THURSDAY, MARCH 30, 1876

BLASIUS ON STORMS

Storms; their Nature, Classification, and Laws, with the Means of Predicting them by their Embodiments, the Clouds. By William Blasius, formerly Professor of the Natural Sciences in the Lyceum of Hanover. (Philadelphia: Porter and Coates. London: Lockwood and Co.)

WHATEVER may be thought of this work as a contribution to the extremely difficult question of the theory of storms, its delightful "positiveness of statement born of conviction," makes it a very readable book; and, further, the fresh facts contained in its pages, collected in most cases with evident care, form a useful repository to meteorologists in the study of atmospherical disturbances. In matters pertaining to general meteorology, the author is less at home as regards his facts, such as when he states that the heat of the sun's rays scarcely penetrates an inch into the surface of the land in the course of a day. He is also at fault as regards the seasonal distribution of atmospheric pressure over the globe, and the extent over which the south-west monsoon spreads to westward off the coast of Africa.

A storm is defined to be "in general the movement of the air caused by its tendency to re-establish an equilibrium which has in some manner been disturbed, and we may call all such movements storms, whether they are gentle breezes or furious hurricanes, whether they are accompanied by more or less condensation of moisture or clouds, or even by none at all; in general the laws of the motion and changes of the wind in re-establishing an equilibrium must be the same, whether the action takes place in a greater or less degree" (p. 43), and the author "is certain that the storm is the conflict of air-currents of different temperatures, and that the barometric depression is the *effect* of their movement" (p. 5). Thus storms are considered in a very broad sense,—in fact, they are regarded as synonymous with the whole atmospherical movements, and the book is therefore an attempt to state the theory of the circulation of the atmosphere.

Since the disturbances which occur in the distribution of the temperature and humidity of the air are either vertical or horizontal, it follows that the equilibrium will be restored either by the setting in of ascending or descending currents of air, or by the setting in of horizontally-flowing currents. Observation affords abundant proofs of the existence of all these types of aerial movements. In accordance with this conception of atmospherical disturbances and consequent movements which follow, Prof. Blasius classes all storms under three heads, viz.:—1. *Local or vertical storms*, which are stationary and centripetal, being produced by the atmosphere tending to re-establish in a vertical direction an equilibrium that has been disturbed; the characteristic cloud being the cumulus. 2. *Progressive or lateral storms* which travel, being produced by the atmosphere tending to re-establish in a lateral direction an equilibrium which has been disturbed. This second class of storms are of two kinds, viz., *equatorial or north-east storms* (winter storms), which are produced by a warm current displacing a cool one to supply a deficiency towards the poles, the temperature changing from cool to warm, the direction in which they travel being to the north-east quadrant, and their characteristic cloud being the stratus. *Polar or south-east and south-west storms* (summer storms), which are produced by a cool current displacing a warm one to supply a deficiency toward the equator, the temperature changing from warm to cool, the direction in which they travel being to the southern semicircle, and their characteristic cloud being the cumulo-stratus. 3. *Loco-progressive or diagonal storms* (tornadoes, hail-storms, sand-storms, waterspouts, &c.), which travel locally, are rotatory, and are produced by the atmosphere tending to re-establish the equilibrium of a polar storm which has been disturbed in the plane of meeting of the two conflicting currents by a peculiar configuration of the ground, their characteristic cloud being what the author calls the conus, instances of which are presented by the funnel-shaped cloud of waterspouts.

