THURSDAY, JANUARY 30, 1879

THE ART OF SCIENTIFIC DISCOVERY

The Art of Scientific Discovery, or the General Conditions and Methods of Research in Physics and Chemistry. By G. Gore. LL.D., F.R.S. (London: Longmans, Green and Co., 1878.)

IT is not easy to say when scientific research, using the expression in its strictest sense, was first commenced. M. Libri remarks: "Les recherches des Pythagoriciens sur les vibrations des corps, sont les plus anciennes expériences de physique qui soient parvenues jusqu'à nous." Archimedes must certainly be credited with some knowledge of research; and to a lesser extent Ptolemy the astronomer, and Hero, of Alexandria. But, as a matter of fact, experimental researches in physics were not made before the epoch of Galileo, nor in chemistry before the epoch of Lavoisier. The discovery of new methods of mathematical analysis on the one hand, and the invention of instruments of precision on the other, were necessary forerunners of the development of research. Moreover, the advocacy of the abandonment of that blind reverence for authority which had retarded the progress of the sciences for many centuries, tended in the same direction. In this respect, whatever we may say of Campanella, Nizolius, Telesius, and others, our own Francis Bacon did more true service than any of his predecessors; and we must always regard his writings as the most potent engine concerned in the overthrow of Aristotelianism, Scholasticism, and the method of pure logic, and in the substitution of the experimental method blended with just logical induction and deduction.

Mr. Gore, whose own devotion to experimental research well entitles him to act as an interpreter of the art of scientific discovery, has in the course of sixty chapters of condensed matter discussed the various lines of thought and of action which converge towards that bright central focus in which new truths lie hidden. His object has been to describe the nature, the methods, and the conditions of success of original scientific research; to point out the causes of failure, the mental and manual discipline by which they may be overcome; and the special modus of thought by which we may hope to ascend from the known to the unknown.

With this object in view he has divided the work into five parts, the first of which contains a general view of the subject:—the nature of scientific ideas, terms, and beliefs, the criteria of scientific truth, and the great principles of science. In the second part he has discussed the general conditions of scientific research:—the starting points, chronological order of discovery, importance of qualitative knowledge, and necessity of classification. The third part is devoted to the personal preparation for research; the fourth to the actual working in original research; and the fifth to special methods of discovery. This latter is divided into ten parts, which treat respectively of discovery:—

1. By extending undeveloped or neglected parts of science.
2. By the use of new or improved instruments.
3. By the investigation of likely circumstances.
4. By devising hypotheses and questions, and testing them.
5. By means of new experiments and methods of working.
6. By means of additional, new, or improved observations.
7. By classifying and comparing known truths.
8. By means of study and inference.
9. By means of new or improved methods of intellectual operation.
10. By means of calculations based upon known truths.

In the discussion of these subjects, the history of various scientific discoveries is traced, and we are not only brought into contact with the investigator's particular train of thought throughout all the steps which led up to the discovery, but we are often taken into the minuter labyrinths and shown the many collateral ideas which were evoked during the course of the research. The influence of previous discoveries upon the main subject at issue is also developed, so that we gain important information regarding the history of the sciences, while at the same time we are becoming acquainted with the art of original research.

In that portion of the work which relates to method, we are not surprised to find that the author has often quoted Lord Bacon. In fact, Mr. Gore's style is sometimes thoroughly Baconian. So penetrated is he by the spirit of the "Novum Organum," that he sometimes unconsciously embodies its aphorisms with his own; for example, when he says: "Science is the interpretation of nature, and man is the interpreter. Original research is the chief source of new scientific knowledge." His work may almost be called a nineteenth century continuation of the second book of the "Novum Organum"—a sort of *newest* organum. He also quotes pretty frequently the "History of the Inductive Sciences," and sometimes the "Novum Organum Renovatum," with which the "Art of Scientific Discovery" has many points of contact. We are surprised to find Descartes so rarely alluded to, albeit portions of the work relating to Method are thoroughly Cartesian in spirit. Here, for example, is an excerpt from the *Regulæ ad Directionem Ingenii*, which we recommend to Mr. Gore's notice for the second edition:—"By method I understand rules certain and easy, such as to prevent any one, who shall have accurately observed them, from ever assuming what is false for what is true, and by which with no effort of mind uselessly consumed, but always by degrees increasing science, a person will arrive at a true knowledge of all those things which he will be capable of knowing." Also we commend to his notice the answer to *Quid sit Cogitatio* ("Principia," Pars 1, ix.); and to that very notable assertion ("Principia," Pars 2, xxiii.), *Omnem materiæ variationem, sive omnem ejus formarum diversitatem pendere à motu.*

Early in the work Mr. Gore points out a fact which we too seldom recognise. "Original research," he writes, "is not a science; it is not a collection of laws. It is an art, because it is composed of rules which must be followed. It is the method of finding new truths by means of study, observation, travel, or other means." Now although we think that an investigator must be born and cannot be made, and that no one can frame his methods upon hard

and fast lines of thought or operation, we are quite of opinion that those who make original researches, or are about to make them, may gain much from orderly methods of manipulation, and a knowledge of the right application of logical inference. Bacon attempted to describe such methods in his "Inquisitio de Forma Calidi," and failed, because at that time there did not exist a sufficient basis upon which to found an exhaustive experimental treatment of the subject.

Two interesting chapters in the first part of the book are devoted, the one to the facts and propositions in science, and the other to the criteria of scientific truth. We are reminded herein of an interesting treatment of these subjects in the "Philosophie Méthodique" of M. de Strada, to which we venture to refer our author.

Among the conditions of success in research Mr. Gore very justly enumerates enthusiasm. Of this he quotes several examples. Becher, of Phlogiston fame, after speaking of the chemists as "a strange class of mortals impelled by an almost insane impulse to seek their pleasure among smoke and vapour, soot and flame, poisons and poverty," adds: "Yet among all these evils I seem to myself to live so sweetly, that may I die if I would change places with the Persian King!" The fascination of original research is undoubted, the enthusiasm which it sometimes inspires is unbounded. We remember an instance of a schoolboy who seriously proposed staying at school for several days at the beginning of the Christmas holidays, when "home, sweet home," is doubly sweet, in order to continue a research. And truly, were it not for the enthusiasm which it engenders, the amount of original work done in the world would be much less than it is, seeing that it is usually accompanied by numberless vexations and disappointments, and that it requires unwearied application and perseverance, joined to the possession of an undaunted spirit.

Of Mr. Gore's work as a whole we may say that it exhibits great industry in the collection of facts and a considerable amount of logical acumen in their discussion. Perhaps, however, the arrangement might be simplified. The mass of matter to be digested is so great that any increased modes of classification of the subjects that could be adopted would add to the value of the book. This could best be effected by numbering the paragraphs; by adding marginal references giving the gist of each paragraph, and by making some of the chapters more aphoristic in character. These changes could be easily effected in a second edition.

G. F. RODWELL

LEISURE-TIME STUDIES

Leisure-Time Studies; chiefly Biological; a Series of Essays and Lectures. By Andrew Wilson, Ph.D., F.R.P.S.E., &c. With numerous Illustrations. (London: Chatto and Windus, 1879.)

THIS volume of Essays and Addresses does not profess to contain anything new, either in the way of observation or theory. Neither is the author's style sufficiently brilliant, or his treatment of the subjects sufficiently original to raise them much above the level of the average lectures of a well-informed naturalist. They will, however, afford some useful and interesting information to the general reader, and may serve to attract attention to the question of the introduction of biology into ordinary education. This is the special subject of the first address, which, however, though somewhat lengthy and profuse, does not attempt to grapple with the difficulty of finding competent teachers of biology for *all* our schools. It is indeed suggested, that "the amount of knowledge required to pass even the primary stage of the biological subjects, in the government examinations, held under the auspices of the Science and Art Department," should fit its possessor for imparting elementary instruction in biology. But we greatly doubt whether the examiners would be of this opinion; and we rather think it would be a distressing sight to witness a teacher, whose whole knowledge of the subject was derived from a course of study just sufficient to enable him to pass such an examination, exposed to the questions of a lot of intelligent country boys and girls, whose practical acquaintance with native plants and animals was far more extensive and accurate than his own. If biology is to be taught in schools it must not be by the regular school-teachers qualifying themselves by a few months' training in London, but by the employment of good naturalists to give lectures, demonstrations, and out-door excursions to all the schools of a district in succession.

In the succeeding address, on "Science-culture for the Masses," too much stress is laid on the teaching of science as "a pleasant system of mental gymnastics." This seems to us altogether a wrong ground to go upon. Science is not to be taught in order to strengthen the mind to do something else by and by, but because it opens the mind to a more adequate conception of the universe in which we live, and is in itself, truly, the knowledge which is power.

The lecture on "The Sea-serpents of Science" is interesting, both as giving a very fair summary of the most recent evidence on this subject, and as showing that the age of incredulity is past, and that naturalists are now prepared to admit that several distinct kinds of oceanic monsters probably exist, of which no single specimen has yet been obtained. Recollecting, however, the number of clever hoaxes to which this subject has given rise, we think that the newspaper account at p. 104, of the declaration before a Liverpool J.P., made by the master and crew of a merchant-ship, to the effect that they had seen a huge serpent twice coiled round a sperm whale, and a similar serpent with its head raised "sixty feet perpendicularly in the air," should not have been inserted as evidence without first ascertaining that such a declaration was actually made before the magistrate named. The trouble of writing a single letter would probably have been sufficient, and would have settled the preliminary question of whether the whole story, from beginning to end, was not a pure newspaper *canard*.

The article on "The Genesis of Life" repeats the now often-told tale of the fluctuations of opinion as to spontaneous generation, and will be interesting to those who have not read it elsewhere. Dr. Wilson tries his best to be impartial, and to place before his reader the exact position of the question at the present time. He acknowledges that "isolation" and "destruction" are the two great points of all experiments on the subject, and that if

these are perfect the question can be settled. It is not denied that hermetically sealed flasks give complete isolation, the only question remaining being, to secure complete destruction of whatever organisms, with their germs, may be within the flasks at the commencement of the experiment. He refers to Dr. Bastian's experiments on the death-point of minute organisms and their germs, which was invariably found to be 158° F., and he points out no fallacy in these experiments. Yet if they are conclusive, Dr. Bastian's numerous other experiments, confirmed as they are by Dr. Burdon-Sanderson and others, demonstrate the production of living organisms from dead matter. The elaborate experiments of Prof. Tyndall are referred to as giving results directly opposed to those of Dr. Bastian; but it is not sufficiently pointed out,—firstly, that in Dr. Tyndall's experiments "isolation" was not effected in the only perfect manner by hermetical sealing, and that many contradictory results hence ensued;—and secondly, that all the results opposed to those of Dr. Bastian were negative, and could therefore not disprove the latter's positive results. Dr. Bastian in his test experiments did not use "old hay," the germs in which are said to be "indurated," but infusions of turnip and cress, and after these were subjected in sealed flasks to temperatures of 270° F., and to 230° F. for upwards of an hour, they produced living organisms of such varied types as *bacteria*, *torula*, *protamœbæ*, and *monads*. ("Evolution and Origin of Life," p. 175-180.) As *similar organisms* and their germs, *produced in similar infusions* have been proved to be killed by a temperature at least 100° lower than that employed in the above experiment, what we require to settle the question is, not thousands of quite different experiments, whose results one way or the other cannot settle the point at issue, but a repetition of the same experiments by other observers with the object of detecting the fallacy, if any, that lurks in them.

The only other article we can here refer to, is that on "The Law of Likeness and its Working," which deals with the question of heredity, and Mr. Darwin's theory of Pangenesis. But no notice is taken of Mr. Francis Galton's very important "Theory of Heredity," published in the *Journal of the Anthropological Institute*, vol. v. p. 329; which, though it may be considered as a mere modification of that of Mr. Darwin, really differs from it in many important points, and affords a more complete and satisfactory explanation of many of the most curious facts; such as the *unlikeness* of children to their parents, the appearance of diseases and even of mental qualities, in alternate generations, and many others. Every one wishing to comprehend this most difficult yet most interesting subject, should study Mr. Galton's paper as a necessary supplement to the theory of Pangenesis.

At p. 70 of Dr. Wilson's book, a letter from the *Times* is quoted, describing the formation of the bees' cell, as due entirely to the *pressure* of opposing bees in adjacent cells. This is not strictly correct; and Mr. Darwin's observations should have been refered to, showing that the cell-walls are first built very thick, and are *gnawed* down to the requisite thinness. There is also some obscurity in the suggested explanation of the "apparent movement" of the crocodile's upper jaw, when it opens its mouth. The fact appears to be that the crocodile, opening its mouth when on land, *must* raise its upper jaw and head (by bending the neck) simply because the lower jaw has not room to move downwards. The movement of the upper jaw is therefore, under these circumstances, real, and not only "apparent" as stated. One of the most interesting chapters is that on "Animals and their Environments," in which an account is given of the curious changes during the growth of flat fishes, and the still more remarkable phenomena which have been recently observed in the metamorphoses of the axolotl, and the alpine salamander.

A. R. W.

OUR BOOK SHELF

An American Geological Railway Guide, giving the Geological Formation at every Railway Station; with Notes on interesting Places on the Routes, and a Description of each of the Formations. By James Macfarlane, Ph.D. (New York: Appleton and Co., 1879.)

DR. MACFARLANE has added a new pleasure to railway travelling, or rather, by means of this geological guide-book, he has done much to make it both enjoyable and instructive. The idea of the book is excellent, and the plan seems to us thoroughly satisfactory. Now that we have this manageable little flexible book before us, it seems strange that such a guide has not been thought of before, but perhaps not so strange that our enterprising friends on the other side should set an example to their Old-World brethren. We believe the prospectus of some such guide was issued in this country several months ago; if so, we recommend its compilers to obtain a copy of Dr. Macfarlane's book, and take several leaves out of it. If no such work is being prepared for the forlorn traveller of our islands, we advise some of our Survey-men to lay their knowledge together, and produce a similar guide as speedily as possible; they will be doing a public service, and if the result is as satisfactory as in the case of the book before us, they will, we feel confident, reap something more substantial than thanks. In compiling his tables, Dr. Macfarlane has had the assistance of some of the most eminent geologists in the States, and some of the information has not before been published. The tables are very similar to railway time-tables, having on the left hand side of the names of the stations, the miles between each, and on the right, instead of the times, the names of the leading formation to be seen along the route. The tables are so constructed as to be useful for a continuous journey through the States in any direction, and, at the same time, to give an idea of the leading geological characteristics of each state. Appended is an index to railroads, and a general geological map of the States. Prefixed are about fifty pages of instructive information, consisting mainly of methodical descriptions of the various formations of North America, and containing Dana's and Hunt's tables of formations. By carefully studying this the traveller will be in a fair position to profit by the tables, and by the faithful use of these much practical knowledge of geology may be acquired even by the ignorant, while to the geologist they will be a constant source of enjoyment; the handy volume is much more easily managed than a map. We may state that the tables refer to Canada as well as the States.

Comment le font les Miracles en dehors de l'Église. Wilfrid de Fonvielle. (Paris: Dreyfous.)

WHY does not M. Dreyfous date his books? We are sure M. de Fonvielle cannot have noticed the suspicious omission. M. de Fonvielle is already pretty well known in France as a popular gossiper on what may be called the eccentricities of science. The present volume is quite equal in interest to anything he has published, and is likely, we should think, to be widely read in France. It

is a series of stories, told with raciness and touches of sarcasm and humour, of the many impostures which have been perpetrated on the public by those who knew how to turn to account some of the results of science. Of course spiritualism comes in for a large share of notice, while the real miracles of science are pleasantly described in one or two concluding chapters. The book is altogether a very curious one, and evidences considerable research in out-of-the-way corners on the part of its author.

On Foot in Spain; a Walk from the Bay of Biscay to the Mediterranean. By J. S. Campion. Illustrated by Original Sketches. (London: Chapman and Hall, 1879.)

MAJOR CAMPION has already proved his power as a charming *raconteur* in his "On the Frontier," and although in artistic finish and exciting incident the present work is not equal to the former, still it is a well-told story of a free and easy walk through a comparatively little-known country. Major Campion did not encumber himself with more baggage than he could carry himself, and with his gun and genial manners and tact he got on without difficulty wherever he went. We should think his work is likely to increase the number of pedestrian tourists in Spain, about the obstacles to travel in which many delusions exist. Major Campion has much to tell in his own way about the places and people he saw, and every now and then we are glad to stumble on a scrap of information about the geology or natural history of the country. His book ought to have many readers.

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.*]

Leibnitz's Mathematics

Tempora mutantur, et nos, &c., seems to hold true for Dr. Ingleby, though not for myself, in this matter. His tone has completely changed since December 4, 1871; and he now puts "scientist" (*in quotation marks*) where you had written "scientific man," although in his former letter he said he "hated" the far less obnoxious word "physicist."

That I did not reply to his former letter was mainly because he said that "the question is not susceptible of *proof* until the Council of the Royal Society, who so grossly disgraced themselves in 1712, shall do" a certain "simple act of justice." Recourse to the spiritualists is our only chance in such a matter, and these have now an admirable opportunity for demonstration of their claims.

All I said of Leibnitz was "who, I fear, was simply a thief as regards mathematics." This was completely justified to my mind at the time, partly by my own reading, mainly by some curious documents which Sir David Brewster once showed me. These appeared to me to leave no doubt whatever as to the dishonesty, not only of Leibnitz, but of several of those who have been, at different times, connected with his side of the dispute.

Your allusion to Gregory's series seems to me to bring in no "collateral question" but a very important central one:—it seems to go, in fact, to the root of the matter. For, if Leibnitz was dishonest, this was probably his first (known) offence. Mr. Bottomley's apt quotation looks like a desperate attempt at justification of conduct which the writer felt to be, to say the least, suspicious.

Instead of appealing to the Royal Society Council of 1712, Dr. Ingleby should demand from the proper authorities the publication of that conclusive MSS. of Leibnitz for which Dr. Sloman *asked in vain* in 1858.

P. G. TAIT

The Magnetic Storm of May 14, 1878, Observed in North America

NOTICING in NATURE (vol. xviii. p. 617, and vol. xix. pp. 148, 173, 220) references to the "magnetic storm" of May 14, 1878, I have had prepared by Mr. C. A. Schott, the assistant having direction of our magnetic observatory at Madison, Wisconsin, the inclosed memoranda relative to observations made at that point, as an item of interest to your readers, illustrating the general character and almost simultaneous action of that great magnetic disturbance.

