THURSDAY, MAY 30, 1878

BALFOUR ON ELASMOBRANCH FISHES

A Monograph on the Development of Elasmobranch Fishes. By F. M. Balfour, M.A., Fellow and Lecturer of Trinity College, Cambridge. (London: Macmillan and Co., 1878.)

MR. BALFOUR has finally completed and issued in the form of an octavo volume the researches on the embryology of the dog-fish and its allies, which he commenced at the now celebrated zoological station of Naples in 1874. His results have been made known from time to time during the progress of his work by a preliminary paper in the *Quarterly Journal of Microscopical Science*, October, 1874, and by a series of articles in the *Journal of Anatomy and Physiology*, the latter, indeed, being identical with the successive chapters of the present volume. Looking at the work as a whole, we may heartily congratulate not only Mr. Balfour, but English science, on the very great value of this contribution to knowledge. Mr. Balfour, before entering upon the study of the deveopment of the shark-like fishes, had thoroughly qualified himself for the task by a careful investigation of the development of the common fowl, a subject which, although it had always been and remains the favourite, because the most handy, for the embryologist's study, yet yielded several new and interesting results to Mr. Balfour's examination. The methods which are applicable to the hardening and slicing, staining and clarifying of the embryo chick are precisely those which it is necessary to employ in the investigation of the very similar egg of the Elasmobranchs, and accordingly Mr. Balfour had well trained himself for the latter task. The prominent position in Vertebrate morphology which had been assigned to the group of Elasmobranch fishes, through the researches of Gegenbaur, rendered a minute examination of their developmental history urgent. It had become clear that we have in these fishes the nearest living representatives of the common ancestors of the great group of Gnathostomous Craniate Vertebrata, and it was to be expected that a full knowledge of their ontogeny or individual development would carry us yet further back in the line of primitive Vertebrata, and yield a mass of explanatory evidence, exhibiting the development of complex and heterogeneous structures from simpler and more homogeneous forms, likely to serve as a satisfactory starting-point for all Vertebrate morphology.

Mr. Balfour has shown in the course of his investigation of this subject not only that he is possessed of the technical skill necessary for the manipulation of such embryos, but that he is gifted with a very large measure of patience and perseverance, and has, moreover, the high critical and speculative capacities which the subject demands for its full and successful treatment.

We shall very briefly notice the successive chapters of Mr. Balfour's monograph, and point to the more important novel observations recorded, having especial regard to those which may be considered as fundamental for the morphology of Vertebrata.

Mr. Balfour begins with the ovarian ovum of the Elasmobranch, this portion of his observations having been made on the skate. He shows that the germinal vesicle atrophies before impregnation. He then proceeds to describe the process of segmentation, which, in its general features, is similar to that of the bird, the only other egg containing so large a proportion of food-material mixed up with the protoplasm of the egg-cell. In the study of the division of the first-formed cells resulting from the segregation and cleavage of the mixed materials of the egg, Mr. Balfour observed and has figured the remarkable spindle-shaped condition of the nuclei, which since has become such a prominent subject of investigation through the initiative of Auerbach, Strasburger, Bütschli, and van Beneden. Very remarkable and important nuclei are also described and figured as making their appearance in that part of the egg *not* concerned in the process of cleavage or the formation of the primitive disc of embryonic cells, and from their occurrence Mr. Balfour is led to the conclusion that the supposed distinction at this period of a purely embryonic and a purely nutritive region in the egg of the Elasmobranch, is imaginary. This is important, because similar observations have necessitated the abandonment of similar erroneous divisions of the egg of the fowl, of osseous fish, and of cephalopods. The mass of cells which form the small commencement of the embryo on the surface of the great unsegmented yelk-mass divide into an ectoderm and "lower layer-cells," and a true "segmentation cavity" comparable to that of the frog is described. The most important of Mr. Balfour's observations and suggestions which have a general bearing upon the formation of the embryonic cell-layers throughout the Animal Kingdom are those in which he points out and gives its probable significance to the fact that in the Elasmobranchs the primitive alimentary cavity (archenteron) arises as a sort of in-pushing beneath the hinder end of the embryo, a cavity being there formed between the "lower-layer-cells" and the nucleated yolk. The orifice of this cavity is spoken of by Mr. Balfour as the "anus of Rusconi," and is identified by him accordingly with the orifice so named in Amphibians. At the same time it is *not* at this orifice that the final closure of overgrowing ectoderm or epiblast takes place, that is to say, of that layer of cells which, increasing by division, spreads from the cleavage disc so as to gradually cover in the whole of the large surface of uncleft yolk. The gradually narrowing margin of these epibolic cells does not in sharks have a centre coincident with the anus of Rusconi; in fact, the blastopore, as the orifice bounded by this gradually narrowing margin is termed, lies behind the embryo altogether. Mr. Balfour suggests (and it is necessary to remember that his statements on this subject were first published three years ago) that *the primitive-streak of the bird's blastoderm is a rudimentary representative of this portion of the blastopore;* it seems necessary to say "this portion," and not the whole blastopore, as Mr. Balfour does; for tracing these various structures back with Mr. Balfour to the blastopore of the Amphioxus, we must admit that in the meroblastic ova of Sharks and Birds the blastopore has become greatly extended along the median line and has its most *anterior* portion represented in the anus of Rusconi of the Elasmobranch, a *middle* portion in the orifice of final closure of the elasmobranch's blastoderm and the primitive streak of birds and mammals, whilst a more *posteriorly* placed extension of the same structure (blastopore) is seen in the actual orifice of final closure of the bird's blastoderm at the antembryonal pole of the yelk-sac. The continuity of the nervous and alimentary tubes, after closure of the Rusconian anus, is a striking feature which Mr. Balfour shows to be common to Elasmobranchs, Ganoids, Osseous fishes, Amphibians, Amphioxus, and Ascidians. To Kowalewsky we are indebted for the first observation of this remarkable disposition in various types of lower Vertebrata, and its full significance is not yet understood.

The next point of great importance which we find in Mr. Balfour's monograph is the derivation of the notochord from the hypoblast or archenteron, from which also the protovertebræ are developed constituting the mesoblast. That the vertebrates' body-cavity, like that of other animals, was primitively a portion of the alimentary cavity appears likely from this observation, coupled with Kowalewsky's more recent results as to the development of Amphioxus, whilst it also seems likely that the notochord made its first appearance as an organ appertaining to the alimentary tract, from which it became gradually separated in function and in structure.

The next thing which we come to is of even more special interest for the limited department of Vertebrate morphology. The *unpaired* and the *paired fins* alike make their first appearance in the Elasmobranchs as lateral ridges of epiblast, and Mr. Balfour accepts the hypothesis that the limbs are remnants of continuous lateral fins. The muscles of the limbs are shown to be derived from the "muscle-plates" of the body which develop from protovertebræ.

It is to the nervous system that some of Mr. Balfour's most original and important observations have reference. He has elsewhere conclusively shown that, contrary to Stieda's statements and in accordance with Owsjannikow's, the spinal nerves of Amphioxus have no anterior roots, that is have only dorsal roots. He now shows that the early condition of the spinal nerves of Sharks agrees with this, they having at first no anterior roots. An enigmatical commissure parallel to the medulla unites the posterior roots in the embryo. The cranial nerves—exclusive of the first and second, and the nerves to the orbital muscles which have peculiar features of their own—are shown to retain permanently the primitive condition implied in the absence of anterior roots. The vagus nerve is shown to be the result of the morphological fusion or concrescence of several segmental nerves—their separate roots (which are all dorsal ones) being "caught" (so to speak) in the sharks in process of disappearance. The identity of the nature of these roots with those of the following spinal nerves is shown by the connection with them of the enigmatical commissure above mentioned.

The segments which are represented in the Vertebrate head have been reduced and blurred by the integration of that region of the axis, but by the aid of the embryonic relations of the cranial nerves, and of a very important and remarkable series of cavities representing the body-cavity of the head (the terms are not contradictory since "head" is chiefly developed from "body") in a segmented condition, which Mr. Balfour has discovered in the Elasmobranchs, he is able to indicate distinctly at least seven post-oral segments in the cephalic axis, and he adduces cogent reasons for supposing that a larger number existed, and have been suppressed by a kind of integration.

As to the brain, Mr. Balfour gives important evidence against the fanciful interpretations of Miklucho-Maclay, whom, strangely enough, Gegenbaur has followed. What most persons call mid-brain, Miklucho-Maclay has identified with the thalamencephalon or twixt-brain (*Zwischenhirn*) of other Vertebrates, being induced by the large size of what is usually called the Elasmobranch cerebellum to consider it as the mid-brain. Mr. Balfour gives strong embryological evidence against this view.

As to the relation of nerves to the primitive germ layers, it is shown (in accordance with Hensen's observations in Mammalia) that the spinal nerves are *outgrowths* of the medulla, and Mr. Balfour, though he is unable actually to demonstrate it, yet brings a variety of evidence to show that the whole growth of the nerves is a centrifugal one, and that therefore the peripheral elements of the nervous system *may* have the same primary origin as have the central.

The important question as to how the axial medulla arose, and whether it is homogeneous with the ventral nerve-cord of Annelids and Insects is discussed in the light of the facts ascertained as to the development of the nerve-medulla in dog-fish. Mr. Balfour on the whole favours the view that the nervous system of elongated animals consisted primitively of two lateral cords, and that in Annelids and Insects these cords have met and fused *below* the alimentary tract, whilst in Vertebrates they have met and fused *above* the alimentary tract.

A curious modification of a part of the nervous system, the meaning of which is as yet entirely beyond the most hazardous speculation of either physiologist or morphologist, is shown by Mr. Balfour to present itself in the supra-renal bodies. They develop from ganglia of the sympathetic portion of the nervous system.

Lastly, we have to mention the series of results relating to the origin of the renal organs and the ducts of the generative system. These are already the most widely known and discussed, though possibly not actually the most important of Mr. Balfour's numerous discoveries. The fact that Prof. Semper, of Würzburg, occupied himself with the investigation of the renal organs of Elasmobranchs at the same time as did Mr. Balfour, and that the two investigators nearly simultaneously arrived at the same results, has given a special value to this part of the observations embodied in the present monograph. Mr. Balfour shows that the Vertebrate kidney is a condensation of tubules, of which primitively one pair existed in each segment of the body, opening into the body-cavity each by a ciliated funnel, and therefore exactly comparable to the segmental-organs or nephridia of the Annelids. Whether, as Gegenbaur holds, these organs were originally a simple pair which became segmented, that is, provided with a separate funnel in each metamere or body-segment, or whether each tubule or nephridium originally opened to the exterior, so that an unconnected series of nephridia existed on each side of the body—a pair in each segment—which subsequently became joined to one another by longitudinal common ducts—one on each side of the body—is still matter for consideration.

The adaptation of the most anterior funnel to the purposes of an oviduct, and of a portion of the middle tubules to those of sperm-ducts is what the observations of Balfour and Semper have established—and more especially the open funnel-like character of the tubules to begin with.

Minor details and important confirmations of the more familiar facts of Vertebrate development I have not space to mention here, the whole series of embryonic phenomena is described with more or less detail by Mr. Balfour, and I have singled out the more striking facts and speculations of the monograph for brief notice.

In commenting on such a work as this, it is strongly brought to one's perception that the method of publication of the results of such laborious investigations is necessarily very imperfect—and subject to a serious deficiency in logical continuity and artistic effect. Mr. Balfour has studied the very widely-diverse phenomena of interest which the developing Elasmobranch presents from the first separation of the egg up to the nearly complete formation of all its organs. In order to state *all* the different results he has obtained, he is obliged, as is usual in embryological monographs, to give them in historical sequence. To the experienced student of embryology this method of statement and the presentation of drawings copied from actual sections and specimens is sufficient. It would be impossible to publish observations within a reasonable period of the date of investigation by pursuing any other method of statement. And yet the monographical and historical method, together with the objective "nature-true" drawings of sections is one which prevents an author from fully exhibiting the import of his observations, and from duly imparting to the reader in a clear and simple form what is, after all, the thing which the reader desires to know, namely, what is the net result of such observations in relation to the great questions of morphology. The fact is, there is no such thing as a science of embryology; it is not even a definite branch of a science. The development of organic form is a necessary part of the science of Organic Morphology, and the results of the study of development can be given with full clearness and in an intelligible manner only when formulated as parts of the general doctrine of the science under which they fall. The conclusion from this is, that the great value of Mr. Balfour's work will not be fully appreciated or rendered clear to the majority of zoological students until they are re-stated, not from the monographical standpoint; but from the more general point of view of Animal Morphology. This more systematic exposition of his Elasmobranch studies and of other like researches in combination with a general survey of the morphology of all groups of the Animal Kingdom as revealed by their developmental histories, we may expect before long to receive from Mr. Balfour himself in the form of a continuation of his well-known Elements of Embryology.

E. RAY LANKESTER

OUR BOOK SHELF

Gold. By Edwin W. Streeter, F.R.G.S. Fifth Thousand (London: Chapman and Hall.)

THE lettering on the cover of the book, as given above will hardly prepare the reader for the statement on the title-page, that the work is a translation and abridgment of Herr von Studnitz' "Die gesetzliche Regelung des Feingehaltes von Gold und Silber-Waaren," by Mrs. Brewer, with notes and additions by Mr. Streeter.

The work itself contains information which it is useful to possess. It embodies brief abstracts of the law of various countries concerning the standard of gold and silver wares, and discusses the question whether the manufacture of articles in the precious metals should be subject to legal control. Mr. Streeter's notes occupy 10 out of the 150 pages. He states that the system of "Hall-marking" was "instituted on the supposition that the assay and test of precious metals was a matter too recondite to render a power of adequate discrimination for so valuable a transfer of property a thing reasonably to be expected of the public generally."

This is a very obscure way of saying that, as the value of gold and silver wares could not be recognised by inspection, it was advisable that all articles should be stamped by authority. The necessity for such control has long been felt, and it was well justified in 1677 by the author of the "New Touchstone for Gold and Silver Wares," who says: "The truth is, the gain by adulterating gold and silver works is so sweet and enticing that what excuse will not these adulterators find that they may have their unlawful liberty."

In London the control has been wisely vested in the Goldsmiths' Company since the fourteenth century, and in the country there are several assay offices which were reported on by a Select Committee of the House of Commons in 1856. Mr. Streeter urges that gold of one standard only—18 carat—should be used, or that if other alloys are employed the tradesman should "mark them with his own name, state the value of the composite matter, and trust to his genius for the sale." Trusting to genius for the sale of articles is all very well, but the practice of a person stamping the wares he sells with his own mark surely affords no protection against the fraudulent tradesman as the marks are not likely to outlive the age in which they are impressed, and would be as readily counterfeited as those of a responsible authority. It should also be added that the initial or distinctive mark of the maker of an article of gold or silver is already included in the Hall mark.

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.*]

Alternate and Stereoscopic Vision

WITH reference to Mr. Galton's observation in his instructive paper in NATURE, vol. xviii. p. 98, that "sometimes the image seen by the left eye prevails over that seen by the right, and *vice versâ*," I may mention that, as I had noticed some years ago, this may be best observed without a stereoscope. If on looking at any object a few feet distant, a nearer object be placed about midway between the first and the eyes, there will of course be two images seen of the near, when the eyes converge on the distant; one of these images seen, say by the right eye, overlaps the distant object as seen by the left eye, and if the two objects be about equally illuminated (or the near one rather the brighter), the overlapping image will alternately solidify and disappear, according to the alternate waxing and waning of sensibility in the eyes. This alternation may be made at will, by desiring to see the near or the distant object; the fluctuations take place about every ten seconds normally, but the changes may be willed (though not so completely) as often as every second.

If the observer can see stereoscopically without an instrument, *i.e.*, can dissociate the usually coincident motions of focussing and convergence, this alternate action of the eyes is seen very plainly; and not only may stereographs be combined by the eyes, more readily and with less fatigue than when using an instrument, but they may as readily be inverted (the near objects appearing distant, and *vice versâ*, as if falsely mounted) by applying each eye to the picture in front of the other, in fact, squinting at it. Thus, pictures of any size can be properly combined by reversing the pictures and crossing the eyes, and the width of the pictures is not limited to the distance between the eyes as in the ordinary way.

An important use of stereoscopic vision is to throw one eye out of use when doing delicate measurement, &c., by directing it to some other and darker object, instead of shutting it; this is less fatigue, and the attention may be willed on to the eye required, so that the image of the other is not noticed, especially if the eyes be changed occasionally.

How far the fact of the eyes changing guard naturally by alternations, may suggest that all duplicated organs of the body have alternate periods of rest, I must leave physiologists to investigate. W. M. FLINDERS PETRIE

Bromley, Kent

Inside Out

IT appears in NATURE, vol. xviii. p. 105, that "if a fourth dimension were added to space, a closed material surface (or shell) could be turned inside out by simple flexure." This implies that flexure is necessary. But without displacing a point or a line in the surface we may consistently suppose a rotation of the normals at each point of it through two right angles in a plane polar to the tangent plane. That seems to do the business. C. J. MONRO

May 28

Physical Science for Artists

MR. NORMAN LOCKYER, in NATURE, vol. xviii., pp. 59, 60, gives some valuable hints to artists, which, if carried out, will go a great way towards preventing our eyes being hurt by the lunar monstrosities we see at the Royal Academy and elsewhere.

May I be permitted to add a hint which he appears to have overlooked, and that is, that the inside boundary of a crescent moon is an ellipse; and in this consists the peculiar beauty of a true crescent. The usual Turkish crescent is struck with two circles, and always looks gouty and bad. Of course the rough edge of a gibbous moon is also an ellipse.