In the explanation offered of the most important of these classes, viz., the winter storms, their origin is attributed to an assumed distribution of atmospheric pressure from the equator to 80° lat. N., and to the polar and equatorial currents which result from this distribution of pressure, and which conflict with each other—the shifting of the atmosphere with the motion of the sun toward the north during spring and summer and toward the south during autumn and winter, taking place not regularly, but interruptedly, by repeated oscillations like the waves of the rising and falling tide. In the winter storm there is an area of lowest barometer marking the region where the equatorial current overlaps the polar current and the theatre of the cloud and rain which accompany the storm, with two regions of high barometer, one in front and the other in the rear of it. These three regions move forward thus:—The region of high barometer, which apparently moves far in front of the storm, is the receding polar current; the region of low barometer, the uprising current; and the region of high barometer, in the rear of the storm, the in-blowing equatorial current. It is asserted that the area of the storm, or the region of conflict between the equatorial and the polar currents, must assume the form of an ellipse, and not that of a circle, and that the air in a storm does not move around one centre, much less around the line when the *plane of meeting* of the polar and equatorial currents meet the earth's surface, but in straight lines from the circumference of the ellipse toward the region of the uprising equatorial current. The progressive velocity of the storm depends on the greater or less resistance with which the polar current opposes the displacing equatorial current, since the greater this becomes during the development of the storm, the more the plane of meeting must rise or approach a vertical position, and thus the progressive velocity of the storm be proportionally retarded.

It is unnecessary to follow Prof. Blasius through his theory of the atmospheric movements comprehending our winter storms, which we have stated as far as possible in his own words. It may be enough to point out that at least those regions of the globe with whose meteorology Prof. Blasius does not appear to be very familiar, present facts not in accordance with his theory, proving that storms do not necessarily arise from great currents which can be traced to equatorial or sub-tropical regions on the one hand, and to polar regions on the other, or even to have had their origin in these regions; and that the idea of two great atmospheric currents having in storms a common *plane of meeting* (Begegnungs-fläche) is a mere supposition. Such statements as these: "The storm is the *conflict* of air-currents," and "the barometric depression is the *effect* of their movement," are, to say the least, altogether unwarranted in the present state of our knowledge.

The tornado is conceived as originating from the sultry calm felt before it, which is due to the conflicting of the two aerial currents, bringing about a state of equilibrium by the great compressive force they exert against each other. This equilibrium is disturbed by a particular uneven configuration of the earth's surface, and the disturbance produces the tornado. Leaving out of view the imagining action of conflicting aerial currents, it will be enough to point to the geographical distribution of tornadoes as disproving this theory of their origin.

Notwithstanding these very serious drawbacks, the book will repay perusal as being the production of one who not unfrequently gives evidence of acute observation, and who has thought out his subject for himself. The following may be given as specimens of what are repeatedly to be met with:—"Meteorology is fully as much a science of the earth's surface as of the air." "The wind changes, not by the veering around of one and the same current, but by a succession of different currents, blowing inwards." "Among the meteorological elements the real direction of the wind is the most difficult to arrive at, especially at observatories above cities or near mountains and coasts, and wind observations are therefore in general the least reliable." "The thermometer, as measuring a primary effect, is, with the hygrometer, at least as important as the barometer."

The theory of atmospheric movements remains still to be stated, and it needs scarcely be added that this cannot be done till we have a better knowledge than we yet possess of the physics of the atmosphere with its vapour, and of the merely mechanical effects of ascending, descending, and horizontally-flowing currents of air.

ANDERSON'S "MANDALAY TO MOMIEN"

Mandalay to Momien: A Narrative of the Two Expeditions to Western China of 1868 *and* 1875, *under Col. E. B. Sladen and Col. Horace Browne.* By John Anderson, M.D., F.R.S.E., &c., Curator of Imperial Museum, and Professor of Comparative Anatomy, Medical College, Calcutta. With Maps and Illustrations. (London: Macmillan and Co., 1876.)

MANY details concerning the latter of the expeditions referred to in this volume have been made known in this country through the daily papers, in connection with the much-to-be regretted murder of Mr. A. R. Margary. Indeed, Dr. Anderson states that his work was suggested by the interest called forth by the repulse of the recent mission and the tragedy attending it. The principal object of both expeditions was to prepare the way for establishing an overland trade-route between Burmah and China, and thus save the delay and expense of a roundabout sea voyage. That the establishment of such a route is desirable from a commercial point of view is maintained by all who have a knowledge of the circumstances, and in the meantime appears to have been frustrated by the machinations of native traders who think it their interest to shut out British enterprise from a field which is evidently capable of great development. This uncivilised shortsightedness will no doubt in the end be defeated, though in the meantime it has cost this country the loss of a brave and accomplished servant.

