THURSDAY, JUNE 6, 1878

MODERN NAVAL ARCHITECTURE

A Manual of Naval Architecture. For the Use of Officers of the Royal Navy, Officers of the Mercantile Marine, Shipbuilders, and Shipowners. By W. H. White, Assistant Constructor, Royal Navy, &c. (London: John Murray, 1877.)

NO one acquainted with our own and foreign navies can doubt that at the time of the establishment of the Royal Naval College at Greenwich Great Britain had been falling very much astern of other countries in the professional education of its naval officers. In the days when ships were all pretty much alike, and differed chiefly in forms and proportions, the greater progress of other navies in technical science was, perhaps, of no great moment; but in days like the present, when the sea teems with experimental ships flying the royal and mercantile flags of this country, the neglect of known principles, or the failure to discover others, may have the worst results. It was for this reason, among others, that the writer of these remarks long since joined those who pressed for the establishment of a Naval College worthy of the country and of the time, and it was doubtless for this reason also that the Government of the day—and more especially Mr. Goschen, then First Lord of the Admiralty—founded the great Naval Institution which now flourishes at Greenwich. It is there that Mr. White, the author of the work before us, has been engaged in expounding naval architecture to naval officers and other students, and it was in the performance of that important duty that he felt the need of such a work pressed upon him.

Mr. White is himself, in no small degree, the product of previous acts of wisdom on the part of Boards of Admiralty; and as those Boards very often come under just censure for their errors and omissions, it is a pleasure to be able sometimes to record their wise and enlightened doings. Mr. White was a distinguished student (and afterwards a Fellow) of that Royal School of Naval Architecture at South Kensington which Lord Hampton took so much pains in promoting, and which the Admiralty founded; having been elected to a studentship at a competitive examination in a Royal Dockyard School, where he had previously undergone a good and extensive grounding in elementary, and indeed in more than elementary, science. It is gratifying to know that Mr. White, like all the naval architects of high position at present in the Admiralty, thus represents a successful system of Government training, commencing in the Royal Dockyard Schools, and advancing through higher-class royal schools, to the most eminent spheres of professional influence.

The object of the present work is to supply to naval officers, and to such others as may need or desire it, a statement of the general principles which underlie the profession and practice of the naval architect. Without some clear and definite knowledge of these principles it is not possible for naval officers to apply modern vessels to their designed services with skill and success. The loss of the *Captain* and the foundering of the *Vanguard* afforded dramatic examples of the class of disasters which may be expected to result and must result from the imperfect handling of modern vessels; and if all the facts of the case could be known, the loss of the *Eurydice* would probably be brought more or less within the same category. In the case of the *Captain* notwithstanding the grievous well-known defects of her design, the laws which regulate a ship's stability and power to carry canvas if known to and applied by those in command of her, would have suggested the paramount necessity of shortening sail in the wind which capsized her; and the evidence taken after the sinking of the *Vanguard* points clearly to the fact that the prompt closing of certain water-tight doors, and other like measures would, in all probability, have saved the ship, or at least have given her ample time for reaching shallow water. As regards the *Eurydice* it is well-known that she was an experimental vessel, designed by a naval officer many years ago, and that some not very usual features tending to reduction of stability entered into her form. It is obvious that in all such cases—and indeed in the cases of all ships—a knowledge of the principles which enter into their construction and use should, as far as possible, be communicated to those upon whom is placed the responsibility of employing them under all the varying conditions of sea-service; and this not merely for the purpose of securing their safety, but also with a view to their efficient and economical employment. Only the few persons who have had special opportunities of observing the facts can imagine the extent to which the performances of ships in steaming, in sailing, and in other operations, depend upon the knowledge and skill of those who command and work them. It is not too much to say that, as regards vessels which in the main resemble each other, the differences in the officers who command them usually obliterate altogether the distinctive qualities of the vessels themselves, and the relative skill of their designers. To the readers of NATURE it is doubtless needless to dwell upon the desirability of the naval officer, whether of the royal or the mercantile marine, possessing the knowledge of ships, and of naval principles, which Mr. White's work is designed to convey.

In composing this work the author has shown himself most judicious in determining the limit beyond which it would not be well to carry theoretical investigation in addressing naval officers. In each of those chapters in which a temptation to over-indulgence in this respect would most be felt by a man highly trained in the theory of naval architecture, the self-restraint of the author is obvious, and deserves all praise. The book is readable throughout by all naval officers who have availed themselves with energy and spirit of the educational advantages which the royal navy now affords to every officer, and it presents to them, for the first time, a sound, well-selected, and trustworthy summary of naval science such as was beginning to be most strongly felt in the merchant no less than in the royal navy.

In his first chapter the author explains the buoyancy of ships, their subdivision into compartments, and the effect of admitting water into compartments variously placed, and discusses with clearness and with the latest information the vexed question of freeboard. The accuracy and perspicuity with which this chapter is written are remarkable, and prove the fitness of the author for the work he undertook. Diagrams are given wherever they would serve to assist the reader in readily apprehending the subject. The second chapter is devoted to tonnage measurement, and, while mainly derived from an article contributed by the author to *Naval Science* (a periodical which is no longer published), it embodies all that naval officers need know upon the subject, including the measurement of yachts, and the regulations of the Suez Canal; these latter, we regret to say, are in some respects carried out at present in violation of the understandings which were come to with the British Government, and were announced as authoritative by our Ministers in Parliament. The third chapter is an admirable statement of the principles which regulate the statical stability of ships—a subject upon which Mr. White well deserves to be pronounced a high authority, and one who has worthily extended this branch of science.[1] This chapter, like some later ones, embodies information which the naval architect would do well to study, more especially as regards the effect upon stability of adding, shifting, and removing weights. At p. 63 the author remarks that the question of neutral or indifferent equilibrium has little practical interest in connection with ships, "for which stability and instability are alone important." However true this may have been a short time ago, it is no longer so, for the case of the *Inflexible* has invested the question of infinitesimal stability, and of neutral equilibrium, with a strong and melancholy interest, and an interest which must go on increasing if such ships are repeated. The investigations of the *Inflexible's* state led the writer of these lines to observe and consider a peculiar condition of stability which may, and doubtless will, arise in citadel ships like the *Inflexible*, with unarmoured ends large in proportion to the citadel, and which would add a curious case to the numerous curves of stability with which this chapter is illustrated. It is the case in which the citadel is so formed and proportioned, that the ship, with her ends penetrated, would have no stability in her upright position, but would acquire a small amount on inclining through a greater or less angle, and lose it again on being inclined still further. This case would exhibit itself in a curve of stability by a mere loop, of greater or less length and depth, and at greater or less distance from the origin, according to circumstances. A similar result would of course arise in any ordinary ship, the statical stability of which was *nil* in the upright position, but become positive at an inclination. We mention it here, however, because in the case of a citadel ship it reduces to an absurdity, and to worse than an absurdity, that reference to "range" of stability which has been much too frequent in recent discussions. In a future edition Mr. White would do well to extend his remarks upon this branch of the subject, as it has become one of great practical, and even of vital interest.

The following chapters, on the Oscillations of Ships in Still Water, on Deep-Sea Waves, the Oscillations of Ships among Waves, and Methods of Observing the Rolling and Pitching Motions of Ships, together form a most valuable treatise on a branch of naval science which is both in form and substance essentially modern, and full both of interest and of future promise. Although this part of the work has necessarily been composed chiefly by compilation, it is the result of much labour, and of a close study of a large number of essays and discussions which have appeared from time to time during the last eighteen years. In the first paper ever read (in 1860) at the Institution of Naval Architects, Dr. Woolley said—"One of the chief benefits to be looked for from the Institution which we are inaugurating to-day is a more systematic inquiry into the laws of nature on which the motions of a vessel at sea depend than has hitherto been attempted." These were prophetic words, for the field of labour to which scientific men were thus invited was very soon entered upon by Mr. Froude, who has most worthily and successfully laboured in it ever since. He has been joined by other labourers who have well and steadily advanced the good work, including several able French *savans* (notably M. Bertin, of Cherbourg, and M. Duhil de Benazé), and most recently Dr. Woolley himself, who a month since contributed to the same institution a most able analytical discussion of the constitution and properties of deep-sea waves. No summary of the recent striking developments of science respecting the constitution of sea waves, and the behaviour of ships among them, which can at all compare with that here given by Mr. White can anywhere be found.

The following chapter on the strains experienced by ships is an equally clear and comprehensive statement of another thoroughly modern branch of study. It is primarily based upon a paper published in the *Philosophical Transactions* by the present writer in 1871, in the calculations and construction of which Mr. White largely co-operated. The methods of investigation pursued were novel, and of so detailed a nature as to place the subject on a solid basis of fact, with the result of either setting aside or subverting most of the opinions—for they were but opinions—which had previously prevailed. The present author gives the substance of those labours, and adds to them the results of many others of more recent date, supplementing the general investigation with brief examinations of minor and local strains. The chapters on the structural strength of ships and materials for shipbuilding, although of less immediate value to naval officers than those that illustrate the behaviour of ships in motion, abound with facts which they will find of daily value afloat. In places the author is somewhat more historical and diffuse, perhaps, than is strictly consistent with the object of the work, but all that he records is valuable, and the narrative passages will doubtless add to its attractions, especially in the eyes of the younger officers. With respect to the important question of the "Resistance of Ships" (so designated, as usual, although "Resistance to Ships" would surely be more correct) the author has performed a like service to that rendered in the case of the recent discoveries respecting waves and the oscillations of ships. He has sketched and summarised the existing knowledge of the subject, and here as elsewhere has kept mere abstract considerations and theories well under the control of practical requirements. He has done justice to the recent labours of Mr. Froude in this sphere

[1] By a valuable paper "On the Calculations of the Stability of Ships," read at the Institution of Naval Architects in 1871, the joint production of Mr. White and Mr. W. John, now of Lloyd's Register Office, and by other contributions to the knowledge of this subject.

of investigation likewise, and has shown how advantageous his experiments with models have been. The present Controller and Constructors of the Navy deserve great praise for giving their steady support and appreciation to Mr. Froude in his experimental work.

The remaining chapters of Mr. White's book are on Propulsion by Sails and by Steam, and on the Steering of Ships. While well and clearly written throughout, and embodying all the settled science of these questions, these chapters appear to us to be less firmly based in some respects, and to present more controversial matter than the earlier parts of the work. We are unable, for example, to approve of the strong preference expressed for the compound engine over other types of marine engine. Basing his views upon the actual performances of three separate types of engine—the ordinary old type of jet-condenser engine, the surface-condenser type, and the compound type—the author goes on to claim for the last-named enormous advantages. In making his comparisons and drawing his inferences, however, he seems to us to generalise too freely, and to leave out of consideration many facts and circumstances which we are bound to consider before pronouncing in favour of the compound engine. Nor are we by any means alone in calling in question the pre-eminent merits of this form of engine when regarded in the light of general principles. Among others who have expressed similar views we may advert to Mr. Neil McDougall, himself an officer of the Admiralty (Steam Branch), who in 1875 published an essay on "The Relative Merits of Simple and Compound Engines as applied to Ships of War," and who arrived, after a long and patient investigation of the Admiralty and other records, at these conclusions, viz., "That there is no insuperable difficulty in the way of working simple engines at the same pressure as that in use at present with the compound engine at sea. Equal economy might, then, fairly be expected with the simple engine, specially fitted, as with the compound, under ordinary working conditions;" and, "all available evidence goes to show that it is impossible that the compound engine can be to any serious extent superior to the rival engine, at present pressures, in point of economy." It is worthy of note that the first prize, in a professional competition, was awarded to this essay, the judges being Prof. Cotterill, M.A., F.R.S.; Chief Inspector W. Eames, R.N., the Chief Engineer of H.M. Dockyard, Chatham; and the eminent engineer, J. Penn, F.R.S.

The rapid perusal which alone we have been able to give to this large volume of more than 600 pages has disclosed to us but few blemishes, and these, for the most part, of the slighter sort, and such as are chiefly due to brevity of treatment. We will refer to those only which we observe in the first chapter. The remark that the equality existing between the total weight of water displaced by a ship and her own weight "is equally true of wholly submerged vessels as of ships of ordinary form, having only a portion of their volume immersed," will create difficulty in the minds of some of the readers for whom the work is intended. The principle is doubtless true of vessels which just float, without any reserve of buoyancy above the water's surface, and which, therefore, are wholly submerged; but its obvious inaccuracy when applied to the *Vanguard*, the *Eurydice*, the *Grosser Kurfürst* and other ships which are very unfortunately, but nevertheless very certainly, "wholly submerged" points to the necessity for a modification of the language employed. Again, the statement that "the weight of the ship in tons multiplied by thirty-five gives the number of cubic feet in the volume of displacement" may puzzle some young readers of the work—and there should be many young readers of it among the naval cadets and others—who may fail to see that the number "thirty-five" is derived from the previous statement that 64 lbs. is the weight of a cubic foot of sea-water. It is impossible to make these elementary matters too clear for young sailor-officers. Such students will also require some assistance in understanding the paragraph in which the author explains the pressures acting on the bottom of a ship. Following the mathematical method of assuming the existence and action at every point of a pressure acting "perpendicularly to the bottom," and treating this as made up of three components, the author justly speaks of the vertical components only as affecting buoyancy, and no less justly dismisses the horizontal components, as they must, on each set, "obviously be exactly balanced amongst themselves." No youngster accustomed to the mathematical treatment of forces or pressures will find the slightest difficulty in all this; but we can well imagine less favoured sailors, of all ages, pausing at the statement that the water pressure can "at every point" be resolved into three such components, and searching in vain for horizontal pressures, for example, under the flat bottom of a ship. It is, we are well aware, impossible to avoid difficulties of this kind without great elaboration; but we hold it to be a primary necessity to avoid them to the utmost possible extent in a work like this, which has been expressly written for those very many persons, outside the naval architect's profession, who are "more or less intimately connected with shipping," and desire to get some knowledge of the subject. In one important respect we think the author's explanations of the sub-division of ships into compartments, and of the consequences of admitting water into them, much too brief, viz., that of the division of ships by middle-line bulkheads. This is dismissed with the remark that the advantages of such bulkheads are too obvious to need comment; but however obvious the advantages of the system may be, the effects of admitting water to the divisions so obtained should certainly have been investigated and set forth. In any case it would have constituted both an interesting and an instructive branch of the subject other parts of which the author has treated so fully and so well; but the recent improvements in the *Alexandra* and other twin-screw ships has made it also a subject of great importance. The introduction of the middle-line water-tight bulkhead wherever it could be applied is one of the most valuable of the many valuable improvements introduced by Mr. Barnaby and his staff since the first charge of our Admiralty Naval Construction became theirs, and officers who are "shipmates" of this system (to use a naval phrase) will be disappointed to find it so summarily dismissed from the author's and the reader's notice. The defect is the more obvious because of the singular clearness and fulness with which the general question of sub-division is expounded. The task of mentioning the blemishes and defects of so excellent and comprehensive a work as this is too distasteful to be pursued through more than a single chapter.

It is impossible to conclude this notice of Mr. White's work, however—which, by the by, is well-indexed and turned out of hand by Mr. Murray in his usual style of efficiency and excellence—without reflecting upon the immense developments which naval architecture has undergone during the present century, is still undergoing, and has yet obviously to undergo. There is not a chapter in this volume of many chapters, which does not abound with illustrations and indications of improvement.

> "Men, my brothers, men the workers, ever reaping something new:
> That which they have done but earnest of the things that they shall do."

Take, for a single example, the material of which the naval constructor builds his ships. How recently is it that wood was in universal use; now far more than nine-tenths of the ships built are of iron. But already iron is being superseded by steel, and a few weeks ago Mr. Martell, the able Chief Surveyor of Lloyd's Registry, said at the Institution of Naval Architects, "The time has now come when it is said by many others besides the manufacturers that steel can be used with as much confidence as iron; and it is held that whilst the properties of mild steel are in every respect superior to those of iron, the cost—having regard to the reduced weight required—will warrant the shipowner, from a commercial point of view, in adopting the lighter material." The manufacture of this "mild steel" for ship-building purposes, and indeed for many other purposes, is, as Mr. White states, "mainly due to the efforts of Mr. Barnaby, Director of Naval Construction, who had previously conducted most of the experiments on steel made in the Royal Dockyards, and done much to develop the use of the material." By insisting upon the combination of increased strength with great ductility in the material, Mr. Barnaby directed the attention of manufacturers to the great importance of turning their energies into a new direction, and the result has been the production of a most excellent material for the purpose, which can now be obtained in any quantity from several firms. But even these most recent forms of steel seem destined speedily to be replaced by the fluid-pressed steel of Sir Joseph Whitworth, who, by the application of enormous force to the metal in a molten state, can solidify and make sound castings which contain whatever proportion of carbon may be desired. By pursuing this process through a thousand details, and many years of costly experiment, this distinguished man has given us a material which is as superior to the best mild steel produced otherwise as that is itself superior to the ordinary forms of ship-building iron, and the process promises to carry us to results of far greater importance still. It is perhaps one of the few reflections which should reconcile us to the persistence of mankind in the pursuit of the arts of war that the eager desire to improve guns and war-ships is continually conducting us to collateral and large improvements in the materials employed for the purposes of civilised life—for commercial ships profit no less than war-ships and guns by the improvements of men like Sir Joseph Whitworth, who has himself contributed immensely to the manufacturing arts, both directly and indirectly. The fields in which Mr. Froude is labouring are not less prolific than this, while the forms of vessels, and the propelling powers applied to them, are receiving continual improvement. In respect of naval architecture, at least, Tennyson is right; and that which we have done is but an "earnest" of the things sooner or later to be done.

E. J. REED

TROPICAL NATURE

Tropical Nature and other Essays. By Alfred R. Wallace. (London: Macmillan and Co., 1878.)

MR. WALLACE tells us that the luxuriance and beauty of tropical nature is a well-worn theme and that there is little new to say about it, and yet he thinks that none have as yet attempted to give a general view of the phenomena which are *essentially* tropical or to determine the causes and conditions of those phenomena. Indeed many very erroneous ideas are commonly entertained about the charms of the tropics and about the brilliant tints of its flowers, and birds, and insects.

In the first three chapters of this most interesting volume Mr. Wallace treats of the climate of the tropics, of its vegetation, and of its animal life. A fourth treats of the humming-birds as illustrating the luxuriance of tropical nature. The next two enter on the discussion of the nature and origin of the bright colours of animals and plants, showing how far and in what way these are dependent on the climate and physical conditions of the tropics. A seventh chapter contains an account of certain curious relations of colour to locality, which are almost exclusively manifested within the tropical zones, while the next and last chapter tries to explain the probable origin of many of the forms of life now characteristic of tropical regions.

Despite its being a well-worn theme and its want of novelty, Mr. Wallace has succeeded in writing a most interesting volume on the peculiarities of tropical life, and this chiefly from the results of his own long experience of nature in the eastern and western tropics of the equatorial zone, while his theory to account for the diverse colours, the special adornments, and the brilliant hues which distinguish certain male birds and insects—a theory quite opposed to that of Mr. Darwin's—cannot fail to attract the attention of all interested in this subject.

Mr. Wallace's account of his theory is perhaps the most important portion of his book; he finds, on close examination, that neither the general influence of solar light and heat, nor the special action of variously-tinted rays, are at all adequate causes for the many wondrous complexities of colours with which we are acquainted. He would therefore take another view, dividing the colours into groups, as they are protective to the creature, act as warning colours, or sexual colours, or typical colours, or simply as in floras, attractive colours.

Mr. Darwin's theory on this subject of colours was that all, or almost all, the colours of the higher forms of animal life were due to voluntary or conscious sexual selection, and that diversity of colour in the sexes is due at least, first of all, to the transmission of colour variations either to one sex only or to both sexes, the difference depending on some unknown law and not being due to simply natural selection; but Mr. Wallace regards this view as erroneous, and to him the very frequent superiority of the male bird or insect in brightness of colour, even when the general coloration is the same in both sexes, seems to be due primarily to the greater vigour and activity and the higher vitality of the male. He reminds us that the colours of an animal usually fade during disease or weakness, while robust vigour and health add to their intensity. This intensity is most developed in the male during the breeding season. It is also very general in those cases in which the male is smaller than the female. This greater intensity of coloration in the male would be further developed by the combats of the males for the possession of the females. Increased vigour, acting thus on the epidermal system, would soon produce further distribution of colour, and even new tints and markings. Nay, even the remarkable display by so many male birds of their peculiar beauties of colour and plumage may be thus accounted for; for at the pairing season these birds are in a state of the greatest energy. Even unornamental birds, at such a season, flutter and spread out their wings and erect their head-crests or their tail-feathers; and there would be a progressive development of these ornaments in all dominant races, and if those portions of the plumage which were originally erected under the influence of anger or fear became largely-developed and brightly-coloured, the actual display under the influence of jealousy or sexual excitement would be quite intelligible; the males would soon find what plumes were most effective, and would endeavour to excel their rivals.

It will thus be seen that Mr. Wallace's theory of colour might almost be called a molecular one. The causes of colour are due to molecular or chemical changes of certain substances, and on the action on these of light, heat, and moisture. They can be produced or intensified by processes of development, and this as the surface bearing these colours is extended or diminished and as there is a surplus of vital energy; or they may be, as in plants, acted on by some, as yet, unknown local action dependent on the soil or on vegetation.