Scientific Club, 7, Savile Row, W., ROBERT J. LECKY

May 25

Dr. P. P. Carpenter's Collection

MAY I ask you to correct an error in the "Notes" of your number for April 25th, relating to the collection of the late Dr. P. P. Carpenter. This collection was permanently placed by him in the museum of this university; and, mounted under his direction on glass tablets, it now occupies a separate fire-proof room erected for it by the university, and constitutes one of the principal scientific treasures of this university and of Canada. Your correspondent was probably misled by the fact that one of the best duplicate sets was reserved by Dr. Carpenter for his own use in his private residence. This has not been publicly offered for sale, but I believe has been privately offered to certain persons and institutions considered likely to value it.

McGill College, Montreal, May 10 J. W. DAWSON

Menziesia Cærulea

IN confirmation of the recent occurrence of the above plant on the Sow of Athol, I may say that it was gathered by Miss Crawford in 1877, from whom I received a specimen. Like the cotoneaster on the Orme, which has also been reported extinct, careful and prolonged search has generally been rewarded by finding specimens, although the cotoneaster is now very rare. I might take this opportunity of saying that the rare spider orchis, *Ophrys aranifera*, which the Rev. M. J. Berkeley has gathered at Southorpe, in Northants, has been destroyed there by the planting of larch. I made a most careful search not only at Southorpe but on the Barnack hills last week, but without seeing a trace of the *orchis*, although *Anemone pulsatilla* and *Aceras anthropophora* are still abundant on the unplanted quarries.

Northampton Natural History Society G. C. DRUCE

Landrails

IT would prove very interesting to know whether landrails are as plentiful in other parts of the country this season as they are in the neighbourhood of Sheffield. They have not visited us in any numbers since the spring and summer of 1875; in 1876 and 1877 scarcely one was heard; while at the present time we hear their well-known calls in almost every meadow. I know of no migratory British bird in whose case this peculiar irregularity of appearance occurs in such high degree as in the landrail.

If the advice of one interested in the subject may be humbly offered, I would recommend ornithologists to pay strict attention to this matter, this season, with a view of elucidating this peculiar trait in the life-history of this singular bird; for the cause of this irregular appearance has, for ought I know to the contrary, yet to be learned. CHARLES DIXON

Heeley, near Sheffield, May 20

Hereditary Transmission

THE letter of Mr. Watt reminds me of a similar instance of "Hereditary Transmission" mentioned in the ninth edition of the "Encyclopædia Britannica."

It is there stated that "George Bernhard Bilfinger was born on January 23, 1693, at Cannstadt, in Würtemberg. His father was a Lutheran minister. By a singularity of constitution, hereditary in his family, Bilfinger came into the world with twelve fingers and as many toes."

After being a Professor of Logic at St. Petersburg University Bilfinger became one of the "best and most enlightened ministers" of state that Würtemberg had yet produced.

Burngreave Road, Sheffield, GEORGE S. WATSON

May 25

THE PHONOGRAPH AND ITS FUTURE

WHAT a surprise is in store for the children next Christmas if Mr. Edison's expectations are realised. Dolls that can say "papa" and "mamma," will be quite at a discount and will bear much the same relation to the doll of the future that the anthropoid ape does to the man of to-day, and the time will probably have come for some Darwinian toy-maker to write the history of doll development, if, indeed, he does not extend his researches to the whole world of toys. We are promised dolls that can speak, sing, cry, laugh; musical-boxes that will grind out the voice and words of the human singer; locomotives and every other species of "animal and mechanical toy," that will give out their natural and characteristic sounds.

But these are only some of the trifles to which Mr. Edison, in an interesting article in the current *North American Review*, tells us his miraculous invention will certainly or probably be put in the near future. And, indeed, a very little consideration will show that there is no end to the uses to which the principle of the phonograph may be applied; that it may be the means of actually realising some of the wildest dreams and speculations of the "frenzied" poet and preacher, and creating a revolution in human intercourse only to be paralleled by the invention of printing, or even of speech itself. Indeed, at first sight it may seem a step backwards, as it is likely to lead to the abolition, to some extent, of writing and printing, and the substitution of the human voice as the main means of intercourse at a distance. Talk of the solidification of the gases! Why we have here the solidification of something infinitely more impalpable—human words and human thought. We referred above to the musical-box of the future, and this suggests the phonographic barrel-organ, which will doubtless by and by take the place of that instrument of torture which makes the lives of delicate-eared artists and *littérateurs* miserable. Instead of having our musical sensibilities harrowed by a murdered reproduction of our favourite operatic air, or our political sympathies shocked by some wretched effusion of the Jingoid type, we shall have those picturesque Italian girls, with their bandit-looking companions, turning out for us a ballad by Sims Reeves or Santley, or a witching air in the voice of Patti. Alas! the invention came just too late to preserve to us for ever the matchless voice of Titiens, for now we need not wish in vain for "the sound of a voice that is still."

Music inevitably suggests love, and the tender cooings of the "lover and his lass, with a heigh and a ho and a heigh no nino." No longer will the far-separated pair have to wait weary weeks or months for a clumsy letter, when phonograph offices are as plentiful as telegraph stations; and when Mr. Edison has managed to make those improvements on the instrument of which he is confident, it will be quite possible for the fond pair to have a daily meeting and exchange across the world all sorts of tender cooings—for sounds of every kind can be registered on and given out by the phonograph.

Mr. Edison tells us that for these and similar purposes he is now perfecting the instrument in mechanical details. "The main utility of the phonograph, however, being for the purpose of letter-writing and other forms of dictation, the design is made with a view to its utility for that purpose.

"The general principles of construction are a flat plate or disk, with spiral groove on the face, operated by clock-work underneath the plate; the grooves are cut very closely together, so as to give a great total length to each inch of surface—a close calculation gives as the capacity of each sheet of foil, upon which the record is had, in the neighbourhood of 40,000 words. The sheets being but ten inches square, the cost is so trifling that but 100 words might be put upon a single sheet economically.

"The practical application of this form of phonograph for communications is very simple. A sheet of foil is placed in the phonograph, the clock-work set in motion, and the matter dictated into the mouth-piece without other effort than when dictating to a stenographer. It is then removed, placed in a suitable form of envelope, and sent through the ordinary channels to the correspondent for whom designed. He, placing it upon his phonograph, starts his clock-work and *listens* to what his correspondent has to say. Inasmuch as it gives the tone of voice of his correspondent, it is *identified*. As it may be filed away as other letters, and at any subsequent time reproduced, it is a perfect *record*. As two sheets of foil have been indented with the same facility as a single sheet, the 'writer' may thus *keep a duplicate* of his communication.

"The phonograph letters may be dictated at home, or in the office of a friend, the *presence* of a stenographer *not being required*. The dictation may be as rapid as the thoughts can be formed, or the lips utter them. The recipient may listen to his letters being read at a rate of from 150 to 200 words per minute, and at the same time busy himself about other matters. Interjections, explanations, emphasis, exclamations, etc., may be thrown into such letters, *ad libitum*.

"The advantages of such an innovation upon the present slow, tedious, and costly methods are too numerous, and too readily suggest themselves, to warrant their enumeration, while there are no disadvantages which will not disappear coincident with the general introduction of the new method."

Then as to books there seems some chance that, ere long the printer's, if not the publisher's, occupation will be to a great extent gone, and the present unwieldy form of communication between an author and his readers be abolished. What would not one give to have the "Christmas Carol" bottled up for ever in Dickens's own voice to be turned out at pleasure? Books, as Mr. Edison truly says, would often be listened to where none are read, and the possibilities of the instrument in this direction may be learned from the fact that a book of 40,000 words might be recorded on a single metal plate ten inches square. We need not point out the uses to which the invention might be put for the preservation of the greatest efforts of our greatest orators, but when Mr. Edison speaks of our thus collecting and preserving "the last words of the dying member of the family" and of great men, we feel as if he were approaching both the ludicrous and the shocking.

Then the compositor will be able to set up his type by ear instead of eye, and we shall have phonographic clocks which "will tell you the hour of the day, call you to lunch, send your lover home at ten," &c.

"Lastly, and in quite another direction, the phonograph will *perfect the telephone*, and revolutionise present *systems of telegraphy*. That useful invention is now restricted in its field of operation by reason of the fact that it is a means of communication which leaves no record of its transactions, thus restricting its use to simple conversational chit-chat, and such unimportant details of business as are not considered of sufficient importance to record. Were this different, and our telephone conversation automatically recorded, we should find the reverse of the present status of the telephone. It would be expressly resorted to *as* a means of perfect record.

"'How can this application be made?' will probably be asked by those unfamiliar with either the telephone or phonograph.

"Both these inventions cause a plate or disc to vibrate, and thus produce sound-waves in harmony with those of the voice of the speaker. A very simple device may be made by which the one vibrating disc may be made to do duty for both the telephone and the phonograph, thus enabling the speaker to *simultaneously transmit and record his message*. What system of telegraphy can approach that? A similar combination at the distant end of the wire enables the correspondent, if he is present, *to hear it while it is being recorded*. Thus we have a mere passage of words for the action, but a complete and durable record of those words as the result of that action. Can economy of time or money go further than to annihilate time and space, and bottle up for posterity the mere utterance of man, without other effort on his part than to speak the words?

"The telegraph company of the future—and that no distant one—will be simply an organisation having a huge system of wires, central and sub-central stations, managed by skilled attendants, whose sole duty it will be to keep wires in proper repair, and give, by switch or shunt arrangement, prompt attention to subscriber No. 923 in New York, when he signals his desire to have private communication with subscriber No. 1001 in Boston, for three minutes. The minor and totally inconsequent details which seem to arise as obstacles in the eyes of the groove-travelling telegraph-man, wedded to existing methods, will wholly disappear before that remorseless Juggernaut—"the needs of man;" for, will not the necessities of man surmount trifles in order to reap the full benefit of an invention which practically brings him face to face with whom he will; and, better still, doing the work of a conscientious and infallible scribe?"

Mr. Edison is certainly very hopeful of the future of the wonderful instrument he has invented, but we think, not too hopeful; for, after the invention itself and its most recent development, the microphone, it would be rash to say that any application of it is impossible. Certainly some substitute or substitutes for the clumsy mode of recording our thoughts by pen and ink, so inconsistent with the general rapidity of our time, must be close at hand; and what form one of these substitutes may take seems pretty clearly pointed out by the actual uses to which Mr. Edison's invention has been put.

THE ENGLISH ARCTIC EXPEDITION[1]

THE edge has been to some extent taken off the public appetite for a narrative of our last great Arctic expedition. The two ships had barely touched the Irish shores ere the papers of the day were teeming with details of the adventures and results of the expedition that had left England scarcely eighteen months before amid the enthusiasm of the nation, and with the strongest expectations of eclipsing all previous expeditions, and returning with the long-sought-for secret of the pole. These newspaper narratives were shortly followed by Capt. Nares's report (which we gave in full with a map in NATURE, vol. xv. p. 24), followed some months after by a thick Arctic blue-book, which those who have seen it may prefer, with its wealth of maps and illustrations, even to the two handsome volumes before us. (See NATURE, vol. xv. p. 505.) Under these circumstances it will not be necessary for us to repeat the story of the *Alert* and *Discovery*. We shall endeavour briefly to sum up the main results obtained by the well equipped and much instructed expedition.

Many a wonderful story lies buried in a blue-book; comparatively few, we believe, have seen the official narratives to which we refer above. The great majority of those, both at home and abroad, who are interested in the expedition commanded by Sir George Nares, have no doubt been waiting for the publication of these volumes, to learn all the details of the story of the hardships endured by our ever-brave sailors "far from all men's knowing," in the most inhospitable region under the heavens. The red-tapeism and stupid conservatism of our government are in nothing more forcibly exhibited than in their obstinate adherence to the unattractive "blue-book" for publications of all kinds that may be considered in any way official. In this respect they present a marked contrast to the United States Government, the story of whose *Polaris* expedition was issued not long ago in a magnificently got-up volume that would do credit even to Messrs. Sampson Low and Co.; and many of our readers must be familiar with the splendid library, issued at the expense of the Austrian Government, on the productive *Novara* expedition.

We are sure Sir George Nares does not expect to be complimented on his skill as a *raconteur*; he has wisely not attempted to do more than give a plain statement of the proceedings of the expedition day by day from the time it left England till its return. Those of our readers who have read the eloquent and methodical narrative of the Payer-Weyprecht expedition, when they look into the one before us, will not fail to be struck with the contrast in this respect. Still, we believe, by many, Sir George Nares's "plain, unvarnished tale" will be preferred to a carefully redacted and condensed narrative; and we are sure that in his pages the simply told successes and failures of the English Arctic Expedition of 1875-76 will fascinate many a reader: it is almost impossible to make the story of an Arctic Expedition uninteresting.

"The scope and primary object" of the expedition was, as contained in the "Sailing Orders," "to attain the highest northern latitude, and, if possible, to reach the North Pole, and from winter quarters to explore the adjacent coasts within the reach of travelling parties." Notwithstanding the ambiguous wording of these orders—no doubt "the highest northern latitude *possible*" was meant—it is a great mistake to imagine, as many did on the return of the expedition, that it was a failure because it did not reach the pole. No doubt it was a primary part of the programme to make the most determined attempt to reach 90° N. lat., and had "the People" not been allowed to believe that this was the main object of the expedition, probably their enthusiasm at its departure would have been no greater than when the *Challenger* left our shores; but, without doubt, the essential point in this matter was to get as far north as possible. No one who reads Sir George Nares's interesting but often sad pages will hesitate to conclude that if a higher latitude than that attained by the forlorn hope, led by Commander Markham, was not reached, neither officers nor men were to blame. Under hardships that could only be paralleled by those which led to the unknown deaths of the members of the Franklin expedition, was the attempt made to carry out the popular part of the programme—hardships, however, which did not surpass those endured by the sledging parties west and east under Aldrich and Beaumont. This is not the place to enter upon the question of the outbreak of scurvy, to which we have, indeed, referred in a former volume (xv. p. 505). After the searching inquiry of the Scurvy Commission; after all that has been written on the subject in the public and medical journals; and after a careful perusal of these pages, we are not inclined greatly to blame either Captain Nares or his officers for their neglect of lime-juice. Evidently we have yet much to learn about the causes and means of prevention of scurvy. All we have to do with here is the fact that under the most adverse conditions imaginable officers and men did more than could reasonably have been demanded of them—though not expected of English sailors—to carry out the purely geographical part of their orders. Markham and his men really reached the highest latitude *possible* under the circumstances, 83° 20′ 26″ N., the highest latitude reached by any expedition. "C'est magnifique, mais ce n'est pas la guerre." It was heroic, but it is not what we want.

The other part of the geographical section of the programme was carried out with equal faithfulness by the sledge parties under Lieuts. Aldrich and Beaumont, and, in the case of the latter, under even greater hardships and with greater fatality than in the case of the northern party. Lieut. Aldrich succeeded in adding to our maps a stretch of 220 miles of coast along what may be regarded as the northern boundary of America, while Lieut. Beaumont considerably extended our knowledge of the north coast of Greenland, and has given us reason to believe that it is bordered by islands. Many rectifications were made, moreover, of the geography of the coasts and islands in Kennedy and Robeson channels, and a considerably fuller and more accurate idea of the nature of the coast regions both on the east and west sides of these channels. The fact is that, geographically, there appears to be little to discover in the region around the *Alert's* winter quarters, and what is really worth knowing in this direction could only be brought to light by an expedition colonised there for some years; Capt. Howgate's proposed experiment will therefore be anxiously watched. Though it was often difficult to tell where the sea-ice ended and the land began, enough was observed both by Aldrich and Beaumont to indicate that these northern shores are mostly rocky, rising rapidly into hills and mountains, and often, especially on the Greenland side, steep and imposing, and deeply cut into by fiords. Markham saw no sign of land as far beyond his farthest north point as he could see, and seems inclined to believe that if there is land it must be a great way off.

Even had the men maintained their health and strength, it is doubtful if any of the sledging parties would have been able to do much more than they did, unless, indeed, they had been able to stay another winter, and make their furthest points bases for farther operations. The great hindrance to progress was the character of the ice which the sledge-parties had to traverse. The nature of this characteristic feature of these regions, the "palæocrystic ice," as it has been named, is already

[1] "Narrative of a Voyage to the Polar Sea during 1875-76, in H.M. ships *Alert* and *Discovery*." By Capt. Sir G. S. Nares, R.N., K.C.B., F.R.S., Commander of the Expedition. With Notes on the Natural History, edited by H. W. Feilden, F.G.S., C.M.Z.S., Naturalist to the Expedition. Two vols. (London: Sampson Low and Co., 1878.)

well known to our readers, and some further idea of it may be obtained from the specimen shown in our illustration (Fig. 1). Things were bad enough for the shore-parties, but, to judge from the description, it would be as easy to go from the Crystal to the Alexandra Palace over the tops of the houses dragging a heavily-laden sledge after you, as to accomplish what Markham and his party did. The valuable result of this expedition is an extension of our knowledge of the nature and formation of the ice which covers these polar regions. That the inconceivably rugged and hilly nature of the ice is partly due to the movement of the pack, and the consequent piling of floe on floe at all sorts of angles, there can be no doubt; but the observations of Dr. Moss (see our Royal Society Report this week) and Lieut. Parr seem to show that the immense thickness of the floes, exceeding eighty feet sometimes, is not due entirely to the piling of floe on floe. Rather it would seem that the same causes are in operation here as in the Alpine glaciers, and that on a thick substratum of sea-ice, snow-fall after snow-fall has been accumulating season after season,—for how long the daring geologist alone can hazard a guess—becoming gradually condensed into ice by pressure. What may be the limit of this process we have no data on which to build conclusions. "The Névé-like stratification, the embedded atmospheric dust, and the chemical characters of our polar floes, indicate, in my (Dr. Moss's) opinion, that they are the accumulated snow-fall of ages, rendered brackish by infiltration and efflorescence." The great "domed" floes, he thinks, tell of gradual decay, "because, wherever we got a section of them, the horizontal strata were cut by the outline of the domes, and the ice of the top of the dome was invariably salt. Occasionally deposits of atmospheric dust were to be met with throughout the stratified ice."

