 pounds per head. The consumption of paper was a more reliable index. At the commencement of the Queen's reign the consumption was 1½ pound of paper yearly; now it was 12 pounds; while in the United States it was 10 pounds, in Germany 9 pounds, in France 8 pounds, and in Italy 4 pounds. But the main question was whether we were developing the national intellect at the same rate as other nations. Our general intelligence is still high, but our trained scientific intelligence is low. Our secondary education in all matters relating to science was far behind that of the United States, Germany, and France. Neither the Government nor the people governed could go on in simple faith on our practical aptitudes by relying on a blind and vain empiricism, like a tree severed from its roots.

The Earl of Derby,—My Lords and Gentlemen: You have asked me to return thanks on behalf of the public services in connexion with science, and Sir L. Playfair, in relation to that toast, has referred to the increased consumption of soap in this country. I have attended a good many public dinners, and I must say that the expenditure of what is vulgarly called soft soap has been great this evening. I am sincerely grateful to him for the quantity of that article which it has pleased him to expend upon me. But really the toast is one which hardly any man is competent to do justice to, and certainly not one who like myself has no connexion with science, except a sincere admiration and respect for its professors, and whose connexion with the public service has only been that of a Parliamentary chief. Under our system the Parliamentary head of a department is mainly concerned to keep it in harmony with the House of Commons and with the public. He has to warn the permanent officials that something that is done, or something that is left undone, or proposed to be left undone, is what public opinion will resent; and, on the other hand, he has to tell outsiders that the things they ask him and press him to do are things unwise or impossible from an administrative point of view. That is useful; it is certainly laborious, and it is often a difficult function; but it does not involve much more scientific knowledge than is implied in driving a cab through a crowded street. It does require some knowledge of men, but that is a department of study to which, as yet, no scientific formula has been found to apply. Sir L. Playfair told us, and I was sorry to hear it, of the loose connexions which exist between science and the Government. I can only say that I am entirely ignorant of any such immoral transactions. But if the departments were better represented here and if they could speak for themselves, I am sure that they would not be backward in acknowledging their obligations to science. The Treasury would tell you that those useful though sometimes ungraceful coins in which our dinner is paid for would not circulate through Europe as they do if they had not been subjected to a careful and complicated process, requiring scientific knowledge. The Excise might tell you, if they chose, of the frauds that might be perpetrated upon the revenue and the public if it were not for the careful and scientific examination of all taxable articles. The Post Office would find no difficulty in acknowledging its obligations to Watt and to Stephenson—for where would postal revenue be without railways?—and in later days to investigators whose researches made the telegraph possible. But the fighting departments, or the spending departments, which is their more common name in Downing Street, would have the most to return thanks for. They would point to the modern ironclad, the most elaborate, the most complete, and the most costly, of all contrivances in which the art of construction has been utilized for purposes of destruction. They would tell you how the chemist, metallurgist, the engineer, the electrician, the mathematician, have all contributed their share to that extraordinary result of science and skill. The War Office would follow the Admiralty. They would not say, as Frederick of Prussia did, that Providence is on the side of the biggest battalions, but they might possibly say that Providence was generally on the side of the army which could bring into the field the most scientifically effective weapon in the hands of the most carefully-trained soldier. If I were to turn to the line of business with which I had once something to do, I might ask any diplomatist or any statesman to explain to you how largely the position of Egypt, and, with that, the diplomacy of Europe, has been affected by that little scratch which the genius of M. de Lesseps drew across the Egyptian sands; and if, as is quite possible, the coal-carrying power of steamers and their speed and their economy are largely increased—I do not speak of those wilder predictions according to which steam is to be superseded as the motor power by something more efficient—suppose I say the large increase of the coal-carrying power of steamers, and the results to which I have referred may be again reversed; and again, at least in war time, the route to India may lie through the South African seas. If I speak of the colonies, everyone conversant with that department would admit that if we had had the ocean telegraph in existence twenty-five years ago half our little wars beyond the seas would never have taken place, and those that have taken place would have been disposed of in half the time. I know that these things are common-place, but I cannot help that. If I could tell you what the next great discovery was going to be, that would not be common-place. But, unfortunately, that is not in my power; and if it were I do not think I should be in a hurry about it, because I have observed that those who are the first to announce a discovery are generally rewarded by having a remarkably unpleasant time. But however great may be the gains which we have derived from the applications of science, they are nothing as compared with those which will and do accrue to us from the acceptance of scientific habits of thought. That is coming already, and it will come more in a not remote future. We have many things in this age and country of which we cannot boast, but we may boast that in science England has done something more than hold her own. The great name of Darwin will survive, it may be, the British Empire itself, and with him will be remembered some others also, whom to single out might perhaps be invidious. But we may be sure of this, that among their names will be included the name of our distinguished guest of to night. It is a common complaint that politicians have done nothing for science. In that I do not agree. They have done the best they could for it—they have let it alone; they have not corrupted it by their intrigues, nor vulgarized it by their squabbles; and they being what they are, and science being what it is, that is probably the best service they could have rendered it.

Lord Rayleigh proposed "The Health of the Chairman."

Prof. Stokes briefly responded, and the company, which numbered nearly two hundred, separated.

THE ELEVEN-YEAR PERIODICAL FLUCTUATION OF THE CARNATIC RAINFALL.

MORE than fourteen years ago, in the pages of NATURE, Mr. Norman Lockyer first drew attention to an apparent periodical variation of the rainfall registered at the Madras Observatory; which seemed to be such that it reached a maximum and a minimum alternately, at about the same epochs as the corresponding phases of the sunspot frequency. The idea, once started, was followed up by others, among whom perhaps the best known is Dr. (now Sir) W. W. Hunter, whose pamphlet on the subject, without laying claim to any originality as regards its subject-matter, attracted very general attention by the charm of its style, and also by its attempt to identify the periodical occurrence of famines in Southern India with the epochs of minimum rainfall shown by the Madras registers.

When, however, the data on which these speculations were based came to be critically examined, the general verdict of men of science was that the conclusions were "not proven." This was certainly my own opinion; and General R. Strachey, in a lecture delivered before the Royal Institution in 1877, and, at greater length, in a paper communicated to the Royal Society in May of the same year, showed that any attempt to educe a true cyclical variation from the recorded figures, ended in a negative result. Admitting that when the annual quantities were tabulated in eleven-year cycles, the means of the homologous terms seemed to indicate a period of maximum between the third and seventh years, and of a minimum between the eighth and second years, he found that, when the mean difference of the individual years from the supposed periodical means was compared with the mean difference of the former from the arithmetical mean of the whole series, the results differed but little.

It was further shown by myself that the supposed connexion between the periodicity of the Madras (Observatory) rainfall and that of famines in Southern India was by no means so intimate as might appear at first sight. The famines in question had occurred sometimes in one part of the peninsula, sometimes in another, by no means always in the country around Madras; but no other station in the peninsula (of those then available for the inquiry) showed even such an approach to a periodical variation of the rainfall as did the Madras Observatory.

At this stage matters have since remained, with the exception that, in 1879, an apparent periodical fluctuation of a very different character was brought to notice by Messrs. Hill and Archibald in the winter rainfall of Northern India. This, which has an interest of its own, I shall not further discuss at present.

In the course of a general investigation of the rainfall of India, the first part of which only has been as yet published ("Indian Meteorological Memoirs," vol. iii. part 1), I have lately had occasion to reconsider these old questions, and to re-examine them by the light of the accumulated data of the last twenty-two years. For convenience of discussion, I have divided India and Burmah into twenty-four rainfall provinces, one of which is the Carnatic.

This consists of the plain below the Eastern Gháts, occupying the south-east of the peninsula, and extending from Cape Comorin to the mouths of the Kistna. Its area may be taken as 72,000 square miles. The town of Madras is situated nearly midway on the sea-coast of this province, and is a fairly representative station; but, in addition to the rainfall registers of the Madras Observatory, I have those of thirty-nine other stations, pretty equally distributed through the province; most of them extending back to 1864. The Carnatic is distinguished by one important peculiarity in the season of its chief rainfall. During the spring months, it receives a certain amount of rain, in common with the southern and eastern provinces of India generally; but while the heavy summer rains are falling in Central and Northern India, and also on the west coast of the peninsula, the Carnatic is but little affected by them. In its southern districts, indeed, the rainfall of June and July is less than that of May; and it is not until the rains are over in North-Western India, viz. in October and November, that this province receives the chief and heaviest rainfall of the year. Hence the vicissitudes of the rainfall of the summer months, which are all important in Central and Northern India, are relatively less important in the Carnatic, even if they affect that province in the same manner as Northern India—and this is far from being always the case—and as a final result the annual fluctuation of the Carnatic rainfall often differs widely from that of other provinces in the peninsula.

The mean annual rainfall of the Carnatic may be taken in round figures at 35 inches, which is about 7 inches less than the general average of the whole of India. The following table gives the annual variation from this average for the twenty-two years 1864–85, which results when the annual total fall of each individual station is compared with its local average, and the mean of all the differences taken for each year.

Annual mean rainfall variation of the Carnatic rainfall.

	Inches.		Inches.
1864 ...	− 5·0	1875 ...	− 5·2
1865 ...	− 5·0	1876 ...	−13·2
1866 ...	− 4·0	1877 ...	+ 8·3
1867 ...	− 9·4	1878 ...	0
1868 ...	− 4·6	1879 ...	+ 2·3
1869 ...	− 0·3	1880 ...	+ 7·0
1870 ...	+ 1·8	1881 ...	− 2·1
1871 ...	+ 5·5	1882 ...	+ 4·4
1872 ...	+11·5	1883 ...	+ 5·2
1873 ...	− 0·1	1884 ..	+11·6
1874 ...	+ 7·3	1885 ...	− 1·1

During the first thirteen years (with the exception of 1873) the fluctuation, here shown, is remarkably distinct and regular. The rainfall reached a minimum in 1867, then rose steadily to a maximum in 1872, and after a drop in 1873, and partial recovery in the following year, fell rapidly to a second minimum in 1876. From 1877 to 1881 it oscillated considerably, but thereafter rose again steadily to a second maximum in 1884, dropping again in 1885 to something below the average. Thus we have, apparently, two complete cycles in the twenty-two years; the first remarkably regular, the second less so, but with the periodical fluctuation still dominant.

In order to ascertain with somewhat greater precision the probable character of this periodical fluctuation in an eleven-year cycle, the coefficients of the first two periodical terms of the harmonic formula have been computed, taking 1864 as the initial epoch. These coefficients are—

$$u' = 5{\cdot}340 \text{ inches.} \qquad u'' = 2{\cdot}873 \text{ inches.}$$
$$U' = 206^\circ\ 29' \qquad U'' = 247^\circ\ 15'$$

and the values of the eleven annual phases of the cycle thus found are—

			Inches.
1864	and	1875	− 5·1
1865	,,	1876	− 6·7
1866	,,	1877	− 4·4
1867	,,	1878	− 1·5
1868	,,	1879	− 0·6
1869	,,	1880	− 0·7
1870	,,	1881	+ 0·8
1871	,,	1882	+ 4·4
1872	,,	1883	+ 7·3
1873	,,	1884	+ 5·9
1874	,,	1885	+ 0·5

Taking the differences of these values from the recorded rainfall of each of the twenty-two years, the mean deviation of the latter in any one year from its periodical value is found to be—

$$\pm\ 3{\cdot}5 \text{ inches,}$$

which is only one-fourth of the range of the periodical variation as above determined; and the probable error ϵ, of the periodical value, as found by the formula—

$$\epsilon = 0{\cdot}6745 \sqrt{\frac{\Sigma\,(v^2)}{n(n-1)}},$$

is ± 0·70 inch.

On the other hand, the mean deviation of a single year from the general average is

$$\pm\ 5{\cdot}2 \text{ inches,}$$

and the probable error of that average ± 0·94 inch.