C. P. PATTERSON,
Supt. Coast and Geodetic Survey

Washington, January 6

THE extensive magnetic disturbance of May 14, 1878, of which accounts have been given in NATURE, vol. xviii. pp. 617, 641, 668, and which was observed in China, Australia, and England, was also making its record in North America at our magnetic observatory, established at Madison, Wisconsin, in the winter of 1876–77. This observatory is in latitude 43° 4′ 29″·5, and in longitude 5h. 57m. 36·5s. W. of Greenwich; in it are mounted a set of Brooke's magnetographs, and daily photographic traces of the changes in magnetic declination and in the horizontal and vertical forces have been produced since March, 1877, and are intended to be kept up for some years. The declination traces for several days preceding the 14th were normal, but about midnight, May 13–14, a series of disturbances commenced consisting in part of some large oscillations to the eastward and westward, and in part of a great number of small and rapid oscillations. The characteristic features of the trace may be given as follows:—

	Madison mean time.	Greenwich mean time.
	h. m.	h. m.
The disturbances in *declination* commenced about	14th, 0 00 A.M.	... 14th, 5 58 A.M.
A principal westerly[1] extreme reached...	,, 1 05 ,,	... ,, 7 03 ,,
,, easterly ,, ,, ...	,, 2 36 ,,	... ,, 8 34 ,,
Range of motion 16″·5; after this a series of smaller oscillations continue to past noon; a maximum westerly position is reached about	,, 0 16 P.M.	... ,, 6 14 P.M.
And an easterly extreme at	,, 0 53 ,,	... ,, 6 51 ,,
Extreme westerly deflection at	,, 3 26 ,,	... ,, 9 24 ,,
A sharp motion to eastward commences at	,, 5 40 ,,	... ,, 11 38 ,,
A principal easterly extreme reached at	,, 6 24 ,,	... 15th, 0 22 A.M.
Range of motion of principal disturbance 31″·7.		
There is also a westerly extreme about...	,, 7 10 ,,	... ,, 1 08 ,,
And an easterly extreme about	,, 9 16 ,,	... ,, 3 14 ,,
After 10 P.M. the irregularities gradually subside.		
Last extreme easterly position	15th, 1 10 A.M.	... ,, 7 08 ,,

It will be noticed that at the Greenwich Observatory the storm commenced on May 14 at 6h. 5m. A.M., at Stonyhurst Observatory at 6h. 4m., at Ti-ka-wei, near Shanghai, also at 6h. 4m. (G. T.), and at Melbourne, supposed at 6h. 20m.; the storm may therefore be taken as simultaneous at these places. At Greenwich the north end of the needle moved eastward between 6 and 9 A.M., but at Madison the general motion was westerly. Again, the sharp deflection commencing at 5h. 40m. Mad. T. (11h. 38m. P.M. G. T.) was to the *eastward* at Madison and to the westward after 11h. 45m. at Greenwich, thus deflecting the magnets in opposite directions.

	Madison mean time.	Greenwich mean time.
	h. m.	h. m.
The northern component of the *horizontal* force was sharply affected at Madison, the force diminishing at ...	14th, 0 05 A.M.	... 14th, 6 03 A.M.
The disturbance continued, but between 2.30 and 7.30 A.M.		
The trace is too indistinct to be read; the small oscillations continue to about 11h. 45m., when they become superseded by a series of larger waves culminating in a maximum extreme at ...	,, 2 30 P.M.	... ,, 8 28 P.M.
And in a minimum at	,, 3 58 ,,	... ,, 9 56 ,,
The large disturbances continue till about 10h. 20m. P.M., having reached a maximum extreme about	,, 4 50 ,,	... ,, 10 48 ,,
And a minimum extreme at	,, 9 03 ,,	... 15th, 3 01 A.M.

Range between max. at 2h. 30m. and min. at 9h. 3m = $\frac{1}{184}$ of the horizontal force, nearly.

In the Greenwich account it is stated "The first start in the trace of the declination magnet at 18h. 5m. (A.R.) is most distinct;" now, within two minutes of this time occurs the first and sharpest deflection in our horizontal force trace at Madison, thus

[1] Referring to the end of the magnet.

marking distinctly the commencement and simultaneousness of the storm.

The disturbance in the *vertical* force commenced about May 14, 1h. 10m. A.M. (7h. 8m. G. T.), and terminated about 3h. 30m. A.M. Between this time and 5h. 45m. P.M. the trace was smooth, but between 5h. 45m. and 6h. 45m. a sharp deflection took place in the opposite direction, the maximum force occurring at 6h. 11m. P.M. (May 15, 0h. 9m. A.M. G. T.); the deflection or increase amounted to $\frac{1}{10}$ of the vertical force.

The vertical force trace did not exhibit any of the tremulous motion noticed in the two other curves.

I may state that a description of the Madison Magnetic Observatory, together with the discussion of the first year's observations and results, is nearly ready in MS. for publication.

Coast Survey Office,
Washington, D.C.,
January 6

CHARLES A. SCHOTT,
Assistant, C. and G. S., in charge of Observatory

Migration of Birds—A Suggestion

I BELIEVE that the migration of birds is one of the most interesting problems to the many who dabble in the narrative branch of natural history. It must also be of some interest to those who study biology in its more strictly scientific form. And yet there appears to be but little accurate information on the subject; nor, as far as I can discover, have any attempts been made to collect careful simultaneous observations over a wide extent of country, which would appear to be the readiest, if not the only means of collecting reliable data.

I am therefore induced to suggest that you should devote a small space in NATURE weekly, at certain seasons of the year, to the record of observations made in all parts of the world, from which correspondents can be induced to send them, of the passage of any migratory birds.

The record should, I think, be in a tabular form, giving the place, the date of appearance or disappearance of any migratory bird, the name of the observer, and some few remarks that may appear pertinent, such as the state of the weather, the direction of the wind, the character of the night, whether dark or moonlight, in the case of birds which are supposed to travel at that time. The necessity for recording other facts will no doubt be suggested by those more conversant with the subject. From these suggestions and the experience gained in a short time, detailed instructions might with advantage be framed for observers. The observers should be careful to state whether they were in a position to know with certainty that the date given was that on which the birds really arrived or left, or whether it was the first time their attention had been called to the circumstance.

I should imagine you would soon have a large number of careful observers interested in the subject not only in the country but abroad also. Among these some means should be taken to enlist captains of ships, or other seafaring men. There appear to be few, if any, recorded observations of birds of passage being seen on the wing at sea—though I may be wrong in this, for I do not profess to have gone carefully into the subject. Still, it is curious that one does not hear oftener of flights of such birds as the woodcock, millions of which come to and leave this country every year, being seen at sea, over long stretches of which they are believed to fly.

J. F. D. DONNELLY

The Formation of Mountains

THE letter of the Rev. O. Fisher in NATURE, vol. xix. p. 266, is conclusive as to the more rapid cooling of the interior than the outer crust of a heated globe under the conditions of our earth, and I thank him for clearing up the point. But the question remains, whether the amount of contraction of the interior, and consequent crumpling of the crust, thereby produced in a definite time, is sufficient to account for the elevation of our mountains. It is necessary to take account of the following facts:—

1. That the greater part of the elevation of all our chief mountain ranges occurred during the eocene and miocene periods.

2. The warmer climates of those periods (certainly due to external and not to internal heat) would have tended to diminish the rate of cooling and consequent contraction of the earth.

3. The Rev. O. Fisher appears to have demonstrated that, even allowing for the total shrinkage due to the earth's cooling for the last hundred million years (from a mean temperature of 7,000° F., as calculated by Sir William Thomson), the amount of elevation thereby caused would be *very much* less than that of existing lands and mountains. But we know that these have been lowered by denudation, and again elevated many times over during that period.

The inadequacy of the alleged cause for the production of our existing mountains would therefore seem to be conclusively established.

ALFRED R. WALLACE

MR. WALLACE'S letters raise three separate difficulties: How can the interior of the earth be cooling faster than its surface? How can this surface be cooling so slowly (or not cooling at all) as it is assumed to be? and, How can the different rates of cooling of interior and surface account for the corrugations observed in the surface?

As to the first question, Mr. Fisher's explanations should remove the difficulty.

As to the second question. So long as the amount of heat radiated from the surface into space equals the amount received by the surface from the sun and from the interior together, its temperature cannot fall. The rate of radiation depends on the difference between the temperatures of the surface and space. The supply of heat from the interior depends on the difference between the temperatures of the interior and the surface. Since the temperature of the interior is falling, the supply of heat from interior to surface must diminish. Hence the temperature of the surface must also fall. We cannot doubt that it does, though at present imperceptibly. The extreme slowness is due partly to the fact that the difference of temperature between interior and surface must many times exceed that between the surface and space, even if the latter be at absolute zero; partly to the low transmissive powers both of earth and atmosphere; partly to the immense mass of the earth, from which immense quantities of heat must pass away to lower the temperature of the interior but a trifle.

As to the third question, whether this small fall in temperature will account for observed corrugations, I will offer no opinion. The size of the earth must be allowed for. A diminution of a foot in radius would diminish the surface by more than nineteen square miles.

I do not understand why corrugations should be confined to the surface. I should expect them to extend as deep as the solid crust. They are possible in any stratum which is contracting more slowly than interior strata.

E. HILL

St. John's College, Cambridge

Bees' Stings

CAN any of your readers inform me why the working honey-bee has such an imperfect weapon of defence as its sting manifestly is? For purposes of self-defence it is apparently worse than useless, for in nearly every case, almost without exception, the bee lays down its life with the sting. The possession of a sting therefore only leads to its own destruction instead of to its preservation so far as the individual bee is concerned. No doubt the hive generally gains an advantage from all its active members having stings and so indirectly do individual bees from the fact that the welfare of the hive, speaking generally, means the welfare of the individuals that compose it. Directly, however, the possession of a sting can only be a disadvantage to the individual bee, unless there are certain enemies from which bees after inflicting a wound can withdraw their stings and escape with life. This so far as my observations go appears to be very unlikely, and therefore no bee can have any knowledge from experience of what a weapon of offence he possesses for he has never used it, nor can he have knowledge from experience of the consequence of using it. All smaller pests bees attack with their jaws. Is it possible, then, that they are so intelligent as to be well aware of the power for mischief to themselves as well as to others which they carry about with them, and that it is only when they altogether lose control over themselves, either through severe pain or through terror lest their queen should be injured that they sign their own death-warrants on our hands and faces? In the death of a few worker-bees a hive suffers very little loss, perhaps none at all; yet it may have gained much in the shape of security from molestation. Are bees so intelligent as to know this fact and communicate it from one to another, or can their conduct be explained on the lower ground of instinct?

It seems that an interesting point is here raised which perhaps

has been fully discussed elsewhere without my knowledge of it. Is the fact that the sting of the worker-bee is an imperfect weapon of defence, a result of its having nothing to do with the propagation of its species, this being left to the stingless queen and drones? Consequently any tendency to develop a more effective sting in one generation of worker-bees has no hereditary effect on succeeding generations, nor apparently have the worker-bees any influence whatever on the worker-bees that succeed them, except by the way in which they feed and educate them, unless, indeed, they can impress their tendencies on the drones or on the future queen before she leaves the hive. If they have no such power, it seems likely that they will always have to lament the use of a weapon which nature might have made as effective as the sting of a wasp. Finally, are there any other insects in the same predicament as worker-bees, *i.e.*, unable to use their weapons of defence without doing themselves more injury than they inflict on their adversary, and unable to help their successors by the transmission of a continually accumulating instinct? R. A.

Manningtree, January 22

Molecular Vibrations

MR. CHAPPELL is certainly right in stating that "the noises in a belfry are most discordant." He might have said (what no doubt he meant) that the sounds emitted by each single bell are most discordant. Every bell which is at all tolerable, possesses, it is true, one predominating note due to the thick part of the bow, where the clapper strikes, but there are also innumerable other notes, some of which *may* be harmonics, while the majority are not so at all. This is presumably often owing to flaws and other defects in casting, but there is another cause common to every case, which is due to the following fact :—

All bells are cast of a conventional shape, with varying diameters from bow to crown. Now every part of a bell, taken vertically, comes into vibration when struck, and in order to give a true note, each horizontal section ought to have a certain exact thickness of metal proportional to its diameter. This is easily verified to the ear by tapping the bell gently at all parts from the bow upwards. Every inch gives a different rate of vibration, and, consequently, a different pitch.

About the time when the second "Big Ben" was cast, which is a long time ago, I tried experimentally to ascertain what the law was which regulated the thickness of the metal in relation to the diameter of the bell, so that every section might be of identical pitch. This was done by casting a series of bell metal rings of varying diameters, and tuning them, by turning in a lathe, to exact unison. So far as my recollection now serves me, the following was the result :—

Measuring all the rings by their outside diameters, no undeviating rule was apparent, and the same was the case when the inside diameters were compared. When, however, a circle was taken whose circumference was, as nearly as possible, one-third from the outside of the thickness and two-thirds from the inside, then the law came out distinctly that the thickness of the metal must be proportionate to the square of the diameter of such circle. It occurred at once that this circle must, in fact, constitute the neutral axis of vibration. Working on this principle, it seemed worth while to try whether a bell could not be constructed free from discordant sounds. I may shortly say that this proved to be possible, but only by turning the actual casting with great care and accuracy in a lathe. It became evident that the slightest variation in the true thickness vitiated the unisonal character of the tone. A "miss was as good (or as bad) as a mile," and consequently the process of casting itself was too rough for obtaining the desired end.

It may fairly be gathered from Mr. Chappell's letter that he is not enamoured of a "triple bob major," and that he does not class bells generally as musical instruments. I am much afraid he never will. If the present shape and mode of construction (and let me add, the present mode of change ringing) is adhered to, a peal of bells which will quite satisfy a musical ear may be regarded as a practical impossibility. R. H.

Missing Nebulæ

IN the note on missing nebulæ in NATURE, vol. xix. p. 221, I find the nebulæ G. C. 132, 4570, and 5051 mentioned together with the Merope nebula as being diffused objects which are "overlooked in very large telescopes, though obvious in much smaller ones." This alludes, no doubt, to the occurrence of these objects in the list of nebulæ not found with Lord Rosse's 6-foot reflector (*Phil. Trans.*, 1861, p. 745).

With regard to the first object, G. C. 132, it has only been looked for once at Birr Castle, and in the N.P.D. 111° 30′ it is possible to account for its non-appearance either by a tilting of the speculum or by the haziness of the sky in this low altitude. G. C. 4570 has been seen three times, and only twice searched for in vain, both times in twilight. G. C. 5051 was set for twice and not found, but 15° north of the zenith the tilting of the speculum almost always changes the index-error of the setting-circle considerably, as expressly stated by the observer on one of the two occasions alluded to. The Merope nebula was last winter seen very distinctly, and roughly sketched with a low power and large field. J. L. E. DREYER

The Observatory, Dunsink, Co. Dublin, January 13

Time and Longitude

NOW that mankind begin to have settlements, even continental, as appears from Mr. Latimer Clarke's account of Sitka, subject to the inconvenience that he and Mr. Layard point out, is it not time that we agreed to make the line dividing "yesterday from to-morrow" avoid all continents, by taking advantage of two very convenient, if not providential, facts, which are certain, though each was *à priori* highly improbable? First, there were great chances against a globe with our existing proportion of land to water, of coast-lines to area, and of large and small lands to each other, having any Behring Strait, admitting one degree of longitude, or thereabouts, to enjoy the above property. But next, there was still greater chance, perhaps, against the exact opposite degree to the strait covering several national observatories; not only more of them, I think, than any equally narrow meridional band, but the only one that, on historical grounds, we can conceive distant civilised nations accepting without jealousy as a common centre. The antimeridians of Copenhagen, Uraniburg, Leipzic, Munich, Padua, Venice, and Florence, seem to avoid both continents; possibly also those of Christiania, Gotha, Verona, and Modena. Those of Berlin, Prague, Naples, and Palermo, seem a very few miles too far east. Europe proper, and its present railways, are very closely bisected by this street of observatories; the local time of the furthest points each way varying but an hour and a half from it. But the chief coincidence is yet unnamed. Would the pride of any existing land, except China, refuse to make a standard meridian of Rome?

The very Chinese must allow Europe a sort of scientific precedence, not as the metropolitan, but the learned continent—earth's university. Europe alone is the adult continent, if there be one; and no other has in a strict sense a metropolis. The history of no other has so turned upon one pivot city as that of Europe has on Rome, nor is likely ever to do so. Some one says that "what a church is to a city, Palestine is (or may some day be) to the world;" but it is less disputable that what the market-place is to a city, Europe is to the world—perhaps permanently. And what the tribunal is to the market-place, Rome has been to Europe, as long as Europe was growing. Observe, too, that in this special connection both our civilised time-reckonings, "Old Style" and New, have come from Rome. Might we not also supersede the distinction of E. and W. longitude, by calling Rome 180°, and reckoning all round, from Behring to Behring, leaving the 0° as yet unmarked? E. L. G.

[E. L. G.'s proposal has been already made by M. de Beaumont. See NATURE, vol. xix. p. 247.—ED.]

Shakespeare's Colour Names

I FEAR you will think that the correspondence on this subjec is becoming a mere criticism on Shakespeare's text, and therefore out of place in your columns, but I trust you will afford me space for a short rejoinder to Mr. Ingleby's letter (NATURE, vol. xix. p. 244).

I am obliged to him for pointing out that Sir T. Hanmer had already suggested the substitution of "keen" for "green" in the passage from "Romeo and Juliet," Act iii. Sc. 5. This had escaped me, but I cannot agree with him that the alteration has been *rightly* rejected by subsequent commentators. I have not at present any opportunity of examining the eyes of any living eagles, but in opposition to Mr. Craig-Christie's evidence (NATURE, vol. xix. p. 221) I must point out that all our best

British ornithologists—Yarrell, Macgillivray, Gould, Meyer, and Morris—describe the eye of the Golden Eagle (the less rare of our two British species, and the one usually referred to by our poets) as *hazel* or *brown*. The eye of the Sea Eagle is described by the same authorities as yellow. I cannot think that so accurate an observer of nature as Shakespeare would call either hazel or yellow eyes *green*. Can Mr. Ingleby cite any authority for such a comparison as "green as is an eagle's eye"? while the keen piercing sight of the bird is as proverbial as the swiftness of its flight. I am well aware that green eyes were held in high estimation by the old poets, especially by those of Spain; Shakespeare, however, does not seem to me to have shared in this predilection, as, setting aside the doubtful play of "The Two Noble Kinsmen," and the passage now in question, he uses the epithet three times only, I think, as applied to the eye, and then always *in malam partem*, viz., "green-eyed jealousy," "Merchant of Venice," Act iii. Sc. 2; "It is the green-eyed monster," "Othello," Act iii. Sc. 3; and in "Midsummer Night's Dream," Act v. Sc. 2, where the "eyes as green as leeks" are met with in conjunction with "lily lips," "cherry nose," and "yellow cowslip cheeks." I cannot think with Mr. Murphy (NATURE, vol. xix. p. 197), that the eyes which the old poets so admired as green were what we call blue; they were more probably *grey*, which often has a shade of green in it—the "eyen grey as glas" of Chaucer's "Prioresse." These green or grey eyes were, I think, usually an attribute of feminine rather than masculine beauty, as in the passage from "The Two Noble Kinsmen," Act v. Sc. 1, where they are mentioned in an address to Diana (not Neptune, as Mr. Ingleby has it). Shakespeare well distinguished between the different colours of eyes—see "Two Gentlemen of Verona," Act iv. Sc. 4, and "Twelfth Night," Act i. Sc. 5, for grey eyes; "As You Like it," Act iii. Sc. 2 for blue eyes; "Romeo and Juliet," Act ii. Sc. 4 for black and grey eyes, and Act iii. Sc. i. of the same play, where hazel eyes are mentioned.