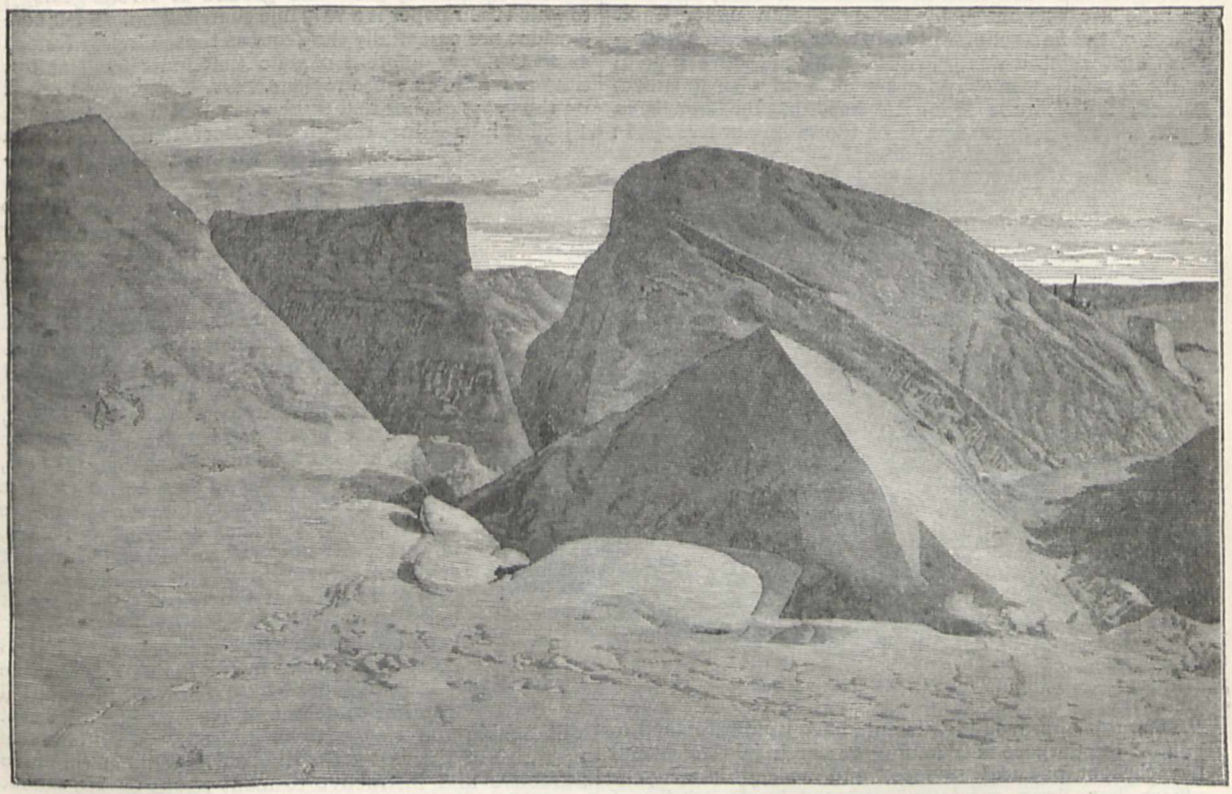


Fig. 1.—Newly-formed Floe-bergs.

As to the movements of the ice in the Polar Ocean, the expedition was unable to make any observations of consequence, though it has made some contributions to a knowledge of those of the ice in the channels through which the ships passed. The general conclusion seems to be that beyond the *Alert's* winter quarters, though there may be occasional open spaces, or polynias, the ice never breaks up sufficiently to enable a ship to pass further northwards, notwithstanding the observations made by the *Polaris* expedition. But this question of the movements of the polar ice is just one of those that can only be satisfactorily settled by a long series of observations, such as those that could be made from the ring of stations proposed to be established by Lieut. Weyprecht.

The tidal observations made, especially on board the *Discovery*, were of great value, in the opinion of Dr. Haughton, who gives an abstract of the results in the Appendix, and seem to confirm the observations made in the *Polaris* expedition:—"The expedition, proceeding up Smith Sound, met the tide coming from the north, at or near Cape Frazer, lat. 79° 40′, and left behind the tides of Baffin's Bay. The new tidal wave, observed on board both ships, is *specifically* distinct from the Baffin's Bay tide, and from the tide that enters the Arctic Ocean through Behring's Straits; and it is, without question, a tide that has passed from the Atlantic Ocean, round Greenland, northwards, and then westwards."

As might be expected in these high northern regions, there were few auroral displays, and though one, at least, was remarkable, none were brilliant, and all comparatively colourless. We do not read, however, that any attempt was made to study this or any other light phenomenon by means of the spectroscope, though, we believe, several of the officers were specially instructed in the use of the instrument before the expedition set out. In this connection we may notice a most interesting solar phenomenon exhibited on p. 300 of vol. i. (see Fig. 2), which will give the reader some idea as to how snow-blindness may be produced, and which might have reminded Capt. Nares that the expedition was provided with the instrument we speak of. When the sun reaches a certain height, above 14°, during clear weather, "the most brilliant prismatic colours are displayed by each minute snow-prism, and in combination form a sparkling arc on the snow-covered ground, the bright light from which is too powerful for the unprotected eye. The 'diamond-dust,' as we term it, becomes more open as the length of the radius is increased. Consequently, when the sun is between fourteen and twenty-three degrees in altitude, the refraction of its rays is set forth with the greatest effect, and snow-blindness has to be guarded against. In the bright arc, while each tiny prism displays its complete set of colours, the red tint is the most prominent nearest the sun, the purple lying on the outside indistinctly defined." We regret that such observations were so rare, and that so little use was made by the expedition under Capt. Nares of the fine set of apparatus for physical observations with which it was provided. This is the weak point of the expedition, and, so far as physical science is concerned, the "Arctic Manual" need hardly have been written. The 26th paragraph of the sailing orders runs :

FIG. 2.—Diamond Dust.

"The most approved instruments have been furnished to you for the purpose of pursuing research in the several branches of physical science, and as certain of your officers have been specially instructed in the modes of observing, you will take care to give them every fair opportunity of adding their contributions thereto." Very few "fair opportunities" seem to have occurred to the expedition.

But after all it is doubtful if the commander of the expedition is so much to blame. The truth is that the instructions to the expedition said more than should have been said about trying to reach the pole. What we wanted and what we still want are steady continued observations of meteorological and other phenomena in the polar area. The Royal Society might have saved itself all trouble if the instructions had been published beforehand. The comparative meagreness of the scientific results is, we believe, due more to the tone of the instructions than to Sir George Nares.

Thanks mainly to Capt. Feilden, however, the expedition has not been altogether barren in scientific results, as the Appendix filling half the second volume will testify. With the exception of the short paper on the tides by Dr. Haughton, this appendix deals with the natural history and geology of the region visited. Each of the departments of natural history, from the mammalia downwards, has been worked out by a specialist, and the results, though seldom novel, are all highly interesting. Life was found in the sea at the highest point reached, and not far from the same point the tracks of a hare were seen. Dr. Hooker has some important observations to make in connection with the flora brought home, which confirms his previous conclusions as to the essentially Greenlandish nature of the Greenland flora. He is inclined to think that vegetation may be more abundant in the interior of Greenland than is supposed, and that the glacier-bound coast-ranges of that country may protect a comparatively fertile interior. We are almost driven to conclude, he thinks, that Grinnell Land, as well as Greenland, are, instead of ice-capped, merely ice-girt lands. The geological results are fully and ably discussed by Mr. De Rance and Capt. Feilden, who indeed traverse summarily the whole ground of Arctic geology, to which their paper is a valuable contribution. Their conclusions are essentially the same as those already formed as to the very different climate that must have at one time prevailed in these regions. Dr. Coppinger's report on the great glacier that discharges into the Petermann Fjord is interesting, though his observations do not seem to agree entirely with Dr. Hooker's conclusions.

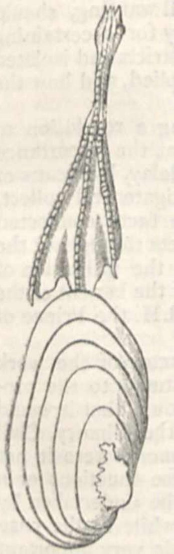
On the whole Sir George Nares's two volumes confirm the opinions we have already published with regard to this expedition. One and all exerted themselves nobly and bravely to carry out the main object of the expedition; the results, geographical and scientific, brought home are of great value, and repay to a considerable extent the outlay and the hardships endured; at the same time, now that the full narrative has been published, we must express regret that the scientific results are not more abundant than they are, and that they contrast so markedly with those of previous English expeditions, and with the expeditions of Germany and Austria, where, however, the officers are all trained men of science. Notwithstanding the results we cannot regret that the expedition was sent out; it has solved the question of Arctic exploration so far that it is clear the Pole is not to be reached by the Smith Sound route—if at all, indeed by any means hitherto tried—unless some line of land be met with that will enable the sledge to be utilised. Meantime this narrative of the last great English Expedition will prove attractive and instructive to many readers. We cannot conclude without saying a word in praise of the many fine illustrations of Arctic scenery, a number of the finest being permanent Woodburytypes. There is also a large map showing the new discoveries, and a special one of Markham's journeys.

TRANSPLANTATION OF SHELLS

IT is well known that animals and plants inhabiting freshwater have, as a general rule, a very wide distribution; yet each river system, with all the pools and lakes in connection with it, seems completely cut off from every other system of the same country. Still more complete is the separation between the freshwaters of distinct continents or of islands; nevertheless they often possess freshwater species in common. In my "Origin of Species" I have suggested various means of transportal; but as few facts on this head are positively known, the case given in the adjoined letter of a living Unio, which had caught one of the toes of a duck's foot between its valves, and was secured in the act of being transported, seems to me well worth recording.

CHARLES DARWIN

DEAR SIR,—The following case will, I think, prove of interest to you, as it corroborates your belief that freshwater shells are sometimes transplanted by the agency of aquatic birds.



In the sketch I have endeavoured to give you a correct idea of the way in which the shell was attached to the duck's foot.

It was given to me by Mr. H. L. Newcomb, who shot the bird, which was a blue-winged teal (*Querquedula discors*), while flying, near the Artichoke river at West Newbury, Mass., September 6, 1877. The shell, the common mussel, or clam (*Unio complanatus*), is a very abundant species, being found in nearly all the rivers and ponds of the Atlantic slope. How long the shell had been attached is only a matter of conjecture, but it had abraded the skin of the bird's toe, and left quite an impression. It was living when the bird was shot.

It would have undoubtedly been transplanted to some pond or river, perhaps miles from its original home, had the bird not been shot, and might then have propagated its kind.

ARTHUR H. GRAY

Dauversport, Mass., May 8

To C. Darwin, Esq.

THE NATIONAL WATER SUPPLY

THE Congress convened by the Society of Arts, at the suggestion of His Royal Highness the Prince of Wales, their President—and which has been presided over by Sir Henry Cole, K.C.B., and numerously attended by Mayors of Provincial Towns, Chairmen of Local Sanitary Authorities, Medical Officers of Health, Members of the Thames Conservancy Board, Engineers, and men of science—may fairly be considered a sufficiently representative body to discuss with some amount of authority a national question.

The papers prepared at the request of the Council of the Society, and discussed by the Congress, may conveniently be divided into three groups:—the quantity of rainfall available for water-supply; the necessity of improved legislation to give it quickly and cheaply to the people; the necessity of compulsory powers being given to a government department, to carry out the amended law.

The first head, quantity available, was appropriately opened by a paper on the rainfall, by Mr. G. J. Symons, the indefatigable head of the 2,000 unpaid observers, whose results leave little to add to our knowledge upon this matter. "No part of the British Isles has, on the average, less than 20 inches of rain per annum," and "the bulk of the supply falls upon elevated mountain tracts, where it ranges from 50 to more than 100 inches per annum."

Mr. Symons did not give the results of his experience on the probable amount of the rainfall evaporated on different soils, under different atmospheric conditions, and in different parts of the country; this figure must ever be an important factor in estimating the quantity of water available in a district. Mr. John Evans, F.R.S., however, informed the congress that while on bare hard rocks nearly the whole of the rainfall is carried off by the *surface streams*, on some porous rocks, such as chalk, an average quantity of not more than six or eight inches per annum finds its way to a depth of three feet from the surface, the remainder being carried off by *evaporation*, and vegetation, and he adds "that for the supply of the population in districts of different geological character, different means must be adopted."

Numerous speakers insisted at some length on the influence of the varying degree of permeability of the rocks, in determining whether the rainfall is thrown off in *floods*, which should be collected and stored in reservoirs, or whether it is *absorbed* into the ground, where it can only be reached by wells, or by carefully preserving lines of springs. Mr. Chadwick, C.B., pointed out that the Map of the Geological Survey of the United Kingdom, and the other publications of this Department, formed an admirable basis for any inquiry into the water-bearing facilities of the British rocks. Mr. Whitaker's Memoir on the London Basin, which contains the particulars of more than 500 wells sunk in and around the Metropolis, is a good illustration of this, and useful as showing the facilities already possessed by a Government Department, which is capable of greater extension in this direction, and whose officers now constantly work in concert with those of the Local Government Board.

Mr. Lucas exhibited a useful map, showing the underground contours of the surface of the water in the chalk, of some 800 square miles of the Thames and Hampshire basins, so that the level of the water in regard to the Ordnance datum line can be seen at a glance. He has also in some cases indicated the underground level of underlying impermeable strata, a method which has been long used in the Geological Maps of Paris; and it is a matter of surprise that a map of London which should answer the purpose of a section in all directions was not published before. However useful such a map may be amongst the permeable rocks of the greensand, chalk, oolites, and new red sandstones, which are penetrated by deep wells,—in the more ancient formations, consisting almost entirely of impermeable rocks, it would be impossible to construct such a map, and the ordinary Geological Map is all that is required.

These porous secondary strata occupy an area of 26,000 square miles in England and Wales, and in Scotland and Ireland are practically absent, and wells of any depth are rare; while the more shallow wells, penetrating the overlying drift, are in all districts, as pointed out by the Rivers Pollution Commission, dangerous sources of supply, though in some cases, as Prof. Prestwich, F.R.S., pointed out to the Congress, the gradual removal of cesspools, and improvement of house-drainage, has caused the shallower well-waters to again improve. He, however, gave a remarkable instance of a retrograde character, that of a deep "dry well," being carried through the London clay, to drain a cemetery near London, into the underlying Thanet Sands, which still give an important quota to the Metropolitan potable waters.

Next to the quantity of water available, there is no question so important as the quality and purity; and on this point Dr. Frankland gave important and reassuring evidence; for though he tells us that the increasing pollution of rivers and streams "renders the supply of wholesome water from them more and more difficult," yet "two sources of wholesome water" still remain in England, viz., "upland surface water and subterranean water." The tables accompanying a paper laid before the Congress by Mr. De Rance, show that the formations yielding water of these two characters occupy the following areas in England:—

FORMATIONS YIELDING:—

Subterranean Waters.	Sq. Ms.	*Moorland Waters.*	Sq. Ms.
Permian and Trias. . .	8,645	Granite, Metamorphic Rocks, Cambrian, Silurian, & Devonian	11,455
Oolites	6,671	Carboniferous Rocks, (without the Carb. Limestone). . . .	10,080
Hastings Sands, Green Sands, and Chalk . . .	11,371		
	26,687		21,535

Of the more permeable rocks constituting the first list, probably four-fifths of the area would yield unpolluted water, and receive into its mass not less than six inches of rainfall annually, or a quantity, if all yielded up to wells, of no less than 240,000 gallons per day for each square mile of area.

Of the second list the rocks are for the most part impermeable, and the most porous portion of the carboniferous generally return the water that has percolated into the strata to the same river basin; these rocks receive the heaviest rainfall of England, seldom falling below forty inches, and often rising to more than a hundred, of which quantity not less than thirty inches per annum may safely be calculated on, as the quantity run off by streams. Assuming that the rainfall is only available for water supply purposes, over one-tenth of the area, or 2,153 square miles, and that one-third of the supply is given back to the streams as compensation to manufacturers, to preserve fish, and for the purposes of inland navigation, the quantity remaining off this selected tract would be more than sufficient for the whole population of England, without recourse to the subterranean supply, which Dr. Frankland more especially recommends for domestic use; so that there can be no shadow of doubt that the quantity of water available for supply to towns and rural populations, of a standard of purity approved by Dr. Frankland, is far in excess of the requirements of our population.

The question of the amount of compensation water which should be returned to streams which are impounded for the purpose of water-supply is one of the gravest national importance. In one case, the River Roddlesworth, taken by the Liverpool Corporation Waterworks, the Legislature permitted "the compensation water," ordered to be returned to the stream by the Act of 1847, to be bought up, for the purpose of supplying a new reservoir, and thus deprived the district drained by the stream, in the words of Mr. Bateman, speaking of a similar proposal,[1] "of all possible participation in the extension of manufactures and in the commercial prosperity of the surrounding district." Mr. Bateman has strongly expressed similar views in his evidence before the Duke of Richmond's Commission, and it is with regret, we notice that though he proposes to take eventually 50 million gallons per day from Thirlmere, he only intends to return $5\frac{1}{2}$[2] million gallons a day to St. John's Beck.

On the second head, the necessity of legislation to give cheaper and more easily acquired water powers to sanitary authorities, through the agency of provisional orders of the Local Government Board—which do not now possess compulsory powers to acquire water-rights, under the Local Government Act of 1875. Mr. A. H. Brown, M.P., read a paper describing the work done by the Select Committee on the Public Health Amendment Bill, of which he was chairman, and which has now been read a third time in the House of Commons and passed. The Bill introduces many sweeping changes, and not only gives to the Local Government Board increased powers, but empowers them "to confer the powers of this Bill or any of them to urban authorities," and further ordains that the Board shall have power to permit Local Boards to purchase water compulsorily, under provisional orders, confirmed by Parliament, "such Provisional Orders to put in force the Water Clauses Act, 1847."

