Louis Agassiz

Engraved by C. H. Jeens from a Photograph.

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NATURE

A WEEKLY

ILLUSTRATED JOURNAL OF SCIENCE

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NOVEMBER 1878 to APRIL 1879

" *To the solid ground*
Of Nature trusts the mind which builds for aye."—WORDSWORTH

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" To the solid ground
Of Nature trusts the mind which builds for aye."—WORDSWORTH

THURSDAY, NOVEMBER 7, 1878

SANITARY ENGINEERING

Sanitary Engineering. By Baldwin Latham. Second Edition. (E. and F. N. Spon, 1878.)

IN the introduction to this book great stress is laid on the necessity for sanitary measures being thoroughly carried out in all towns and dwellings; one might suppose that this was fully admitted on all sides, but we have no doubt that every medical officer of health throughout the country could easily give numberless instances of the greatest possible neglect and callousness on the subject. While all admit the necessity of efficient sanitary works and are generally quite ready to attribute to defective arrangements illness occurring in a neighbour's house or another town, each individual seems to ignore the possibility of a terrible punishment falling on him for his own neglect. He should recollect that the punishment which must sooner or later overtake him cannot be moderated by the clemency of a chairman of Quarter Sessions or the gentler feelings of a jury, but is ruthlessly administered by the inexorable laws of nature.

Mr. Baldwin Latham doubtless finding it almost useless to preach to people on the necessity of taking care of their own and their neighbours' health very wisely tries an appeal to them through their pockets, and shows the amount of actual pecuniary saving from improvements in the sanitary condition of a community. The town of Croydon is taken as an instance; in this case the average mortality from 1848-55 inclusive was 24·03 per thousand, while that for the twenty years since 1855, when the sanitary works were nearly completed, has averaged 19·56, showing a saving of 4·47 per thousand. But this is not all that is to be looked for; there is evidence that at times the mortality of Croydon proper is considerably increased by an impure water supply, and from the lower mortality of Norwood it appears that a still further improvement could be obtained by the adoption of suitable measures. Multiplying the average saving in the rate of mortality by the population and by the assumed value of labour per head taken at 19*l.* 10*s.* per annum after a deduction of nearly one-half for persons of an unsuitable age for work, the author obtains 413,395*l.* as the value of the saving from the lessened number of deaths in ten years on an average population of 43,912. The saving in cases of sickness not resulting in death is taken at 1*l.* per case on twenty-five times the number of deaths, that being the estimated ratio of cases of illness to deaths; this gives 98,150*l.* as the result, and to this is added the cost of funerals saved, 3,926 at 5*l.* each equal to 19,630*l.*, or a total saving of 531,375*l.* It would appear to us more correct to leave out this last item, as the expense though saved for the present must be regarded as a deferred charge and must be incurred sooner or later. The works having cost in this case 267,665*l.* there remains as a dividend for the twenty years an amount in the aggregate equal to nearly twice the capital. This in the days of discarded gas and failing banks ought, unaided by the arguments of zymotic disease, to persuade the ratepayer to seek an investment in sanitary progress.

A great number of very useful tables are embodied in the text; those of the velocity and flow in pipes and sewers from p. 91 to 153 will be found of great service to the sanitary engineer, being calculated over a much greater range than in other books on the subject, and having been extended in the present edition. We should suggest with reference to tables 29, 30, 31 that a very useful addition to make in a future edition would be a table of areas and hydraulic mean depths with other fractional depths of flow besides one-third and one-half full, and that the use of the velocity and discharge tables would be facilitated by giving the corresponding fall in feet per mile side by side with the given rate of inclination expressed in a numerical ratio.

A chapter is devoted to the question of the admission of rainfall into sewers; the reasons for its exclusion as far as practicable are stated to be (1) to increase the manurial value of the sewage; (2) to obviate the inconvenience attending the purification of a large and uncertain volume of sewage in times of rainfall; and (3) to give to the streams of the country the natural volume of water due to the rainfall within their collecting area, and the adoption of this course receives but partial recommendation. We believe the author has omitted by far the most important reason, viz., the deposit of heavy road detritus caused by the admission of storm waters, which retards the free flow of the sewage and retains a mass of decomposable matter in the sewers quite sufficient to account for the abominable effluvium emitted by the gullies and ventilators of the London system. A reason advanced for the admission of surface water into sewers given by the author, and to which much weight is attached, is the fact that it was found by the analyses of Prof. Way that the washings from the streets of London resulting from rainfall were equal in impurity to average sewage. If the surface of our streets is permitted to become so filthy that, even when diluted with rain-water, the product is only suited for admission into the sewer, it would surely be better to turn more attention to the collection and carting away of the filth rather than to get it washed away out of sight where its presence will not be remarked until the next dry weather renders it painfully apparent. It is somewhat to be regretted that the author has not devoted a small amount of space to a subject having so important a bearing on the sanitary condition of a town and the successful operation of a system of sewerage as the scavenging of streets, especially when we consider how much improvement in this respect is needed in the metropolis. We believe that in London and other large towns, the saving in the destruction of clothing would at least pay for the proper cleansing of the streets without making any allowance for the saving of time and discomfort in locomotion.

The chapters dealing with the properties of materials and the construction of sewers, contain much useful information derived from the author's own experience and other sources, and may be consulted with much advantage by those engaged on works not only of this, but of other descriptions.

The much vexed question of sea outfalls and the influence of tidal currents on the selection of site is discussed, but it seems a pity that where ignorance and prejudice demand this mode of disposal, the author should not have laid stress upon the necessity of abstracting much of the solid matters held in suspension, thus much diminishing what is becoming an intolerable nuisance in many sea-side places. In giving so much importance to dilution with tidal water, it should have been borne in mind that this takes place in the direction of the breadth and depth of the volume of liquid discharged; but in the case of the solids floating on the surface, only in the former direction, and in both cases very slowly, as may be seen by an inspection of the metropolitan outfalls. It is impossible in the space at our disposal to notice the numerous details described and illustrated; the plates of all the more important are carefully drawn and well executed, no trouble being spared to make them thorough working drawings, while a sufficient number of woodcuts are introduced amply to illustrate the text. There are other books on this subject of a more popular nature, containing most of the information required by those who desire a general knowledge of the subject freed from too technical matters, but this is undoubtedly the best book hitherto published in this country for the student of practical sanitary science and for the engineer who requires a thorough treatment in detail of that branch of his practice.

THE NAPLES ZOOLOGICAL STATION

Mittheilungen aus der zoologischen Station zu Neapel, zugleich ein Reperatorium für Mittelmeerkunde. Erste Band, I. Heft. (Leipzig: 1878.)

SINCE the foundation of the Zoological Station at Naples, nearly one hundred naturalists have worked in the laboratory connected with it, and a goodly number of papers, which have resulted from their labours, are scattered through the biological periodicals of almost all the civilised nations of Europe. Gratifying as this success must be to Dr. Dohrn, the founder of the institution, he does not show himself inclined to repose on his laurels, but aims at still further extending the scope of the station by starting two publications in connection with it. One of these, of which we have the first number before us, is published in octavo size, and, as we learn from the preface, is intended for smaller papers, and general notes on the habits of animals living in the Aquarium, and other zoological topics. It will, moreover, be the medium for recording the systematic observations now being carried on by the permanent staff at the station. The second publication will be in quarto size, and will bear the title "Fauna u. Flora des Gulfes von Neapel und der angrenzenden Meeresbezirke." As its name indicates it will consist of fully illustrated monographs of the various groups of animals found in the Bay of Naples or adjoining seas. The parts may be purchased separately, or may be subscribed for by the payment of 1*l*. yearly. The contents of the first part of the "Mittheilungen" promise very well. Dr. Schmidtlein, who manages the public aquarium, contributes three short papers. One of them gives an interesting account of the habits of a large number of the various animal forms living in the aquarium. A second deals with the periodic appearances of pelagic animals in the Bay of Naples during the two past years, and the third is a list of the breeding times of the marine forms inhabiting the Neapolitan seas. Dr. Hugo Eisig, the general manager of the station, contributes a paper of very great importance on the segmental organs of the Capitellidæ. He shows that, in some species of this group, it is normal for several segmental organs to be present in a single segment, and that the number of these organs present in a segment increases in passing from before backwards. Dr. Eisig compares the segmental organs in Annelids with the segmental tubes in Vertebrata, and points out how closely the arrangement he has found in the Capitellidæ agrees with that described by Dr. Spengel in some Amphibia. There is an illustrated paper by Dr. Meyer on some points of crustacean anatomy, and two botanical papers by Drs. Falkenberg and Smitz. Dr. Dohrn himself communicates some observations on the Pycnogonidæ, in which he adduces a large amount of evidence to prove that the view as to the number of their appendages put forward by him some years ago, which was subsequently attacked by Semper, is, in all essential points, correct.

The number as a whole is very creditable to the zoological station, and we may congratulate the founder upon the continued prosperity of the institution, as evinced by its ever-increasing activity in all directions.

F. M. B.

OUR BOOK SHELF

Theorie der algebraischen Gleichungen. Von Dr. Jul. Petersen. xii. and 335 pp. (Kopenhagen, 1878.)

THE author tells us that this work owes its origin to the lectures he has given on the theory of equations at the Copenhagen Polytechnic School. In the preparation of it he has made use of J. A. Serrêt's "Cours d'Algèbre Supérieure," Todhunter's "Theory of Equations," and Jordan's "Traité des Substitutions." The first section treats of equations in general; Cap. I. general properties of algebraic equations; Cap. II. relations between the coefficients and roots; Cap. III. on elimination, describing the methods of Labatie, of Euler, of Sylvester, of Bezout, and of Poisson; Cap. IV. the transformation of equations. The second section is devoted to the algebraic solution of equations, viz., of the cubic (the methods of Hudde, Lagrange, Tschirnhausen, and Euler); of the biquadratic (the methods of Lagrange, Descartes, and others); the binomial equation, the Quintic, the breaking-up of a rational polynomial into rational factors, Abelian equations (a long chapter, including the division of a circumference into seventeen equal parts, and the reduction of the equation $x^{13} = 1$).

The third section is on the Numerical Solution of Equations: Cap. I., on the Separation of Roots (Descartes', Budan's, Rolle's, Sturm's, and Newton's theorems); Cap. II., the Calculation of the Roots in Numerical Equations (interpolation, of Newton's Method of approximation, also Lagrange's and Horner's methods). The fourth part, which treats of Substitution in four chapters: Cap. I. Substitution in General; Cap. II. (a long chapter, including the theorems of Lagrange and Cauchy, alternate, transitive, and intransitive groups, linear substitutions, &c.); Cap. III. Galois' Theory (this has not found its way into English text-books; Prof. H. J. S. Smith classes Galois, for early precocity, with Pascal and Gauss); Cap. IV. Applications of Galois' Theory (Abelian equations, the Galois and the Hessian equations).

This bare enumeration of the principal articles will show that this carefully-written treatise takes up some ground which has not yet been opened out or even alluded to in our common text-books on equations.

The Botany of Three Historical Records, Pharaoh's Dream, The Sower, and the King's Measure. By A. Stephen Wilson. (Edinburgh: David Douglas, 1878.)

THIS is a curious little book, the author's aim being to throw what light he can, either by comparison or suggestion, upon the probability of the plants referred to in these Scripture records being this or that species of cereal. Mr. Wilson seems to have given a good deal of consideration to each of the above questions, which, as he says in his preface, have only one bond of connection between them, namely, "a common basis in the botany of the cereal grasses." Notwithstanding the pains the author has evidently given to each of the subjects, we cannot but think that it will prove of but little value, the points advanced being by no means conclusive, and even the subjects in themselves being of small importance. It may be of some value to know whether the cereals "stand in the same alimentary relationship to mankind as they did when Joseph laid up the surplus of the plenteous years in the granaries of Egypt," because such a knowledge, if it could be proved, would show the progress made in developing the productive resources of these grasses, but whether the plant in Pharaoh's dream was *Triticum compositum*, or any other species of *Triticum*, is perhaps of little moment to mankind at the present time. As an illustraiton of what is to our mind mere speculation, we quote the following from p. 6:—"The wheats of 'Minnith,' in the Belka (Ezek. xxvii.) grown by the farmers of Judah and Israel, seem to have been in demand in the corn-market of Tyre. Probably Minnith was a remarkably good locality for wheat, so that when the husbandman in other districts got seed from this place they called it Minnith wheat."

The author's summing-up of this his first "Historical Record," namely, that "seven ears of corn came up upon one stalk," is that it "may be wrong, and probably is wrong, whereas the reading here proposed, that seven ears of corn came up upon one *stock*, while probably expressing the full meaning, can only err by defect, and must necessarily be right, as embracing an essential morphological fact common to all varieties of corn."

The Commercial Products of the Sea; or, Marine Contributions to Food, Industry, and Art. By P. L. Simmonds. With thirty-two illustrations. (London: Griffith and Farran, 1879.)

THIS is the first example this year we have had of a work antedated, in this case by more than two months. We cannot possibly see what is gained by this; is it meant to make readers of future years believe that a work was published a year later than it really was? If this is so, is it quite honest and respectable—to put it in the mildest possible form? When one gets over Mr. Simmond's extraordinary and often misleading style (for which we commit him to the tender mercies of the literary Dr. Birch), it is found that his work contains a great mass of useful and curious information, showing great diligence in the collection of facts, if not much skill in putting them together. Mr. Simmonds' work is divided into three parts, dealing with food-products obtained from the sea, marine contributions to industry, and marine contributions to art. Detailed accounts and statistics are given of the various fisheries of the world, under the first head; under the second head the sponge fisheries are dealt with, oils, isinglass, shells, seaweed, marine salt, and other products; and under part iii. tortoise-shell, mother-of-pearl, coral, and amber. It will thus be seen that the work has a wide range; it shows how much has been done, and how much yet remains to be done by science, to make the most of the products with which the waters swarm. Altogether the work contains much useful and interesting information in a handy form.

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.*]

A Question raised by the observed Absence of an Atmosphere in the Moon

IT is known that there is physical evidence of an absence of atmosphere in the moon. It would appear reasonable to conclude that the moon at one time had an atmosphere; for, according to the generally-accepted principles of Laplace, which make the sun and members of the solar system to have a common nebulous origin, it would seem very extraordinary if the particular offshoot of the common nebula which formed the moon had no gaseous constituent in it. If we admit, therefore, as probable that the moon at one time had an atmosphere, the question naturally suggests itself as to what has become of it. Various surmises have been hazarded in reply to this. I would venture to submit the following as a possible explanation, which, as far as it goes, is based on accepted principles:—It is known to be a demonstrated fact in connection with the established kinetic theory of gases that the velocities of the molecules of a gas vary among themselves from zero to an *indefinitely* great velocity, *i.e.*, a velocity to which apparently no limits can be set. It is true that the molecules which in the accidents of collision among themselves acquire these enormous velocities, have been mathematically proved to be relatively few in number, the greater number of the molecules possessing velocities approaching the mean value. But it would seem to follow necessarily that molecules situated in the top stratum of any atmosphere, and which acquire these enormous (indeterminable) velocities, can sometimes overcome gravity, and be projected into space, so as not to return; as it is a known fact that only a *finite* velocity is required to effect this result. I have therefore to suggest that by this cause the moon's atmosphere has gradually disappeared. It is probable, no doubt, that it would take a vast period of time to have brought about this result, but we have an almost unlimited time at disposal. It might possibly be asked, How is it that the earth's atmosphere has not shared the same fate? In answer to this I would reply, first, that the value of gravity on the earth is known to be very much greater than on the moon, and second, that possibly (for aught we can tell) part of the earth's atmosphere may have thus disappeared; or the earth's atmosphere may be less dense at present than at one time, for anything we can say to the contrary. It would seem a curious fact to note in connection with this that there would be apparently grounds for inferring that the *constitution* or composition of the earth's (or any other planet's) atmosphere might have changed from the above cause, as evidently the lighter gaseous constituents, whose molecules acquire in the accidents of collision the highest velocities, would be first dissipated into space in the above manner. Thus, for example, any trace of that very prevalent constituent of the universe, hydrogen, that might have at one time existed in the earth's atmosphere, would have tended to become relatively rapidly eliminated, as the molecules of hydrogen are known to possess a normal velocity about four times as great as that of the constituent molecules of the earth's atmosphere.[1] It might be said that changes so great as those above indicated are scarcely realisable, but then it should be kept in view that we have an almost limitless range of time to draw on, and it is generally admitted to be very important to take the effect of time into due consideration, as, for example, is done in the case of geology, where mountain ranges are recognised by incontrovertible physical proof to have been carved out by the slow disintegrating action of rain and atmospheric influences prevailing through countless centuries. The gradual disappearance of an atmosphere (earth's or moon's) under the above cause might possibly be compared in slowness of operation to the other cosmical changes that the solar system is known to be undergoing, such as the gradual approach of the earth to the sun (and of the moon to the earth) through the friction of the material media in space, the accomplished stoppage of the moon's axial rotation by tidal action on its mass, and the gradual diminution of the earth's rotative velocity from the same cause. These slow changes, imperceptible in the range of human experience, become important in large time epochs, and it becomes desirable in the interests of truth, in tracing back events, to give due weight to these time epochs. In suggesting the above explanation, I have endeavoured to confine myself strictly within the limits of mathematically proved facts as a basis to draw deductions upon, and I should be glad to accept any criticisms that might be offered, either with the view to point out a difficulty or confirm the truth.

London, October S. TOLVER PRESTON

Remarkable Local Colour-variation in Lizards

THE following extract from a letter received some months since from Baron de Basterot, of Rome (a Fellow of the Geological Society of London), records an interesting case of local colour-variation, about which some of your correspondents may be able to give us further information:—

"Capri is a mass of the usual yellowish-white Apennine limestone, forming precipitous cliffs nearly all round the island. At its southern extremity are three high and nearly inaccessible rocks called I Faraglioni, one of which, pierced by a natural arch, has been frequently depicted by artists. Two of these rocks are completely detached from the mainland, and, I need hardly add, uninhabited.

"On the island, and on the first of the Faraglioni rocks which is connected with it, the lizards are of the usual species so common in Italy—coloured grey, mixed with more or less green. On the two outward Faraglioni rocks, which are completely separated from the shore, their colour is totally different. The back is of a blue so dark as to appear nearly black; the sides of a brilliant blue, like lapis-lazuli; the belly light whitish-blue, with a very slight tinge of green.

"An English gentleman whom I met in Capri had several of these lizards alive, which had become quite tame in the course of a couple of months. I believe he intends bringing them to England. He is of opinion that they differ in colour only from the lizards of the island, and that, though very different in appearance, they are the same species.

"Whether this be so, or whether they are specifically different, their presence on these isolated rocks and their total absence on the island is equally remarkable."