ROBERT BREWIN

Exeter, January 20

Intellect in Brutes

SIR HARRY LUMSDEN allows me to publish the following little incident:—Late last autumn some partridges, which he had tamed and kept about the house, disappeared as usual and became wild. When the excessive cold set in and Aberdeenshire was deep in snow, Sir H. Lumsden was greatly pleased and surprised one morning to find his old friends on the doorstep waiting to be fed. Next morning they appeared with a wild covey of eleven birds, and the tame cock sat on the doorstep and crowed to the wild birds, evidently encouraging them to come and eat the food, which, however, they declined to do till it was put further from the house. Soon after the tame birds appeared with two covies. How did they entice the wild birds except by actual bird talk?

WALTER SEVERN

Feeding a Python

THE attack of a constrictor, at all events in confinement, is very often unsuccessful; but perhaps this may be because the reptile is not hungry. I have often seen the constrictors in the London Zoological Gardens strike several times at birds, pulling out feathers and even getting a firm hold and then releasing their prey, to renew the attack presently either with or without success. When the membrane over the eye is becoming opaque in consequence of the change of skin they frequently fail to hit the prey at all, but still persist until they secure it. I saw one of the large pythons take a rabbit in a way which must be unusual, I think. The rabbit was hopping about near the snake's coils when the reptile suddenly made a loop in its body, and firmly inclosed the victim without touching it at all with the mouth, or even raising its head. The rabbit died there, but the snake paid no attention to it for a quarter of an hour and subsequently swallowed it very leisurely.

ARTHUR NICOLS

THE GRAHAM LECTURE, ON MOLECULAR MOBILITY

THIS lecture, the institution of which was referred to in NATURE, vol. xix. p. 254, was delivered on the 22nd inst., by Mr. W. Chandler Roberts, F.R.S., Chemist of the Mint, before the Philosophical Society of Glasgow, in the hall of the University, where Graham graduated in 1824.

The audience, which was very large, included most of the professors of the University.

Mr. James Mactear, president of the Chemical Section, pointed out that they were doubly fortunate in having secured the services of Mr. Roberts, whose co-operation in his work Graham repeatedly acknowledged in the warmest terms, and in the fact that Mr. James Young, F.R.S., of Kelly, the life-long friend of Graham, had consented to preside on this occasion; he therefore vacated the chair in favour of Mr. Young, who introduced the lecturer.

Mr. Roberts briefly traced the influence of Black and Thomson in turning the attention of Graham to the study of molecular physics, to which he patiently devoted his life. In connection with the law of the diffusion of gases the lecturer claimed that Priestley made in 1799 an observation on the escape of hydrogen from a cracked jar. The subsequent and independent discovery of this phenomenon by Doebereiner in 1823 has hitherto been considered the starting-point of the experimental study of gaseous diffusion to which it undoubtedly attracted Graham's attention. After a brief review of the influence of Eastern and Greek thought on the study of molecular movement, allusion was made to Sir Christopher Wren's model representing the effects of all sorts of impulses that result from the impact of hard globulous bodies, which, according to Dr. Sprat, historian of the Royal Society, he proposed as the principles of all demonstrations in natural philosophy, it being considered "that generation, corruption, and all the vicissitudes of nature are nothing else but the effects arising from the meeting of little bodies, of different figures, magnitudes, and velocities."

Herepath's revival of Bernoulli's view as to the movement of gaseous particles was considered, and Mr. Roberts then described in detail the experiments that enabled Graham to establish the law of the diffusion of gases, and he illustrated experimentally the passages of gases through porous bodies, such as unglazed earthenware and artificial graphite, as well as through a layer of the hard translucent variety of opal known as hydrophane. The mode in which Graham studied the diffusion of the momentum of gases, by observations on viscosity as indicated by rates of flow through capillary tubes, was then described. It was pointed out that his law of diffusion forms the basis of the science of molecular mechanics, and his measurements of the rates of diffusion prove to be the measure of molecular velocities which have been so profoundly investigated mathematically by Clerk-Maxwell, Clausius, and Boltzmann, and experimentally by Loschmidt in developing the dynamical theory of gases. The lecturer then considered the passage of gases through colloid or jelly-like bodies which have no sensible pores, dwelling more especially on the separation of oxygen from air by the transmission of air through a thin film of india-rubber, a circumstance of special interest from a physiological point of view.

The liquefaction of gases formed the subject of one of Graham's earliest papers, in 1826, and it occupied his attention at intervals during his life. He held the view that hydrogen when absorbed by palladium is reduced to the metallic form, a supposition which has received strong confirmation from the success that has attended M. Raoul Pictet's efforts to solidify this gas; and that distinguished physicist stated in a letter to Mr. Roberts that it is probable Graham's indication of the density of solid hydrogen will prove to be nearly correct. Allusion was then made to Graham's opinion that the various kinds of matter now recognised as different elementary substances may possess one and the same ultimate or atomic molecule existing in different conditions of movement, the varying degrees of rapidity of this movement constituting, in fact, the difference between the elementary

bodies. In other words, if the molecular energy of a so-called element could be changed, the element would be dissociated, a view of special interest in relation to the researches of Lockyer. The lecture was illustrated by many effective experiments, and concluded with the statement that it had not been instituted from the merely special interest of Graham's researches to the physicist and chemist, but in honour of the labours of a life the memory of which will be as enduring as its work, and to stimulate others to investigate as patiently and earnestly the varied phenomena whose basis is "molecular mobility."

Sir William Thomson, in proposing a vote of thanks to the lecturer, called attention to a diagram on the wall recording the rates of passage of gases by diffusion, effusion, transpiration, and by the peculiar passage through such "colloid septa" as non-crystalline metals or india-rubber; and he stated that before Graham's time these valuable physical constants were absolutely unknown. They had listened with much interest to the connection which had been traced between Graham's law of diffusion and the science of molecular physics, as well as to the account of Graham's work generally, so carefully set before them by Graham's pupil and friend.

PRELIMINARY NOTE ON THE SUBSTANCES WHICH PRODUCE THE CHROMOSPHERIC LINES[1]

HITHERTO, when observations have been made of the lines visible in the sun's chromosphere, by means of the method introduced by Janssen and myself in 1868, the idea has been that we witness in solar storms the ejection of vapours of metallic elements with which we are familiar from the photosphere.

A preliminary discussion of the vast store of observations recorded by the Italian astronomers (chief among them Prof. Tacchini), Prof. Young, and myself, has shown me that this view is in all probability unsound. The lines observed are in almost all cases what I have elsewhere termed and described as *basic lines*; of these I only need for the present refer to the following:—

b_3	ascribed by Ångström and Kirchhoff to iron and nickel.		
b_4	,,	Ångström to magnesium and iron.	
5268	by Ångström to cobalt and iron.		
5269	,,	,,	calcium and iron.
5235	,,	,,	cobalt and iron.
5017	,,	,,	nickel.
4215	,,	,,	calcium, but to strontium by myself.
5416	an unnamed line.		

Hence, following out the reasoning employed in my previous paper, the bright lines in the solar chromosphere are chiefly lines due to the not yet isolated bases of the so-called elements, and the solar phenomena in their totality are in all probability due to dissociation at the photospheric level, and association at higher levels. In this way the vertical currents in the solar atmosphere, both ascending and descending, intense absorption in sun-spots, their association with the faculæ, and the apparently continuous spectrum of the corona and its structure, find an easy solution.

We are yet as far as ever from a demonstration of the cause of the variation in the temperature of the sun; but the excess of so-called calcium with minimum sun-spots, and excess of so-called hydrogen with maximum sun-spots follow naturally from the hypothesis, and afford indications that the temperature of the hottest region in the sun closely approximates to that of the reversing layer in stars of the type of Sirius and α Lyræ.

If it be conceded that the existence of these lines in the chromosphere indicates the existence of basic molecules in the sun, it follows that as these lines are also seen generally in the spectra of two different metals in the electric arc, we must be dealing with the bases in the arc also.

[1] Paper read at the Royal Society on January 23, by J. Norman Lockyer, F.R.S.

ON A THEORY OF THE VISCOSITY OF THE EARTH'S MASS[1]

IN these two papers the investigation is continued of the physical results which follow from the theory that the mass of the earth is either viscous or imperfectly elastic. In the first paper of the series (which was read before the Royal Society on May 23, 1878, and of which an account appeared in NATURE, vol. xviii. p. 265) the theory of the bodily tides of such a spheroid was considered. In that paper it was shown that the bodily tides would lag, and that this lagging would produce an acceleration of the time of high water of the oceanic tides relatively to the nucleus. The author's attention was directed to the tidal reports of the British Association by Sir W. Thomson, and he has tried to find whether the tidal observations give any indications of a yielding of the earth's mass. The theory of the semi-diurnal and diurnal oceanic tides is so imperfect that it is impossible to say whether or not high water takes place earlier than it would do on a rigid nucleus; the long-period tides are those from which alone any indications are to be expected.

The fortnightly tide is the most marked of these, but its height is very small, and the results in the tidal observations show so much irregularity that it cannot be asserted with certainty that they represent the true fortnightly tide. Nevertheless, it is interesting to learn that, out of eleven years of observation at Ramsgate, Liverpool, Hartlepool, Brest, and Kurrachee, the fortnightly tide appears to be accelerated in eight cases and only retarded in three. Although the accelerations are exceedingly irregular, it may perhaps be maintained that these observations give some indications of a tidal yielding of the earth's mass.

The first of the two papers of which we are here speaking deals with the effects of the tidal distortion of the spheroid on its rotation, and with the reaction on the tide-raising satellite. An account of some of the results of the investigation was read before the British Association at Dublin, and an abstract appeared in NATURE, vol. xviii. p. 580, and therefore the principal results will be here merely repeated.

For convenience of diction the spheroid is spoken of as the earth and the tide-raising body as the moon.

It was found, then, that the obliquity of the ecliptic, the length of day and of the month, become variable, and that, if we look into the remote past, we find the obliquity less, and the day and month very much shorter than at present. When the changes were traced backwards as far as possible it was found that the whole diminution in the obliquity was about 10°, and that the beginning from which the earth and moon must have started was a state in which they rotated, as though fixed rigidly together, in 5h. 40m., the moon being then only 10,000 miles distant from the earth's centre.

In the second paper (read before the Royal Society on December 19) some other problems were considered. The first of these is concerning the secular distortion of the spheroid. Under the attraction of the moon the earth becomes distorted into an ellipsoidal shape, with the longest axis in the plane of the equator, but, since the tide lags, this longest axis does not point directly towards the moon. The excess of the attraction of the moon on the nearer protuberance above that on the further one gives rise to the tidal frictional couple. This couple tends to retard the earth's rotation; but it is clear that unless the tidal protuberance has some special form

[1] An account of two papers, "On the Precession of a Viscous Spheroid, and on the Remote History of the Earth," and "Problems Connected with the Tides of a Viscous Spheroid," by G. H. Darwin, read before the Royal Society on December 19, 1878.

the whole earth cannot be retarded exactly as though it were a rigid body. Now the tidal protuberance has not this required form, and therefore there results a slow secular distortion of the earth arising from the unequal distribution over the surface of the forces which constitute the tidal frictional couple.

The greater part of the pull which retards the rotation is applied in the equatorial regions, and therefore the rotation of those regions will be more rapidly retarded than that of the polar regions. As the earth's rotation is from west to east, it follows that the polar regions will outstrip the equator and will move very slowly from west to east relatively to the equatorial parts.

The exact mathematical solution for this kind of a distortion of a viscous spheroid shows that it consists in a simple cylindrical motion round the axis of rotation, each point moving from east to west with a linear velocity proportional to the cube of its distance from that axis.

The distortion of the surface of the globe consists of a motion in longitude from west to east, relatively to a point in the equator, the rate of change of longitude being proportional to the square of the sine of the latitude.

Numerical calculation shows, however, that in the later stages of the earth's history (the development being supposed to follow the laws found in the paper on "Precession") the distortion must have been very small. With a certain assumed viscosity it is found that, looking back 45,000,000 years, a point in latitude 60° would lie 14′ further east than at present. From this it follows that this cause can have had little or nothing to do with the crumpling of geological strata.

As, however, the distorting force varies inversely as the sixth power of the moon's distance, it seems possible that in the very earliest stages this cause may have had sensible effects. It is therefore noteworthy that the wrinkles raised on the surface would run north and south in the equatorial regions, with a tendency towards north-east and south-west in the northern hemisphere, and north-west and south-east in the southern one. The intensity of the distorting force at the surface varies as the square of the cosine of the latitude.

An inspection of a map of the earth shows that the continents (or large wrinkles) conform more or less to this law. But Prof. Schiaparelli's map of Mars[1] is more striking than that of the earth, when viewed by the light of this theory; but there are some objections to its application to the case of Mars. If, however, there is any truth in this, then it must be postulated, that after the wrinkles were formed the crust attained sufficient local rigidity to resist the obliteration of the wrinkles, whilst the mean figure of the earth adjusted itself to the ellipticity appropriate to the slackening diurnal rotation: also, it must be supposed that the general direction of the existing continents has lasted through geological history.

The second problem considered in this paper is concerning the distribution of the heat, which would be generated by the internal friction of the tidal distortion.

It was shown in the preceding paper that a very large amount of heat might be thus generated, and it appeared at first sight as though this might serve to explain in part the observed increase of underground temperature; but the solution of a certain problem concerning the cooling of an infinite slab of rock 8,000 miles thick, in which heat is being generated according to a certain law of distribution, shows that the frictional heat could not possibly explain a rate of increase of underground temperature near the earth's surface of more than 1° Fahr. in 2,600 feet.

It follows, therefore, that Sir W. Thomson's investigation of the secular cooling of the earth cannot be sensibly affected by this cause.

[1] *Memorie della Società degli Spettroscopisti Italiani*, 1878, vol. vii.

The last part of the paper does not lead to results of interest to the general reader, as it is concerned with the part played by inertia in the tides of viscous, fluid, and elastic spheres.

INDIAN METEOROLOGY[1]

IN the article "Atmosphere" of the *Encyclopædia Britannica* it has been justly remarked that one of the most important steps that could be taken towards the development of the science of meteorology would be extensive series of observations from such countries as India, which offers splendid contrasts of climate at all seasons, has a surface covered at one place with the richest vegetation, and at others with vast stretches of sandy deserts, and presents extensive plateaus and sharp ascending peaks, all which conditions are indispensable for collecting the data required for the solution of the problem of atmospheric physics. In working out this problem it is necessary, owing to its extreme complexity and difficulty, to give attention, not merely to questions immediately bearing on the physics of the atmosphere, but also to climatic and other practical inquiries, which may be handled with comparative ease and which afford results that contribute indirectly but very materially to the solution of the higher problem. The publications enumerated below admirably follow up this two-fold line of inquiry, and even already several important practical and theoretical conclusions seem not far from the point of being reached by the meteorologists of India.

The "Report on the Meteorology of India" is the second Annual Report issued since the administration of the Indian Meteorological Establishment was concentrated in the Central Office at Calcutta for the whole of India including British Burmah and the Islands of the Bay. In the scheme of publication of the monthly results of the observations made at the various stations over India, we note with satisfaction that the form proposed by the Permanent Committee of the Meteorological Congress at Vienna has not been adopted in some of its more important details. Thus in Mr. Blanford's tables, instead of a general monthly mean of atmospheric pressure, the mean monthly pressure for each hour of observation is given—an essential requisite for the presentation of the data required in discussing various of the more important problems of international meteorology. Indeed these tables possess the very high merit of being, with perhaps one exception, entirely suited for the discussion of climatic questions of an international character—the single exception being the lumping together of the two or four daily observations of the winds into one monthly mean, instead of a monthly mean for each hour of observation as is so admirably carried out by Professor Rubenson in his annual reports of Swedish meteorology.

The most interesting part of this report is that which deals with the failure of the rains in Western and Southern India which resulted, as is only too well known, in one of the most terrible and wide-spread famines of recent years. The mode of treatment is grounded on the practice adopted by the Office, in framing forecasts of coming seasons to which we have several times drawn the attention of the readers of NATURE (vol. xiii. p. 66, &c.), and which may be described as proceeding on the assumption that there is a certain persistency in meteorological conditions; that, for instance, the longer a given state of weather has lasted, the less the probability of a speedy change; and that as regards the distributions of pressure, on which weather is so dependent, certain states of the atmosphere tend to perpetuate or reproduce themselves in the same region in such a manner as to maintain a

[1] "Report on the Meteorology of India in 1876." By Henry F. Blanford. "The Indian Meteorologist's *Vade-Mecum*." By Henry F. Blanford. "Indian Meteorological Memoirs;" issued under the direction of Henry F. Blanford. Vol. i., part 2. "The Meteorology of the Bombay Presidency." By Charles Chambers, F.R.S.

constant difference between the average pressure of two neighbouring regions which, though protracted, is not permanent, but disappears after a longer or shorter time. Mr. Blanford largely inclines to trace the failure of the rains to an unusually great expanse of snow covering the southern slopes of the Himalayas, much of which fell very late in the season, and which acted as a cooling agent, bringing about an abnormal distribution of pressure, and consequently of winds, temperature, and rainfall, conditions which, once fairly established, went on reproducing themselves so that cyclonic and anti-cyclonic areas of an abnormal character gained a certain persistency over those parts of India where the rainfall was deficient and where it was in excess. Should future observations confirm this hypothesis, the result will be one of the most important yet arrived at in practical meteorology.

The least satisfactory part of the report, perhaps, is that referring to the relation of rainfall to the sun-spot period, in which too much stress appears to be laid on the results of data collected from a wide geographical superficies, and too little stress upon data referring to limited regions; the data of which regions, it may be added, require for their satisfactory discussion to be examined with reference to their seasonal as well as annual variations during the sun-spot periods.

The practical part of the "Indian Meteorologist's *Vade-Mecum*" being part I of the work, is in many respects a model-handbook for the observers for whose use it is intended. The clearness with which the difficulties attending the making of real observations of temperature are apprehended is altogether admirable; and the provisions and precautions as regards instruments, hours, and modes of observing actually taken are of such a nature as likely to secure observations of a high quality, owing to an increased intelligence, and efficiency on the part of the observers who work in accordance with the principles and instructions laid down for their guidance.