Should Mr. Brown's Bill pass the Upper House but little additional legislation would appear to be necessary, for the labours of the various Royal Commissions, Parliamentary Committees, and the British Association Committee of Inquiry "into the Secondary rocks of England, as a source of water supply," have amassed, as we have seen, a large volume of information as to the rainfall, available yield, and quality of the water suitable for domestic purposes; and the new powers obtained by the Local Government Board will enable them more quickly and cheaply to give facilities for the construction of works for water supply than heretofore. All that is still wanting, should the Bill pass, is some further machinery for ascertaining by Government inspection what rural districts and isolated hamlets are not at present properly supplied, and how the difficulty is to be solved.

The Congress have met it by passing a resolution to "urge upon Her Majesty's Government the importance of taking steps, with the least possible delay, by means of a small scientific commission to investigate and collect, for the information of the public, the facts connected with water supply, in the various districts throughout the United Kingdom, in order to facilitate the utilisation of the national sources of water supply for the benefit of the country as a whole, as suggested by H.R.H. the Prince of Wales."

The high value and important character of the work done by several bodies similarly constituted to the proposed Water Commission is so well known, that it would be needless to mention as examples the Charity, Civil Service, and Ecclesiastical Commissioners, were it not to point out that these gentlemen exercise functions of a special character, which could not well be undertaken by any other Government department; while in the case of the proposed Water Commission, this very important *raison d'être* appears to be absent, for special knowledge and long experience appear to be already possessed by the staff of the Local Government Board, and by that of the Geological Survey, which under the interchange system of Government officers recommended by Dr. Lyon Playfair's Commission could easily be directed to assist them, especially as to a limited extent it already does so, and has besides aided the Rivers Pollution Commission in its labours.[1]

The Congress has certainly been useful in showing how impossible it is to separate *water supply* from *drainage*, and the absolute necessity of there being a central authority, with supreme power over water, whether at the surface or underground, whether *used* for the purpose of water supply, canalisation, supplying motive power, or *disposed of* in the form of drainage and sewerage. As Dr. G. W. Child well remarked, "the bane of all local government in England is the chaos of different and often conflicting authorities, existing each for a special purpose." How the formation of the proposed permanent Water Commission will facilitate matters by adding another to the long list of governing bodies it is difficult to see.

Facts are useful; but we first want simplification and unification of the law, and the carrying out its provisions entrusted to one central authority, directed by a Minister of Public Health, with power so to modify and increase his department as to be able to collect information at the same time that he administers the law, and remove from us the possibility of the reproach that we have carried on scientific investigations to complete our knowledge of water supply, without applying, for the use of our population and the prevention of disease, the information we already possess.

[1] "Borough of Liverpool New Water Supply Report," by Mr. John Frederick Bateman, C.E., F.R.S. Liverpool, 1875.

[2] The compensation of $13\frac{1}{2}$ million gallons, stated in several journals, and lately quoted by us, is incorrect, the amount being only $5\frac{1}{2}$.

[1] Annual Reports of the Science and Art Department.

PHYSICAL SCIENCE FOR ARTISTS[2]

IV.

I AM afraid there is no use in shirking the notion that my last paper may have seemed wofully dull, frightfully technical, and terribly wide of the mark to some artists who took it up, always supposing of course that

[2] Continued from p. 89.

any of them did. Although I must plead not guilty to the last count, still I am so persuaded that in the nature of things these opinions were likely to be held that I feel compelled, before I go further into the experimentation with our improvised spectroscope, to draw attention to the kind of knowledge I hope to show can be gained by its use.

It might at first sight appear that, limiting ourselves to the sun as a great radiator of light, the artist has only to do with white light in his pictures. This is true in one sense, but only in one sense; for the artist has to deal with sunlight after it has been filtered through—after it has been absorbed by—many substances, notably the aqueous vapour of our air, and after reflection from others. I shall show in the sequel, for instance, precisely how it is that the sun is red at sunset; at such a time as this the sun practically gives us coloured light because our atmosphere has abstracted from it some of the constituent rays which fell on the upper air. A familiar instance of this may be referred to. The colour of a ripe cornfield, in an autumn sunset, comes from the fact that, for the moment, in consequence of this absorptive effect, our sun has been a red star instead of a white one. Cause and effect are there before our eyes, and science connects them, and yet, alas, I have seen pictures in which the grand colour of the corn has been given in perfection, while the painter has been so ignorant of the cause of it that he has given us a cold, grey, cloudy sunset, instead of a red, cloudless one.

Further, as all the ordinary colours of natural objects depend upon the way in which they reflect or absorb white light, the colours of all must change at the hour of sunset or sunrise, if the light which they usually transmit to us is wanting in the light which they then receive. An object, for instance, which appears blue at noonday will appear black if only the red light of sunset falls upon it. The blue sky over head is really a rich source of light of all colours; it is not a true blue, it is a mixture of blue and white; so that sunrise and sunset effects are much more potent when a great bank of cloud overhead leaves only the eastern or western horizon open. A striking thing for an artist to observe under this condition is the difference of luminosity of a red brick house, and such objects as trees and fields; the house seems glowing with light as if it were red hot, and for the simple reason that it gets as much light of the red kind, which is what it wants, and which it reflects to us, from the setting sun, as it does from the noonday one; whereas the trees and grass are no longer supplied with that light which they throw back to us usually, and so appear black for the same reason that lampblack appears black at noonday.

It has been a favourite theme with many astronomers to enlarge upon the marvellous coloured phenomena which must take place in those planets which are near enough to stars of strongly contrasted colours to receive their light, now from one, and now from the other, producing not only a perfect modulation and combination of the colours of both, but also the strongest contrasts, such as, for instance, the setting of a red sun followed by the opposite rising of a blue one. No doubt such phenomena would be very enthralling, but to my mind the chromatic effects produced by the aqueous vapour in our own air absorbing the various elements in the light of our single white sun when it is not more than ten degrees above the horizon, supply us with a world of beauty with which we may well be content, and I for one have not found the beauty one whit less enthralling since I have endeavoured to picture to myself the causes to which it is due.

All this, however, by way of anticipation: the causes so far as I am acquainted with them at work in the circumstances I have named, and in ten thousand others, are easily to be got at by a little simple experimentation.

In my last paper I suggested a simple form of spectroscope. Here is a figure which will show how the prism should be placed and what it will do to the light coming through the slit C.

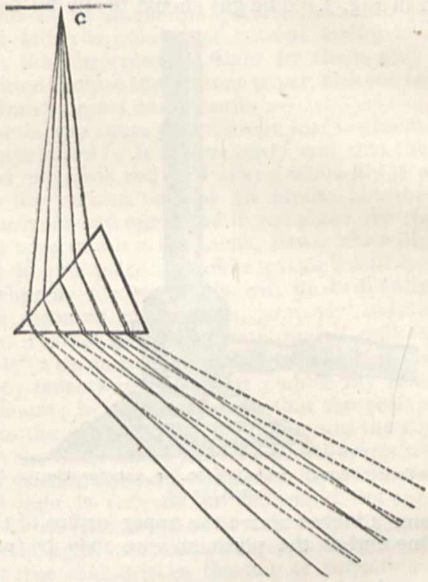


FIG. 1.—Showing arrangement of slit and prism.

We may place a lens behind the prism as shown in Fig. 2, and throw an image on a screen, which may conveniently be a piece of white paper.

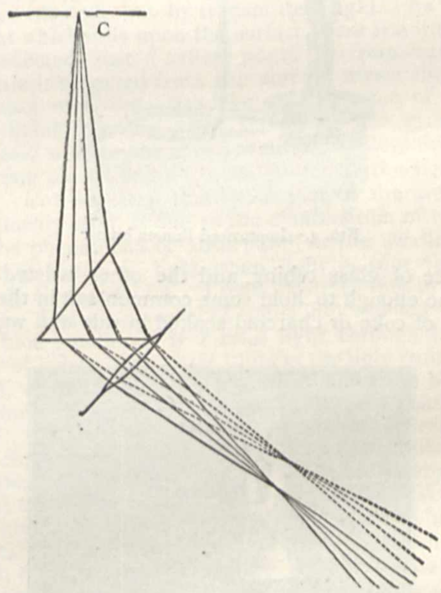


FIG. 2.—Introduction of a lens to produce an image.

If we are content to use the lens and screen which constitute the human eye it must be placed near the prism.

An expenditure of a few shillings is all that is required to enable the origin of one class of coloured phenomena to be investigated, that class, namely, in which the colour is due to the giving out, by the light source, of certain kinds of light only. This money should be expended in buying two little glass or brass tubes, $\frac{1}{10}$ inch and $\frac{1}{2}$ inch in diameter, and 5 inches long, a little glass tubing of very small bore, a few inches of platinum wire, a small quantity of red and green fire, and india-rubber tubing to convey gas from an ordinary burner to a table. Of the two tubes a Bunsen burner can be easily constructed

This is an apparatus for burning a mixture of ordinary gas and air; the gas should be supplied through the smaller tube inserted into the lower end of the larger one, as shown in Fig. 3. The gas should be lit by holding a

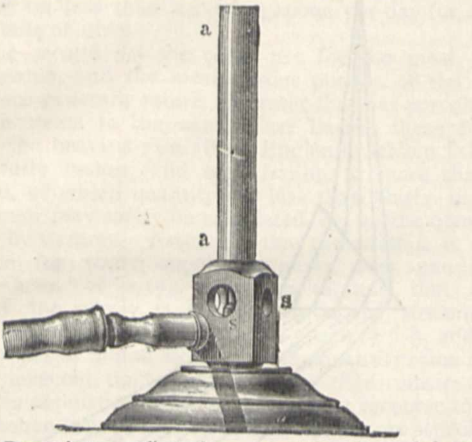


FIG. 3.—Bunsen burner, ordinary form. a, exterior tube; s, holes to admit the air.

match some 4 inches above the upper orifice of the wide tube. One end of the platinum wire may be fused into

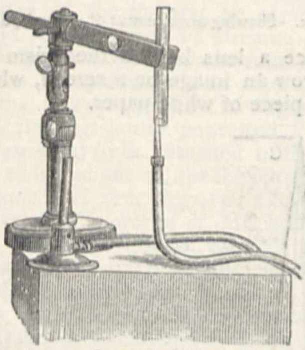


FIG. 4.—Improvised Bunsen burner

the piece of glass tubing, and the other twisted into a loop, fine enough to hold some common salt in the flame. A piece of coke or charcoal soaked in salt and water will

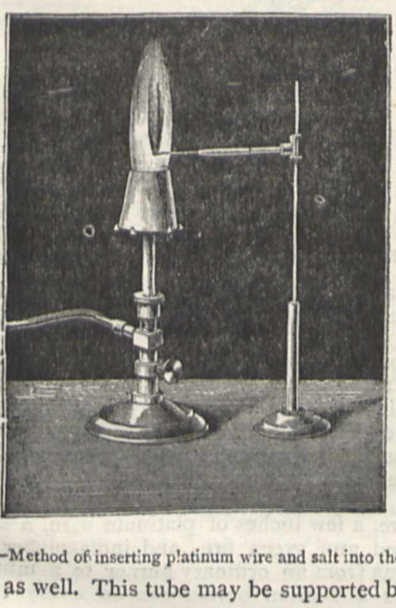


FIG. 5.—Method of inserting platinum wire and salt into the flame.

do almost as well. This tube may be supported by a piece of wood after the fashion of Fig. 5. The Bunsen burner will give us a very hot bluish flame, into which the loop of platinum containing common salt, *sodic chloride*, may be inserted, as shown in the accompanying woodcut. We shall get a brilliant yellow flame, which is worth notice on its own account, and if the artist is performing these experiments in his studio, let him look at some of his choicest pictures by means of this light, before he goes any further. He will be considerably surprised at their appearance, and I hope he will set himself to think about the cause of it—a point on which there will be a good deal to be said in the sequel. It will be better, however, to get at the physics in the first instance. To do this, put the improvised Bunsen burner and the platinum wire with the salt on it in front of the slit, and look at the slit through the prism; it will be found that there is only a yellow image of the slit visible. If the things have been nicely arranged, the appearance of the spectrum will be so entirely changed that a beginner will be apt to fancy that something has gone wrong. Nothing has gone wrong however; we have simply passed from the spectrum of polychromatic to that of monochromatic light—from white light to coloured light.

These experiments touching the giving out of light can be easily and cheaply varied by burning green and red fire in front of the slit; the effect of these differently-coloured lights on a picture is also very striking.

The next thing we have to do then is to represent the action of an absorbing body,—to study the action of our theoretical screen—the action of bodies when they absorb light, and *therefore transform the original colour which that light possessed.* Liquids will form our most convenient screen to illustrate this, and they can be placed in a "cell" like that shown in the accompanying woodcut.

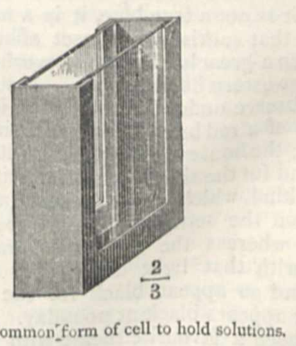


FIG. 6.—Common form of cell to hold solutions.

It will not be necessary to buy such an apparatus; two squares of glass, with a piece of india-rubber tubing between them, bent in the shape of a U; the glasses being kept in contact with the tubing by two india-rubber bands, form a cell which is wonderfully tight, and will serve our present purpose. This cell should be placed in front of the slit.

A little potassic permanganate added to the water in the cell, will act as a screen, and cut off the yellow part of the spectrum, and the adjacent regions of the orange and green. Solutions of blood or magenta will give also very definite indications of absorption, and if we have one of those handy little pocket spectroscopes, which now, I am glad to think, are becoming common, the absorption of the light of a candle by the blood in the lobe of a friend's ear, or in the interval between two closed fingers can be well seen by placing it between the slit and the light.

Let us now sum up as tersely as may be the conclusions at which we have so far arrived.

I. White light analysed by a prism gives us a continuous spectrum.

II. Coloured light analysed by a prism gives us a discontinuous spectrum.

III. Light may be coloured because originally the series of its components was not complete.

IV. Light may be coloured because although the series of its components was once complete, some parts of the light have been absorbed in its passage to the eye.

V. The bodies which give us white light are generally complex as to their molecules.

VI. The bodies which give rise to the phenomena of bright or dark lines are generally simple as to their molecules.

I now begin, with fear and trembling, to touch upon a part of my subject in which I ought to be the first to acknowledge that our ideas are not of the sure and certain kind. In what has gone before I have been careful to point out that, though the effect of incandescent bodies in producing and absorbing light was not likely to be directly applied by the artist, it was still in this sure and certain region that he should endeavour to follow the workings of laws clearly made out which might be in force elsewhere.

This brings me to state that in my opinion the colours of most natural bodies depend upon the fact that there are definite molecular states of all kinds of matter lying between those two extreme stages to the phenomena presented by which attention has been directed. Some years ago, in a communication to the Royal Society, I drew attention to evidence which seemed to indicate that many substances which emit under certain conditions a white light giving a continuous spectrum, and coloured light associated with the spectrum of lines under others, exist also in molecular groupings between these extreme conditions, giving us for one grouping a continuous spectrum at the red end, and for the other a continuous spectrum in the blue.

This is the most general statement that I can make, and I make it on account of the utility of such a statement. It has not yet been proved to be universally true, but the evidence I have already accumulated justifies me in setting it up as a working hypothesis. Not least valid among the lines of evidence on which I rely is the curious fact that the colours of almost all natural bodies can be at once explained by assuming them to be built up of these two molecular groupings to which I have called attention.

Let us take gold as an instance. It is yellow; but why is it yellow? Because the molecules of gold, as I believe, generally exist in two complexities, one of them competent to harmonise with the red rays of light, and therefore to absorb them, the other doing the same thing with the blue light, and for the same reason.

Gum a piece of gold leaf on a piece of glass for easy manipulation, and look at a bright light through it; it will be seen that the gold is green, or, in other words, that the blue and red have been absorbed, we have

V I B G Y O R

changed into

V I B G **Y O R**

by one set of molecules stopping

V I B

and another

Y O R

The reason that we get yellow light by reflection is that more of the central light is reflected than is transmitted.

If we do not consider reflection, the thing becomes simpler: for instance, if I take a tube one foot long and fill it with chlorine gas, and observe the spectrum of a white light through the tube, we find that chlorine absorbs only in the blue; the yellow and red are freely transmitted; a glass coloured red is so coloured because its molecules absorb the blue, and a blue glass is blue because it absorbs the red.