ALFRED R. WALLACE

Termites kept in Captivity by Ants

WHEN entomologising in Portugal in 1877, in the neighbourhood of Cintra, I found the nest of *Formica nigra* under a stone. On my turning it over there was, as usual, great consternation in the community, and I discovered that it was evidently caused by the fear lest a colony of *Termes lucifugus*, which the Formicas had enslaved, should escape. The Nigras instantly began seizing the Termites, driving them underground by the nearest orifices, in the meantime wrenching and pulling off their wings in the most unceremonious manner. I observed a large number of wings lying in heaps here and there in the nest as if this treatment had been practised before. In the nest there was also a great number of Termite larvæ. The great object of the owners of the "location" seemed to be to get these larvæ underground as speedily as possible. The ants fell on them with great impetuosity, seizing them anyhow and anywhere, dragging them against the most strenuous opposition (their behaviour strikingly contrasting with the meekness of their winged fellows) into the nearest apertures of the underground home. Very often this opposition resulted in a long and stern fight, in which the larvæ were often badly wounded, being deprived sometimes of their antennæ, sometimes of half their jaws, and not seldom killed outright. Occasionally, however, the larvæ were victorious, beating off the Formicas, in which case they (the larvæ) did not make off, but remained perambulating the nest. I saw one larva drawn at the end of a long fight by its antenna, while it strenuously held on to a small ball of earth which had proved a vain anchorage for its feet, for larva and clod together were dragged across the top of the nest (made by the impression of the stone) five or six inches, up the side, $1\frac{1}{2}$ inch, and away among the grass, where, losing the ball of earth, it seized a stalk so firmly that its abductor could not drag it farther, whereupon, after reconnoitring the ground for a little distance the latter disappeared, but returned shortly with a companion, with whose aid the larva was detached. This done, the helper returned home while the abductor proceeded with his prisoner till lost to view in the grass, some twelve or fourteen inches from the spot whence it originally started.

In the same neighbourhood I watched for some time a nest of *Formica ligniperda*. An injured female was placed in the nest, but no assistance was rendered, while it crawled along towards the nearest orifice leading underground. At the spot where this individual was injured some of the fluid of its body which had oozed out was eagerly lapped up by the others; some even applied their mouths to the wounds on the body. During the operation of lapping the maxillæ were kept perfectly still, and the antennæ close to the side of the head "feel-feeling" the ground with the tips, as if to discover the spot where the liquid was to be found. Every now and then, however, they were extended at right angles to the body, as if to obtain a more general survey of things, and then immediately returned to their previous position. On several of those which were busy lapping I poured some spirits of wine. They instantly became stupefied, and for a time motionless. When in this condition they were

[1] The realisation of a possible diversity at a former epoch in the constitution and density of the earth's atmosphere raises rather a curious question in connection with the known diversity of the plants and animals that formerly inhabited the globe, as compared with those at present existing. It might be observed that admitting the possibility of the former existence of an atmosphere on the moon, it would seem to follow that an interchange of molecules between the two atmospheres (those of the earth and moon) must have taken place at one time to a certain extent under the above cause, though the considerably less value of gravity on the moon compared with the case of the earth would facilitate the passage of molecules away from the moon and render correspondingly difficult the passage in the reverse direction.

visited by many of their fellow colonists, who, having cursorily examined them, fell to touching them with their antennæ on the abdomen, reminding me much of a mesmerist making passes over a victim. The effect was almost electrical. I was surprised to see the incapables at once begin to rally. After stretching their legs and moving their antennæ they moved along slowly for one or two steps and then went along as if nothing had happened. Others came and drank of the spirit not quite evaporated, but did not seem to suffer any bad effects. I buried a member of the community as it was in the act of carrying off a larva. Although many came and looked on none took compassion or attempted to relieve their friend. A small heap of larvæ, however, which I pressed down into the soft earth with my pencil, thereby injuring some of them badly, was disinterred, and every individual carried into a place of safety.

A stranger placed in the nest was very soon set upon, and before long its head was travelling on a direction opposite to that of its abdomen. The headsman's reward was a long draught of blood from the severed abdomen.

On my turning over the stone at first, the larvæ were exposed, but were soon begun to be carried off. Some of the workers were certainly busybodies, fussing about, pretending to do a great deal, while in fact they were shirking their fair share of the household duties. They would rush at the larvæ, seize one and be off with it in a great hurry, but they had not gone far (not even always in the direction of the entrance) before they changed their minds, threw down their load to return for another helpless infant, which was treated in the same way, being carried generally in a direction contrary to the previous, and dropped down anywhere, sometimes beyond the limits of the nest altogether.

My observations with regard to ants dropping intentionally or jumping from small heights do not quite agree with Sir John Lubbock's, but they are not yet full enough to give in detail. I hope to have fuller opportunities for the investigation of the habits of this most interesting class in the Malayan Archipelago, whither I am now bound.

Meantime I hope these few notes may have some interest for the readers of NATURE. HENRY O. FORBES

S.S. *Celebes*, off Naples, October 18

Colour-blindness

HAS it been suggested that the traditional blindness of Homer may have been—in the absence among the ancients of a specific name for colour-blindness—*merely* the colour-blindness for which Dr. Pole makes out so good a case? To readers ignorant of Daltonism, blindness must have appeared the only explanation of a glaringly misapplied colour-epithet. It is at least clear that the author of the Homeric poems was not always blind in the modern sense of the word.

Brighton, November 1 CLEMENTINA BLACK

THE conclusion of Dr. Pole's valuable paper will doubtless stir up many to investigate the question whether or not dichromatism was the rule at an early stage of human vision.

Will you allow me to adduce, towards the solution of this question, the evidence of a literature, which though not nearly so ancient as the Greek, goes back further than that of many European nations. I mean the Irish. I find in some of the earliest works in that language an ambiguity in the application of adjectives of colour very similar to that noticed in the Homeric writings by Mr. Gladstone. *Glas*, for instance, is used, indifferently, apparently, for green, grey, and blue. *Uaithne* is used to indicate the colour of grass, and also that of the human eye. *Dearg* is employed to denote the colour of wine, and also that of clay. *Ruadh* (red) is similarly ambiguous.

182, Adelaide Road, N.W. EDMUND MCCLURE

Carrier-Pigeons

IN NATURE (vol. xviii. p. 682) it is stated that carrier pigeons are being "turned to useful account" in a new direction in Germany, for Consul Ward writes to the Foreign Office "that the successful results attained by the establishment of communication between the two Eider lightships and the Port of Tönning, in Schleswig, by these means has led to its organisation" elsewhere. This mode of communication is, however, not new, as carrier-pigeons were employed early in this century as a means of communication with the Bell Rock Lighthouse, as mentioned in my late father's "Account" of that work. The pigeons passed between the lighthouse and the shore—a distance of eleven miles in eleven minutes. The employment of these birds, however, was, I suppose, found to be more curious than convenient, for they have long since ceased to be employed. The pigeons were presented to the establishment by the late Sir Samuel Brown, R.N. THOMAS STEVENSON

Edinburgh

Globular Lightning

As the curious phenomenon known by the above name seems to be attracting some attention just now, I venture to send you the following details, which, though of rather ancient date, are still, owing to their startling character, very fresh in my memory.

I think it was in the year 1866, in the beginning of the month of August, that I was walking in the garden when the atmosphere became exceedingly oppressive (there had previously been a very long drought), and thinking by the appearance of the sky, which looked lurid and threatening, that a storm was coming on, I made for the house. As I was going up our front steps some rain-drops fell, which were the largest I ever saw. I had just reached the dining-room and was standing near the window, which looks north, when I saw a large ball of fire, which appeared to me, looking at it as I did from a distance, to be the size of a globe such as is used in schools, descend towards the earth. In descending it struck the church, which is immediately opposite our house, and brought with it a number of slates and part of a stone cross, making a terrific noise. There was a flash of lightning soon after, followed by a moderately loud clap of thunder, but nothing more. As there were not at that time any houses near to ours I did not hear the occurrence mentioned by any one. The noise, though extremely loud, was not at all like thunder. The illumination of the rooms by the ball of fire was seen by two other persons in the house. CHARLOTTE HARE

St. John's Road, Putney, S.W.

Speaking-Trumpets

THE antiquity of the speaking-trumpet may be proved upon far higher authority than that of the imaginative Athanasius Kircher. It is literally as old as the Pyramids. While examining Lepsius's great work upon ancient Egypt for my "History of Music" I noted two examples among the plates of the fourth dynasty of Egypt (see Lepsius's "Denkmäler," Dyn. 4, Abt. 2, Blätter 27 and 30). The Egyptian speaking-trumpets seem to have been some five feet or more in length, and too wide in diameter to have been blown by the mouth. They are conical, and lack the contraction near the mouth-end which is so observable in their war-trumpets.

WM. CHAPPELL

Toughened Glass

MY own experience supports the necessity for caution in using Bastie's toughened glass. Shortly after its introduction I had some graduated measures, and although they were sufficiently tough to bear the shock consequent on falling five or six feet to the ground, yet after a time some short scratches appeared on their surface, and these rapidly spreading till they nearly covered the whole of the glass, when but a slight touch was sufficient to make the measure fly into fragments. One placed on a shelf subject to rather rapid change of temperature, without any handling or apparent cause, broke up suddenly into tiny pieces, behaving, indeed, as if it were a Rupert's bomb.

Northampton, October 29 G. C. DRUCE

POTTERY AT THE PARIS EXHIBITION

THE extensive collections of pottery at the Paris Exhibition brought together from so many countries, is of high interest from a technical, as well as from an art point of view. All that is now being done in pottery manufacture, all that has lately been achieved in the way of progress, has been here illustrated. An examination, even a rapid one, shows at once how far in advance of other countries England and France are.

Leaving all strictly art questions out of consideration, it is instructive to notice how the technical processes of manufacture impose limits on an artist's scope, and how these scopes have been widened by recent discoveries. It is not intended in this short note to do more than allude to the more important of these, and before doing so it is worth while mentioning that for domestic purposes English earthenware is still unapproached.

The *pâte-sur-pâte* decoration, so largely used in England and in France, is a good illustration of how a process in itself confines the artist's power within certain limits.

The nature of the ornamentation consists in applying by the brush, and modelling with tools, raised decorations of "paste," which is often, for the sake of artistic effect, in high relief. The paste is of much the same composition as the body on which it is applied, and requires a similar temperature to convert it into China, *i.e.*, 1,800° C. The colours which can be used for staining this paste must therefore also be capable of being produced at this heat, and the result is an entirely new range of ceramic colours. It is fortunate for the success of this style of decoration that the colours obtained are harmonious, of a subdued tone. They are quite unlike any that can be produced at a lower heat. Apart from the artist's manipulation, which may vary much in delicacy, the general effect of the production is almost wholly the natural result of the process, and is therefore not due entirely to the artist.

Another and distinct application of raised decoration is very largely represented in the French court. It was first used at Bourg-la-Reine some ten years ago, and is now made in many other localities. It consists of painting in clay on earthenware with pallet and brush in various gradations of relief, somewhat like impasto. The heat for firing is comparatively moderate, and the range of colours that can be employed is very wide.

The difficulties of painting *under glaze* are by degrees being overcome, and one manufacturer has, for the first time, produced gilding *under glaze*. The coloured glazes shown are rich and brilliant, and are well worth the particular notice of those who have paid attention to their production.

The organisation of the Sèvres manufactory and the fact that it is under the direction of a chemist of repute lead to expectations of discovery resulting from the research here carried out. And as a fact discoveries of no small value have been made of late years. Besides such discoveries as of compounds yielding new colours, there are some which take rank as new processes.

For example, the late François Richard, an artist on the staff of the manufactory, found that a large proportion of the enamel colours can be made which will bear a temperature of 600° C.—a higher temperature than has been hitherto supposed possible. This higher temperature now employed fuses and softens the glaze; the colours painted on it blend with it so that, on cooling, there is produced that softness and brilliancy hitherto characteristic of *pâte-tendre* decoration. This process has been named the *demi-grand feu*. A great benefit arising from this discovery is that many vases damaged in firing, which would formerly have been abandoned, can now be preserved, as the accidents which so often happen in firing can be repaired. Defects in glaze and colour can be concealed, as, during an exposure to this *demi-grand feu*, they are at melting-point, and new glaze, when added, becomes so fused that no line of junction is visible. This process also gets over a difficulty that had long been felt in decorating kaolin, or *pâte-dur*, china, with a pleasing result, as the colours were always crude and harsh, being *on* the glaze. When a soft effect was required recourse was had to the less durable and more costly *pâte-tendre* as a body on which to paint.

A new method for decorating porcelain where gold alone is employed has been invented by M. Réjoux. Formerly, when porcelain was decorated with gilding alone, the ornamentation was limited to the production of a pattern by the greater or less relief of the gold, and by its being burnished or left dead. Even the thinnest part of the gilding was opaque. No delicate effects could be produced, and the style was suited rather for an abundant display of barbaric wealth than for refinement of expression.

The new process enables the gilding to be put on so thinly and transparently that the most delicate effects of light and shade can be produced. It is, however, applicable only to vases of *gros bleu* colour, that is, to vases coloured with oxide of cobalt. Upon this ground the pattern is drawn with a pigment composed of oxide of aluminium. This is then subjected to a firing which fuses the oxides together, and a brown surface results. This surface is found to be more suitable than any other known for the reception of the gold paste, which can be laid on in a thin film, and then, further, by subsequent removal, can be made to give great transparency. This property of the brown surface is not destroyed by being tinted before the gilding, so that it is possible to tint it with different colours which shall show through. A further variation is very frequently obtained by changing the tone of the gold by mixing it with alloys. This admirable effect of transparency has not been produced by any other means, and the first piece made (with many subsequent) is exhibited.

Another of the processes invented at Sèvres is that of enamelling on *pâte-tendre* body. In this the colours are applied in powder in the same way as in enamelling on metal, and are fused at a very low temperature. They have more body and are more decided than are those produced by the older enamelling process. On some of the vases shown at the Exhibition the white ground seen is not that of the creamy *pâte-tendre* body, but the pearl white of the stanniferous enamel.

Other recent improvements which should find a place in a more technical and exhaustive notice have been illustrated at the Exhibition. It may be mentioned that other nations are striving to adapt some of the traditionally recognised styles and their method of manufacture.

We may, in conclusion, refer to a small but important exhibition of porcelain, *allant-au-feu*, useful for laboratory as well as for domestic use. It is a very good white, thin and hard, and will bear high temperatures if the changes are not too sudden.

SUN-SPOTS, ATMOSPHERIC PRESSURE, AND THE SUN'S HEAT

THE question whether the atmospheric pressure varies with the spotted surface of the sun was noticed by me in a paper on the Isobars within the British Isles. I could not, however, find any appearance of a decennial law in the yearly mean pressures: such a relation presented itself however in the varying directions of the isobars (*Proc.* Roy. Soc., 1877, p. 599). The yearly mean pressures in our latitudes are subject to large irregular variations, and several decennial periods would be requisite before these could be neutralised in the decennial means. As the irregularities are much smaller within the tropics, I did not fail to examine the yearly means for India which were in my possession at the time; and I found their variations very small and apparently without any relation to the decennial period. Mr. F. Chambers's interesting letter to NATURE (vol. xviii. p. 567) has induced me to make a more careful study of the Indian observations at present before me.

Mr. C. Chambers had noticed previously the appearance of "a periodicity of very small range, which nearly corresponds in duration with the decennial sun-spot period ("The Meteorology of the Bombay Presidency." London, 1878, p. 12). It is obvious, whatever may be the results derived for a single station, that they can have little value if contradicted by those obtained elsewhere. I have shown that from Singapore to Simla the *daily* mean pressure varies in nearly the same way, the maxima as well as the minima occurring simultaneously or nearly so over all India (*Proc.* Roy. Soc., 1876, p. 24). Are then the *years* of maximum and minimum pressure also the same over India? The question has considerable interest independently of the existence of a decennial law. The following table contains the differences (Δ B) of yearly mean barometric height in *thousandths of an inch* from the means derived from the whole series of observations at each station. I possess at present unfortunately only an incomplete series for Trevandrum and the variations for 1841-48, and for 1853-64 are those from the means for each of these series of years, the barometers having been different. I have also added the differences of yearly mean temperature in *tenths* of a degree Fahr. from the means for each series of years during which the thermometer preserved the same position. The different series are separated by bars ——.

Year.	Δ B				Δ T			
	Singapore.	Trevan-drum.	Madras.[1]	Bombay.[2]	Singapore.	Trevan-drum.	Madras.[1]	Bombay.[2]
1841	0	0	—	—	− 1	− 2	—	—
1842	− 4	+ 6	− 5	—	+ 2	+ 1	+ 1	—
1843	− 5	− 11	− 7	—	+ 3	0	− 6	—
1844	− 3	− 1	− 3	—	− 4	− 2	0	—
1845	+ 11	+ 19	+ 19	—	+ 1	+ 7	+ 7	—
1846	—	+ 13	+ 14	—	—	+ 5	+ 2	—
1847	—	− 13	− 8	− 11	—	− 8	− 2	− 6
1848	—	− 9	− 7	− 3	—	− 1	− 2	+ 2
1849	—	—	− 23	− 10	—	—	+ 2	0
1850	—	—	− 7	0	—	—	− 2	+ 3
1851	—	—	− 6	− 12	—	—	− 5	− 1
1852	—	—	+ 5	− 3	—	—	− 4	0
1853	—	+ 3	+ 6	+ 6	—	—	+ 2	+ 3
1854	—	− 2	+ 2	− 4	—	—	+ 4	+ 4
1855	—	+ 15	+ 20	+ 16	—	—	+ 5	+ 5
1856	—	+ 5	—	− 2	—	—	—	+ 3
1857	—	—	—	0	—	—	—	− 3
1858	—	—	—	+ 4	—	—	—	− 1
1859	—	+ 2	—	+ 4	—	—	—	0
1860	—	− 6	—	− 4	—	—	—	− 2
1861	—	− 11	—	− 11	—	—	—	− 9
1862	—	− 19	—	− 25	—	—	—	+ 4
1863	—	− 11	—	− 16	—	—	—	− 4
1864	—	+ 23	—	+ 24	—	—	—	− 3
1865	—	—	—	+ 3	—	—	—	+ 8
1866	—	—	—	+ 14	—	—	—	− 9
1867	—	—	—	+ 16	—	—	—	− 6
1868	—	—	—	+ 28	—	—	—	− 1
1869	—	—	—	+ 6	—	—	—	+ 7
1870	—	—	—	− 11	—	—	—	− 2
1871	—	—	—	− 3	—	—	—	+ 9
1872	—	—	—	− 13	—	—	—	− 3

[1] There is an error of − 20 in the mean pressure given in the Madras observations for 1846 (here corrected).

[2] The variations for Bombay are taken from Mr. C. Chambers's "Meteorology of the Presidency of Bombay," pp. 11 and 32.

An examination of the quantities ΔB will show that all the principal deviations from the mean pressures occur in the same years and have the same sign at all the stations. The agreement of the variations for Trevandrum and Bombay in the six years 1859-64 is also very remarkable.

We see that there was a maximum of pressure in 1845 at the first three stations, a minimum at Madras and Bombay in 1849 (best marked at the former station), a maximum at the last three stations in 1855; a minimum in 1862, and a maximum in 1864 at Trevandrum and Bombay (the latter station showing a maximum also in 1868). These means, then, confirm in a general way for all India the result obtained by Mr. C. Chambers for Bombay,[1] that on the whole the mean atmospheric pressure is greatest near the epochs of minimum, and least near the epochs of maximum sun-spots, though there is no exact agreement between the two classes of variations from year to year.

And as the maximum pressure in 1855, shown at all the three stations, is considerably before the epoch of minimum sun-spots, it would be difficult to conclude that the epochs of one phenomenon lag on those of the other.