Doubtless this theory will give rise to much controversy; and in the course of this, no doubt, many important facts will be elucidated. Thus, Mr. Wallace reminds us that, in the case of those female birds with brighter plumage than the males, the females are larger, more pugnacious, and show more of vital energy.

One portion of tropical nature Mr. Wallace has overlooked in the volume—that which spreads its brilliant colouring over the white rocks that lie under the sea. Crowds of lovely forms are here; and they are worthy of a chronicle.

E. PERCEVAL WRIGHT

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 ensure the appearance even of communications containing interesting and novel facts.]

Extinct and Recent Irish Mammals

PROF. BOYD DAWKINS, in his interesting "Preliminary Treatise on the Relation of the Pleistocene Mammals to those now living in Europe," just published by the Palæontographical Society, places the Irish elk (*Cervus megaceros*) among the prehistoric mammals in consequence of its presence "*in the peat bogs* of England, Scotland, and Ireland;" indeed, in a former monograph on the British Pleistocene Mammals, by Messrs. Dawkins and Sandford, and published by the same Society, it is stated that "the *C. megaceros, C. tarandus, C. elaphus*, and *Bos longifrons*, have been found associated *in peat in Ireland*."

Now although remains of the red deer and short-horned ox are not uncommon in Irish turbaries, there is not a single authenticated instance of either the Irish elk or reindeer having been discovered *in peat*. This observation as regards the Irish elk was made by Prof. Owen long since (1846) in the "British Fossil Mammals," and a wide field of observation confirms my impression of its truth as far as Ireland is concerned. Moreover, there is no reliable evidence to show that man and the Irish elk, reindeer, mammoth, horse, and bear, were contemporaneous in this island.

With reference to the smaller Irish mammals referred to in Mr. Dawkins's treatise. On the authority of Wilde and others it is stated that both the *Martes foina* and *M. abietum* are natives. The former, at best a doubtful British species, has never been authenticated in Ireland, but the latter is not uncommon.

Again, neither the weazel (*M. vulgaris*) or polecat (*M. putorius*) have any claims to be included in the Irish fauna. As to *Felis catus* there is much doubt, the individuals being in all probability domesticated cats run wild.

In regard to the rodents given in Mr. Dawkins's list as Irish, neither the *Arvicola agrestis* nor the *Arvicola amphibius* have been identified; but on the other hand, the *house mouse* (*M. musculus*), reported absent, is unfortunately too plentiful in many districts.

The red deer is still a native of the mountains around the Killarney Lakes, and until recently a few lingered in the wilds of Connaught, but certainly it is not just now on the Tipperary Mountains, though the fallow deer does occur there. Of the shrews, none of which are given in Mr. Dawkins's list, the pigmy (*S. pygmæus*) is the only species hitherto identified in Ireland. I mention these facts, having lately bestowed much attention to the study of Irish mammals.

A. LEITH ADAMS

Royal College of Science, Dublin, May 25

Hints to Workers with the Microscope

I AM now and have been for the last fortnight enjoying a treat which everyone who possesses a microscope, a slip of glass to lay on the stage, and a piece of thin microscopic glass with a little cotton wool, can enjoy for the price of 1*s*. Mr. Bolton, formerly of Stourbridge and now of 17, Ann Street, Birmingham, sends me weekly supplies of rotifers, and has just sent me *Rhinops vitræa* and *Hydatina senta* in great profusion. With ordinary *compressoria* and live boxes these are troublesome to see, as they are very lively rovers. To those who may not know the *Midland Naturalist* or the *Microscopic Transactions*, I recommend a particular method which I recently sent to those publications.—Take a plane glass slide, on it drop one or more of the rotifers in a drop of water about half-an-inch in diameter, and draw off the surplus water, if any, carefully with the empty pipette. Then fray out a very very small portion of cotton wool (I always use a watchmaker's glass in the eye to do all such operations) until it is much extended, and spread out and lay this on the drop. Upon that lay the thin microscopic glass, the thinner the better, and then set up the capillary attraction by gently touching it with a needle. Draw off any superfluous water from the edges with the pocket-handkerchief and you will have a little wilderness of wool in which the rotifer is restrained in its movements, protected from pressure, and within reach of very high powers. The amount of wool depends on the size of the rotifer. Hydatina requires more depth than rhinops. The same plan answers equally well for all roving animals. The poduridæ in particular when placed in deep glass cells are easily seen by this apparatus, and it saves many a weary and vexatious five minutes with the compressorium, which, even at the best, requires with living animals extraordinary patience. The rotifers are easily found and secured with the pipette and a watchmaker's glass in the eye after a very little practice. Mr. Bolton's studio is of the greatest value to naturalists and cannot be too well known, for to those who have not time to look for specimens it is a great privilege to be able to purchase them.

Fort Hall, Bridlington Quay, Yorks, May 25

F. A. BEDWELL

The Virial in Thermodynamics

IN my letter on the Virial in NATURE, vol. xviii. p. 39, a line of the description of a force's "radiancy" (as it was there termed) with respect to a given point was accidentally omitted; and the definition should have been the product of the distance of its point of application from the given point or "focus," and the resolved part of the force in the direction of that distance, the last and most important member of which product was unmentioned by some unintentional oversight in the description. It would also be wrong, in the dynamical equation of the virial, the *vis viva* and the radiancy of momentum of a system to range the *vis viva* and virial together (as I did in the letter) in the class of physical agents, bound therefore by known laws of conservation, since either their joint or their separate effects in changing the system's total radiancy of momentum are easily seen, if we suppose one of them, for example, the *vis viva*, to act alone, to be totally unfettered, and therefore their actions to be of a measurable kind, but not subject, like that of natural agents, to any known laws of physical connection.

The rate of acceleration of a fourth part[1] of the triple sum formed of a system's moments of inertia round any three axes at right angles to each other is the rate of change of its total radiancy of momentum, and if the various parts of the system are all moving uniformly in straight lines, their joint *vis viva* measures the rate of this change; but it cannot be said to cause or produce it, since, by the laws of motion, the bodies *unassisted* or left to themselves will continue to produce, by their *vis viva*, the same rate of change, without connection in doing so, with any known physical agent, from whose class, accordingly, it is evident that both *vis viva* and the "virial," or the radiances of a system's forces as linked in an equation with acceleration of total moment of inertia, are formally excluded. The equation has very important applications: as when, on an average of a sensible time, the total moment of inertia remains unaltered, or when a system is apparently at rest: for example, in the case of immobility of a gravitating atmosphere in a state of equilibrium, under any possible assigned law of variation of temperature. But the idea of this state of immobility being a necessary one, which the *vis viva* and virial together of the ponderable mass is constrained to conserve—placing them together in a fixed and definite relationship to each other, or to any other agents of physical phenomena, subject to known laws of conservation—was evidently a totally mistaken and unreal one.

A. S. HERSCHEL

[1] The acceleration of the total moment instead of the fourth part of the total moment of inertia was wrongly written in the postscript of my letter as equal to the rate of change of momentum-radiancy. Actual energy and "virial," as defined by Clausius, are also half of the quantities here described as *vis viva*, and radiancy of a force.

The Meteor of May 12

IN NATURE (vol. xviii. p. 105) the statement occurs among the "Notes" that the brilliant bolide of May 12 was seen at Geneva, the local time being said to agree. May I call your attention to the fact that the difference between Greenwich and Geneva is 25 minutes (or 2 minutes more if Berne time is compared). Thus 9.45 is 9.20 Greenwich time, nearly half an hour after the meteor recorded by English observers. It is now a well recognised fact that large meteors come in *groups*, naturally raising the suggestion that such groups form part of a slowly disintegrating mass.

From records I have obtained from Scarborough, Leeds, and Bradford, combined with an excellent observation of the latter part here, and the notices contained in your number for May 16, I find the *probable* positions of beginning and end to be from 4 miles west of Northallerton to 5 miles west of Hawick, a distance of 94 miles, the angle of flight being 38° with the horizon, making an actual course of 108 miles described in about 9 seconds, giving the unusually slow rate of 12 miles per second. This, and one or two other points, make it possible that the course really extended further. But the end was in nearly all instances obscured by clouds and the observations in line with the meteor's course. An exact description of its course by your Edinburgh correspondent (especially as to whether it passed near the zenith) would make this certain.

May I venture to make one or two suggestions to your correspondents who favour you with notes on meteors? When, for any reason, celestial measurements cannot be given, *rough measurements* of the positions made either by holding a ruler in front of you, or, if light allows, by the minutes upon a watch face, which shall enable the actual height, and distances from a point of the compass to be determined, are by far the most valuable items, accompanied of course by exact time, date, and place. Thus a meteor might appear at a height of 15 inches on a base 27″ (an arm's length) and 12″ W. of north. Or, having placed 12 o'clock level, the hand at 10 minutes past might point to the place of disappearance, an angle of 7 minutes (42°) giving the distance E. of south. Prof. Herschel, of Newcastle, gives some capital hints in a letter published in the *Scotsman*, May 1, upon the March daylight meteor. Either he or Capt. G. L. Tupman (or I myself) would at any time be glad of observations, in which case a rough plan, indicating its position among the stars, would be of great value. The position of the meteor with reference to houses, trees, &c., the course across a window, if seen indoors (the observer's position and distance being also given and the points of the compass), and many similar items are very useful for after reference and may lead to very exact determinations.

J. EDMUND CLARK

20, Bootham, York, May 28

"Divide et Impera"

VERILY we have divided and subdivided, and as yet are but little nearer the "command" promised.

I am a subscriber to your able magazine, which is extensively read in South America, and beg to bring the following subject to the notice of your zoological readers:—

At this distance of 8,000 miles and at the outskirts of civilisation, books of reference are scarce, or, if existing, difficult of access. In constructing some zoological tables I am constantly beset by the difficulty of discovering two, three, four, five, six, or more synonyms for the same species, or in the case of a supposed new species find afterwards that the same animal has been described under another name; the genera often differ! the families constantly vary, and even the higher classification is by no means constant.

Where is all this perpetual confusion to end? In the science being destroyed by excessive or faulty nomenclature? We want an Ariadne with her thread to lead us out of the maze, for such it is, especially to young zoologists like myself.

Is it too much to expect that an international zoological congress should be constituted with power to methodise and reduce to order this chaotic classification, and print and publish authorised lists of fauna? How are young naturalists to progress, constantly hindered as they are by wasting weary hours in seeking for that which should be patent at a glance?

Such a congress should, by unanimous consent of the chief zoological societies of Europe, fix immutably not only the superior classification, but also the generic and specific nomenclature; and in the event of new species being discovered, whilst conceding the right to the discoverer and describer to affix its title, this should in all cases be subject to the approval of the International Congress, which might sit permanently in the shape of one or two deputies.

It seems to me the science has already emerged from its swaddling clothes, and it is high time for our scientific authorities to give up that fatal habit of generating and clinging to their own superstitions, and fostering that intense jealousy so characteristic of them, which, leading to multiplicity of systems, leads only to distraction.

There may be aberrant forms yet undecided (there will be such, perhaps, to the end of time); borderlands to be limited; yet there is ample material to fix unalterably and universally the skeleton of that science, to fill in whose details there are multitudes of willing and skilled hands, ready to aid, in all parts of the world.

E. W. WHITE

Buenos Ayres, May 1

A Quadruple Rainbow

IN the afternoon of Friday, the 24th ult., while proceeding by rail to Dublin, and before reaching Abbeyleix station, I observed the curious phenomenon of four rainbows forming a single bow—that is, without any dark space intervening between the colours. The four bows were all of the same, or nearly the same, breadth, but I cannot say whether all the colours were present in each.

The brighter colours—as the red and yellow—showed that the bows were arranged in the same order.

I called the attention of several other passengers to the novel spectacle.

A word in explanation of this strange appearance from some of your learned contributors would, I think, be interesting.

Model School, Waterford, June 1 HENRY P. DOWLING

Classes for Women at University College

IN view of the new charter enabling the University of London to confer degrees on women, and the increased demand for a higher education of women, the council of this college have determined to provide for them systematic instruction in regular college classes.

In most subjects the junior classes for women will be distinct from those attended by male students. The senior classes will more generally be open to both sexes, and these classes, which are already open to both, as fine arts, philosophy of mind, &c., will remain so.

Prospectuses embodying the results of this change will be ready by the 18th inst. TALFOURD ELY

University College, London

[OUR St. Petersburg correspondent, "C. S." must send us his name (in confidence), before we can publish his last communication.]

PROF. JOSEPH HENRY, LL.D.

PROF. HENRY was born December 17, 1797, at Albany, New York, where also much of his early life was passed. The year of his birth seems, however, uncertain, some authorities placing it in 1799, or even later. He had at first the advantages of only a common school education. A parish library supplied him with boyish reading, and his early tastes were in the direction of romance and the drama. He was nearly grown when the accidental possession of a copy of Robinson's "Mechanical Philosophy" turned his thoughts towards natural philosophy. After two years of work as a watchmaker, he came under the training of the Albany Academy, where he developed a degree of mathematical talent which, in 1826, led to his selection for the duties of instructor in mathematics in that institution. Prior to this, having had some experience in the field as a surveyor, he was associated with Amos Eaton in the Geological Survey along the line of the Erie Canal, projected and sustained by General Stephen van Rensselaer. Failing physical health led to his taking this step. He returned home with a robust constitution, which never failed him throughout his life.

While occupied with his duties as mathematical instructor in the academy—then in charge of Dr. T. Romeyn Beck—he commenced that line of investigation in electricity which resulted in the important discoveries that have made his name famous. He attended the lectures on chemistry of Dr. Beck, and assisted in the preparation of his experiments. At this time he devised and published an improved form of Wollaston's sliding-scale of chemical equivalents, in which hydrogen was adopted as the radix—a contrivance which is hardly known, even by name, to the present generation of chemists. Thus, while Prof. Henry's original contributions to science were chiefly physical, his first scientific work was in the department of chemistry. His work with Dr. Beck enabled him, after his removal to Princeton—where he became professor of natural philosophy in 1832,—to take up the duties of the chemist, Dr. John Torrey, when that well-known teacher was disabled for a time by ill health.

It was in the interval between 1828 and 1837 that the most important work of his life was accomplished in the line of strictly scientific research.

If we compare the poverty of his apparatus and the poverty of his means for research and publication with the importance of the results which he reached, we may accord him a place by the side of Faraday as an experimentalist. He became the sole discoverer of one of the most singular forms of electrical induction, and was among the first, perhaps the very first, to see clearly the laws which connect the transmission of electricity with the power of the battery employed. One of the problems to which he devoted himself was that of producing mechanical effects at a great distance by the aid of an electro-magnet and a conducting wire. The horse-shoe electro-magnet, formed by winding copper wire round a bar of iron bent into the form of a U, had been known before his time, and it was also known that by increasing the number of coils of wire greater force could be given to the magnet, if the latter were near the battery. But when it was removed to a distance the power was found to weaken at so rapid a rate that the idea of using the electro-magnet for telegraphic purposes seemed hopeless. Henry's experiments were directed toward determining the laws of electro-motive force from which this diminution of power resulted, and led to the discovery of a relation between the number of coils of wire round the electro-magnet and the construction of the battery to work it. He showed that the very same amount of acid and zinc arranged in one way would produce entirely different effects when arranged in another, and that by increasing the number of cells in the battery there was no limit to the distance at which its effects might be felt. It only remained for some one to invent an instrument by which these effects should be made to register in an intelligible manner, to complete the electro-magnetic telegraph, and this was done by Morse. Henry himself considered the work of an inventor as wholly distinct from that of a scientific investigator, and would not protect the application of his discoveries, nor even engage in the work of maturing such applications. He never sought to detract from Morse's merits as the inventor of the magneto-electric telegraph, but did on one occasion, under legal process, give a history of the subject which was not favourable to Morse's claim to the exclusive use of the electro-magnet for telegraphic purposes. Some feeling was thus excited; but Henry took no other part in the controversy than to ask an investigation of some charges against himself contained in an article of Morse's.

The results of these researches are chiefly recorded in the *Transactions* of the Albany Institute, the volumes of the *American Journal of Science and Arts* for the period, and the *Transactions* of the American Philosophical Society. His "Contributions to Electricity and Magnetism" were collected in a separate volume in 1839. The analysis of these important researches, and the discussion of the questions of priority connected with them, will be the duty of the academician to whom shall be assigned the preparation of a memoir or eulogy of the distinguished author.

The memoir in the *American Journal* gives a list of twenty-two memoirs and discoveries by Prof. Henry. To these papers should be added an important series of communications, made chiefly to the National Academy of Sciences during the past four or five years, upon the laws of acoustics as developed in the course of investigations conducted for the Light-House Service in order to determine the various conditions involved in the transmission of fog-signals. These investigations have been carried forward mainly in government vessels, and occupied Prof. Henry's close personal attention during many weeks of each season.

Besides these experimental additions to physical science, Prof. Henry is the author of thirty reports, between the years 1846 and 1876, giving an exposition of the annual operations of the Smithsonian Institution. He has also published a series of essays on meteorology in the Patent Office Reports, which, along with an exposition of established principles, contain many new suggestions, and, among others, the origin of the development of electricity, as exhibited in the thunderstorm.

In 1837 he visited Europe and made the acquaintance of Faraday, Wheatstone, Bailey, and other eminent physicists, discussing with Wheatstone their projects for an electric telegraph. He returned to his lectures with the zest and vigour acquired by this exchange of views with men of like pursuits with himself, and held his place as the foremost of American scientific teachers until 1846, when he was called to an entirely different sphere of activity.

Ten years before Congress had accepted, by a solemn act, the curious bequest of James Smithson, made to the United States in trust, "to found at Washington an establishment for the increase and diffusion of knowledge among men." The will gave no indications whatever as to the details of the proposed establishment, and long consideration was therefore necessary before the Government could decide upon its organisation. It was not until 1846 that a definite plan of organisation was established by law. When this was done Prof. Henry was at once looked upon as pre-eminently the man to be the principal executive officer of the institution. He accepted the position with "reluctance, fear, and trembling," upon the urgent solicitation of Prof. Bache. From the beginning two different views of the proper direction in which the energies of the establishment should be devoted have been entertained. There was a scientific party, which held that the operations of the establishment should be confined strictly within the limits prescribed by the donor, and in the sense in which he himself, as a scientific investigator, would naturally have construed his own words—in fact, that it should be entirely an institution for scientific research and publication. Another party was desirous of giving it a larger scope and wider range, including literature and art as well as science. The new secretary, of course, sympathised entirely with the scientific party, who considered most of the objects proposed by the other party as foreign to the proper purpose of the institution, and the expenditure of money upon them as contrary to the expressed intention of the donor. The whole policy of Henry was directed towards diminishing, as far as possible, the expenditure of the Smithsonian fund upon the library, the building, the museum, and art-gallery, by having these several objects provided for in other ways. He got the library removed to the Capitol and deposited in the Library of Congress, and the art-gallery superseded by the Corcoran Gallery of Art. The impropriety of charging the Smithsonian fund with the support of the governmental collections was so obvious that Congress has for several years provided for the maintenance of the National Museum, as it has now become, in connection with the institution. He aimed at a complete separation of the museum from the institution; the Government leasing the building for the use of the former, while the latter should find more modest and appropriate but less expensive quarters. This project, however, he did not live to carry out.

Henry was, of course, the authority most frequently and regularly consulted by the Government on all questions which arose involving applications of science or of scientific principles. His greatest services to the Government were rendered as a member of the Light-House Board, a position which he held from the time the Board was organised. His principal duties were at first to inquire into the various methods of illumination, and especially to test the oils proposed for this purpose. Of late years he began to investigate the subject of fog-signals, which led to a very extended series of experimental researches on the causes which influence the propagation of sound through the air, and which sometimes render it inaudible at comparatively short distances. These experiments were mostly published in the annual reports of the Light-House Board.

The idea of using the telegraph for communicating the weather reports originated with Professor Henry, and was put in operation at the Institution at an early period of his connection with it.

It was while engaged in the discharge of certain experimental work on Staaten Island last December, connected with the photometric laboratory of the Light-House Board, that he experienced a partial paralysis, which yielded soon to treatment, but was doubtless the precursor of the nephritic attack to which he succumbed. In April he presided at the opening meeting of the session of the National Academy of Sciences, held in the rooms of the Secretary of the Smithsonian, and submitted an address to his associates, read by the Home Secretary, recounting with touching simplicity his recent decline of power, and expressing his desire to be relieved from the cares of the office of President. As a mark of affectionate respect, the Academy unanimously requested him to retain this post during his life—leaving the duties to be discharged by the Vice-President. It was on this occasion that the announcement was made to the Academy, by Prof. Henry, and, subsequently, in fuller details, by Prof. Fairman Rogers, the treasurer, of the creation of an endowment to be called "the Joseph Henry Fund." This fund consists of forty thousand dollars, securely invested, the income of which is for the support of Prof. Henry and that of his family, during the life of the latest survivor. Afterwards the fund is to be transferred, in trust, to the National Academy of Sciences, the income to be for ever devoted to scientific research. No more graceful and well-merited tribute of respect and affection was ever bestowed upon a man of science by the spontaneous offerings of personal friends and associates. Alas! that its honoured object should have remained so brief a time to enjoy the peace and satisfaction of this gracious endowment. If it is true that republics are ungrateful, it is pleasant to know that the absence of imperial and kingly patronage may be compensated by a sovereignty not less potent.