Prof. Stokes, in one of his lectures in South Kensington, dealt with the colours of natural bodies in connection with the absorption of light by them, and I may be permitted to close the present paper with the following extract from so great an authority:—

"What is the cause why a green leaf is green, or why a red poppy is red? It is frequently said that the reason why a red poppy is red and that a white lily is white is, that the lily reflects rays of all kinds, but the poppy reflects only the red ones, and if you place the red poppy in a pure spectrum it is luminous, like a white lily, in the red; but if you place it in the green it will be almost black, whereas the white lily will be brilliantly green. Now the common explanation, properly understood, is true; but it is not the whole truth, and if understood as it is liable to be understood, it is false. It is true that a red poppy reflects red rays, and a white lily reflects rays of all colours; but it is not true that the preference for the red to the green in the one case and the equality of action in the other takes place *in the act of reflection*. It is not a phenomenon of coloration by reflection. The coloured light is reflected or you would not see it; it is sent out of its course before it enters your eye, and it is true that the light, in its life's history, undergoes reflection; it is not true that it is in the act of reflection that the one colour gets the preference over the other. Here I have some solution of the colouring matter of green leaves in alcohol, and here is some more alcohol, with which I will dilute the former. I have obtained a beautiful green solution, although the green colour is not seen now by reflected, but by transmitted light. As regards the light which falls upon the surface there is a little white light reflected, just a s there would be from water, but very little is reflected from the surface where the fluid is in contact with the glass, the chief portion of that reflected being from the outer surface of the glass itself. You would not see any green at all in it unless there were something placed behind so as to reflect the light backwards. You see there that the colour of the green leaf, as ordinarily seen, is due to the combination of reflection with the phenomena of absorption, or the swallowing up of certain kinds of light when light is sent through a perfectly clear medium. I may illustrate this in another manner. Here is a vessel of water into which I will pour some blue solution. If I send light through it, it will appear of a deep blue, but if I hinder the light from coming behind, which I can do by putting black cloth behind it, it is simply dark; you do not see the blue colour at all. Why? Because there is nothing behind to reflect the light. Suppose I make it a little muddy by pouring into it some pounded chalk, you see the blue colour immediately. Why is that? You know that if powdered chalk were put into water it would not colour the fluid. But here each little particle of uncoloured chalk reflects a small quantity of light falling upon it, so that it fulfils the same office as a mirror placed behind the fluid. You may imagine that the particles of chalk are so many minute mirrors capable of reflecting light. If you take any one particle of chalk, say one-tenth of an inch deep, in the liquid, the light from the sky falls upon the fluid, it undergoes absorption in passing through that first tenth of an inch, and then the portion of light which is left is reflected by that little particle of chalk, and passes out again, and so, as regards that single particle, the light which reaches your eye from beneath that depth has itself gone through a stratum of fluid of one-fifth of an inch in thickness, and accordingly you see the colours produced by selective absorption; that is to say, by the absorption of certain kinds of light, which are more greedily devoured by the fluid than the other kinds. This is what takes place in the green leaf and in the petals of flowers. Let us take the white lily. If the petal of the flower had been merely a sheet of thin glass you would not have seen that white colour. There would have been a little light reflected from the first surface and the back surface, but the petal is really composed of a vast assemblage of little cells, at each of which partial reflection takes place, so that it resembles some finely-powdered glass, which would form a white powder, because each little surface is capable of reflecting the light, although a single sheet of glass would not be white. The petal of the white lily is just in the condition of the powder. It is full of little cells, full, optically speaking, of irregularities, from each of which a portion of light is reflected, so that, all kinds being reflected alike, and there being nothing in the white lily to cause preferential selection of one over the other—nothing to sift the light, as it were—you get a considerable quantity of light reflected back to the eye, but it is white. What is the difference between that and the red poppy? The red poppy is, as it were, a white lily infused with a red fluid; there is light continually reflected backwards and forwards, just as before, at the surface of the cells; but that light, in going and coming, passes through the coloured juice of the plant. It is the same thing with a green leaf. The structure is irregular, optically considered; there are constantly reflections, backwards and forwards, of light, which penetrates a little depth and is reflected, and has to pass through a certain stratum of this colouring matter, to which the name chlorophyll has been given, but which is really a mixture. That is what takes place generally as regards the coloration of bodies; it is a phenomenon not of reflection, not of selection of one kind of light for more copious reflection than another, but of absorption, or the swallowing up of certain kinds of light. Reflection comes in in order to enable us to see the light which otherwise would not enter the eye at all, but would go off in another direction."

J. NORMAN LOCKYER

COSMIC METEOROLOGY

SEVERAL articles have appeared at different times under the title of "La Météorologie Cosmique," from the pen of M. Faye: the last and most complete forms an exceedingly interesting "Notice Scientifique," appended to the *Annuaire* of the Bureau des Longitudes for 1878. In this memoir, written with the usual clearness and talent of the distinguished French astronomer, a number of results connected with real or supposed solar, lunar, and planetary actions on our earth are examined and criticised. As M. Faye has omitted several facts of considerable importance in his "Notice," and has misunderstood others, a reconsideration of some of the questions he has studied, with the additional light to be obtained from the facts alluded to, may not be without interest and use.

M. Faye's thesis is given in the first words of his article "Meteorological phenomena have their origin in solar heat." It is added, "This is now no longer sufficient. Cosmic influences are introduced, those of the planets, of the spots and rotation of the sun, of shooting-stars, the moon, and besides these, magnetic and electric actions are supposed to intervene incessantly between the bodies of the solar system." I shall refer to some of the most important questions under their different heads.

The Moon's Influence in producing Atmospheric Variations.—The popular beliefs in the moon's influence on the weather are first disposed of; they are conclusions from unrecorded observations where the coincidences are remembered and the oppositions are forgotten; and they are opposed to strict deductions when all the facts are employed.

Agreeing, as all men of science do, with this decision, the question remains, Whether the moon may not have some slight effect in producing meteorological variations? She reflects, absorbs, and radiates the solar heat; may this heat, in accordance with the thesis, not produce some effect on our atmosphere?

Sir John Herschel had observed the tendency to disappearance of clouds under the full moon; this he considered a fact which might be explained by the absorption of the radiated lunar heat in the upper strata of our atmosphere. He cited Humboldt's statement as to the fact being well known to pilots and seamen of Spanish America. I may add the testimony of Barnardin de St. Pierre, who, in his "Voyage à l'Ile de Réunion," says: "I remarked constantly that the rising of the moon dissipated the clouds in a marked way. Two hours after rising, the sky is perfectly clear" ("Avril, 1768"). Herschel also cited in favour of his "meteorological fact," a result supported by the authority of Arago, that rather more rain falls near new than near full moon.

Arago's conclusion that the phenomenon was "incontestable of a connection existing between the number of rainy days and the phases of the moon" was founded on the observations of Schübler, of Bouvard and of Eisenlohr, three series which, on the whole, confirmed each other. Schübler also, as Arago showed, had found that the *quantity* of rain which fell was greater near new than near full moon. These results, accepted by Arago, have not been noticed by M. Faye when he cites Herschel only, as one of those "men of science who interest themselves in popular prejudices, take bravely their defence in hand and exert themselves to furnish not facts but arguments in their favour." It seems, indeed, to have been forgotten that Herschel's argument was given to explain what he considered a meteorological fact.

M. Faye founds his argument wholly on the conclusions of M. Schiaparelli from a weather register kept at Vigevano by Dr. Serafini during thirty-eight years (1827-1864).[1] The Italian physician entered the weather as clear, cloudy or mixed (*misti*), or rainy from morning to evening. M. Schiaparelli finds from this register that the sky was clearest in the first quarter of the moon. It has not been remarked that if the moon's heat has any effect in dissipating clouds, as Herschel and others believed, this must be seen best when the moon is near full, that is to say, during the night hours, for which Dr. Serafini's register has nothing to say. In confirmation of the conclusion that the moon does not dissipate the clouds, another result from the Vigevano weather register is cited, namely, that the greatest number of rainy days happens near full moon. This result is opposed to that derived from the observations of Schübler, Bouvard, and Eisenlohr.

The value to be given to observations of the number of rainy days must evidently depend on whether the observations include the rainy nights; and an investigation on this question, to have any considerable weight, should depend rather on the measured rainfall than on the term "rainy day," for which no distinct definition is given.

The great objection to M. Faye's conclusions, as far as the facts go, is to be found in their entire dependence on the Vigevano weather register (*da mane a sera*). No notice is taken of other observations and results showing a lunar action on our atmosphere, such as those already mentioned, which Arago considered incontestable, those of Mädler and Kreil, and the more recent investigations of Mr. Park Harrison and Prof. Balfour Stewart. All of these, and many others, must be carefully considered before we can accept the conclusion that the moon has no influence on our atmosphere. The subject is, however, too large to be entered into here at present, and it will be possible to study it better after other conclusions of the learned French Academician have been examined.

There is, however, a part of the argument, whatever the results obtained may say, which merits particular

[1] *Memorie del R. Istituto Lombardo*, L. 10.

consideration; and that is, that the moon's *heat* cannot produce the phenomena in question. M. Faye shows that if the moon's reflected heat is in the same proportion as the reflected light, such heat cannot produce a change of temperature of a thousandth of a degree Fahrenheit. I would remark that Lord Rosse's carefully-made experiments with the most delicate apparatus have shown for the total heat radiated and reflected, nearly ten times the proportion given by the reflected light; but, as M. Faye observes, if the proportion were increased a hundredfold the effect would still be insensible. "How then," it is added, "can we expect such an action to dissipate the clouds when that of the sun does not always succeed?"

If, however, we can establish that real lunar actions exist which cannot be explained by the moon's heat reflected or radiated, the only philosophical conclusion will be that the moon must act in some other way, which it will be in the interests of science to seek out.

Influence of the Sun-spots on the Earth's Magnetism.—It has been found that since the first accurate series of magnetic observations, towards the end of last century, to the present time, the maximum and minimum of sun-spot frequency have occurred at the same times as those of the diurnal oscillation of the magnetic needle; but because Dr. Wolf has believed that in the interval between 1787 and 1818, when the observations of both phenomena were very incomplete, there were only two cycles for both, and because Dr. Lamont and myself believe there were three for both, M. Faye concludes that the mean length of the period for the magnetic needle was different from that for the sun-spots. The true conclusion is—if Dr. Wolf is right the mean length of the period for *both* phenomena since 1787 is nearly twelve years; if the other view is right the mean length for *both* is 10·45 years. I have already considered this question in NATURE (vol. xvii. p. 262). The *egalité rigoureuse* of the lengths of the periods sought by M. Faye is thus established whatever view be taken.

It is next sought to show that any variation in the earth's magnetism in the decennial period cannot be due to variations in the solar heat produced by the spots. Founding on Mr. Langley's observations for the temperature of the photosphere, of the nucleus and the penumbra of spots, M. Faye shows, making use as before of the absolute temperature of the earth, that the variation of temperature due to the spotted surface of the sun cannot exceed $\pm$ 0°·3 Fahr.

I venture here to remark, in the first place, that I have given strong grounds elsewhere for believing that the sun-spots are not the cause of the decennial period in the magnetic variations; but that both are due to the action of a common cause.[1] I quite accept then M. Faye's conclusion. It may, however, be argued that more heat proceeds from the sun in years of many, than in those of few, spots, but the observations of temperature, which have been made with so much accuracy in many countries for the last twenty years, will prove at once that no marked variation of the mean temperature occurs in the decennial period.

We must here, however, consider the reasoning which has been employed on this subject. It is shown that the variation of temperature due to the spotted surface of the sun cannot explain the change in the amplitude of the diurnal oscillations of the needle. It is, however, a mere assumption that this oscillation is due to the solar heat, an assumption for which there is really no sound basis, unless some very rude attempts at a hypothesis can be so considered.

M. Faye says, "This phenomenon is absolutely general; it appears over the whole earth following the same laws, only the amplitude of the oscillation, which in the mean is at Paris 10′, is reduced to 1′ or 2′ between the tropics, and goes on increasing thence towards the poles." This variation of range is afterwards compared with the inverse law for the diurnal variation of the barometer, the range of which *diminishes* from the tropics towards the poles.

This view, however, is founded on a misconception of the facts; we might just as well say that the earth's magnetic force diminishes from the equator towards the poles (which is just the reverse of the truth), because its horizontal component does so. The mode in which this movement varies in amount from the tropics towards the poles appears to be imperfectly known, and as it is essential to make this clear before we can compare the facts with any hypothesis, I shall now attempt to do so.

Diurnal Variation of the Magnetic Declination.—The horizontally suspended magnetic needle performs an oscillation in twenty-four hours, during which the north end is most westerly in the northern hemisphere, and most easterly in the southern hemisphere, between 1 and 2 P.M. If we consider needles at different stations on the same meridian, it might be supposed that as we approached the equator this opposition of movement would result in the needle becoming stationary. That was Arago's conclusion. This idea, however, neglected the position of the sun. The range of the oscillation is greatest in our hemisphere when the sun is north, and greatest in the southern hemisphere when the sun is south of the equator. It is only when the sun has the intermediate position, near the time of the equinoxes, that the forces are nearly balanced at equatorial stations.

If we suspend an unmagnetic steel needle horizontally on its centre of gravity, so that it can move both in a horizontal and vertical plane, and then magnetise it, the needle *dips* with one end below, the other above, the horizontal plane passing through its centre; and the direction in which it lies is that of the earth's magnetic force. If this needle moves up, or down, or sideways, this is because other forces pull it one way or another, or because the direction of the earth's magnetic force has changed. Let us suppose in the first instance that the latter hypothesis is the true one, that is to say, that the earth's magnetic axis moves with the sun, the north pole having the greatest movement when the sun is in the northern hemisphere. If the dipping needle moves in the plane perpendicular to the vertical plane, through an angle u, and we wish to know what would have been the corresponding movements eastwards or westwards, if the needle had been made horizontal, by the addition of a small weight to the end above the horizontal plane, we must *divide* the angle u by the cosine of the *dip* below the horizon. Now in England the cosine of the dip is about $\frac{1}{3}$, so that the horizontal needle would have moved through an angle of $3u$. We obtain similarly the former from the latter by *multiplying* by the cosine of the dip.

We may now see, from observations made at different stations, what is the range of the monthly mean diurnal variation of the horizontal needle multiplied by cosine *dip* in a month for which it is a maximum in the northern hemisphere. The following are approximations to the mean ranges thus obtained from several years observations in the month of August for the northern hemisphere.

Station.	Lat.	Dip.	Range of hor. needle × cos. dip.
Makerstoun	56° N. ...	71° N. ...	4′·0
Toronto	44 ,, ...	75 ,, ...	3·5
Simla	31 ,, ...	42 ,, ...	4·6
Bombay	19 ,, ...	19 ,, ...	5·4
Madras	13 ,, ...	8 ,, ...	4·8
Trevandrum	9 ,, ...	3 S. ...	3·6

In a similar way we find for the month of February for southern stations—

[1] "On the Decennial Period" (*Trans.* Roy. Soc. Edinb., vol. xxvii. p. 593).

St. Helena	16 S. ...	22 S. ...	4·8
Cape of Good Hope	34 " ...	53 " ...	4·6
Hobarton	43 " ...	70 " ...	4·3

It will be seen that in the months in which the sun's action is a maximum,[1] for each hemisphere the east and west movements of the needle in its true position (that which is independent of gravity) do not vary with latitude; the maximum range appears, in fact, to take place near the tropics, and when the sun is in the zenith.

If, however, we should prefer to consider the oscillations of the horizontal needle due to the direct action of electrical currents upon it rather than upon the earth, we must remember the change of the force which directs the needle as we proceed from one latitude to another. If we wish to compare the vertical heights through which a body will fall in a second of time on an inclined plane at two stations, we must take into account not only the force of gravity at each station, but also the angle which the inclined plane makes with the horizontal plane. When we employ the same unit of directive force at the stations in the north hemisphere for the month of August, we obtain the following comparative ranges :—

Makerstoun ...	4·3
Toronto ...	4·8
Simla	4·4
Bombay ...	4·6
Madras	3·8 (September = 4·2).

The values are less for Trevandrum and St. Helena, but there is no appearance of a law which can be referred to latitude; and there is no way in which we can examine the question which will satisfy such a relation. If we take any zone or zones of the earth which will include as much of the northern as the southern hemisphere, the mean movement for them will be nearly zero, on account of the opposite directions of the oscillations; it is for this reason that there is a diminution of range, especially in the months near the equinoxes, for equatorial stations.

On the whole the conclusion is, that the diurnal law of oscillation east and west of a magnetic needle is nearly the same in all latitudes for a given position of the sun and a given directive force. The deviations from this rule are connected with magnetic disturbances which have most effect near the poles, and with the opposition of forces near the equator. We have thus to deal with a phenomenon which is little dependent upon local causes, and which may, in its great features, be considered cosmic.[2]

These facts understood, we are now in a better position to consider the great change in the range of this oscillation, which occurs in the decennial and sun-spot period. Let us examine what that change really is. It does not matter here whether we refer to the motion of the horizontal or of the dipping needle; we find that if the mean range is counted 10 in England in the years when it is a minimum, the years of fewest spots, then it becomes 16 or thereby when it is a maximum, that is, in the years for which the sunspots are most numerous.

Now this great change in the effect of the solar action is felt in nearly the same way, and to the same proportionate amount, all over the globe. The law of the oscillation is not changed; the needle attains the most westerly position in one hemisphere, and the most easterly in the other, at the same hour as before, but the oscillation is nearly sixty per cent. greater at Hobarton in Van Diemen Island, at Trevandrum on the magnetic equator, at Toronto in Canada, and in England.

When the observations have been continued sufficiently long with equal care, we can find that the ratio of the maximum range to the minimum is undergoing, at some stations at least, a slow change. Thus, at Trevandrum the successive ratios of the maximum of 1860 and 1870 to the preceding and following minima are—

$$\frac{1860}{1856},\ 1·59;\ \frac{1860}{1866},\ 1·57;\ \frac{1870}{1866},\ 1·55;\ \frac{1870}{1877},\ 1·51.$$

It must be remembered that the thesis which M. Faye supports is, that the diurnal oscillations of the magnetic needle are due to the solar heat, and that he has shown that no appreciable change of temperature is due to the spotted surface of the sun. We may ask then, Where are we to find the change of temperature which causes so great a variation in the sun's action? We need not calculate the difference of temperature between the photosphere and the nucleus of a spot, and we need not theorise on the possible difference between the solar radiation when there are few and many spots; we have got thermometers; we have even observations of the evaporation of water, from which solar action M. Faye finds the atmospheric electricity which should produce the magnetic variations. What do they say? If effects have any relation whatever to their causes, surely when the effect of the solar action in producing the diurnal variations of the earth's magnetism is increased by a half or three-quarters of its value from the time of most spots till the sun shines with unspotted surface, we should expect some marked changes of temperature from year to year, if change of temperature is in question. I have already remarked that no such change of temperature has been found.