I consider this result to be one of very great importance. The exactness with which the mean barometric height has been obtained with standard instruments in first-class observatories, and the general agreement of the years of maxima and minima over India, give a weight to it which cannot be extended to an element so variable with locality as the rainfall. Although the barometric variations are not sufficient to explain those of the amount of rainfall (which is so different in different years, and for the same years at the different stations), yet they give a probability to the existence of similar laws in the variations of the meteorological elements which I believe was previously wanting.

Mr. F. Chambers's note was written chiefly with the object of deducing from the barometric means evidence that the emission of solar heat is greatest in years of most sun-spots. He says: "It is well known that in Central Asia the annual variation of the barometric pressure is greater than in any other portion of the globe, and it is universally admitted that this variation is due to the great variation of temperature between summer and winter, the barometer being low when the temperature is high, and *vice versâ*. If, therefore, the absolute heat of the sun is subject to considerable variations, we ought to find the barometric pressure in Central Asia responding to those variations just as it does to the annual variations of temperature; in other words, the summer barometric minimum should be lowest in those years when the sun is hottest, and the winter maximum should be highest in those years when the sun is coldest."

As I do not admit that the annual barometric oscillation is due to variation of temperature, I do not accept the conclusion: for though there is an apparent relation between the two, within certain geographical limits, and under certain local conditions (which are altogether independent of the heat emitted by the sun), there is no evidence that the one is *due* to the other. The true conclusion is that the local conditions which are favourable to a large oscillation of the monthly mean atmospheric pressure are favourable to a large oscillation of the monthly mean temperature: always within certain geographical limits, for beyond these the relation does not hold.

The subject is, I think, a very interesting one, and merits consideration. The relation of the oscillations of the monthly means will be seen from the ranges in the following table:—

[1] They also add another epoch, 1845.

	Lat. N.	Long. E.	Ranges of Monthly Means.[1]		
			Barometer.	Thermometer.	Bar. oscillation for 1°.
	° ′	°	in.	°	in.
Singapore	1 19	104	0·064	3·1 F.	0·021
Trevandrum ...	8 30	77	0·112	4·8	0·023
Madras	13 4	80	0·299	11·8	0·025
Bombay	18 54	73	0·285	11·4	0·025
Pekin	39 54	117	0·777	52·8	0·015
Catherinenburg ...	56 50	60	0·310	64·8	0·005

I have not the means for Calcutta, but they confirm the relation shown for the other Indian stations. At Madras and Bombay the barometric oscillation corresponding to 1° F., attains a maximum, it has diminished considerably at Pekin, and at Catherinenburg it cannot be said to exist; for though the minimum pressure occurs near the epoch of maximum temperature, the time of maximum pressure may be said to extend over the seven months, October to April, during which the monthly mean temperature varies 37° F. Also at Hobarton (42° S.) the relation does not exist, though the range of monthly mean temperature is nearly 20°.

When we examine the individual cases in India more in detail, several facts present themselves which are opposed to any relation of cause and effect between the two phenomena. Thus, as regards the epochs of maximum of one variation, and minimum of the other, though both show considerable approximation to the solstices, yet there are some marked differences. At Bombay, where the barometric oscillation is greatest for a given oscillation of temperature, the epoch of maximum pressure *precedes* by nearly a month that of minimum temperature, while the epoch of minimum pressure occurs more than a month *after* that of maximum temperature. We find also that the relation of increasing oscillation of pressure with increasing oscillation of temperature which holds within certain limits for different places does not hold at the same place. That is to say, years of largest annual variation of monthly mean temperature are not years of largest variation of monthly mean pressure, and in some cases (as at Trevandrum) the range of monthly mean temperature may be nearly twice as great in one year as in another.[2]

No better example, however, of the relation of the two oscillations to *local* conditions can be given than that presented by the variations at two stations within sixty English miles of each other, Trevandrum within four miles of the sea on the western side of the Ghats, and Pallamcottah on the eastern side, nearly in the centre of the burning plain of Tinnevelly, and about 13′ north of Trevandrum. The monsoon, which commences in the end of April on the western side of the Ghats, is not felt at Pallamcottah; the annual range of temperature is thus much greater than at Trevandrum. The following are the mean ranges at Pallamcottah, derived from three years' observations by Col. W. H. Horsley, of the Madras Engineers, to whose kindness I owe them.[3] The

[1] From means of five years' observations at Singapore and Catherinenburg, six years at Pekin, twenty-six years at Bombay, eight years at Trevandrum, and fourteen years at Madras.

[2] When the twenty-six years' observations at Bombay are placed in two groups, one with the largest, the other with the smallest annual variation of monthly mean temperature, I find the following ranges of the monthly means, derived in each case from the means of thirteen years' observations:—

Ranges of	Temperature.	Pressure.
	°	in.
Maximum	12·6	... ... 0·295
Minimum	9·9	... ... 0·289
Difference	2·7	... ... 0·006

The difference of ranges of pressure for 2°·7 at Bombay at the rate of 0·025 in. for 1° should be 0·067 in. The difference of pressure found is in the right direction, but is not one-eleventh of what it should be if the two were related as cause and effect, or even if the two were exact measures of variation of a single cause.

[3] The approximate height of Pallamcottah above the sea-level is 125 feet; that of the Trevandrum barometer being 193 feet.

Trevandrum ranges are placed beside them for comparison:—

	Lat. N.	Long. E.	Ranges.		
			Barometer.	Thermometer.	Bar. range for 1° F.
	° ′	° ′	in.	°	in.
Pallamcottah ...	8 44	77 48	0·241	10·3	0·023
Trevandrum ...	8 31	77 0	0·112	4·8	0·023

It will be seen that the ranges at Pallamcottah are more than double those at Trevandrum. The consequence of this remarkable fact is that, with an equal mean pressure at the two stations, the monthly mean pressure at Pallamcottah must be 0·065 inches greater in January and 0·065 inches less in June than at Trevandrum, and that the oscillations appear independent of the laws of equilibrium of pressure in gases.[1]

That the higher pressure in January at Pallamcottah does not depend on the lowness of the temperature in that month is proved by the fact that at Trevandrum, sixty miles distant, the mean temperature for January is more than 4° F. lower, and the pressure, instead of being higher on this account, is really more than half a tenth of an inch of mercury *lower* than at Pallamcottah for the same level.

The question which is now particularly before us refers to the yearly mean pressures. As the exact height (above sea-level) and index-error of Col. Horsley's barometer are unknown to me, and the direction of mean isobars can be determined with any accuracy only from observations with standard instruments, whose heights above the sea-level have been ascertained by levelling, I have employed for their determination observations made at the three observatories of Trevandrum, Madras, and Bombay, which fulfil the requisite conditions. The following are the mean pressures and mean temperatures at the stations; I have added a near approximation to the mean pressure at Singapore.[2]

	Barometer.	Thermometer.
	in.	°
Singapore	29·904 ...	80·3 Fahr.
Trevandrum ...	29·878 ...	78·4 ,,
Madras	29·864 ...	83·0 ,,
Bombay	29·846 ...	79·0 ,,

When the latitudes and longitudes given previously are considered, it will be seen that the mean pressure *diminishes* from the equator at the rate of 0·003 inch for each degree of latitude; and that the yearly mean isobars run parallel to the equator (this conclusion is independent of the mean for Singapore). It will be seen also that the yearly mean isotherms do not lie parallel to the equator, their directions being determined chiefly by local conditions.

As the difference of latitudes of Trevandrum and Pallamcottah is only 13′, the difference of yearly mean pressures should, by the preceding result, not exceed 0·001 inches, while the mean temperatures differ by nearly 7° Fahr.[3]

We may conclude, then, that, whatever may be the apparent relation of the annual oscillations of pressure

[1] I do not enter at present into the consideration of the pressure-oscillation from 10 A.M. to 4 P.M., which appears to show a similar relation to the temperature-oscillation.

[2] The mean pressure at Singapore is corrected by + 0·020 in. for reduction to sea-level, a correction which it is believed is near the truth. The mean temperatures have received no reduction to sea-level at three stations as the heights are small, from 20 to 37 feet. That for Trevandrum has been *increased* by 0°·6 F. (height 193 feet) as an approximate reduction to the height of the other stations. The Madras barometric mean is corrected for index-error by − 0·005. See Introd. Madras Obs. 1851-55.

[3] Since by the annual oscillations 1° Fahr. is equivalent at both stations to 0·023 inches of mercury, if Mr. Chambers' analogy held, the yearly mean pressures should differ by $7 \times 0{\cdot}023 = 0{\cdot}161$ inches. The mean barometric pressure, corrected approximately for height at Pallamcottah, is very nearly the same as at Trevandrum. This is independent of the conclusions from the directions of the isobars.

and temperature, this relation does not hold for the yearly means; that is to say, it does not follow that a high yearly mean pressure should indicate a low yearly mean temperature, and *vice versâ*.

We may, however, arrive at a satisfactory answer to the original question by an examination of the variations of yearly mean temperature at the different stations. It must be remembered that the relation of the oscillations of monthly mean pressure to those of monthly mean temperature are deduced from the variations shown by a thermometer four or five feet from the ground. Does the thermometer at any station, or at a combination of stations, show a high yearly mean temperature with a low yearly mean pressure, and *vice versâ?* I have given the variations of yearly mean temperature (ΔT) at different stations, and it requires only a cursory examination of them to see that there is no such relation. There is, however, another fact of very great importance to be deduced from the values of ΔT, and that is, *the very great constancy of the yearly mean temperature* at all the stations, in spite of the known considerable variations in the amount of rain and of other meteorological results from year to year.

It may be asked how we can explain the facts which seem to relate the annual oscillations of the mean pressure and mean temperature with the independence of the variations of the yearly means. As an illustration we can suppose that with a strong wind and high temperature the height of the tide may be increased at a certain port, while a following north wind with low temperatures will diminish the height at low water; we should in such a case, especially if the temperature varied with the force of the wind, have a larger oscillation of the water with a larger oscillation of temperature: we would not, however, attribute the high tide to the greater heat; we can also conceive that the mean temperature might, in the case supposed, vary, but the level of the ocean would remain constant. Other illustrations might be suggested.

The conclusions at which I have arrived are:—

1. That the years of greatest and least mean barometric pressure are probably the same for all India.

2. Therefore, that the apparent relation to the decennial period found by Mr. C. Chambers for Bombay holds for all India.

3. That the annual oscillations of monthly mean pressure and monthly mean temperature have nearly a constant ratio in India.

4. That these oscillations depend on local conditions in the same latitude, at places quite near each other, which are independent of the heat emitted by the sun.

5. That the yearly mean isobars run parallel to the equator in India and are independent of local conditions.

6. That the directions of the yearly mean isotherms vary with the local conditions.

7. That there is no relation between the variations of yearly mean temperature and yearly mean pressure.

JOHN ALLAN BROUN

THE SIZE OF THE TIGER

IN a work on the tiger, published in 1875,[1] I made the following remarks in reference to the size of the animal:—

"The size of the tiger varies: some individuals attain great bulk and weight, though they are shorter than others which are of a slighter and more elongated form. The statements as to the length they attain are conflicting and often exaggerated; errors are apt to arise from measurements taken from the skin after it is stretched, when it may be 10 or 12 inches longer than before removal from the body. *The tiger should be measured from the nose along the spine to the tip of the tail as he lies dead on the spot where he fell before the skin is removed. One that is* 10 *feet by this measurement is large, and the full-grown male does not often exceed this,* though no doubt larger individuals (males) are *occasionally* seen, and I have been informed by Indian sportsmen of reliability that they have seen and killed tigers over 12 feet in length. The full-grown male Indian tiger, therefore, may be said to be from 9 to 12 feet or 12 feet 2 inches, the tigress from 8 to 10 or *perhaps in very rare instances* 11 *feet* in length, the height being from 3 to 3½, or, rarely, 4 feet at the shoulder."

[1] Royal Tiger of Bengal, pp. 29, 30.

The point I now especially desire to elucidate as it has been the subject of discussion, but is one that has never yet been satisfactorily settled, is the greatest length the tiger attains.

Jerdon and others say that the average size of a full-grown male tiger is from 9 to 9½ feet in length; and Jerdon remarks that he has not seen any authentic account of a tiger that measured more than 10 feet and 2 or 3 inches.

I agree with Jerdon that 9 to 9½ or 10 and 2 or 3 inches are the lengths attained by the majority of tigers met with; but the occasional occurrence of tigers of upwards of 10 feet 2 or 3 inches (the authenticity of which is doubted) is attested by the evidence of several competent and reliable observers, who are quite aware that the measurements should be those of the animal as he lies where he fell, and before being despoiled of his skin, and that measurements of the skin after removal are deceptive.

I have taken some pains to ascertain the views of those who are most likely to be well informed on the subject, and I add the results of my own observations during considerable experience in Bengal, Oude, and Nepal; it would seem that the evidence wanted by Jerdon is forthcoming, and that tigers above 10 feet 3 inches, 11 feet, and even 12 feet, *are* occasionally met with, and *have been accurately measured.*

I may remark that it is very possible that like boars, and other animals, they may differ in size according to locality, food, and other conditions of life; and that such being the case, it is probable that tigers of one province or district may exceed those of another in size. Indeed I am inclined to believe that such is the case, and that therefore those who contend for the larger may be equally right with those who maintain the smaller measurements. I am rather inclined to agree with Mr. C. Shillingford, who suggests the possible progressive degeneration of the tiger; what, certainly, according to some, obtains in the case of stags in the continuously over-shot deer forests of Scotland, may also be going on in the tiger of the much-hunted jungles of India. However this is a mere suggestion, but be it as it may, the inches of the big tiger are, I think, an ascertained fact, for it can hardly be maintained that the authorities who vouch for it are either mistaken or misinformed, or that they do not know how to measure a tiger accurately.

Sir G. Yule, K.C.S.I., Bengal Civil Service, says: "I never had the luck to fall in with a 12-foot tiger; 11 feet odd inches I have killed twice or thrice. I have heard once, at least, of a 12-foot fellow fairly measured, and I cannot see why there should be any doubt as to the occasional occurrence of such exceptions to the general rule."

Col. George Boileau, Bengal army, says he killed a tiger at Mutearah, in Oude, that was well over 12 feet. He writes:—"I can speak positively as to the size of the tiger—his length was well over 12 feet before the skin was removed. He was, of course, quite an exceptional size, and unequalled, so far as my own experience goes, which extended over seventeen years of constant hunting after the species. My own experience of the size of tigers is that, in the female, the size runs from 8 feet to 9½ feet—the latter exceptionally large; in the male, from 9 feet to 11 feet; a well-grown adult tiger is seldom less than 10 feet in length. I speak of hunting-grounds frequented by myself (chiefly Oude and Nepal Terai), for no doubt the size varies according to locality, abundance of food, and its reverse must of course produce their usual results."

Col. Sleeman, Bengal army, says:—"I don't remember having killed a tiger measuring more than 10 feet 6 inches in his skin, but I have seen skins of tigers 11 feet 6 inches in length, and once, at Dinagepore, in Bengal, over 12 feet. I have the skin of the largest tiger I think I ever saw, and it measures 12 feet 2 inches. This tiger was killed near Jubbulpore, in Central India, by an old Thakoor sixty years of age, and I preserve the skin as a trophy of native pluck and vigour in age."

The skins above alluded to were, no doubt, stretched, and therefore do not prove more than that they were taken from large animals, which may have been probably between 10 and 11 feet in length!

Col. J. Macdonald, Bengal army, Revenue Survey, says:—"The largest tiger I have ever measured out of seventy was 10 feet 4 inches, and out of all these only three have touched 10 feet. But I do believe that tigers have exceptionally reached 12 feet." "The skin of a tiger ten feet in length, as he lies dead, would stretch to nearly twelve feet, but after curing it returns to nearly its normal size. I have often measured the distance between a tiger's marks on the ground; average and large animals are from 4 feet 4 inches to 4 feet 8 inches; well! I once found marks 5 feet 10 inches apart, this must have been the mark of a gigantic beast—the breadth of the impression of the fore paw, and the depth of the impression, showed his great size and weight. This was in the Sunderbunds. Mr. M., of Morel-Gunge, told me that once when going through a narrow creek in the Sunderbunds, he saw a stupendous brute, far exceeding in size anything he had before seen in tigers or could have believed possible. The heaviest male tiger I have seen weighed 448 lbs., the lightest, a tigress, 242 lbs."

The Hon. R. Drummond, B.C.S., late Commissioner, Rohilkund, says:—

"I have never seen a 12-foot tiger. The largest I ever shot was 11 feet 9 inches as he lay on the ground immediately he was shot, and before being padded. I measured him because I was struck with his large size."

F. B. Simson, Esq., B.C.S., says:—"I have killed or been at the death of about 180 tigers; I never actually handled one 11 feet long, but I fully believe that they reach that length occasionally, and every now and then a monster is found. The largest skins by far I have seen, came from China. I give you the exact measurements of several I have killed and fairly measured immediately after death, and before they were padded with dates:—

			Tigers' length. Ft.	In.	Height at Shoulder. Ft.	In.
1855	October	15	9	5	3	6
1856	February	13	10	4	3	8
,,	,,	11	10	11	3	7
1858	March	15	9	1	—	
			Tigresses' length.			
1855	October	14	8	8	3	3
,,	,,	13	8	5	3	5
,,	,,	19	8	11	3	5
,,	November	22	8	10	3	3
1856	October	6	9	4	3	10¼
1857	February	8	8	10	3	4

"All these were killed on the churs of the Megna, between Backergunge and Noakhally. In later years I killed tigers in Purneah, Docca, Mymensingh, and Assam, but their exact dimensions were not recorded. I do not remember any exceeding generally in size the measurements I have given. I once killed a tiger which stood almost 4 feet at the shoulder.

"I have often been referred to about hogs. I have taken about 900 first spears, and hunted in nearly every zillah in Bengal, but I never speared the boar that would not have walked under a standard of 3 feet 3 inches. This statement has disappointed many; but the facts are at your service, and you may use my name to authenticate them when you choose."

Major-General Sir H. Green, K.C.S.I., C.B., Bombay, says:—"The biggest tiger I was ever at the killing of was in 1848, near Surat, and it measured, *pegged out*, 12 feet 4 inches. I heard by last mail from Claude Clerk at Hyderabad, who said he had just killed, to his own gun, the biggest tiger *he* had ever seen, as it measured 11 feet 6 inches *before* skinning."

Sir H. Green also writes:—"I inclose a letter from Col. Stewart regarding tigers, and I have made many inquiries about them since, and there can be no doubt that a 12-foot tiger is very rare, although I have no doubt there are instances of that size having been exceeded. I find, by reference to my journal, that I have a record of some I have killed, and that the one I mentioned as 12 feet 4 inches, pegged out, measured, *before* skinning, 11 feet 11 inches. Measures before skinning:—

11 feet 11 inches.
10 ,, 11 ,,
9 ,, 9 ,,
9 ,, 6 ,, .—Tigress.
9 ,, 3 ,,
8 ,, 6 ,, .—Tigress; pulled down my elephant."