What, then, is the numerical probability of the cyclical variation, thus determined, being a true periodical fluctuation, representing a regularly recurrent phenomenon? As a general problem this cannot be solved, because we do not know all the variations to which the rainfall may conceivably be subject. But we can compare the relative probability of this particular variation being the result of a periodic law, and of its being a mere fortuitous series of variations from a constant average. That it is the most probable variation, having the assumed period of eleven years (with the exception of such as might be computed from a larger number of periodic terms), is assured by the method of its computation, which is based on that of least squares; and one may assume that this relative probability for a single year is represented by the inverse ratio of the probable errors of the two means above determined, viz.—

$$\frac{0{\cdot}94}{0{\cdot}70}.$$

This ratio of probability increases in geometrical progression, as the number of years during which it is found to hold good increases in arithmetical progression[1]; and, for twenty-two years, becomes—

$$\left(\frac{0{\cdot}94}{0{\cdot}70}\right)^{22} = 655 : 1.$$

This ratio, although by no means amounting to demonstration of the exact validity of this particular cycle, affords at least a very high probability that the apparent undecennial fluctuation is no chance phenomenon.

Apart from the approximate identity of its period, the oscillation of the rainfall, thus disclosed, is very different in character from that of the sunspot curve. The periodical minima of both rainfall cycles preceded those of the corresponding sunspot cycles by two years; the actual year of minimum rainfall coincided with that of sunspot minimum in the first cycle, and preceded it by two years in the second. The periodical maximum of the first cycle followed the sunspot maximum by two years, that of the second cycle coincided with the corresponding phase of sunspots, which, in this case, was retarded by two years. The actual rainfall maximum occurred two years later than the sunspot maximum in the first cycle, and one year later in the second.

Hence, as far as the evidence of two cycles goes, the minimum of the rainfall tends to precede the minimum of the sunspots, the maximum of the former to follow that of the latter; and it is noteworthy, as I shall afterwards show, that the droughts which, during the last century, have visited with more or less intensity certain portions of the Indian peninsula, have, on an average, preceded years of sunspot minimum by about one year.

In the other provinces of tropical India, an eleven-year cycle is hardly, if at all, to be detected; a conclusion fully in accord with that which I drew, in 1877, from an examination of the rainfall registers of Bangalore, Mysore, Bombay, Nagpur, &c. The more pronounced phases of the Carnatic cycle are indeed reproduced as a rule, more or less distinctly, as seasons of high or low rainfall respectively, in most parts of the peninsula; but some of the intermediate years are characterized by vicissitudes as great, and even greater than these, destroying the appearance of anything like regular fluctuation.

The Carnatic minimum of 1867, which was the culmination of five years' (1864–68) deficient rainfall, was represented also in Mysore and Bellary, in Malabar and the Deccan; but, in the last two of these provinces, 1866 had a still lower rainfall: and in Berar and Khandesh, while the deficiency of 1866 was (relatively to the average) greater than in any of the more southern provinces, that of 1867 was above the average. In the Konkan again, there was no very great deficiency before 1871, and this was shared more or less by the whole of the peninsula, excepting only the Carnatic and Malabar, which had an excess of 16 and 13 per cent. respectively.

The Carnatic maximum of 1872 was reproduced in Orissa and the Northern Circars—that is to say, in all the eastern provinces of the peninsula—and also in Berar and Khandesh; but in other parts of the peninsula the rainfall of this year differed but little from the average. 1874, however, was a year of excessive rainfall in all the western and southern provinces of the peninsula.

The great drought of 1876 (the second Carnatic minimum) extended with even greater intensity to Mysore, Bellary, Hyderabad, and the Deccan districts of Bombay, and affected more or less the whole of the peninsula, and, in addition, a great part of extra-tropical India. But in the Konkan and Malabar the deficiency was only 18 per cent. of an average fall. In the Konkan the deficiency of the following year was much greater; and in the northern provinces of Bombay, as well as in the greater part of North-Western India, the summer rainfall of 1877 failed almost completely; whereas in the Carnatic the rainfall of that year was remarkably copious.

[1] The probability of throwing any given series of numbers of a single die, in any prescribed order, repeatedly for n throws, is obviously the same as that of throwing a single given number n times in succession, viz. $\left(\frac{1}{6}\right)^n$; and the probability of throwing, in like manner, one out of a given series of dyads or triads, the dyads or triads varying in any prescribed order is $\left(\frac{2}{6}\right)^n$ or $\left(\frac{3}{6}\right)^n$. The relative probability of the dyad to the triad series is $\left(\frac{2}{3}\right)^n$; and generally the relative probability of a phenomenon, the law of variation of which is unknown, varying n times in succession, between limits $\pm\, p$ and $\pm\,(p + 2)$ respectively, is—

$$\left(\frac{p}{p+x}\right)^n.$$

Similar reasoning holds good when p and $p + x$ are the measures of the *mean* variation; and also when, as in the case before us, they represent the probable errors of alternative averages. Finally, the relative improbability of the more limited range, as a chance result—in other words, the probability of the limitation being the result of a regulating cause—is expressed by the inverse ratio.

The following year, 1878, was one of remarkably copious rainfall in nearly all parts of the peninsula, with the exception of the Carnatic, where the rainfall did not exceed the average. In Hyderabad it was greater than that of any other year since regular registers have been kept; and, on the general average of the peninsula (excluding the Carnatic), it is approached only by that of 1874 and 1882.

Finally, the Carnatic maximum of 1884 coincided with a small excess in Hyderabad and with a larger excess in the north-west of the peninsula (the Central Provinces, Berar, Khandesh, the Konkan, and Guzerat); but this was due to independent conditions. In Mysore, Bellary, Malabar, the Deccan, the Northern Circars, and Orissa, the rainfall of the year was more or less deficient, especially in Mysore, where the fall was only three-fourths of the average.

It may, then, be considered as demonstrated that the apparently periodical variation of the Carnatic rainfall is by no means representative of a similar variation in that of Southern India generally; and I might here conclude the discussion, were it not that the independent evidence of a certain apparent regularity in the recurrence of droughts and dearths seems to require a few words of notice.

At page 21 of the Report of the Indian Famine Commissioners is given a list of all the serious droughts, and consequent seasons of dearth, that have affected India during the last century. Selecting those that have chiefly affected some part of the peninsula, we have the following:—

Droughts.	Intervals.
1782	
1791	9 years.
1802	11 ,,
1806	4 ,,
1812	6 ,,
1823	11 ,,
1832	9 ,,
1844	12 ,,
1853	9 ,,
1865	11 ,,
1876	11 ,,

Omitting that of 1806, which divided the ordinary interval into two, the mean interval is 10·36 years, and the deviation from this mean in no case amounts to two years. According to Wolf's table, the years of minimum sunspots and their intervals were:—

Sunspot Minima.	Intervals.
1784	
1798	14 years.
1810	12 ,,
1823	13 ,,
1833	10 ,,
1843	10 ,,
1856	13 ,,
1867	11 ,,
1878	11 ,,

the mean interval being 11·18 years. The coincidence of these mean intervals is hardly so close as might be anticipated were there any real physical interdependence between recurrent phases of the sun's condition, and the recurrence of the droughts. And a comparison of the dates in detail brings to light further discrepancies. Thus the years of drought vary in their relations to the nearest years of minimum sunspots as follows:—

One, midway between two sunspots minima; seven years distant from each;
One, four years earlier;
One, three years earlier;
Three, two years earlier;
One, one year earlier;
One, coincident;
One, one year later;
One, two years later;
One, four years later.

Omitting the first (that of 1791), which occurred four years after a year of maximum sunspots, and midway between two minima, in an unusually prolonged cycle, the years of drought, on a general average, anticipated the sunspot minima by somewhat less than a year, instead of following the minima, as might have been expected on the hypothesis of the former standing to the quiescent condition of the sun in the relation of effect to cause.

I should not, however, hastily conclude from these facts that there is no relation between the recurrence of drought in Southern India, and the periodical variation of the solar photosphere; but merely that the interdependence of the two classes of phenomena, if real, is far from being simple and direct, and also that other and, as far as we know, non-periodic causes, concur largely in producing drought. If we accept the conclusions, drawn in the first part of this note, as to the highly probable periodicity of the Carnatic rainfall, one must admit that there is, in that province, a recurrent tendency to drought at eleven-year intervals, though it does not always culminate in drought of disastrous intensity; and this epoch anticipates by about two years that of the sunspot minimum. This tendency is evidently much weaker in other parts of the peninsula; and in Northern India there is some indication of a tendency to the recurrence of drought about the time of maximum sunspots, as in 1803, 1837, 1838, and 1860—all years of disastrous drought in Northern India; and the experience of late years has demonstrated that these droughts generally extend to the northern provinces of the peninsula.

HENRY F. BLANFORD.

NOTES.

WE print elsewhere a report of the speeches delivered by Mr. Goschen and by some members of the influential deputation who waited upon him last Thursday to press the claims of University Colleges. The deputation had certainly no reason to complain of the manner in which they were received. Mr. Goschen, speaking as Chancellor of the Exchequer, was of course obliged to adopt a cautious tone; but it was plain enough that those who addressed him represented a cause with which he had strong personal sympathy. His promise that the Government would give the matter "its most serious attention," means, we may hope, that the principle of State aid for University Colleges has been practically accepted.

ON Monday the foundation-stone of the Imperial Institute was laid by the Queen. No representative of science, as such, was invited to be present at the ceremony, and NATURE did not receive a Press ticket. Evidently science is to have little to do with the New Institute.

THE Prussian Society for the Promotion of Industry has recently offered a prize of about £150 for the most exhaustive critical comparison of all kinds of existing bronze, tombac, and brass alloys, used or recommended for machinery, giving their chief properties with regard to resistance, ductility, friction at different temperatures, malleability, electrical conductivity, behaviour with acids, hydrogen and carbon sulphides, chlorine, and other strongly corrosive substances met with in practice. The same Society also offers a gold medal and £250 for the best work on light and heat radiation of burning gases. The time limit in the former case is the end of 1887; in the latter, the end of 1888.

The National Association for the Promotion of Technical Education has now been formed. A meeting of persons interested in the movement was held on the 1st inst. at the rooms of the Society of Arts, Adelphi. Lord Hartington presided, and among those present were Lord Rosebery, Mr. John Morley, Sir Lyon Playfair, Sir John Lubbock, and representatives from Colleges, technical schools, trade-unions, School Boards, national Societies, and Chambers of Commerce.

About 40 members of Parliament were also present. Lord Hartington, in opening the proceedings, said their object was not so much to stimulate public interest in this great question as to consider from a practical point of view the channels into which such interest ought to be directed. He had been struck by the facts relating to technical education at home and abroad which had been presented in very voluminous form to the public in the reports of our Consuls. We had in this country attained to a great industrial and technical supremacy in the world. We had attained this position partly by the possession of great resources in coal and iron and other industrial materials, partly from the character, energy, and industry of our people, and partly—and here he might be trenching upon controversial grounds—from the fact of our having adopted a sound commercial policy. At the same time, concurrently with our attainment of this supremacy, wonderful scientific discoveries had been made, and more and more science was being applied to the industrial occupations of the world. Other nations had been quick to perceive this, and were striving to make their position equal to ours by developing at immense cost to the State and public funds that scientific instruction which would enable their manufactories and workmen to compete successfully with ours. If we were passive in the matter—if we were indolent—it was conceivable not only that foreign nations would rival us, but they might also succeed in passing us, with consequences which it would be difficult to contemplate. If we were satisfied to go on as we were, if we were content to rely in the future as we had done in the past on those advantages which had given us our present position, and if we did not think it necessary to organize more completely our system of technical instruction than at present, that decision should be the result of deliberate and well-formed consideration and not the result of apathy or indolence. Sir Lyon Playfair, moved that the Association be formed, that Lord Hartington be invited to become President, and the following gentlemen Vice-Presidents:—Lord Granville, Lord Ripon, Lord Rosebery, Lord Spencer, the Bishop of London, Mr. Broadhurst, Prof. Huxley, Sir John Lubbock, Mr. Mundella, Sir Lyon Playfair, Sir B. Samuelson, Prof. Stuart, Dr. Sullivan, Sir R. Temple, and Prof. Tyndall. Mr. John Morley, in seconding the motion, said the time for further inquiry had gone past, and the time had arrived when they could no longer with wisdom, or even with safety, delay the movement they that day commenced. The resolution was carried unanimously. Sir J. Lubbock moved the appointment of an executive Committee, which was carried; as was a motion, made by Mr. Mundella and seconded by Lord Rosebery, that those present be invited to join the Council. A discussion ensued on the proposed objects of the Association, after which Sir B. Samuelson moved, and Mr. Howell seconded, a resolution inviting the assistance of large towns and the chief industrial centres. The motion was duly carried, and votes of thanks closed the proceedings.