M. Faye says with reference to the question, What is the cause of the decennial magnetic variation? "The question would perhaps be embarrassing if we had only that to ask, but the elements of terrestrial magnetism present other variations which are as much independent of the sun-spots. Such are the secular changes which displace gradually the magnetic poles of our terrestrial globe. We must seek the cause not in the heavens but in the slow modifications of which the earth's surface is the theatre. They are due probably to the works of men and above all to the continued action of geological forces." Of course these ideas refer to the secular changes, even for which they will not be readily accepted, but they do not touch on the fact that an explanation is offered, that of the solar heat, for the diurnal variations, and that no evidence is produced that the supposed cause undergoes any change in the decennial period.

I have referred only to oscillations of the magnetic needle, which may be considered due to the variations of an easterly force; but the force of the earth's magnetism which directs the needle north and south, obeys also a law of diurnal variation, and the range of this variation follows the same law in the decennial period as that for the ranges of the oscillations. Thus at Trevandrum, near the magnetic equator, the *range* of the diurnal variation of the total force of the earth's magnetism in the year for which it was a maximum, was to the range in the year of minimum as 17·7 to 10. The corresponding ratio of the ranges for the oscillations of the needle having been as 15·9 to 10.[1]

As there is not the slightest evidence of a decennial variation in the solar heat, and as there is an absolute certainty that if any variation exists it is of an amount so very small that it could not account for the great changes in the magnetic variations, the conclusion appears to me inevitable, that these variations are *not* due to the solar heat. We fortunately possess the means of deciding this question by the study of phenomena due to our satellite, whose heating action M. Faye has shown to be quite inappreciable. JOHN ALLAN BROUN

(*To be continued.*)

[1] The month of maximum varies within the tropics: at Madras it is in September, and for that month the range multiplied by cos. dip = 5'·3 nearly.

[2] For these reasons no such inverse relation with latitude exists as M. Faye has supposed between the diurnal oscillation of the magnetic needle and of the barometer.

[1] I would remark here that the epoch of maximum range of force was not exactly the same as that for the maximum oscillation, a fact which may not be without importance when the mode of solar action is investigated.

THE MICROPHONE

WE have received the following communications on this subject:—

AT a discussion upon Mr. William Preece's paper on the microphone, which took place before the Society of Telegraph Engineers on Thursday last, the Duke of Argyll called attention to the important part which that invention was likely to play in physiological research. As chairman of the meeting I took occasion to refer to the intimate connection between the microphone and its two elder sisters, the telephone and the phonograph, in conjunction with which it formed a discovery which would probably be hereafter regarded as one of the greatest achievements in natural science of the present century. I ventured further to draw an anology between the action of the phonograph and the action of the brain in the exercise of memory, and with your permission I will enlarge upon this speculation to the extent of making my reasoning clear enough to submit the same to the critical test.

All impressions received by us from without, either through the tympanum of the ear, the retina of the eye, or through the sensitive nerves of the skin, are, it is generally believed by physiologists, communicated to corpuscular bodies in the brain, which lie embedded in a grey substance, the nature and precise function of which have not yet been fully explained. It would appear that the corpuscular bodies in which the sensitive nerves terminate are connected, through the medium of extremely delicate filaments, with the nervous system of volition, the reaction of the one system upon the other being attributable to mental energy. It may be conceived that any fresh impressions received on the extremely complex sensitive network of the brain may give rise then and there to acts of volition; but how, it may be asked, can acts of volition arise from impressions that were communicated through the sensitive nerves years before, having been committed in the meantime to what we term the memory? But in order that the mind can deal with an impression previously received it seems necessary that it must have the power of reproducing the same from some material record by which the impression has been rendered permanent. Take the case of a tune that we have heard in early youth and which may not have since recurred to us. By some incident or other that tune and the words connected with it become suddenly revivified in the mind. If the tune had been sung into a phonograph it could have been reproduced at any time by releasing a spring moving the barrel of the instrument; and it seems a fair question to ask whether the grey substance of the brain may not, after all, be something analogous to a storehouse of phonographic impressions representing the accumulated treasure of our knowledge and experience, to be called into requisition by the directing power of the mind in turning on, as it were, one barrel or another.

Such a hypothesis might possibly serve also to explain how in sleep, when the directing power of the mind is not active, a local disturbance in the nervous system may turn on one or more phonographic barrels at a time, and thus produce the confused images of dreamland! A powerful mind would exercise a complete control over the innumerable barrels constituting our store of knowledge, whereas in a weak mind the impressions of the past would be brought back into evidence in a confused and irregular manner. Such a supposition might also account for the more vivid recollection of impressions received in early life, when the mechanical record stored up in the brain may be supposed to have been more distinctly and indelibly rendered. In speaking of these impressions as phonographic it does not follow that they were originally conveyed through the tympanum of the ear. Mr. Willoughby Smith, at the meeting above referred to, called attention to the fact that, by substituting crystalline selenium for carbon in the microphone, a ray of sunlight directed upon the selenium produces a noise comparable with that produced by a Nasmyth hammer; and it is quite feasible that the impressions received through the retina of the eye, and the nervous system generally, would be equally susceptible of being recorded in the cerebral storehouse. The record itself might be supposed to be of a mechanical, or, more probably, of a molecular character, the one thing important being that it must be material.

These observations are, no doubt, extremely crude, but may serve possibly to direct the attention of physiologists to a point of interest to their science; nor would it be the first occasion on which a phenomenon of inanimate nature had revealed the secrets of animate organisation.

C. WILLIAM SIEMENS

I HAVE been much interested in your account of the microphone of Prof. Hughes, and I have made, as doubtless many of your readers have also done, the different forms of instrument described by him. The action of the instrument is there apparently attributed to the change of conductivity of the charcoal or carbon or of the mercury globules therein, under the influence of sonorous waves; and whether this is correct is a question worthy of consideration in your columns, and I therefore write more for the purpose of leading others into the inquiry than of making assertions on the subject. My experiments point to another cause, viz., the variation of conducting sectional area of a bad conductor due to the increased or diminished pressure on the point of contact. I am not, of course, referring to the action of the instrument when the vibration is sufficient to absolutely sever the contact, which simply causes the telephone plate to vibrate either in its own period, or some other than that due to the acting sound, as is the case when a musical box is placed on the same table with the instrument; but to the forced or articulate vibrations—the reproduction of the sound acting on the microphone.

Of the several forms of instrument described by Prof. Hughes I have chiefly used that consisting of a rod of charcoal pointed at both ends, supported in a vertical position with its lower point resting in a hollow in a similar piece of charcoal, while its upper end rests against the sides of a similar hollow above. This form is extremely sensitive, and it is difficult to prevent the circuit being broken while having it sufficiently near the source of sound to be reproduced; the sound of a musical box is perfectly rendered, when so far away that there is an absence of jarring from breaks in the circuit; but in talking to the instrument, any rise in the voice breaks contact and produces the jarring sound in the telephone, to the exclusion of all articulation.

I find that any sort of charcoal or carbon will answer, whether soaked with mercury or not; I therefore conclude that the mercury has little or nothing to do with the action. I have tried the effect of sound on rods of carbon and charcoal both saturated with mercury and not so saturated, so arranged that the vibrations could not alter the area of contact, and have obtained no sound whatever from the telephone in the circuit; I therefore conclude that the action takes place at the point or points of contact, and is due to the change of conducting area. To Prof. Hughes is due the credit of inventing a means of varying the electric current with extreme rapidity and slight motion without absolutely breaking the circuit, but I doubt whether a *microphone* is a proper term for describing the instrument. In gently brushing the stand of the instrument, sound is heard in the telephone, but it does not at all follow that what we hear is a magnified reproduction of the brushing sound; for if the rapidity of the vibrations or motion produced by brushing is insufficient to produce sound, still they may move the charcoal sufficiently to produce alternations of current, each of which may be able to set up vibrations in the telephone plate in its own period, or a modification of it, giving what I call the jarring sound. If, therefore, we have this sound, we know that either the microphone is exposed to sounds so loud as to produce complete break of contact, or that there is a motion going on affecting it, of insufficient rapidity to be audible.

With the object of reproducing the voice or musical notes, I have made the following modification of the instrument:—A ferrotype plate 3 inches in diameter is fastened over a hole $2\frac{1}{2}$ inches in diameter in a thick piece of wood; a flat piece of gas carbon weighing a few grains and having a fine copper wire attached to it is fastened to the top of the plate in the centre; over the piece of carbon is suspended by a wire spring another piece of carbon finely pointed, weighing about $\frac{1}{4}$ oz., and adjusted so as just to touch the carbon plate. The current is then led by the wires through the carbon point, and by careful adjustment of the latter almost any degree of sensitiveness can be attained. Whenever the sound becomes too loud the current is broken, and minute sparks are seen at the carbon point, and the jarring sound is heard at the same time in the telephone. The sound of a musical box is perfectly reproduced when the box is held in the air; the instrument is therefore sensible to sound-waves in air as in solids. GEO. M. SEABROKE

Rugby

I SEND an account of an experiment with the microphone which may interest some of your readers.

A microphone, made of three pieces of gas carbon (as described by Prof. Hughes) and the primary wire of a Du Bois Reymond's induction-coil, are placed in the circuit of a single Daniell cell. The wires from the secondary coil (pushed home) are attached to the poles of a Lippmann's capillary electrometer. The Daniell and microphone are twenty-five feet distant from the electrometer. If an observer watches the capillary-tube and speaks or sings to the microphone (*which is twenty-five feet distant*) definite and large movements of the mercury-column will be seen. The movements for various letters resemble those which have been previously observed to take place with the telephone, the "w" giving its curious double movement.

F. J. M. PAGE

Physiological Laboratory, University College, London

EARTHQUAKE IN VENEZUELA

IN the evening of the 12th of this month a severe earthquake destroyed the town of Cua, in this country. Cua is situated on the left bank of the River Tuy, in 10° 8′ 15″ L. N. and 66° 55′ W., Greenw. The height over the level of the Caribbean Sea I found in 1873, by barometrical measurement, 232 metres. It was the centre of a very flourishing agricultural district (annual produce, about 80,000*l*. a year), and had about 3,000 inhabitants.

The weather had been for weeks exceedingly hot, as generally this year in Venezuela. At 5 o'clock in the afternoon, before the earthquake, a temperature of 100° is said to have been noticed, and six days later, at the same hour, I observed myself 95°. The sky was clear, and the moon in perfect brightness. The shock occurred some minutes before a quarter to nine o'clock, and so violent was it that in less than two seconds all the centre of the town was a heap of ruins. It is impossible to fix the exact time of the shock, but it was felt in Carácas at 8h. 41m. 34s., the distance in a straight line between both places being about twenty-six English miles.

The centre of the town was situated on a small hill, about 20 metres over the lower part. The hill is composed of gneiss, micaceous and chloritic schists, rising rather steep towards W.S.W. This hill is surrounded by strata of clay and marl, covered by a deep stratum of alluvial soil, and resting on dark limestone and argillaceous schists, containing numerous crystals of iron pyrites.

Only the upper town was laid waste; the lower part suffered comparatively very little. From actual observations I found that the angle of emergence of the shock was about 60°. The centre cannot have been very deep, as the destruction was limited to a spot measuring only one square mile, although the shock of the transverse wave was felt in places 100 miles distant. The soil had burst at different places, giving issue to water highly impregnated with sulphuretted hydrogen. The shocks continued for several days, and are not yet entirely gone, but no further damage has been caused. About 300 people were killed; the loss of property is said to be about 300,000*l*. sterling.

I have reason to think that this earthquake had nothing to do with volcanic forces, but was due to an interior subsidence or downfall of calcareous rock, as I intend to prove in a special memoir on this subject, as soon as I shall have visited the locality once more.

Carácas, April 30 A. ERNST

OUR ASTRONOMICAL COLUMN

TEMPEL'S COMET, 1873, II.—We continue the ephemeris of this comet, for the latter half of June, as given by M. Schulhof in the Paris *Bulletin International* of May 7. If the calculated epoch of perihelion passage be approximately correct, the intensity of light will be increasing, and the comet would arrive at its least distance from the earth early in July. But the possible error in the mean motion determined from the observations of 1873, may render a search over a wide extent of sky unavoidable, if the comet is to be recovered at the present return. Shortly before the completion of his calculations M. Schulhof informed the writer that the probable error in the mean daily motion would not exceed $\pm$ 7″, but this degree of uncertainty involves a difference of nearly $\pm$ 20 days in the date of perihelion passage, so that the comet may be found after close search in a position considerably distant from the computed one. As in other similar cases, if the observer has the command of an equatorially-mounted instrument of good aperture, the most promising plan of search will be to commence at the calculated declination for the day, extending the sweep to 30m. or 40m. on each side of the calculated R.A., and to continue the same proceeding for 3° or 4° on each side of the calculated declination. It may be remarked that the computed R.A. for a certain change in perihelion passage, varies more rapidly than the computed declination. Perhaps there is a greater probability of the comet being detected at the latter end of June than subsequently, if the weather is generally favourable for a careful search.

The following positions for Paris midnight are deduced on the assumption that the comet will arrive at perihelion Sept. 1·5, the most probable date:—

	Right Ascension. h. m. s.	N. Declination. ° ′	Distance from earth.	Intensity of light.
June 15	15 34 44	5 6	0·667	0·90
,, 19	15 32 15	4 17	0·659	0·95
,, 23	15 30 20	3 20	0·654	0·99
,, 27	15 29 5	2 15	0·651	1·03
July 1	15 28 31	1 1	0·649	1·06

THE RECENT TRANSIT OF MERCURY.—In the instructions for observing this phenomenon suggested by Prof. Newcomb, and circulated by the United States Naval Observatory, it is remarked that "its accurate observation is of especial importance as affording data which will be decisive of the question whether the result of Leverrier, that the motion of the perihelion of Mercury is much greater than that due to the action of the known planets, is really correct." So far as the results of observation have been made known, there is every indication that the theory of Leverrier will receive a striking confirmation therefrom. The observations of first internal contact in Europe are closely accordant with calculation; and in a telegram from M. André, in charge of the French expedition, despatched through the liberality of M. Bischoffsheim to Ogden, in the Utah territory, for the observation of the transit, it is stated—"sortie confirme théorie."

Adopting Leverrier's diameters of sun and planet, deduced from his elaborate discussion of the transits of Mercury observed to 1832, and the value of solar parallax determined by Professor Newcomb (8″·848), we have the following equations for the reduction of observed times of first external and internal contacts to the centre of the earth:—

	h. m. s.		s.		s.		° ′
First ext. cont. ...	3 13 1·0	+	74·53, $\rho \sin l$	+	80·89, $\rho \cos l$, cos	(L −	56 49·3)
First int. cont. ...	3 16 8·4	+	74·92, $\rho \sin l$	+	81·32, $\rho \cos l$, cos	(L −	56 2·4)

Where l is the geocentric latitude, which may be obtained together with ρ, the radius of earth at the point of observation, from Bessel's Tables in the *Berliner astronomisches Jahrbuch* for 1853, and L is the longitude from Greenwich counted towards the east: the resulting times are for Greenwich. We shall give next week a comparison between observation and calculation.

ENCKE'S COMET.—The *Fürstl. Jablonowski'schen Gesellschaft* of Leipsic have offered a prize in 1881 for a new investigation on the motion of this body, their former similar offer for 1877 not having met with a response. It is urged that the researches of Dr. von Asten, so far as they are known, have not led to any definite result, and other periodical comets not having shown any indications of a resisting action such as is apparent in the motion of Encke's comet, a further complete and separate investigation (*vollständige Neubearbeitung*) is much to be desired. Accordingly the Society's prize of 700 mark is again offered. It is stipulated that all known perturbing forces are to be taken into account, and the calculation is at least to include the period from 1848 to the last appearance of the comet. A similar work for the earlier portion of the interval elapsed since the first discovery in 1786, is reserved as the subject for a future prize.

In connection with the anomalous motion of Encke's comet it may be remarked that Brorsen's comet of short period appears deserving of much closer computation than it has yet received. After that of Encke's comet its perihelion distance is considerably less than in the case of any of the other comets forming this particular group, as the following statement will show. Biela's and De Vico's comets are omitted:—

Perihelion Distance of		Encke's Comet	0·333
,,	,,	Brorsen's	0·595
,,	,,	Winnecke's	0·781
,,	,,	D'Arrest's	1·280
,,	,,	Tempel's (1873, II.) ...	1·339
,,	,,	Faye's	1·687
,,	,,	Tempel's 1867, II.) ...	1·769

GEOGRAPHICAL NOTES

THE Anniversary Meeting of the Geographical Society on Monday was not marked by any unusual feature. The address of the president—his retiring one, as it turns out—consisted as usual of a comprehensive review of the geographical work of the past year, an unusually eventful one in exploration. The Society is as prosperous as ever in members and money. There were on April 30 3,334 Fellows on the register, of whom no less than 762 are life members On the motion of Sir Henry Rawlinson, the meeting adopted an alteration of the rule regulating admission to meetings of exceptional interest, with a view to obviate certain difficulties which have arisen in this respect. The Royal Medals, the award of which we have already announced, were presented to Count Münster, the German ambassador, on behalf of Baron F. von Richthofen, the President of the German Geographical Society, and to Capt. H. Trotter, R.E., personally. The schools' prize medals were also presented to the successful competitors whose names we have before recorded. From the new president, Lord Dufferin, we may next year expect an address marked by unusual raciness, eloquence, and intelligence; Lord Dufferin will probably return to this country in autumn.