Col. D. G. Stewart writes:—"I have never seen or heard of a *bona fide* 12-foot tiger, *i.e.*, as he lay in his skin. The largest I ever saw or killed was, as he lay, 11 feet and ¼ inch. I have personally measured eighty tigers or more of my own shooting, and the dimensions I have given are those of the largest of my victims. I saw a skin in San Francisco, of a Chinese tiger, which might have been 12 feet long in life. I never saw anything Indian to approach it. The Chinese skin was fairly treated, had breadth as well as length, the fur was long and soft. The average size of large males in the Central Provinces I found to be 10 feet 6 inches to 10 feet 8 inches; the tail had a good deal to do with the last two or three inches. The largest tigress I killed was, I think, 9 feet 3 or 4 inches, but I speak from memory. Of two males the girth of the fore-arm of one was 48 inches, the average being 32 to 34 inches. One of the most remarkable measurements is that of the tail where it joins the carcass. I have repeatedly found it in males 12 inches."

The Hon. Sir H. Ramsay, K.C.S.I., C.B., Commissioner, Kumaon, writes: "I have always understood that Bengal tigers are larger than ours in the north-west. The largest tiger I ever killed measured 10 feet 5 inches, and I consider anything above 10 feet a large tiger; a tigress very seldom gets beyond 9 feet. I have heard of Bengal tigers measuring 12 feet. G. tells me his father, a Bengal civilian, shot a tiger that measured 12 feet 4 inches, but I never shot in Bengal."

Mr. C. Shillingford, indigo-planter, Purneah (with whom I have shot many tigers) says: "My experience extends over thirty-five years, during which I have shot more than 200 tigers. In 1849 I shot one of the largest tigers I have ever seen, with a party of four. He measured, *as he fell*, 12 feet 4 inches, was very old, and his marks had become faint; the hair was short, like that of a greyhound. I shot another tiger which measured, as he fell 11 feet 10 inches, and another in 1855, 11 feet 4 inches; several of 10 feet 6 inches and 10 feet. The majority of male tigers seldom exceed 10 feet, and many attain only 9 feet 8 inches or 9 feet 10 inches."

Cumming says he has shot a few over 11 feet, and gives three instances—one at Rohinipore, 11 feet 4 inches: one at Kaliastrich in 1865, of 11 feet 2 inches; and another at Gour in 1871. My nephew has also shot one or two over 11 feet.

I think these very large tigers are rare, and are only to be found in the Ganges churs; I am also inclined to believe that they are degenerating, as I have not shot large ones for several years: or it may be that there is a keener set of sportsmen now-a-days, and no sooner is a tiger heard of than he is shot. The tigresses are seldom over 8 feet, though I have known some that attained 9 feet to 9 feet 6 inches. Cumming says he has seen the claw-marks of a tiger on a tree 18 feet high. The men who are difficult to convince about the large tigers are those who have shot them in hills and rocky places, and those tigers are of a different class and seldom grow large."

Major Bradford, C.S.I., of the Political Service, says: "10 feet 5 inches was the largest tiger I ever saw; but I sent the question to Martin and inclose his reply and the inclosures to it. I remember hearing of the immense tiger White speaks of."

Col. C. Martin, C.I. Horse, says he shot a tiger at Putulghur 10 feet in length, and alludes to a large tiger shot near Goona by Mr. White, which was measured by Mr. Angelo, and is described as follows by the latter gentleman: "I can remember, beyond all doubt, the length was 12 feet 4 inches from tip of nose to tip of tail; 2 feet 2 inches from ear to ear! The direct breadth of wrist 8 inches, spread of foot 10 inches, heel to withers 4 feet, and the tail was 3 feet in length."

These measurements were recorded in the *Delhi Gazette*, but there is some doubt as to their accuracy; so that they may hardly be regarded as proving more than that the tiger was a very large one. Col. Martin says, in a subsequent letter, "W.'s tiger, which I had always thought 12 feet 4 inches, is no longer to be relied on for scientific inquiry, though it probably exceeded 10 feet."

Lieut. James Ferris, B. Army, says: "I have had a good deal of experience, as I have shot in the Central Provinces, and for several years in Oude and Nepal. The largest tiger I know of was shot by Wilkinson, in 1873, in Nepal, he measured 10 feet 4 inches from tip of nose to tip of tail. Wilkinson, who has shot more tigers than most men in India, told me this was the largest he had ever seen; the largest tiger I ever shot myself I got the same season in Nepal; he measured 10 feet 2 inches—he was considered a monster. The tigers in Lower Bengal may be larger, but in the Central Provinces they are certainly smaller; it depends a great deal on how the tiger is measured."

Gen. Ramsay, Bengal Army, says: "The largest tiger I ever saw I shot in conjunction with Col. Stewart, a fine old sportsman, who died many years ago at Benares. The tiger was not found for some days, when he was discovered dying from loss of blood and starvation. The skin was removed, and measured 12 feet from the nose to end of tail." This skin was no doubt stretched. "A tiger of 10 feet 6 inches is a very fair sized tiger. Tigresses seldom grow so large." General Ramsay adds: "My friend Col. H. Shakspeare writes me that 'the two largest tigers he ever killed were, when brought in and measured, 11 feet 8 inches and 11 feet 6 inches respectively—the latter a tigress.' He does not think he has ever seen larger ones. There probably are tigers that measure 12 feet or more, but they would be very rare."

Mr. F. Buckland has kindly given me the following extract from his "Curiosities," 1866, in regard to a tiger shot by Col. Ramsay, who says that he and Major B. shot a tiger at Huldwana, in the Kumaon Terai, that they estimated to be about twelve years of age, and was of the following dimensions:—

	ft.	in.
Length from nose to end of tail	12	0
,, of tail	3	9
Height from heel to shoulder	3	7
Girth of body behind shoulder	5	3
,, forearm	2	10½
,, neck	3	7
From ear to ear	1	6½
Length of upper canines	0	3
,, lower ,, ...	0	1¾
,, claws	0	3

On referring to some of my own tiger shooting notes I find that I have recorded the following measurements:—

Oude Terai, 1855

		ft.	in.
1.	Tiger	9	5
2.	,, ...	8	0
3.	Tigress, very large, pregnant with five cubs, measurement not preserved.		
4.	Tiger	10	0
5.	Tigress, large, but measurement lost		
6.	Tiger	9	0
7.	Tigress	8	10
8.	,, ...	8	11
9.	,, ...	8	9
10.	Tiger, cub	5	9
11.	,, ...	9	7
12.	,, ...	9	11

Oude Terai, 1857

		ft.	in.
13.	Tigress	8	0
14.	Tiger	8	3
15.	Tigress, with three cubs	8	10
16.	Tiger ... ... Lost measurement.		
17.	Tigress ... ... ditto.		
18.	,, ... ditto.		
19.	Tiger ... ... ditto.		
20.	Tigress, very large, pulled G.'s elephant down, lost measurement.		
21.	Tiger killed in Hangua (drive) from a tree, very large.		

Maldah, Bengal, 1870

22.	Tigress }	Measurements lost.
23.	,, }	

Ulwar

24.	Tiger	Measurement lost.

Purneah, Bengal, 1869

		ft.	in.
25.	Tigress	8	2
26.	Tiger	10	0
27.	Tigress	8	7
28.	,, }		
29.	,, } ... ... Measurement lost.		
30.	,, }		
31.	Tiger	10	8

Purneah, 1871

		ft.	in.
32.	Tigress	9	0
33.	,, ...	7	6
34.	,, ...	7	8
35.	,, ...	8	8

Oude and Nepal, 1871

		ft.	in.
36.	Tigress	8	9
37.	Tiger	9	7
38.	Cub	5	5
39.	,, ...	5	8½
40.	Tigress	8	7
41.	,, ...	8	6
42.	Tiger	9	6
43.	Tigress	8	8
44.	Tiger	10	6
45.	,, ...	7	0
46.	Small cub		
47.	Tiger	9	4
48.	Tigress (I killed her with a single bullet in the neck)	8	11
49.	{ Her	6	11
50.	{ three	6	5
51.	{ cubs.	6	10

These are all the notes I can lay my hands on at present. The largest tiger was 10 feet 8 inches, the largest tigress, 9 feet. Both were Bengal Purneah tigers. My own experience, therefore, confirms so much of Jerdon's estimate as that the tiger averages from 9 to 9½ feet, the tigress between 8 and 9 feet; but that which he and others doubt, viz., that tigers do occasionally attain the length of upwards of 10 feet 3 inches and even 11 feet or 12 feet, and the tigress up to 10 feet or even more, is conclusively attested by the evidence of the gentlemen whose statements I have quoted.

I am indebted to Sir Dighton Probyn for an interesting letter from Capt. Gerard, of Goona, a high authority on Indian felidæ. He expresses his belief that tigers seldom if ever exceed Jerdon's measurements, a tiger of 10 feet 1 inch being the length of the largest he has ever killed or seen. He refers to examples of large tigers described by various observers, but he doubts the accuracy of the measurements, which he thinks may have been unintentionally exaggerated. His own experience is very large, his accuracy as an observer well known, and his opinion consequently of much value; but it is hardly sufficient to invalidate that of others who are no less competent to note and record facts, and who certainly give a greater length, as the extreme growth of the tiger, than that within which Capt. Gerard limits him. The matter then stands thus: Mr. C. Shillingford, Col. G. Boileau, and Sir C. Reid,[1] vouch for tigers of over 12 feet. The same gentlemen, with Sir H. Green, Sir G. Yule, the Hon. R. Drummond, Col. D. G. Stewart, Mr. Cumming, and Col. Shakespeare vouch for tigers of 11 feet and upwards. The above, with Col. J. Sleeman, Sir Joseph Fayrer, Mr. B. Simson, and the Hon. Sir H. Ramsay vouch for tigers of 10 feet 5 inches and upwards, all from measurements taken before the animals were skinned. Unless these gentlemen, all of whom are accustomed to shoot and measure tigers, were *mistaken*, the question of length may, I think, be regarded as decided beyond dispute.

In conclusion, after thanking sincerely those gentlemen who have given me information derived from their own experience, I would just say that the mere length of a tiger is not necessarily an indication of its real size. The tail is included in the measurement—so tiger hunters have ruled that it shall be—but the tail is a somewhat variable element; in some it is long, in others short, and it is quite possible that a 9-foot 6-inch tiger with a short tail may be heavier, stronger, and larger than a 10-foot tiger with a long tail. No doubt anything over 10 feet is very large, and those of 11 or 12 are rare and exceptional, even though part of their great length may be assigned to an immensely long tail. But I think that, while making all allowances for errors of measurement—which doubtless are not uncommon, though unintentional—there is still sufficient evidence from accurate measurements to show that tigers may exceed 10 feet 3 inches, and that a few—perhaps rare and exceptional instances—do exceed even 11 and 12 feet.

J. FAYRER

[1] Since writing the above I have been informed by General Sir C. Reid, K.C.B., Bengal Army, that he has shot, and measured before the skin was removed, in the Dhoon a tiger of 12 feet 3 inches.

THE TELEPHONE, ITS HISTORY AND ITS RECENT IMPROVEMENTS[1]

II.

IN the preceding article we traced the history and development of the magneto-telephone. This instrument, even if it served no other purpose, has given to physicists a galvanoscope of surpassing delicacy. In the columns of this journal (vol. xvii. p. 343) Prof. Forbes showed how the feeblest thermo-electric currents could be detected by its means, whilst the subsequent discovery of the microphone was but another application of the same fact. This latter instrument and the early history of the carbon telephone we now propose to consider.

In the spring of the present year Mr. W. H. Preece startled every one by announcing that Prof. Hughes, the well-known inventor of the type-printing telegraph, had discovered that a couple of bits of charcoal, or a few fragments of metal in loose contact, when in circuit with a telephone and a voltaic cell, were able to reveal the faintest tremor or even to transmit the sound of the voice itself. Universal interest was excited by this discovery; a direct transformation of sonorous vibrations into electricity was supposed to have been discovered, but soon it became apparent that the explanation originally suggested was untenable, and that the true theory of the microphone was to be found in minute variations of current strength. The quivering of the loose fragments produced variable degrees of contact or of pressure, and the marvellous sensitiveness of the magneto-telephone revealed these otherwise inappreciable fluctuations in the resistance of the circuit.

On account of its sensitiveness, the microphone has been suggested as likely to be of use in auscultation.[1] M. du Moncel gives a form of stethoscopic microphone made by M. Ducretet, and shown in Fig. 2. The microphone pencil, C, rests upon a lower plate of carbon, P, which is adapted to a caoutchouc capsule, T, and this again is connected by a flexible tube to a second capsule, T', which can be applied to different parts of the body of the patient. Although the sensibility of the microphone can to some extent be regulated by the counterpoise, P O, yet still the objection to this apparatus is its over sensibility, for it reveals every noise or tremor, so that it is difficult to distinguish one sound from another. It is not impossible, however, that this or some other arrangement of the microphone may ultimately be found to yield important results in the hands of a physician who has made himself skilled in its use. At the same time we must bear in mind that, after its employment in a surgical case by Sir H. Thompson, the large expectations that were formed of the microphone as an exploring instrument in surgery have not as yet been fulfilled. We are not aware whether the microphone has been tried by seismologists, or by military men to detect the mining operations of an enemy, though we should fear the same causes that operate against it elsewhere might also occur wherever it is employed. The disturbances to which the instrument is subject are most conspicuous when the microphone is used, as it can be, to transmit speech. Nevertheless a particular arrangement, designed by Mr. Hughes (Fig. 3), gives fair results. The two fragments of carbon are shown at C D, the upper one being attached to a light metal arm A B, controlled by a spring R, the tension of which is regulated by a screw *t*. The whole is inclosed in a light wooden box H I G, surrounded by a second box M J L, the end of which is left open. A single carbon only, may even be used, touching the metal arm, as is shown at E. In this case the carbon is supported by a strip of paper gummed to the bottom of the box. Loud as is the articulation transmitted by means of this arrangement, the noisy roar which accompanies it, from tremors picked up by carbons, render many words quite inaudible in the receiving telephone. So far, in fact, the microphone has not proved a practical instrument; it seems, however, likely to become a useful adjunct in physical or physico-chemical researches. In any case science is indebted to Prof. Hughes for first making known an entirely novel, simple, and delicate instrument for the detection of minute mechanical motions.

We say first making known, for some twelve months before Prof. Hughes published his description of the microphone, an arrangement designed by Mr. Edison was singularly like the microphone in its extreme delicacy to the minutest tremor. A couple of inches of silk ribbon rubbed with plumbago and made stiff with gum, was laid upon two metal supports joined in circuit with a telephone and a small battery. Such an arrangement not only de-

[1] "The Speaking Telephone, Talking Phonograph, and other Novelties," by G. B. Prescott. Illustrated. (New York: Appletons, 1878.)—"Le Téléphone, le Microphone, et le Phonographe," par Le Comte Th. du Moncel. (Hachette, 1878.) Continued from vol. xviii. p. 700.

[1] *Apropos* of the microphone a literary friend sends me the following extract from an ancient Turkish tract containing an exposition of the Moslem creed:—"He (Allah) hears alike the loudest and gentlest sounds and sees all things, even the walking in a dark night of a black ant on a black stone, and hears the treading of its feet, and this without eyes or ears."

tected the slightest vibration but was capable of transmitting speech with remarkable loudness. Although this result has hitherto been unpublished, an application of the same principle was made by Mr. Edison in his "carbon relay," a reprint account of which was published in the *Telegraphic Journal* for July 1, 1877, and the simple form of this relay (Fig. 4) resembles in construction and in principle the so-called hammer and anvil microphone. Edison's carbon rheostat, lately illustrated in our pages, also depends upon the same principle, namely, the compression of a semi-conductor, such as carbon, increasing its conductivity, a diminution of pressure increasing the resistance. It is true that it has been suggested that a rheostat of this kind was made by M. Clérac in 1865, but so far as we have been able to ascertain, no mention is made of varying pressure in the use of Clérac's instrument, but simply the difference in the resistance of columns of plumbago of varying length.

The curious effect of pressure on the electric resistance of semi-conductors was noticed so long ago as 1856 by M. du Moncel, who wrote as follows in the second edition of his "Exposé des Applications de l'Electricité" (vol. i. p. 246):—"Une chose assez curieuse, et qui paraît être au premier abord en contradiction avec la théorie que l'on s'est faite de l'électricité, c'est que la plus ou moins grande pression exercée entre les pièces de contact des interrupteurs influe considérablement sur l'intensité du courant qui les traverse. Cela tient souvent à ce que les métaux de l'interrupteur ne sont pas toujours dans un état parfait de décapage au point de contact, mais, peut-être aussi à une cause physique encore mal appréciée." Subsequently, in an elaborate memoir on the electric conductivity of bodies, the same distinguished author wrote:—"La pression exercée sur les electrodes joue un grand rôle sur la conductibilité de celui-ci quand il est susceptible d'une certaine compression, comme les bois, les corps mous et pâteux et ceux qui sont réduits à un grand état de division." In 1873 Edison states (Prescott, p. 223) that he independently discovered "the peculiar property which semi-conductors have of varying their resistance with pressure while constructing some rheostats for artificial cables, in which were employed powdered carbon, plumbago, and other materials in glass tubes."

In January, 1877, Edison states that he first applied to *telephonic* purposes the effect of pressure on carbon. It will be remembered that the principle of Gray's telephone was the variations in the resistance of a liquid, proportional to the motions of a diaphragm vibrated by the voice, but that the decomposition of the liquid by the current introduced practical difficulties in the working of this arrangement. Casting about for a means of overcoming this difficulty, Edison recalled his early observations on the electrical properties of carbon, and, encouraged by his preliminary trials, was led onwards step by step, his unwearied efforts being at last crowned by the construction of his carbon telephone, towards the close of last year. In another article we shall trace the evolution and present achievements of the carbon telephone, an instrument which we believe is destined to have a great future.

In conclusion, it is interesting to note that just as Gray and Bell were both led to the discovery of the principle of an articulating telephone through working at the important problem of multiple telegraphy, or the simultaneous transmission of several different signals on one wire, so, too, Edison travelled in the same direction. In Prescott's work he writes: "Some time in or about July, 1875, I began experimenting with a system of multiple telegraphy which had for its basis the transmission of acoustic vibrations." His transmitters were a chord of tuning-forks maintained in vibration by the action of a current; the current not being interrupted at each vibration, but made to vary in strength: his receivers consisted of electro-magnets acting upon iron diaphragms, which closed the end of a series of resonant tubes. In practice difficulties arose, but the system contained two important features.