Dr. Anderson, who was medical and scientific officer to both expeditions, does not pretend to give in the present volume more than a narrative of the general work of the expedition; a full and illustrated report of the natural history results, is, however, we are glad to learn, in active preparation, and will be published by the aid of the Indian government. Meantime, in the very interesting narrative before us, numerous details will be found of value from a scientific point of view. Dr. Anderson has made use not only of his own notes, but to some extent of those of other members of the expeditions, and has evidently studied with care the results achieved by the French expedition from Saigon to Yunnan under Lagrée, Garnier, and Carné, as well as the literature of the subject generally. The result is that the reader will obtain a clear account of the British connection with the route referred to, and be in a position to understand the present state of affairs and future negotiations.

The real starting-point of the first expedition under Col. Sladen, was Mandalay, the capital of Burmah, which was founded by the present king only in 1853, but which is already a large and evidently well-constructed city. An interesting description of Mandalay, and of the royal court, and some of its customs, is given by D. Anderson. From this point the party sailed in a steamer up the Irawady to Bhamo, which is about thirty miles from the boundary between Burmah and Yunnan, and is the headquarters of the Chinese traders who seem at present to monopolise the trade between Burmah and Western China, and whose opposition had no doubt much to do with the difficulties encountered by both expeditions. After many troubles with regard to carriers the expedition left Bhamo and proceeded by Ponline, Ponsee, Manwyne and Nantin to Momien, beyond which it could not proceed.

Yunnan was at the time in a state of anarchy caused by the rebellion of the Panthays, or Chinese Mohammedan population of this region, which added greatly to the difficulties and dangers of the expedition. Nevertheless, even from a commercial point of view, it seems to us much was accomplished, but probably more from a scientific point of view. The natural history results, we have said, are being prepared for publication, but in the present volume the geologist, and especially the ethnologist, will find a great deal that is valuable and interesting. Dr. Anderson has evidently taken careful notes as he journeyed along, and a somewhat minute description of the course of the Irawady and of the country on its banks, and its antiquities between Mandalay and Bhamo will be found in the work. There were so many delays between Bhamo and Momien that Dr. Anderson had many opportunities of studying the country and the people, and these he evidently took ample advantage of. To the Kakhyens and Shans especially he paid great attention, and his account of these peoples must be considered a valuable contribution to ethnology.

The Kakhyens especially are a very interesting race. They inhabit the mountains on the border-country between Burmah and China, and Dr. Anderson considers them as cognate with the hill tribes of the Mishmees and Nagas. The name Kakhyen is a Burmese appellation, and the people are widely spread under other names, as Singphos, Kakoos, &c. They do not seem, however, to be a genuine aboriginal population in Burmah and China as so many hill-tribes are elsewhere.

"By their own account the hills to the north of the Tapeng, for a month's journey, are occupied by kindred tribes. South of the Tapeng, they occupy the hills as far as the latitude of Tagoung, and, as mentioned, were met with on our voyage near the second defile. To the east, they are found occupying the hills, and, intermixed with the Shans and Chinese, almost to Momien. Here they, as it were, run into the Leesaws, who may be a cognate,

Kakhyen Men.

but are not an identical, race. The two chief tribes in the hills of the Tapeng valley are the Lakone and Kowrie or Kowlie, but numerous subdivisions of clans occur. All are said to have originally come from the Kakoos' country, north-east of Mogoung; and Shans informed us that two hundred years ago Kakhyens were unknown in Sanda and Hotha valleys. To give one instance of their migrations. The Lakone tribe have at a very recent period driven the Kowlies from the northern to the southern banks of the Tapeng. A Lakone chief, having married the daughter of a Kowlie, asked permission to cultivate land belonging to his father-in-law; receiving a refusal, he took forcible possession, and drove the Kowlies across the river to the hills where they now dwell."