The whole course of Prof. Henry was marked by an elevation of character entirely in keeping with his intellectual force. Placed in a position where the temptation to lend the use of his name to commercial enterprises was incessant, he so studiously avoided every appearance of evil that the shadow of suspicion never rested upon him. His services to the Government in many capacities, especially in that of member of the Light-House Board, where his experiments saved it hundreds of thousands of dollars, were entirely gratuitous. His salary was paid from the Smithsonian bequest, and he never asked the Government for a dollar on account of his services. An elevated but genial humour, a delicate poetic taste, a memory replete with anecdote, a refined intellectual face, and an impressive bearing made him one of the most valued members of the intellectual society of Washington.

Prof. Henry leaves a wife and three unmarried daughters, who have been assiduous helpers in the scientific work of their father, making good to a degree the loss of an only son, whose death in early manhood was a sad disappointment of parental hopes and youthful promise.

Prof. Henry was buried May 16, in the Rock Creek Cemetery, near Georgetown, D.C. The President of the United States, the cabinet officers, diplomatic corps, and members of Congress and of the National Academy, were among the mourners.

Prof. Spencer F. Baird, long the Assistant-Secretary to the Smithsonian Institution, was, on May 17, unanimously elected by the Board of Regents as Prof. Henry's successor in the office of Secretary of that institution. No more acceptable appointment could have been made.

We express our indebtedness to Prof. Silliman, who has kindly forwarded us early sheets of an article on Prof. Henry to appear in the *American Journal of Science and Arts*. From this, together with an article in the New York *Nation*, we have gathered most of the above details of Prof. Henry's life and work.

MAJOR-GENERAL SIR ANDREW SCOTT WAUGH

FROM a paper in the *Royal Engineer Journal*, we obtain some interesting facts concerning the career of this able officer of the Indian Survey, whose death we announced at the time (vol. xvii. p. 350). Having been appointed in July, 1832, and retiring in 1861, his services in the department extended over a period of close upon thirty years.

Under Col. Everest, as his astronomical assistant, Waugh took part in the measurement of the great arc of the meridian extending from Cape Comorin, the most southern point of the peninsula of India, to the Deyrah Doon at the base of the Himalayas. In December, 1834, we find him with his Chief at the measurement of the northern base-line in the above valley, an operation that extended over a year. In the connection of this base with that near Sironj, about 450 miles to the south, Waugh took a large share of work, and also, in 1837, at the re-measurement of this Sironj base with the new bars that had been used in the Deyrah Doon. The wonderful accuracy secured on these grand operations may be estimated by the difference of length of the Deyrah base-line, as measured, and as deduced by triangulation from Sironj, being only 7·2 inches.

He shared with Everest the arduous observatory work, carried on simultaneously at the stations of Kaliana Kalianpur, and Dumargidda, by which the arc of amplitude was determined, and brought this important work to an end in 1841. Between 1834 and 1840 he was also conducting the Ranghir series in the North-West Provinces, and in 1842, the triangulation through the malarious Rohilkund Terai, which Everest acknowledged to be "as complete a specimen of rapidity combined with accuracy of execution as there is on record." Sir Andrew thus had the good fortune to be the immediate pupil of the great geodesist who placed the Indian Surveys on their present footing, for the whole system was then. elaborated and brought to a high pitch of excellency This, Waugh, on succeeding to the appointment of Surveyor-General and Superintendent of the Topographical Survey in 1843, made it his first object to keep up and improve. Sir George Everest's high opinion of the man who had served under him on so many important operations may be understood from the singularly strong terms which he used when recommending Waugh as his successor.

He began by carrying out the remaining series, seven in number, a total of 1,300 miles in length and embracing an area of some 28,000 square miles, originating from the Calcutta longitudinal series on the *gridiron system* projected by Sir G. Everest. The eastern side was formed by the Calcutta meridional series (begun in 1844 and finished in 1848), which terminated in another base-line near the foot of the Darjeeling hills.

One of the finest of surveying operations, commenced about this period of Sir A. Waugh's tenure of office, was the north-east Himalaya series, connecting the northern ends of all the before-mentioned meridional series. In these field-operations Waugh took a leading part. The line of country was along the base of the Himalayas (the Terai). These operations led to fixing the positions and the heights of some of the highest and grandest of the Himalayan peaks in Nipal and Sikkim; one of these, 29,002 feet above the sea, was named by Waugh Mount Everest, and was found to be the highest in the world; its name, one well-known to the natives, is Devidanga. Mr. Clements Markham, in his exhaustive memoir on the Indian Surveys, states that the dangers and difficulties in the execution of this work were air greater than have been encountered in the majority of the Indian campaigns.

On the South of India, the South Concan, the Madras coast series, the South Parisnath and South Maluncha series were also begun and finished, and several pages might be written of the dangers and difficulties the Survey Staff had to contend with. Of all the Indian Survey work that originated in Col. Waugh's tenure of office, on account of the general interest attaching to the country, its beauty, and its vastness, the survey of Kashmir was chief. This important and difficult survey, finally completed in 1864, was in full swing of work at the time Sir Andrew Waugh retired from the department, and we cannot do better than quote the lines the late Lord Canning was pleased to write privately to Sir Andrew Waugh in July, 1859, on being shown the first instalment of this work. Coming from so high and intelligent a source, they are a tribute not only to the Surveyor-General, but to the whole department. "I cannot resist telling you at once with how much satisfaction I have seen these papers. It is a real pleasure to turn from the troubles and anxieties with which India is still beset, and to find that a gigantic work of permanent peaceful usefulness, and one which will assuredly take the highest rank as a work of scientific labour and skill, has been steadily and rapidly progressing through all the turmoil of the last two years. I never saw a more perfect or artistic-like production of its kind than this map."

The other meridional series were also pushing forward. Jogi Tila by Jhelum and the Gurhagurh by Umritsur to join the Arumlia. Kattywar and Cutch must also be included. For a full and detailed account of these many difficult operations that comprise every kind of country and climate that India presents, we must refer the reader to the memoir before-mentioned.

Space will not allow us to enter into detail of all the important work done for the survey of India during Waugh's tenure of office, but it may be stated roundly that he advanced the triangulation by no less than 316,000 square miles, an area three times that of England, Wales, Scotland, and Ireland, and of this 94,000 were topographically surveyed. Col. Waugh retired from the service in 1861, receiving as usual the honorary rank of Major-General, and Her Majesty conferred on him the honour of knighthood in the same year. He had held the post of Surveyor-General for seventeen years, had maintained the high character of the survey, and was highly esteemed by the whole department. The results of the work during his incumbency are given in some thirteen different volumes and reports deposited in the India Office, parts of which originally complete appear to have been lost. In 1856 the Royal Geographical Society presented him with their gold medal, and in 1858 he was elected a member of the Royal Society. For some years past his health had been failing, and he suffered much, dying on February 21, 1877, at his residence in South Kensington.

THE HARVEY TERCENTENARY

ON Saturday evening the Royal College of Physicians commemorated the tercentenary of Harvey, the discoverer of the circulation of the blood, by a banquet in the library of their institution in Pall Mall. The president, Dr. Risdon Bennett, occupied the chair, and the company included the Marquis of Ripon, Viscount Cardwell, Mr. Gladstone, M.P., Mr. Lowe, M.P., Mr. Spencer Walpole, M.P., Baron Cleasby, Mr. Justice Denman, Prof. Huxley, Dr. Allen Thomson, Prof. Owen, Capt. Cameron, R.N., Dr. Carpenter, Mr. Benett-Stanford, M.P., Dr. Lyon Playfair, M.P., the President of the Royal College of Surgeons, the President of the College of Physicians, Ireland, Dr. Richardson, Sir W. Jenner, and Sir W. Gull.

The Marquis of Ripon and Mr. Walpole, M.P., in responding to the toasts of the House of Lords and the House of Commons, respectively, paid high tributes to the memory of Harvey and to the reputation of the College of Physicians, while Lord Cardwell spoke with much satisfaction of the legislation resulting from the investigations of the Commission on Vivisection. The speech of the evening, however, was undoubtedly that of Prof. Huxley, in responding to the toast of the evening, "The Memory of Harvey," proposed in happy terms by the President. Prof. Huxley replied as follows:—

Mr. President,—In attempting to fulfil the task you have imposed upon me, I am mindful that I address myself to an audience which is already familiar with William Harvey's claims to the honour which we are assembled to show him. For, within these walls, the memory of your illustrious Fellow and chief benefactor, is kept perennially green by the customary piety of the speaker of the annual oration which Harvey founded; and his merits have been placed before you, with exhaustive completeness, by a long succession of able and eloquent orators. Even if the time and place were fitted for a disquisition on these topics, I could not hope to be able to add to the facts already known, or to place them before you in a new light. And, happily, this is not my function; I have to act simply as your remembrancer, to play the part of the herald who announces the familiar titles of a monarch on a state occasion.

Harvey's titles are three—he was the discoverer of the circulation of the blood; he wrote the "Exercitatio de Motu Cordis et Sanguinis"; he formulated anew the theory of epigenesis, and thereby founded the modern doctrine of development.

His first and, in general estimation, his greatest title to our honour has been challenged; but only to the confusion of the challengers. A century ago, your Fellow, Dr. Lawrence, in the excellent memoir prefaced to the College edition of Harvey's works, met the arguments of those who had, up to that time, attempted to dim his fame, with a solid refutation, which has never been answered and to my mind remains unanswerable. In our own day, Dr. Willis has stated the facts of the case, and deduced the inevitable conclusion, with no less force and cogency. And, having taken some pains to get at the truth of the matter myself, I may state my clear conviction that Harvey stands almost alone among great scientific discoverers, not so much that, as Hobbes said, he lived to see the doctrine he propounded received into the body of universally accepted truth, but because that doctrine was both absolutely original, and absolutely new. I have yet to meet with a single particle of evidence to show that, before Harvey declared the fact that the blood is in constant circular motion, there was so much as a suspicion on the part of any of his predecessors or contemporaries that such is the case. Neither in Galen, nor in Servetus, nor in Realdus Columbus, nor in Cæsalpinus, is there a hint that a given portion of blood sent out from the left ventricle, passes through the body and the lungs, and returns to the place from whence it started; yet this is the essence of Harvey's discovery.

Hence when we hear of pompous inscriptions being put up in Spain to Michael Servetus, "the discoverer of the circulation," or in Italy to Cæsalpinus "the discoverer of the circulation;" it is well to recollect that churchyards have no monopoly of unhistorical inscriptions. Indeed, have we not ourselves, within easy walking distance, that famous monument, the subject of Pope's scathing but just lines—

> "And London's column, soaring to the skies
> Like a tall bully, lifts its head and lies."

Sir, I have no sympathy with Chauvinism of any kind, but, surely, of all kinds that is the worst which obtrudes pitiful national jealousies and rivalries into the realm of science. We will not shame ourselves by permitting the fact of Harvey's English birth to enter into the consideration of his claims as a discoverer; but those claims once established beyond dispute, it is, I hope, something nobler and better than mere national vanity which brings us together to celebrate his birth; to take an honest pride that such a man came of our English race; and as, I hope, to feel the deep responsibility which is laid upon us to have a care that the stock which in the same hundred years bourgeoned out in a Harvey and a Newton, shall not have its capacity for producing like growths in the present and in the future, starved by devotion to mere material interests, or stunted by ignorant outcries against scientific investigation.

The second title which I have claimed for Harvey is that of author of the "Exercitatio de Motu Cordis et Sanguinis." And that title is, happily, quite indisputable. But some may suppose that I have so far thrown myself into the spirit of my assumed office as to insert a superfluous appellation—a sort of "Defender of the Faith." However, this would be an error. Harvey might have discovered the circular course of the blood; he might have given sufficient evidence of his discovery; and yet he might have been quite incapable of writing that little essay of fifty pages which no physiologist of the present day can read without wonder and delight. For, not only is it a typical example of sound scientific method and of concise and clear statement; but, in addition to the evidence of the course of the blood through the body, it contains the first accurate analysis of the motions of the heart; the first clear conception of the mechanism of that organ as a pumping apparatus; the first application of quantitative considerations to a physiological problem; and the first deductive explanation of the phenomena of the pulse and of the uses of the valves of the veins. "Libellus aureus," Haller called it—and never was epithet more aptly bestowed.

Harvey's third title to honour is the authorship of the "Exercitationes de Generatione." In this treatise Harvey grapples with two of the most difficult problems of biology—the physiological problem of generation and the morphological problem of development. It was simply impossible that he should solve these problems, for they can be approached only through the microscope; and Harvey was dead before Hooke, Malpighi, Swammerdam, or Leeuwenhoek, the fathers of microscopy, began their work. He saw the circulation in shrimps "ope perspicillo" indeed—but the perspicillum was a mere handglass. Hence it is not wonderful that Harvey's theory of fecundation is altogether erroneous: and that he is no less mistaken respecting the nature of the parts of the embryo which first make their appearance and the mode of their formation.

Nevertheless, just as it is the fate of dulness to be blind to the significance of justly observed facts, so is it the rare privilege of men of the highest genius to discern the true light among the *ignes fatui* of error. They know the truth, as Falstaff discerned the true prince among his pot companions, by instinct. Explain the matter how we will, it is an indubitable fact that though Harvey's fundamental observations were either inadequate or erroneous, some of his most important general conclusions express the outcome of modern research.

For a whole century Harvey's successors, even though the illustrious Haller was among them, went wrong when Harvey was right; and though Caspar Wolff returned to Harvey's views and thereby laid the foundation of modern embryology, the definitive triumph of the doctrine of epigenesis is the result of labours which have been effected within the memory of living men.

Such appear to me to be the chief claims of Harvey to be held in everlasting honour among men of science. We know that they represent a mere fraction of what he did. But the violence of an unhappy time has robbed us of the rest. I should trespass unwarrantably on your time if I insisted on the applications of Harvey's discoveries to medicine and surgery in the presence of those whose daily avocations bear witness to them.

I have hitherto dwelt upon the claims to our honour of Harvey the philosopher; one word, in conclusion, concerning Harvey the man. There have been great men whose personality one would gladly forget: brilliant capacities besmirched with the stains of inordinate ambition, or vanity, or avarice; or soiled by worse vices; or men of one idea, unable to look beyond the circle of their own pursuits. But no such flaw as any of these defaces the fair fame of William Harvey. The most that tradition has to say against him is, that he was quick of temper and could say a sharp thing on occasion. I do not feel disposed to cast a stone against him on that ground; but rather, such being the case, to marvel at the astonishing, not only self-control, but sweetness, displayed in his two short controversial writings—the letters to Riolan; a man who really was nothing better than a tympanitic philistine, and who would have been all the better for a few sharp incisions.

Moreover, in such a temperament, while the love of appreciation is keen, the sense of wrong at unjust and wilful opposition is no less strong. But I do not recollect, in all Harvey's writings, an allusion to the magnitude of his own achievements or an angry word against his assailants.

Ready to welcome honour if it came, but quite able to be content without it; caring little for anything but liberty to follow in peace his search into the ways of the unfathomable cause of things—"sive Deus, sive Natura Naturans, sive Anima Mundi appelletur"[1]—one fancies this man of the true Stoic stamp would have summed up his eighty years of good and evil in the line of the poet, which was the favourite aphorism of his great contemporary, Descartes—

"Bene qui latuit bene vixit."

But he lived too well that the memory of his life should be allowed to fall into oblivion; and we may hope that recurring centennial anniversaries will find our successors still mindful of the root from whence their ever-widening knowledge has sprung.

After this Mr. Lowe replied in his usual racy style to the toast of the Universities, naturally having a little fling at the aspirations of Owens College and other recent institutions. Mr. Lowe remarked that anything like competition among the persons who conferred degrees and honours must be productive of evil. The result of such a system had been a kind of Dutch auction of degrees and honours, there being in some quarters a desire to secure as many students as possible by lowering the standard of qualification; but he was happy to think that that evil was about to be remedied, and that they were approaching a time when they would obtain what not only the medical profession, but every individual in this country had a right to demand, namely, that no one should be allowed to heave the lead into the depths of his fellow creature's physical constitution without possessing a certain proved degree of skill. That had been the dream of all sound medical reformers for a long time. It had hitherto remained only a dream, but as he had indicated, it was about to be realised, and he was bound to say that, as far as he understood the question, it was about to be so mainly through the noble and disinterested conduct of the universities, who, instead of displaying selfishness, had expressed their readiness to surrender the privilege they now enjoyed of admitting persons to the medical profession, and to hand over this duty to a certain body possessing the power of fixing a standard of qualification below which no person whatever should be admitted to practise.

Mr. Gladstone, in responding to the toast of "General Science and Literature," said—Great as had been their profession in former times, every one must feel that it was growing greater, wider, more solid from year to year and from generation to generation. He did not speak now of literary culture; for although he felt that literature had stood in a very important relation to the medical profession of late years, still literature was necessarily fluctuating, and had been so in all periods of the world. They had gone through a great literary age, as other races had done before, and they could hardly expect the succeeding generation to maintain the same literary level. But as regarded science the case was very different. Nothing here seemed to be required but that patient labour which it was in the power of all men to bestow, together with those large opportunities for observation which we all enjoyed in some degree if we would but use them, and which medical men perhaps enjoyed in a greater degree than any other class of men. As society was developed, as civilisation became more elaborate, as the wants of men, as the enjoyments of men, and as, perhaps, also the dangers of men multiplied, and as the connection of body and mind, which was daily under their eyes, became revealed, they would find their way more and more into the very innermost chambers, so to speak, of human nature. As science progressed their responsibilities would increase, but he was sure they would never be wanting in that capacity and zeal which had ever distinguished them, and that in proportion as their influence over human welfare and human happiness increased, they would obtain that respect and gratitude which, amid their imperfections, mankind were ever ready to extend to their benefactors.

[1] "Exercitationes de Generatione," Ex. 50.

OUR ASTRONOMICAL COLUMN

THE TRANSIT OF MERCURY.—Unfavourable weather appears to have very generally interfered with observations of the first contacts in the transit of May 6, in this country, and in France a similar adverse state of atmospheric conditions also prevailed. At Antwerp, Christiania, Göttingen, Josephstadt (Vienna), Kiel, and San Fernando (Cadiz), the contacts were observed and the results have been mostly published in the *Astronomische Nachrichten*. In two cases only is there any distinction made between what has been called geometrical contact, when Mercury appears perfectly round and his outer limb in coincidence with the sun's limb, and the instant when a fine filament of light is perceptible (or a connecting ligament is broken) which more correctly distinguishes the true internal contact. Thus at Kiel the time was noted when the planet appeared round and when the narrow luminous thread (*deutlicher Lichtfaden*) appeared. But the most complete observations of the first contacts hitherto printed are those made at the Observatory of San Fernando, near Cadiz, which are detailed in a circular issued on May 8, by Señor Cecilio Pujazon, the director of the establishment. Amongst the observers were Señores Garrido and La Flor, who had also experience in the case of the transit in November, 1868, at the same observatory, and with the same or very similar instruments, achromatics by Troughton and Simms of 80 mm. aperture. Three of the observers distinguish between what is termed "first internal contact" and separation of the limbs (*desprendimiento de los limbos*), the mean interval noted between the two phases being 18 seconds.

At Palermo the contacts were noted both with the spectroscope and on the ordinary telescopic method. Prof. Tacchini communicated the particulars to the Paris Academy of Sciences on May 20, at the same time stating that he had been informed of the ill-success attending the observation of the transit at Naples, Florence, Venice, Gallarate (Baron Dembowski's observatory), Genoa, and Modena, on account of unfavourable skies.

In the United States the phenomenon appears to have excited a very unusual degree of interest, occasioned, no doubt, by the instructions for observing it widely-circulated by the authorities of the Naval Observatory, Washington, and the presence in the country of a special expedition composed of French astronomers. Judging from the accounts published in the New York papers on May 7, observations were more or less successful in many astronomical institutions, both the first and last contacts being generally well observed, and numerous photographs obtained during the passage of the planet across the sun's disc. At Ogden, Utah, where the French astronomers were located, the clouds prevented more than imperfect observations of the first contacts; but those at egress were satisfactory. Up to one o'clock only three photographs were obtained, but subsequently as many as seventy-five were secured, and the results, as a whole, were considered satisfactory. At the observatory of Dr. Draper, Hastings, on the Hudson, a number of observers, including Prof. Holden, of Washington, availed themselves of the admirable instrumental resources, and the weather being for the most part advantageous, very good results attended their efforts: of eighteen negatives taken by Dr. Draper several were particularly perfect. In addition to observations at the U.S. Naval Observatory Prof. Newcomb and assistants made satisfactory ones at the office of the *American Ephemeris* in Washington, noting the first internal contact at 10h. 7m. 43s. A.M., according to the *New York Times*, and the second internal contact at 5h. 53m. 50s. P.M.