IN his statement on Monday about the progress of business in the House of Commons, Mr. W. H. Smith said: "There is a measure for promoting technical education, which we have every reason to believe will be accepted unanimously by the House—at all events, we hope that a very slight discussion will be sufficient to pass that measure into law."

ON July 25, 1837, the first practical essay in telegraph working was made by Messrs. Cooke and Wheatstone between Euston and Camden Town. In the material order of things few more magnificent triumphs have ever been achieved, and it has very properly been decided that the fiftieth anniversary of the occasion shall be celebrated. Some time ago an influential Committee was formed to take the matter into consideration, and the other day there was a well-attended meeting of the members at the offices of the Society of Telegraph-Engineers and Electricians. Mr. Preece, F.R.S., was appointed chairman. In his opening speech he said they had met to make arrangements for a dinner which was to be given in celebration of the jubilee of the telegraph. It was the success of the essay made by Messrs. Cooke and Wheatstone that led to the association of Robert Stephenson, George Parker Bidder, Brunel, and other well-known men in those days, with the telegraph, and from that little beginning they had seen how the telegraphs had spread all over the face of the earth. In England, where the first step was taken, they had succeeded in keeping well in the van, and it was only fitting that such an important event, probably the greatest event that had happened during the long reign of Her Majesty, should be celebrated, and that those who had been instrumental in bringing telegraphy to its present great position should meet together and talk over old times. It so happened that there were several reasons why the celebration should not take place on July 25, which was really the proper day. In the first place, Mondays were busy days with legislators in their House over the way, and it would be extremely difficult to get many of those whom they hoped to attract if the proposed dinner took place on a Monday; again, it was quite impossible on a Government night, like Monday, to get the Postmaster-General, who, it was thought, should take the chair, to attend; and, further, on July 23 there was to be a great naval review, and a great many who would wish to attend the dinner would not be able to get back until late on Monday afternoon. For those reasons it would be difficult to hold the dinner on the 25th, and Wednesday, the 27th, had been suggested as meeting everybody's convenience. He therefore moved that a dinner be held on July 27 to celebrate the jubilee of the telegraph. Mr. Willoughby Smith seconded the motion, which was unanimously agreed to. Discussion followed respecting matters of detail, and an Executive Committee was elected, consisting of Messrs. W. H. Preece, E. Graves, C. H. B. Patey, C. E. Spagnoletti, A. Siemens, and A. Stroh, with Messrs. H. Alabaster and C. H. W. Biggs as Honorary Secretaries, and Mr. F. H. Webb, Secretary of the Society of Telegraph-Engineers and Electricians, as Acting Secretary. The guarantee fund was at once opened, and names were soon down for upwards of £100. The meeting was adjourned until Tuesday, the 12th inst., when the Executive Committee will report as to the progress of the arrangements.

A NUMBER of vacation courses in Natural Science will shortly be started in Edinburgh. During the present summer there will be two courses—one on Practical Botany, conducted at the Royal Botanic Gardens of Edinburgh by Mr. G. F. Scott Elliot, an assistant to the Professor of Botany; another on Practical Zoology, conducted at the Scottish Marine Station, Granton, Edinburgh, by Mr. J. Arthur Thomson, Lecturer on Zoology in the School of Medicine, with the co-operation of Mr. J. T. Cunningham, the Superintendent of the Station. These courses ought to be of great service to teachers and others occupied during the University terms, for whom they are primarily intended.

WRITING to us from Tashkend on June 12, M. Wilkins says that the city of Vernoje was completely ruined by the earthquake of June 9. More than 800 bodies had been excavated. "The disaster," he says, "is beyond description. Mud and water are said to flow abundantly from the disturbed mountains to the scene of the catastrophe, and many crevasses are noticed in the ground. The exact time of the tremendous shock is given as 4h. 35m. local time. At 4h. 18m. (Tashkend local time) on the same morning, we felt here a flat wave which set in motion suspended objects. Taking into account the difference of longitude between Tashkend and Vernoje and the consequent difference of time, it appears that the wave travelled in a straight line more than 400 miles in the short time of 13·5 minutes, crossing on the way, in a diagonal direction, the whole western half of the Thian Shan range."

ON June 30 a Philippine Exhibition was opened at Madrid by Queen Christina. The most important exhibits are specimens of the natural products, vegetable and mineral, of the Philippine Islands. Some forty natives, male and female, with their native houses and arms, are present. The late King of Spain started the idea of a permanent Colonial Museum, to contain the mercantile products of the Spanish colonies. The opening of this Exhibition is regarded as the first step towards the realization of his scheme.

ANOTHER instalment of his valuable work on high temperature dissociations has just been given forth from the laboratory of Prof. Victor Meyer, at Göttingen. The molecular condition of phosphorus, arsenic, and antimony at the highest accessible temperatures has been the subject of this recent work, and the following are the experimental results obtained. As is well known, the experiments of Deville and Troost brought to light the fact that as high as 1040° in the case of phosphorus, and 860° in the case of arsenic, the observed densities are such as can only be explained on the supposition that the molecules of these elements consist of four atoms. J. Mensching and Victor Meyer now show that as the temperature is gradually raised to a red heat the molecular weights begin to diminish—that is, the four-atom molecules commence to break down—and at a white heat so large a number are dissociated that the values obtained for the vapour-densities approximate to those required on the supposition that the molecules each contain but two atoms. Hence, at a white heat the vapour-densities of phosphorus and arsenic are normal, and the molecules consist of the usual two atoms. In the case of antimony, no thoroughly trustworthy work has hitherto been published as to its molecular state, but it has been generally supposed to consist also of four-atom molecules. Mensching and Meyer, however, find that it behaves quite unlike phosphorus and arsenic, inasmuch as immediately on volatilization its density is found to correspond to a molecule of but three atoms, and although dissociation continues to the limit of terrestrially procurable temperatures, yet when this is attained, the level of the normal state is not reached, and more definite results must perforce be deferred until, by some ingenious device, temperatures far higher are obtainable.

THE Royal Meteorological Institute of the Netherlands at Utrecht has recently published its *Jaarboek* for 1886, containing observations taken three times daily at ten places, and daily rainfall values at eighty stations. These volumes, which have now been regularly issued for thirty-eight years, form one of the most complete series of meteorological observations in Europe, and they also contain valuable discussions on the climatology of distant parts. The volume now in question contains observations taken at San Salvador (on the Congo) for 1885, and at Djedda (Arabia Felix), Paramaribo (Dutch Guiana), and Culebra (Panama Canal) for 1886. The Director of this Institution (Dr. Buys Ballot) first enunciated the law that now bears his name, showing the universal relation of the direction of the wind to barometric pressure, which has been so instrumental in popularizing weather knowledge. This Office also deals largely with maritime meteorology, and has published a long series of papers on this subject entitled "Uitkomsten van wetenschap en ervaring," as well as wind charts for the various oceans.

AN unusual number of foreign men of science will be present at the Manchester meeting of the British Association for the Advancement of Science. The following is the first list of foreigners who have accepted invitations to attend the meeting:—*Section A* (*Physics and Mathematics*): Cleveland Abbe, Meteorological Office, Washington; Von Hefner Altneck, Berlin; A. Cornu, Ecole Polytechnique, Paris; A. Crova, Montpellier; J. R. Eastman, U.S. Naval Observatory; W. Foerster, Director of the Berlin Observatory; W. de Fonvielle, Paris; A. Horstman, Heidelberg; F. Kohlrausch, Professor of Physics, Würzburg; A. Kundt, Professor of Physics, Strassburg; William Libbey, Princeton College, N.J.; G. Lippmann, Paris; R. Lipschitz, Professor of Mathematics, Bonn; Malcolm McNeill, Princeton College, N.J.; O. E. Meyer, Breslau; G. Quincke, Professor of Physics, Heidelberg; Schering, Director of the Observatory, Göttingen; Ernst Schroeder, Karlsruhe; J. Violle, Ecole Normale, Paris; E. Warburg, Professor of Physics, Freiburg; H. Wild, St. Petersburg; A. C. Young, Princeton College, N.J. *Section B* (*Chemistry*): A. Bernthsen, Heidelberg; J. W. Brühl, Freiburg; Caro, Mannheim; Le Chatelier, Paris; F. W. Clarke, Washington; De Clermont, Paris; F. B. Fittica, Marburg; R. Fittig, Strassburg; Hempel, Dresden; Reinhardt Hoffman, Biebrich; A. Ladenburg, Kiel; J. W. Langley, University of Michigan; A. Lieben, Vienna; C. Lieberman, Berlin; Oscar Liebreich, Berlin; Lunge, Zurich; J. W. Mallet, University of Virginia; C. A. Martius, Berlin; Mendelejeff, St. Petersburg; Menschutkin, St. Petersburg; Lothar Meyer, Tübingen; Noelting, Muhlhausen; Pauli, Höckst; Silva, Paris; G. Wiedemann, Leipzig; Otto Witt, Berlin; J. Wislicenus, Leipzig. *Section C* (*Geology*): E. Cohen, Greifswald; H. von Dechen, Bonn; Anton Fritsch, Prague; Alfred Nehring, Berlin; Abbé Renard, Bruxelles; F. Zirkel, Leipzig. *Section D* (*Biology*): A. de Bary, Strassburg; Von Boddaert, Cutsem; C. W. Braune, Leipzig; A. Chauveau, Paris; F. Cohn, Breslau; C. Dervalque, Liége; C. Gegenbauer, Heidelberg; Asa Gray, Harvard College, Cambridge, U.S.; W. His, Leipzig; A. Hubrecht, Utrecht; Ch. Julin, Liége; F. Kühne, Heidelberg; Count von Solms Laubach, Göttingen; Lortet, Lyon; Marey, Paris; C. S. Minot, Harvard College; G. S. Morse, Salem, Mass.; P. Preyer, Jena; Pringsheim, Berlin; J. von Sachs, Würzburg; De Saporta, Aix; A. Weismann, Freiburg; R. Wiedersheim, Freiburg. *Section E* (*Geography*): Comodore Jansen, The Hague; M. Lindemann, Bremen; M. Venukoff, Paris. *Section F* (*Economic Science*): Carl Greven, Leyden; Dana Horton; Judge Mackay; A. de Marcoartu, Madrid; Carl Menger, Vienna; *Section G* (*Engineering*): Thos. Egleston, Washington; J. B. Francis, Past President of the American Society of Civil Engineers; A. Gobert, Bruxelles; Quinette de Rochemont, Havre; R. H. Thurston, Sibley College, Cornell University. *Section H* (*Anthropology*): Dr. O. Finsch; Marquis de Nadaillac, Paris.

MESSRS. MARCUS WARD AND CO. will publish, early this autumn, a work, in two volumes, on the Canary Islands. The writer, Mrs. Olivia M. Stone, author of "Norway in June," visited with her husband all the islands of the group—a feat which had never before been accomplished by English people. Illustrations from photographs taken during the tour, and eight maps made from the author's personal observations, will accompany the letterpress.

IN a letter printed by us last week, describing a meteor which was seen in West Sussex by daylight, the meteor is said to have "disappeared near the *meridian* of Antares." For "meridian" read "position."