THE Committee of the African Exploration Fund of the Royal Geographical Society have at length definitely resolved to despatch a carefully organised expedition to explore the unknown tract of country lying between the caravan road which, as we have before mentioned, is being constructed from Dar-es-Salaam (a few miles south of Zanzibar), to the northern end of Lake Nyassa. Mr. Keith Johnston will, we believe, be in command, and, will be accompanied by another European not yet selected. Should this expedition prove successful, and, what is equally important, sufficient funds be forthcoming, the committee contemplate pushing their explorations to the southern end of Lake Tanganyika, a further distance of 190 miles, thus completing approximately two of the routes sketched out in the circular issued last summer. In order to enable the committee to despatch this expedition, which is expected to furnish important and valuable geographical information, the Council of the Geographical Society have just made a further grant of 500*l.* to the fund, and it is hoped that the public and the subscribers will lend it such additional support as will be required to carry out the objects in view.

AT the last meeting of the Geographical Society of Paris M. de Lesseps stated that Col. Gordon had pushed the Egyptian advanced posts up to the equator, and that now any traveller can go from Paris to the equator within sixty days if he has procured a letter of introduction from M. de Lesseps. Abbé Debaize, who, as we have already stated, intends to cross Africa, has availed himself of this privilege and is probably now on the banks of the Albert Nyanza. M. de Lesseps states, moreover, that the number of lakes is greater than was supposed after Stanley's mission, and Col. Gordon is making a careful survey of the newly Egyptianised country. He has sent to M. Daubrée, Director of the School of Mines, some specimens of gold and silver ores brought from the interior, in order to ascertain their value. The Society has recently received a detailed account of the expedition made by MM. Cambier and Marno from Zanzibar during the past winter. The journey lasted seven weeks, and was accomplished without loss of life. The chief object of this tour was to test the availability of the route by Mpwapwa for expeditions into the interior of Equatorial Africa. It was found to be well adapted even for waggons.

THE Italian Consul at Aden, who is now in Europe, is occupied with the formation of a society for the purpose of acquiring a portion of land and forming an Italian colony at Shoa. The object of the colony is to establish commercial relations between Italy and Central Africa. The African traveller, Carlo Piaggia, is now making the final preparations for a new journey to Equatorial Africa. This journey will be his fourth; formerly he has principally visited Abyssinia and Soudan.

MR. GORDON BENNETT'S polar expedition, to which we have already referred, is not to start, it would seem, till 1879, when, in June, it will probably leave San Francisco for the route by Behring's Straits. The *Pandora,* which will be re-christened the *Jeannette,* is being thoroughly refitted in Walker's yards on the Thames.

NOTES

THE Haarlem Society of Sciences resolved, some years ago, to award, biennially, a medal to the individual who, by his researches, discoveries, or inventions, during the previous twenty years, had, in the judgment of the society, distinguished himself in an exceptional manner in a particular branch of science. This year the medal was to be devoted to astronomy, and on the 18th inst. was awarded to Prof. Simon Newcomb. We believe the medal would have been awarded to Sir George Airy if the committee had felt themselves at liberty to embrace a period greater than twenty years past; but, according to the rules regulating the award, they are rigidly confined to the period stated.

MR. CARL BOCK, F.G.S., who has had considerable experience in the collection of shells and other specimens of natural history, is leaving England at the end of this week for Padang, in Sumatra, in order to explore and collect in the highlands of the interior of the island.

RECENT advices from Auckland state that Signor Beccari and Capt. D'Albertis, a nephew of the New Guinea explorer, were visiting the colony on their way to Europe, and that Signor Beccari had with him some 12,000 specimens of the flora and fauna of New Guinea.

IF Mr. Herbert Spencer remembers his Bible, an oft-quoted passage must have occurred to him on Sunday (appropriately)—"A prophet is not without honour save in his own country and among his own people." On that day, in Paris, where he has been spending a few days, a semi-public dinner was given to the English philosopher by a number of his admirers, headed by the well-known publisher M. Germer-Baillière. Mr. Spencer, in replying to the toast of his health—and he actually replied to a toast, and that too in a style not much out of the common—hinted that he was better known and better appreciated in France than in England, where, so far as we know, he never appeared either on a public or semi-public occasion. A decidedly social evening seems to have been passed by the assembled *savants*, Mr. Spencer concluding his genial and complimentary reply by drinking to the peculiarly French sentiment—Brotherhood (*à la fraternité*).

M. BARDOUX has written to the Meteorological Society of France asking them to organise the congress of meteorology. The committee of this association have written to the Association Scientifique of France, and other societies, requesting them to appoint a number of their members to serve on the committee of organisation. The aeronauts are not pleased because no invitation has been directed to any of the aeronautical societies of Paris.

NOTWITHSTANDING that M. Bardoux has published his decree on the meteorological organisation of France the great commission has held no sitting, and the list of presentations for the successor to M. Leverrier has not yet been deliberated upon. This singular delay is preventing the government from taking any step towards the realisation of the newly-established meteorological institution.

THE "Associations" are waking up once more and beginning to warn all whom it may concern to be ready for the great annual autumnal talk. So far as the British Association is concerned it has confined itself as yet to the usual preliminary advertisement and the private invitation circular intimating that this will be an Irish year—and no doubt Dublin will give a thoroughly Irish welcome—that the opening day is August 14, and that Dr. W. Spottiswoode is the President-Elect. Our American friends in this, as in some other things, are ahead of us, for already we have received the printed circular of arrangements for the twenty-seventh meeting of their Association, which is to be opened at St. Louis a week after our own, on August 21, with Prof. O. C. Marsh as President. Any English men of science who are likely to be in the States about the time of the meeting and who would like to be present, should write to Prof. F. W. Putnam, Salem, Mass., the Permanent Secretary, who, we are sure, will gladly give all necessary information.

IT is stated on good authority that the Water and Woods Department of the French Exhibition will be preserved as a permanent museum, and re-erected in the Bois de Boulogne after the close of the Exhibition. All the specimens of woods exhibited by foreign nations will be purchased if not presented by their respective departments, and added to the intended museum. All the parts of the central building have been constructed so that they can be utilised for railway stations, large markets, &c., and will be sold accordingly. It is stated that the greater part has been already disposed of. We see that parliament has also authorised the purchase of suitable apparatus for the Conservatoire des Arts et Métiers. Although intended originally only to last up to October it is pretty certain the Exhibition will not close before November, according to the universal wish expressed by exhibitors and public.

WITH the month of June will be commenced the visits to the Exhibition of the pupils of the several municipal schools of Paris, under the guidance of their teachers, with free tickets given by the Government in accordance with the votes of the Chamber of Deputies.

A SINGULAR blunder for a city that has established an official committee on lightning conductors has been committed by the architect of the Paris Exhibition; iron conductors have been placed on the central building which is in solid iron. This extraordinary error was also committed in 1867. It shows how little the principles enunciated by Franklin are understood even by scientific people.

GIFFARD'S captive balloon is almost ready; the two steam-engines, 150 horse-power each, for working the monster cylinder, will be tried next week. The cylinder, which weighs 49,000 kilograms, has a length of 12 m. and a diameter of 175 ctm.; it will revolve with a velocity of 30 turns per minute. The exact weight of the rope is 1,950 kilos. for a length of 600 m. It has been made at Angers in less than a week, and will be tried within a few days. Next week the apparatus for manufacturing hydrogen gas at a rate of 1,000 cubic m. per hour will be completed. The varnishing of the monster balloon began on Monday; the preliminary ascents and police inspection will take place from June 10 to 20, and the balloon is expected to be opened on the 22nd. The expense will probably exceed £20,000.

THE portrait of Harvey, which we give this week, properly belongs to the previous volume, and ought to be bound along with it. Prof. Huxley's notice of Harvey will be found at p. 417 of that volume.

THE *Sydney Mail* of March 30, we learn from *The Colonies*, contains a letter from a Mr. Severn, dated Newcastle (New South Wales), March 24, in which he gives details of a singular discovery he has made, whereby deaf people can be made to hear by means of the telephone. After describing a very simple telephone which he constructed out of a tin pot, the closed end of which he opened and tied over it a piece of parchment, passing a fine string through the centre and making a knot inside, he says:—"Make a loop in the string some three feet long, put this loop over the forehead of the listener (the deaf man), cause him to place the palms of his hands flat and hard against the ears, let the loop pass over the hands, and now this listener will hear the smallest whisper, let him be deaf or not. This fact may appear extraordinary; it is, nevertheless, true that a deaf man may thus be made to hear the voice, music," &c. A diagram is published in the *Mail*, showing the working of the telephone as described.

THE cultivation of the opium poppy (*Papaver somniferum*), which has hitherto been exclusively confined to the east, bids fair to become thoroughly established and remunerative in Eastern Africa. Seeds of the best kinds have been imported from Malwa into Mozambique, where 50,000 acres of uncultivated State land have been granted to a company, with a capital of 178,000*l.*, for the purpose of cultivating and trading in opium. Besides the grant of land, the company also receives from the State "the exclusive right for twelve years to export opium free of duty through all the custom-houses of the province." It is satisfactory to learn that the poppy plants are thriving, and the fruits are reported to be larger than those produced in the best opium districts of India.

A RUSSIAN medical paper draws attention to *Sarracenia purpurea* as a remedy for gout, administered in the form of a powder in the proportion of one or two teaspoonfuls morning and evening.

THE thirty-second meeting of German philologists and pedagogues will take place at Gera at the end of September.

HERR FERDINAND NOLL, of Brandenburg, has presented to the International Postal Congress, now sitting at Paris, the drawings and descriptive plans of a decimal clock as well as two models of the clock itself. Its object, as its name implies is to introduce a division of time on the decimal system in accordance with that already in use for measures, weights, and moneys. Herr Noll therefore divides his dials into twenty hours and gives 100 minutes to the hour, each minute having fifty seconds, and each second fifty "tertien." Dr. Forster, the Director of the Berlin Observatory, gave a very favourable opinion of the invention when submitted to him two or three years since.

A FRENCH agricultural paper announces the discovery of an extremely simple and cheap means to protect houses from being struck by lightning. This consists merely in bundles of straw attached to sticks or broom-handles and placed on the roofs of houses in an upright position. The first trials of this simple apparatus were made at Tarbes (Hautes Pyrenées) by some intelligent agriculturists, and the results were so satisfactory that soon afterwards eighteen communes of the Tarbes district provided all their houses with these bundles of straw, and there have been no accidents from lightning since in the district.

THE Emperor of Austria has conferred the Cross of the Order of Francis Joseph upon the two well-known African travellers, Drs. Georg Schweinfurth and Gerhard Rohlfs.

THE large botanical library left by the late Prof. A. Braun, formerly director of the Berlin Botanical Gardens, is now being sold by Messrs. List and Francke, of Leipzig.

OUR readers will be glad to learn that Sir William Thomson and Prof. Tait have nearly completed for publication the first part of the new edition of their work on natural philosophy, which will be brought out very shortly by the Cambridge University Press.

NO less than twelve separate subterranean shocks are reported from Ancona as having occurred between May 9 and 12.

THE Government of Uruguay intends to construct a railway which will unite Uruguay with the Province of Rio Grande do Sul, in which many thousands of colonists are settled. The line is to begin on the right bank of the Quarahim River, and is to extend as far as the town of Uruguayana. On the Quarahim River this railway will join the line in course of construction between Salto and Santa Rosa, which is already finished and in use as far as Jacuhi (some 300 miles), and which in turn corresponds with the line between Salto and Fray Bentos, where the great Saladeros (slaughter-houses) of the "Liebig Company" are situated, at which over 1,000 head of cattle are killed daily to make the well-known "Liebig Extract of Meat."

MR. J. M. WILSON, of Rugby, has in the press a treatise on geometry written to correspond with the Syllabus of the Geometrical Association. The work will be published by Messrs. Macmillan and Co.

MESSRS. MACMILLAN AND CO. are preparing for publication a treatise on the nature and origin of coal and the extent of the supply in this country, written by the Professors of the Yorkshire College of Science, Leeds. The authors propose to sketch out the state of the country at the time when coal was coming into being and the processes by which it was formed; next to deal with the present, and give an account of the methods of working coal and some of the uses to which it is now being put; lastly, to endeavour to forecast the future and speak of the probable duration of our coal supply. The work will be edited by Prof. T. E. Thorpe, F.R.S. In it Prof. Rücker will treat the subject from the physicist's point of view; Prof. Miall will discuss its natural history; Prof. Green will take the geology of the question; Prof. Thorpe the chemistry; and Prof. Marshall will write on the economics of coal.

IN a recent paper in *L'Aeronaute*, Col. Laussedat gives the results of experiments made by a Commission appointed by the French Minister of War. For Captive Balloons it is absolutely necessary to employ the best silk and cordage, which, with the least weight, offers the greatest guarantee of durability. After much research a special varnish has been found which renders the aërostat impermeable to gas. Instead of numerous ropes held by men as in former military ballooning, a single cable has been adopted to work by a simple but secure capstan. Capt. Renard has discovered a rapid and economical new process of manufacturing hydrogen. For Postal Balloons Capt. Renard also has devised a secure and easily-worked valve. A liquid instead of a solid ballast has been resolved on, and a fluid is being sought which will not congeal in the low temperatures of the upper atmosphere. The valve and the ballast may work automatically and maintain the balloon at any given height. Among the methods of stopping the balloon experimented on are the javelin anchor of Meusnier and a sort of iron arrow devised by Capt. De la Haye. For Directable Balloons the principles which guided Dupuy de Lôme have, for the most part, been adopted by the Commission. That experimenter found that with an engine of eight horse-power turning a screw he could deviate from the direction of the wind by a considerable angle, with ordinary winds, and even travel, with reference to the earth, in all directions which it would be wished to follow. The Commission, however, instead of placing the screw in the car, at a great distance from the point of application of resistance of the air, have constructed the balloon so that the screw may work in the very centre of the aërostat.

THOSE familiar with the treasures of the Bibliothèque Nationale in Paris will appreciate the importance of a law lately laid before the French Chamber by the Minister of Public Instruction, providing for the demolition of all buildings adjoining the library, in order to insure its complete protection from danger by fire. The great building will in the future be entirely surrounded by an open space laid out in gardens and walks.

THE Paris Jardin d'Acclimatation has just received from the Seychelles Islands three of the largest tortoises known. The heaviest weighs 187 kilogrammes and is 1⅛ metre in diameter.

AT the meeting of the Institution of Surveyors on the 13th May, Mr. R. W. Peregrine Birch read an important paper on the use of sewage by farmers. Mr. Birch has collected a considerable quantity of statistics on this unsavoury but important subject, from which the following conclusions are drawn :—1st. That there are upwards of 100 owners and occupiers of land in Great Britain who use sewage for the sake alone of what they can get out of it by agricultural means. 2nd. That of this number more than sixty are tenant farmers who continue to use it although they have, annually at least, the option of ceasing to do so. 3rd. That of the latter number about five-sixths, and of the total number about three-fourths, actually pay money for the use of the sewage, either in the form of out-fall rent, unquestionable increase of land rent, or the price of occasional dressings. Nearly 4,000 acres of sewage land have been referred to, and these are in the hands of more than a hundred distinct occupiers. These occupiers may be divided into three classes :—Those who have to cleanse a certain quantity of sewage on a certain area of land ; those who may take, or leave alone, as much of a town's sewage as they please ; and those who may take, or leave alone, what sewage can be spared by others having a prior right. The first class occupies 1,670 acres of sewaged land, and deals with the sewage of twenty distinct sanitary districts, or a population of about 200,000 on as many as twenty-one different farms. Mr. Birch's paper will be published as a pamphlet by Messrs. Spon.

AMONG the novelties in the German book trade for May, we notice the following scientific works :—"Teleologie und Darwinismus," Dr. Kalischer (Berlin) ; "Gedanken über die Teleologie in der Natur," v. Bärenbach (Berlin) ; "Reisebriefe aus Kordofan und Dar-Fur," Dr. F. Pfund (Hamburg) ; "Die allgemeinsten chemischen Formeln," Prof. C. Willgerodt (Heidelberg) ; "Der Sternhaufen χ Persei, beobachtet in der Leipziger Sternwarte von 1867–70," H. C. Vogel (Leipzig) ; "Die Verbreitung der Atmosphäre," M. Thiesen (Berlin) ; "Aus der Physik des Luftmeers," G. Münter (Herford) ; "Praxis der Naturgeschichte botanische, zoologische, und Akklimatisations-gärten, Aquarien, &c.," P. L. Martin (Weimar) ; "Atlas cœlestis eclipticus viii.," E. Heis (Cologne) ; "Die Fauna des Graptolithen-Gesteines," K. Haupt (Görlitz) ; "Bericht über die Beobachtung des Venus-Durchgangs vom 8ten December in Luxor," A. Auwers (Berlin) ; "Theorie der Wärme," translated from Prof. J. C. Maxwell by F. Neesen ; "Das Nervensystem &c., der Medusen," O. and R. Hertwig (Leipzig) ; "Journal des Museums Godeffroy—A. Garret's Fische der Südsee" (Hamburg) ; "Fungi italici authographice delineati," P. A. Saccardo (Berlin). The three last are very expensive works.

THE additions to the Zoological Society's Gardens during the past week include three Common Rheas (*Rhea americana*) from South America, presented by Mr. Frank Parish ; four Water Ouzels (*Cinclus aquaticus*), British, presented by Mr. R. J. L. Price ; a Hairy Tapir (*Tapirus roulini*) from Columbia, two Great-Billed Rheas (*Rhea macrorhyncha*), two Sulphury Tyrants (*Pitangus sulphuratus*) from South America, received in exchange ; two Chimpanzees (*Troglodytes niger*) from West Africa, deposited ; two Bar-headed Geese (*Anser indicus*) from India, purchased ; a Great Kangaroo (*Macropus giganteus*), two Wild Boars (*Sus scrofa*), two Wild Cats (*Felis catus*), born in the Gardens ; two Geoffroy's Doves (*Peristera geoffroii*), seven Chilian Pintails (*Dafila spinicauda*), a Yellow-Legged Herring Gull (*Larus leucophæus*), bred in the Gardens.