Mr. Chambers' book is an elaborate and important work on the Meteorology of the Bombay Presidency, based on all the observations made in the Presidency down to 1874. Its splendid porte-folio of eighty highly finished maps and diagrams printed in colours, as well as its excellent typography with 159 tables of results, many of them being wholly or in part laborious and elaborate analyses of the different data of observation, render the work an *édition de luxe*. The contributions with which this work enriches Indian meteorology are twofold, viz., the results of the hourly observations made for many years at Kurrachee, Deesa, Bombay, Poona, and Belgaum; and the monthly averages for numerous stations throughout the Presidency, from which the temperature, rainfall, and winds of this part of Asia are charted with a fulness and consequent approximation to the truth not hitherto attainable. The influence which the broad physical features of the region, such as its lofty mountain ranges, high plateaus, river valleys, and extensive sandy deserts, has on the climatology of the Presidency is worked out with great skill and ability. Still more able are the discussions of the hourly observations of pressure, temperature, humidity, and cloud, made at the five chief stations, together with many suggestive reflections on the results developed, which will well repay the reader's best attention, even though he may sometimes not see his way to agree with the opinions expressed.

A healthy feature of Indian meteorology is the vigorous manner in which the making of hourly observations is pushed at many stations which have been admirably chosen as respects the objects sought to be attained, and the comparatively full and prompt discussions of the results which are published from time to time. Of the problems handled in those discussions the most frequent as well as the most important is that of the diurnal oscillations of the barometer. To this very difficult problem Mr. J. Eliot, for example, makes a valuable contribution in a paper on two storms in Bengal during 1876, which were accompanied with increased atmospheric pressure, and the apparent reversal of the normal diurnal oscillation of the barometer. This reversal was found to be accompanied with an instantaneous and complete change of wind direction and a great fall of temperature, which, as they occurred before the rain began to fall, proved that they were not due merely to an inrush of a strong humid current from the Bay of Bengal. The sudden chilling of the air, accompanied as it was by an increase of pressure, also proved that the changes were not due to the internal action of a mass of air or to horizontal or surface currents from the interior, which would have been warm currents, but that they were probably produced by the downrush of a cold upper current, a conclusion which will doubtless receive further examination not only from its bearing on barometric fluctuations but also on the theory of storms.

OUR BIG GUNS

WE may leave the explanation of the disaster on board the *Thunderer* for the present to those who have been appointed to inquire into the matter. But in the mean time it will be well to consider what are the elements of weakness, if any, in the construction of our big guns.

The system of building up large guns by shrinking coiled iron tubes over a central steel tube seems extremely well adapted to prevent a *lateral* explosion; for even when the steel tube has had a *longitudinal* crack, the gun has been frequently fired without any further evil consequence.

But our guns are manifestly deficient in *longitudinal* strength, for the steel tube is the only tube continuous from end to end. If, then, there should be any ring-crack in the steel tube, there is little to prevent its separation into two parts beyond the friction of the coiled tubes, and the dove-tailing by which it is attempted to join the coiled cylinders together.

Now considerable longitudinal stress on the steel tube must be caused every round by the rifling necessary to give the shot its proper rotation, and occasionally, by a jamming of the shot. Also every discharge of the gun must cause a violent vibration in every part, and should the junction of the I B coil with the C coil and breech-piece work rather loose, this would be likely to cause a ring-crack in the steel tube in that neighbourhood.

When rapidly-exploding powder was used in the service the guns were very properly rifled with an *increasing* twist with a view to remove every possible obstruction to the *initial* motion of the shot. The increasing twist is still in use notwithstanding all the efforts that have been made to manufacture a powder that will burn slowly, so as to make the propelling pressure on the shot more nearly uniform. With a view to distribute the work of giving rotation to the shot uniformly along the bore, the rifling should be calculated to give a nearly constant pressure on the studs. But this depends upon the law of explosion of the powder. And this law is very variable, and very little understood. Only we know this—that the more nearly the force propelling the shot becomes constant, the more nearly the rifling approaches the uniform twist in order to obtain a constant pressure on the studs. Now the objection to the increasing twist is that it throws the chief part of the work of giving rotation towards the muzzle, where the gun is weakest. Also there is a difficulty in arranging the studs on the shot, and it now appears that the increasing twist allows the shot to slip forward when the gun is depressed. It seems, therefore, desirable to revert to the uniform twist of rifling now an improved powder is used.

But in order to give the gun additional strength in direction of its length, it seems desirable that the steel

tube should be supported by an outer tube of equal length and thickness, but composed of more tenacious metal—wrought iron or gun metal.

If this cannot be satisfactorily accomplished, then the steel tube might be covered with at least two layers of coiled tubes breaking joint. In this case the tubes should be *screwed* as well as shrunk over each other. If the screwed part was slightly conical it would be possible to adjust the tension with nicety. B.

January 25

THE ELECTRICITY OF THE TORPEDO

THE recent researches of Prof. Marey on the electric discharge of the torpedo have been presented by the author in an extended memoir published last year.[1] We propose to present to our readers the main conclusions reached by M. Marey, and the experimental demonstrations on which the principal of these are based. But before entering into details of the experiments let us indicate summarily the processes employed by M. Marey.

In previous researches,[2] made in 1871, he had at his disposal only the reactions of the muscles of the frog to analyse the electric phenomena of the torpedo; he caused to be recorded, upon an inclosed plate, the shock of a frog's muscle produced by the discharge of the electric apparatus of the torpedo. The instant of the excitation of an electric nerve or of the nervous centres of the torpedo was recognised; and it was seen that the movement of the foot of the frog presented, at the instant of excitation, a considerable retardation, equal, *e.g.*, to four-hundreds of a second, measured on the chronographic scale. But into this total retardation entered several diverse elements, which M. Marey took into account by causing the muscle of the frog to contract by an excitation directly acting upon it. The time lost by the muscle of the frog representing nearly the half of the total retardation, it was concluded that the time-test by the electric apparatus is equal to that of the muscle of the frog.

Since these first researches, M. Marey has been able to study more directly the electricity of the torpedo by making use of the electro-magnetic signals of M. Deprez and of Lippmann's electrometer.

M. Deprez's signal is composed of a small electro-magnet provided with an extremely light armature of soft iron, which is applied to the coils when the current which traverses them is closed, and which is drawn from it, without delay in demagnetisation, at the moment of the rupture of the current, by the contraction of the tight india-rubber thread. The armature is provided with a style which traces on the inclosed cylinder the closures and ruptures of a current, the duration and frequency of these successive acts, with such perfection that it is easy thus to obtain the record of 1,000 vibrations per second. In the tracing underneath the apparatus (Fig. 1) is seen the signals which it furnishes when acted on by a non-continuous scale of 500 simple vibrations per second.

It is this electro-magnetic signal which M. Marey placed in the circuit formed by the torpedo, whose apparatus was held between two metallic plates joined to the coils of the apparatus by two conducting-wires. We shall see, further on, what use he has been able to make of this.

The second instrument by means of which certain special points of the experiments have been made is Lippmann's capillary electrometer. This apparatus is formed essentially of a column of mercury sustained by capillarity, in a tube of extremely fine glass, the extremity of which is plunged in a bath of dilute acid. When the mercury of the apparatus and the acidulated water are placed in connection with two points of an electric circuit of unequal tensions the capillary column is displaced and is carried towards the side of strongest tension. This displacement is instantaneous, and if the variations of electric tension are produced successively with great rapidity we need not fear the inertia of the capillary column. All the variations are signalled whatever be their frequency. But as the movements of the capillary column cannot be registered themselves, M. Marey has had recourse to photography in a certain number of experiments.

Let us now consider the results following the order which we have indicated at the outset.

1. *A torpedo's discharge is not a continuous current; it is formed of a series of successive waves added one upon another.*

The fundamental experiment upon which the demonstration of this proposition rests was performed with the electro-magnetic signal (Naples, October, 1876). Having compressed one part of the apparatus of an active torpedo just drawn from the water between two metallic plates furnished with conducting wires, M. Marey placed the signal-machine of M Deprez in contact, and the magnet being stimulated he heard a shrill noise resembling that made by filing the end of a hard splinter of wood. The vibrations of the armature, therefore, had been produced by a series of successive electric acts. In defining these vibrations one is justified in stating that the discharge of the torpedo produced by the animal as the result of a local excitation, was composed of a variable number of waves or currents succeeding each other. Fig. 2 represents two tracings so produced. The great advantage resulting from the use of the electro-magnetic signal was to show definitely that the discharge is complex, an analysis which was not possible with the frog's-foot signal. The muscle used as reagent does not in fact react by means of the shocks apart from impulses which are sudden and frequent; it remains in a state of permanent contraction.

But the electro-magnetic signal, whilst showing the dissociation of the torpedo discharge, furnished no other result. It did not indicate how those successive waves follow each other, it seemed even to lead to the conclusion that one wave is quite completed when the next succeeds. At this point the induction is interrupted and the experimentalist adopts another mode of solving this question of the succession of waves in a discharge. M. Marey, in fact, being convinced that the electric action of the torpedo and muscular action should be assimilated, and wishing to see in the discharge the analogue of induced tetanus and even of voluntary contraction, could not resign himself to the admission of an absolute discontinuity between the successive acts constituting a discharge. Yet the electro-magnetic signal apparatus seemed to pronounce his theory wrong. But on passing through Lippmann's electrometer a slight current from the total discharge, M. Marey observed that the column underwent a series of successive impulses, the effects of which unite together. This progression by successive jerks indicated an increase of the intensity of the discharge, an increase in which each new wave is joined to what remains from those which have preceded it. Thus we derive the proof that the electric waves are *partially* united to one another like the muscular shocks of a tetanised muscle.

This first fact being gained, it was necessary to follow up the analysis of the torpedo-discharge, determine the nature of each of the independent electric acts which the electro-magnetic signal had revealed, measure their duration, phases, &c. These different points have been elucidated, each in its turn.

2. *To measure the duration of the electric-wave in the torpedo*, M. Marey has had recourse to the method devised by Guillemin for determining that of very short current, and used afterwards by Bernstein to measure the

[1] "Compte Rendu des Travaux du Laboratoire de M. Marey." T. iii. Paris: G. Masson. 1877.

[2] "Annales de l'École Normale Supérieure." 2e s., t. i., pp. 86-114.

duration of the negative variation of nerves and muscles. Guillemin's method is applicable in every case where a current passes several times in succession through a metallic circuit, with duration always the same. The electric condition of the circuit is investigated during a succession of very short intervals, beginning at the moment when current is complete.

The apparatus used by Guillemin and Bernstein was the galvanometer; M. Marey preferred to use a frog's foot, which, in the successive investigation, gives a movement which can be graphically recorded, as often as there is an electric current.

The graphic method, by which each duration is transformed into a length easily measured on the paper, is easily applied in performing those experiments of which we are about to explain the principle.

Let the point O (Fig. 3) correspond to the moment of electric excitation of a torpedo-nerve, and let the successive points 1, 2, 3, &c., denote successive hundredths of a second, which correspond to very short intervals during which the torpedo apparatus is put in contact with a metallic circuit passing through a frog's foot. In the two first trials, 1 and 2, after the excitation of the electric nerve, there are no signals recorded; the frog's foot

Fig. 1.

remaining motionless shows that the discharge of the torpedo has not reached it, because, in fact, the phenomenon has not yet had time to take place. But at the instant 3 the frog moves, which is expressed in the diagram by a vertical stroke; at the instants 4, 5, 6, 7, 8, 9, and 10, the frog receives shocks which are indicated on the diagram by vertical lines; and finally, at the instant 11, and those succeeding, the frog shows no action, whence we conclude that the electric wave of the torpedo was finished before these last trials; and we see that, according to the tracing, the wave began three-hundredths of a second after the instant of nerve-excitation and finished ten-hundredths after the same instant.

It was exactly in the same way that M. Marey proceeded to measure the duration of the electric wave in the torpedo. An arrangement easily fixed induced electric action in *e* (Fig. 4) at constant intervals. A metallic contact, susceptible of being displaced at will, allowed him, during very short intervals of different lengths, to complete the circuit made by the electric wave of the torpedo to reach the frog's-foot-signal. Moreover, to avoid confusion of the curves which were registered by the successive experiments, he took care to change the position of the style each time, so that the curves appeared one under the other in order.

Fig. 4 shows that the first appearance of the electric

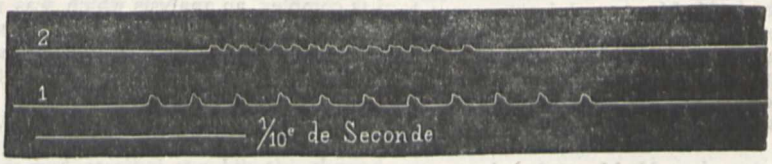

Fig. 2.

wave took place at instant 1; that in a series of successive trials, each later than the preceding, after the excitation of the nerve, the wave was indicated at the instants 2, 3, 4, 5, and 6; and that at the 7th trial the frog gave no signal. The wave, therefore, was completed. Finally, by bringing the instant of trial nearer to that of nerve-excitation, the wave was retraced in experiments 8, 9, 10, 11, and 12; but in the 13th, occurring too soon after the instant of nerve-excitation, it was shown that the electric wave no longer existed.

The approximation of these measurements necessarily

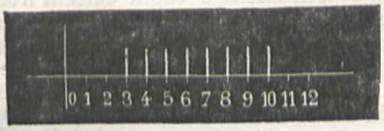

Fig 3.

depends on the number of successive trials, and is more delicate in proportion as they succeed each other more frequently.

3. *Each electric wave presents a phase of suddenly increasing intensity, followed by a phase of gradually decreasing intensity.*

On examining the tracings of electric waves obtained by the electric-magnetic signal, we observe an apparent contradiction between the indication, the wave-duration furnished by this apparatus, and that which we have just seen determined by the frog's foot. The waves traced by the signal of Deprez seem to measure not more than one-hundredth of a second; by Guillemin's method, on the contrary, their duration is much more considerable, being seven-hundredths of a second. This apparent contradiction results from the fact that in the torpedo the waves have not sufficient energy during the whole of their duration to act upon the signal, whereas, from beginning to end of their course they can act upon the frog's muscle, which is much more sensitive. There are, then, in every electric wave, phases of increasing intensity and decreasing intensity which remain to be determined.

M. Marey has endeavoured to obtain a tracing of these phases of variable intensity by a modification of the apparatus of M. Deprez. Instead of limiting the excursion of the style between two fixed obstacles, he allowed it an excursion which varies and is proportional to the intensity of the currents acting upon it.

With this object an india-rubber thread, bent over two bridges, was stretched horizontally between the soft iron bars of the armature (Fig. 5). The bars had a groove filed on the top to receive two demi-cylinders of metal which were soldered to the lower part of the armature. In this way the nearer those parts are brought which are subjected to the magnetic attraction the greater is the resistance. Thus if we consider the armature in its different stages when gradually lowered, first it meets the elastic thread with the two demi-cylinders borne on its lower surface, and then the extensibility of the thread is very great. But as the thread is lowered more and more, it rests on points more and more separated, and becomes less and less extensible. Lower down the india-rubber thread stretched

over the groove made in the soft-iron bars is still less extensible; and finally, when the thread has taken the curvature of the surrounding parts, it opposes any further descent of the armature with the resistance which a stretched thread of india-rubber presents against being pressed or crushed.

This apparatus, to which M. Marey has given the name *electrodynamograph*, has still to receive further improvements, but even as it is, it has already furnished some interesting evidence as to the decrease in volume of the electric waves from the beginning to the end of the discharge; also as to the shape of these waves and the occurrence in the electric tetanus produced by strychnine, &c. Of these different results we shall at present consider only one—the form of a wave is traced by the electro-dynamograph :—an investigation which brings us to the analysis of the wave-phases.

In Fig. 6 the continuous line *b* is the tracing of a single wave obtained with the electro-dynamograph. From it alone we already have evidence that the ascending phase is much more sudden than the descending phase as also takes place in a muscular shock. We can

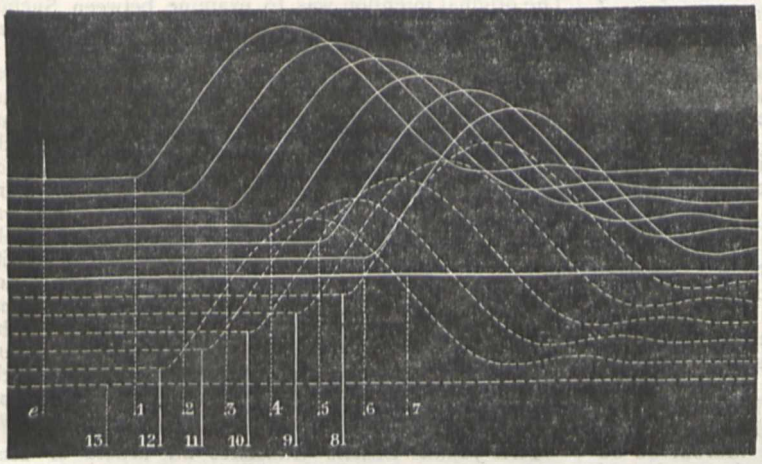

FIG. 4.

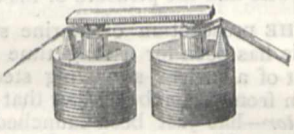

FIG. 5.

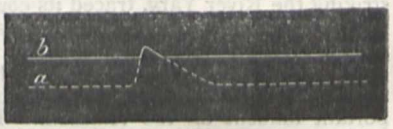

FIG. 6.

further theoretically complete the curve by taking into account what we have learned by Guillemin's method in the preceding paragraph about the duration of a wave. All that is necessary is to produce downwards the two ascending and descending lines till they intercept between them a distance equal to that which represents (on the time-line) the duration of the whole wave. Thus (Fig. 6) the pointed line *a* represents the actual position of the axis of abscissas and that part of the tracing which the instrument was unable to trace on account of its insufficient sensibility.

It is true that this curve is only probable, but there are great presumptions in favour of its reality. The points of origin and termination can determine it experimentally, as we have seen done by Guillemin's method (see 2).

We can now understand the reason of the special characteristics presented by currents induced in a secondary coil by the waves of a torpedo discharge which have been passed through an inducting coil. The phase of sudden increase of each wave is alone capable of giving birth to an inducted current. FRANCOIS FRANCK

(*To be continued.*)

GEOGRAPHICAL NOTES

AT the meeting of the Geographical Society on Monday, Sir H. Rawlinson read a paper, On the road to Merv from the Caspian. After some interesting remarks on the comparative geography of the eastern shores of the Caspian Sea, Sir Henry read some portion of the Russian letters on the earlier stages of the road to Merv, of which a summary appeared in our last issue, and afterwards gave from Russian official documents an account of two ancient cities, the probable relics of Khowrasmian times—Mestorian, or Mestdovran, and Meshed. The former in past ages was one of the most important cities of Central Asia, if one may judge from the remarkable aqueducts leading into it, which were the chief arteries of an entire system of irrigation canals thoroughly watering the whole country, and from the number of its buildings, the remains of which exist to this day. The course of the aqueduct was explored by General Lomakine's orders some two or three years ago, and was traced to the Sumbar, a tributary of the Attrek, a length of some sixty-five versts. The city of Mestorian appears to have consisted of a citadel and of two other inclosures with thick, high walls built of enormous bricks. The mass of the *débris* at the place is so extensive and in such good preservation, that it would be possible, we are told, to make use of it for building a large new town! The bricks, it may be added, are stated to be as hard as stone, and often carved and ornamented with friezes in relief, arabesques, and well-executed inscriptions; the last are sometimes in various colours, illuminated with flowers, and the letters about seven inches in height. Five versts from Mestorian is another remarkable place, known in the country as Meshed; it is, strictly speaking, an ancient necropolis. Here, according to report, is an open coffer holding the sacred books, a hanging lamp, and vases for ablutions, and although in a desert place and wholly unprotected, no one dreams of touching its contents. Sir Henry Rawlinson afterwards dealt at some length with the geography of the country further to the eastward, more especially with that on the northern slopes of the Attock, which is inhabited by three divisions of the Tekké Turcoman tribes.