The following differences between the calculated and observed times of first internal contact have been obtained by comparison with Leverrier's elements, with Newcomb's value of the solar parallax; the Greenwich mean time for the centre of the earth resulting from a calculation of somewhat greater refinement than that previously introduced in this column being 3h. 16m. 12·5s.

Place of Observation.	Observed G.M.T. reduced to earth's centre. h. m. s.	Error of Calculation. s.	
Antwerp	3 15 46·0	+ 26·5	Two observers.
Christiania	— 41·2	+ 31·3	"Apparent internal contact."
,,	— 52·9	+ 19·6	"True internal contact."
Göttingen	— 34·8	+ 37·7	Prof. Klinkerfues.
,,	— 47·7	+ 24·8	Boeddicker and Heidorn.
Josephstadt	— 48·5	+ 24·0	Three observers.
Kiel	— 38·6	+ 33·9	Planet round.
,,	— 53·3	+ 19·2	"Deutlicher Lichtfaden."
Palermo	— 55·9	+ 16·6	Spectroscope.
,,	— 46·1	+ 26·4	Ordinary telescopic method.
San Fernando	— 49·1	+ 23·4	Geometrical contact.
,,	3 16 11·7	+ 0·8	Separation of limbs.
Washington	3 15 58·4	+ 14·1	Prof. Newcomb and assistants.

The Greenwich mean time of second internal contact similarly calculated is 10h. 43m. 57·3s., which, compared with Prof. Newcomb's observations at Washington, shows a difference of + 19·6s. Other observations of the second internal contact given in the New York journals are either provisionally reduced or apparently affected by typographical errors or errors of transmission.

THE ZODIACAL LIGHT AND SUN-SPOT FREQUENCY.—In a letter addressed to Gruithuisen in February, 1839, published by the latter in his *Astronomisches Jahrbuch* for 1840, Olbers remarks, "My grandson, Wilhelm Focke, Doctor of Law, who with attachment and zeal often contemplates and scrutinises the starry heavens, asserts that the zodiacal light has been observed in January and February with quite exceptional brightness;" which, Gruithuisen observes in a note, is "a new confirmation of Cassini's observation that the zodiacal light is much more brilliant when numerous and large sun-spots are present, and diminishes in brightness when the spots are few. My observations show that during January and February the sun has exhibited unusually large and numerous spots," and he adds, "viel Licht und fast immer eine grosse negative Refraction." This refers to Cassini's concluding statement in his memoir entitled "Découverte de la lumière céleste qui parait dans le Zodiaque." "It is a remarkable circumstance that since the end of the year 1688, when this light began to grow fainter, spots have no longer appeared in the sun, while in the preceding years they were very frequent, which seems to support in some manner the conjecture that this light may arise from the same emanations as the spots and *faculæ* of the sun." In a previous part of the memoir Cassini, endeavouring to assign a possible cause for the appearance of the zodiacal light, remarks that the observations of that century had made known that the sun is not only the source of light, but also of "une matière propre à terminer, à détourner, et à réflechir ses rayons;" and that "cette matière ne coule pas toujours de la même manière, mais qu'elle a des vicissitudes sans règle, selon lesquelles nous voyons en certain temps dans son disque des facules, qui sont plus claires que le reste de la surface, et des taches obscures qui ne sont point pénétrées par sa lumière." And he goes on to say that if the matter which is the subject of this light is of the same nature as that which forms the *faculæ* and spots on the sun, it should be liable to the same changes and irregularities. However inadequate or incorrect is the explanation of the spots and faculæ given by Cassini, his conjecture that the brightness of the zodiacal light varies with the number and magnitude of the solar spots is worthy of note, though we do not remember to have seen any allusion to it in our popular astronomical treatises.

THE INTERNATIONAL GEOLOGICAL CONGRESS

THE time of the opening of this Congress in Paris has been finally fixed by the local committee for the 29th August, and the Congress will remain in session about two weeks. Further details as to organization and place of meeting will soon be made public. Meanwhile, it is announced that from the 20th August to the 15th September, the library and reading-rooms of the Geological Society of France, No. 7, rue des Grands-Augustins, Paris, will be at the service of members of the Congress. As before, it is requested that all those who desire to take part therein will make it known to the general secretary, Dr. Ed. Jannetaz, at the above address, where, also, the subscription of twelve francs, required for each member, may be sent to Dr. Bioche, treasurer. Ladies are admitted to the Congress.

The local committee add to the above announcements:—There is reason to believe that the numerous collections of geology and palæontology, minerals, rocks, fossils, maps, sections, plans, models in relief, &c., to be found in the *Exposition Universelle*, will realise the expectations expressed in the circular of the International Committee, of an International Geological Exhibition. All exhibitors of such collections are requested to send, as above, such lists as will enable the secretary-general, Dr. Jannetaz, to prepare a special catalogue of them for the use of the Congress.

T. STERRY HUNT,
Secretary of the International Committee

A KINEMATICAL THEOREM

TAKE a plane, and, for clearness of idea, consider it as fixed horizontally. On this fixed plane lay another, and throughout the subsequent movement let the surfaces of the two planes always remain in contact. Now let the upper plane, starting from any position, be moved about in any manner whatever, making any number (N) of rotations, the points on it describing curves of any desired degree of complexity on the lower plane; and let it finally settle down again into its initial position, the curves described by the points on it being, in consequence, closed curves. Take the upper plane, and let us investigate the position on it of those points which have described curves of any given area (A) on the fixed plane.

However complex the curves described by them may be, the points will be found to form a circle on the upper plane; and if we give to A different values, the corresponding circles will be found to be all concentric. Further, if we call the circle corresponding to the value $A = 0$ the *zero-circle*, the area of the curves described by the points on any other circle of the system equals N times the ring inclosed between that circle and the zero-circle. It is remarkable that such a singular point as the centre of the circles should exist.

In the special case in which $N=0$, *i.e.*, where there has been only an oscillatory movement of the upper plane and no complete rotation, the system of concentric circles is replaced by a system of parallel straight lines, the area of the curves described by the points on any straight line of the system being proportional to the distance of that line from the zero-line.

It should, perhaps, be pointed out that the area of a figure 8 is zero, as the two halves are of opposite signs; also that when a point reciprocates on a curve the area inclosed by it in its path is zero. For example: if we take the interesting case of a circle rolling inside another of twice its diameter, every point on its circumference reciprocates on a straight line, and consequently the circumference is the zero-circle.

This theorem was suggested to me by reading a paper by Mr. C. Leudesdorf in the *Messenger of Mathematics*, where I have already enunciated it. It seems, however, to be one which, from its somewhat startling simplicity, may interest a larger class of readers than a purely mathematical one.

The proof is simple. Let P, P' be two points on the moving plane, and let A, A' be the areas described by them. Let $P P' = r$, and let the total movement of P' perpendicular to $P P' = n$.

$$\text{Then } A - A' = n r + N \pi r^2.$$

If we take P' as origin and the position of $P' P$ in which n is a maximum and equal to n' as initial line, $n' = n \cos \theta$. Thus $A - A' = n' \cos \theta \cdot r + N \pi r^2$, the equation to a family of concentric circles. Transforming to the centre, we have

$$A = N\pi (r^2 - a^2),$$

where a is the radius of the zero-circle.

A. B. KEMPE

OLD MAPS OF AFRICA

MR. STANLEY, in the paper which he read at the Geographical Society on Monday, spoke of Africa being brought to light after an oblivion of 6,000 years. Notwithstanding the somewhat confused phraseology, Mr. Stanley's meaning is clear enough: Central Africa, with its great lakes and rivers, is now known, he means to say, for the first time. But recent investigation seems to show that the oblivion of Africa must be counted by hundreds and not thousands of years; that, in fact, it is only within two or three centuries that a knowledge of Central Africa has been allowed to lapse. A more rigorous search may show that between the fourteenth and the seventeenth centuries the great features that have been placed on modern maps within the past few years were discovered and recorded on the maps of the time.

We have recently referred, on more than one occasion, to two very curious globes that have been brought to light, one in the National Library in Paris, and the other in the Library of Lyons. On the Lyons globe, the date of which is 1701, the Congo is made to issue from a great lake, and wind its way westwards to the Atlantic, in a direction to some extent coincident with that recently discovered by Mr. Stanley. As a sort of preparation for the work of the great traveller, so soon to be issued, some account of the *data* on which these maps may have been constructed, may not be uninteresting. Our information is based on an article in *La Nature*, and on a report by a commission of the Lyons Geographical Society, appointed to investigate the value and origin of the Lyons globe.

The discovery made at Lyons is, in reality, no surprise to those who know the history of geographical exploration. Not only in the seventeenth century, does the Zaire-Congo appear on most of the maps with the direction definitely assigned to it by Stanley, but nearly all old documents, from the fifteenth century—and the date should be noted—make the great river issue from a considerable mass of water far in the interior of the African continent.

Already, in the year 1500, the famous *mappemonde* of Juan de la Cosa, the pilot of Christopher Columbus, gives the same indications; the picturesque *mappemonde* known as that of Henry II., repeats them with some variations, as also the master-work of Mercator (1569), the founder of modern geography. All the old geographers, or nearly all, repeat the same data:—Forlani (1562), Castaldi (1564), Sanuto (1588), Hondius (1607), Nicolas Picart (1644), Blœu (1569), Sanson, &c. Therefore there need be no surprise to find on a globe of the eighteenth century information which for more than 200 years previously had been registered on the map of Africa.

Whence, however, came this knowledge which our fathers had of certain regions in Central and Equatorial Africa? The reply is simple: from the Portuguese, who, since the fifteenth century, undertook not only extensive maritime voyages, but several times crossed Africa from west to east and from east to west. It is even very possible that they discovered the sources of the Nile, the great equatorial lakes; thus, in the midst of the simplicity and incoherence of their tracings we find, in their old parchments, the great lines of African geography almost as science now represents them. Most of these Portuguese, with the exception of some missionaries, were but poorly educated; they travelled much oftener as traders than as experienced explorers; nevertheless, we have almost the certainty that before the year 1500 they had furnished very precise information on the centre of Africa. In nearly all these maps, and in that of Lyons, the Congo flows in a nearly straight line from Lake Zaire or Zembre to the Atlantic; it bends only a very little to the north, and does not pass the equator, as we now know it does.

As a sort of exception, there has been found among the riches of the National Library at Paris, a Spanish globe of copper (without date, but probably between 1530 and 1540), which is not content with presenting the same data, but which reproduces, with wonderful closeness, the course of the Congo as discovered by Stanley. The river issues from a lake, flows towards the north, describes a large curve well to the north of the equator, then turns west-south-west to the Atlantic. This is indeed a summary of the last journey of the intrepid American correspondent. Fig. 1 gives a perfectly accurate idea of a portion of this valuable globe.

From all this it must not be concluded that Stanley has discovered nothing new. These discoveries of the ancient travellers, if genuine discoveries they were, seem to have ben forgotten as soon as they were recorded; and although the maps referred to above have been known for generations, no one ever seems to have taken them as trustworthy guides to the lines of African exploration. Indeed, it is only now that Stanley has made a discovery never to be forgotten that these old maps have come to have a real interest, for we suspect that till now geographers regarded the tracings as having their basis in the cartographers' imaginations. The glory of being really the first discoverers of the two Nyanzas, Nyassa, Tanganyika, Bangweolo, and the course of the Congo cannot be taken away from Speke and Baker and Burton and Livingstone and Stanley; or if so it must be by some ancient Arab or possibly Egyptian, many, many centuries ago, for there can be no doubt that long before Europe awakened to modern geographical enterprise, these great features of Central Africa were known; Herodotus had an inkling of them, and Ptolemy all but located the central lakes. These modern explorers deserve the glory of first discoverers as much as Columbus deserves that of discoverer of America.

Without then detracting from the originality of the work of modern explorers, it is evident that from the fifteenth century onwards some travellers whose names have fallen into oblivion but who may have been companions of Diego Cam and Martin Behaim, ventured into the heart of Africa; followed certain arteries of communication and discovered the course of the Congo; geography kept possession of these discoveries for two centuries and gave them as articles of faith; besides, in the sixteenth and seventeenth centuries many Portuguese, Capuchins or simple traders, entered anew the interior, published the same facts, sometimes with corrections and additions.

Father Ricioli, a Jesuit and very intelligent man, furnished the Fathers Placide, of St. Amien, and Crespinien, two laborious monks, with documents to prepare the Lyons globe, in 1701. The actual constructor of the globe seems to have been the celebrated Lyons mechanician, Henri Marchand, in religion Père Gregoire, a Franciscan, with the help of the Venetian Contarini, a pupil of Nolin, belonging to the Flemish cartographic system. Evidently this was the last word of science. Fig. 3 is a much reduced copy of the facsimile made from the globe by M. Deloncle, the reporter of the commission we have alluded to.

How came it that just about the same time, about 1700, one of the princes of modern geography, Guillaume Delisle, was so badly inspired as to reconstruct an altogether New Africa in which he accumulated heresy on heresy? The Central lakes, the immense reservoirs of the Nile, disappear at one stroke of the pen; as to the Congo it is no longer connected with the lakes of the interior, although it is allowed to retain a little of its semicircular direction. The error accredited by a celebrated geographer like Delisle made way. The old map of Africa was demolished stone by stone, so to speak. In short the work was so well done that, after having piled nonsense upon nonsense, for the sake of peace, all was expunged; after having believed in tribes with dog-heads, placed a few anthropophagi everywhere, and confounded countries situated a thousand miles from each other, they ended by making a *tabula rasa* and leaving a white space where formerly were rightly placed the great lakes and sources of the Nile. Yet a few years and here was geography doubting, denying, and ridiculing the follies of our predecessors. The students of geography of the period of 1840-50 were too much on their guard to commit the colossal blunder at that period of making the Nile issue from the lakes to the south of the equator. "So far as concerns a part of Africa," to quote M. R. Cortambert, in the article in *La Nature*, "the past has been resuscitated: 'old things have become new.' That which was laughed at yesterday is taken seriously to-day. Then, my friends, these good ancestors of the fifteenth and sixteenth centuries, who counted among them Columbus, Gama, Magellan, and many other conquerors of the world, have not, perhaps, left altogether to their descendants of the nineteenth century the glory of inventing geography."

From the report of the Lyons Commission we learn that the following works were probably accessible to the Flemish map-makers, and later to the constructors of the

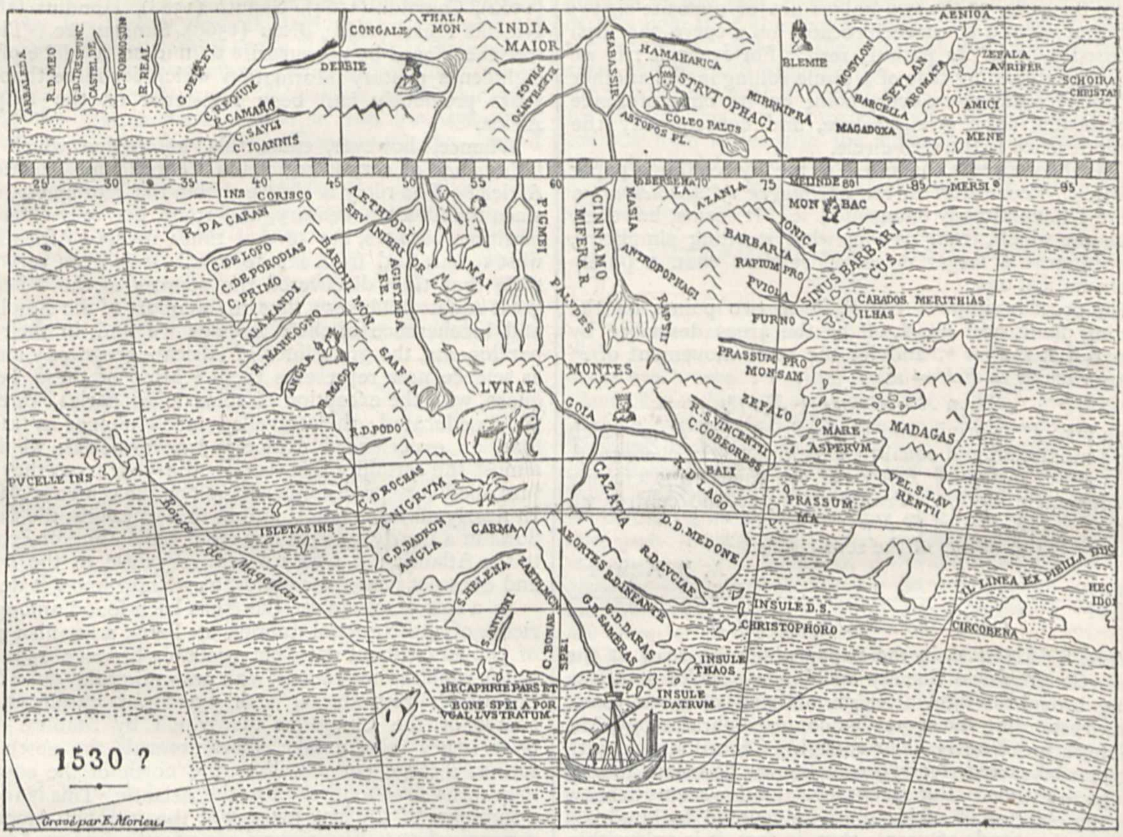


FIG. 1.—Portion of a Spanish Globe of 1530-40, found in the National Library, Paris.

Lyons globe:—1. The Geography of Ptolemy; 2. The "Portuguese Asia" of De Barros (1552); 3. The "Description of the Congo," by Pigafetta, according to Lopez (1592); 4. The "Historical Description of Ethiopia" of Dom Francesco Alvarez" (1558); 5. The "Africa" of Leo Africanus (1556); 6. And the old maps and portulans.

Among these old maps and portulans, those which appear at this period to have had a certain influence are:—1. The Medicean Portulan (1351); 2. The Catalan Atlas (1375); 3. The Map of Mecia de Viledestes (1413); 4. The Map of Johannes Leardus (1448); 5. The *Mappemonde* of Fra Mauro; 6. The Ambrosean Map (1480); 7. The *Mappemonde* of Juan de la Cosa (1500); 8. The Map of Diego Ribera; 9. The Spanish *Mappemonde* of the National Library, Paris (Fig. 3) (1540); 10. The Maps of Ramusio, of Pigafetta, and of Hugues Linschoten.

In the detailed report which the Lyons Commission will communicate to the Society will be shown to what extent each of the above documents contributed to the establishment of the Flemish maps, on which probably the Lyons globe was more immediately based. The same Report will contain an investigation into the travels known or unpublished which, from the tenth century, have contributed to the progress of the African geography of the Middle Ages and of the Renaissance. This investigation will include the following:—1. The Arab voyages and Compendia; 2. The voyages of the mendicant Spanish friar of the fourteenth century; 3. The expedition of eight dominicans of Montpellier to the sources of the Nile (1317-1350), unpublished; 4. The travels of

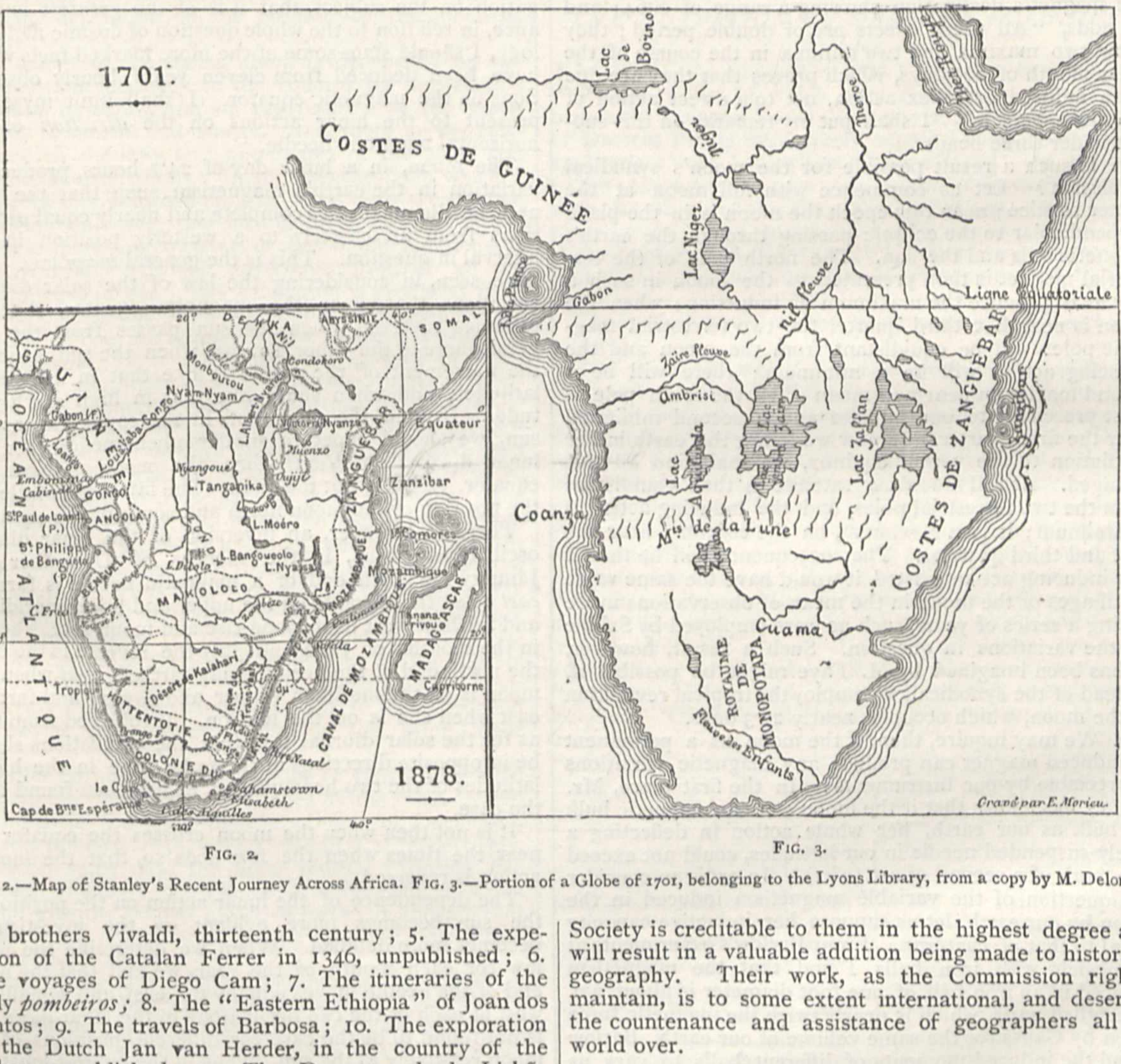


FIG. 2. FIG. 3.