THE FRENCH METEOROLOGICAL SERVICE

WE learn that M. Mascart has been appointed head of the meteorological bureau. He is professor in the Collége de France, his special subjects being light and electricity. He is author of a work in two volumes, on static electricity.

Last week we gave a brief sketch of the new organisation of the French meteorological service by the government, and this week we are able to publish a translation of the decree, from which it will be seen how much alive the French government is to the national importance of a complete meteorological service. How Article 2, referring to "Titular Meteorologists," "Adjoint Meteorologists," and "Assistant Meteorologists," must surprise our "Meteorological" Council! In France they actually insist upon meteorologists to do meteorological work and to advise upon meteorological matters.

Article 1.—The meteorological division of the Paris Observatory forms a distinct service, which takes the title of "Bureau Central Météorologique." This service comprises the study of the movements of the atmosphere, meteorological advertisements to the ports and to agriculture, the organisation of the meteorological observations, and of the regional or departmental commissions, the publication of their works, and the whole of the researches on meteorology or on climatology.

2. The meteorological service of France comprises titular meteorologists, *adjoint* meteorologists, and assistant meteorologists. The salary of the titular meteorologists varies from 3,000 to 10,000 francs. The *adjoint* meteorologists are divided into three classes, whose salaries vary from 2,500 to 5,000 francs. The assistant meteorologists are divided into two classes, whose salaries vary from 1,500 to 2,000 francs. This staff is distributed among the central bureau and the regional or departmental observatories, in proportion to the needs of these establishments.

3. The scientific staff of the central bureau comprises a titular meteorologist acting as director, two titular meteorologists placed under him, *adjoint* meteorologists, and assistant meteorologists. One of the *adjoint* or assistant meteorologists acts as secretary of the central bureau.

4. The director is charged with the general service of the establishment, the correspondence, the presentation to the minister of the proposed annual budget, the meteorological service, and a detailed account of the yearly expenses. He ought to secure the co-ordination and execution of the works which demand the concurrence of the different services placed under his orders, and see to the regularity of the publications. No order may be given without his authorisation.

5. The scientific works are divided as follows :—(1) Service of advertisements to the ports and to agriculture. (2) Service of the general movements of the atmosphere. (3) Service of climatology and of inspections. Each of the chiefs of the service remits monthly to the director a summary report on the progress of the works, and brings directly before the committee, instituted in the following article, the scientific questions of the service.

6. The heads of the service meet each month, on a fixed day, under the presidency of the director. This committee may hold extraordinary meetings at the instance of the director.

7. The titular meteorologists and the director are nominated by decree, on the proposition of the minister, and after the advice of the council, to be spoken of afterwards. The *adjoint* and assistant meteorologists are appointed by orders after advice of the same council.

8. The heads of the regional meteorological observatories are placed under the authority of the director of the central bureau. Each of these officials addresses to the central bureau, under cover of the minister, the observations and works of his establishment. He proposes to the council, through the director of the central bureau, the advancement of the meteorologists under his orders.

9. The meteorological observatories and stations of every order will be visited annually by the meteorologist of the central bureau charged with the service of climatology and inspections. They may also be visited by the director of the bureau or by a member of the council appointed for that purpose. In cases where the departments or towns contribute to the expenses of a meteorological observatory, the inspection will take place in company with the delegate of the general or municipal council interested.

10. There is established beside the central meteorological bureau, a council composed of (1) A representative of each of the Ministries of Agriculture and Commerce, of Public Works, of War, Marine, Foreign and Home Affairs and of the Administration of Telegraph Lines ; (2) Two delegates from the Ministry of Public Instruction ; (3) Two members of the Academy of Sciences ; (4) The director of the central bureau. The heads of the special service of the bureau are admitted to the council, with a consultative voice for questions which interest them. The members of the council are appointed for three years, by decree, on the proposal of the Ministry of Public Instruction.

11. The council of the central bureau will meet once every quarter on a fixed day. It may hold extraordinary meetings at the instance of the minister. The council gives its advice in the budget proposed by the director, on the construction of buildings or instruments intended for regional meteorological observations, on the collective investigations carried on in the various establishments, on the nominations and promotions of the officials, &c.

12. The president, vice-president, and the secretary of the council are appointed annually by the minister on the proposal of the council.

13. The council holds a general meeting yearly at which may be present the heads of the central bureau and of the regional observatories, the delegates of the regional and departmental commissions, and three delegates of the French Meteorological Society.

A regulation deliberated in council and approved by the minister will determine the mode and number of the delegations.

This meeting will hear the report of the president and council on the work of the year, and, if there are any, the reports and memoirs of the heads of the observatories that receive subventions, and those of the delegates of the regional or departmental commissions. It will discuss the views submitted to it, and transmit them to the minister. The report of the president will be printed.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

A TOWN meeting was held at Liverpool on Friday, the Mayor presiding, with the view of establishing a college for higher education, so as to qualify for degrees in art, science, and other subjects at any of the universities.

LEYDEN.—The university shows an attendance at present of 823 students, divided among the faculties as follows:—Law, 487; theology, 41; medicine, 184; philology, 58; science, 53. The corps of professors numbers 47.

AGRAM.—This young university is attended at present by 348 students, of whom but four are from countries outside Austria.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 16.—"Experimental Researches on the Electric Discharge with the Chloride of Silver Battery. Part II. The Discharge in Exhausted Tubes." By Warren De La Rue, M.A., D.C.L., F.R.S., and Hugo W. Müller, Ph.D., F.R.S.

"Note on Legendre's Coefficients." By I. Todhunter, F.R.S.

May 23.—"Observations on Arctic Sea-water and Ice." By Dr. Moss.—The paper consists of physical and chemical observations made during the Expedition of 1875-76 on polar ice and sea-water, and is accompanied by a tabulated statement of chlorine and specific gravity estimations; the latter made by the method devised by Mr. Buchanan of the *Challenger*. The author remarks that the low specific gravity of the Polar Sea (1·02467) indicates that even the deepest samples obtained had already received the dilution characteristic of outflowing polar currents. This low specific gravity was maintained through the winter. The highest temperature observed in the deep stratum of dense warm water in Smith's Sound was below 32°, but since its specific gravity was above that of Atlantic water, the northward flowing current may have a slightly higher temperature at a greater depth. The disturbed proportion of sulphates to chlorides in polar waters is attributed to the littoral source of their dilution and to the difference in the behaviour of the sulphuric and chloric cryohydrates (rather than to absence of fucoidal plants or volcanic influence, as suggested by Forchhammer). A detailed description is given of a *névé*-like stratification in the polar ice, proving, in the author's opinion, that the stupendous floes met with by the recent, and many other expeditions, are due, not to progressive freezings of sea-water, or to the sliding up of thinner ice-fields, but to a perennial accumulation of polar precipitation. The stratification includes and overlies air-carried *débris* of crystalline rocks, chiefly quartz, augite, and magnetite.

The strata are often built upon a conglomerate formation (including salt-water Diatomaceæ) affording evidence of a lateral extension of the floating glacier (by the freezing together of fragments in fissures). The "blue-domed" floes belong to the outer zones of the polar ice-cap, where waste exceeds precipitation. Their undulating surfaces intersecting the horizontal stratification and pitted with the ice-dust left from the layers above, are the surface signs of the decay which finally restores polar precipitation to the ocean in the shape of the increased dilution of outflowing polar currents.

May 25.—"On the Equations of Circles." (Second Memoir.) By John Casey, LL.D., F.R.S., M.R.I.A., Professor of Mathematics in the Catholic University of Ireland.

"Contributions to the Anatomy of the Central Nervous System in Vertebrate Animals. Part I. Ichthyopsida. Section 1. Pisces. Subsection 1. Teleostei." By Alfred Sanders, M.R.C.S. Communicated by Prof. Huxley, Sec.R.S.

Zoological Society, May 7.—F. D. Godman, F.Z.S., in the chair.—Mr. T. J. Parker read some notes on the stridulating organ of *Palinurus vulgaris* which had first been described by Dr. K. Möbius, but on whose observations Mr. Parker offered several criticisms.—A communication was read from Dr. F. Buchanan White, entitled "Contributions to a Knowledge of the Hemipterous Fauna of St. Helena, and Speculations on its Origin." In the first part of his paper the author, after briefly noticing what was known with regard to the fauna and flora of that remote and interesting oceanic island, and mentioning the various theories that had been brought forward to account for their origin, discussed the difficulties of the animals, and argued that they had evidently been derived at a remote period from the Palæarctic region by way of Madeira, the Canaries, and the Cape de Verde Archipelago. In the second part of his communication Dr. F. B. White described the Hemiptera collected in St. Helena by the late Mr. T. V. Wollaston, during the recent visit of that lamented naturalist to the island. The collection included thirty species, of which five were probably introduced; one appeared to be indigenous, but seemed identical with a European species, and the remaining twenty-four were regarded by the author as new and peculiar to the island. Seven new genera and one new sub-genus were created for the reception of ten of the species, the rest, with one exception, being referred to European genera. Specimens and drawings of details were exhibited in illustration of the paper.—Mr. P. L. Sclater, F.R.S., read some further remarks on *Fuligula nationi*, a species of duck from Western Peru, of which he had lately received a nearly adult male from Prof. Nation, the discoverer of the species.—Mr. A. G. Butler, F.Z.S., read the descriptions of a small collection of lepidoptera made at Kingston, Jamaica, by Mr. James J. Bowry.—Mr. Edgar A. Smith, F.Z.S., read a paper containing the description of three new land shells from Jamaica and Borneo.—A communication was read from Mr. D. G. Elliot, F.Z.S., containing a memoir on the fruit pigeons of the genus *Ptilopus*. Mr. Elliot recognised seventy-one species of this genus.

Meteorological Society, May 17.—Mr. C. Greaves, F.G.S., president, in the chair.—A. H. J. Crespi, B.A., M.R.C.S., Rev. David Lamplugh, William Morris, M. Inst. C.E., James Muir, M. Inst. C.E., and Miss E. A. Dymond, were elected Fellows of the Society.—The following papers were read:—On the daily inequality of the barometer, by W. Rundell, F.M.S.—Meteorology of Mozufferpore, Tirhoot, for the year 1877, by C. N. Pearson, F.M.S.—Note on the great rainfall of April 10–11, as recorded at the Royal Observatory, Greenwich, by William Ellis, F.R.A.S.—Observations of Sea Temperature at slight depths, by Capt. W. F. Caborne, F.M.S.

Anthropological Institute, April 30th.—Major-General A. Lane Fox, F.R.S., vice-president, in the chair.—Mr. Francis Galton, F.R.S., read a paper on composite portraits, made by combining those of various persons into a single resultant figure (NATURE, p. 97).—The Director read a paper by Mr. C. Staniland Wake on the origin of the classificatory system of relationships used among primitive people. After criticising Mr. Morgan's explanation of the classificatory system as having originated in the practice of marriage among consanguinei, Mr. Wake proceeded to show that the social condition of the Polynesian peoples, who possessed the simplest form of that system was inconsistent with the origin assigned to it by Mr. Morgan. The author of the paper then showed, by an examination of various phases of the classificatory system, especially the Australian, that, although kinship may for certain purposes have been originally traced through the mother, the regulations as to marriage were based also on the relationship of a father to his child, and that the ideas which gave rise to those regulations also originated the classificatory system.—Mr. A. L. Lewis described a rude stone monument, known as the "Devil's Arrows," near Boroughbridge, Yorkshire.

Geologists' Association, May 3.—Prof. J. Morris, F.G.S., president, in the chair.—On the coralline oolites, &c., of Yorkshire, by W. H. Hudleston, M.A., F.G.S. The beds between the Kimmeridge clay and L.C.G. may be summarised as follows:—

		Feet.
Supracoralline	Upper Calcareous Grit: "Throstler," Cement Stone, &c.	6–36
Zone of *A. plicatilis*	Coral Rag: Subzone of *Cidaris florigemma*	8–40
	Coralline, Oolite and Middle Grit.	30–80
Upper part of zone of *A. perarmatus* ..	Lower Limestones, including Lower Coral Rag, Passage-beds, &c. ...	20–120

The great mass of limestone in the zone of *A. perarmatus* is an exceptional feature as regards England: fauna Oxfordian, especially towards the base, but the highest shell-bed in some places may be classed with the succeeding zone. The middle grit, a sandbank of variable thickness, developed in the Tabular range, and passing upwards through a series of shelly sub-oolitic grits (*e.g.* Pickering *Trigonia*-beds) into the coralline oolite, the whole constituting the lower subdivision of the zone of *A. plicatilis*; absence of Brachiopoda. The topographical sections described, and the Howardian oolites compared with those of the Tabular range: lithology of the group.—The coral rag sometimes sharply separated from underlying oolites; sometimes, though more rarely, blended by coralliferous oolites, coral shell-beds, &c. Ammonites rare, but where found in the intercoralline pastes, a peculiar form of *A. plicatilis* (? *A. varicostatus*, Buckl.) prevails. Topographical sections described, and the strong contrast between the coralline oolite of Pickering and the coral rag of North Grimston pointed out. Observations on the silicification of the Rag, and the frequent formation of flints; presumed connection in certain cases, with abundance of sponge spicules, especially "globo-stellates." The supracoralline beds principally argillaceous limestones, and sandy shales, with a capping of upper calcareous grit; numerous ammonites of a lower Kimmeridge type, but other fossils few and badly preserved. A slight sketch of the stratigraphy of the region surrounding the Vale of Pickering, and a brief notice of the palæontology of the entire corallian series in Yorkshire, explanatory of the table of fossils, concluded the paper.

Institution of Civil Engineers, May 21.—Mr. W. H. Barlow, F.R.S., vice-president, in the chair.—The paper read was on the design generally of iron bridges of very large spans for railway traffic, by Mr. T. C. Clarke, M.Inst.C.E., of Philadelphia.

PARIS

Academy of Sciences, May 20.—M. Fizeau in the chair.—The following among other papers were read:—On the temperature of the air at the surface of the ground and in the ground to 36 m. depth, also the comparative temperature of turf-covered and bare ground, during 1877, by MM. Becquerel. The temperature was a little higher on an average in the turf-covered than in the bare ground, and in the former it never sank below zero. In the latter, at 0·05 m., it only once sank below the temperature of melting ice.—On the action of the nervous system on the sudoriferous glands, by M. Vulpian. These glands seem (like the salivary glands) to be subject to two antagonistic influences exerted by different nerve-fibres; the one class, which conduct the exciting influence, nearly all come directly from the bulbo-medullary centres; the other class, which conduct the moderating influence, also emanate from these centres, but indirectly, through the great sympathetic.—A letter from Prof. Hughes stated that by inserting an induction-coil in the circuit, in his arrangement, the Bell telephone could be heard over a large hall. His system was a very sensitive thermoscope, &c.—Application of the telephone on board the cruiser *La Desaix*, by M. Trève. This ship communicated very successfully with another in tow by means of the telephone, the circuit being completed by the sea and copper sheathing. Another application was, fitting a telephone to the head of divers' apparatus.—On alloys of gallium and aluminium, by M. Lecoq de Boisbaudran. One such alloy (containing a good deal of aluminium) is solid but brittle; it decomposes water, with rise of temperature, liberation of hydrogen, and formation of a brown powder, later resolved into white flocks of alumina; nearly all the gallium is liberated in the form of globules. Liquid or pasty alloys may also be formed, with much greater decomposing power.—Production of liquid and gaseous carbonised hydrogens by the action of pure water on a carburetted alloy of iron and manganese, by M. Cloez. Water alone, acting with heat on such an alloy, yields its oxygen to the metal, forming, first, protoxides, which afterwards, by action of air, pass to a higher degree of oxidation. A part of the hydrogen enters into the free state; the rest combines with the carbon to produce hydrocarbons similar to petroleum.—On an induction machine, by M. Gaiffe. To obtain a current as constant as possible he employs a bobbin of elliptical section instead of circular (Siemens); the change of polarity is thus effected gradually during the whole of a half-revolution.—On an allotropic modification of copper, by M. Schützenberger. This is got by electrolysis of a solution of about 10 per cent. of acetate of copper (previously boiled), with two Bunsen or three Daniell elements, the negative platinum plate being placed parallel to the larger positive copper electrode, and 3 to 4 ctm. from it. The allotropic copper is then deposited on the platinum, as brittle, rugous metallic plates, of bronze aspect. The specific gravity is 8·0 to 8·2; that of ordinary copper is 6·9. The moist plates quickly oxidise at the surface in air. Allotropic copper is changed to ordinary copper by heat, and prolonged contact with dilute sulphuric acid.—On a new synthetic method of formation of carbides of hydrogen, by M. Randolph. This is by causing a substance like fluoborethylene to act on oxygenated organic compounds capable of furnishing given carbides by dehydration.—On pelletierine, an alkaloid from the bark of the pomegranate tree, by M. Tanret. This volatile alkaloid is thought to explain the tænifuge properties of pomegranate bark in the fresh state.—On the distinction between luminous and chromatic sensations, by M. Charpentier. The luminous sensibility may change under certain conditions (rest in darkness and exposure to bright rays), while the sensibility to colours remains constant.—On the existence of reflex trembling, in the unparalysed member, in certain hemiplegic subjects, by M. Dejerine. This may be produced, *e.g.*, by flexion of the foot on the leg.—On the terminations of nerves in the sudoriparous glands of the cat's paw, by M. Coyne. The glandular *cul-de-sac* is brought into relation with the peripheric nerve system (1) by nerve tubes losing themselves in the limiting membrane; (2) by cells similar to multipolar nerve-cells situated outside the limiting membrane.—On the unity of forces in geology, by M. Hermite.

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