THE additions to the Zoological Society's Gardens during the past week include three Blotched Genets (*Genetta tigrina*) from South Africa, presented by Gen. J. J. Bisset; an Ocelot (*Felis pardalis*) from South America, presented by the Earl of Dudley; a Barn Owl (*Strix flammea*), British, presented by Mr. —— Wickham; a White-tailed Sea Eagle (*Haliaetus albicilla*), European, presented by Mr. G. J. Mayer; a Ceylonese Jungle Fowl (*Gallus stanleyi*) from Ceylon, presented by Mr. Hugh Neville; six Corn-Crakes (*Crex pratensis*), British, presented by Mr. G. J. B. Willows; a Magpie (*Pica rustica*) from France, presented by Mr. Walter H. Ince; a Yellow-fronted Amazon

(*Chrysotis ochrocephala*) from Guiana, deposited; six Chinchillas (*Chinchilla lanigera*) from Chili, a Burrowing Owl (*Speotyto cunicularia*) from Buenos Ayres, two Hoopoes (*Upupa epops*), British, a Gould's Monitor (*Varanus gouldi*) from Australia, purchased; two Mule Deer (*Cariacus macrotis*), a Yellow-footed Rock Kangaroo (*Petrogale xanthopus*) born in the Gardens; two Blood-breasted Pigeons (*Phlogœnas cruentata*) bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

RELATIVE POSITIONS OF THE PRINCIPAL STARS IN THE PLEIADES.—We have received vol. i., part 1, of the Transactions of the Astronomical Observatory of Yale University, containing an important paper by Dr. W. L. Elkin, giving the results of his researches with the Yale heliometer on the relative positions of sixty-nine stars situated in the above-mentioned group. The work consists, in reality, of two independent triangulations: one resting on measurements of the distance of each star in the group from each of four stars situated near its outer limits, so that nearly the entire group is inclosed symmetrically by the quadrilateral formed by them; the other resting on measurements of distance and position-angle from Alcyone, the central star of the group. These two independent determinations are in very satisfactory agreement, and Dr. Elkin has thus furnished a most accurate catalogue, for the epoch 1885, of the relative positions in R.A. and declination of these sixty-nine stars. For comparison of his results with the Königsberg places for 1840, Dr. Elkin has adopted the corrections to the latter resulting from Prof. Auwers's researches, and brought up the newly reduced places to 1885, exhibiting the comparison in the form of apparent displacements in R.A. and in declination, the place of Alcyone being made identical in both series. For the six largest cases of relative displacement there is a remarkable community both of direction and amount of apparent motion, and it is remarkable that this general drift is very similar to the reversed absolute motion of Alcyone as given by Newcomb. For two of the stars, Bessel's Nos. 14 and 35, the coincidence is, in Dr. Elkin's opinion, sufficiently close to warrant the deduction that these two stars at least do not belong to, but form only optical members of, the group. It is possible, if not probable, that the other four should also be placed in the same category. The general character of the internal motions of the group appears, however, to be extremely minute, and Dr. Elkin thinks that the hopes of obtaining any clue to the internal mechanism of this cluster seem not likely to be realized in the immediate future. Dr. Elkin also compares his results with the micrometrical measures of M. Wolf at Paris and of Prof. Pritchard at Oxford, and arrives at the conclusion that "the use of the filar micrometer for such large distances as those under consideration is likely to be accompanied with considerable casual error, and, unless great care is taken, with large systematic error. The conclusions of Messrs. Wolf and Pritchard as to the relative motions in the group have thus been unfortunately vitiated, and must be replaced by those formulated" in Dr. Elkin's most able paper.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 JULY 10-16.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on July 10

Sun rises, 3h. 57m.; souths, 12h. 5m. 1·7s.; sets, 20h. 13m.; decl. on meridian, 22° 16′ N.: Sidereal Time at Sunset, 15h. 27m.

Moon (at Last Quarter on July 13) rises, 22h. 32m.*; souths, 3h. 51m.; sets, 9h. 19m.; decl. on meridian, 8° 32′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury	6 14	13 40	21 6	15 33 N.
Venus	8 10	15 10	22 10	10 57 N.
Mars	2 21	10 42	19 3	23 59 N.
Jupiter	13 8	18 26	23 44	9 4 S.
Saturn	4 34	12 35	20 36	21 16 N.

* Indicates that the rising is that of the preceding evening.

July.	h.	
12	2	Mercury at greatest distance from the Sun.
13	20	Venus at greatest elongation from the Sun, 46° east.
14	14	Mercury stationary.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.
U Cephei	0 52·3	81 16 N.	July 12,	22 52 *m*
o Ceti	2 13·6	3 29 S.	,, 11,	*m*
Algol	3 0·8	40 31 N.	,, 12,	21 12 *m*
S Leonis	11 5·0	6 4 N.	,, 13,	*M*
W Virginis	13 20·2	2 48 S.	,, 11,	3 0 *M*
δ Libræ	14 54·9	8 4 S.	,, 15,	23 42 *m*
β Lyræ	18 45·9	33 14 N.	,, 12,	22 0 *M*
R Lyræ	18 51·9	43 48 N.	,, 16,	*M*
δ Cephei	22 25·0	57 50 N.	,, 15,	1 0 *M*
R Pegasi	23 1·0	9 56 N.	,, 13,	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A. °	Decl. °	
From Herculis	271	21 N.	Very slow.
Ophiuchus	280	14 S.	Very slow.
Near π Pegasi	329	36 N.	Swift. Red streaks.
From Andromeda	352	38 N.	Swift.

REPORT OF THE COMMITTEE OF INQUIRY INTO M. PASTEUR'S TREATMENT OF HYDROPHOBIA.

THE following is the text of this important Report to the President of the Local Government Board:—

SIR,—In accordance with the instructions contained in a letter dated April 12, 1886, from your predecessor, the Right Honourable Joseph Chamberlain, M.P., appointing us to be a Committee to inquire into M. Pasteur's treatment of hydrophobia, we beg leave to present to you the following Report.

In order to answer the several questions involved in the inquiry, we found it necessary that some of the members of the Committee should, together with Mr. Victor Horsley, the Secretary, visit Paris, so as to obtain information from M. Pasteur himself, and observe his method of treatment, and investigate a considerable number of the cases of persons inoculated by him; and, further, that a careful series of experiments should be made by Mr. Horsley on the effects of such inoculation on the lower animals. The detailed facts of these observations and experiments are placed in the Appendix to this Report; a summary of them, and the conclusions which we believe may be drawn from them, are given in the next following pages.

The experiments by Mr. Horsley entirely confirm M. Pasteur's discovery of a method by which animals may be protected from the infection of rabies. The general facts proved by them may be thus stated:

If a dog, or rabbit, or other animal be bitten by a rabid dog and die of rabies, a substance can be obtained from its spinal cord which, being inoculated into a healthy dog or other animal, will produce rabies similar to that which would have followed directly from the bite of a rabid animal, or differing only in that the period of incubation between the inoculation and the appearance of the characteristic symptoms of rabies may be altered.

The rabies thus transmitted by inoculation may, by similar inoculations, be transmitted through a succession of rabbits with marked increase of intensity.

But the virus in the spinal cords of rabbits that have thus died of inoculated rabies may be gradually so weakened or attenuated, by drying the cords, in the manner devised by M. Pasteur and related in the Appendix that, after a certain number of days' drying, it may be injected into healthy rabbits or other animals without any danger of producing rabies.

And by using, on each successive day, the virus from a spinal cord dried during a period shorter than that used on the previous day, an animal may be made almost certainly secure against rabies, whether from the bite of a rabid dog or other animal, or from any method of subcutaneous inoculation.

The protection from rabies thus secured is proved by the fact that, if some animals thus protected and others not thus protected be bitten by the same rabid dog, none of the first set will die of rabies, and, with rare exceptions, all of the second set will so die.

It may, hence, be deemed certain that M. Pasteur has discovered a method of protection from rabies comparable with that which vaccination affords against infection from small-pox. It would be difficult to over-estimate the importance of the discovery, whether for its practical utility or for its application in general pathology. It shows a new method of inoculation, or, as M. Pasteur sometimes calls it, of vaccination, the like of which it may become possible to employ for protection of both men and domestic animals against others of the most intense kinds of virus.

The duration of the immunity from rabies which is conferred by inoculation is not yet determined; but during the two years that have passed since it was first proved there have been no indications of its being limited.

The evidence that an animal may thus, by progressive inoculations, be protected from rabies suggested to M. Pasteur that if any animal or any person, though unprotected, were bitten by a rabid dog, the fatal influence of the virus might be prevented [1] by a timely series of similar progressive inoculations. He has accordingly, in the institution established by him in Paris, thus inoculated a very large number of persons believed to have been bitten by rabid animals; and we have endeavoured to ascertain with what amount of success he has done so.

The question might be answered with numerical accuracy if it were possible to ascertain the relative numbers of cases of hydrophobia occurring among persons of whom, after being similarly bitten by really rabid animals, some were and some were not inoculated. But an accurate numerical estimate of this kind is not possible. For

(1) It is often difficult, and sometimes impossible, to ascertain whether the animals by which people were bitten, and which were believed to be rabid, were really so. They may have escaped, or may have been killed at once, or may have been observed by none but persons quite incompetent to judge of their condition.

(2) The probability of hydrophobia occurring in persons bitten by dogs that were certainly rabid depends very much on the number and character of the bites; whether they are on the face or hands or other naked parts; or, if they have been inflicted on parts covered with clothes, their effects may depend on the texture of the clothes, and the extent to which they are torn; and, in all cases, the amount of bleeding from the wounds may affect the probability of absorption of virus.

(3) In all cases, the probability of infection from bites may be affected by speedy cauterizing or excision of the wounded parts, or by various washings or other methods of treatment.

(4) The bites of different species of animals, and even of different dogs, are, probably, for various reasons, unequally dangerous. Last year, at Deptford, five children were bitten by one dog and all died; in other cases, a dog is said to have bitten twenty persons, of whom only one died. And it is certain that the bites of rabid wolves, and probable that those of rabid cats, are far more dangerous than those of rabid dogs.

The amount of uncertainty due to these and other causes may be expressed by the fact that the percentage of deaths among persons who have been bitten by dogs believed to have been rabid, and who have not been inoculated or otherwise treated, has been, in some groups of cases, estimated at the rate of only 5 per cent., in others at 60 per cent., and in others at various intermediate rates. The mortality from the bites of rabid wolves, also, has been, in different instances, estimated at from 30 to 95 per cent.

To ascertain, as far as possible, the influence of these sources of fallacy in cases inoculated by M. Pasteur, the members of the Committee who went to Paris requested him to enable them to investigate, by personal inquiry, the cases of some of those who had been treated by him. He at once, and very courteously, assented, and the names of 90 persons were taken from his notebooks. No selection was made, except that the names were taken from his earliest cases, in which the periods since inoculation were longest, and from those of persons living within reach in Paris, Lyons, and St. Etienne.

[1] The terms referring to "preventive" treatment will be used for that designed to prevent the occurrence of the disease in one already infected; those referring to "protective" treatment for that designed to protect a man or an animal from the risk of becoming infected. And it may be well to state that, though the usual custom is followed of employing the name of "hydrophobia" for the disease in men, and of "rabies" for that in animals, they are really the same disease.

The notes made on the spot concerning all these cases are given in the Appendix, and they include, as far as was possible, the evidence whether the dogs deemed rabid were really so, the situation and kind of bites, the immediate treatment of them, the statements of medical practitioners and veterinary surgeons to whom any useful facts were known.[1]

Among the 90 cases there were 24 in which the patients were bitten on naked parts by undoubtedly rabid dogs, and the wounds were not cauterized or treated in any way likely to have prevented the action of the virus; there were 31 in which there was no clear evidence that the dog was rabid; others in which the bite, though inflicted by undoubtedly rabid animals, having been through clothes, may thus have been rendered harmless. Among these, therefore, it is probable that, even if they had not been inoculated, few would have died. Still, the results observed in the total of the 90 cases may justly be compared with those observed in large numbers of cases similar to these as regards the uncertainties of infection, but not inoculated. The estimates published as to the mortalities in such unassorted cases are, as we have said, widely various. We believe that among the 90 persons, including the 24 bitten on naked parts, not less than eight would have died if they had not been inoculated. At the time of the inquiry, in April and May 1886, which was at least eighteen weeks since the treatment of the bites, not one had shown any signs of hydrophobia, nor has any one of them since died of that disease.

Thus, the personal investigation of M. Pasteur's cases by members of the Committee was, so far as it went, entirely satisfactory, and convinced them of the perfect accuracy of his records.