WE regret to record the death, on Saturday afternoon, at a comparatively early age, of Commander G. C. Musters, so well known as the explorer of Patagonia. His work, "At Home with the Patagonians," is at present the best authority we have on this inhospitable country and its people, and Mr. Musters, as readers of the work know, obtained his information by living with the Patagonians for many months as their "king," and it was only by a *ruse* that he managed to get away from a people who

had learned to adore him. Mr. Musters was a fearless explorer, and a man of unfailing tact and winning manners. Two or three years ago Mr. Musters returned from Bolivia, with which little-known country he had made a thorough acquaintance, and had, we believe, collected material for an interesting work. He recently received the appointment of Consul at Mozambique, and was to have left this month for his post, from which, he was delighted to think, he would be able to do some valuable exploring work in the African interior. By his death Her Majesty has lost a faithful and able servant, and science an eager explorer. Musters was loved by every one who had the pleasure of his acquaintance.

THE prosecution of marine surveys on the coasts of India has been for a long time much hampered by the want of a proper surveying steamer, and we are glad to learn from a Bombay paper that a new vessel—the *Investigator*—has just been launched, which will supply the defect. The steamer is well provided with all the necessary appliances for chart-making, deep-sea sounding, &c.

THE November number of the *Bulletin* of the French Geographical Society contains Dr. Jules Crevaux's account of his exploration in the interior of French Guiana in 1877. Dr. Crevaux, with little assistance and in the face of not a few difficulties, ascended the River Maroni, and striking the River Yary, traced its course to its junction with the Amazon. The two main results of his journey are the crossing for the first time of the Tumuc-Humac chain at the level of the sources of the Maroni, and the discovery and complete delineation of the Yary, an important affluent of the Amazon. The Maroni he describes as a fine river of about 140 leagues in length, with a breadth of 1,200 to 1,500 metres at 20 leagues above its mouth, and from 400 to 500 at 90 leagues. The River Yary, Dr. Crevaux considers as more important than the Maroni; it is 150 leagues long, and both rivers are much obstructed by falls and cataracts. Dr. Crevaux gives some very useful notes on the forests of Guiana and the different species of trees which they contain. The highest summits of the Tumuc-Humac range do not exceed 400 metres above sea-level. In summing up his observations on the geology of the region traversed, he says that all the formations met with from the mouth of the Maroni to that of the Yary have an ancient physiognomy. They are mainly composed of schistose rocks which may be divided into three systems, which are, in order of age—1. The gneiss of the mouth of the Maroni. 2. The schists and mica schists of the middle course of the river. 3. The ferruginous schists and quartzites of the Man-Bari and the Yary; these latter are very wide-spread. All these are frequently traversed by granites and trachytes.

THE possibility of water communication between the Obi and the Yenissei seems to be more and more confirmed by further explorations. Baron Aminoff, after having explored the water-parting between these rivers, arrives at the conclusion that the hydraulic works which would be necessary for the construction of a canal with sluices would not present serious difficulties. The canal would be very short, and the marshes at the sources of the Kas and Yazevaya rivers afford a sufficient amount of water.

THE *Golos* of January 22 says it learns that authentic intelligence respecting Prof. Nordenskjöld's Arctic expedition has been received from Baron Frederichs, Governor-General of Eastern Siberia. According to these advices the steamer *Vega* is ice-bound forty miles from East Cape. The authorities at Jakutsk have been instructed to inform the natives of the dangerous position of the steamer, and to issue a general summons to the people to render assistance to the expedition. At the same time a special expedition has been organised which will attempt to reach the *Vega* by a journey over the ice with the aid of reindeers or dogs. Herr Sibiriakoff has telegraphed to Baron Frederichs, asking him to send a party to the assistance of the Swedish Expedition. He has received a letter from Dr. Lindemann, of Bremen, in which the former says that at the coast where the *Vega* is believed to be lying there is a large native village, and from this village the nearest post of white merchants is distant only about 200 English miles, which may be traversed in winter in three or four days.

A REPORT has been received of a journey by Mr. Baber in the north-west of the Chinese province of Szuchuen. The original intention was to examine, between Suchow and Kiating, the River Tatu, which falls into the Yangtze-Kiang, and then to cross the mountains from Kiating to Fu-lin in long. 103°. At Fu-lin, however, Mr. Baber was induced to extend his exertions into the country further west, and he travelled onwards to a place called Tzu-Tati, the head-quarters of a Sifan chief. Here he heard of the existence of a mountain path to Ta-chien-lu, the French missionary station lying nearest to Tibet. After travelling three days through pine forests, the mountain range was crossed by a snowy pass, and on the northern slope yaks were found grazing, and many slates, inscribed with Sanskrit characters, were noticed. The appearance and language of the people also pointed to the fact that though Tibet proper is many hundred miles west of this point, yet tribes of the Tibetan race and language extend up to the banks of the Tatu River. This confirms the views already expressed by Mr. T. T. Cooper and other travellers.

THE *Japan Gazette* translates an article of some interest from the *Osaka Nippo*, on the subject of Japanese relations with Corea. After commenting on the progress made by Japan during the past few years, the writer remarks that "the Coreans, on the contrary, obstinately cling to the customs of Gio, Wu, and Shin, three Chinese sages who lived 3,000 years ago, and they believe their country is a part of Paradise, next in rank to China, and that western countries are inhabited by barbarians and savages."

COUNT WILCZEK and Lieut. Weyprecht intend to visit the northern coast of Novaya Zemlya in the course of this year, and will remain at that station for a twelvemonth in order to make a series of exact magnetic, electric, hydrographical, and meteorological observations. The cost of the expedition will be defrayed by Count Wilczek himself.

OUR ASTRONOMICAL COLUMN

THE VARIABLE-STAR ALGOL.—Considering the long period during which this star has been under observation, and the many investigations which have been made with the view to reduce its fluctuations of light within some law, much interest attaches to a remark by Prof. Winnecke that the times of *minima* of Algol in the last year have fallen about one hour earlier than those assigned in the ephemerides of variables published by the *Astronomische Gesellschaft*. Dr. Julius Schmidt, director of the Observatory at Athens, to whom we owe the greater number of recent observations on this star, has not yet made known his results for 1878, but we have his determinations of the times of *minima* in 1875-76-77. For comparison with them we may take the last formula given by Prof. Schönfeld in his second catalogue of the variable stars, which appeared in 1875, viz., for Paris mean time:—

Min. ... 1869, Nov. 9, 3h. 39m. 34s. + 2d. 20h. 48m. 53s·67 E.

The following are the differences from the observed times of *minima*, during the last six months of each year, wherein the observations are most numerous:—

1875.	m.
Aug. 20 ...	+10·1
,, 23 ...	−29·6
Sept. 9 ...	− 7·2
,, 12 ...	−15·8
Oct. 5 ...	+ 7·1
,, 28 ...	+17·2
Nov. 20 ..	−15·1

1876.	m.
July 29 ...	+26·1
Aug. 21 ...	+ 7·2
Sept. 10 ...	+ 9·4
,, 13 ...	+21·9
Oct. 3 ...	+ 3·8
,, 6 ...	+ 1·8
,, 9 ...	− 1·2
,, 26 ...	+52·0
Dec. 11 ...	+53·9

1877.	m.
Aug. 20 ...	+62·1
,, 23 ...	+18·5
Sept. 12 ...	+33·3
,, 15 ...	+17·1
Oct. 8 ...	+54·0
Dec. 10 ...	+70·1
,, 13 ...	+30·6

If yearly means of the above differences are taken we have :—

	m.
1875·76	− 4·8
1876·76	+19·4
1877·73	+40·8

These figures appear to indicate that a perturbation of the period is taking place, which of late has increased the differences between observation and calculation at the rate of about twenty-two minutes annually; the star is evidently one which deserves close attention at the hands of those observers who are following up the variables. With reference to previous observations of Algol and results derived from them, the reader will best consult Argelander in the seventh volume of the Bonn observations, and Schönfeld in *Vierteljahrsschrift der astronomischen Gesellschaft*, vi. p. 60.

THE REAPPEARANCE OF BRORSEN'S COMET.—It is notified that M. Tempel, director of the observatory at Arcetri, Florence, detected the short-period comet of Brorsen on January 14, in a position north following the nebula No. 4900 of Sir John Herschel's General Catalogue. The ephemeris by Dr. Schulze, of Döbeln, who has carried on the calculation of the perturbations from the last appearance of the comet in 1873 to the present year, does not commence until February 19, so that it appears to have occurred to M. Tempel that, with his advantages of climate and optical means, there was a possibility of an earlier observation of the comet, and he has taken steps to that effect accordingly. Dr. Schulze's elements for 1879 give for the place of the comet on January 14, at 6h. M.T. at Florence, R.A. 23h. 10m. 38s., N.P.D. 118° 57′, which is north—following the nebula named, so that there is no reason to doubt that the object observed, which is described as small, but brighter than the nebula (one of Sir W. Herschel's second class) is really Brorsen's comet. In this case, however, M. Tempel has succeeded in observing the comet, when, according to theory, it possessed a much less intensity of light than at any previous observation. At the time of his observation it would be distant from the sun 1·42, and from the earth 1·915, whence the theoretical intensity of light, represented by $\frac{1}{r^2\triangle^2}$, is 0·135; the smallest value with which it had previously been observed corresponded to the last glimpse of the comet at Berlin on June 22, 1857, viz., 0·337. Dr. Schulze's ephemeris will be found in No. 2220 of the *Astronomische Nachrichten*, commencing, as we have said, on February 19. On February 7 the comet's position at 6h. G.M.T. is in R.A. 23h. 59·9m., N.P.D. 109° 42′, and on February 11, at the same hour, in R.A. 0h. 9·9m., N.P.D. 107° 46′.

SUN-SPOTS AND THE NILE

ON the 21st instant Mr. Francis Cobb read a paper on the financial and economical condition of Egypt, at the Society of Arts, in which he of course referred to the periodical rise of the Nile, and spoke of the desirability of discovering some system in the variations of this rise. Mr. Cobb, in considering this subject, has been naturally drawn to an examination of the sun-spot period, and has attempted to discover if any relation exists between this period and the variations in the rise of the river. The period of Mr. Cobb's examination extends from 1866 to 1878, and as might have been expected, he finds no relation whatever between any sun-spot maxima and minima, and the maxima and minima of the Nile floods. The years 1866-67 were sun-spot minima; the rise of the Nile in these years was 28¼ and 24½ feet respectively; 1872 was a maximum sun-spot year, and the rise of the Nile was 25½ feet; 1877 a minimum sun-spot year, with 18 feet rise in the river; last year the rise was 30 feet. As some of the speakers in the discussion remarked, there is at present a desire to find relations between the stupendous cosmical phenomenon of sun-spots, and terrestrial occurrences, without considering local peculiarities. We do not know what might be the result if the records of Nile floods for a century were obtainable, and were compared with the various sun-spot periods during that time; but we should say beforehand that in considering so literally narrow an occurrence as the rise of the Nile, many local considerations would have to be taken into account.

"It is impossible to say," Mr. Cobb went on, "that the rule, maximum spots, maximum rainfall, applies to Egypt. The cause of the irregularities of the Nile must clearly be looked for locally, the Blue Nile and Nyanza lakes having probably more to do with the matter than sun-spots. The telegraph, combined with a vigilant series of the operations of the Upper Nile, especially at the confluence of the Blue Nile, will prove more reliable for the protection of Egypt another year than any calculations based upon solar physics."

We fear Mr. Cobb has but a vague idea of the application of solar physics to meteorology and other terrestrial phenomena. A perusal of the many letters which appear in NATURE from our Indian meteorologists, will show that without a careful consideration of local and regional conditions no deduction drawn from sun-spot periods *per se* are of much value.

In the discussion which followed Dr. Mann endeavoured to draw the attention of the meeting to the science of the subject. While he apparently endorsed Mr. Cobb's opinion that the spots on the sun would not be found to have any direct relation to the high and low Niles, he thought it would be perhaps as well to state exactly how this matter stood. The last development of the search after sun-spot influence, Dr. Mann said, took the form of the discovery that the constantly recurring financial crises in this country were due to the sun-spots; and he should like to point out what really was the influence of the sun upon the great physical changes going on in the world. There was no doubt that the presence of sun-spots had relation to the amount of force and energy issuing from the sun, and that when spots were abundant more solar energy was thrown out into space. When that was the case, the earth shared with all the other orbs in getting some increased force from the sun. There was no doubt either that movement of every kind on the earth was dependent on solar action; and when increased energy was thrown out from the sun it told immediately on the water of the earth, and raised more of it into the sky in the form of vapour. But this did not mean that there would be an increased rainfall in one particular spot, but only that, being more vapour, there would be a greater rainfall over the whole earth. In a case like Egypt, the amount of rainfall was due to the presence or absence of an ocean wind blowing over the high grounds of Abyssinia. Therefore, though no doubt the sun-spots had to do with the total rainfall, they had not necessarily anything to do with the local rainfall in one particular country like Egypt.

Dr. Mann explained that in these remarks he did not intend to imply that there was not a periodicity and regular order of some kind in social conditions and events which were connected with the requirements of finance, crisis, and things of that kind. He was quite satisfied that there was. But he thought there was too great an

inclination to refer locally restricted events to large general causes.

Mr. Hyde Clarke, who was in the chair, drew attention to the fact that it was by a paper of his thirty years ago that public attention was first directed in what he might term a scientific form to this periodicity. Prof. Stanley Jevons, who was the great advocate for the application of the sun-spot theory to commercial crises, had reproduced the statements he made thirty years ago, and thus fresh attention had been called to them. For his part, he was no advocate for what was called the sun-spot theory, for he believed the sun-spots had no direct bearing on the periodicity of commercial crises, or upon the height of the Nile; but as what Dr. Mann had said might appear to throw discredit on the periodicity of crises, he would briefly revert to the facts to which he had formerly called attention. He had then gone through the corn harvests, as shown by the prices in England for the last 400 years, for which data could be obtained, and his observations, which had since been repeated by Prof. Jevons, gave a series of facts over six centuries, showing that there was a periodicity in the crops, and consequently in the commercial phenomena dependent on them, of somewhere about ten years. Prof. Jevons had fastened on to that one fact, but had not referred to other observations he had made, which gave the clue to the question Mr. Cobb had raised, whether it was possible to predict these periods. There was certainly, in a long period, a periodicity of about ten years, and if you laid out a diagram you would find this plainly shown, but yet in some places the lines of dearth or plenty would seem to come in the wrong place, and no one has yet been able to hit on the true law. He had stated that, as far as he could discover from the facts before him, there were, besides the periods of ten years, other periods of about twenty-six years, and likewise a period of about 104 years, and the opinion he formed was that these longer periods interfered with the shorter ones, and prevented any absolute calculation as to the future. At the same time the observation of these phenomena was not by any means an idle matter; there was this practical lesson to be drawn from it, that in periods of prosperity we must look forward to a period of adversity and prepare for it. Therefore the observation of Governments, and of the commercial community and financial institutions should be directed to these great phenomena of nature, which, after all, did govern the individual operations of man.

And this is all we contend for. That there is a connection between certain well-known cosmical phenomena, centring in the sun-spot period, is admitted by all whose researches give them a right to pronounce an opinion on the subject. What is the exact nature of this connection has yet to be discovered, though that we are on the road to it every careful reader of NATURE must admit. The immense social and economical results depending on the definite ascertainment of this connection make it the bounden duty and the interest of civilised Governments to do all in their power to further research in this direction, and we have no doubt that when the full truth is known it will be found that even the apparently capricious Nile is obedient to influences that may be regarded as ultimately cosmical.

NOTES

WE are pleased to see a suggestion in the *Midland Counties Herald* that in considering the arrangements for the restoration of the Reference Library, recently almost destroyed by fire, the authorities will not miss the opportunity they now have of supplying an omission in the public institutions of Birmingham, by organising a Natural History Museum, of equal value with the Reference Library which they are doing their best to restore. We heartily endorse this suggestion, and indeed it seems strange that so energetic and intelligent a town as Birmingham, with one of our most enterprising Natural History Societies in its midst, should not have had such an institution long ago. We are sure the matter only needs to be properly brought before the authorities and the citizens to have the blank speedily and properly filled up.

MR. JOHN SADLER, so long assistant to Prof. Balfour, has been appointed to succeed the late Mr. McNab as curator of the Royal Botanic Gardens, Edinburgh.

IT is expected that Russian Turkestan will be very well represented at the anthropological exhibition which will be opened next summer at Moscow. We may already mention a very interesting collection of some dozens of skulls, found at Samarkand and belonging to a very remote epoch. A collection of dresses and implements of the inhabitants of the Zarafshan valley will be accompanied by a collection of ethnographic photographs; and among the inhabitants of this valley, the photographs and the skulls from the Galchi tribe will probably draw the special attention of the scientific world. This tribe, which lives in the clefts of the Hindu-Kush at the sources of Zarafshan river, differs from all other Central Asian tribes, and is said to be the remnant of the army of Alexander the Great; indeed, its features are like those of the Greeks; but the tribe remains almost quite unexplored, because of their wildness and the insecurity of travel in those regions. Altogether, the Zarafshan district sends to the exhibition plenty of very valuable anthropological and ethnographical materials.

THE unveiling of the Humboldt monument in Tower Grave Park, St. Louis, U.S., took place on December 24 last. The monument, as our readers will remember, is cast in bronze and executed after the design of the eminent German sculptor, Herr Ferdinand von Miller.

THE Berlin Humboldt Academy, founded by the Scientific Central Union of that city, was inaugurated on January 13 last.

THE competitive examination held at the Paris Conservatoire des Arts et Métiers for the appointment of a Professor of Physics and Meteorology to the National School of Agriculture is said to have been very brilliant. It has ended by the appointment of M. Duclaux, Professor to the Faculty of Lyons, who was trained by M. Pasteur.

THE *Times* Paris Correspondent telegraphs on January 24 that the eruption of mud at the foot of Mount Etna was still going on, but with varying intensity. For two days after the earthquake of the 24th ult. it was considerably stimulated, but it has since slackened, and the mud is more watery. An area of 7,000 square metres is already covered.