These were, first, the employment of undulatory electric currents, an electrode attached to one prong of the trans-

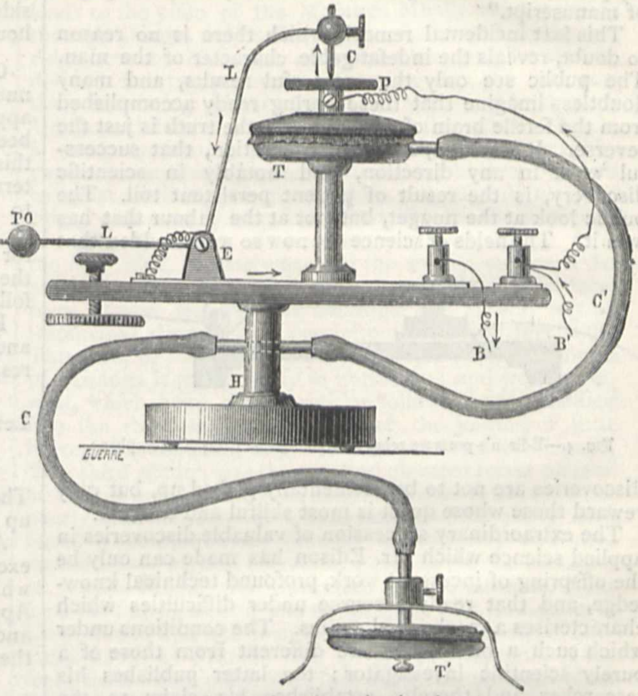

Fig. 2.—Ducretet's stethoscopic microphone.

mitting fork being made to vary the resistance of the circuit by vibrating in a vessel of water, an idea which Edison claims to have patented as early as 1873; and the second is the use of an iron diaphragm and adjacent electro-magnet as a receiver. Substituting a membrane moved by the voice, the transmitter becomes the same as that patented by Gray in 1876, and the receiving instrument is similar to that used in the magneto-speak-

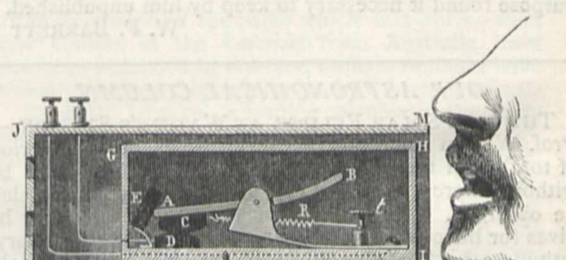

Fig. 3.—Hammer and anvil microphone as arranged for transmitting speech.

ing or Bell's telephone. Hence, if these facts are unassailable, and the latter is at present the subject of litigation in America, to Edison is also due the happy inspiration of first using a thin iron diaphragm as a receiver. At the same time Edison, in a recent letter to the present writer, remarks, "Bell had the merit of discovering such a receiver would act as a transmitter," and likewise in

Prescott's book, Edison distinctly says: "I can lay no claim to having discovered that conversation could be carried on between one receiver and the other upon the magneto-principle, causing the voice to vibrate the diaphragm. . . . My first attempts at constructing an articulating telephone were made with a Reis transmitter and one of my resonant receivers. My experiments in this direction, which continued until the production of my present carbon telephone, cover many thousand pages of manuscript."

This last incidental remark, which there is no reason to doubt, reveals the indefatigable character of the man. The public see only the successful results, and many doubtless imagine that these spring ready accomplished from the fertile brain of Mr. Edison; the truth is just the reverse. It is a trite, but true observation, that successful work in any direction, and notably in scientific discovery, is the result of patient persistent toil. The public look at the nugget, but not at the labour that has won it. The fields of science are now so well trodden that

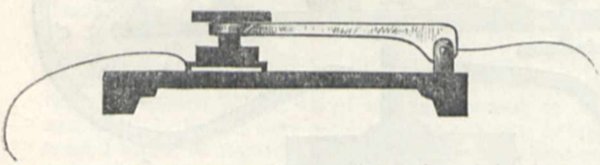

FIG. 4.—Edison's pressure relay resembling one form of microphone.

discoveries are not to be accidentally picked up, but only reward those whose quest is most skilful and diligent.

The extraordinary succession of valuable discoveries in applied science which Mr. Edison has made can only be the offspring of incessant work, profound technical knowledge, and that ready resource under difficulties which characterises a mechanical genius. The conditions under which such a man works are different from those of a purely scientific investigator; the latter publishes his researches and thereby establishes his claim to the priority of the work he has done; the former can publish nothing till the end he has in view is achieved, and the pecuniary benefit accruing from his labours secured by legal processes. And because the reward sought in the two cases is very different, the investigator must often expect to see others reaping the benefit of applications that may be made of his observations, and the inventor ought not to grumble when he finds others claiming credit for work he may previously have done, but for his purpose found it necessary to keep by him unpublished.

W. F. BARRETT

OUR ASTRONOMICAL COLUMN

THE LATE SOLAR ECLIPSE AT WATSON'S STATION.—Prof. Watson made such excellent use of the brief period of totality in the eclipse of July 29, that it will not be without interest to record the circumstances under which he observed. In a communication to M. Mouchez he gives for his position at Separation, Wyoming Territory, latitude 41° 45′ 50″, longitude 2h. 1m. 36s. west of Washington, corresponding to 7h. 9m. 48·1s., west longitude from Greenwich. Prof. Newcomb's corrections to Hansen's place of the moon at this time are − 0·63s. in right ascension, and + 3·3″ in declination; whence if we take 10h. 24m. Greenwich M.T. for a special calculation, we have for the position of the moon, R.A. 8h. 38m. 11·96s., decl. + 19° 5′ 59·3″. Combining this with the sun's place from Leverrier's Tables and the *Nautical Almanac* semi-diameters, there results

Beginning of total eclipse, July 29 ...	3h. 13m. 32·5	Mean times at Separation.
Ending	3h. 16m. 24·0	

Thus the duration of totality was 2m. 51·5s.

If for the *Nautical Almanac* values we substitute Leverrier's semi-diameter for sun and deduce the semi-diameter of the moon from her horizontal parallax with Burckhardt's ratio, we find the times of beginning and ending of totality are respectively 3h. 13m. 32·0s. and 3h. 16m. 24·5s., showing a duration of 2m. 52·5s.; we may therefore take 2m. 52s. for the interval which was available to Prof. Watson in his search for intra-mercurial planets. The middle of totality occurred at 3h. 14s. 58·3s. M.T. at Separation, or at 11h. 44m. 41·9s. sidereal time, when the sun's altitude was 44½°, and his hour-angle 46½° W.

CALCULATION OF EXCENTRIC ANOMALIES.—The number of bodies in the minor planet group is now approaching two hundred, yet so far as their orbits have been satisfactorily determined only two or three out of this number have the angle of excentricity, as it is termed, or $\sin^{-1} e$, greater than 20°, which corresponds to $e = 0·342$. More than ten years since Mr. Godward, of the *Nautical Almanac* Office, prepared some tables for the direct computation of the excentric anomaly from the mean to this limit of excentricity. His process is as follows:—

In orbits where the excentricity is not great, M, u, and v being the mean, excentric and true anomalies respectively, and ϕ the angle of excentricity—

$$\tan \tfrac{1}{2} v = \tan^2 (45^\circ + \tfrac{1}{4} \phi) \tan \tfrac{1}{2} M \text{ nearly.}$$

Let M' be an angle such that

$$\tan \tfrac{1}{2} v = \tan\ (45^\circ + \tfrac{1}{2} \phi) \tan \tfrac{1}{2} u$$
$$= \tan^2 (45^\circ + \tfrac{1}{4} \phi) \tan [\tfrac{1}{2} M + \tfrac{1}{2}(M' - M)].$$

Then the Table contains $\tfrac{1}{2}(M' - M)$ for any value of ϕ up to 20°, the arguments being $\tfrac{1}{2} M$ and $\tfrac{1}{2} \phi$.

As an example of the use of this Table, suppose the excentric anomaly of Juno is required for the time to which the elements of the planet are reduced in the Appendix to the *Nautical Almanac* for 1881. The mean anomaly $(= \epsilon - \pi) = 168^\circ\ 39'·43$ and $\tfrac{1}{2} \phi = 7^\circ\ 23'·22$, then

$\tfrac{1}{2} M$	84° 19′·72	
$\tfrac{1}{2}(M' - M)$	— 11·42	from the Table.
$\tfrac{1}{2} M'$	84 8·30	
$\tan \tfrac{1}{2} M'$	0·98858	
$\tan (45^\circ + \tfrac{1}{2} \phi)$	0·11325	
$\tan \tfrac{1}{2} u$	1·10183	
$\tan \tfrac{1}{2} v$	1·21508	
$\tfrac{1}{2} u$	85° 28′·64	
$\tfrac{1}{2} v$	86 30·74	
u	170 57·28	
v	173 1·48	

Here $\tan \tfrac{1}{2} v$ is obtained by adding together the two previous lines, so that there is no subtraction in the operation.

Mr. Godward's Table was printed by the *Nautical Almanac* Office in 1866. It is applicable to all the satellite-orbits showing excentricity, as *Hyperion*, where $\phi = 7^\circ\ 11'$.

THE MINOR PLANETS.—From No. 100 of the *Circular zum Berliner astronomischen Jahrbuch* it appears that the small planet at first announced as No. 190 is proved by Herr Leppig's calculation of its orbit to be identical with No. 94 (*Aurora*); the succeeding discovery therefore takes its number, and for planets found since the beginning of the summer, the numbers, names, and dates will stand thus:—

No. 188	Menippe,	1878,	June 26
,, 189	Phthia,	,,	Sept. 9
,, 190	Ismene,	,,	Sept. 22
,, 191	Kolga,	,,	Sept. 30

Several members of the group as *Dike*, *Medusa*, and others with better determined orbits remain to be virtually rediscovered, and the most interesting of all, from its long period and near approach to the orbit of Jupiter (*Hilda*), was not found at its last opposition. *Atalanta* and *Felicitas* are now nearer the earth than is usual with the minor planets, both being within the mean distance of the earth from the sun; they have the brightness of stars of the tenth magnitude. The following positions are for 12h. M.T. at Berlin, or about 11h. G.M.T.:—

	Atalanta. R.A. h. m. s.	Decl. ° ′	*Felicitas.* R.A. h. m. s.	Decl. ° ′
Nov. 7 ...	1 38 10	+37 50·6	3 11 22	+29 59·1
,, 9 ...	1 35 45	37 56·0	3 9 24	30 4·3
,, 11 ...	1 33 27	38 0·0	3 7 23	30 8·6
,, 13 ...	1 31 18	38 2·8	3 5 23	30 11·9
,, 15 ...	1 29 17	38 4·4	3 3 23	30 14·2
,, 17 ...	1 27 27	+38 4·9	3 1 24	+30 15·7

GEOGRAPHICAL NOTES

WE regret to learn that the Earl of Dufferin will be unable to open the session of the Royal Geographical Society on Monday next, as he has received Her Majesty's commands to attend at Balmoral on that day, but it is hoped that he will be able to preside at the meeting on December 9.

PROF. F. V. HAYDEN, in charge of the Geological Survey of the U.S. Territories, has crossed the Rocky Mountain Divide ten times during the past season. He has explored some of the most noted passes, and among them the celebrated Two-ocean Pass, of which he made a careful chart; an account of this we hope soon to be able to give. We hope also to receive from Prof. Hayden an account of the discovery of recent glaciers in the Wind River Mountains of Wyoming Territory, the first known to exist in the United States east of the Pacific Coast. A fine glacier was observed on the east side of Wind River Peak, and two grand ones on the east side of Fremont's Peak. The latter Dr. Hayden named Upper and Lower Fremont Glaciers. Dr. Hayden took great pleasure in traversing much of the same ground passed over by him in 1860, eighteen years and three months before.

A REUTER'S telegram states that Gen. Severtsoff, the explorer of the Pamir plateau, has returned to St. Petersburg, having visited the unknown districts of Lake Rang-Kul and the Sariz Pamir and Alitchur Pamir plateaux. He reports having found a continuous valley extending from Lake Kara-Kul to the Aksu River. Gen. Sjevertsoff has considerably altered the map of these regions and thrown much light on the geography of the Pamir plateau.

WE are sorry to state that no news from the *Florence* has reached Washington from September 13, the date of the last telegram which Capt. Tyson sent to New York when leaving St. John's, Newfoundland. It is feared that the ship was sunk by the recent heavy gales which raged in this part of the Northern Atlantic a few days after its setting out.

THE London Missionary Society have received intelligence of the arrival of their Tanganyika expedition at Urambo, in Unyamwesi, on July 27; they were to leave that place early in August, and hoped to reach Ujiji by the beginning of September. Mr. Hore in his letter divides his geographical description of the country between Kirasa, forty-five miles east of Mpwapwa, and the capital of Unyamwesi into four sections, each of which furnishes interesting details respecting the region traversed by the party. From Kirasa in S. lat. 6° 42′ 30″, elevation 2,700 feet, to Mpwapwa, lat. 6° 22′, 3,200 feet, they were still in the coast region, the country gradually rising to Mpwapwa along an inclosed plain. As it is approached, the mountains of that range bound the view westward, forming the distinct boundary-line of the maritime region. The waters of the Limbo and of the Mpwapwa stream appear to be mere tricklings left by an immense and irregular flow of water during the rains, which, Mr. Hore suspects, will alter the whole face of the country and reconcile the conflicting accounts we have had about the Gombo Lake. The Chunyo Pass is the back door of the maritime region; a slight descent leads to the plain of the Marenga Mhali, which extends through Ugogo, unless the break of elevated forest and ridge between Kididimo and Nyambwa may be said to divide it into two portions. Assuming this, the first portion, consisting of the Marenga Mhali and Eastern Ugogo, exhibits a similar character throughout, that is, a gently undulating plain, with harsh, thorny, scrubby vegetation and small trees, its monotony broken by small irregular and rugged granite hills. A slightly elevated ridge, with a really beautiful forest, divides the first from the second section of the journey. Descending from the first ridge, the party entered the second section, a flat plain, crusted with a salt deposit, in which tall palm-trees form a new feature. At Mizanga the second section terminated abruptly at a precipitous wall 800 feet high. This wall, or "step," extends north and south, but north of Mizanga it trends away to north-west and west-north-west, which bend the expedition followed, and mounted into the third section or stage of the journey a little beyond Makondoku, the westernmost town of Ugogo. This third section was the vast and elevated forest plateau of Uyanzi and Unyamwesi, extending almost unbroken to nearly the meridian of Unyanyembe. The party here found a comparatively bracing atmosphere, and also reached their highest elevation, 4,400 feet, in the meridian of Jewe-la-Singa. At Uyui (lat. 4° 53′, altitude, 3924 feet) the fourth section was entered, the hills and dales of Unyamwesi, and the country maintained the same character as far as Urambo (lat. 4° 37′ 30″, altitude, 3,815 feet), from which place Mr. Hore wrote that the hills, often little elevated ridges, trend generally north and south, and many of their shoulders had to be crossed. This is the region of the Gombe Nullah. "To the passing traveller," Mr. Hore says, "the driftwood and grass in the trees overhead speak to him of some vast inundation rather than of a stream. The Gombe Nullah is the lowest drain of a vast body of water, whose general direction towards the Malagarasi is indicated by it. . . . This fourth stage has brought us on to the water-shed of the Tanganyika."

THE proceedings of the party which last spring went to New Guinea in the *Colonist* from Australia, have hitherto been shrouded in mystery, though rumours have occasionally reached this country as to their want of success. Recently, however, a leading member of the party has been obliged to go to Cooktown, Queensland, through ill-health, and his report of their proceedings has been furnished to the *Brisbane Courier*. Almost all that has previously been heard of them is that they had formed a camp on the Laloki River. Starting north from this point we learn that they proceeded through open country for eight miles, and struck the Goldie River, where they found the first colours of gold. Twelve miles up this river they crossed and proceeded for two miles in a northerly direction, when they recrossed on finding that the river trended to the east. They then took a north-easterly direction for thirteen miles, partly through dense scrub, and reached what they named the Top Camp, thirty-five miles from the Laloki. They made two journeys up the Goldie, one party going a distance of fifty miles, but found no indications of gold. They saw many villages, some numbering 1,000 inhabitants, and all the natives were friendly. Afterwards the party moved further down the river, and camped near the junction of the Mawmika and the Goldie, the former of which flows into the latter from the east, about five miles north of the Laloki. From this point a portion of the party travelled eastward, between the Mawmika and the Laloki, towards the Astrolabe range, through open forest country, hoping to reach the table-land seen from the spurs of Mount Owen Stanley in their first journey. After about forty miles they met with magnificent waterfalls (about 150 ft.) on the Laloki, and on the fourth day reached the table-land after great labour. They went along it for twenty miles, when their further advance on horseback was stopped by the scrub. Two of the party remained with the horses, while three others, led by three natives carrying provisions, went through the scrub for thirty miles, and struck the head waters of the Goldie. They then proceeded for six miles in each direction, and found, after their great toil, that they had got entirely away from all indications of gold. They returned to the Laloki camp after an absence of thirty-two days, and since then no exploring work has been done. The members of the expedition still maintain that good gold must exist, and they propose to return to Top Camp, and thence to cross a small range to the north-west, from which the gold found in the Goldie River is supposed to have come; from that point they hope to get to another river supposed to run under Mount Owen Stanley, in the direction of Redscar Bay. The total distance traversed by the party during their three prospecting trips was 370 miles, and throughout the whole of that distance not half a grain of gold was discovered.

EARLY in the summer Mr. J. V. Williams was sent to New Caledonia to inspect the nickel mines there on behalf of an English smelting company, and we learn from an Australian contemporary that he thinks most favourably of their prospects. The mines extend from Noumea northwards, along the coast for 120 miles, and the French authorities, disheartened by the inefficient manner in which mining has hitherto been carried on in the island, are said to be prepared to give an English company all reasonable assistance and encouragement.

FROM a Singapore paper we learn that Baron Overbeck and Mr. Alfred Dent, the promoters of the cession of a portion of Northern Borneo to an English company, to which we referred some few months back, arrived at that port at the end of August, after a visit to the lately-ceded territory. They state that matters are quiet there, and that no disturbances are apprehended. A Ceylon planter who went to report upon the adaptability of the soil of the territory for planting purposes, is of opinion that coffee might be cultivated on the west coast, while on the east coast the sugar-cane and other products which usually flourish in the same soil, would thrive well. Altogether the most sanguine expectations appear to exist as to the productiveness of the country.

THE WERDERMANN ELECTRIC LIGHT

WHILE the world is waiting for the announcement of Mr. Edison's method of splitting up the electric light, Mr. Richard Werdermann, a gentleman well known in connection with electric lighting, seems to have solved the problem, to some extent, at least, and he believes that after further experiments he will be able to divide the current into 50, 100, or even 500 lights. Experimental exhibitions of the light have been given with satisfactory results at the works of the British Telegraph Manufactory, Euston Road.

The chief object of Mr. Werdermann is to demonstrate that a number of lights can be placed in one circuit, the current being produced by an electroplating Gramme machine, having an electromotive force of 4 to 4½ Daniell cells. The principle of Mr. Werdermann's invention is that of keeping a small vertical pencil of carbon in contact with a large disk of the same material. In some earlier experiments he found that when he increased the sectional area of the one carbon and reduced that of the other, he produced an electric light with the carbons in actual contact, a small arc appearing at the point of contact. The small carbon is a pencil 3 mm. in diameter; the upper or negative carbon is a disk of 2 inches diameter and an inch thick. The upper carbon is not consumed, so that the waste takes place only in the lower.

In his lamp he places the disk uppermost with the pencil vertically beneath it, sliding up a metal tube which acts as a guide and contact. The pencil is kept in contact with the disk by means of chains attached to its lower extremity passing over pulleys and down again to a counterweight of about 1½ lb. About ¾ in. of the lower carbon appears above the tube, and is rendered incandescent by the passage of the current between it and the disk. This pencil is pointed, and retains this point all the time of burning. It is between this point and the disk that the small electric arc appears which gives the greater part of the light.