FIG. 2.—Map of Stanley's Recent Journey Across Africa. FIG. 3.—Portion of a Globe of 1701, belonging to the Lyons Library, from a copy by M. Deloncle.

the brothers Vivaldi, thirteenth century; 5. The expedition of the Catalan Ferrer in 1346, unpublished; 6. The voyages of Diego Cam; 7. The itineraries of the early *pombeiros;* 8. The "Eastern Ethiopia" of Joan dos Santos; 9. The travels of Barbosa; 10. The exploration of the Dutch Jan van Herder, in the country of the Akkas, unpublished; 11. The Derrotero desde Lisbõa Al Cabo de Bueno Esperanza y India Oriental, anonymous and unpublished; 12. The description of the Congo, by Martinus Abarca de Boléa et Castro (1601), unpublished; 13. The "Universal Book of the Navigations of the World" (1590?), Spanish, unpublished; 14; The Travels of the Belgian Pierre Fardé from Algiers to the Congo (1686), unpublished; 15. The "Travels of Manoël Godinho" (1663); 16. The Letters of Father Mariano, the Jesuit, on Kaffraria, &c., &c.

The work undertaken by the Lyons Geographical Society is creditable to them in the highest degree and will result in a valuable addition being made to historical geography. Their work, as the Commission rightly maintain, is to some extent international, and deserves the countenance and assistance of geographers all the world over.

COSMIC METEOROLOGY [1]

II.

INFLUENCE of the Moon on the Earth's Magnetism.—There is a fact in connection with the moon's influence on our earth for which an explanation is necessary, and M. Faye has proposed for this end a hypothesis in advance. He had already pointed out Dr. Lloyd's investigation which showed that the diurnal magnetic variations could

[1] Continued from p. 128.

not be explained by the hypothesis that the sun acts as a magnet. But, it is said, "May the moon not acquire induced magnetism under the action of the earth, perpetually variable according to the relative position of the two bodies? If we consider the enormous magnetic power of the earth, that Gauss finds equal to that of 464 trillions[1] of magnets weighing a pound each, and if we remark besides that the distance of the moon to the earth does not exceed thirty times the length of this gigantic magnet, we may give an affirmative answer to the question proposed. But then the magnetism induced in the moon should in its turn exercise a small action upon the proper magnetism of the earth in the period of a lunar month. The observations alone can decide this provided they are of great precision."

M. Faye then cites the results obtained from the Toronto observations by Gen. Sir E. Sabine, that for the magnetic declination showing a range of 0'·64; and he adds, "All these effects are of double period; they show two maxima and two minima in the course of the lunar month of 29½ days, which proves that they are due to an induced or reflex action, not to a direct action of the moon herself." I shall put my remarks on this subject under three heads.

1. Is such a result possible for the moon's synodical revolution? Let us commence with full moon at the winter solstice; near this epoch the moon is in the plane perpendicular to the ecliptic passing through the earth's magnetic axis and the sun. The north pole of the terrestrial magnet is then presented to the moon in such a way as to produce the maximum of induction; when the moon is near her third quarter the two terrestrial magnetic poles will be equidistant from the moon and the inducing action will be a minimum; there will be a second maximum near new moon when the south pole is most presented to our satellite and a second minimum near the first quarter. If now we follow the earth in her revolution to the vernal equinox, we shall find all this changed. At full moon our satellite is then equidistant from the two terrestrial poles, and the inducing action is a minimum; it is a maximum, on the contrary, near the first and third quarters. The consequence will be that if any inducing action existed it would have the same value at all ages of the moon in the mean of observations made during a series of years, such as were employed by Sabine for the variations in question. Such a result, however, as has been imagined by M. Faye might be possible if, instead of the synodical, we employ the tropical revolution of the moon, which occupies nearly 27·3 days.

2. We may inquire, then, if the moon as a permanent or induced magnet can produce any magnetic variations appreciable by our instruments? In the first place, Mr. Stoney has shown that if the moon were as magnetic bulk for bulk as our earth, her whole action in deflecting a freely-suspended needle in our latitudes, could not exceed one-tenth of a second of arc (0''·1).[2] In order to consider the question of the variable magnetism induced in the moon by our earth, let us suppose her inductive capacity equal to that of cast-iron. From Barlow's experiments at Woolwich with iron balls I find that the magnetism induced in an iron ball of one foot diameter is about 2·0, in English units, which is nearly twice the magnetic force given by Gauss for the same volume of our earth. Barlow found the induced moments of different balls to vary as their volumes, and assuming that the induced magnetism varies inversely as the cube of the distance of the inducing and induced bodies, we find at the moon's distance (60 terrestrial radii) the induced magnetism at the maximum, under the most favourable condition, could not be more than $\frac{2}{60^3} = \frac{1}{108,000}$ of that supposed in the first case, that is when as magnetic as the earth. Her whole action on a magnetic needle here, then, due to the earth's induction, could not exceed one millionth of a second of arc. It is advantageous to get rid of hypotheses which are so completely insufficient, and we may put aside for the future any consideration of the moon's action by her own permanent magnetism, or by a variable magnetism induced in her by the earth.

3. M. Faye has also misunderstood the facts which he wished to explain. The results obtained by Sabine have reference to a variation which occurs in 24¾ *hours*, the lunar *day*, and not the lunar month of 29½ days. The laws of the lunar diurnal variations were obtained first by Kreil for the magnetic declination, and by myself for the magnetic force and inclination. This action of the moon is, however, so very different from what is generally supposed, and from what was concluded from the first investigation on the subject, that it is of the greatest importance, in relation to the whole question of cosmic meteorology, I should state some of the more marked facts which have been deduced from eleven years' hourly observations on the magnetic equator. I shall limit myself at present to the lunar actions on the *direction* of the horizontal magnetic needle.

The moon, in a lunar day of 24·7 hours, produces a variation in the earth's magnetism, such that the magnetic needle makes *two* complete and nearly equal oscillations from an easterly to a westerly position in the interval in question. This is the general *mean* law. We have seen, in considering the law of the solar diurnal variations that, near the magnetic equator, the law becomes reversed when the sun passes from the one hemisphere to the other, so that when the sun is north, the movement of the needle is like that in high north latitudes, and when south, like that in high south latitudes. If, then, the moon acts in the same way as the sun, we should expect a similar phenomenon for the lunar diurnal variation when the moon crosses the equator. This is not the fact. The law differs little for the position of the moon north and south of the equator.

There *is*, however, an inversion of the lunar diurnal oscillations; thus, in the months of December and January the north end of a magnetic needle is farthest *east* when the moon is on the upper and lower meridians, and farthest west near moon-rise and moon-set; whereas in the months of June and July the reverse is the case, the north end of the needle being farthest *west* when the moon is on the meridian (upper and lower) and farthest east when she is on the horizon. It followed from this, as for the solar diurnal law, that the oscillations should be in opposite directions at the same time in the higher latitudes of the two hemispheres, as has been found to be the case.

It is not then when the moon crosses the equator but near the times when the *sun* does so, that the moon's action is reversed.

The dependence of the lunar action on the position of the sun becomes more evident as the investigation becomes more detailed. When we determine the mean law for each month of the year, we find that the north end of the needle moves equally far east and equally far west at each of the two oscillations in the lunar day; this is not found to be the case for different positions of the moon relatively to the sun. Thus in the quarter lunations including full moon, in the months of December and January, the greatest *west-east-west* oscillation of the needle occurs when the moon is on the *lower* meridian; not when the moon, but when the *sun*, is shining on the place of the needle. The oscillation from moon-rise to moon-set, that is to say, while the moon is above the horizon, is little more than one-third of the oscillation for the half day when she is below the horizon; the two westerly extreme positions when the moon is on the horizon are nearly the same.

[1] M. Faye uses the word trillions, but the trillions are English, not French, the latter being a very different number.
[2] *Phil. Mag.*, vol. xxii. p. 294.

Similar results are obtained for the other quarter lunations. In all cases that oscillation is the greatest of the two for which the sun is above the horizon, whether the moon be above it or not.

There are still some remarkable facts connected with this variation at the magnetic equator. Limiting our examination of them always to December and January, we find, if we determine the oscillations due to the moon for the day when she is in conjunction and for each of the six following days, that in the first three days of the seven the oscillation is *west-east-west* during *the day*, that is, from sunrise to sunset; and in the last three days it is *east-west-east*. In the middle day of the seven the lunar action is almost null; the oscillation of the needle is very small, as we might expect, since on that day the change at sunrise from a *west-east* to an *east-west* motion takes place. The lunar hours of the maximum and minimum extremes thus oscillate about two hours on each side of the mean, depending on the position of the moon at *sunrise*.

The action of the moon, then, is dependent on the sun's position relatively to the equator (or the earth's position in its orbit), and on the position of the moon relatively to sunrise and sunset. But there is no relation between the laws and amplitudes of the solar and lunar diurnal oscillations. In the months from which I have taken my illustrations, the solar diurnal variation is a single oscillation: that for the moon, however taken, for single days, for quarter or for whole lunations, is always double. Through the combination of all the varying modes in which this oscillation is produced from day to day, *the mean for a lunation* is a regular double oscillation. The amplitude of this mean oscillation is three times as great in January as in June or July; whereas the amplitude of the mean solar diurnal variation is a half greater in June or July than in January.

I shall add another fact, one of the greatest importance in connection with this subject. We have seen that the lunar diurnal variation changes in the relative *amplitudes* of the two oscillations from day to day; the consequence of this is that when the means for a whole lunation, or even a quarter lunation, are taken, the mean amplitude is much less than that which is shown by each day separately. Thus I have found that the range of the mean lunar diurnal oscillation for the lunation December 16, 1858, to January 15, 1859, at Trevandrum, was 1′′25, while the ranges of the mean oscillations for the quarter lunations varied from 1′′70 to 2′′70, these quarter lunations giving exactly the same laws as have been deduced from eleven years observations for the same lunar epochs.

In order to understand the value of these results we must compare them with the ranges of the solar diurnal oscillations for the same months; those for December, 1858, and January, 1859, were 2′′20 and 2′′24 respectively. And as on some days the lunar diurnal variation has amounted to nearly 5′′0 (which is equivalent to 12′ in England with the smaller directive force), it appears that the lunar action is sometimes greater than the solar action at the magnetic equator.

As long as the lunar diurnal action was considered to be of the minute character first discovered, it was always possible for the supporters of the heat thesis to suspect that some small unknown heat action was in question. Such an idea is no longer possible. The lunar is sometimes greater than the solar diurnal action; and the former is dependent for its magnitude on the light and heat vibrations *due to the sun* shining on the place of the magnetic needle.[1]

If the solar light and heat vibrations can increase the magnetic action, there can be no difficulty in believing that these vibrations may in their turn suffer some modification of intensity. It would be difficult to measure small variations of the sun's light with sufficient accuracy as yet, though Mr. Willoughby Smith has suggested a selenium photometer for this end; we can, however, measure the variations of temperature, and the fact that the direct heating action of the moon is inappreciable is no longer sufficient to disprove the results of Mädler, Kreil, Park Harrison, and Balfour Stewart. We have in fact a mode of lunar action with which M. Faye was unacquainted and could not take into account. The whole basis of his argument is therefore destroyed.

The view now given opens up a wide field of inquiry, and cosmic meteorology appears under another aspect. I hope to be able at another time to present other facts which seem to relate to magnetical and meteorological phenomena.

JOHN ALLAN BROUN

[1] Mr. Willoughby Smith's experiments show that the light vibrations of the ether in selenium diminish in a very marked manner the electrical resistance of the crystal; and it does not seem improbable that the increase of the lunar magnetic oscillation in sunlight may be due to some similar action.

THE NUTRITION OF DROSERA ROTUNDIFOLIA

DURING the summer of 1877 I began an experiment, the results of which were given in a paper read before the Linnean Society, January 17, 1878. A number of Drosera Plants were freely supplied with meat, while another set were kept without animal food. At the end of the season the two sets were compared in various ways with the object of deciding whether or not carnivorous plants profit by an animal diet. In the abstract of my paper published in NATURE (vol. xvii. p. 222), it may be seen how numerous were the advantages gained by the fed plants.

The further results of the experiment are not without interest.

The plants on which I worked were cultivated in six soup-plates, and after all the flower stems had been cut, the plants in three of the plates were removed from the moss in which they grew, and were counted and weighed. The plants in the other plates were left undisturbed with the object of comparing the new plants which should spring up from the winter buds of the two sets in the following year. They were removed to the hothouse in the course of the autumn, in order that they might rapidly send up the next year's leaves. By the middle of January, 1878, it became quite clear that far more leaves were springing up from the winter buds of the plants which had been fed than from the others. Both sets of plants were now kept without food, and on April 3 they were removed from the plates, and carefully counted, dried, and weighed. The following numbers give the result of the examination:—

	Actual numbers and weights.		Proportion between two first columns.	
	Not fed.	Fed.	Not fed.	Fed.
Number of plants	89	105	100	118 0
Total weight	grams. ·206	grams. ·518	100	251·6
Average weight per plant	·0023	·0049	100	213·0

It will be seen that there is only comparatively a small difference (18 per cent.) between the number of not-fed and fed plants. Numerous minute offsets were found among both sets, and were all counted as separate plants. But, judging either by the total or average weights, no doubt can be entertained of the great advantage gained by the fed plants. It is a striking fact that, in spite of the far larger yield of flower stalks, seeds, &c., produced during the previous summer by the fed plants, they were nevertheless enabled to lay by a far greater store of reserve-material than their not-fed competitors.

It is a curious coincidence that while I was at work on Drosera, an almost identical research was being conducted in Germany. The experiment of Drs. Kellermann and von Raumer were described before the Phys. Med. Society of Erlangen in July, 1877, and the final results were communicated by Rees, of Erlangen, to the *Botanische Zeitung*, April 5, 1878.

The research was evidently conducted with extreme care, and it is very satisfactory to me to find that my results agree (speaking generally) with those of Kellermann and von Raumer. The plants used in their experiments were fed on aphides, and do not seem to have thriven quite so heartily as mine did on a meat diet. This appears from the following figures :—

	Kellermann and von Raumer's results.	Mine.
Number of flower stems ...	100 : 152	100 : 165
Number of capsules	100 : 174	100 : 194
Weight of seeds	100 : 205	100 : 380

In testing the relative powers of the fed and not fed plants in laying by reserve-material in the winter buds, the Erlangen observers adopted a more accurate method than mine, namely, that of weighing the winter buds, instead of waiting until the new leaves had grown. They found that the weights of winter buds for the fed and not fed plants were as 173 : 100. FRANCIS DARWIN

PHYSICAL SCIENCE FOR ARTISTS[1]

V.

THE simple and forcible language employed by Prof. Stokes in the extract I gave in my last paper, should have made it quite clear that in nine cases out of ten, when bodies reflect light to us, they have really absorbed a part of it in the process, and that to this absorption of light bodies by their colours are chiefly to be ascribed.

Those bodies which give back to us light in the middle of the spectrum—light, in other words, containing green and yellow—are those which are most liable to change with different intensities of light. I shall endeavour, if I have space, to return to this point in the sequel, but I feel that my first duty, now that the phenomena of absorption have, I trust, been clearly explained, is to pass on to the application of this knowledge to the various colours of the sky.

Having, then, this idea of absorption, a very important consideration comes in : the absorption of a substance generally increases with its thickness, and when we deal with those substances that for a given thickness absorb either the red or the blue, we often find that when the thickness is considerably increased the absorption spreads over the whole spectrum from the blue or the red end respectively. This can be shown graphically as follows :—

Blue Absorption.

V I B G Y O R } No absorption.
V I B G Y O R
V I B G Y O R
V I B G Y O R
V I B G Y O R
V I B G Y O R
V I B G Y O R

} Effect of increasing thicknesses.

[1] Continued from p. 126.

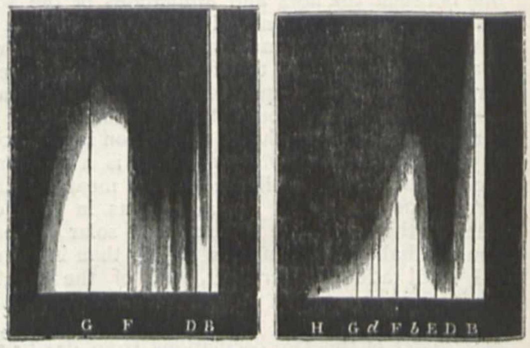
Here, then, we have an absorption beginning at the blue end and gradually closing everything except the red. I may remark *en passant* that we here have the physics of sunrise and sunset colours ; similarly we might begin at the red end and then we should get

V I B G Y O **R**
V I B G Y **O R**
V I B G **Y O R**
V I B **G Y O R**, and so on.

These effects may be experimentally observed by either using different thicknesses of the absorbing materials or by putting them into a V-shaped vessel, and observing the change which takes place when the light passes through the greatest and least thicknesses of the absorbing material. It is of importance also for the artist to observe the effect of the residual light independently of the spectral phenomena. For instance : if we take a chlorine tube of such a length that it begins to cut off the

Greatest thickness.

Least thickness.



Potassium permanganate. Chromic chloride.

FIG. 1.—Showing phenomena of absorption produced by great and small thicknesses of the same substance in a wedge-shaped cell.

blue the residual light will be a delicate green ; a tube twice the length will give us a colour in which the rich golden yellow predominates.

Although we have been compelled to leave out several steps in the argument, we are in a position now to approach the cause of the various colours of the sky.

Let us assume that our complex atmosphere—complex because it consists of a mixture of two pure gases and aqueous vapour—absorbs the light which passes through it, and that the absorptive effect depends upon the thickness of the atmosphere through which the light has to pass before it reaches the eye.

Now there are many grounds for supposing that the

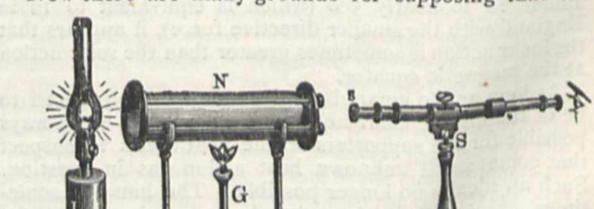


FIG. 2.—Arrangement for observing the absorption of a great thickness of gas or liquid. L, lamp; N, tube; S, spectroscope; G, Bunsen burner when used.

general absorptive effect both of the pure gases and of the aqueous vapour is of the blue kind: that is to say, that the smallest thickness which has any visible effect will absorb in the blue. We also know that the absorptive effect of aqueous vapour is enormously greater than that of the pure gases.

I feel bound to show at once that this is no scientific abstraction, and it would be impossible to find two better examples to show exactly what I mean than those afforded by two of the pictures which I have chosen as texts—Mr. Vicat Cole's "Rosenlaui" and Mr. Peter Graham's "Wandering Shadows." Whether I am right or wrong about the molecular states of aqueous vapour, there is no doubt that the quantity of it varies considerably. The clouds in Mr. Graham's picture show us that the air is charged with it, for the simple reason that if it were not there would be no clouds to cast the shadows which he has so exquisitely caught. Look now at the dark hill in the distance; see how blue the air is between us and it—for it is true that it is the colour of the air, or rather of the aqueous vapour in the air, as Leonardo da Vinci first discovered, and not the colour of the hill which Mr. Graham here paints. We are in presence of aqueous vapour competent to be set in vibration by blue light, and because it vibrates in this way it appears blue.

What would have happened if the dark hill had not been there? If the stratum of aqueous vapour had had a background of bright sky, it would have absorbed the blue light of that sky. By virtue of the principles which I have stated, the sky would have appeared red in consequence of the abstraction of blue light. This, by the way.