A CORRESPONDENT of the *Colonies and India*, writing from Wellington, New Zealand, on December 7, says that a most important discovery of graphite has just been made in the back portion of the province. The Colonial laboratory has received specimens from boulders found in a creek, and these prove to be the purest and most compact samples yet discovered in the Colonies. The value of the discovery is enhanced by the fact that the existence of coal in immediate proximity is thus indicated. In another spot, between Westport and Keefton, an extensive limestone cave has been discovered, and it is stated that it is traversed by a creek yielding good payable gold. The Geological Survey is being steadily pushed on, and Dr. Hector is now attempting to work his way to Waikato, in order to gather information as to the geology of that hitherto unexplored region.

MESSRS. LECHERTIER, BARBE, AND CO., of Regent Street, have sent us a wonderful shilling moist colour-box, which, in utility and the quality of the colours, surpasses anything we have seen.

It is of japanned tin, can be put in the pocket, has every convenience for immediate use, and is altogether excellent and astonishingly cheap. It is a pity that students of science generally don't know how to use colours to give clearness to their note-books and diagrams.

MR. BRYCE WRIGHT has just issued a catalogue of his extensive mineralogical, geological, conchological, and archæological specimens and collections, with several well-executed illustrations. Those interested in the subjects mentioned would do well to obtain a copy of the Catalogue.

MR. GOWER, an American, who has lectured in America with Mr. Bell on the telephone, has realised an interesting improvement on Bell's instrument. The new telephone differs mainly in the form of the magnet, which has been calculated so precisely that the sounds can be heard at any distance from the speaker in a large room. The telephonic current is so powerful that the contact of a magnet can be worked by it, and a signal given in a central telephonic office.

HERR PETZOLD, of Vienna, sends us several specimens of insects preserved apparently in Canada balsam and mounted on microscopic slides, which surpass in several respects anything of the kind we have seen. Herr Petzold informs us that for years he has been working to devise some means of preserving insects and other small animals in such a way as to prevent their being injured by accident or by any of the numerous enemies of museum collections. By a process of mummification, and inclosure in a transparent material he seems to us to have succeeded. The specimens sent can be clearly seen, are natural and life-like in appearance, and completely protected from all atmospheric influences.

THE fall of sleet which occurred in Central France on January 23 was so terrific that an immense number of large branches were broken by the weight of icicles adhering to the leaves. Almost all telegraphic communication between Paris and Central or Southern France was interrupted by the breaking of the telegraphic wires. The messages from Paris to Marseilles were sent *viâ* London, Lisbon, and Malta. This state of things, which had been anticipated by *Électricité*, raised a number of angry remarks from the principal papers. It is very likely that the German system of entombing the wires of the large lines will be resorted to, and special credits asked from the Chamber of Deputies next session.

THE Postal Microscopical Society is not, as its name would seem to imply, a Microscopical Society for Post Office officials. It embraces a much wider constituency, being commensurate (potentially) with the area of the kingdom embraced by our postal service. It is, in short, an association for the distribution by post of microscopical slides among its members, with facilities for these members making remarks on the slides they receive. From the Report of the fifth annual meeting we see the Society has many members over England, that the organisation is excellent, and works well. Several improvements are contemplated in the sphere of this Society. In consequence of a number of medical men having recently joined the Society, it has been arranged to circulate a special series of histo- and pathological slides. These special slides will circulate almost exclusively amongst the medical members, in addition to the usual fortnightly box of slides which goes the whole circuit of the Society, whether members are medical or otherwise. It is also proposed at the request of many members, to circulate a series of slides devoted to botanical subjects; these, after going the round of the contributors, it is proposed should go the whole circuit of the members. Those desiring to join the Society should apply to Mr Alfred Allen, 1, Cambridge Place, Bath.

AN occasional correspondent of the *Daily Press* of Hongkong, gives a somewhat melancholy account of the condition of affairs in Formosa, where it was hoped that the Chinese were showing signs of progress. The Woosung Railway plant, he says, continues to generate rust, the dredger so urgently needed has not been ordered, and the scheme for introducing Swatow coolie emigrants has fallen through. The accounts of the Kelung Colliery are not hopeful, sickness having prostrated European and native miners alike; in the petroleum region, too, all the members of the exploring party are reported to be laid up with fever. What is worse, however, in that quarter, is, the boring rod has snapped low down, and the American experts are said to have spent three weeks in a vain endeavour to connect it again. The only favourable item of news is that there is every prospect of a large sugar-crop in the south of the island.

A CONTEMPORARY in China states that petroleum is obtainable at several places in both North and South Formosa. Some time back a large spring was discovered in the hills to the south-east of the Port of Owlan, in about 24° 30′ N. lat. and 121° E. long. The principal spring is situated close to the Owlan River, at the foot of a hill. At certain times of the year the river overflows into this spring, and the oil is carried away down the stream. When the discovery of this well was made, the oil could be tasted in the water some distance off. On exploring the hills behind the spring, large fire-holes were found, and a small spring was met with on the top of a high hill. The lofty ranges of hills to the east of the petroleum valley have been explored to some extent, and in almost every range were found evidences of the existence of coal, but none of the veins or seams are being worked.

WE have received the first two numbers of the Italian *La Natura*, a weekly scientific journal, which we understand is the new form of *L'Elettricita*, founded some time since. *La Natura* is mainly devoted to the physical sciences, and, judging from the first two numbers, is likely to take a creditable place among its scientific contemporaries. In the first number Prof. Schiaparelli writes on the Perturbations communicated by Jupiter to Brorsen's comet in 1872, and in the second number on Recent Researches on the Topography and Physical Constitution of the Moon. There are several other good papers relating both to Italian and to foreign science.

Revue d'Hygiène et de Police Sanitaire is the title of a new sanitary monthly edited by Prof. E. Vallin and published by Masson of Paris.

THE *Annuaire* of the Bureau des Longitudes for 1879 contains as usual a great mass of useful and well-digested information. It contains an interesting paper by Dr. Janssen on Recent Advances in Solar Physics.

WE are glad to learn from the *Royal Gazette* of British Guiana that a Bird Ordinance has been promulgated in that colony, which is likely to prevent the stamping out of birds whose feathers are so eagerly sought after by ladies to add to their charms.

WE have received the first number of the *Revue Mycologique*, a three-weekly journal devoted to the subject of fungi, and edited by M. C. Roumeguère. The Paris publishers are Baillière and Sons.

IN the course of a recent excavation for a railway from Persan to Neuilly-en-Thelle, in the north of France, a field has been cut, which contains numerous sepultures, and was probably used as a cemetery at some early period. Nearly all the tombs (M. Millet tells us, in *La Nature*) are of hard stone and composed of two pieces (exceptionally three), with transverse joints, and the cover generally a single piece. One tomb is made of plaster.

Various objects have been met with, arms and armour, vases, ceramics, &c., and in one tomb, it is said, a warrior has been discovered fully equipped, and in such preservation that the beard was intact. M. Millet recalls the fact that in the invasion of Gaul by Julius Cæsar, there was a camp of great importance on the plateau above Gouvieux, some ten kilom. from the place of excavation, and on the route from Chantilly to Persan; the place is still known as Cæsar's Camp. Numerous battles took place in the valley of the Oise, as is attested by the medals, coins, &c., often found by farmers in that region. One of these combats was so murderous that the place where it was probably fought bears the name of *Pres de tuerie;* it is at the foot of Beaumont-sur-Oise. The excavations referred to are still in progress, and will doubtless be watched with interest.

THE annual general meeting of the Manchester Field Naturalists' and Archæologists' Society was held on the 21st inst., Mr. John Angell, F.C.S., vice-president, in the chair. Mr. Alfred Griffiths, secretary, read the report for the past year, which stated that 1878 had been devoted to the aims of the Society, with an average success. Mr. Angell gave an address on the science of 1878, in which he reviewed, in an intelligent and appreciative manner, some of the main scientific points of interest during the past year.

THE Geological "Landesanstalt" and Mining Academy at Berlin has recently been considerably enlarged. The institution has moved into new buildings which have just been completed, and which contain a rich collection of maps, minerals, rocks, fossils, &c., besides a large library and laboratories for geological, analytical, metallurgical, and technological work.

THE existence of a subterranean oak forest in the neighbourhood of Rotenburg, Prussia, was proved last summer by the State geologist of that district, Dr. Moesta, of Marburg. The investigations of this gentleman have shown that in the plain of the Fulda valley an oak forest lies buried at a depth of some two or three metres, the origin of which dates back to the tertiary period perhaps, and of which the river Fulda has laid bare many traces by erosion. The wood of the oak trees thus brought to light has by the long action of the water been stained quite black, but still retains considerable firmness. The size of the trees is very considerable, and it remains yet to be proved whether they belong to the same family as the oaks now existing.

A GERMAN paper states that a descendant of the great Copernicus is living now at a small town of Posen, exercising the calling of shoemaker. It is known that Copernicus was a canon of the chapter of Frauenburg, and must be supposed to have died without leaving any issue. It has been said that his true son was Kepler, and that in his turn Kepler was the scientific father of Newton.

THE additions to the Zoological Society's Gardens during the past week include an Entellus Monkey (*Semnopithecus entellus*) from India, presented by Mr. J. Mills, R.H.A.; two Prairie Marmots (*Cynomys ludovicianus*) from North America, presented by Miss Agneta B. Dykes; four Common Gulls (*Larus cana*), a Common Widgeon (*Mareca penelope*), four Grey Plovers (*Squatareola helvetica*), three Knots (*Tringa canutus*), a Dunlin (*Tringa cinclus*), European, presented by Mr. F. Cresswell; a Blue and Yellow Macaw (*Ara ararauna*) from South America, presented by F. G. J. Lillingston, Lieut. R.N.; two Coypu Rats (*Myopotamus coypu*), a Brown Coati (*Nasua nasica*), a Chilian Sea Eagle (*Geranoaëtus melanoleucus*), a Dinca Finch (*Dinca grisea*), two Saira Tanagers (*Pyranga saira*), two Dark Green Maize Eaters (*Pseudoleistes virescens*), two Blue-bearded Jays (*Cyanocorax cyanopogon*) from Buenos Ayres, two Garden's Night Herons (*Nycticorax gardeni*), an Ariel Toucan (*Ramphastos ariel*), a White-bellied Thrush (*Turdus albiventris*) from Bahia, a Great Frigate Bird (*Fregata aquila*) from Pernambuco, a Brazilian Blue Grosbeak (*Guiraca cyanea*) from Mexico, purchased; two Cuming's Octodons (*Octodon cumingi*) from Chili, deposited.

EARLY EXPERIMENTS ON THE CONDUCTION OF ELECTRICITY BY SUBMARINE WIRES FOR ILLUMINATING DISTANT PLACES AND PROPOSALS FOR THE DIVISION OF THE LIGHT INTO SEPARATE LIGHTS

I DO not profess to be acquainted with the means which have been recently employed for conveying electricity to illuminate places at a distance or for sub-dividing the electric light, nor is it with the slightest wish to derogate from the merit of recent inventors that I now submit a few facts as to earlier labours in the same field which may perhaps be interesting to the readers of NATURE.

So far as I know, the first suggestion of communicating electricity for lighting purposes to distant places was in the fourth volume of the *Trans.* Roy. Scott. Soc. of Arts, vol. iv., 1854. In describing the apparent light on a sunken reef in the sea at the entrance of Stornoway Loch, which was lighted in 1851, I stated that "it occurred to me that in some cases gas-pipes might be laid or even *submarine wires, so as to illuminate a lantern placed on a beacon or buoy.*" I did not, however, consider it safe, "at least in the present state of our knowledge," to adopt either of these plans; but gave the preference to an apparent light illuminated by a beam of rays projected from a lens placed on the shore at a distance of 530 feet from the sunk rock, which plan has been in use since 1851.

In 1852, and therefore not long after the erection of the Stornoway light, Admiral Sheringham used electricity for producing *heat* for the purpose of igniting gas at a buoy.

My friend, Mr. Alan Brebner, C.E., suggested, as referred to in Messrs. Stevenson's Report on the electric light in 1865, that the lighthouses of Scotland might be illuminated from one great central station.

In 1865 I made experiments for the Commissioners of Northern Lighthouses with the sanction of the Board of Trade, on lighting beacons by submarine wires, and on the suggestion of my friend, Prof. Swan, increased the flashes by combining a Leyden jar with an induction coil. On January 13, 1866, I communicated to the Secretary of the Roy. Scott. Society of Arts that the induction spark placed in the focus of lighthouse apparatus gave in all respects satisfactory results at the distance of half a mile, which, owing to intervening objects, was the greatest distance from which it could be seen. The primary current was also kept for a week passing continuously night and day, through 800 feet of wire without any sensible waste of the platinum electrodes. I next attempted to pass the current through a cable under the sea, but without success, when Messrs. Stevenson applied to Dr. Siemens for his assistance in the matter, and he recommended an electro-magnet on the beacon with a contact lever actuated by the armature of the electro-magnet in the manner of a Neft's hammer. The luminous effect was increased by the deflagration of mercury. This plan, as tried at Granton Harbour, was quite successful; but the products of combustion were deposited on the optical apparatus, and some mechanical difficulties interfered with its continuous working.

Being thus thrown back on the old plan of the induction spark, I was enabled to overcome the difficulties by the following expedients:—Mr. Brebner suggested placing the induction coils with condensers close to the optical apparatus on the beacon and the battery and contact breaker on the shore, so as to pass only the primary current through the cable. Mr. Hart, electrician, also designed an improved break for the purpose, and Prof. Tait recommended the enlargement of the earth terminals. By these arrangements the current was passed successfully under the sea. The experiment was repeated at Granton, at the request of the Trinity House of London, in presence of Captains Fenwick and Nisbet, and Mr. Douglass, the engineer, accompanied by Mr. Farrer and Mr. Shaw Lefevre of the Board of Trade.[1] The distance between the battery and break on Granton Pier and the induction coils and optical apparatus on Newhaven Pier is

[1] "Proposals for the Illumination of Beacons and Buoys," by T. Stevenson, p. 14. (Edinburgh: A. and C. Black, 1870.)

upwards of half a mile, but the actual length of wire which was submerged, and through which the current passed, was upwards of a mile. The spark was about ¼ inch in length, bluish white in colour, and very striking and interesting in effect. It was placed in the focus of a holophotalised parabolic reflector.

At the same time in order to ascertain if by means of a single battery under the charge of one keeper a succession of flashes could be produced and a string of isolated dangers illuminated, the light was sub-divided first into two separate flashes and afterwards into six different sparks. The separate lights were quite satisfactory, though they were not as might have been expected of the same power as the original single one. But as the separate sparks were very close to each other this cannot, I presume, be held to be a proper sub-division of the light.

In 1867, at the British Association at Dundee, I suggested that the "effect of the light might be also increased without using additional cells if the same current could be again utilised so as to generate a second spark in the same focus. This was proposed to be done by 'using additional coils' for the same focus or separate sparks in the foci of separate reflectors.[1] I also added that the "time is perhaps not far distant when the beacons and buoys in such a navigation as the entrance to Liverpool may be lit up by submarine conduction from a central station on either shore, while the whole management may be trusted to the charge of one or two light-keepers."[2]

I may add that similar trials were made with Wilde's electromagnetic machine, which gave a light of much greater volume and power. The electrodes employed in all these experiments were made of platinum, but several other metals were experimented with, and of all that were tried bismuth was found to give the brightest light.[3]

A committee of the Scottish Society of Arts, consisting of Dr. Ferguson, convener, Dr. Lees, and the late Dr. Strethill Wright reported in the following terms:—"The peculiar character of the light, which is flickering, though continuous, is well marked and would be easily understood. So far as Mr. Stevenson's experiments go, they seem to prove the practicability of his proposal, and your committee do not anticipate any serious obstacle to its realisation."

THOMAS STEVENSON

Edinburgh

UNDERGROUND TEMPERATURE[4]

DR. STAPFF has continued his observations of the temperature in the St. Gothard Tunnel, and has contributed to the Swiss Natural History Society a paper[5] of fifty-six quarto pages, embodying the results.

The following is his description (pp. 26, 27), of the mode of observing the temperature of the rocks in the tunnel:—

"The exact determination of the temperature of the rocks in the tunnel formerly occasioned a notable expenditure of time and money. At first thermometers about a metre long (made by J. Goldschmid, of Zurich) were employed for this purpose; their tubes being cemented into a wooden cylinder, so that only the bulb (surrounded by a perforated steel cap) projected below, and the scale (extending from 15° to 30° C.) above. Tallow was poured round the wooden cylinder, and the whole thermometer was then thrust into a bore-hole a metre deep, so that only the scale projected, from which readings were taken from time to time until the temperature became constant. The final reading had to be corrected not only for rise of zero but also for the temperature of the quicksilver in the thermometer tube which extends from the opening to the bottom of the bore-hole. Another very notable correction was required for the more or less oblique position of the thermometer; for the hydrostatic pressure of the quicksilver presses out the glass bulb so far that without change of temperature the long thermometer reads from 0°·4 to 1°·0 less in the vertical than in the horizontal position.

"After about from three to ten days, the reading of a thermometer luted into a bore-hole ceased to alter.

"Separate trials with thermometers of similar construction, but different length, showed, moreover, that, after months, the temperature of the rock at about a metre deep was still unchanged. This is obviously owing to the small difference of temperature between the rock and the surrounding air.

"From the observations at No. 8 and No. 15, in Table III., it is seen that the temperature at the bottom of the bore-hole was sometimes a little lower and sometimes a little higher than nearer its mouth.

"This mode of observing gave correct results, but was laborious and costly, not only on account of the necessity of making special bore-holes for the purpose, but because almost every experiment cost a thermometer. The projecting end was often maliciously broken off, and on account of the swelling of the wooden case it almost never happened that at the end of an experiment a thermometer was drawn out again uninjured.

"Hermann and Pfister remedied this latter evil by surrounding the thermometer-tube from the bulb to the scale, with a glass case, and this with a steel jacket. This arrangement, however, involves not only conduction through the steel, but also continual interchange of heat by currents of air in the glass case, from the mouth to the bottom of the hole. For these reasons the observations made with these thermometers could not be employed without intricate corrections.

"Later I tried a Thomson's maximum thermometer,[1] kindly placed at my disposal by Prof. Everett, which (after previous strong cooling) was left for several days at the bottom of the bore-hole, closed air-tight. The results agreed with those obtained by other methods; but who can guarantee that the higher temperature prevailing in a newly-bored hole is always just so much depressed by the cold mass of the thermometer and its copper case, that the rock-temperature alone determines the final indication of the maximum thermometer?

"This consideration induced me to employ for rock-temperature observations (and they also serve for air and water observations), the above-mentioned short thermometers with insulated bulbs, the first of which Prof. Everett caused to be made by Negretti and Zambra for this express purpose. These thermometers, inclosed in a metal box provided with a handle, are thrust to the bottom of the bore-hole, which is at least a metre deep. To the handle is fastened a strong cord reaching to the mouth of the hole, by which it can be drawn out again at the end of the trial. The bore-hole, from the thermometer to the mouth, is stopped with greased rag or other similar material, as air-tight as possible. After two or three days, the thermometers have usually assumed the temperature of the surrounding rock, that is to say, their reading has ceased to alter. The insulation of the quicksilver prevents alterations during the drawing-out and reading of the thermometer. The correctness of the result is in no way prejudiced by sediment from the boring which may yet remain in the hole. The pouring in of some water may even be useful in accelerating the experiment. Wet bore-holes with standing-water are, however, to be avoided, because rock-temperature and water-temperature are not identical.