The Kakhyens are divided into numerous tribes or clans, between which there does not appear to be any strong common bond of union. Indeed, in very many respects, the system of government, if it may be so called, which exists among these half wild tribes, reminds us strongly of that which prevailed in the Scottish Highlands in the so-called "good old times," before the abolition of the hereditary jurisdictions. They have many curious superstitions and customs, which Dr. Anderson describes with great minuteness. Everything in "the heavens above, the earth beneath, and the waters under the earth" seems to have its particular "nat" or spirit; "every accident or illness is the work of some malignant or vindictive one of 'these viewless ministers.'" The will of these "nats" is consulted by means of a medium, who works himself up into a state of great agitation, so as to become "possessed." The Kakhyens are not very bold warriors, though they are great braggarts, and very troublesome to deal with. They are altogether a very interesting people.

With regard to the wide-spread Shan race, also, many details will be found in the volume, the result both of Dr.

Kakhyen Matrons.

Anderson's own observation and of the observations of previous explorers. The Shans of Yunnan belong to the Tay-Shan or Great Shans of the Tai-race, the branches of which, under different names, are found extending to the eleventh parallel, their various states being tributary to Siam, Burmah, and China. The Shans of Burmah have become closely assimilated to the Burmese in all respects; the Yunnan Shans are the remnants of the Shan Kingdom of Pong, conquered by the Chinese in the fourteenth century, and which included part of Burmah.

"The Shans proper of these valleys are a fair race, somewhat sallow like the Chinese, but of a very faintly darker hue than Europeans, the peasantry, as a rule, being much browned by exposure; they have red cheeks, dark brown eyes, and black hair. In young people and children, the waxen appearance of the Chinese is slightly observable. The Shan face is usually short, broad, and flat, with prominent malars, a faint obliquity and contraction of the outer angle of the eye, which is much more marked in the true Chinese. The nose is well formed, the bridge being prominent, almost aquiline, without that breadth and depression characteristic of the Burman feature. The lower jaw is broad and well developed; but pointed chins below heavy, protruding lips are not infrequent. Oval faces laterally compressed, with retreating foreheads, high cheek-bones, and sharp retreating chins, are not infrequent; and the majority of the higher classes seemed to be distinguished from the common people by more elongated oval faces and a decidedly Tartar type of countenance. The features of the women are proportionately broader and rounder than those of the men, but they are more finely chiselled, and wear a good-natured expression, while their large brown eyes are very scantily adorned with eyebrows and eyelashes. They become much wrinkled by age, and, judging from the numbers of old people, appear to be a long-lived race. They are by no means a tall people, the average height for men scarcely reaching five feet eight, while the women are shorter and more squat in figure.

A minute account of these people, their manners, customs, dress, &c., is given by Dr. Anderson. Some of the ornaments worn by the women are of most artistic workmanship.

The latter part of the volume contains a clear account of the second expedition undertaken to open a trade route between Burmah and China, but which, as we have said, came to an untimely and sad end about a year ago in the murder of Mr. A. R. Margary. Dr. Anderson sets forth the whole circumstances with evident fairness, and yet it is difficult to say exactly who was to blame in the matter. That such a trade-route as it was attempted to establish would be of great advantage to all concerned, there is no doubt; and no doubt also it only requires time to establish it. There is yet a very great deal to be learned both with regard to the natural history of that part of the world, and with regard to the several interesting races of people which form its population. Dr. Anderson's work is a valuable contribution to such a knowledge, and the clear and straightforward manner in which he writes adds greatly to the intrinsic interest of the information with which his pages teem. The illustrations of the country and the people are charming, and the two maps enable the reader to follow satisfactorily the footsteps of the explorers.

OUR BOOK SHELF

A Class-book of Chemistry. By Edward L. Youmans, M.D. (London: Henry S. King and Co., 1876.)