After the first few months in which M. Pasteur practised his treatment, he was occasionally obliged, in order to quiet fears, to inoculate persons who believed that they had been bitten by rabid animals, but could give no satisfactory evidence of it. It might, therefore, be deemed unjust to estimate the total value of his treatment in the whole of his cases as being more than is represented by the difference between the rate of mortality observed in them and the lowest rate observed in any large number of cases not inoculated. This lowest rate may be taken at 5 per cent. Between October 1885 and the end of December 1886, M. Pasteur inoculated 2682 persons, including 127 who went from this country. Of the whole number, at the rate of 5 per cent., at least 130 should have died. At the end of 1886, the number of deaths stated by M. Vulpian, speaking for M. Pasteur, was 31, including 7 bitten by wolves, in three of whom the symptoms of hydrophobia appeared while they were under treatment, and before the series of inoculations were complete. Since 1886 two more of those inoculated in that year have died of hydrophobia.

The number of deaths assigned by those who have sought to prove the inutility of M. Pasteur's treatment is, as nearly as we can ascertain, 40 out of the 2682; and in this number are included the seven deaths from bites by wolves, and probably not less than four in which it is doubtful whether the deaths were due to hydrophobia or to some other disease. Making fair allowance for uncertainties and for questions which cannot now be settled, we believe it sure that, excluding the deaths after bites by rabid wolves, the proportion of deaths in the 2634 persons bitten by other animals was between 1 and 1·2 per cent., a proportion far lower than the lowest estimated among those not submitted to M. Pasteur's treatment, and showing, even on this lowest estimate, the saving of not less than 100 lives.

The evidence of the utility of M. Pasteur's method, indicated by these numbers, is confirmed by the results obtained in certain groups of his cases.

Of 233 persons bitten by animals in which rabies was proved, either by inoculation from their spinal cords, or by the occurrence of rabies in other animals or in persons bitten by them, only 4 died. Without inoculation it would have been expected that at least 40 would have died.

Among 186 bitten on the head or face by animals in which rabies was proved by experimental inoculations, or was observed by veterinary surgeons, only 9 died, instead of at least 40.

And of 48 bitten by rabid wolves only 9 died; while, without the preventive treatment, the mortality, according to the most probable estimates yet made, would have been nearly 30.

[1] The Committee are much indebted to M. Arloing, Director of the Veterinary School at Lyons; M. Savary, Veterinary Surgeon at Brie-Comte-Robert; and M. Charlois, Veterinary Surgeon at St. Etienne, for assistance in their inquiries.

Between the end of last December and the end of March, M. Pasteur inoculated 509 persons bitten by animals proved to have been rabid, either by inoculation with their spinal cords, or by the deaths of some of those bitten by them, or as certified by veterinary surgeons. Only 2 have died, and one of these was bitten by a wolf a month before inoculation, and died after only three days' treatment. If we omit half of the cases as being too recent, the other 250 have had a mortality of less than 1 per cent., instead of 20 or 30 per cent.

It has been objected that the number treated by M. Pasteur, which, from October 1885 to the end of 1886, included 1929 French and Algerians, was much greater than could reasonably be supposed to have been bitten by rabid animals. But there had hitherto been no careful registration of such cases, and the numbers that have occurred in the present year are not less than in the same part of last year, when the alarm about hydrophobia was greatest.

From the evidence of all these facts, we think it certain that the inoculations practised by M. Pasteur on persons bitten by rabid animals have prevented the occurrence of hydrophobia in a large proportion of those who, if they had not been so inoculated, would have died of that disease. And we believe that the value of his discovery will be found much greater than can be estimated by its present utility, for it shows that it may become possible to avert by inoculation, even after infection, other diseases besides hydrophobia. Some have, indeed, thought it possible to avert small-pox by vaccinating those very recently exposed to its infection; but the evidence of this is, at the best, inconclusive; and M. Pasteur's may justly be deemed the first proved method of overtaking and suppressing by inoculation a process of specific infection. His researches have also added very largely to the knowledge of the pathology of hydrophobia, and have supplied what is of the highest practical value, namely, a sure means of determining whether an animal, which has died under suspicion of rabies, was really affected with that disease or not.

The question has been raised whether M. Pasteur's treatment can be submitted to without danger to health or life; and, in answering it, it is necessary to refer to two different methods of inoculation which he has practised, and which are fully described in the Appendix.

In the first, which may be called the ordinary method, and which has been employed in the very large majority of cases, the preventive material obtained from the spinal cords of rabbits that have died of rabies derived, originally, from rabid dogs is injected under the skin, once a day for ten days, in gradually increasing strengths.

In the second or intensive method (*méthode intensive*) which M. Pasteur adopted for the treatment of cases deemed especially urgent, on account either of the number and position of the bites or of the long time since their infliction, the injections, gradually increasing in strength, were usually made three times on each of the first three days, then once daily for a week, and then in different degrees of frequency for some days more. The highest strength of the injections used in this method was greater than the highest used in the ordinary method, and was such as, if used at first and without the previous injections of less strength, would certainly produce rabies.

By the first or ordinary method, there is no evidence or probability that anyone has been in danger of dying, or has in any degree suffered in health even for any short time. But after the intensive method deaths have occurred under conditions which have suggested that they were due to the inoculations rather than to the infection from the rabid animal.

There is ample reason to believe that in many of the most urgent cases the intensive method was more efficacious than the ordinary method would have been. Thus, M. Pasteur mentions that, of 19 Russians bitten by rabid wolves, 3 treated by the ordinary method died, and the remaining 16, treated by the intensive method, survived; and he contrasts the cases of 6 children, severely bitten on the face, who died after the ordinary treatment, with those of 10 similarly bitten children who were treated by the intensive method, and of whom none died; and M. Vulpian reports that, of 186 persons badly bitten by animals that were most probably rabid, 50 treated by the intensive method survived, and of the remaining 136 treated by the ordinary method 9 died.

The rate of mortality after the intensive method was not greater than that after the ordinary method; for among 624 patients thus treated only 6 died, or, counting one doubtful case, 7. But that which excited suspicion was the manner of death in some of them; and this manner was observed in a man named Goffi, sent from England. On September 4 last, he was severely bitten at the Brown Institution by a rabid cat, to which, in spite of repeated warnings, he exposed his naked hand. Twelve wounds were inflicted. They were at once treated with pure carbolic acid, and, six hours later, he was put under the influence of chloroform at St. Thomas's Hospital, the wounded portions of skin were freely excised, and the wounds thus made were treated with carbolic acid. On the same evening he was sent to Paris, and on the following morning M. Pasteur commenced the intensive treatment, and it was continued during twenty-four days. During all this time the man was repeatedly intoxicated.[1] He once fell into the Seine; and while crossing the Channel on his return home he was severely chilled.

On October 10 he returned to his work, and appeared to be in his usual health; but he became unwell, with pain in the abdomen, like colic, and with pain in the back. On the 18th he had partial motor paralysis in the lower limbs, and on the 19th complete motor paralysis of these limbs and of the trunk, and partial motor paralysis of the upper limbs and face. He was taken to St. Thomas's Hospital, where he died on the 20th.

To the last he was free from all the usual symptoms of hydrophobia, and the progress of his disease and the manner of his death were so similar to those of what is described as acute ascending paralysis, or Landry's paralysis, that a verdict to this effect was given at a coroner's inquest. But the certainty that his death was due to the virus of rabies was proved by experiments by Mr. Horsley. A portion of his spinal cord was taken to provide material for inoculations, and rabbits and a dog inoculated with it died with characteristic signs of paralytic rabies, such as usually occurs in rabbits.

In most of the other cases of death after treatment by the intensive method, the symptoms have been nearly the same as those just related; but in none of them has the same test of death from hydrophobia been applied. The likeness of the symptoms to those of the form of rabies called dumb or paralytic, usually observed in rabbits, has suggested, as we have said, that the deaths were due not to the virus of the rabid dog or cat, but to that injected from the spinal cord of the rabbit. But this is far from certain. In the case of Goffi, especially, the incubation period was such as would have followed the bite of the cat, not the inoculation of highest intensity; and the incubation period in the rabbits and dog inoculated from his spinal cord were such as have been observed after similar inoculations with virus derived, not only from rabbits inoculated in series by M. Pasteur, but from a dog, a cat, and a wolf that died of ordinary rabies. It may well have been, therefore, that the intensive inoculations in him and in the other persons who died after them were not themselves destructive, but that they failed to prevent the rabies which was due to the bites. They may also have modified the form in which the rabies manifested itself; giving it the characters of the paralytic rabies usual in rabbits, instead of the convulsive or violent form usually, but not always,[2] observed in man after bites of cats or dogs.

The question is likely to remain undecided; for to avoid the possible, however improbable, risk of his intensive treatment, M. Pasteur has greatly modified it, and even in this modified form employs it in none but the most urgent cases.

The consideration of the whole subject has naturally raised the question whether rabies and hydrophobia can be prevented in this country.

If the protection by inoculation should prove permanent, the disease might be suppressed by thus inoculating all dogs; but it is not probable that such inoculation would be voluntarily adopted by all owners of dogs, or could be enforced on them.

Police regulations would suffice if they could be rigidly enforced. But to make them effective it would be necessary: (1) that they should order the destruction, under certain conditions, of all dogs having no owners and wandering in either town or country; (2) that the keeping of useless dogs should be discouraged by taxation or other means; (3) that the bringing of

[1] Other cases, as well as this, have led M. Pasteur to believe that the risk of death from hydrophobia is much increased by habits of drunkenness.

[2] Cases of paralytic hydrophobia have been observed, though rarely, in men bitten by rabid animals, and not treated by inoculation. It may, indeed, be suspected that at least some of the cases of "acute ascending paralysis" may have been cases of this form of hydrophobia, although, in the complete absence of the usual violent symptoms, no suspicion of the source of the disease was entertained.

dogs from countries in which rabies is prevalent should be forbidden or subject to quarantine ; (4) that, in districts or countries in which rabies is prevalent, the use of muzzles should be compulsory, and dogs out of doors, if not muzzled or led, should be taken by the police as "suspected." An exception might be made for sheep-dogs and others while actually engaged in the purposes for which they are kept.

There are examples sufficient to prove that, by these or similar regulations, rabies, and consequently hydrophobia, would be in this country "stamped out," or reduced to an amount very far less than has hitherto been known.

If it be not thus reduced it may be deemed certain that a large number of persons will every year require treatment by the method of M. Pasteur. The average annual number of deaths from hydrophobia, during the ten years ending 1885, was, in all England, 43 ; in London alone, 8·5. If, as in the estimates used for judging the utility of that method of treatment, these numbers are taken as representing only 5 per cent. of the persons bitten, the preventive treatment will be required for 860 persons in all England ; for 170 in London alone. For it will not be possible to say which among the whole number bitten are not in danger of hydrophobia, and the methods of prevention by cautery, excision, or other treatment, cannot be depended on.

We have the honour to be,
Sir,
Your obedient Servants,
(Signed) JAMES PAGET, Chairman,
T. LAUDER BRUNTON,
GEORGE FLEMING,
JOSEPH LISTER,
RICHARD QUAIN,
HENRY E. ROSCOE,
J. BURDON SANDERSON.

VICTOR HORSLEY, Secretary, June 1887.

The Report is followed by appendices, two of which we reprint :—

Abstract Report of Mr. Horsley's Experiments.

The first object of the experiments was to test M. Pasteur's method of transmitting rabies by inoculation, and to compare its effects with those of rabies due to the bites of dogs found rabid in the streets.[1]

Through the kindness of M. Pasteur, two rabbits inoculated by him were placed at the disposal of the Committee on May 5, 1886, and were conveyed within 24 hours safely to the Brown Institution, where the experiments were carried out by Mr. Horsley.

In these two rabbits the first symptoms of rabies appeared on May 11 and 12, and the disease followed exactly the course described by M. Pasteur.

At first the animals appeared dull, but continued to take food readily until symptoms of paralysis appeared. The first of these symptoms was commencing paralysis of motion of the hind-legs, not accompanied by any loss of sensibility. The paralysis soon extended to the muscles of the fore-legs, and later to those of the head, and the animals died comatose.