Turn now to Mr. Vicat Cole's picture, and see the work of the vapour upon each receding buttress of rock on the left of the valley; the depth of atmosphere is rendered to perfection, but we do not get the blue that Mr. Graham gave us, for the reason that there is less aqueous vapour mixed up with the air.

Many an artist, I am sure, has noticed that at times there appears to be no atmosphere at all; all sense of distance is lost; buttresses such as those painted by Mr. Vicat Cole, although obviously, as may be gathered from the structure of the mountain, at different distances from the eye, seem yet to lie in the same vertical plane.

I saw this effect myself in its very strongest form last year at Cadenabbia. Looking eastwards from the hotel there, over the lake of Como, one sees Belaggio, the hills between Bellano and Lecco on the other side of what is called the Lecco leg of Como forming a magnificent background; these hills recede from the eye in a magnificent series of buttresses. Although some of these buttresses were three or four miles on the other side of Bellaggio, it was impossible to get rid of the feeling that lake, Bellaggio, background to the furthest buttress, was a painted canvas between us and the water. I called the attention of several friends to this wonderful sight; they saw it exactly as I did. The explanation is quite simple: although the permanent gases of the air were there, the aqueous vapour was not, at all events, in that form which by its action on light gives us what artists call atmosphere in a picture. To me this afforded the strongest possible proof of the statement I have already made that the absorption of the permanent gases of the air goes for nothing so far as art is concerned.

As I have already hinted, the molecular form of aqueous vapour with which we have most to do is one, the motions of which lie chiefly at the blue end of the spectrum; a small thickness of it cuts off the extreme blue, and as the thickness increases even the green may be dimmed by it.

In order to show how on such a point as this, art, representing an accurate study of natural phenomena, may help science, I will here give the result of some observations which my friend Dr. Schuster was good enough to make at my suggestion in the Himalayas and Tibet, with a view to test this very question.

Theory had led me to expect that with the enormous thickness of air available there, absorption at the red end of the spectrum by aqueous vapour would be seen as well as the absorption at the blue, which is so common with us. Seeing the sun a vivid green through the steam of the little paddle-boat on Windermere first led me to inquire into the possibility of aqueous vapour following the same law as that which I think we may now accept in the cases of the vapours of metals. As in these experiments with vapours, absorption of the red end alone was seen, as well as absorption at the blue end alone, the assumption that these two absorptions existed in aqueous vapour at once accounted for the green sun, which, I may remark *en passant*, I caught again last year through a thin veil of mist at the extreme summit of the pass of the Simplon.

Here, then, are Dr. Schuster's observations made at Simla when the rainy season had just begun:—

June 27, 8 A.M.—B (one of the Fraunhofer lines at the red end of the spectrum), beautifully shaded. Light visible in the blue as far as 4040; most likely further; but the telescope cannot be moved to greater deviation.

9 A.M.—Space beyond B closes up, while in the blue the spectrum is visible, as before.

11.15 A.M.—The red closed up still more; the blue as clear as before.

6.30 P.M.—Sun very near horizon; spectrum seen from C to G. (This means that both ends of the spectrum are now absorbed.)

Dr. Schuster further states that he was at the same time struck by the fact that the peculiar redness of the clouds in the evening, which we observe so often in our climates, was only rarely seen, and, when seen, that the colour was rather yellow than red. He adds, "On making this remark to a friend competent to judge, and who, through a long sojourn in Simla, was enabled to form an opinion, I heard that the redness of the sky at sunset was often beautifully seen at the end of and after the rainy season."

So much for the observations at Simla. I now pass on to some observations made in Upper Tibet, where there is no rainy season. I give them in Dr. Schuster's own words:—"The observations all point to the remarkable clearness in the blue. As I have said, the hygroscopic state of atmosphere, as measured by the wet and dry bulb or barometric pressure, cannot alone account for all the phenomena. I find, for instance, that the presence of vegetation affects the atmospheric absorption in a remarkable degree. In the Kyan Chu plain, for instance, the plateau on which I observed the mirage described in NATURE, vol. xiii. p. 67, objects at ten miles distance look as sharp and distinct as those half a mile off; it is, in fact, impossible to judge of distance. Crossing the Tagalung Pass (18,000 feet), we descended from that plain into the Valley of the Indus. As soon as we reached vegetation, at a distance of only two marches from the above-mentioned plain, and at a height still above 12,000 feet, the whole aspect of the country is a different one. Distant mountains now take the lofty blue colour which gives such peculiar charm to the landscape. In the evenings, especially, you cannot help knowing that there is something between your eye and a distant object, which affects its colour and distinctness, and through it you get a standard for judging distances. Without vegetation, even at a lower height, as, for instance, in the Valley of the Bagha (Lahoul), you seem to look through a vacuum. In the upper part of the Valley of the Indus, of which I am now speaking, I have not seen that clearness in the atmosphere which I have invariably seen in Switzerland at a height of 3,000 feet. The strong radiating power of the sun, which stands much more vertical in India, is evidently the cause of this, for it can only be organic matter floating in the atmosphere which can produce such a striking result; that the absence of any rain or deposit of any kind must not be left out of account is clear. The air in the side valleys of Cashmere, although rich in vegetation, is particularly transparent. Strange enough, the principal valley of Cashmere, *i.e.*, the valley of the Jehlum, is generally hazy, although there is a good deal of rain. I have seen the planet Mars look almost white; Jupiter and the other stars at that time had a bluish tint."

I have been anxious to give these extracts not only because they form a valuable contribution to science, but because we see here the student of science doing what an artist is generally supposed to do, namely, interesting himself in the colouring of natural objects, and I cannot omit pointing this remark with the statement of my belief that when the artist attacks these also from the scientific point of view as well as the artistic one, his eye will lose nothing of its keenness, and his interest in the glory of nature will be nothing the less.

Let us consider, then, the action of those molecules which absorb the blue light.

Now since these molecules absorb blue light we know that they will reflect blue light, and, practically speaking, nothing else. Here, then, we have the cause for the blue colour of the sky.

Those who are familiar with the brilliant researches of Dr. Tyndall on the action of light upon vapours will recollect that he also has arrived at a somewhat similar conclusion from a different line of reasoning and a different method of experimentation.

To return one moment to oxygen and nitrogen, the gaseous constituents in our atmosphere, I must here remark that we have no evidence that the pure gases in our air change their molecular constitution; but we know that the aqueous vapour does to an enormous extent, and there is one state to which at present no reference has been made. There is a condition of aqueous vapour which is competent to absorb white light without giving rise to any coloured phenomena; this is the form of which mist and clouds are built up; why they are so dazzlingly white in the sunshine; why we have a dark grey day absolutely devoid of colour when a pall of cloud hangs over the whole sky. In addition to this we know also not only that condition to which I have already referred, which absorbs in the blue, but certainly of one, and in all probability two which absorb in the red. One of these absorptions indicates that the form of vapour which produces it is of the most delicate kind, while that which gives us the continuous absorption in the red end is perhaps the last stage reached before clouds are formed. If this be so, the very complex nature of the true cause of sky colour will be obvious. We have three molecular colour-giving states to contend with, and the action of these will depend largely upon the thickness of aqueous vapour traversed by the sunlight. A diagram will at once explain how the action of these different thicknesses is brought about.

In the diagram, Fig. 3, we have a section of a part of the earth and its atmosphere, supposing the latter to be

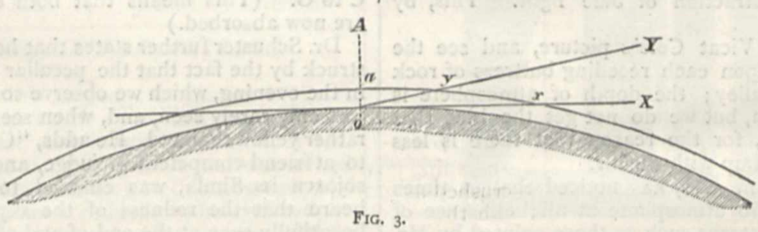


Fig. 3.

somewhere between forty and fifty miles thick. On the assumption that the aqueous vapour, which, as I have shown, is the effective absorber in the air, is equally distributed, let us see how the question of thickness of absorbing layers comes in. Take an observer at o, supposing him in the tropics, and that he sees the sun overhead at A; notice the distance $o\,a$, which represents the thickness of air traversed when the sun is overhead, and compare it with $o\,x$, when the sun is rising or setting as at X, and when, therefore, the greatest thickness is traversed, taking no account of refraction.

The whiteness of the sun at a high altitude and the redness of it when rising or setting is associated then with the fact that at these times the light traverses the least and greatest thickness of the atmosphere respectively; an intermediate height of the sun is represented at Y, and obviously the distance $o\,y$ will vary for intermediate altitudes from $o\,a$ to $o\,x$.

Now the thinner we make our atmosphere the greater will be the difference of the thicknesses $o\,a$ and $o\,x$, and, as a matter of fact, the effective aqueous vapour lies very low down; so that the differences will be greater when we consider the aqueous vapour alone than when we consider the whole atmosphere. The thickness of the aqueous vapour, therefore, increasing from a to x, let us take the case of a perfectly clear sky at sunset; the white light reflected, as I have already shown, in conjunction with the blue, will find itself most absorbed in the line $o\,x$, least absorbed in the line $o\,a$. In the line $o\,x$ we get everything absorbed but the red; we get, therefore, a red sky. A little higher everything is absorbed but the red, orange, and yellow; this will produce a rich golden colour above the red; higher still, the green and part of the blue is allowed to pass; in fact, only the extreme blue is absorbed; and, as I stated before, when I referred to the absorption of chlorine gas in a tube, the residual light will be green. Above the green we have the blue.

This is the order of the colours of *the sky;* the sun in consequence of its greater brilliancy can overcome this absorption until it has reached a very extreme limit, *sunset clouds lighted up by the sun, therefore, must put on the colour of the sun,* because the light which has reached our eye is red light, which has travelled to us *viâ* the cloud, *hence the green is limited to a band of sky,* between the gold and the blue, a green cloud is impossible, and it is on this ground that I ventured to criticise Mr. Ellis's picture, "The Last of the Wreck," 555. Mr. Ellis has painted green clouds; I am certain he never saw one in his life; for a similar reason I have objected to Mr. Oakes's picture, "The Dee Sands." Sky colour is begotten by a low sun.

I do not think that after what I have said it is necessary to point out how it comes that the blue clouds which Mr. Thornburn has chosen to paint are also impossible; a cloud can only be of a colour which is got from the sun directly or indirectly. Now a blue sun is possible, but clouds illuminated by a blue sun are impossible in a picture, because for the sun to be blue there must be nothing but a thick veil of mist.

I have drawn another diagram, which, although it looks rather complicated, may, I think, be rendered clear by a short description. The object I have had in view has been to show how the colours of the sky may be complicated after sunset. I believe in three pictures of sunrises or sunsets out of four, the phenomena presented have really been observed after sunset, in fact, in most pictures of sunset, the sun is a little too slow, we get sunset colour too soon.

In Fig. 4, oX representing the direction of sunset or "sunrise," my object is to show that a cloud high up, say at x, when the sun has set so long as to be at S^3 instead of at X can really receive light from the sun, and the distance xs''' added to ox will represent the total amount of atmospheric absorption undergone by the light. It is

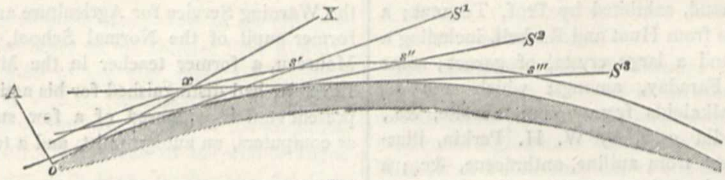


FIG. 4.

under these conditions, too, that, in consequence of the reduced illumination of the background, the sky puts on its most beautiful green, which I think is partly a physiological effect due to the molecular constitution of the retinas of our eyes. Similarly, by drawing a line from S^1 we can see how a cloud absolutely in the zenith of the observer at o may have its colour transformed by a considerable atmospheric absorption *after sunset*.

J. NORMAN LOCKYER

THE MICROPHONE IN SURGERY

ON Tuesday at 3 P.M., before a crowded audience of students and medical men, Sir Henry Thompson gave a demonstration in the anatomical theatre, University College, on the microphone as applicable in operations for stone.

In old days, the lecturer said, patients used to be sent away when they came to the doctor "because their case was not ripe.." The risk involved in the operation of cutting for stone was so serious that a surgeon seldom liked to undertake it except under compulsion, and when cutting had to be resorted to it was not much more difficult to remove a large than a small calculus. In the newer operation of stone-crushing it was better, of course, to have the stone to be crushed as small as possible, and it was essential to deal with the smallest fragments to which the operation reduced it. It was often said, indeed, in objection to lithotrity that to leave even the smallest fragment as a nucleus was to render further treatment necessary and, in time, inevitable. However that might be, it was clearly important to be able to deal with the smallest calculus in the bladder.

Before going further, Sir Henry Thompson emphatically stated that in his belief the present methods of lithotrity are quite sufficient in the hands of any surgeon of fair practice in the operation to enable him to deal successfully at all events with almost every case. He compared the use of the new instrument which he was going to describe to that of the endoscope for the urethra, which, however satisfactory on paper, had not been found important in practice, or, better perhaps, to that of the higher powers of the microscope, which were not necessary nor perhaps even advantageous in ordinary work, but which were a valuable resource in questions of unusual difficulty.

The apparatus consisted of the ordinary feeble battery with wires, connected with two telephones running to different parts of the room, and applied to the ears of the listeners. The ordinary Sound used in operations for crushing the stone was attached by a wire to the circuit of the battery. Near the handle a piece of carbon, such as is used by Prof. Hughes, was carefully balanced and attached by a delicate spring to the battery circuit. When the end of the sound strikes against the smallest piece of calculus the acoustic wave is transmitted along the steel of the instrument to the carbon, where it is transformed into electric vibrations, which are multiplied through the telephone, so that the noise becomes loud and unmistakable. But Sir Henry Thompson pointed out that in practice many things might interfere with the advantageous use of the instrument. The carbon arrangement on the sound must not be too delicate—not such, for instance, as could make us hear the walk of the fly like the tramp of an elephant—else the mere friction of the instrument on the walls of the bladder would produce a noise quite capable of being confounded with that caused by the presence of calculus. The battery must not be too strong, else mere accidental friction of the wires or the noises of the room would produce a distinct sound in the telephone. But when care was taken there would be no difficulty in detecting the noise. An ordinary calculus was put in a bladder in a basin of water, and the listeners could distinctly hear the different noises produced by the point of the sound rasping against the walls of the bladder or striking the stone. A sharp stroke of the former was sometimes not quite unlike the latter. But with the microphone properly adjusted, and the battery not too strong, it was not easy by trial to detect the presence of even a minute fragment of unremoved calculus in the bladder. The carbon needed only to be fitted to the probe, of course, to detect bullets or fragments of bone. But while it was quite possible for a skilful surgeon to make himself absolutely certain by means of the microphone of what he was previously only morally convinced of, Sir Henry Thompson did not appear to anticipate any very remarkable results, at least in ordinary practice, from the use of the instrument.

NOTES

WE are happy to state that a Commission appointed by the French Chamber of Deputies has reported favourably on the erection of a large observatory at Meudon, on the site of the Château which has been in ruins since the Franco-German war. The credit given is 690,000 francs, which will be paid in two instalments, 345,000 in 1878, and 345,000 in 1879. A large part of that sum, 390,000 francs, is destined for the construction of a large refractor, 250,000 francs are for the buildings, and 50,000 francs for the salary of M. Janssen, his assistants, and petty expenses during two years. The credit will be voted very likely *nemine obstante*.

WE learn, with much satisfaction, that the Swedish Diet has granted the necessary funds to the Meteorological Observatory at Upsala, so well known for the high excellence of its work, and that it will commence its new course as a separate institution, distinct from the Astronomical Observatory, on January 1, 1879.

ON Thursday, May 30, Dr. Gladstone, F.R.S., P.C.S. gave a *soirée* to the Fellows of the Chemical Society at Burlington House. Amongst the numerous objects of interest were the following:—A magnificent collection of immediate principles from the brain exhibited by Dr. Thudichum, who also demonstrated the absorption spectrum of a new colouring matter, derived from eggshells, the bands being identical with those of cruentin, obtained from the blood. In the library were specimens of artificial corundum and emerald, made by Feil and Frémy; a large Cape diamond, exhibited by Prof. Tennant; a collection of precious stones from Hunt and Roskell, including a fine pink topaz, cat's eye, and a large crystal of garnet; some interesting apparatus of Faraday, amongst which was his rheostat; a collection of alkaloids from opium, aconite, &c., by Dr. Wright; a splendid case, by W. H. Perkin, illustrating the colouring matters from aniline, anthracene, &c.; a specimen of artificial alizarin and preparations of natural and artificial salicylic acids, the latter of which the exhibitors, Messrs. Hopkin and Williams, have succeeded in obtaining in crystals exactly resembling those of the natural product. Minerals containing liquid carbonic acid were shown by W. N. Hartley, who also demonstrated the effect of heat on the liquid inclosed in the cavities: crystals from Owens College, including a large, almost perfect octohedron of chrome alum; various interesting products, &c., were exhibited by Prof. Odling, Prof. Frankland, Dr. Russell, Dr. Armstrong, Dr. Witt, Dr. Schorlemmer, Dr. Hugo Müller, and M. M. P. Muir. In the room adjoining the lecture-room were some candles which had been acted upon by sea-water for 173 years, a large collection of meteoric stones, an interesting series of photographs of invisible fluorescent bodies, &c., exhibited by the President; a splendid photograph of the solar spectrum, shown by Mr. Rutherfurd; the spectrum of bismuth was shown by Mr. Browning; dichroic crystals of nickel and cobalt salts, by J. M. Thomson; photographs illustrating his recent researches in solar chemistry, by J. N. Lockyer; an enormous cut cairngorm, weighing 51 oz.; an opal cameo, and various minerals, by Bryce Wright, &c. In the lecture and preparation rooms were the microphone, exhibited by Prof. Hughes, which attracted considerable interest; Mr. W. De la Rue showed some phosphorescent tubes which, after a momentary exposure to some burning magnesium, flashed back all the colours of the spectrum; Byrne's pneumatic battery, and the copper zinc couple were shown in action; Messrs. Murray and Heath exhibited, under the microscope, some pretty crystals of gold, silver, &c.; Sir Joseph Whitworth and Dr. Siemens showed specimens of steel; in the same room Dr. Guthrie exhibited the formation of cryohydrates. Many other objects of interest were exhibited, but it would be impossible to enumerate all. The *soirée* was most successful, and although the attendance was numerous, the arrangements were so good that at no time were the rooms inconveniently crowded.

THE publication of a work on the algæ of North America, to consist of the plants themselves properly put up and labelled, was commenced a year ago by the three eminent American algologists, Dr. W. G. Farlow, of Cambridge, Prof. D. C. Eaton, of Yale, and Dr. C. L. Anderson, of California. A second fasciculus has lately appeared, and maintains the high character of the first.

THE death is announced of the venerable Baron von Ettingshausen, in his eighty-second year. We hope to give some notice of his life next week.

M. MASCART, the new director of the French Central Meteorological Bureau, took possession of the *Bulletin International* on June 1, without making any alteration in the nature of its contents or its periodicity. On the preceding day he visited for the first time the meteorological division of the observatory, and warned the officials to prepare for being removed from the establishment at an early date, their rooms being wanted for enlarging the astronomical service. Thus the principle of separation will be carried into effect very shortly. We are glad, however, that M. Mascart stated that the services of M. Leverrier's assistants had been quite appreciated by the Government. None of them will lose their present situations, and an increase of their present salaries is contemplated. The head of the Warning Service for Agriculture and Marine is M. Fron, a former pupil of the Normal School, and the sub-director, M. Moureau, a former teacher in the Mutual Schools, whom M. Leverrier had distinguished for his activity and ingenuity. The present staff is composed of a few subordinates acting merely as computers, an autographist, and a telegraphist.

WE notice the death, on May 14, in Dresden, of Prof. Wilhelm Friedrich Georg Behn, an able anatomist and zoologist. He was born in Kiel in 1808, and, after the completion of his scientific studies, entered as private docent in the Kiel University, where he received the chair of zoology in 1852. After the annexation of Schleswig-Holstein to Prussia he exchanged his professorship for one in Dresden. Here he was elected, in 1869, to the presidency of the Leopoldina-Carolina Akademie der Naturforscher, a position which he occupied up to his death. The academy, although the oldest in Germany—being founded in 1652—was then nearly on the point of dissolution. Prof. Behn's energetic efforts succeeded, however, in resuscitating it, and rendering it once more the centre of Saxon scientific life.

AT a general meeting of the Royal Irish Academy on Monday last week Cunningham gold medals were awarded to Dr. Aquila Smith for his inquiries into Irish numismatics; Dr. Allman, F.R.S., for his researches into the natural history of the Hydrozoa; and Dr. Casey for his important mathematical discoveries.