At the exhibitions which have been given only ten lights were in circuit at once, Mr. Werdermann having no more lamps at hand. The lights were put in what is called the parallel circuit, that is, they all branched off from one wire of the machine and met again on the other. The lamps were estimated to give a light of forty candles each, and the results obtained were most satisfactory, all the lights burning equally well, giving a beautiful white light, which was perfectly steady. By this system of lighting a large number of lamps could be lighted simultaneously, could be put out, and again re-lit. If one lamp should be extinguished it does not affect the others, except by making them burn slightly brighter; but this effect will be obviated by a switch arrangement for regulating the current of the extinguished lamp. The current produced by the machine being very low in tension, the insulation of the conducting cables could be cheaply and easily maintained.

We may state that two larger lamps were shown of 360 candles each. The effect of the light is not dazzling to the eyes, and it was shown naked; in actual practice a common glass globe, as in the ordinary gas lamps, will be a sufficient protection.

NOTES

WE are requested to state that on the occasion of Prof. Wurtz's Faraday lecture at the Royal Institution on Tuesday next, the visitors' tickets issued by the Chemical Society entitle ladies as well as gentlemen to admittance.

SOME of our readers may be aware that Mr. Alfred Russel Wallace is a candidate for the post of Verderer of Epping Forest. We are sure no one can be better fitted than Mr. Wallace to perform the duties attaching to such an office, and as, so far as we know, no more suitable candidate has appeared, the duty of those who have the filling-up of the appointment is plain.

IT is with regret we announce the death of Mr. Charles R. Thatcher, the well-known conchological collector. It will be remembered that we alluded a few months ago to the number of genera and species of shells lately described, due to the indefatigability of this gentleman, including the unique and wonderful *Thatcheria mirabilis*, described in the *Proceedings* of the Zoological Society by Mr. G. F. Angas, and the *Delphinulopsis Lesourdi*, described by Mr. Bryce-Wright in the *Journal de Conchyliologie*, Paris. Mr. Thatcher started, a few months ago, on a five years' collecting tour, and had made the most perfect arrangements for deep-sea and shallow dredgings. He was attacked suddenly by fever, and died a few days after his arrival at Shanghai. There is no greater loss to conchological science than this gentleman's death, as he was undoubtedly the most successful collector of the day.

PROF. F. V. HAYDEN has recently been elected honorary member of the Société Vaudoise des Sciences Naturelles, Lausanne, Switzerland; Société Malacologique, Brussels, Belgium; and the Geographical Society of Halle, Prussia.

IT is announced that the Sheffield meeting of the British Assotion will begin, not on August 6, but August 20, 1879.

FROM a Government Minute we have received from Mauritius, we learn that the governor, Sir Arthur Phayre, laid before the council of July 20 an application from the President of the Meteorological Society of Mauritius for a grant of money, in order that the results of the meteorological observations made during many years by Dr. Meldrum, the Government observer, may be published in a form which will be advantageous to science and to navigation, as well as to commerce. The form which it is proposed the publication shall take is twofold, namely: synoptic weather charts for the Indian Ocean, extending over twelve consecutive months, and a storm atlas of the Indian Ocean, exhibiting the principal results which have been obtained respecting the more marked atmospheric disturbances in the Indian Ocean during the last thirty years. The objects are set forth in detail in a report by Dr. Meldrum, of which a copy is annexed to the Minute. The matter was referred to the Finance Committee, who, we trust, will grant the sum asked for. If they have any regard to the interests of navigation and commerce in the Indian Ocean, there can be no doubt as to their decision.

COMMANDER PERRIER has been making some interesting experiments with the Giffard captive balloon for the purpose of comparing the qualities of the best aneroid barometers selected from the Paris Exhibition. Each French maker who obtained a silver medal sent two instruments, which were sent up five or six times, and compared with a standard Fortin barometer placed at the foot of the cable. It was discovered that very few of these barometers recovered their original readings until after the lapse of a considerable time. Some photographs were taken of Paris at this high altitude, and are most interesting, although the motion of the car has caused some want of distinctness in the parts removed from the centre of station.

SOME interesting objects have recently been brought to light from the lake-dwellings in the lake of Neufchatel, and are now exhibited at the Neufchatel Museum. Amongst them are three particularly worthy of notice: (1) a large and extremely well preserved piece of amber; (2) a golden earring of masterly workmanship, of the bronze age; and (3) a canoe cut out from the trunk of a single oak tree in perfect preservation. Its length is 7 metres, its breadth 55 centimetres at the prow, and 65 centimetres at the stern; its depth is 19 centimetres, its total height from 22 to 24, and the thickness of its sides 6 to 8½ centimetres.

NEWS from Panama states that the volcano Cotopaxi is in a state of violent activity. Its crater is surrounded by ice and snow, but the clouds of ashes and smoke rising from it can be seen even at Guayaquil on the shores of the Pacific.

THE *Daily News* Naples Correspondent under date November 2, telegraphs that after numerous variations the activity of the eruption of Vesuvius appeared to be then at its height. The lava flowed into the same ravine into which it fell during the eruption of 1872. The seismographs denoted an approaching increase in the eruption.

TWO fresh shocks of earthquake are reported from Buir (Rhenish Prussia), of which the first was felt for many miles around. It occurred on October 24, at 12.30 A.M., and the second one on the same date at 3.45 P.M.

A VIOLET-COLOURED meteor, with a reddish train, was seen at Stanislas, Austria, on the evening of the 24th in the Great Bear and moving in a northerly direction. It is described as thrice the size of Jupiter.

IN NATURE (vol. xviii. p. 652) we gave an account of a verification of M. Pervouchine's first result (vol. xviii. p. 104). We have now seen a very short and neat verification not only of Pervouchine's first but also of his second result (vol. xviii. p. 456). The author (Mr. John Bridge, M.A.) uses the scale whose radix is 16 instead of the binary scale. He assumes r_n to be the remainder arising from the division of 2^{2^n}, then since $2^{2^{n+1}} = (2^{2^n})^2$, it follows that r_{n+1} is the remainder arising from the division of $(r_n)^2$. Hence the remainders can be successfully calculated. Thus for the first result the divisor is (in scale 16) 1(12)001

$$\therefore r_5 = -5249,\ r_6 = +9(11)4,\ r_7 = -59(10)6,$$
$$r_8 = +(12)5(11)4,\ r_9 = -10(10)(15),\ r_{10} = +1702,$$
$$r_{11} = +(13)(10)(13)6,\ r_{12} = -1.$$

Hence $2^{2^{12}}$ has remainder -1, and therefore $2^{2^{12}} + 1$ is divisible $7 \times 2^{14} + 1$. The second result is obtained in the same manner, the divisor being $(10)(16)^6 + 1$, *i.e.*, (10)000001, and the last remainder r_{23}. These verifications have been presented to the London Mathematical Society by the author.

A SUPPLEMENT to No. 37 of the *Boletin de la Institucion libre de Enseñanza* consists of a prospectus giving an account of its aims and of its statutes, which we sketched out in a former notice of the Institution. It also contains a list of officers (among the four honorary professors we notice the names of John Tyndall, de Lóndres, and Charles Darwin, de Lóndres) and of the courses of lectures for the students. We have received, also, a copy of the Vice-Rector's (Montero Rios) address, "Las Elecciones Pontificiales." Among other papers read at the *con. erencias* we note "El Aqua y sus Transformaciones," por D. Francisco Quiroga; "Relaciones entre la Sciencia y el Arte," por D. Federico Rubio; "Teorias Modernas sobre los Funciones Cerebrales," por D. Luis Simano; "La Vida de los Astros," por D. A. G. de Linares. No. 33 has papers on "La Geometria Sintética" (continuation); "Los Principales Publicaciones sobre Plantas Insectivoras" (two by Mr. F. Darwin are noticed), and the catalogue of the "Coleccion de Rocas" in the Natural History Cabinet is proceeded with.

THERE has been opened at Berlin the Telegraphic Museum established by M. Stephan, the General Director of Postal Telegraphs in Germany. The exhibition has been located in two large rooms of the General Post Office at Berlin. This is not the first institution of the kind, a telegraphic museum, and even laboratory, having been established at Tokio by the Japannese Government for the use of the pupils in telegraphic engineering. A number of interesting experiments have been already made in that laboratory under the guidance of Messrs. Ayrton and Perry.

WE have received the *Transactions* of the Norfolk and Norwich Naturalists' Society for 1877-8. The society is now in its tenth year, and is both financially and numerically stronger than at any previous period of its existence, the number of members being nearly 190. A glance at the contents of the part shows that the society is fairly carrying them out. We would particularly call attention to the series of letters on the ornithology of Norfolk, by eminent deceased naturalists, edited by Prof. Newton. The society is making it a feature of its work to rescue all such valuable records from loss. The society is also endeavouring to rescue from oblivion the memory of men who did good work in their time, but are fast being forgotten. The complete meteorological report is also an annual contribution; the observations are recorded by the valuable set of instruments, the property of the Norwich Meteorologica Society. Mr. Stevenson's "Ornithological Notes" are also continued from year to year. The last paper is perhaps the most important: it is part viii. of a carefully compiled fauna and flora of the county. The subjects of the previous parts have been mammals, reptiles, marine, fresh-water, and land shells, fungi, lepidoptera, flowering plants and ferns, diatomaceæ. Other sections will follow as opportunity occurs. We have also received the *Proceedings* of the Norwich Geological Society, part i., which contains some interesting papers, a creditable Fourth Annual Report of the Lisburn School Association, and a satisfactory Report of the Committee of the Goole Scientific Society, which is in its third year.

WE have received the programmes of the arrangements for the session of the various association societies of Cumberland, of the organisation and activity of which we have had already occasion to speak. The programmes seem to us on the whole very satisfactory.

A FINE statue of Corinthian metal resting upon a marble pedestal has just been found through excavations which are being made at the Ponte Sisto at Rome. The statue is 3 metres high and is slightly damaged; the right arm is entirely broken off; yet it is hoped that the damage is not beyond repair. The general belief among archæologists is that the statue represents the Emperor Probus.

AT Berlin a "Society for Ornithology and the Protection of Birds" has just been formed, under the presidency of Dr. Karl Russ. It consists of about fifty members at present, amongst whom there are numerous eminent ornithologists. The special purposes of the new Society are the discussion and practical testing of ornithological questions in regular meetings, the establishment of frequent ornithological exhibitions on as large a scale as possible, and the delivery of public lectures on the science of birds in all its branches.

M. NICOLLE, the organiser of the Exhibition of Maritime and River Industries, which took place at the Palais de l'Industrie, Champs Elysées, in 1876, has received from the French Government, authority to use the same building in 1879 for an exhibition of science applied to industry. M. Nicolle is now busy making his arrangements for next spring. We understand that a very large place will be devoted to the wonders of electricity. This exhibition is opened to all nations, and scientific exhibitors at the Champ de Mars and the Trocadéro will receive circulars giving details before the close of the exhibition. A sum of 700,000 francs has been voted out of the National subscription for covering the travelling expenses of 5,000 working men to the exhibition. A sum of from 4*l*. to 6*l*., according to distance, has been handed over to each of the chosen delegates, and the railway companies have agreed to take only half of the ordinary fares.

FROM America we have received from the twenty-first to the twenty-eighth annual reports of the New York State Museum of Natural History, edited by the regents of the University of the State of New York. Their contents are of the highest interest, comprising many geological, botanical, and zoological communications of value. Amongst others we note an elaborate paper (with map) on the Niagara and Lower Helderberg groups, their relations and geographical distribution, by Dr. James Hall, the director of the museum; numerous entomological contributions by J. A. Lintner; an account of an ascent of Mount Steward and its barometrical measurement, by Verplanck Colvin; remarks on some peculiar impressions in sandstone of the Chemung group, New York, by Dr. Hall and R. P. Whitfield; notes and observations on the Cohoes Mastodon, by Dr. J. Hall. Each part contains a number of well-executed plates. We may, at a future date, return to some of the contents at greater length. We have also before us the *Transactions* of the Academy of Science of St. Louis (vol. iii. No. 4); the principal contents are a valuable treatise by Prof. G. Seyffarth, entitled "Corrections of the present Theory of the Moon's Motions according to the Classic Eclipses;" a note on the larval characters and habits of the blister beetles, by Charles T. Riley; a synopsis of American firs, by Dr. George Engelmann, and other botanical communications by the same gentleman. Also the following publications of interest for entomologists:—Collecting Butterflies and Moths, by Montagu Browne; Preliminary Studies on the North American Pyralidæ, by A. R. Grote; Butterflies and Moths of North America, by Herman Strecker; Lepidoptera, Rhopaloceres, and Heteroceres, by the same (Nos. 14 and 15).

M. W. DE FONVIELLE writes us that Capt. Howgate has not forgotten the suggestion made by him of utilising for ballooning purposes the magnificent coal seam discovered by our Arctic Expedition on the shores of Lady Franklin Bay. Capt. Howgate has made use of the delays arising from the slowness of Congress in granting the required credit for his Arctic colony, in having experiments made to ascertain whether it is possible to inflate balloons without recurring to the ponderous process of preparing ordinary lighting gas. From a report on the experiments which have been made by Prof. Samuel King in the inflation of the "King Carnival," we learn that great success has been met with. The inflation, we are told, was accomplished successfully throughout in seven hours' time. Gas for the purpose was supplied by what may be termed a five-foot generator of the Lowe pattern or build, which employs the modern method of a steam in conjunction with an air blast. Five charges or turns were required to fill the balloon. The amount of gas produced by each charge or turn approximated 6,000 cubic feet. As will be inferred from the two last statements, the capacity of the balloon is about 30,000 cubic feet. The generator employed could have filled the balloon in less than six hours, and would have done so had not the operations been purposely delayed, which delay was occasioned by a state of weather somewhat unfavourable to the ascension which it was proposed to make. The external dimensions of the generator are, height eleven feet, diameter five feet. It is cylindrical in shape, and has an inside fire-brick lining of about six or seven inches thickness, thus leaving a clear diameter for generating purposes of less than four feet. About ten inches of the height is also taken up by the bottom lining. The gas for the inflation was made from anthracite coal. Steam is passed through the incandescent coal. There comes from the generator an impure hydrogen gas containing carbonic acid and oxide of carbon. The carbonic acid is then removed by a suitable and familiar process, and the carbonic oxide remains with the gas. About forty-six lbs. buoyancy is obtained to every thousand feet of gas. The cylinder of the generator can be made in sections, of cast-iron, if no fire-bricks are used, and of wrought-iron if fire-bricks are used. The sections can be luted with clay and then bolted together. The results of these interesting experiments will be laid before the Geographical Society of Paris and Minister of War of France. M. De Fonvielle inquires whether it is really hopeless to find a liquid which might absorb the largest quantity of carbonic oxide, and restore to the hydrogen gas its natural buoyancy.

IT is stated that a committee is being formed in Paris with a view to a permanent International Exhibition at the Crystal Palace. French exhibitors are invited to transfer their productions from the Champ de Mars to Sydenham, and thus realise the original idea of the Crystal Palace as a cosmopolitan museum and warehouse.

WE have received from Mr. Clifton Ward two papers by him reprinted separately—"Quartz as it Occurs in the Lake District," and "Notes on Archæological Remains in the Lake District."

WE have on our table the following books:—"A Manual of Anthropometry," by Charles Roberts, F.R.C.S. (J. and A. Churchill); "The Art of Scientific Discovery," by G. Gore, LL.D., F.R.S. (Longmans); "Dogma, Doubt, and Duty," by Charles Hoare (Aston and Mander); "Leisure-Time Studies, Chiefly Biological," by Andrew Wilson, Ph.D., F.R.P.S.E. (Chatto and Windus); "Medicinal Plants," Parts 32 to 35, by Robert Bentley, F.L.S., and Henry Trimen, M.B., F.L.S. (J. and A. Churchill).

THE additions to the Zoological Society's Gardens during the past week include a Toque Monkey (*Macacus pileatus*) from Ceylon, presented by Mrs. Tranchell; two Macaque Monkeys (*Macacus cynomolgus*) from India, presented by Mr. C. Loveless; a Green Monkey (*Cercopithecus callitrichus*) from West Africa, presented by Mr. Henry Hands; a Grison (*Galictis vittata*) from South America, presented by Mr. H. Potier; a Common Hedgehog (*Erinaceus europæus*), European, presented by Mr. Edwin Etty Sass; a Common Boa (*Boa constrictor*) from South America, presented by Mr. D. W. Bell; two South American Snakes (*Zamenis hippocrepis*) from South America, presented by Mr. G. H. Hawtayne; two Small-Scaled Mastigures (*Uromastix microlepis*) from Busreh, presented by Capt. Phillips; a Mesopotamian Fallow Deer (*Dama mesopotamica*) from Mesopotamia, deposited; two Japanese Pheasants (*Phasianus versicolor*) from Japan, a Gold Pheasant (*Thaumalea picta*) from China, a Lineated Pheasant (*Euplocamus lineatus*) from Pegu, two Barred-Tailed Pheasants (*Phasianus reevesi*) from North China, a Siamese Pheasant (*Euplocamus prælatus*) from Siam, two Swinhoe's Pheasants (*Euplocamus swinhoii*) from Formosa, a Bewick's Swan (*Cygnus bewickii*) from North Asia, purchased.

ON THE VERTICAL DISTRIBUTION OF THE LIGHT FROM LIGHTHOUSES PLACED AT HIGH ELEVATIONS ABOVE THE SEA-LEVEL

THE strongest beam of rays proceeding from lighthouse apparatus in high towers is sent to the sea horizon, as being the direction in which the light can be seen at the greatest distance in clear weather.

My late brother, Mr. Alan Stevenson, suggested the dipping of dioptric lights below the normal to a plumb line in his Report of December 10, 1839, to the Commissioners of Northern Lighthouses in the following passage:—"A more serious inconvenience in using catadioptric zones is that in very high towers where some correction of the position of the apparatus becomes necessary so as to direct the rays to the horizon, the means of regulating the zones in a manner similar to that used for the mirrors is inapplicable. The adoption of a high point in the flame for the focus of these zones, however, affords a considerable compensation for this defect, and it might even be entirely obviated by constructing each set of zones of the form suited to the known height of each tower and the required range of each light if such a correction were found to be of sufficient importance to warrant its application."[1]

But though the precaution of dipping the strongest of the light to the sea horizon was followed out by Mr. Alan Stevenson in high towers it was not always attended to, till the year 1860, when Mr. J. F. Campbell, the Secretary of the Royal Commission on Lighthouses, brought the subject prominently forward, and suggested the internal mode of adjustment. Since then the strongest beam has been invariably dipped to the horizon.

It must, however, be remembered that when the weather becomes even in the least degree thick or hazy, not to say foggy, the range of the light is greatly curtailed by atmospheric absorption and refraction; which last produces during fogs irregular diffusion of the light in every plane. So that at high towers where the beam is pointed to a very distant horizon, it is obvious that the strongest light is directed to a part of the sea, where it cannot be seen with certainty unless when the weather is exceptionally clear.