After post-mortem examination, portions of the spinal cord of each of these rabbits were crushed, according to M. Pasteur's method, in sterilized broth, and the liquid so obtained was injected beneath the dura mater into four rabbits and the same number of dogs, all being first rendered insensible with chloroform or ether.[2]

Of the four rabbits so inoculated, the first two showed the first symptoms seven days after the inoculation ; the third and fourth on the sixth day. The symptoms as well as the incubation period exhibited by these rabbits were exactly the same as were observed in those brought from M. Pasteur's laboratory. Careful notes and photographs were taken in the case of all the animals, in order that the constant and specific nature of the disease might be demonstrated by observations during life and after death. It was also observed that during the incubation period the temperature of the body remained normal, that is, about 39°·4 C. With the first definite symptom the temperature rose to about 40°·4 C., which is the temperature usually observed during the first day of the obvious illness. By the next day it began to fall, and on the third day after the appearance of the first symptom it averaged 37°·5 C. On the last day it was always below normal, and on one occasion fell before death to 24° C. The animals did not appear to suffer any pain whatever in the course of the disease. They were free from the spasms which, in the earlier stages of the malady in man, form so painful a feature of the disease, and indeed the disease in them resembled throughout that rapidly fatal, but painless, disease of man known as acute ascending paralysis.

The post-mortem appearances in the rabbits were remarkably uniform. As a rule nothing abnormal, save congestion, presented itself either in the brain, spinal cord, heart, blood-vessels, or serous membranes. The larynx, pharynx, and, more especially, the epiglottis, and the root of the tongue, were frequently intensely congested. The lungs showed almost invariably capillary congestion ; and sometimes small patches resembling broncho-pneumonia were observed. The mucous membrane of the stomach was very markedly congested, and there were at its cardiac extremity numerous hæmorrhages.[1] The constancy of these appearances was most remarkable, and corresponded in every particular with those subsequently observed in rabbits which had died of rabies from the bite of rabid dogs.

Of the four dogs inoculated, the first showed on the eighth day after inoculation an alteration in the voice and commencing excitement ; on the following day the excitement became excessive, and the bark was quite characteristic ; on the eleventh day the dog was aggressive, notwithstanding slight paralysis of the legs ; on the twelfth day the paralysis had increased, and on the next day there was complete paralysis and coma, and death occurred on the fifth day after the onset of the symptoms.

The second dog showed the first symptom on the ninth day after inoculation, when it was very dull and partially paralyzed ; its bark was characteristic. Next day the paralysis was almost complete, and on the twelfth day the animal died. This was therefore a case of the rapid paralytic form ; whilst in the first dog the disease was of the ordinary furious form of rabies terminating in paralysis.

The third dog showed the first symptom on the ninth day after inoculation, and from that time became gradually paralyzed, and died on the sixteenth day.

The fourth dog showed the first symptom in from eight to nine days after inoculation, and during the first day was extremely aggressive ; on the two following days the characteristic bark was observed ; and on the twelfth day there was paralysis of the hind-legs ; it died on the thirteenth day. Thus the furious form and the paralytic or dumb form of rabies were represented in equal numbers, whereas, in the usual mode of infection by biting, the former is more prevalent.

The post-mortem appearances were as follows :—The brain and central nervous system were in some of the dogs the seat of considerable congestion ; in others these organs appeared normal. The serous membranes were perfectly normal ; the larynx especially, and sometimes the pharynx, were congested ; the lungs always congested, especially in the lower lobes ; the heart normal ; the blood usually fluid, occasionally with post-mortem clots ; the stomach was always found to contain foreign bodies, such as straw ; and its mucous membrane was congested, frequently showing numerous hæmorrhages ; the small intestine was always empty, and the large glandular organs showed venous congestion.

For the purpose of exact comparison of the disease just described with that produced when rabies is communicated to the rabbit in the ordinary way, some rabbits previously narcotized with ether were caused to be bitten by rabid dogs of the streets, or were inoculated by trephining with material obtained from the spinal cord of dogs or other animals which had died of rabies, and in one instance from that of a man who had died with hydrophobia.

Four series of experiments of observations in which rabbits were bitten by rabid dogs from the streets were made. In one of them the dog by which the rabbit was bitten exhibited the dumb form, in others the furious form, of the disease. In each series excepting the first a large proportion of the rabbits died ; the symptoms presenting themselves in these cases were identical with those observed in the rabbits inoculated from M. Pasteur's virus, but the duration of the symptoms was usually longer. As

[1] This expression is adopted from that usual in France, "*rage des rues*."
[2] All the experiments performed in this inquiry were thus made painless.

[1] In some, signs of post-mortem digestion were found.

has been stated, rabbits inoculated by M. Pasteur's virus rarely show symptoms during more than three days before death, whereas the rabbits bitten by rabid dogs from the streets often live for a week after the appearance of the first symptoms.

The post-mortem appearances in the rabbits dying after having been bitten by rabid dogs of the streets were the same as those already described in rabbits inoculated with the virus from M. Pasteur's rabbits.

In the case of rabbits inoculated by trephining with the virus from animals dying of rabies of the streets, the incubation period was from 14 to 21 days. In all cases the symptoms were similar to those produced by M. Pasteur's virus, and those of rabbits bitten by rabid dogs from the streets; but in the prolongation of the disease approached more closely in character to the latter.

The results of these experiments confirm several of the chief observations made by M. Pasteur; especially—

(1) That the virus of rabies may certainly be obtained from the spinal cords of rabbits and other animals that have died of that disease.

(2) That, thus obtained, the virus may be transmitted by inoculation through a succession of animals, without any essential alteration in the nature, though there may be some modifications of the form, of the disease produced by it.

(3) That, in transmission through rabbits, the disease is rendered more intense; both the period of incubation, and the duration of life after the appearance of symptoms of infection, being shortened.

(4) That, in different cases, the disease may be manifested either in the form called dumb or paralytic rabies which is usual in rabbits; or in the furious form usual in dogs; or in forms intermediate between, or combining, both of these, but that in all it is true rabies.

(5) That the period of incubation and the intensity of the symptoms may vary according to the method in which the virus is introduced, the age and strength of the animal, and some other circumstances; but, however variable in its intensity, the essential characters of the disease are still maintained.

The certainty that the virus of rabies can thus be transmitted without essential change made it desirable, in the next place, to ascertain whether, as M. Pasteur states, it can be so attenuated that it may be inoculated without risk to life, and whether animals thus inoculated are thus made safe from rabies. The methods for this protective inoculation which M. Pasteur has employed are described.

To test them, six dogs were "protected" by injecting subcutaneously the emulsions of spinal cords of rabbits which had died of rabies; beginning with that of a cord which had been dried for 14 days, and, on each following day, using that of a cord which had been dried for one day less, till at last that from a fresh cord was used.

None of these dogs suffered from the injections; and when they were completed, the six dogs thus "protected," and two others unprotected, and some rabbits unprotected, were made insensible with ether, and were then bitten by rabid dogs, or by a rabid cat, on an exposed part.

A "protected" dog, No. 1, was bitten on July 8, 1886, by a dog which was paralytically rabid. It remains perfectly well.

An "unprotected" dog, No. 1, was bitten a few minutes afterwards by the same rabid dog, and died paralytically rabid.

A "protected" dog, No. 2, was bitten on November 6, 1886, by a dog which was furiously rabid; it remains well. At the same time, four "unprotected" rabbits were bitten by the same rabid dog, and of these two died of rabies in the usual form (*i.e.* 50 per cent. of animals bitten).

The same results followed with the "protected" dog, No. 3, and the "unprotected" rabbits, bitten at the same time. The dog still lives, the rabbits died of rabies.

The "protected" dogs, Nos. 4 and 5, were bitten on January 20, 1887, by a furiously rabid dog; and on the same day the "unprotected" dog, No. 2, and three "unprotected" rabbits were bitten by the same dog. The "protected" dogs remain well; the "unprotected" dog and two rabbits died with rabies (*i.e.* 75 per cent. of the animals bitten.)

The "protected" dog, No. 6, was bitten on three different occasions by a furiously rabid cat on September 7, 1886; by a furiously rabid dog on October 7, 1886; and by another furious rabid dog on November 6, 1886. It died ten weeks after being bitten for the third time, but not of rabies. It had been suffering with diffuse eczema during the whole of the time that it was under observation, and it died of this. At the post-mortem examination, no indication of rabies was found; and two rabbits, inoculated by trephining with the crushed spinal cord, showed no sign of rabies, either during life or, when they were killed several months afterwards, in any appearance after death. It was thus made certain that the dog was not rabid.

Thus, all the experiments performed by Mr. Horsley have confirmed those of M. Pasteur, and the experiments last described have shown that animals may be protected from rabies by inoculations with material derived from spinal cords prepared after M. Pasteur's method. The protection may be deemed somewhat similar to that given by the inoculation for anthrax, or by vaccination for small-pox, though the theory of the method of inoculation devised by M. Pasteur is very different from that upon which vaccination for small-pox and inoculation for anthrax is based. The further step, the prevention of rabies or hydrophobia in animals or in persons into whom the virus has already been introduced by bites or otherwise, is considered in the body of the Report.

In the course of his experiments, Mr. Horsley observed many interesting facts concerning the modification of the action of the virus according to the method of its inoculation, and the condition of the animal inoculated; but he found nothing to justify a belief that any animal not inoculated is insusceptible of rabies, or that the disease ever arises spontaneously.[1]

Coincidently with these experiments, some were made by Mr. Dowdeswell for the purpose of ascertaining whether any drugs can protect an animal from rabies. Their result is recorded in a paper read before the Royal Society, and may be summed up in the statement that rabies can neither be prevented nor influenced in its course, unless it be for the worse, by any of the drugs that were employed, including allyl alcohol, atropine, benzoate of soda, chloral, cocaine, curare, iodine (dissolved in iodide of potassium), mercuric perchloride, quinine, salol, strychnine, urethane.

M. Pasteur's Methods of Preventive Inoculation.[2]

M. Pasteur believes that the virus of rabies is a living micro-organism, and that, like some others, it produces in the tissues it invades an excretory substance by which, when present in sufficient quantity, its own development and increase are checked, as are those of the yeast ferment by the alcohol produced in the vinous fermentation. In accordance with this theory, he thinks that the spinal cords of animals that have died of rabies contain both the virus and this excretory substance which, practically, may be deemed its antidote. He believes therefore that by injections of an emulsion from such spinal cords into the systems of animals bitten or inoculated with the virus of rabies, the antidote may be able, during the period of incubation, to arrest and prevent the fatal influence of the virus. But, in order to avoid the possibility of injecting a still potent virus, M. Pasteur holds that the virus in the spinal cord must be weakened by drying the cord in a pure and dry atmosphere at a temperature of 20° C.; in which drying the efficiency of the antidote may be reduced to a much less extent than the potency of the virus. By such drying this potency may be so reduced that an emulsion of the dried spinal cord may be injected without any risk of producing rabies: and this risk is in no measure increased by the daily injections of emulsions from cords dried during a gradually less number of days, and which, though more virulent than those first used, still contain a larger proportion of the antidote than of the virus.

In accordance with this theory, the method of the preventive injections first used by M. Pasteur was adjusted in the following manner:—

Days of Inoculation.	1st.	2nd.	3rd.	4th.	5th.	6th.	7th.	8th.	9th.	10th.
Days during which the spinal cord had been dried	14	13	12	11	10	9	8	7	6	5

In consequence of some deaths among those who had been thus treated, M. Pasteur deemed it necessary, in cases of very severe bites and of persons bitten long before the treatment

[1] The minuter facts connected with all these experiments will soon be communicated to one of the scientific Societies.