THE Dutch Society of Sciences held its 126th general meeting on May 18. It was at this meeting that the Huygens Medal was awarded to Prof. Newcomb, who, along with Sir George Airy, Dr. Auwers of Berlin, Prof. Du Bois Reymond of Berlin, M. V. Duruy of Paris, and Dr. C. F. W. Ludwig of Leipzig, were elected foreign members. For a paper on the question "What are the meteorological and magnetic phenomena which there are sufficient reasons for believing are connected with solar spots," Prof. Fritz of Zurich Polytechnic, was awarded a prize of 150 florins. The proposer of the question, Dr. Buys Ballot, was awarded a silver medal. A number of subject for prizes were proposed for competition in 1879 and 1880, the most important of which we hope to give next week.

IN the June number of the *American Journal of Science and Art* Prof. Marsh announces one of the most interesting discoveries yet made in the Palæontology of the Rocky Mountains, which have lately produced so many novelties. This is the right lower jaw of a small opossum, of the family Didelphydæ, for which he proposes the name *Dryolestes priscus*, from the Upper Jurassic series, in which no mammalian remains have previously been found in America.

THE first annual meeting of the Midland Union of Natural History Societies on May 27, at Birmingham, was, we are glad to say, a great success. After a luncheon given by the president, Mr. Tonks, a meeting was held at 3 P.M. in the Midland Institute, which was largely attended, as was also the brilliant *conversazione* in the evening in the town-hall. An excursion to Dudley on Tuesday, attended by about 400 members and their friends, brought to a close a pleasant and profitable meeting.

THE annual meeting of the Sanitary Institute was held on Friday last, Dr. B. W. Richardson, president, in the chair. It was shown that the Institute had already done good work, and exercised a decided influence in relation to sanitary matters. Dr. Richardson was re-elected president.

THE Tay Bridge, at Dundee, was opened on Friday, in presence of a large and distinguished company.

TWO Reports come to us from Scotland—one on the Glasgow Industrial Museum, and the other on the Dundee Free Library. From the former we are glad to see that, under the energetic curator, Mr. James Paton, F.L.S., the Glasgow Museum is gradually becoming worthy of the second city of the kingdom. Many important additions are being made to the well-arranged museum, with which, we see, have been incorporated the Corporation Galleries of Art. We trust the successors of "Bailie Nicol Jarvie" and his contemporary councillors will exercise a wise liberality and speedily raise their museum to the position it ought to occupy. From the Report of Mr. Maclauchlan we are pleased to see that scientific works are in considerable demand among the busy people of enterprising Dundee. The interesting museum, also, is gradually becoming possessed of that complete collection of the Arctic fauna which strangers naturally look for in the museum of the chief seat of the whaling trade.

IN the *Annalen der Hydrographie* we notice the account of a group of three islands discovered by Capt. Caller in 1877 on the north-west coast of Australia. These islands, which in their highest point do not rise more than thirty feet above the level of the sea, are covered with a thick deposit of guano, containing an unusual amount of ammonia and phosphates. On account of their nearness to the continent these valuable deposits will probably play an important part in the agricultural development of Australia.

ON October 4 last we gave an account of the post-mortem examination of a white whale (*Beluga*) that died after a few days' residence in a tank at the Westminster Aquarium. Mr. Farini then commissioned Zack Coup to obtain three more and bring them over from Labrador. They were packed each in a separate box lined with sea-weed, and four men were engaged to relieve one another in throwing water over the heads of the animals during the entire voyage. On Tuesday, May 27, they arrived at Liverpool, when one specimen was sent to Blackpool, one to Manchester, and one was brought under the personal care of Mr. Farini and Mr. Carrington, the naturalist of the Aquarium, to Westminster. This London specimen is 13ft. 6in. long, and arrived in apparently good condition. On Friday it was found requisite to "sling" it in order to remove an eel that had become entangled in its right flipper, when its quick sight in trying to avoid the sling was noticed with interest. The legs of a man sitting on the edge of the tank it carefully avoided, but it did not seem to mind the presence of those standing round. After the whale had been in the tank four days an indication of malaise and apparently of some accident having occurred attracted the careful attention of those who had charge of it. It was then ascertained the specimen was a female, and was for a while a subject of interest to physiologists especially.

A NEW improvement in the microscope is reported from Germany. Herr I. von Lenhossék has constructed an apparatus which permits no less than sixty microscopical preparations being observed in immediate succession, without the trouble of changing the slides and readjustment of the object-glass. Its construction is similar in principle to that of the well-known revolving stereoscopes, and the inventor has given the new apparatus the name of "polymicroscope."

UPON the occasion of unveiling the statue of Giordano Bruno, which will take place at Rome on February 19, 1879, a new edition of his works will be published. They are being reprinted at the expense of the Italian government.

THE Vienna Academy of Sciences held its annual public session on May 29, in the presence of representatives of the Court and Government. After the announcement of the various prizes and reports on the progress in the several sections of the Academy, Prof. Hann delivered an address on the "Problems of Modern Meteorology."

AN Ethnographical Exhibition, organised by the Anthropological Society of Paris in an annexe to the Trocadero, was opened on May 31. M. Teisserenc de Borg, Minister for Commerce and Agriculture was present on behalf of M. Bardoux, the Minister of Public Instruction, and declared the exhibition open. The addresses were delivered by MM. Quatrefages, Henri Martin, the president of the Society, and Dr. Broca. This exhibition is an extension of the Provisional Museum established for some months at the Palais de l'Industrie, in the Champs Elysées.

WE noticed at the time that M. Jules Simon, when French Minister for Public Instruction, had opened in the buildings of the Ministry, a provisional Pedagogical Museum, but a change having supervened in the Cabinet the scheme was dropped. It will, we learn now, be revived by M. Bardoux, who has asked special credit for that purpose from the Chamber of Deputies.

THE electric-light display in the Paris streets and thoroughfares is becoming one of the attractions of Paris. Eight electric lamps have been placed in the Place de l'Opéra, twenty-four others in the Opera Avenue, and eight more on the Place du Théâtre Français. Six lamps were lighted for the first time on June 1 on the part of the Palais Bourbon facing the Place de la Concorde. We should notice also the private illumination of the Grands Magasins du Louvre, about seventy lamps; Belle Jardinière, eight; Concert de l'Orangerie des Tuileries, twenty; and the Hippodrome, thirty-two. This last illumination, being in a closed building, cannot be viewed from the streets. All these illuminations are made by the Jablochkow candles. An electric lamp has been placed also on the top of the Trocadero Palace.

WE notice that the list of jurors for the Paris Exhibition has been gazetted.

THE last number of the *Journal* of the Society of Arts contains a valuable paper, recently read by Mr. J. M. Thomson, F.C.S., before the Society, on the Position of Chemistry in a System of Technical Education, as illustrated by some of its Applications. We are glad to see the Society turning its attention to a subject of such great importance.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. J. Farmer; a Geoffroy's Cat (*Felis geoffroii*) from Uruguay, presented by Mr. Ronald Bridgett; a Brazilian Caracara (*Polyborus brasiliensis*) from South America, presented by Miss Amslie; a Tamandua Anteater (*Tamandua tetradactyla*) from South America, deposited; a White-eared Bulbul (*Pycnonotus leucotis*) from North-west India, received in exchange; four Temminck's Tragopans (*Ceriornis temminckii*), bred, a Yellow-footed Rock Kangaroo (*Petrogale xanthopus*), an Axis Deer (*Cervus axis*), born in the Gardens.

THE REDE LECTURE[1]

WHEN, about two years ago, news came from the other side of the Atlantic that a method had been invented of transmitting, by means of electricity, the articulate sounds of the human voice, so as to be heard hundreds of miles away from the speaker, those of us who had reason to believe that the report had some foundation in fact, began to exercise our imaginations in picturing some triumph of constructive skill—something as far surpassing Sir William Thomson's Siphon Recorder in delicacy and intricacy as that is beyond a common bell-pull. When

[1] Given at Cambridge by Prof. Clerk-Maxwell, F.R.S., May 24, 1878 Subject—"The Telephone."

at last this little instrument appeared, consisting, as it does, of parts, everyone of which is familiar to us, and capable of being put together by an amateur, the disappointment arising from its humble appearance was only partially relieved on finding that it was really able to talk.

But perhaps the telephone, though simple in respect of its material and construction, may involve some recondite physical principle, the study of which might worthily occupy an hour's time of an academic audience: I can only say that I have not yet met anyone acquainted with the first elements of electricity who has experienced the slightest difficulty in understanding the physical process involved in the action of the telephone. I may even go further, and say that I have never seen a printed article on the subject, even in the columns of a newspaper, which showed a sufficient amount of misapprehension to make it worth preserving—a proof that among scientific subjects the telephone possesses a very exceptional degree of lucidity.

However, if the telephone has something to say for itself, it would seem hardly necessary for me to take up your time with any tedious introduction. It is unfortunate, however, that up to the present time the telephone has kept all his more perfect utterances to be whispered into the privileged ear of a single listener. When he is older, he may get more accustomed to public speaking, but if we force him, in his present immature state, to exert his voice beyond what is good for him, it may sound rather too like the pot quarrelling with the kettle, and may call for the criticism with which Mr. Tennyson's Princess complimented the disguised Prince on his "Song of the Swallow:"—

> "Not for thee, she said,
> O Bulbul, any rose of Gulistan
> Shall burst her veil: marsh divers rather, maid,
> Shall croak thee sister, or the meadow crake
> Grate her harsh kindred in the grass."

Is it for this, then, that we are to forsake the luncheons and lawn tennis and all the engrossing studies of the May Term, and to assemble in this solemn hall, where the very air seems thick with the accumulation of unsolved problems, or else redolent of the graces of innumerable congregations?

It is not by concentrating our minds on any problem, however important, but rather by encouraging them to expand, that we shall best fulfil the intention of Sir Robert Rede when he founded this lecture.

It would be as useless as it would be tedious to try to explain the various parts of this small instrument to persons in every part of the Senate House. I shall, therefore, consider the telephone as a material symbol of the widely separated departments of human knowledge, the cultivation of which has led, by as many converging paths, to the invention of this instrument by Professor Graham Bell.

For whatever may be said about the importance of aiming at depth rather than width in our studies, and however strong the demand of the present age may be for specialists, there will always be work, not only for those who build up particular sciences and write monographs on them, but for those who open up such communications between the different groups of builders as will facilitate a healthy interaction between them. And in a university we are especially bound to recognise not only the unity of science itself, but the communion of the workers in science. We are too apt to suppose that we are congregated here merely to be within reach of certain appliances of study, such as museums and laboratories, libraries and lecturers, so that each of us may study what he prefers. I suppose that when the bees crowd round the flowers it is for the sake of the honey that they do so, never thinking that it is the dust which they are carrying from flower to flower which is to render possible a more splendid array of flowers, and a busier crowd of bees, in the years to come. We cannot, therefore, do better than improve the shining hour in helping forward the cross-fertilization of the sciences.

Before we go further, I wish to express my obligation to Mr. Garnett for the able assistance he has given me. He has not only collected the apparatus before you, but constructed some of it himself. But for him, I might have given you some second-hand information about telephones. He has made it possible for you to hear something yourselves. I have also to thank Mr. Gower, who has brought his telephone harp, and Mr. Middleton, who has contributed several instruments of his own invention.

We shall begin with the telephone in its most obvious aspect, as an instrument depending on certain physical principles.

The apparatus consists of two instruments, the transmitter and the receiver, doubly connected by a circuit capable of conducting electricity. The speaker talks to the transmitter at one end of the line, and at the other end of the line the listener puts his ear to the receiver, and hears what the speaker says.

The process in its two extreme stages is so exactly similar to the old-fashioned method of speaking and hearing that no preparatory practice is required on the part of either operator.

We must not, however, fall into the error of confounding the principle of the electric telephone with that of other contrivances for increasing the distance at which a conversation may be carried on. In all these the principle is the same as in the ordinary transmission of sound through the air. The different portions of matter which intervene between the speaker and the hearer take part, in succession, in a certain mechanical process. Each receives a certain motion from the portion behind it and communicates a precisely similar motion to the portion in front of it, in doing which it gives out all the energy it received, and is again reduced to rest.

The medium which takes part in this process may be the open air, or air confined in a long tube, or some other medium such as a brick wall, as when we hear what goes on in the next house, or a long wooden rod, or a metal wire, or even a stretched string. In all these it is by the actual motion of the successive portions of the medium that the message is transmitted.

In the electric telephone there is also a medium extending from the one instrument to the other. It is a copper wire, or rather two wires forming a closed circuit. But it is not by any motion of the copper that the message is transmitted. The copper remains at rest, but a variable electric current flows to and fro in the circuit.

It is this which distinguishes the electric telephone from the ordinary speaking tube, and from the transmission of vibrations along wooden rods by which Sir Charles Wheatstone used to cause musical instruments to sound in a mysterious manner without any visible performer.

On the other hand, we have to distinguish the principle of the articulating telephone from that of a great number of electrical contrivances which produce visible or audible signals at a distance. Most of these depend on the alternate transmission and interruption of an electric current. In some part of the circuit a piece of apparatus is introduced corresponding to this instrument which is called a key. Whenever two pieces of metal, called the contact pieces, touch each other, the current flows from the one to the other, and so round the circuit. Whenever the contact pieces are separated the current is interrupted, and the effects of this alternation of current and no current may be made to produce signals at any other part of the circuit.

In the Morse system of signalling, currents of longer and of shorter duration are called dashes and dots respectively, and by combinations of these the symbols of letters are formed. The rate at which these little currents succeed one another depends on the rate at which the operator can work the key, and may be increased by mechanical methods till the receiving clerk can no longer distinguish the symbols.

But the capability of the telegraph wire for transmitting signals is by no means exhausted; as the rapidity of the succession is increased, the ear ceases to distinguish them as separate signals, but begins to recognise the impression it receives as that of a musical tone, the pitch of which depends on the number of currents in a second.

Tuning forks driven by electricity were used by Helmholtz in his researches on the vowel sounds, and the periodically intermittent current which they furnish is recognised as a most valuable agent in physical and physiological research. The tuning forks are of the most massive construction, and the succession of currents goes on with the most inflexible regularity, so that whenever we have occasion to follow the march of a process which takes place in a short time, such as the vibration of a violin string or the twitch of a living muscle, the tuning fork becomes our appropriate timepiece.

Apparatus of this kind, however, the merit of which is its regularity, is quite incapable of adapting itself to the transmission of variable tones such as those of a melody.

The first successful attempt to transmit variable tones by electricity was made by Philip Reis, a teacher in a school at Friedrichsdorf, near Homburg. On October 21, 1861, Reis showed his instrument, which he called a telephone, to the Physical Society of Frankfort on the Main. He succeeded in transmitting melodies which were distinctly heard throughout the room.

The transmitter of Reis's telephone is essentially a make and break key of so delicate a construction that the sound-waves in the air are able to work it.

The air vibrations set in motion a stretched membrane like a drumhead, with a piece of platinum fastened to it. This piece of platinum, when vibrating, strikes against another piece of platinum, and so completes the circuit every time contact is made.

At every point of the circuit there is thus a series of currents corresponding in number to the vibrations of the drumhead, and by causing these to pass through the coil of an electromagnet, the armature of the electromagnet is attracted every time the current passes, and if the armature is attached to a resonator of any kind, the succession of tugs will set it in vibration, and cause it to emit a sound, the pitch of which is the same as that of the note sung into the transmitter at the other end of the line.

[Mr. Gower here played the "March of the Men of Harlech" on the telephone harp placed in the Geological Museum. The instrument consists of a set of steel reeds worked by percussion, which make and break contact on the battery circuit, of which the primary wire of an induction coil forms part. The receivers are worked by the secondary current. There were four receivers, one of them Prof. Bell's original one, placed in different parts of the Senate-house.]

If the pitch of a sound were the only quality which we are able to distinguish, the problem of telephony would have received its complete solution in the instrument of Reis. But the human ear is so constructed, and we ourselves are so trained by continual practice, that we recognise distinctions in sound of a far more subtle character than that of pitch; and these finer distinctions have become so much more important for the purposes of human intercourse than the musical distinction of pitch, that many persons can detect the slightest variation in the pronunciation of a word who are comparatively indifferent to the variations of a melody.

Now, the telephone of Prof. Graham Bell is an articulating telephone, which can transmit not only melodies sung to it, but ordinary speech, and that so faithfully that we can often recognise the speaker by his voice as heard through the telephone. How is this effected? It is manifest that if by any means we can cause the tinned plate of the receiving instrument to vibrate in precisely the same manner as that of the transmitter, the impression on the ear will be exactly the same as if it had been placed at the back of the plate of the transmitter, and the words will be heard as if spoken at the other side of a tinned plate.

But this implies an exact correspondence, not only in the number of vibrations, but in the type of each vibration.

Now, if the electrical part of the process consisted merely of alternations between current and no current, the receiving instrument could never elicit from it the semblance of articulate speech. If the alternations were sufficiently regular, they would produce a sound of a recognisable pitch, which would be very rough music if the pitch were low, but might be less unendurable if the pitch were high; still, at the best, it would be like playing a violin with a saw instead of a bow.

What we want is not a sudden starting and stopping of the current, but a continuous rise and fall of the current, corresponding in every gradation and inflexion to the motion of the air agitated by the voice of the speaker.

Prof. Graham Bell has recounted the many unsuccessful attempts which he made to produce undulatory currents instead of mere intermittent ones. He had, of course, to give up altogether the method of making and breaking contact. Every method involving impact of any kind, whether between electric contact pieces or between the sounding parts of the instrument, introduces discontinuity of motion, and therefore precludes a faithful reproduction of speech.

In the ultimate form which the telephone in his hands assumed, the electric current is not merely regulated but actually generated by the aërial vibrations themselves.

The electric principle involved in Bell's telephone is that of the induction of electric currents discovered by Faraday in 1831. Faraday's own statement of this principle has been before the scientific world for nearly half a century, but has never been improved upon.

Consider first a conducting circuit, that is to say, a wire which after any number of convolutions returns into itself. Round such a circuit an electric current may flow, and will flow if there is an electromotive force to drive it.

Consider next a line of magnetic force, such a line as you see here made visible by sprinkling iron filings on a sheet of paraffin paper. This line, as Faraday also first showed, is a line returning into itself, or, as the mathematicians would say, it is a closed curve.

Now, if there are two closed curves in space, they must either embrace one another so as to be linked together, or they must not embrace each other.

If the line of force as well as the circuit were made of wire, and if it embraced the copper circuit, it would be impossible to unlink them without cutting one or other of the wires. But the line of force is more like one of Milton's spirits, which cannot

> "In their liquid texture mortal wound
> Receive, no more than can the fluid air."

Now, if the copper circuit or the lines of force move relatively to each other, then in general some of the lines of force which originally embraced the circuit will cease to embrace it, or else some of those which did not embrace it will become linked with it.

For every line of force which ceases to embrace the circuit there is a certain amount of positive electromotive force, which, if unopposed, will generate a current in the positive direction, and for every new line which embraces the circuit there is a negative electromotive force, causing a negative current.

In Bell's telephone the circuit forms a coil round a small core of soft iron fastened to the end of a steel magnet. Now lines of magnetic force pass more freely through iron than through any other substance. They will go out of their way in order to pass through iron instead of air. Hence a large proportion of the lines of force belonging to the magnet pass through the iron core, and, therefore, through the coil, even though there is no iron beyond the core, so that they have to complete their circuit through air.

But if another piece of soft iron is placed near the end of the core it will afford greater facilities for lines which have passed through the core to complete their circuit, and so the lines belonging to the magnet will crowd still closer together to take advantage of an easy passage through the core and the iron beyond it. If then the iron is moved nearer to the core, there will be an increase in the number of such lines, and, therefore, a negative current in the circuit. If it is moved away there will be a diminution in the number of lines, and a positive current in the circuit. This principle was employed by Page in the construction of one of the earliest magneto-electric machines, but it was reserved for Prof. Bell to discover that the vibrations of a tinned iron plate, set in motion by the voice, would produce such currents in the circuit as to set in motion a similar tinned plate at the other end of the line.

It will help us to appreciate the fertility of that germ of science which Faraday first detected and developed if we recollect that year after year he had employed the powerful batteries and magnets and delicate galvanometers of the Royal Institution to obtain evidence of what he all along hoped to discover—the production of a current in one circuit by a current in another, but all without success, till at last he detected the induced current as a transient phenomenon, to be observed only at the instant of making or breaking the primary circuit.

In less than half a century, and by the aid of no second Faraday, but in the course of the ordinary growth of scientific principles, this germ, so barely caught by Faraday, has developed on the one hand into the powerful currents which maintain the illumination of the lighthouses on our coasts; and on the other, into these currents of the telephone which produce an audible effect, though the engine that drives them is itself driven by the tremors of a child's voice.

Prof. Tait has recently measured the absolute strength of these telephone currents. He produced them by means of a tuning fork vibrating in front of the coil of the transmitter. Before the transmitted note ceased to be audible at the other end of the line he measured by means of a microscope the amplitude of the vibrations of the fork.

He then placed a very delicate galvanometer in the circuit and found what deflexion was produced by a measured motion of the fork.

Finally he measured the deflection of the galvanometer produced by a small electromotive force of known magnitude. He thus found that the telephone currents produced an audible effect when reversed 500 times a second, though their strength was no greater than what a Grove's cell would send through a million megohms, about a thousand million times less than the currents used in ordinary telegraphic work.