In an interesting paper on what he calls the "*horizontal visual penetrability of the atmosphere*," lately published in the *Journal* of the Scottish Meteorological Society,[2] Mr. A. Cruikshanks, M.A., makes the following remarks as to daylight observations:—"This shortening of the range of view arises from various atmospheric causes. Such obstructions to the horizontal view as low clouds or mists, or falling showers of rain, snow, or hail circumscribe it at once or abruptly from the observer at a distance of from a few yards to many miles off. The other great obstruction to the horizontal range of view is haze, which may or may not co-exist with the other obstructions, and is supposed to consist of minute particles of water, dust, and smoke floating in the air. The haze, unlike the abrupt obstructions to the view, apparently gradually increases with the distance from the observer till objects become invisible at the distance of three or four miles up to seventy, &c., miles."

Mr. Cruikshanks also gives the following table showing the mean results of twenty-one years' observations made at the middle of the day on terrestrial objects such as hills and mountains:—

Under 5 miles. Seen on days.	5 miles but under 10 miles. Seen on days.	10 miles but under 20 miles Seen on days.	20 miles but under 30 miles. Seen on days.	30 miles but under 40 miles. Seen on days.	40 miles but under 50 miles. Seen on days.	50 miles. Seen on days.
19	—	—	—	—	—	—
81	81	—	—	—	—	—
41	41	41	—	—	—	—
49	49	49	49	—	—	—
38	38	38	38	38	—	—
48	48	48	48	48	48	—
90	90	90	90	90	90	90
366	347	266	225	176	138	90

From this table it appears that during the twenty-one years there were 366 days per annum when an object under five miles distance could be seen, and only ninety days when an object fifty miles distant could be seen.

It has lately appeared to me that the strongest beam should be dipped lower than my brother proposed, and as is now everywhere adopted.

The best of the light should certainly be directed to the place where the safety of shipping most requires it. Now it may in most cases be laid down as axiomatic that the peril of any vessel is inversely proportional to her distance from the danger, whether that danger be a lee shore or an insulated rock. Confining ourselves to this one view of the subject, it would follow that the strongest of the light should, in hazy states of the atmosphere, be thrown as near to the shore, or the rock, as would admit of vessels keeping clear of the danger. But such a restriction as this would, if permanent, greatly impair the usefulness of the light by unduly curtailing its range in clear states of the atmosphere; and of course, *cæteris paribus*, the farther off a sailor is warned of his approach to the shore the better and safer. Besides, the loss due to atmospheric absorption increases in a geometric ratio, and as the rays diverge in cones from the apparatus, the power of the light is further decreased in the inverse ratio of the squares of the distances from the shore.

It is of course well known that the sun itself is extinguished by fog, and we cannot expect to compete with that luminary. But seeing there are endless variations in the density of fogs and in the transparency of the air when there is no fog properly so called, it always appeared to me that had we an easy way of doing so, we ought to increase *temporarily* the dip of the light, and thus during haze and fogs to direct the strongest beam to a point much nearer the shore than the sea horizon. At present we direct our strongest light not only in clear weather, when it *can* be seen, but also during fogs, when it cannot possibly be seen, to a *part of the sea where the danger to shipping is in most situations the smallest*, and this is done to the detriment of that region where, even when the weather is hazy, there is at least some chance of the light being visible, and to a part of the sea where the danger to shipping is unquestionably the greatest.

The simplest mode of depressing the light temporarily would be to raise the lamp itself in relation to the focal plane of the lens. But this is, for several reasons, very inexpedient. The proper adjustment of the apparatus to the focus, so as to secure

[1] "On the Application of Catadioptric Zones to Lighthouses," by Alan Stevenson, LL.B. (Edinburgh, 1840.)

[2] *Journal* of the Scottish Meteorological Society, new series, vol. v. p. 97.

its being situate in the section of maximum luminosity of the flame, is a somewhat delicate one, and ought, if possible, not to be disturbed oftener than is necessary for changing the lamp. Moreover, while the raising of the lamp would depress the light which passes through the refracting portion of the apparatus it would have precisely the opposite effect upon the portions which pass through the totally reflecting prisms placed above and below the refracting part, which would then throw the rays upwards to the sky, where they would be useless. But any desired change could be effected by surrounding the flame with prisms spheric on their inner faces, and concentric with the foci of the different parts of the apparatus, so as to depress the rays before they fall upon the main apparatus. Those prisms which subtend the lens would have their thicker ends lowest, and those subtending the reflectors would have their thicker ends uppermost.

The great disadvantage, unless in the case of electric lights, of employing the temporary apparatus which has just been described, arises from the loss of light by divergence, due to the relation subsisting between the radius of the flame and the radii of curvature of the apparatus itself.

But this loss may be prevented by another plan. Outside of the apparatus, and either close to it, or what would be more convenient, close to the glazing of the lantern, movable refractors made of panes of plate glass could be placed during fogs. In ordinary states of the weather, these fog screens, which would be hung by chains passing over pulleys fixed to the top of the lantern, would be close to the inside of the parapet wall of the light-room, and below the apparatus. If these refractors were fitted with counter-weights they could, in the course of a few minutes, be hauled up in front of the apparatus by the hand when the thick weather came on, and pulled down again when it became clear. The panes of plate glass which act as refractors, would be of prismoidal section vertically having their thicker ends placed downwards. The vertical angle of the prisms would in each case depend on the height of the light-room above the sea, and the distance off shore to which the strongest beam of light required to be dipped during fog. But after more fully considering the question, I have come to the conclusion that a great improvement could be effected even without resorting to temporary expedients. From a series of observations made with two kinds of photometer by Messrs. Stevenson, in 1865, on the penetrative power of light from a first order lens and cylindric refractor, it appears that for an angle of 0° 30′ in altitude above the plane of maximum intensity, and for 0° 30′ below that plane, the power of the light does not vary more than at greatest from 4 to 6 per cent., and that if the strongest part be sent to the horizon, about one-half of the whole is sent uselessly to the skies.

Power of Lens in the Vertical Plane.

° ′		Means of four sets of observations.
0 40	above the level of maximum	·90
0 30	,, ,,	·94
0 20	,, ,,	·97
0 10	,, ,,	·98
0 0	maximum power	1·00
0 10	below the level of maximum	·99
0 20	,, ,,	·97
0 30	,, ,,	·96

Note.—These results, which are the means of four sets of observations, did not extend further in the vertical plane.

Result of Dipping Light as Proposed, Contrasted with Present System.

Present System. Above horizon. ° ′	Power.	Proposed System. Above horizon. ° ′	Power.
0 40	·90 lost on sky.	0 20	·90 lost on sky.
0 30	·94 ,, ,,	0 10	·94 ,, ,,
0 20	·97 ,, ,,	0 0	·97 on horizon.
0 10	·98 ,, ,,		
0 0	1·00 on horizon.		
Below horizon.		Below horizon.	
0 10	·99 on sea.	0 10	·98 on sea.
0 20	·97 ,,	0 20	1·00 ,,
0 30	·96 ,,	0 30	·99 ,,
		0 40	·97 ,,
		0 50	·96 ,,

Applying these observations, so far as they extend in the vertical plane, to the case of lighthouses elevated much above sea-level, we see that to dip the strongest beam to a point much nearer the shore than the sea horizon, while it would not appreciably affect the visibility there, would even, so far as the observations go, increase the power of the light nearer the shore. Those who have been close to a lighthouse on a hazy night must have noticed the luminous rays passing through the air far above the sea-level, and cases are adduced by Mr. Beazeley of shipwrecks having occurred when the light could not be seen by the sailors, although their vessels were stranded close to the tower. As the lens has the greatest divergence, and is the only agent for giving light near the shore, it only should be dipped so as to throw as few of the rays as possible uselessly on the skies, while the reflecting prisms, which have much less divergence, will remain as at present throwing their rays to the horizon. By this different distribution of the light from the lens and the prisms, although the strongest beams from the lens were dipped 0° 20′ below the horizon, which causes a loss there of 3 per cent. of lens power, yet the loss on the *whole* light coming from both lens and prisms, taken at Mr. Chance's valuation of 70 and 30 respectively, will be reduced to only about 2 per cent., while the sea near the shore will be more powerfully illuminated than at present. It may, however, be fairly questioned whether the strongest beam ought not to be dipped to 0° 30′, as this would still further increase the power near the shore, and would only depreciate the light at the horizon by about 5·8 per cent. It is well to remember that, should the flame, through neglect of the keeper, fall at any time below the standard height, such a defect will operate most injuriously on the light falling near the shore, and not so much on that sent to the horizon. Now there can be no question that in all ordinary cases a vessel with such an offing as twenty miles, which is the sea-range due to 300 feet of elevation, is in a far safer position than if she were within a mile or two of the shore, and hence the propriety of increasing the light near the shore so long as we do not to any appreciable extent reduce it at the horizon.

T. STEVENSON

MEDICAL ENDOWMENTS AT OXFORD

WE have been requested to publish the following details of existing endowments assigned by founders to the study of Medicine and of Human Anatomy and Physiology as bearing on Medicine :—

I. The Regius Professorship of Medicine, as at present constituted, is worth about 500*l*. a year. The items are : (1) from the Queen's exchequer, 35*l*. ; (2) as Master of Ewelme Almshouse, 250*l*. ; (3) as Aldrichian Professor of Medicine, 126*l*. ; (4) examination and graduation fees, 70*l*. to 100*l*.

II. Lord Lichfield's Clinical Professorship, which is not united with the Regius Professorship, is worth 200*l*. a year.

Dr. H. W. Acland holds both the Regius and the Clinical Professorships : no instruction is given by Dr. Acland in either capacity.

III. The Linacre Professorship of Physiology and Anatomy has absorbed the old foundations for the encouragement of human anatomy, namely, the Tomlinsian Prælectorship and the Aldrichian Professorship. It is worth 800*l*. a year, the sum which Merton College pays in place of the original endowment entrusted to it by Thomas Linacre, founder of the College of Physicians, and once a lecturer on medicine in Oxford. The Linacre Professor is engaged in teaching Comparative Anatomy to candidates for the B.A. degree.

IV. A separate Demonstratorship of Anatomy, worth 200*l*. a year, also still exists, and was intended by the commissioners of 1852 to provide for the teaching of human anatomy, as designed by Tomlins and Aldrich. The gentleman who holds this post is Curator of the Museum of Comparative Anatomy and does not teach Human Anatomy.

V. The beautiful old Physic Garden founded by Earl Danby in 1622 is another heirloom of the Medical Faculty of Oxford. The chair of Botany was endowed by Dr. Sherard and the College of Physicians of London elect the professor. By special provision, the clergy are excluded from this professorship, and *preference is to be given to a medical graduate.* The chair is now worth, with later additions, about 400*l*. a year.

VI. Lastly, a very important trust fund is administered by the governing body of Christ Church, the bulk of which was left by Dr. Matthew Lee in 1755 to provide for anatomical teaching in relation to medicine *exclusively*. Dr. Lee's expression of his intentions is very clear and precise. He assigns, in his will, 100*l.* a year as the salary of a reader in anatomy; 50*l.* for expenses of two bodies and dissection; 30*l.* to a reader in mathematics and physics, and the remainder to be given in annual prizes of 10*l.* to scholars from Westminster School. The trust is now worth 3,400*l.* annually. It is spoken of in the return made by Christ Church to the Commissioners of 1874 as "Dr. Lee's Benefaction for Senior Students in Natural Science." This is not quite accurate: firstly, because Dr. Lee designed the major portion of his benefaction for students in anatomy as bearing on medicine, and not for natural science generally; and, secondly, because Christ Church uses nearly half of Dr. Lee's trust-money to pay classical scholars from Westminster School; whilst the remainder is used to support a most efficient chemical laboratory, and to pay, in part, the salaries of the accomplished chemist, zoologist, and physicist, who are styled "Lee's Readers." No part of Dr. Lee's bequest is now assigned to medical studies, though it should be stated that the present application of Dr. Lee's fund has obtained Parliamentary sanction.

ON THE THERMAL PHENOMENA PRODUCED BY THE PASSAGE OF ELECTRICITY THROUGH RAREFIED GASES[1]

A PORTION of the experiments described in this paper were made before the publication of Wiedemann's experiments on the same subject. The authors state that if they had known of the work of the German physicist they would probably not have undertaken the investigation, but they have continued the experiments and think them worthy of description as the methods employed differ from those of Wiedemann.

The apparatus used consisted of ordinary Geissler tubes, the electrodes being in wide tubes connected by a narrow one. A large Ruhmkorff's coil with a secondary wire 100,000 metres in length was set in action by two large Bunsen cells, and the current was made and broken by a Foucault's interrupter. In order to measure the induced current a reflecting galvanometer was employed, being placed at such a distance from the coil that the effect of the electro-magnet on the needle was very small; this slight deviation was, however, applied as a correction in all the readings. It was first proved that the current induced on completing the primary circuit was incapable of passing through the Geissler tube, for the galvanometer needle was equally deflected whether placed in the secondary circuit or not, indicating that the movement of the needle was due solely to the direct action of the magnet of the coil; on the contrary, when the primary circuit was broken, a considerable deflection of the needle occurred when the galvanometer was in the secondary circuit, and a slight one, but in the opposite direction, when the needle was influenced by the magnetism of the coil alone.

Tubes containing chlorine, carbonic anhydride, and hydrogen, were employed in the experiments, the electrodes being soldered to the wires from the coil by means of Wood's fusible alloy. The narrow part of the tube was placed in a copper cylinder containing water, or preferably mercury, in which a delicate thermometer was immersed, the deflection of the galvanometer was read every thirty seconds, and the thermometer every minute. When the current indicated by the galvanometer was greatest the increase of temperature was most rapid, but the important fact shown by these experiments is that in every case the rise of temperature divided by the deflection is a constant. Unfortunately the different constants are not comparable, as the experiments were not made with the same tube containing the different gases at known pressures, but with different tubes of nearly the same sizes; but the pressures of the gases are not given.

Some experiments were undertaken to determine the heating effects in the neighbourhood of the electrodes. For this purpose the upper end of a Geissler tube containing chlorine was placed in the calorimeter, the latter surrounding the part of the tube containing the platinum wire. When the electrode was negative about eight times more heat was developed than when it was positive. As the quantity of heat produced when the electrode was positive was very small the relation between the deflection of the galvanometer and the rise of temperature was not so regular in the different experiments as in the other case; and when the electrode was negative it was observed that the quantity of heat increased a little more rapidly than the deflection. More accurate results were obtained by immersing the lower end of the tube in the calorimeter; under these circumstances the quantity of heat collected when the electrode was negative was 22·8 times as great as in the experiments in which it was positive, and while the deflection of the galvanometer varied from 100 to 640, the rise of temperature divided by the deflections increased from 100 to 120 only.

It being suspected that the different effects at the two electrodes might be due to a cause similar to the Peltier effect in solid conductors, an attempt was made to discover if the positive electrode is cooled during the passage of the electricity. There appeared to be a very slight diminution of temperature at the commencement of the experiment, but it was soon marked by the conduction of heat from other parts of the tube.

The calorimeter was next placed on the wide part of the Geissler tube, but not surrounding the electrode, which was 16·5 mm. from the calorimeter. In this case, also, a larger quantity of heat was developed near the negative electrode than near the positive, but the ratio was only 4·9. When a portion of the narrow part of the tube was placed in the calorimeter, that near the negative electrode was slightly more heated than the other portion. When a tube containing hydrogen was used similar results were obtained, but the difference between the quantities of heat at the two ends was very much less than in the case of chlorine.

The next series of experiments was made to determine the effect of different diameters of tube. For this purpose a U-tube containing air of the pressure of two mm. was used, one limb of the tube having a sectional area of 36·3 square mm., the other 12·6 square mm., both limbs being surrounded by calorimeters. The quantity of heat developed in the narrow tube was only very slightly greater than in the other, the ratio being about 1·1. By using another tube with the areas of 116·9 square mm. and 4·5 a similar result was found, but in this case the ratio was not greater than 1·2.

[1] By Dr. Naccari and M. Bellati (vol. iv. ser. v., degli *Atti del R. Instituto veneto de Science, Lettere ed Arti*).

NOTES FROM NEW ZEALAND

THE following notes have been sent us from New Zealand by Mr. T. H. Potts, of Ohinitahi:—

Maori Food Feast.—At the great meeting of Kingite natives convened by Tawhaio to meet Sir George Grey and the Hon. Native Minister, amongst other very interesting incidents was the food feast which was held at Hikurangi on May 8.

A procession formed of several hundreds of women, each carrying a neatly woven basket filled with food, proceeded through the village till it arrived opposite to Sir George Grey's tent; at a given signal the baskets were placed on the ground and stacked into a huge heap. The presentation of each article of food was accompanied with an appropriate chant or ngori, with dancing and facial contortions of an extraordinary character, many of the most ancient persons of different hapus taking part in the celebration.

Amongst the various articles of vegetable food in season was offered:—

Pohua.—The root of *Convolvulus sepium*, as flowery as a potato with a slightly bitter taste.

Sowthistle.—*Sonchus oleraceus.* The Hauhaus, when compelled to use cooked sowthistle, found to their surprise they did not lose condition on this spare diet.

Para.—The thick solid scale from the rootstock of the grand fern *Marattia fraxinea.* This edible was pinkish or pale purple when cut, solid, tough, almost tasteless, with a slightly bitter flavour.

Marnaku.—This esculent appeared in thick junks of about a foot in length; it is the mucilaginous pith of the great black tree-fern *Cyathea medullaris.* It was presented ready dressed, was soft, very sweet to the palate.

Roi.—The rhizome or root of the bracken *Pteris aquilina*, var. *esculenta.* It was offered in the uncooked state, in which it is usually kept ready for use.

Tawha.—The prepared berries of a common forest tree *Nesodaphne tawha.*

Hakeke.—The Jew's-ear fungus *Hirneola auricula-Judæ.* It is found in the forests of Pirongia; that which grows on the Karaka is most esteemed.

On Moa Remains, &c.—There has been so warm a controversy as to the probable date of the extinction of the dinornis, that any reliable fact connected with the remains of the wonderful animals may be of value to the biological student.

On the low hills and flats north-east of the gorge of the Rakaia, from 1854 to 1858, there were quantities of gizzard-stones lying in small heaps on the surface of the ground; for years no one collected them for scientific purposes, but boys or bush-hands sometimes turned over the heaps, and picked out a "few pretty ones" that happened to take their fancy.

In April, 1857, with two friends, I went up the course of the Rakaia, followed the southern stream, then through the country west of Mount Hutt and Mount Somers, returned to the "plains" by the Ashburton or Haketere River; this was then all new country, not taken up. On the southern side of the Ashburton or Haketere River, on the flats above the gorge, a vast number of moa bones lay exposed *on the surface of the soil;* after I had taken up a run there, I used frequently to pick up specimens from amongst these bones and throw them into heaps, with the view of making a selection therefrom at some future time.

It may be worth mentioning that near that spot, at a now well-known place called "Paddle Hill," I found a paddle made of totana (*Podocarpus totana*), with a longer handle and much broader blade than any hoe that I have seen used by natives; it seemed too large for a paddler kneeling or squatting; it had probably been used to propel a moki or raft.