"In the manner last described, at every available opportunity, that is to say, when the work of the tunnel is from any cause compelled to cease for a few days, rock-temperature observations are now instituted in bore-holes ready to our hand. The observations are simple, give exact results if taken with proper precaution and sufficient duration of the experiment, and cause no further expense, since the thermometers, being sunk in the rock, are secured against wanton injury, and there are always bore-holes available."

Dr. Stapff further states by letter that the two original thermometers supplied by Negretti and Zambra having been broken, he has had others made, in which he has introduced the improvement of hermetically sealing the outer glass case, instead of closing it with a waxed cork, which gradually admitted moisture.

In the Report for 1876 an account was given of the observations of Herr Dunker in a bore about 4,000 feet deep at Sperenberg, and allusion was made to the undue weight which had been attached by some writers to the empirical formula in which Herr Dunker sums up his observations; a formula which indicates a retarded rate of increase, and, if extended to greater depths, leads to the conclusion that the temperature reaches its maximum at the depth of about a mile.

A discussion has been carried on in Germany on this subject,[2]

[1] British Association Reports, 1867. [2] *Ibid.*

[3] "Proposals for the Illumination of Beacons and Buoys," pp. 14-15. (Edinburgh: A. and C. Black.)

[4] Eleventh Report of the British Association Underground Temperature Committee, by Prof. Everett.

[5] "Studien über die Wärmevertheilung im Gotthard," 1 Theil. "Der schweizerischen naturforschenden Gesellschaft zu ihrer sechzigsten Jahresversammlung in Bex gewidmet," von F. M. Stapff. Bern, 1877.

[1] It was one of the protected Negretti maximum thermometers constructed for the Committee.

[2] See papers by Mohr, Heinrich (two papers), Dunker, and Hottenroth, in the "Neues Jahrbuch" for 1875, 1876, and 1877; by Brauns, in the "Zeitschrift für die gesammten Naturwissenschaften," 1874, p. 483; and by Hann, in the "Zeitschrift der österreichischen Gesellschaft für Meteorologie," 1878, p. 17.

chiefly in the "Neues Jahrbuch für Mineralogie," &c., and the best authorities seem to be unanimous in rejecting the hypothesis of a retarded rate of increase in the earth's surface as unwarranted, either by the Sperenberg observations or any others. Herr Dunker himself concurs in this opinion. Dr. Stapff also, though some of his own empirical formulæ indicate a retarded rate of increase, writes to Prof. Everett in the following terms:—"As to my formulas, I beg you to remember that they are not constructed for expressing laws of Nature. They simply are made for facilitating the view over a heap of figures and data of observation. And generally I beg you to be sure that those formulas, in my mind, cannot express any law for the increase of warmth at greater depths than those in which the tunnel observations were made. The formulas give good means for eliminating empirically some of the influences of the shape of surface which occur in the profile of the mountain."

Mr. W. Galloway, one of H.M. Inspectors of Mines, has taken observations in Fowler's Colliery, Pontypridd, South Wales. The shaft is 846 feet deep, and the air current down it amounts to between 20,000 and 30,000 cubic feet per minute.

In order to determine the normal temperature of the coal, a hole 1½ inch in diameter was bored in the side of a narrow place that was being rapidly driven in the solid coal. The hole was bored in the very face, to the depth of 4 feet. The thermometer (one of the committee's slow-action non-registering instruments) was placed at the inner end; then a wooden cylinder of nearly the same diameter as the bore-hole, and 9 inches long, was pushed in until it came in contact with the copper case of the thermometer; and lastly a wooden plug, wrapped round with cloth, was driven firmly into the mouth of the hole. The thermometer was at 58° F. when it was put into the hole, and after remaining there from 2 P.M. on August 25, 1876, to 3.45 P.M. on the following day, it stood at 62°·7. There was no water whatever in the hole, and the depth below the surface of the ground was 855 feet.

The circumstances of this observation seem to preclude any considerable disturbance of the normal temperature; and combining it with the mean annual temperature at the surface, which is said to be 51°·5, we have an increase of 11°·2 F. in 855 feet; which is at the rate of 1° F. for 76 feet.

Two other observations were taken in other parts of the mine. They are not directly available for the purposes of the Committee, but were intended to test the influence of air-currents on the temperature of the coal; and they show variations of 2° or 3°, according to the season of the year.

Observations are being taken for the Committee by Mr. G. F. Deacon, Borough Engineer of Liverpool, in a bore which has attained the depth of 1,004 feet, in connection with the Liverpool Waterworks at Bootle.

The temperature at this depth is 58°·1. The observation nearest the surface was at the depth of 226 feet, the temperature at this depth being 52°. We have here a difference of 6°·1. in 778 feet, which is at the rate of 1° for 128 feet, and the same rate is approximately maintained throughout the descent. For instance, at 750 feet, the temperature was 56°, which gives 1° for 131 feet by comparison with the depth of 226 feet, and 1° for 121 feet by comparison with the bottom.

The bore is 24 inches in diameter, and the observations were taken with a protected Phillips's maximum thermometer every Monday morning. The operation of boring was continued up to twelve o'clock on Saturday night, and was not resumed till the temperature had been taken on the following Monday. The time that the thermometer remained at the bottom was not less than a quarter of an hour, and was sometimes half an hour.

The rock-formation consists of the pebble beds of the Bunter or lower trias, and most of it is described as hard, close-grained, and compact. The speed of boring is indicated by the dates of the observations at 226 and 1,004 feet, the former being November 12, 1877, and the latter August 12, 1878. A month was lost by the jamming of the drilling tool, in May and June, 1878, when a depth of about 890 feet had been attained.

The depth from the surface of the ground to the surface of the water in the bore has gradually decreased from 66 feet, when the bore was at 318 feet, to 52 feet, when the bore was at 800 feet, and to 51·1 feet, at the present depth. It would thus appear that the inflow of water from below has increased with the depth attained. There is a slow percolation from the upper part of the water-column to an underground reservoir near at hand, the top of the water-column being considerably higher than the top of the water in the reservoir. Mr. Deacon remarks that the slow upward flow which supplies the water for this gradual discharge is favourable to the accuracy of the observations (which have always been taken at the bottom), by checking the tendency of the colder and heavier upper water to descend and mix with the lower. As bearing on the subject of the disturbance of temperature by the stirring of the water in boring, as well as by the generation of heat in the concussions of the tool, it may be mentioned that the last observation before the month's interruption by the jamming of the tool was 57°·5, at 886 feet, and the first observation after the extraction of the tool was 57°·0, at 898·6 feet, the former being on May 20, and the latter on July 1. The smallness of the difference between these two temperatures seems to indicate smallness of disturbance by the action of the tool.

It appears from these various circumstances that the observations are entitled to considerable weight, and that the rate of increase of temperature downwards at Liverpool is exceptionally slow. It will be remembered that the rate found by Mr. Fairbairn, at Dukinfield Colliery, in the adjacent county (Cheshire), was also very slow, though not nearly so slow as that indicated by these Liverpool observations.—(See our Report in the volume for 1870).

Mr. E. Wethered, of Weston, near Bath, has also commenced observations in a colliery in that neighbourhood. Mr. J. Merrivale, of Nedderton, near Morpeth, has received a thermometer for observations in a colliery. Mr. J. T. Boot, of Hucknall, near Mansfield, has received a second thermometer (in place of a broken one) for observations in a deep bore, and Mr. Rowland Gascoigne, of the same town, has received one for a similar purpose.

In the eleven years which have elapsed since the appointment of this Committee a large amount of useful work has been done, by methods of observation not requiring any elaborate or expensive appliances, or any special training on the part of the observers.

Two difficulties are encountered in investigating underground temperature. We have to contrive instruments which shall truly indicate the temperature at the point of observation, and we have further to insure that this temperature shall be the same at the time of observation as it was before the locality was artificially disturbed.

As regards the first of these difficulties the Committee have been completely successful, and have largely increased the resources at the command of observers.

But in regard to the second difficulty the same amount of success has not been attained. The circulation of water in bore-holes and of air in mines are disturbing elements difficult to deal with. Even such firm plugging as was employed to isolate portions of the water-column in the great bore at Sperenberg cannot altogether remove the error arising from convective disturbance; for the long-continued presence of water at a temperature different from that proper to the depth affects the temperature of the surrounding rocks, and the temporary isolation of a short column would not abolish this source of error, even if the plugs themselves were impervious to conduction and convection.

After the experience which has now been gained of rough and ready methods, it is time to consider the propriety of resorting to a more special method, which has been more than once suggested, but has hitherto been postponed on account of the additional labour and skill which would be requisite for carrying it out.

There can be no doubt that the surest way to bring any point of a boring to its original temperature is to fill up the bore, and reduce it as nearly as possible to its original condition. Several instruments have been contrived which, when buried in the earth, with wires coming from them to the surface, admit of having their temperature observed by electrical means.

One of these is Siemens' resistance thermometer, another is Wheatstone's telegraphic thermometer, of which a description will be found in the Report of the Dundee Meeting of the British Association; another is Becquerel's thermo-electric apparatus, which has been employed by its inventor and his son and grandson for some forty years. It is described in the following terms in the first Report of this Committee (1868):—

"The thermo-electric method might also be followed with great advantage. Two wires, one of iron and the other of copper, insulated by gutta-percha or some other covering, as in submarine cables, and connected at their ends, might be let down so as to bring their lower junction to the point where the temperature is to be taken, their upper junction being immersed

in a basin of water, and the circuit completed through a galvanometer. The temperature of the water in the basin might then be altered till the galvanometer gave zero indication."

Sir Wm. Thomson now adds the recommendation that, in carrying out this method, the two wires, each well covered with gutta-percha, should be twisted together; that the wires should be stout and as homogeneous as possible throughout, and that a piece of stout copper tube should be attached to the lower junction, this tube being uncovered and in close contact with the earth all round, its purpose being to insure that the junction takes the proper temperature.

It would probably be desirable, in filling up the bore, to mix clay with the original material to render it watertight, for it would be impossible to render the filling of the bore as compact as the surrounding rock.

Several pairs of wires would be buried in the same bore, with their lower junctions at different carefully-measured depths.

The upper junctions would be kept in a room provided with a steady table for a mirror-galvanometer.

THE RAINFALL OF THE WORLD[1]

1. THE pamphlet referred to below embodies the outline of an attempt to bring into harmony the disconnected, and in some cases apparently irreconcilable results that have hitherto attended comparisons of terrestrial rainfall and sun-spot variations. It relates, therefore, to the entire rainfall system of the globe.

2. The plan by which it is thought this object will be best attained is one which divides the world into a number of rainfall zones where either *à priori* considerations or actual experience would lead us to expect typical changes in the effects of a recurring secular variation in solar radiated heat upon the rainfall; it being immaterial as far as regards the practical advantages secured by this method of hyeto-graphical subdivision, whether the solar radiation be ultimately found to vary directly or inversely with the sun-spots.

3. The way in which typical changes may arise in different parts of the earth from the effects of an assumed recurring secular change in solar radiated heat, is shown by a reference to the general scheme of atmospheric circulation in conjunction with the two leading factors of variability, viz., season and latitude.

4. A consideration of these points leads the author to divide the world into five zones, which either theoretically might, or are actually known to, involve some typical change in the secular variation of the rainfall either of one season or the whole year.

5. Partly to illustrate this mode of subdivision by applying a reasonable working hypothesis, and partly in the absence of absolutely conclusive evidence in its favour, by exhibiting the harmony of existing facts with the conditions theoretically deduced from it, to promote its ultimate adoption, the theory of the inverse variation of solar radiated heat with the sun-spots is assumed throughout.

6. It is also shown in the Introduction that we have a good deal of evidence in favour of the same theory, both *à priori*, from a consideration of the principle of conservation of energy as applied to the sun, as well as indirect, from the results of thermometrical observations.

7. In applying this hypothesis to determine the rainfall variation, account is mainly taken of the direct relation between wind velocity and temperature, the secular changes in solar radiation being assumed to cause *similar* effective secular changes in the velocity of the larger atmospheric convection currents.

8. An induction from Messrs. Blanford and Eliot's theory of cyclone-generation is then made use of, in combination with the preceding hypothesis, from which it appears that while, owing to the diminished solar temperature, evaporation might be lessened in the tropics at the epoch of maximum sun-spot, the diminished carrying power of the wind (by which the prevalence of cyclones at this epoch would be accounted for, according to Blanford and Eliot's theory) might allow of greater precipitation near the place of evaporation, and therefore of a generally heavier rainfall in these regions. At the opposite epoch, on the other hand, the increased velocity of the wind would probably cause a wider distribution of tropical vapour, and therefore in combination with the direct effects of the assumed increase in solar radiation at the same epoch give rise to a deficiency of rain in parts, more especially those in which the local conditions normally tend to produce aridity.

9. These hypothetical results are then shown to approximately agree with the actual results of observations recorded in these regions.

10. It is next shown that the effects of the assumed secular change in the velocity of the anti-trade (the prevailing wind of the temperate zone) should differ considerably from those in the case of the monsoons and trades of the tropics, an increased velocity in the case of the anti-trade causing a greater quantity of tropical vapour to be conveyed to the temperate regions, and consequently a greater degree of humidity to ensue there. When, therefore, the direct effects of the assumed increase of solar heat at such an epoch are at a minimum, that is to say, in the *winter*, the relative humidity, and consequently the rainfall, should be *increased*. It is also evident that such an effect should be most conspicuously felt in those regions where rain falls *only* in the winter, and is due to the descent of the anti-trade.

11. The occurrence of this inverse variation in the zone of winter rains, which in the case of the Mediterranean stations (*Zeitschrift für Meteorologie*, Band viii. No. 6), had hitherto been deemed unfavourable to Messrs. Lockyer and Meldrum's generalisation regarding the direct variation of terrestrial rainfall with the sun-spots, is also shown to be visible in the winter rainfall of Northern India, and the rainfalls of Jerusalem and California, thereby affording some preliminary support to the notion that it holds over a still wider extent of the globe where the rain falls mostly during the winter.

12. The attempt is then made to show that while the direct effects of the secular change in the sun's heat over extra-tropical continents may, during the summer, operate so far as to destroy the indirect effects produced by the corresponding variations in the strength of the anti-trade, and as Dr. Hahn has shown in the case of the summer rainfalls of several stations in Central Europe, actually cause a *direct* variation with the sun-spots, there are, as there should be, in accordance with the hypothesis, some preliminary indications of an *inverse* variation of that proportion of the total which falls during the winter months alone, even in those places where the rain falls throughout the year. This fact, then, would imply that a change of season causes a change of type in the character of the variation, so that in order to render the variations distinctly apparent we should compare the winter and summer falls separately. It may also be inferred that the quality of the variation in the total annual fall will depend on the preponderance of the summer or winter falls respectively, which fact may help to account for the numerous anomalies noticed by those who have hitherto compared the total annual falls of places in the temperate zone with sun-spots.

13. It is finally inferred in the appendix, as a direct result of the hypothesis assumed throughout, that the winter gales of the temperate zone and the cyclones of the tropics should bear a complementary relation to each other, the former being most frequent about the time of minimum, and the latter about that of maximum, sun-spot. Some evidence in favour of this notion was recently communicated to NATURE by Mr. S. A. Hill (vol. xviii. p. 616).

14. The pamphlet is intended by the author to be considered as merely tentative, and not by any means conclusive. It is the method of division into zones and the separate comparison of seasonal falls, rather than the accordance of data with theoretical deductions, to which he desires to give prominence, and which he thinks may be of some assistance to other workers in the same field.

[1] The Rainfall of the World in Connection with the Eleven-Year Period of Sun-spots. With an Introduction and Appendix. By E. D. Archibald, Professor of Mathematics in the Patna College. (Calcutta and London: Thacker and Co. 1878.)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

THE Cambridge Mathematical Tripos list was published on the 24th. This year the list contains 91 names. There are 28 classed as Wranglers, 33 as Senior Optimes, 29 as Junior Optimes, and 1 Ægrotat. In 1878 the list contained 94 names, 31 being Wranglers, 30 Senior Optimes, 29 Junior Optimes, and 4 Ægrotant. The first three Wranglers are Mr. A. J. Campbell Allen, of St. Peter's, Mr. George Walker, of Queen's, and Mr. Carl Pearson, King's. Mr. Campbell Allen, of St. Peter's College, the Senior Wrangler, is a native of Belfast, and was born in 1856. He received his elementary education at the

Royal Academical Institution, Belfast. In 1872 he became a student at Queen's College, Belfast, where he succeeded in winning several scholarships and also two Peel Exhibitions, one for general proficiency and the second for mathematics. In 1875 he was elected to an open scholarship at St. Peter's, and in October of that year he graduated B.A. at the Queen's University, subsequently proceeding M.A. On each occasion he obtained a first-class for mathematical science, and was awarded a gold medal. He has won several college prizes during his residence at Cambridge. Mr. Walker is a native of Durham, and was educated at Durham University, of which he is a Fellow, and proceeded to Queen's College in October, 1875. He has been a prizeman of the college for mathematics. Mr. Pearson was educated at University College School, and also under private tuition with the Rev. L. Hensley, of Hitchin. He gained an open scholarship at King's College in 1875, and has been each year college prizeman in mathematics.

A REPORT just published by the Swiss Statistical Board gives some information as to the state of primary instruction in the various cantons of Switzerland. Out of 21,875 recruits examined during the year 1877, 11·7 per cent. proved to have primary instruction quite insufficient, and were sent back to the primary military schools. The better educated cantons are those in which manufactures are more developed, namely, Basel (town), Geneva, and Zurich, Schaffhausen and Thurgau. The worst educated are those of Appenzell (land), Uri, Wallis, and Freiburg (Catholic). Primary education seems to have become worse during recent years, as the results for 1877 are far below those of 1876.

SCIENTIFIC SERIALS

Journal of the Franklin Institute, December, 1878.—From experiments here described by Mr. Jacques, it appears that currents of air of varying density, as in Tyndall's well-known experiment, not only diminish the *intensity* of a sound, but affect its *distinctness*. This holds good especially for the human voice, and for musical instruments with few overtones (as the flute). The effect on the voice is that of a repetition of each syllable several times in close succession. Sound-waves were traced out in the space of an auditorium in Boston, and their confusion shown on introducing air-currents. The good acoustic properties of the Baltimore Academy of Music are proved to be due to arrangements by which a large volume of air is conducted, in gentle current, across the stage and diagonally towards the roof. When, by closing certain valves, ventilation was arrested and currents of circulation generated, the sound was noticed to be "dead," or "confused and indistinct."—Dr. Dudley investigates the chemical composition and physical properties of steel rails, deducing some rules for guidance of the Pennsylvania Railroad Company.—Mr. Dupuy writes on the direct process of making wrought iron and steel,—Mr. Dumont on tests of boiler iron,—and Prof. Haupt on the use of the heliotrope in geodetic surveys.