One great beauty of Prof. Bell's invention is that the instruments at the two ends of the line are precisely alike. When the tin plate of the transmitter approaches the core of its bobbin it produces a current in the circuit, which has also to circulate round the bobbin of the receiver, and thus the core of the receiver is rendered more or less magnetic, and attracts its tin plate with greater or smaller force. Thus the tin plate of the receiver reproduces on a smaller scale, but with perfect fidelity, every motion of the tin plate of the transmitter.

This perfect symmetry of the whole apparatus—the wire in the middle, the two telephones at the end of the wire, and the two gossips at the ends of the telephones—may be very fascinating to a mere mathematician, but it would not satisfy an evolutionist of the Spencerian type, who would consider anything with both ends alike to be an organism of a very low type, which must have its functions differentiated before any satisfactory integration can take place.

Accordingly, many attempts have been made, by differentiating the function of the transmitter from that of the receiver, to overcome the principal limitation to the power of the telephone. As long as the human voice is the sole motive power of the apparatus it is manifest that what is heard at one end must be fainter than what is spoken at the other. But if the vibration set up by the voice is used no longer as the source of energy, but merely as a means of modulating the strength of a current produced by a voltaic battery, then there will be no necessary limitation of the intensity of the resulting sound, so that what is whispered to the transmitter may be proclaimed *ore rotundo* by the receiver.

A result of this kind has already been obtained by Mr. Edison by means of a transmitter in which the sound vibrations produce a varying pressure on a piece of carbon, which forms part of the electric circuit. The greater the pressure, the smaller is the resistance due to the insertion of the carbon, and therefore the greater is the current in the circuit.

I have not yet seen Mr. Edison's transmitter, but the microphone of Prof. Hughes is an application of carbon and other substances to the construction of a transmitter, which modulates the intensity of a battery current in more or less complete accordance with the sound-vibrations it receives. The energy of the sound produced is no longer limited by that of the original sound. All that the original sound does is to draw supplies of energy from the battery, so that a very feeble sound may give rise to a considerable effect. Thus, when a fly walks over the table of the microphone the sound of his tramp may be heard miles off.

Indeed, the microphone seems to open up several new lines of research. We shall have London physicians performing stethoscopic auscultations on patients in all parts of the kingdom. The Entomological Society have recently been much interested by Mr. Wood-Mason's discovery of a stridulating apparatus in scorpions. Perhaps ere long a microphone, placed in a nest of tropical scorpions, may be connected up to a receiver in the apartments of the society, so as to give the members and their musical friends an opportunity of deciding whether the musical taste of the scorpion resembles that of the nightingale or that of the cat.

I have said that the telephone is an instance of the benefit to be derived from the cross-fertilization of the sciences. Now this is an operation which cannot be performed by merely collecting treatises on the different sciences, and binding them up into an encyclopædia. Science exists only in the mind, and the union of the sciences can take place only in a living person.

Now, Prof. Graham Bell, the inventor of the telephone, is not an electrician who has found out how to make a tin plate speak, but a speaker, who, to gain his private ends, has become an electrician. He is the son of a very remarkable man, Alexander Melville Bell, author of a book called "Visible Speech," and of other works relating to pronunciation. In fact, his whole life has been employed in teaching people to speak. He brought the art to such perfection that, though a Scotchman, he taught himself in six months to speak English, and I regret extremely that when I had the opportunity in Edinburgh I did not take lessons from him. Mr. Melville Bell has made a complete analysis and classification of all the sounds capable of being uttered by the human voice, from the Zulu clicks to coughing and sneezing; and he has embodied his results in a system of symbols, the elements of which are not taken from any existing alphabet, but are founded on the different configurations of the organs of speech.

The capacities of this new mode of representing speech have been put to the test by Mr. Alexander J. Ellis, author of "The Essentials of Phonetics," a gentleman who has studied the whole theory of speech acoustically, philologically, and historically. He describes the result in a letter to *The Reader*:—

"The mode of procedure was as follows:—Mr. Bell sent his two sons, who were to read the writing, out of the room—it is interesting to know that the elder, who read all the words in this case, had only had five weeks' instruction in the use of the alphabet—and I dictated slowly and distinctly the sounds which I wished to be written. They consisted of a few words in Latin, pronounced first as at Eton, then as in Italy, and then according to some theoretical notions of how the Latins might have uttered them. Then came some English provincialisms and affected pronunciations, the words 'how odd' being given in several distinct ways. Suddenly German provincialisms were introduced; then discriminations of sounds often confused. Some Arabic, some Cockney English, with an introduced Arabic guttural, some mispronounced Spanish, and a variety of shades of vowels and diphthongs.

"The result was perfectly satisfactory—that is, Mr. Bell wrote down my queer and purposely exaggerated pronunciations and mispronunciations, and delicate distinctions, in such a manner that his sons, not having heard them, so uttered them as to surprise me by the extremely correct echo of my own voice. . . . Accent, tone, drawl, brevity, indistinctness were all reproduced with surprising accuracy. Being on the watch, I could, as it were, trace the alphabet in the lips of the readers. I think, then, that Mr. Bell is justified in the somewhat bold title which he has assumed for his mode of writing—'Visible speech.' I only hope that for the advantage of linguists, such an alphabet may soon be made accessible, and that, for the intercourse of nations, it may be adopted generally, at least for extra-European nations, as for the Chinese dialect and the several extremely diverse Indian languages, where such an alphabet would rapidly become a great social and political engine."

The inventor of the telephone was thus prepared, by early training in the practical analysis of the elements of speech, to associate whatever scientific knowledge he might afterwards acquire with those elementary sensations and actions, which each of us must learn from himself, because they lie too deep within us to be described to others. This training was put to a very severe test when, at the request of the Boston Board of Education, Prof. Graham Bell conducted a series of experiments with his father's system in the Boston School for the Deaf and Dumb. I cannot conceive a nobler application of the scientific analysis of speech, than that by which it enables those to whom all sound is

"expunged and rased
And wisdom at one entrance quite shut out"

not only to speak themselves, but to read by sight what other people are saying. The successful result of the experiments at Boston is not only the most valuable testimonial to the father's system of visible speech, but an honour which the inventor of the telephone may well consider as the highest he has attained.

An independent method of research into the process of speech was employed by Wheatstone, Willis, and Kempelen, the aim of which was to imitate the sounds of the human voice by means of artificial apparatus. This apparatus was in some cases modelled so as to represent as nearly as possible the form as well as the functions of the organs of speech, but it was found that an equally good imitation of the vocal sounds could be obtained from apparatus the form of which had no resemblance to the natural organs.

Several isolated facts of considerable importance were established by this method, but the whole theory of speaking and hearing has been so profoundly modified by Helmholtz and Donders, that much of what was advanced before their time has come to possess only an historical interest.

Among all the recent steps in the progress of science, I know none of which the truly scientific or science-producing consequences are likely to be so influential as the rise of a school of physiologists, who investigate the conditions of our sensations by producing on the external senses impressions, the physical conditions of which can be measured with precision, and then recording the verdict of consciousness as to the similarity or difference of the resulting sensations.

Prof. Helmholtz, in his recent address as Rector of the University of Berlin, lays great stress on that personal interaction between living minds, which I have already spoken of as essential to the life of a University. "I appreciate," he says, "at its full value this last advantage, when, looking back, I recall my student days, and the impression made upon us by a man like Johannes Müller, the physiologist. When one finds himself in contact with a man of the first order, the entire scale of one's intellectual conceptions is modified for life; contact with such a man is perhaps the most interesting thing life may have to offer."

Now, the form in which Johannes Müller stated what we may regard as the germ which fertilized the physiology of the senses is this, that the difference in the sensations due to different senses does not depend upon the actions which excite them, but upon the various nervous arrangements which receive them.

To accept this statement out of a book, as a matter of dead faith, may not be difficult to an easy-going student; but when caught like a contagion, as Helmholtz caught it, from the lips of the living teacher, it has become the guiding principle of a life of research.

No man has done more than Helmholtz to open up paths of communication between isolated departments of human knowledge; and one of these, lying in a more attractive region than that of elementary psychology, might be explored under exceptionally favourable conditions, by some of the fresh minds now coming up to Cambridge.

Helmholtz, by a series of daring strides, has effected a passage for himself over that untrodden wild between acoustics and music—that Serbonian bog where whole armies of scientific musicians and musical men of science have sunk without filling it up.

We may not be able even yet, to plant our feet in his tracks and follow him right across. That would require the seven league boots of the German colossus; but to help us in Cambridge we have the Board of Musical Studies, vindicating for music its ancient place in a liberal education. On the physical side we have Lord Rayleigh laying the foundation deep and strong in his "Theory of Sound." On the æsthetic side we have the University Musical Society doing the practical work, and in the space between, those conferences of Mr. Sedley Taylor, where the wail of the siren draws musician and mathematician together down into the depths of their sensational being, and where the gorgeous hues of the phoneidoscope are seen to seethe and twine and coil like the

> "Dragon boughs and elvish emblemings"

on the gates of that city where

> "an ye heard a music, like enow
> They are building still, seeing the city is built
> To music, therefore never built at all,
> And therefore built for ever."

The special educational value of this combined study of music and acoustics is that more than almost any other study it involves a continual appeal to what we must observe for ourselves.

The facts are things which must be felt; they cannot be learned from any description of them.

All this has been said more than two hundred years ago by one of our own prophets—William Harvey, of Gonville and Caius College. "For whosoever they be that read authors, and do not by the aid of their own senses, abstract true representations of the things themselves (comprehended in the author's expressions) they do not resent true ideas, but deceitful idols and phantasms, by which they frame to themselves certain shadows and chimæras, and all their theory and contemplation (which they call science) represents nothing but waking men's dreams and sick men's phrensies."

Prof. Maxwell was assisted in his practical demonstrations by Mr. Garnett, of St. John's College.

SOCIETIES AND ACADEMIES

LONDON

Physical Society, April 13.—Prof. R. B. Clifton, vice-president, in the chair.—The following candidates were elected Members of the Society:—W. Campbell, R. W. F. Harrison, Rev. T. N. Hutchinson, M.A., B. W. Richardson, M.B., F.R.S.—The Secretary read a paper by Messrs. J. Nixon and A. W. Heaviside, describing their experiments on the mechanical transmission of speech through wires or other substances, to which Mr. Preece had referred at a previous meeting of the Society. After describing a number of experiments in which metallic discs soldered on to the ends of the conducting wires were employed, they went on to enumerate the more successful experiments in which wooden discs were mainly employed. The first actual transmission of speech was effected by placing the belly of a violin against the receiving end of the wire, when every syllable spoken was distinctly audible. Very good results were obtained by employing mouth-and-ear pieces, formed as in a telephone, the disc being replaced by thin wooden discs, six inches in diameter, and a No. 4 wire was found to be most satisfactory. On suspending a length of this wire in such a manner that it had no rigid attachments, it was ascertained that 120 yards is the limit through which a conversation can be carried on.—Capt. Abney, F.R.S., described the method he adopted for photographing the least refrangible end of the spectrum. He pointed out that it is impossible, with the ordinary sensitive salts employed in the usual way, to photograph further than the Fraunhofer line E, though by a preliminary exposure to light of a Daguerrotype plate, Draper was able to photograph beyond the extreme limit of visibility in the red end of the spectrum. This method, however gave what is known as a reversed picture, the lights and shades being transposed, besides requiring a lengthened exposure. It enabled Becquerel to photograph the spectrum in its natural colours, and later St. Victor obtained coloured images of coloured cloths. The object of Capt. Abney had been to obtain unreversed pictures of this portion of the spectrum; in other words, to obtain a compound that would be similarly sensitive to the red and the blue components of white light. Such a compound he had at last obtained by what he termed *weighting* silver bromide with resin, and now he obtains it by causing the molecules of silver bromide to weight themselves. He showed an ordinary bromide of silver plate, and the colour of the transmitted light was of a ruddy tint, showing absorption of the blue rays; another film was shown containing weighted bromide of silver, which transmitted blue light and absorbed the red. Photographic plates prepared with the latter compound he showed were sensitive to the red and ultra-red waves of light, and he threw on the screen photographs of the spectrum from the line C to a wave-length of 10,000, the ultra-red showing remarkable groupings of lines. He further showed that by friction the blue film was changed to the red, and in that state was not sensitive to the lower part of the spectrum. These photographs were taken by means of a diffraction grating, and Capt. Abney demonstrated Fraunhofer's method of separating the various orders of spectra produced by it. He then explained that recently he had elucidated the reason of the reversal of Draper's pictures by the least refrangible end of the spectrum. He finds that it is accelerated by exposing the plates in weak oxidising solutions, such as those of hydroxyl, bichromate of potash, permanganate of potash, and nitric acid, or exposure to ozone. The red rays, in other words, seemed to oxidise the photographic image, and to render it incapable of development.—Mr. H. Bauermathen exhibited some paper models illustrative of the disposition of the planes of symmetry in crystals. These included octants of the sphere with inclosed cube and octahedron faces pointed into their corresponding hexakis-octohedral faces, a cubic skeleton built up from nine planes of symmetry with a removable outer shell, and a system of axial planes of an unsymmetrical mineral inclosing a solid nucleus contained between three parallel pairs of planes. They were constructed for the purpose of showing popularly the difference between planes of symmetry and other diametral planes by laying upon them a small mirror or plate of mica, when in the first case the inclosed nucleus gave a symmetrical image corresponding in position to the plane immediately behind the mirror, but in the second a broken image is produced.—Dr. Guthrie exhibited the arrangement of apparatus he had employed, in conjunction with his brother, to ascertain the effect of heat on the transpiration of gases. The main difficulty connected with the research was the securing of an absolutely constant pressure on the air operated upon. This was secured by inserting into the neck of the vessel which served as an air-chamber a tube turned up at its inner end and terminating externally by a small funnel; as the tube was kept constantly full of water, the funnel overflowing, a pressure represented by the difference between the heights of these levels was maintained. After passing through a series of drying tubes the air traversed the (U-shaped) capillary tube in a beaker containing water of known temperature, and was finally received in an inverted tube contained in an overflowing dish of water. Among other results it was found that the resistance of a tube is the same as that of its several portions, and if t be the time occupied, T the absolute temperature, $p_1\ p_2$ the pressures, and α and β constants, they find that—

$$t = \alpha T\left(T + \frac{\beta}{p_1 - p_2}\right).$$

Chemical Society, May 16.—Dr. Gladstone, president, in the chair.—The following papers were read:—On the detection and estimation of free mineral acids in various commercial products, by Peter Spence and A. Esilmann. The method is based on the fact that peracetate of iron even in dilute solutions has a distinct yellow colour, not perceptibly altered by acetic acid or solutions of persulphates, but instantly bleached by free sulphuric, hydrochloric, and nitric acids. The solution is made by dissolving ten parts of iron alum and eight parts of crystallised acetate of soda in 1,000 parts of 8 per cent. solution of acetic acid (25 per cent.).—The action of hypochlorites on urea, by H. G. H. Fenton. The author has found that when urea is acted on by a hypochlorite in the cold, in the presence of a caustic alkali, only half the nitrogen is evolved. From various experiments it was proved that the nitrogen remains behind as a cyanate.—On the behaviour of metallic solutions with filter paper and on the detection of cadmium, by T. Bayley. The author has investigated the action which takes place when drops of metallic solutions are placed on filter paper, the extent to which the solutions spread being tested by sulphuretted hydrogen. In some cases the solution seemed to concentrate itself in the middle, in others round the edge of the spot. Dilution, temperature, and the kind of filter paper used, have an important influence on this phenomenon. The salts of silver, lead, &c., when moderately concentrated, give a wide water ring containing no metal, while the salts of copper, nickel, cobalt, and especially cadmium, must be much more dilute to present the same appearance. This property of cadmium to spread itself over the whole drop is so marked that it affords an elegant means of detecting it in the presence of metals whose sulphides are black.—On essential oil of sage, by S. Siguira and M. M. P. Muir. The oil consists mainly of two terpenes, one boiling at 152·156° the toher 162·167°, an oxidised liquid and a camphor.—A small quantity of absolutely pure sage oil has been examined, and consists mainly of a terpene boiling at 264·270°, of a dark emerald green colour.—On the action of bromine upon sulphur, by J. B. Hannay. The author has investigated the evidence as to the existence of any compounds of these two elements by boiling points, the spectrum of the vapour, specific gravity, and vapour tension. He concludes that the action of any quantity of bromine or any quantity of sulphur is an action on the whole mass and not in multiple proportion, but that if at low temperatures the compound containing one atom of sulphur to two of bromine meets a body with which it can form a molecular combination, *e.g.*, arsenic, it assumes the crystalline form in conjunction with such a body.—On the determination of high boiling-points, by T. Carnelly and W. C. Williams. The authors have determined the boiling-points of various substances by observing whether or not certain salts fuse when exposed to the vapour of the boiling substance. The melting-points of the salts have been determined by Carnelly. The salts are contained in capillary tubes.—On high melting-points, Part IV., by T. Carnelly, D.Sc. The author has perfected his (specific heat) method of determining melting-points, and eliminated two sources of error. In the present paper he gives the melting-points of over one hundred substances. He promises a paper embodying theoretical results deduced from the above observations.

PARIS

Academy of Sciences, May 27.—M. Fizeau in the chair.—The following among other papers were read:—On the production and constitution of chromised steels, by M. Boussingault. This memoir gives experiments proving that chromium, without the presence of iron, does not communicate to pure iron the properties of steel; analyses of cast chromium steel; experiments on the temper, and resistance to shock and traction, of chromised steel; mode of preparation of it and ferrochrome, &c. —On the action of anæsthetics on the respiratory centre and cardiac ganglions, by M. Vulpian. In chloralised dogs faradisation of the upper cephalic segments of the cut vagi stops the respiratory movements just as in dogs not anæsthetised; but whereas, in the latter, the respiration in general easily and spontaneously commences again, spite of the electrisation being continued, it is not so with the former, and the animals die unless electrisation be stopped and artificial respiration be produced, aided, it may be, by energetic faradisation of the trunk. The heart, too, may finally stop in such a case. M. Vulpian thinks this explains certain accidents in clinic anæsthesia.—On the origin of the excito-sudoral nerve-fibres in the sciatic nerve of the cat by M. Vulpian. Those in the abdominal cord of the great sympathetic come from the spinal cord, chiefly by the first and second lumbar nerves; but there are others, and more, which come directly from the spinal cord by the roots of the sciatic nerve. An analogy with the nerves of the salivary glands is indicated.—M. de Lesseps gave details of the pacific conquests, made in the name of the Khedive of Egypt, by Gen. Gordon, and quoted from an official Egyptian report on Capt. Burton's recent important discoveries in Arabia.—Transparent hydrated silica and hydrophane opal, obtained by action of oxalic acid on alkaline silicates, by M. Monier. The experiment should be made with 500 to 600 grammes of silicate of 35° or 40° B; the oxalic acid is diluted to only four degrees. Letting the acid act six months at ordinary temperature, a transparent silicious layer was obtained, which, after heating to expel hygrometric water, took the milky colour and the hardness of opal. It becomes translucid again in water.—On the cost of establishment of lightning-conductors, by M. Melsens. He proves that his system of numerous free conductors and multiple earth-connections is generally less expensive than the construction of the ordinary lightning-conductors.—On a disorder, not hitherto described, of wines of the south of France called *vins tournés*, by M. Gautier. This appears after warm and rainy autumns. The wine becomes troubled, its surface irisated; the colouring matter passes from red to violet-blue, and is precipitated, the supernatant liquor being yellowish-brown, and having a baked odour and an acidulated and slightly bitter taste. These changes are worked by a parasite which appears in filamentous form in the deposit.—On the production of the luminous sensation, by M. Charpentier. Where we find less red substance in the retina, we observe a less luminous sensibility, and wherever the red appears in excess this sensibility is exaggerated. It is concluded that the luminous sensibility, defined as the simple and original reaction of the visual apparatus to all luminous excitations of whatever nature, is in relation to the degree of photo-chemical action exercised on the red of the retina by all the luminous rays.—On the physiological properties of conine, by MM. Bochefontaine and Tiriakian. Conine pure, or bromhydrate of conine, is not a very formidable poison, and not to be compared with hydrocyanic acid (as has been supposed). 65 centigr. of pure conine introduced under the skin of a dog weighing 7 kil. odd killed it in a little over twelve hours; 50 centigr. sufficed for a similar dog when introduced into the stomach. The chlorhydrate and bromhydrate are always more active than the pure conine. M. Mourrut has separated from the conine furnished as pure in shops a resinoid matter, which, like curare, paralyses the motor nerves.—*Rôle* of auxiliary acids in etherification; thermal experiments, by M. Berthelot.—On some peculiarities presented in the arrangement of fire-damp in pits and old works, by M. Coquillion.

CONTENTS

ERRATA.—In Prof. Lankester's review of Balfour's "Elasmobranch Fishes," vol. xviii. p. 114, 2nd column, line 22 from top, for *homogeneous* read *homogenetic*. In Dr. Siemens' letter on the microphone, p. 129 1st column, lines 25 and 28 from top, for *corpuscular bodies* read *corpuscular matter*.