The Pahu.—The Hauhaus at the Hikurangi meeting were called to their place of worship by the beating of the pahu; it is a long, sonorous piece of wood, made (when possible) from an aromatic tree called porokaiwhiria (*Hedycaria dentata*). It is hung from a cross pole supported at either end by a forked stick. The sound may be heard to an extraordinary distance. It is produced from this rough kind of wooden drum being beaten on its edges by several persons furnished with short batons.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

THE Cambridge Syndicate appointed to consider the higher mathematical studies and examinations of the university have issued a further report in which they state that they have been led by the observations of members of the Senate in the Arts School, and by the results of the voting on the scheme of examination recommended in their report of March 29, to believe that in the opinion of the Senate the examination in Part III. of the Mathematical Tripos should be independent of the preceding parts, and also that the Senate would be averse to any scheme in which it was not provided that all the subjects should be included in the examinations of each year. They have framed regulations in accordance with these principles in substitution for those rejected by the Senate on May 29, under which it is provided that from the results of the examination in Parts I. and II., the candidates shall be arranged in order of merit as Wranglers, Senior Optimes, and Junior Optimes. Only Wranglers are to be admitted to the examination in Part III., and from the result of the examination in that part only, the Moderators and Examiners shall publish in three divisions, each division arranged alphabetically, a list of those examined and approved. Power is given to the Moderators and Examiners to place in the first division any candidate who has shown eminent proficiency in the subjects of any one group in Schedule III. In each of the papers in bookwork in Part III. a limit is to be fixed to the number of questions to which any candidate shall be permitted to send in answers, such limit to be printed at the head of each paper. The subjects in Part III. are grouped in four divisions. Group A consists of differential equations, calculus of variations, higher algebra, higher parts of theory of equations, higher analytical geometry (plane and solid), finite differences, higher definite integrals, elliptic functions, theory of chances, including combination of observations. Group B, Laplace's and allied functions, attractions, higher dynamics, Newton's "Principia," book i. sec. ix.–xi., lunar and planetary theories, figure of the Earth, precession and nutation. Group C, hydrodynamics, including waves and tide, sound, physical optics, vibrations of strings and bars, elastic solids. Group D, expression of functions by series or integrals, involving sines and cosines, thermodynamics, conduction of heat, electricity, magnetism. From the discussion which took place on the subject on November 2, opinion seems to be pretty much divided at Cambridge as to the advisability of the proposed alterations.

A POST-MASTERSHIP at Merton College, Oxford, for Natural Science, has been awarded to Mr. Geo. Howson, a pupil of Giggleswick School.

SCIENTIFIC SERIALS

Memorie della Società degli Spettroscopisti Italiani, April, 1878, contains a table showing the solar spots and faculæ for each day of observation for the months of January, February, and March 1877. A note by Prof. Tacchini on the solar spots of the first three months of 1877, compared with those of the same months in 1878.—An account of the observations of solar prominences made at Palermo during the first three months of the present year.—Drawings of the chromosphere for the months of March, April, May, and June, 1871.

May.—This number contains full accounts of the transit of Mercury of May 6, 1878, as observed at Rome, with notes on the same by Respighi, St. Ferrari, Millosevich, and Tacchini. —Drawings of the chromosphere for June and July 1871.

June.—Tables of solar spots and faculæ for April and May, 1878.—Note on the spots and solar eruptions of April, May, and June, 1878, by Prof. Tacchini.—A long paper on a cause for the appearance of bright lines in the solar spectrum, by Mr. Meldola.

July.—A paper containing tables of the solar prominences observed at Palermo in April, May, and June, 1878, by Prof. Tacchini.—A long paper by Schiaparelli on the observations of the rotation and topography of the planet Mars made at Milan during the opposition of 1877.

August.—Spectroscopic observations of the chromosphere made at Palermo during the months of April, May, and June, 1878. Tables showing the number of protuberances and spots on the sun for each day of observation for the months of August, September, October, November, and December, as seen at Rome, with notes thereon.—Drawings of the chromosphere for the last part of July, 1871, made at Palermo.

Bulletin de l'Académie Royale de Belgique, No. 7, 1878.—It has been affirmed by more than one observer that, during auroræ boreales, the intensity of scintillation of stars has been considerably increased; a singular influence, if real. M. Montigny, testing such statements, observed twice such an increase (on the nights of April 5, 1870, and June 1, 1878). He notes the fact that this increase coincided with a lowering of temperature of the air in the locality. In the one case this fall occurred exactly at the moment of the aurora and observation of the scintillation; in the other case it preceded the aurora, but was more marked the night of it, and a few hours after observation of the increase in question. He considers the increase due to the cooling, which must have affected first the upper regions of the atmosphere through which the stars' rays pass; and this agrees with the fact that the stars furthest above the horizon showed the increase most.—MM. Spring and Durand study some obscure points in the composition of oxygenated compounds of nitrogen. Finding that the products of reaction, with water, of the body formed by action of chlorine on nitrite of silver, are exclusively nitric acid and chlorhydric acid, their surmise was verified that the chlorine is substituted for the silver of the nitrite, atom for atom, forming nitric chloride. Hence the structure of the group NO_2 of nitric acid is inferred to be the same as that of its correspondent nitrous acid, and the rational formulæ of these two substances must be written respectively $HO.N = O$ and $HO.O - N = O$. M. Melsens seeks to refute M. du Moncel's statements about the cost of his system of lightning-conductors, as applied to the Hotel de Ville, in Brussels, and criticises the instructions of the Paris Commission for erecting conductors on public buildings. —Some letters in a controversy between M. du Moncel and MM. Navez on the theory of the telephone, appear in this number.—M. Malaise announces the discovery of Brachiopoda of the genus Lingula in the Cambrian formation of Stavelot.

No. 8.—The digestion of albuminoids in some invertebrates forms the subject of a paper here from Dr. Fredericq. From a combination of his results with those got by Hoppe-Seyler and Plateau, it appears that the mechanism of digestion is the same throughout the animal kingdom, and the transformation of food in invertebrates is effected through substances that have the greatest similarity to the digestive ferments of vertebrates (solubility in water, precipitation by alcohol). Digestion by means of a peptic ferment is very rare among invertebrates; a ferment similar to thrypsine, on the other hand, is met with among different classes of these animals.—M. Plateau communicates an account of experiments (with the graphic method and poisons of the heart), on the movements and innervation of the central organ of circulation in articulate animals. *Inter alia*, section of the cardiac nerve diminishes the number of pulsations (in vertebrates, it produces acceleration).—M. Renard describes the diabase of Challes, near Stavelot, in the Cambrian system.—Dr. Koninck continues his researches on Belgian minerals; and there are some papers on mathematical subjects.

THE *Bulletin de l'Académie Impériale des Sciences de St. Pétersbourg* (vol. xxv. No. 2) contains the following papers of interest:—On the occurrence of the musculus peroneo tibialis in Quadrumana, by Dr. Wenzel Gruber.—On a control barometer, by H. Wild (with plate).—On the reduction of Kirchhoff's spectral observations to wave-lengths, by Dr. B. Hasselberg.—On the observed ingress of Mercury upon the solar disc, at the transit of 1878, May 6, by O. Struve, of Pulkowa (with plate),—Catalogue of forty-two new red stars, by E. Lindemann.—On the Russian species of the mollusc *Clausilia*, Drap., by Dr. Oskar Boettger.—On the theory of curves of the shortest parameter on curved surfaces, by F. Minding.—On the hexylenes resulting from tertiary hexylalcohols and their polymerisation, by L. Jawein.—On the action of tertiary iodide of butyl upon isobutylene in the presence of metallic oxides, by J. Lermontoff.—On tetramethylethyl and its derivates, and on the chemical composition of pinacone, by D. Pawlow.

The Journal of the Russian Chemical and Physical Societies of St. Petersburg (vol. x. No. 6) contains the following papers of interest:—On tetramethylethylene and the chemical structure of pinacone, by M. D. Pavloff.—On the glucose derived from lactose, by M. Foudakovsky.—On the action of bromide of aluminium in the formation of the bromides of aromatic hydrocarbons, by G. Gustavson.—On the dextrogyrate terpene obtained from Russian terpentine, by F. Flavitzky.—On the chemical structure of terpenes, by the same.—On dibenzoyldinitrodiphenol, by M. Goldstein.—On some new mineral springs in the Caucasus, by E. Wroblewsky.—On the influence of the surrounding medium upon electrodynamical actions, by J. Borgmann.—On the determination of the magnetic function of liquids, by the same.

Verhandlungen der naturforschenden Gesellschaft zu Freiburg i. B. (vol. vii. part 2).—From the part we note the following papers: On organic cyanides and their decomposition, by A. Claus.—Note on wine analysis, by the same.—On the equilibrium of a system of expanded molecules and the theory of elastic after effects, by E. Warburg.—Observations on the torsion oscillations of an iron wire heated to redness, by Dr. Messer.—On the sensitiveness of alum crystals towards minute variations in the concentration of their mother-liquor, by F. Klocke.

SOCIETIES AND ACADEMIES

CAMBRIDGE

Philosophical Society, October 21.—Dr. Pearson read a paper on a series of lunar distances taken by him during the years 1875-77, mostly at Cambridge and at a place not far distant, the longitude and latitude of each spot being accurately known. He said that they entirely bore out the conclusions at which he had arrived some time back from a much smaller number of observations, and which were communicated by him to the Society in a paper read by him, March 13, 1876 (see *Proceedings*, ii. pp. 414-418), viz., that the errors are such as cannot be called errors of observation of any kind, and may probably arise from the solution of the spherical problem on which the result depends not being, as at present given, strictly accurate. It was mentioned that there is much to justify this conclusion; for example, this method of obtaining longitudes is not much resorted to now in practice (from which it may be argued that it is actually found inaccurate). It is not formally adopted in Germany, though it still is retained in the *Nautical Almanac*, and in the corresponding publication, the *Connaissance des Temps*, issued at Paris. Capt. Toynbee, F.R.A.S., in a paper in the *Nautical Magazine* for February, 1850 (of which there is an abstract in the *Monthly Notices* of the R.A.S.), distinctly states that lunars taken east of the moon give always a result thirty or forty seconds different from those taken west, though his mean result he says was entirely satisfactory; and until the early part of this century all East Indian longitudes were in error nearly 3m. to the east, a result which very nearly agrees with the errors resulting from these observations, supposing them to have been deduced from the new moon of five to eight days old, probably the most convenient time at which to take them from the sun. The whole series, it was stated, consists of 250 separate distances, each distance being either a mean of three or two, or else only one observation, there being about an equal number of each class, though there is no reason to think that the last are less trustworthy than the others in any serious degree; the Greenwich mean time for each being established, with the exception of a very few, within certainly ten seconds. Only 200 of these, the number at present thoroughly verified, were discussed on the present occasion. Classing these in groups of about forty, it was found that the first group gave thirty-two results where the measured distance was in defect of the theoretical distance, and thirteen in which it was in excess. Assuming the rule given in p. 417 of the paper referred to to be correct, this result exactly agrees with what might be expected, it being almost always most convenient, especially for a beginner, to take lunars, at any rate from the sun, under such circumstances as will give this result, while the example of India, founded apparently on observations made at Madras, seems to imply this probable facility, and also that they were made on the new moon, these being more easily taken in our hemisphere than those made on the old one. In the four remaining groups the proportions are 26 to 18: 28 to 15: 25 to 17: 17 to 14: giving a total of 128 observations in defect, and 77 in excess. Rejecting three or four certainly questionable results, the greatest errors occurring are 2′ 59″ in defect, and 2′ 48″ in excess. The true mean has not yet been ascertained, but is certainly in each case not far from 1′—1′ 20″; which, on an average, will give the observer an error of about half a degree of longitude, or of twenty to thirty-five miles, advancing from our own latitude to the equator. There are probably not a dozen clear exceptions to the rule suggested in the communication of March, 1876, that if the luminaries are both on the same side of the meridian, the observed distance is always in defect of the true if the moon be nearest to it, and in excess if she is farther distant; while the same rule holds good, but with less certainty, when the two luminaries are on different sides. The only exceptions seem to arise where the one more distant from the meridian has a greater altitude than the other, or is of a considerably higher declination, and when the distances are very great, *i.e.* from 120° to 130°, in which case the measured distance seems generally to be slightly in excess of the true; but, as might naturally be expected, these last distances cannot often be taken in our own climate. It was explained that all the reductions had been made by Borda's formula, stated in the *Philosophical Transactions* for 1797 to have been the first strictly mathematical solution of the problem. But the results vary only by a few seconds of arc from those given by the system adopted in the large folio published in 1772 by Mr. Shepherd, Plumian Professor at Cambridge University under the superintendence of the Commissioners of Longitude, and while Dr. Maskelyne was Astronomer-Royal; or from other methods which it is believed are allied to this. Two examples were also exhibited of distances reduced according to the elaborate method suggested by Bessel in the *Astronomische Nachrichten* of 1832; Bessel's results, however, do not differ to any great extent from those obtained otherwise. It was suggested that the problem is really one of spherical trigonometry, and from the fact that the errors seem to depend on the position of the luminaries towards the meridian, whereas the old methods of solution depend on their altitudes, and also that the different ways suggested for eliminating the error due to the difference between the geocentric and geographical latitude of the place of observation give different results, a hope was expressed that if these two circumstances were thoroughly reconsidered in dealing with the question, means might be found of discovering a farther correction of the observed distance, which would give a really accurate result.

MANCHESTER

Literary and Philosophical Society, October 15.—J. P. Joule, F.R.S., &c., president, in the chair.—Relative brightness of the planets Venus and Mercury, by James Nasmyth, C.E., F.R.A.S., Corresponding Member of the Society. "On many occasions, when observing Mercury and Venus in full daylight, I have always been impressed with the strikingly inferior brightness of Mercury as compared with Venus; and as such a condition is the very reverse of what might be expected by reason of Mercury being so much nearer to the sun than Venus, I awaited the rare event of a very close conjunction of these two planets that occurred on September 26 and 27 last. With the advantage of a perfectly clear sky I had the two planets before me for several hours, so to speak, side by side in the field of the telescope at the same time, thus affording me a most perfect opportunity for making a comparison of their relative brightness. It is difficult to convey in words an exact impression of the difference in the brightness of such objects, but I may attempt to do so by stating that Venus looked like clean silver, while Mercury looked like lead or zinc. Were I to indicate my impressions by way of number I would say that Venus was fully twice as bright as Mercury. So remarkable an inferiority in the brightness of Mercury, notwithstanding his much greater nearness to the sun, appears to me to indicate the existence of some very special and peculiar condition of his surface in respect to his capability of reflecting light—a condition that may be due to the nature of his envelope, if such exist, or of that of his surface, by which the fervid light of the sun's rays falling on him are in a great measure quenched or absorbed so as to leave but a small residue to be reflected from his surface. If this be so, it appears to me to be reasonable to suppose that the absorption of so much light must result in a vast increase in the heat of the surface of Mercury beyond what would have been the case had Mercury possessed the same surface conditions as Venus. Whether in the progress of spectroscopic investigation we shall ever be enabled to detect some evidence of metallic or other vapours or gases clinging to or closely enveloping the surface of Mercury that might in some respect account for so remarkable an absorption of the sun's light, we must be content to await the acquirement of such evidence if it ever be forthcoming. It appears to me, however, to be well to raise such a question, so that our astronomical spectroscopists may be on the outlook for some evidence of the cause of so very remarkable a defective condition in the light-reflecting power of Mercury to which I have thus endeavoured to direct attention."—On the water of Thirlmere, by Harry Grimshaw, F.C.S., and Clifford Grimshaw.

Paris

Academy of Sciences, October 28.—M. Fizeau in the chair.—M. De la Gourmerie read a note on the works of the late M. Bienaymé.—The following papers were also read:—On the decomposition of hydracids by metals, by M. Berthelot. The heat of formation of gaseous chlorhydric acid from its elements is surpassed by that of all anhydrous chlorides, even chlorides of lead, copper, mercury, and silver; gold is the only exception among ordinary metals. The inference that all these metals, except gold, must decompose chlorhydric gas with liberation of hydrogen, is confirmed by experiment. Platina and palladium, also, their chlorides having low heats of formation and little stability, did not decompose chlorhydric gas up to 550°.—On Vice-Admiral Cloué's "Pilote de Terre Neuve," by M. Faye.—On the state in which carbonic acid exists in the blood and the tissues, by M. Bert. The escape of carbonic acid during the respiratory act requires a dissociation of the super-carbonised salts of the blood. These salts were saturated with carbonic acid neither in the arterial nor the venous blood, nor in the tissues. The life of the anatomical elements can only be maintained in presence of carbonic acid in the state of combination. When the alkalis are saturated, and this gas appears in excess in the state of simple solution it rapidly causes death.—Influence of the nervous system on the phenomena of absorption, by M. Moreau. He attached to the dorsal fins of fishes that had swimming bladders a small glass balloon, lighter than the water, and in a few hours the volume of the fish had diminished through absorption of a part of the air contained in the bladder. When a piece of metal was substituted for the balloon, the volume of the fish increased. There is thus a sensation of thrust upwards or downwards, and it is under influence of the former that absorption of air in the bladder takes place, probably through a reflex action.—On *decipium*, a new metal of samarskite, by M. Delafontaine. In the samarskite of North Carolina he finds yttria, erbine, terbine, philippine (yellow (PpO), equiv. about 90; characteristic absorption band about 449 in λ), decipine (white (DpO), equiv. about 122, band 416); thorine and oxides of didymium and cerium. The equivalents of the metals in some of these earths are shown to present interesting numerical relations. *Decipium* is so called from *decipiens*, deceiving. The didymium of cerite is probably a mixture of several bodies, by M. Delafontaine. This is based on spectral observations.—Reply to a recent communication by M. Hirn, on a gyroscopic apparatus, by M. Gruey.—Classification of double stars, by M. Flammarion. Of the 11,000 double or multiple stars discovered, he finds there are only 819 that give certain indications of a relative motion of the components. These groups are divided into 731 doubles, 73 triples, 12 quadruples, 2 quintuples, and 1 sextuple, in all 1,745 stars variously associated. Of these couples in motion 558 have been found with orbital systems, and 316 whose components have been connected merely by the chance of celestial perspectives and form optical groups. In the orbital systems there is a preponderance of retrograde motion from north to south by west (Several other facts are given.)—On the integration of the equation (1) $Ay'^2 + Byy' + Cy^2 + Dy' + Ey + F = O$, by M. Alexeeff.—On involution in curves of n degree, by M. Serret.—Remarks on two integrals obtained by Lamé in the analytical theory of heat, by M. Escary.—Reply to an observation of M. Boltzmann, by M. Lévy.—On the magnetisation of tubes of steel, by M. Gaugain. When a system formed of two parts having different coercive forces is subjected to action of a weak current, the part having the least coercive force is always that which takes the strongest magnetisation (whichever its position, tube, or core).—On a telephone call, by M. Perrodon. This consists in connecting a Ruhmkorff coil with the plate of the telephone, so as to get a loud continuous sound.—On the transformation of valerylene into terpilene, by M. Bouchardat.—Artificial reproduction of melanochroïte, by M. Meunier. This is by keeping fragments of galena in dilute aqueous solution of bichromate of potash.—On the elimination of salicylate of soda, and the action of this salt on the heart, by MM. Blanchier and Bochefontaine. It stimulates various secretions, notably the salivary. In man it is at once expelled by the kidneys (appearing in the urine in 20 mm.); in the dog it appears both in the urine and the saliva, also in the bile and pancreatic fluid. The hypersecretion of saliva is due to action of the salicylate on the grey substance of the central nervous system. In strong doses, the salt stops the heart in diastole.—On parthenogenesis in bees, by M. Sanson.

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