[2] As derived from the observations made by the Committee, and from a full description supplied by Prof. Dr. Grancher, April 11, 1887.

could be commenced, to increase the intensity of the treatment by more speedily increasing the strength of the injections, by more frequent repetitions of them, and by using on certain days spinal cords dried during only three, two, and one days. Thus in September and October 1886 he adopted the following formula :—

Days of Inoculation.	1st.	2nd.	3rd.	4th.	5th.
Days' drying of the cords	14, 13, 12	11, 10, 9	8, 7	6, 5	4, 3

Days of Inoculation.	6th.	7th.	8th.	9th.	10th.	11th.
Days' drying of the cords	2	1	6, 5	4, 3	2	1

In very severe and perilous cases this course was repeated even three or four times. It was distinguished as the *méthode intensive*, and among such severe cases it was followed by a marked diminution of mortality. But when it appeared possible that it might be dangerous, M. Pasteur changed it for that which he now uses, and which may be thus represented :—

Days of Inoculation.	1st.	2nd.	3rd.	4th.	5th.	6th.	7th.
Days' drying of the cords	14, 13	12, 11	11, 10	10, 10	9, 9	9	8

Days of Inoculation.	8th.	9th.	10th.	11th.	12th.	13th.	14th.	15th
Days' drying of the cords	8	8	7	7	7	6	6	5

The material for injection is prepared by crushing portions of the dried spinal cord, and diffusing them in sterilized broth free from all risk of putrefaction, decomposition, or any change due to the presence of other micro-organisms ; and the injection is made with syringes through fine tubular needles into the subcutaneous tissue.

For transmissions of rabies through rabbits, in order to obtain the spinal cords required for its prevention in other animals, injections of virus of highest intensity are made through minute holes in the skull into the space under the dura mater or fibrous covering of the brain.

The materials for the protective inoculations are prepared in the same manner as those for the preventive, from spinal cords dried from ten days to one day.

UNIVERSITY COLLEGES AND THE STATE.

ON Thursday last, June 30, a deputation consisting of members of Parliament and others had an interview with the Chancellor of the Exchequer, who was accompanied by Mr. Jackson, M.P., to urge that Government assistance should be extended to local university colleges situated in various parts of the country. Among those present were Sir John Lubbock, M.P., Mr. Mundella, M.P., Mr. J. Chamberlain, M.P., Sir Lyon Playfair, M.P., Mr. Bryce, M.P., Mr. Arnold Morley, M.P., Mr. Jesse Collings, M.P., Mr. R. Chamberlain, M.P., Sir U. Kay-Shuttleworth, M.P., Mr. Theodore Fry, M.P., Mr. Burt, M.P., Sir Henry Roscoe, M.P., Sir A. K. Rollit, M.P., Prof. Stuart, M.P., Sir Bernhard Samuelson, M.P., Mr. Howard Vincent, M.P., Sir W. H. Houldsworth, M.P., Dr. Percival, and Sir Philip Magnus.

Sir John Lubbock, as the representative of the University of London, introduced the deputation. Their request was that a Parliamentary grant should be made to English colleges, as was already made to those in Ireland, Scotland, and Wales. The colleges on behalf of which they appeared were doing excellent work, but were greatly hampered for want of funds. The claims of these colleges were not based alone on their services to learning and study ; they were calculated to contribute largely to the material prosperity of the country. We now imported £150,000,000 worth of food annually, and our population increased at the rate of about 350,000 a year. How were so many to be fed, and how could a revival and return of trade be promoted? Our rivalry with foreign nations was now not on the battlefield but in the manufactory and the workshop ; and it was none the less severe because it was a competition rather than a contest. The need of the assistance for which they asked was very pressing. Without going into details as to particular colleges, he observed that the more recent institutions were generally spending more than their income, and even the oldest and the richest were sadly crippled for want of funds. It was found practically impossible to increase the subscriptions, and local authorities, as a rule, had no power to supplement their funds. As to raising the fees so as to make the colleges self-supporting, that might be possible but would be very undesirable. He only wished the fees could be abolished altogether, for those receiving education at the colleges benefited not only themselves but the whole nation. As to the expenditure on education, it was in the opinion of some people very large, but it was small in comparison with other items. Our ignorance cost us very much more than our education. Moreover, the principle for which they contended had been conceded in regard to Scotland, Ireland, and Wales. The grants to Irish colleges amounted to £25,000, to Scotland £16,000, and to Wales £12,000. The University of Glasgow had a special grant of £150,000 for building. None of the English colleges had such aid. Their request simply was that in this matter of education England should be treated in the same way as Ireland, Scotland, and Wales.

Mr. J. Chamberlain said that he attended as the representative of Mason College, Birmingham. Their case was the same in principle as that of all the other colleges. They urged that State-aided education had been accepted in principle in England and in all other countries, but in England alone we had not followed out the principle to its logical conclusion. We had stopped at the lower grade, and in this respect had made a great mistake. If it was of national importance that every one should have placed within his reach the instruments of education, it was of equal importance that they should be stimulated and encouraged to make use of these facilities. An attempt had been made in some halting fashion to redress the inequality in which this country was placed. The Charity Commissioners had recently been diverting funds which were, to some extent at all events, intended for the benefit of the poorer classes of the population to the purposes of higher secondary education. That practice was open to very serious objection, because it was robbing Peter to pay Paul ; and also because under that system nothing whatever was done for the colleges represented by the deputation, which were carrying on and extending the education given in the primary and secondary schools. The enormous development of primary and secondary education had created a demand for higher education. Proof of that was to be found in the fact that, although the institutions now represented were nearly all of them the creation of the present generation, they had had, in spite of deficiency of means, the most remarkable success ; and the daily increasing number of their students showed that they were established to meet a real want. The pressure of commercial competition came almost exclusively from those nations in which technical instruction and higher education had been developed and stimulated by the action of the State. The demand now made upon the Government was really a very moderate one, and the sum asked for was never likely to assume any very large amount. He believed the grants for primary education amounted to something between £2,000,000 and £3,000,000 a year, and that the additional grant now asked for would only amount to something like £50,000.

Mr. Goschen,—Will the deputation be able to supply me with a scheme for the distribution of the £50,000 or with the principle?

Mr. Chamberlain replied that, in his opinion, the grants should be made conditional upon further local aid. In that way the Treasury would be able to distinguish the colleges which were entitled to share in the grant.

Mr. Goschen,—Conditional upon further local aid?

Mr. Chamberlain,—Proportioned, in the first place, to the number of students, and, in the second place, conditional upon the amount of local aid.

Mr. Mundella,—Not in all cases further local aid.

Mr. Chamberlain agreed with Mr. Mundella that in some cases large local contributions were being made, and those cases should be taken into account. In conclusion, he urged upon the Government the consideration of a nationality that was sometimes apparently forgotten, and in the name of the 25 millions of English population he asked that they should receive a recognition in the matter of education proportionate to that given to Ireland, Scotland, and Wales.

Mr. Mundella pointed out that there were precedents for what they were asking in the grants given in Scotland, Wales, and elsewhere, and, in his opinion, Sir John Lubbock had rather underrated the benefit which schools in Scotland derived from the system. The high schools in Scotland were aided by grants out of the rates, and all middle-class education was largely supplemented by grants. In every country in Europe which really rivalled England a first-class technical education was within the reach of the humblest classes. In France very much the same education as given in our colleges could be obtained free and at the expense of the State. There was at present no power to aid the colleges in England, but he felt sure the Education Department would have no difficulty in framing a scheme for the purpose; and he ventured to hope the Chancellor of the Exchequer would assist them, and that grants should be allowed to these institutions in proportion to the efficiency of each college.

Sir Lyon Playfair observed that as the Government was going to introduce a Bill for giving to School Boards and other authorities power of rating for higher and technical education, he thought it would be well to extend the operation of the Bill by giving power to the same bodies to rate for higher colleges. The authorities were quite ready to be rated, and only wanted the necessary powers. The experience of commercial nations throughout the world was that the competition of industries was a competition of intellect. The difference between the policy of this and other countries was that while in other countries the State recognized the fruits of education and acted upon their perception of them, we left the first steps to the efforts of intelligent men in various localities. These men had now done their part, and, in consequence of the action of the English Government in the past, he thought it was the duty of the Government to come to the rescue of this small and highly intelligent body of men who had got up these colleges, and give to them that permanence which they were not likely to have without some small support from the public funds.

Sir Bernhard Samuelson said that in the course of the investigations of the Technical Education Commission he had the opportunity of visiting nearly all the colleges now appealing for aid, and, as far as his judgment enabled him to form an opinion, he must say that, considering the means at their disposal, these colleges were doing a thoroughly good work, and a work which deserved the encouragement of the community.

Mr. Thomas Burt, M.P., was not quite clear that this was the best way of spending money educationally, but he was quite sure it was a very good one. He could testify to the value conferred upon the miners and the industrial classes generally of the North by the College of Science in Newcastle. That institution had been greatly crippled in its resources. There was among the miners a widespread desire for improved education, and the College of Science and the University Extension lectures had not only given positive instruction of a valuable kind, but had conferred still greater advantages by creating and stimulating a desire on the part of the industrial classes for improved education. If the Government could see their way to help this and kindred institutions, a very great benefit would be conferred on the industrial classes of the country.

After a few words from Prof. Tilden (who differed from other speakers as to charging local rates), Dr. Perceval, and Sir George Young,

Mr. Goschen, in reply, said:—When Mr. Tilden sat down just now I was thinking on the whole that it was rather advisable for the deputation that the list of speakers was very nearly exhausted, because the differences of opinion began to be manifest. Mr. Tilden objected to powers being given to corporations or boroughs to rate themselves, whereas one of the objects of the deputation, or, at all events, one of the suggestions made to me during the course of this deputation, was that we should be sure to give powers to localities to rate themselves for these purposes. I do not know what the view would be of the deputation upon the subject, but I suspect that the bulk of the deputation is in favour of power being given, which of course would be optional, for large towns to rate themselves for this purpose. Mr. Tilden argued that it would be unfair that a college which drew students from other quarters should be supported by local rates; but I am inclined to think that that is a dangerous argument to use, because you might find whole masses of the population—the agricultural population, for instance, which would derive comparatively little advantage from these colleges—who might say that they would not wish to be taxed towards national funds which were to be applied to the big towns for the support of their colleges. So that I think it is rather a dangerous argument, and I further think the towns derive a very considerable advantage from having these large institutions, and that they ought not to look too narrowly to the area over which those rates would be charged. Then, Mr. Burt, I think, made a remark which to my ears was rather significant—that though this was a good way of spending money, he was not sure that this was the best way of spending money upon education. The interpretation I put upon that remark was that probably a further demand would be made upon the national purse for educational assistance in other directions which, in Mr. Burt's opinion, would be the best. No one is more aware, gentlemen, than you, who have studied this subject so deeply, that there are many directions in which, and many points of view from which, this educational question has to be considered. I am glad to have received this most important deputation. I might almost call it in some respects a formidable deputation; but I know that this is not the only direction in which pressure would be put upon the Treasury with regard to education. You represent here what I understand to be the higher forms of technical, scientific, and I think you may also say of literary instruction. There is the elementary education, and the expense attendant upon that; then there is the Science and Art Department, which in some respects is apart from the elementary education; and there remains a field for which I am sure pressure will be put upon the Treasury, which is that technical education which lies between the elementary education and that higher form of education which I understand these colleges give. I make these remarks to show that it would be impossible, I think, for the Government—though, of course, upon that it would be rather for the Education Department than for the Chancellor of the Exchequer to speak—to look upon this matter simply from a partial point of view. We must review the whole of the demands which are likely to be made upon the Government for educational purposes. It is certain that the comparative test which has been applied by this deputation is a very dangerous one to the finances of the State. The grant to certain colleges is certain to lead to a grant to other colleges; and if some of the gentlemen present had heard how the Scotch gentlemen argued that their case was practically peculiar, and that the assistance given to the Scotch Universities could not be possibly made applicable to the English colleges, they would see the scope of the remarks which I have made. I am far from saying that, while there is this serious pressure likely to be put upon the Government, this is not a question which must be carefully and deliberately faced, and looked upon in all its bearings. You have come to me to-day, I presume, in order to remove what we may call any financial scruples with regard to your proposals; but, of course, it would mainly rest with the Education Department to work out any system if these colleges are to be granted assistance upon the scale which you suggest, and so you will not expect me to give you any declaration of policy to-day. But I presume that you are anxious that the arguments you have used should sink into my mind, in order to remove any opposition I might make financially to proposals that would be made by other departments and through other channels. Now, from the financial point of view, I gather that you assent to certain propositions, and the main of those propositions is that assistance should only be given by the State where there has been a distinct local effort in support. I admit that we cannot argue any more that the State should not aid education up to a certain line. That line seems to be by public opinion raised every day, and while formerly it was only the very poor, now it is a higher class; and so from class to class it appears to me the demand for State education is rising very rapidly, perhaps I might say very formidably. But I am glad to take note of the fact that, anxious as you are, representing as you do immense educational efforts in various parts of the country, you do not wish in any way to stop that magnificent flow of private contributions towards education which has been the glory of this country in many ways. It would be deplorable if by the action of the State you were to arrest that action;