"THIS book is not designed as a manual for special chemical students. It aims to meet the wants of that considerable class, both in and out of school, who would like to know something of the science, but who are without the opportunity or the desire to pursue it in a thoroughly experimental way. Such a class-book as the present . . . . must be but a brief compendium of general principles and descriptions of the most important substances, and is not to be judged of by the fulness of its details." This extract from the author's preface sufficiently explains the objects which he has had in view in compiling the book before us. Certainly the work has no claims as a textbook for students; for the general reader we are afraid it will prove of little interest. Within the compass of about 350 pages we have an account of Gravity, Heat, Molecular Attraction, Electricity and Light, besides Chemistry proper. Surely the day has passed when this kind of thing could be tolerated in a book which professes to teach science. People cry out against the teaching of science as a regular part of educational discipline. It is all very well in its own place, they say, but the only true mental training is to be derived from a study of classics.

If boys and youths devote years to the careful study of ancient languages, they can scarcely fail to receive at least some benefit. If, on the other hand, they pass rapidly through a course of training (?) in science, with the aid perhaps, of such a book as that before us, they quickly forget what they have learned, and, so far as mental training is concerned, they had better have left science alone altogether. Our chief objection to the present work is that it seems calculated, probably unconsciously calculated, to further the delusion that science is a thing to be taken up in a leisure hour, but not a thing the study of which requires, while at the same time it increases, every activity of the mind. If the study of science is to be made a discipline, that study must not be pursued in the spirit of Dr. Youmans' book. The student must not content himself with a superficial knowledge of a few facts, nor even with gaining one or two generalisations; he must be taught to amass facts on the basis of his own observation, to separate the more important from the less important facts, to classify these facts and at last to rise to a generalisation which shall enable him to group together and so explain what had appeared to be isolated phenomena. Dr. Youmans' book can afford the student little help in such a process as this.

Of course it must be admitted that there is a large class of people who have neither leisure nor inclination to make science a study, but who are nevertheless desirous, and properly desirous, of knowing something of what science has done and of the way in which she has accomplished her work. Such people will, we are afraid, receive but little enlightenment from the work we are noticing. There is just sufficiency of detail to make the whole subject appear uninviting, but not enough to make the book valuable to the student. The mass of isolated facts is too great for the ordinary reader; he would soon, if not bewildered, become fatigued.

A book designed for the purposes stated in the preface to the present work requires to be written more from the standpoint of some central idea, round which is grouped together such a number of facts as may serve to illustrate and enforce that idea. The relation of the facts to the general theory and of the theory to the facts may then be made the means of inculcating a certain amount of true scientific training.

While we thus complain of the general scope of Dr. Youmans' book, we must give the author praise for the manner in which some parts of his work are written, more especially the chapter on theoretical chemistry. The chapters on descriptive chemistry are exceedingly meagre in details, but pretentious in the ground which they *appear* to cover.

M. M. PATTISON MUIR

Injurious Insects of Michigan. By A. J. Cook, of the Michigan State Agricultural College. Fourth Report of State Board of Agriculture for 1874.

THIS useful and instructive pamphlet is to a great extent compiled from the writings of Messrs. Riley, Fitch, Le Baron, Walsh, Harris, Curtis, and Packard. It is illustrated by numerous woodcuts from the able pencil of the justly celebrated Prof. Riley, of Missouri. Its object is to enlighten farmers, gardeners, and fruit-growers of the State of Michigan, as to the general appearance, structure, and habits of noxious insects; at the same time suggesting means by which the increase of these pests to agriculture may be arrested. The "Colorado Beetle" and the "Grape Phylloxera" occupy a conspicuous place among these enemies of man.

The pamphlet winds up with a valuable hint to housekeepers whose carpets are in danger from the attacks of the Clothes Moth. "Take a wet sheet or other cloth, lay it upon the carpet, and then run a hot flat-iron over it, so as to convert the water into steam, which permeates the carpet beneath and destroys the life of the inchoate moth."

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.*]

Water-supply of the Metropolis

I HAVE no intention of entering into a controversy in your columns with my friend Dr. Frankland, but his letter in your impression of March 16 seems to require some reply.