THE *Archives des Sciences physiques et naturelles* (parts 251 and 252, November and December) contain the following papers of interest:—On ytterbina, a new earth contained in gadolinite, by C. Marignac.—On a transformation of dibromethylene into an acetone with four atoms of carbon, brought about by the action of hypobromous acid, by E. Demole.—A note on Dr. Heine's work on the formation of mountains, by E. Renevier.—On the geography and archæology of forests, by Dr. Asa Gray.—Recent researches in solar chemistry, by J. Norman Lockyer.—Observation of a case of migration of carps, by A. Bartholoni.—On a general method of continuous integration of any numeric function, applied to several theorems furnished by the mathematical analysis of the calculation of the curves of a new thermograph, by Raoul Pictet and Gustave Cellérier.—On the limnograph of Secheron, near Geneva, by Ph. Plantamour.—A note on the useful effect of magneto-electric machines and the production of electric light, by A. Achard.—On the reappearance of Encke's comet of short period, with a history of this comet, by Alfred Gautier.—Some remarks on the migration of carps by G. Lunel.—On the ophite of Spain, by M. Calderon.

Bulletin de l'Académie Royale de Belgique, Nos. 9 and 10, 1878.—This contains an account, by M. Dupont, of a recent important "find" of fossils in the Sainte Barbe, one of the coal mines of Bernissart (a village near the French frontier), consisting of five skeletons of large adult iguanodons together with tortoises, numerous fishes, and plant-impressions, constituting a fauna and flora wholly new for the country. The bones are unfortunately impregnated with pyrites, so that they are readily disaggregated on contact with air, but they have been carefully removed in plaster to Brussels, after precise noting of position, &c. The fossils were found at several different levels separated by layers of sterile clay. There is no indication of molluscs of any kind. The deposit is thought to be of the Wealdian horizon, and is remarkable, both in itself, and in its relations to the subterranean geography of the valley of Mons, and the lower cretaceous strata of Hainaut.—M. Plateau writes on a law of the persistence of impressions in the eye. With two disc having the same number of sectors, and the white sectors of the one being equal in angular width to the black discs of the other, the "times of apparent constancy," of the two impressions are to each other in inverse ratio of the brightnesses of the two grey tints producing these impressions. A complete impression, whether intense or weak, has no appreciable time of apparent constancy; and the time is longer, the more incomplete the impression. The degree of illumination of the object has but a weak and indirect influence on the time of apparent constancy.—M. Longchamps contributes further additions to the synopsis of the Gomphines; and M. Renard lithological researches on the phtanites of the carboniferous limestone of Belgium.—M. Montigny describes an experimental arrangement for the study of coloured stars.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, December 19, 1878.—"On the Torsional Strain which remains in a Glass Fibre after Release from Twisting Stress," by J. Hopkinson, D.Sc., F.R.S.

It has long been known that if a wire of metal or fibre of glass be for a time twisted, and be then released, it will not at once return to its initial position, but will exhibit a gradually decreasing torsion in the direction of the impressed twist. The best method of approximating to an expression of the facts has been given by Boltzmann ("Akad. der Wissensch. zu Wien," 1874). He rests his theory upon the assumption that a stress acting for a short time will leave after it has ceased a strain which decreases in amount as time elapses, and that the principle of superposition is applicable to these strains, that is to say, that we may add the after-effects of stresses, whether simultaneous or successive. Boltzmann also finds that, if $\phi(t)\tau$ be the strain at time t resulting from a twist lasting a very short time τ, at time $t = 0$, $\phi(t) = \frac{A}{t}$, where A is constant for moderate values of t, but decreases when t is very large or very small.

The glass fibre I examined was about twenty inches in length. The glass from which it was drawn was composed of silica, soda, and lime; in fact, was glass No. 1 of my paper on "Residual Change of the Leyden Jar" (*Phil. Trans.*, 1877). In all cases the twist given was one complete revolution. The deflection at any time was determined by the position on a scale of the image of a wire before a lamp, formed by reflection from a light concave mirror, as in Sir W. Thomson's galvanometers and quadrant electrometer.

The first point to be ascertained from the results was whether or not the principle of superposition, assumed by Boltzmann, holds for torsions of the magnitude used.

The experiments indicate a large deviation from the principle of superposition, the actual effect being *less* than the sum of the separate effects of the periods of stress into which the actual period may be broken up.

They also appear to indicate the form $\phi(t) = \frac{A}{t^\alpha}$, α being less than, but near to, unity. If $\alpha = 0{\cdot}95$ we have a fairly satisfactory formula for the case in which the fibre was twisted two hours.

In the author's paper on "Residual Change of the Leyden Jar" that subject is discussed in the same manner as Boltzmann discusses the after-effect of torsion on a fibre, and it is worth remarking that those results can be roughly expressed by a formula in which $\phi(t) = \frac{A}{t^\alpha}$. For glass No. 5 (soft crown) $\alpha = 0{\cdot}65$, whilst for No. 7 (light flint) it is greater: but in the electrical experiment no sign of a definite deviation from the law of superposition was detected.

January 16.—"On the Effect of Strong Induction-Currents upon the Structure of the Spinal Cord," by William Miller Ord, M.D.

Conclusions.—1. That, in young dogs, the protoplasmic constituent of the grey matter contracts under the influence of strong faradaic currents.

2. That it contracts unequally and irregularly by reason of its unequal and irregular sectional area, causing thereby condensations at certain points—notably in the anterior horns and around the central canal—and rarefaction at others—notably in the middle of each crescent, such rarefaction going on sometimes to rupture of tissues.

3. That nerve-corpuscles contract in various degrees according to the strength and duration of currents, and that while they tend in contraction to become spherical, they also tend to become vacuolated.

4. That the vessels are in some places strongly contracted and empty; in others dilated and filled with blood-clot, having the appearance of embolus.

5. That the appearances correspond so decidedly with appearances in chorea and tetanus as to give ground for the supposition that contractions, such as are produced by electricity, do actually occur during life under the effect of nervous shock, and may be phenomena, casual, or associate, of such diseases.

"On some Points connected with the Anatomy of the Skin," by George Thin, M.D. Communicated by Prof. Huxley, Sec. R.S.

It is partly the object of this paper to describe some methods by which it can be demonstrated that the connective tissue-bundles of the cutis are, as has been long ago pointed out by Rollett, composed of subdivisions, which are again composed of minute fibrillæ. These subdivisions the author terms primary bundles to distinguish them more markedly from the fibrillæ, and also to describe some other points in the anatomy of the skin which were observed by means of these methods.

The primary bundles isolated by these methods were flattened, cylindrical elements, even contoured, homogeneous in appearance, and uniform in breadth over the whole length isolated. The difference in breadth between individual bundles was very slight. By measurement he found that they were from 0·004 to 0·005 millim. broad.

In gold preparations the following facts regarding the disposition of the elastic fibres were noted:—

If a portion of skin is hardened in bichromate of potash, and the sections moderately stained by eosin, all the large elastic fibres are stained much more intensely than the bundles, and it is then observed that they lie on the surface of the bundles, and run parallel to them. In the gold preparations, after maceration in formic acid, further details regarding the fibres can be detected. It is then seen that there is a close network of minute elastic fibres, of which no traces are observed in eosin-stained bichromate preparations, on the surface of the bundles, and that at certain points the larger fibres give off branches which join this network. At these points the network is so dense over a small defined space that the size of the meshes is nearly equalled by that of the fibres.

Elastic fibres which penetrate the bundles enter between the primary bundles, and the primary bundles are embraced by the fibres which entwine them very closely.

The dark very finely granular deposit produced by the reduction of the gold chloride had a special relation to the elastic fibres. Strictly defined narrow strips of this deposit were found investing the fibres, and this so closely that it was only at points where it had been disturbed in the preparation that the fibre itself could be observed.

The distinctly localised character of the deposit around the elastic fibres supports, according to the author, the idea that the larger ones are surrounded by an albuminous fluid, of a like nature to that shown by gold preparations, to be present between the laminæ of the cornea.

The "spiral" fibre, as observed on the bundles of the skin, is an elastic fibre that encircles the bundles like a ring, and is stained yellow by pikro-carminate of ammonia.

The cells seen in the preparations were in two positions. Some of them were found in a delicate tissue between the bundles; other cells were found in direct connection with the bundles. Of the latter cells the greater number seen were applied to the surface of the bundles, but others were found in the substance of the bundles between the primary bundles.

These cells were all of the endothelial type. In all of them the cell-contour was clearly marked, and in none of those observed was there a trace of a process, or of ridges and depressions similar to those described by some histologists in tendon.

"On Hyaline Cartilage and Deceptive Appearances produced by Reagents, as observed in the Examination of a Cartilaginous Tumour of the Lower Jaw." By George Thin, M.D. Communicated by Prof. Huxley, Sec. R.S.

This paper is written with a twofold object: firstly, as a contribution to the histology of hyaline cartilage; secondly, to illustrate how much the apparent structure of a tissue which is being examined microscopically depends on methods of preparation.

The author was able to isolate the cells from the cartilaginous substance of the tumour after the action of osmic acid. All the cells thus isolated were flattened, rounded, or somewhat polygonal bodies, with round nuclei. Their contours did not correspond exactly with those of the rounded cartilage "capsules."

The examination of this tumour showed that most delusive appearances as regards the nature of cartilage cells may be sometimes produced by staining and hardening agents. Carmine and eosin by staining an unformed substance that exists in the structure in a localised form, may simulate branched protoplasmic cells, and bichromate and logwood preparations, either in sections or teased out, may as closely simulate cells with fibre processes.

The facts adduced by the author justify, as he believes, serious doubts as to the correctness of interpretation in all cases in which histologists have described branched cells in hyaline cartilage, whether the latter existed as a normal structure or as a pathological growth. They further show that, taken alone, carmine or eosin staining should not be held as conclusive evidence of the existence or limits of cellular protoplasm in any animal tissue.

Meteorological Society, January 15.—Annual Meeting.—Mr. C. Greaves, president, in the chair.—The Report of the Council showed that the chief features of the proceedings during the year 1878 had been the final completion, on a comprehensive and well-organised basis, of the arrangements for systematic inspection of the Society's stations, an object which has engaged the sedulous attention of successive Councils for the last four years; and the delivery of a series of lectures on Meteorology by certain Members of the Council. The total number of Fellows now amounts to 425, forty-one having been elected during the year.—The President having delivered his address on Dryness *versus* Humidity, the following gentlemen were elected officers and Council for the ensuing year:—President: Charles Greaves, M. Inst. C.E., F.G.S. Vice-Presidents: Charles Brooke, M.A., F.R.S., F.R.C.S., Henry Storks Eaton, M.A., Rev. William Clement Ley, M.A., Capt. Henry Toynbee, F.R.A.S. Treasurer: Henry Perigal, F.R.A.S. Trustees: Sir Antonio Brady, F.G.S., Stephen William Silver, F.R.G.S. Secretaries: George James Symons, F.R.S., John W. Tripe, M.D. Foreign Secretary: Robert H. Scott, M.A., F.R.S. Council: Arthur Brewin, F.R.A.S., Edward Ernest Dymond, William Ellis, F.R.A.S., Rogers Field, B.A., M.Inst.C.E., Rev. Charles Higman Griffith, William John Harris, M.R.C.S., James Park Harrison, M.A., John Knox Laughton, M.A., F.R.A.S., Robert John Lecky, F.R.A.S., Hon. Francis A. Rollo Russell, Richard Strachan, Henry Samuel Tabor.

Royal Microscopical Society, January 8.—J. W. Stephenson, treasurer, in the chair.—Five gentlemen were proposed for election as Fellows, and the list of nominations for the Council was read.—The following papers were read:—Observations on *Dactylocalyx pumiceus* (Stuchbury), with description of a new variety, *D. Stuchburyi*, by Mr. W. J. Sollas.—Note on a revolver immersion prism for sub-stage illumination, by Dr. James Edmunds.—Immersion illuminators, by Mr. J. Mayall, jun.—Is not the genus Pedalion of Hudson synonymous with Hexarthra of Schmarda? by Mr. J. Deby.—The thallus of Diatoms, by Mr. F. Kitton.—Mr. Crisp (secretary) described the two new sense-organs in insects discovered by Prof. Graber, of Czernowitz.—The following were exhibited:—Specimens showing parasitism of a coral on a sponge (Dr. Matthews); the Sorby miniature micro-spectroscope; Recklinghausen and Meyer's pathological micro-photographs and specimens of microscopic printing (Mr. Crisp); sections of mistletoe on an apple tree double stained (Mr. Ward); Amici's original form of camera lucida referred to at the December meeting (Mr. Ingpen).

Entomological Society, January 15.—Anniversary meeting. H. W. Bates, F.L.S., F.Z.S., president, in the chair.—The following gentlemen were elected Members of the Council for the ensuing year, viz.:—H. W. Bates, F.L.S., F.Z.S., W. L. Distant, Rev. A. E. Eaton, M.A., E. A. Fitch, Ferd. Grut, F.L.S.,

R. Meldola, F.C.S., Edw. Saunders, F.L.S., J. Jenner Weir, F.L.S., J. W. Dunning, M.A., F.L.S., Sir Jno. Lubbock, Bart., V.P.R.S., Saml. Stevens, J. Wood Mason, F.G.S. The following officers were elected :—President : Sir Jno. Lubbock, Bart., V.P.R.S. Treasurer : J. Jenner Weir. Librarian : F. Grut. Secretaries : R. Meldola and W. L. Distant. The retiring president delivered an address which was immediately ordered to be printed.

WELLINGTON, N. Z.

Philosophical Society, November 9, 1878.—Mr. Carruthers, vice-president, in the chair.—Further contributions to the ornithology of New Zealand, by Dr. Buller, C.M.G. This paper consisted partly of technical matters and partly of observations on the habits and life-economy of a number of the more common species of native birds. The author gave the results of his examination of the group of *Platycerci* in the British Museum, and showed that many of the so-called species had no real existence, the same bird having been described under different names by different naturalists. He gave his reasons for considering *Platycercus Rowleyi*, described by himself from specimens in the Canterbury Museum, a good and valid species. He disputed Mr. Sharp's generic substitution of *Harpa* for *Hieracidea* in the British Museum catalogue of Accipitres, and his reduction of *H. ferox* to the rank of a "sub-species," as being unintelligible; either the two forms of sparrow-hawk represent distinct species, as the author and others believe; or they are one and the same, as contended for by Captain Hutton in the controversy which took place some time ago. Referring to that discussion and to Captain Hutton's emphatic denial that the New Zealand kingfisher ever caught fish, he proceeded to give some further facts in support of his own view to the contrary. In treating of the kaka (*Nestor meridionalis*) he mentioned the singular circumstance that at a certain season of the year, when these birds are migrating across the Strait at its widest part, numbers of them, owing to their fat condition, succumb to fatigue, and are washed up in Golden Bay and on the spit beyond, the set of the current being in that direction. The paper contained many other interesting notes, and a full account of the capture and subsequent history of a specimen of the plundering gull (*Stercorarius antarcticus*) still living in the author's garden.

PARIS

Academy of Sciences, January 20.—M. Daubrée in the chair.—The following papers were read :—On the development of the perturbative function in the case where, the eccentricities being small, the mutual inclination of the orbits is considerable, by M. Tisserand.—Observations on the second reply of M. Pasteur.—Reply by M. Pasteur, &c.—On the special apparatus of nutrition of phanerogamous parasite species, by M. Chatin. He distinguishes in the sucker a *cone de renforcement* (the central, mostly solid part), and a *cone perforant*, or parenchymatous cone continuing the other, and capable, notwithstanding its delicacy of tissue, of progressing through the hardest woods. (There are variations from this in some cases.) The suckers of parasites show great analogies to ordinary roots of plants.—On the temporary magnetic properties developed by induction in different specimens of nickel and cobalt, compared with those of iron, by M. Becquerel. The ratio of the temporary magnetic effects developed at ordinary temperature, by increasing magnetic inductions, in any of the nickel bars and in a bar of soft iron of the same length, weight, and section, is a number variable with the magnetic intensity to which the metals are submitted. This ratio, for very small intensities, first decreases, passes a minimum, then increases to a maximum, and lastly decreases to an inferior limit. Carburetted and forged nickels show the variations most. Pure cast or porous bars of nickel give results very like those of soft iron. Cobalt behaves similarly to nickel. The variation of the ratio considered is due to unequal saturation of the two metals.—On linear differential equations of the third order, by M. Laguerre.—On the classification of colours, and on the means of reproducing coloured appearances by three special photographic negatives, by M. Cros. Under the word colours he distinguishes lights and pigments. To get immediately the elementary tints of lights and pigments, look through a prism at a white bar on a dark ground, and a black bar on a white ground; in the first case you see a spectrum orange, green, violet; in the latter a spectrum red, yellow, blue. In the one case the orange, green, and violet are elementary lights; in the other, the blue, red, and yellow, are lights combined two and two. This he demonstrates with an apparatus he calls a *chromometer* (which distinguishes the colours by numerical data); and he makes this act on the positions from three negatives obtained through green, violet, and orange screens, ultimately reproducing coloured appearances.—Researches on the effects of induction through telephonic circuits, by means of the microphone and the telephone, by Prof. Hughes. A battery of three Daniells, a microphone, an inducing spiral, and a clock, were put in one circuit; another helix (to receive induction) and a telephone in another circuit. The sounds were still heard in the induced circuit when the spirals (containing 100 m. wire) were 30 ctm. apart. Conducting plates interposed weakened the effect, and spirals with closed circuit better. Flat helices gave more intense reproduction than long ones. Putting a telephone bobbin, in circuit with a microphone, to one ear, and the bobbinless telephone to the other, one can hear thus; and the arrangement is a sort of electric analyser revealing what passes in organs traversed by currents. (Other experiments are given.)—New voltaic element with constant current, by M. Heraud. The exciting liquid is chlorhydrate of ammonia, the depolarising body protochloride of mercury, or calomel. The former, in presence of zinc, gives chloride of zinc with ammonia and hydrogen. The hydrogen reduces the protochloride, giving metallic mercury, chlorhydric acid, and consequently, chlorhydrate of ammonia. To prevent deposition of ammoniacal oxychloride of zinc on the zinc, the solution of sal ammoniac used is diluted one-tenth with liquid ammonia. The zinc is suspended by a coated copper-plate about the middle of the liquid. The positive electrode is carbon in a canvas bag. One element, after 248 days' use, retained 0·66 of its original intensity.—On tetric acid and its homologues, by M. Demarçay. —Researches on the development of eggs and of the ovary in mammalia, after birth, by M. Rouget.—Description of the strata forming the ground in the department of Meurthe-et-Moselle, by M. Braconnier.

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