you are rather anxious that that action should be stimulated by moderate contributions from the public funds and from rates. As to contributions from rates, I think that as the municipal institutions of our country are more and more reformed and developed, and the more power is given to them in the course of that process of decentralization which is now accepted by almost all politicians, the more power you may give to these localities to act with a certain freedom in the way of assisting such institutions as they think are calculated to advance their interests in every way. There is one point on which I should like to say something from a personal point of view. When I made a remark that your colleges were partly literary and partly technical, I did not wish to convey the impression that I think colleges for general culture deserve less recognition than colleges for technical education I think that they must to a very great extent stand or fall together, and that it would be an error—though I am aware there may be others here who take a different view—if technical education were too much to displace that general education and development of the intellect which surely must always be one of the great objects of education in every form. I do not know whether the sum which was first mentioned, I think, by Sir John Lubbock has been arrived at by any general agreement, college by college, or whether it is a mere general guess. But I am sure it would be necessary, as a preliminary examination of the case which you have put before me, that there should be some standard suggested, either of numbers or of local contribution, and also of work, before the matter could be taken into serious practical consideration; because not only are there these twelve colleges, but I fancy that, as soon as any arrangement had been made in favour of them, we should find another list of colleges, not precisely on the same footing, but which were sufficiently strong to make a kind of claim on that comparative system which is constantly increasing the national expenditure. I think those of the deputation who are members of Parliament will acknowledge that it would be perfectly impossible to deal with the matter in the supplementary estimate this year, even if we assented to it, without much further examination, for it is really the Education Department which must examine this matter. I have not had the opportunity of consulting my colleagues on the magnitude of the sum which you suggest, or on the general principle. All I can say to-day is that I am glad to receive the suggestions which you have made; that I recognize, of course, the great importance of further developing technical and scientific education; but I cannot pledge myself to any particular sum or to any particular mode of carrying out your wishes. The deputation may rely on the Government giving the matter its most serious attention, and we shall be most willing to receive suggestions from such men as Sir Lyon Playfair, Sir John Lubbock, Mr. Mundella, and the other gentlemen who take so deep an interest in education, to see what practical shape can be given to the wishes of the deputation.

Mr. Mundella observed that the condition of some of the colleges was such that it was desirable that the intentions of the Government should be known at the earliest possible moment.

Sir John Lubbock moved, and the Mayor of Sheffield seconded, "That the thanks of the deputation be given to Mr. Goschen for his courtesy."

The deputation then withdrew.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 16.—"Dispersion Equivalents," Part I. By J. H. Gladstone, Ph.D., F.R.S.

The object of this paper was to bring to the notice, especially of chemists, the subject of dispersion equivalents; a property of bodies similar to the refraction equivalents which are now generally recognized. In the paper of Gladstone and Dale in the Phil. Trans. for 1863 they had adopted the difference between the refractive indices for the solar lines A and H as the measure of dispersion. This, divided by the density, gave the specific dispersion. In 1866 they multiplied this by the atomic weight, and termed the product the dispersion equivalent. The subject has scarcely been touched since that time either by English or Continental observers.

The author holds that the following conclusions are warranted by the accumulated data:—

(1) That dispersion, like refraction, is primarily a question of the atomic constitution of the body: the general rule being that the dispersion equivalent of a compound is the sum of the dispersion equivalents of its constituents.

(2) That the dispersion of a compound, like its refraction, is modified by profound differences of constitution; such as, changes of atomicity.

(3) That the dispersion frequently reveals differences of constitution at present unrecognized by chemists, and not expressed by our formulæ.

The dispersion equivalents of a few elements may be determined by direct observation, such as phosphorus, sulphur, selenium, &c.; but more important results have been obtained from organic substances by comparing the dispersion equivalents of different liquid or dissolved compounds of carbon.

From a consideration of the data afforded by Continental observers, or obtained by the author himself, the following table has been drawn up. The dispersion figures must be taken merely as approximate.

Substance.	Atomic weight.	Refraction equivalent A.	Dispersion equiv. H—A.
Phosphorus	31	18·3	3·0
Sulphur, double bond	32	16·0	2·6
,, single bonds	,,	14·0	1·2
Hydrogen	1	1·3	0·04
Carbon	12	5·0	0·26
,,	,,	6·1 ?	0·51
,,	,,	6·1	0·66
Oxygen, double bond	16	3·4	0·18
,, single bonds	,,	2·8	0·10
Chlorine	35·5	9·9	0·50
Bromine	80	15·3	1·22
Iodine	127	24·5	3·65
Nitrogen	14	4·1	0·10
CH_2	14	7·6	0·34
NO_2	46	11·8	0·82

An examination of many salts of potassium and sodium in aqueous solutions led to the conclusion that there was always a difference of about 0·09 in their dispersion equivalents; but it was not so easy to determine the actual dispersive value of the metals in question, the determinations of potassium from different salts varying from 0·40 to 0·59.

The main conclusion is that the specific dispersive energy of a compound body is a physical property analogous to, but distinct from, its specific refractive energy; and that the phenomena of dispersion are capable in like manner of throwing light upon chemical structure.

Royal Microscopical Society, June 8.—The Rev. Dr. Dallinger, F.R.S., President, in the chair.—Dr. E. M. Crookshank exhibited a series of cultivations of micro-organisms, and called attention to the somewhat unusual circumstance of being able to show such a typical series all growing at the same time. One of the specimens shown was a chromogenic *Spirillum*, which had developed its colour in the depths of the gelatine, contrary to the general rule. He also showed a micro-organism which had been said to cause the swine fever—or, rather, swine erysipelas—in Germany. It was to be noted that in Germany there had been many cases of swine disease, and that a different organism had been found associated with it there from the one found here, and recognized as the cause of Dr. Klein's swine fever. So far as he (Dr. Crookshank) had been able to make out, they were not identical, the German form being an extremely minute Bacillus forming only a cloudy appearance and seeming to be similar to mouse septicæmia. He thought there was good ground for regarding the two diseases as distinct from each other, the German form being swine erysipelas as distinct from swine fever. He also exhibited an example of a Bacillus obtained from putrid fish, which caused the remarkable phosphorescence frequently noticed when fish was decaying.—Mr. Freeman exhibited a number of series of sections of the anatomy of spiders, worms, &c., made by Mr. Underhill at Oxford.—Mr. Eve called attention to some specimens of *Actinomyces* from the jaw of an ox, and described the effect of the disease upon the animal.—Prof. Rupert Jones and Mr. C. D. Sherborne's paper "On the Foraminifera, with especial reference to their variability of form, illustrated by the Cristellarians," was read.—Mr. G. Massee gave a *résumé* of his paper on the genus Lycoperdon, illustrating the subject by drawings on the blackboard.—Prof. Bell said that the Fellows of the Society would remember that in the course of last winter he described what he had observed in some diseased grouse which had been sent to him for examination. Within the last few weeks, the disease, whatever it might be, had been killing grouse in considerable numbers on the moors in the south-west of Scotland. He had received some of these grouse, and examined them very carefully to see if he could discover any cause of death. In the case of the first, though there were tapeworms, there was no evidence that they were the cause of death; in the second case the birds had died from inflammation of the intestines, the cause of which was not quite clear; and in the third case they died of *Strongylus*. It would therefore appear that what was called "grouse disease" must be either more than one disease, or it must be a disease which could kill its victims in different stages. He was himself disposed to think that there was more than one cause of disease, but up to that time there was no diagnostic sign internally to show conclusively what those causes were.—Mr. Grenfell's paper on "New species of *Scyphydia* and *Disophysis*" was read.

PARIS.

Academy of Sciences, June 27.—M. Hervé Mangon in the chair.—Remarks accompanying the presentation of a volume on the geodetic and astronomic junction of Algeria with Spain, by General Perrier. In this work, published at the joint expense of the French and Spanish Governments, a detailed account is given of the methods of observation and of the results obtained by the protracted operations which secure for the physical science of the globe the accurate measurement of an arc of the meridian of over 27° comprised between the Shetland Islands and Laghwat in Algeria.—Remarks accompanying the presentation of the first volume of a course of infinitesimal analysis intended for the use of persons who study this science with a view to its mechanical and physical applications, by M. Boussinesq. This work, the first volume of which deals with the differential calculus, is addressed more especially to those physicists, naturalists, engineers, and others, who are little accustomed to the treatment of algebraic formulas, but who, for their special purposes, feel the want of understanding in its essence and chief results the calculus of the infinitely little, or of continuous functions.—Memoir on submarine sound-signals, by M. Brillouin. The two chief results already obtained are transmission of sound to a distance of thirty-five kilometres, and the neutralization of all violent surface disturbances, such as thunderstorms or hurricanes. A summary description is given of the apparatus, together with a general statement of the circumstances under which these signals might be used with advantage.—Observations of Barnard's Comet (May 12, 1887), made at the Bordeaux Observatory with the 0·38 m. equatorial, by MM. G. Rayet, Flamme, and E. Courty.—Observations of a planet sighted at the Observatory of Marseilles, by M. Borrelly. The observations of this body, which is of the twelfth magnitude, extend from June 9 to June 19 inclusive.—On linear differential equations of the third order, by M. Paul Painlevé. In supplement to the paper published in the *Comptes rendus* of May 31, the author here deals with the linear and homogeneous equation of the third order—

$$y''' + ay'' + by' + vy = 0;$$

and he arrives at the general conclusion that, given a linear and homogeneous equation of the third order, it may always be ascertained, by a limited number of purely algebraic operations, whether its integral be algebraic, or the equation may be reduced to a quadrature.—Determination of the quantity of bisulphate of potassa in a diluted liquor, by M. E. Bouty. The author here deals with the difficulty of determining this quantity, which arises from the fact that in diluted solutions the bisulphate of potassa is always accompanied by sulphuric acid and sulphate of potassa. He shows that the bisulphate is stable especially in hot and concentrated liquors, and that the proportion of this salt increases with the excess of one or other of the reacting bodies.—On the ammoniacal vanadates, by M. A. Ditte. The vanadates here treated are those of methylamine, of ethylamine, ammoniaco-magnesian vanadate, and the double ammoniacal vanadates. The general study of these vanadates, prepared by the dry and wet processes, shows that all these compound bodies are reducible to a few well-defined types and simple formulas, such as: $3VO_5,MO$, $2VO_5,MO$, $3VO_5,2MO$, for the acids; VO_5,MO for the neutral vanadates; $VO_5,2MO$, $VO_5,3MO$, $VO_5,4MO$, for the basic salts, apart from the water, the quantity of which varies according to the circumstances in which the crystallization is effected.—Solubility of uric acid in water, by MM. Ch. Blarez and G. Denigès. For the determination of this point the authors have applied the process of analysis by chameleon indicated in their previous note.—On the hydrochlorate of ferric chloride, by M. Paul Sabatier. M. Engel having recently announced that he had succeeded in preparing this substance, which had been anticipated but not isolated by M. Sabatier, the author remarks that so early as 1881 he had obtained and fixed the composition of the hydrochlorate of ferric chloride (*Bulletin de la Société Chimique*, second series, p. 197, 1881).—On the identity of dambose and inosite, by M. Maquenne. A careful study of a remarkably pure specimen of dambonite prepared according to M. Girard's indications from the caoutchouc of the Gaboon, shows that dambose is identical in every respect with the inosite already described by the author. Hence dambonite should be considered as the dimethylene of inosite, and the term dambose should be replaced in chemical nomenclature by that of inosite, which has the right of priority and is in other respects more convenient.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

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CONTENTS. PAGE