When I made the remarks which are called in question by Dr. Frankland, I was careful to say that I might not unfairly be accused of having done so from interested motives, an admission of which no one who reads Dr. Frankland's letter can say that he has not taken the full advantage. I am not ashamed of my occupation, and am quite ready to admit another historical parallel afforded by Jack Cade, and confess that I, or those who have gone before me, "against the king, his crown, and dignity, have built a paper-mill." But, whether paper-manufacturers "in the exercise of what they call their rights" are polluters of streams or no, is a question into which I never entered, and is entirely beside the points which I raised.

These are in the main avoided by Dr. Frankland. The two Commissioners, a portion of whose report I criticised, and of whom it is as well to observe Dr. Frankland is one, recommend that the Thames and the Lea should be entirely abandoned as sources of supply for domestic use in London, and particularly refer to the Chalk in the neighbourhood of London, and not to the distant springs of the upper Thames as the future source of supply. In his letter to you Dr. Frankland states that "The Commissioners advise that the drinking water of London should continue to be derived from its present sources, but that it should be led away to its destination before it is mixed with the sewage of Oxford, Reading, Windsor, and other towns, and before it is fouled by the filthy discharges of paper-mills, and by other disgusting refuse." I presume that these two statements can be reconciled, but looking at the proposal that the water should be procured "within a moderate distance of London" the calculations as to the area of 849 square miles of Chalk and Upper Greensand within thirty miles of London, and looking at the enormous expense of conveying water more than thirty miles, I took that radius as representing the area out of which some district was to be placed under unnatural conditions with regard to its springs and streams, in order to supply our vast metropolis, which I am told it is contemptuous to term "overgrown." I never spoke of the fertile meadows of the Thames valley, about which Dr. Frankland makes merry, and I never intentionally alluded in the slightest degree to the main valley of the Thames, except to say that both below and above London there might be spots in it from which a limited supply of water might be pumped without much injury to the neighbouring property. My comments were intended to be confined to districts in which the proposal of the Commissioners could be carried out of sinking wells below the present spring-heads, and so constantly drawing upon them that there should be always a void below the level at which the drainage naturally escapes. If this does not mean the drying up of the streams by cutting away their natural sources of supply I shall be glad to know what it does mean.

If Dr. Frankland were as well acquainted as I am with the gravelly soil of some of the low meadows in Chalk districts, he would cease to be surprised at the possibility of their being converted into "arid wastes" by the abstraction of the water with which they are now charged up to within a very few feet of their surface. In the valley in which I live I have known the peaty soil above such gravel, even without the artificial abstraction of the moisture below, become during a dry summer sufficiently arid accidentally to catch fire and continue burning for days.

But then I am told that the wealthy City of London would be able and willing to pay for any damage it might inflict in procuring its water supply. I can only say that the word "compensation" does not occur in the Index to the Report of the Rivers Commissioners, and I have sought in vain for any allusion to it in the text. Perhaps Dr. Frankland is not aware that at the present time the state of the law is such that even when compensation has been provided for by Act of Parliament, it has been held to be inapplicable in the case of wells being dried, on the ground that an action will not lie in respect of the loss of underground water, and therefore that no statutable damage has been inflicted.

As to the prescription for increasing the supply of spring water in a Chalk district by lowering the level of the subterranean reservoir, I may observe that in most of such districts floods are almost unknown, the soil being sufficiently absorbent to imbibe all the rain that falls, except when by chance the surface is frozen. The lowering of the water which, except in the valleys, is now usually from 100 to 200 feet below the surface, would make no difference in the receptive power of the soil on the hills, and could not be effected in the valleys without laying the streams, which now flow through them, dry.

As to London encountering the expense of a separate water supply for dietetic purposes, I can only say that if it can be effected for 2,000,000*l.*, as suggested by Dr. Frankland, it will in my opinion be far cheaper than the plan the Commissioners advocate. It is as a rule more economical to make use of what we have, than to discard all existing appliances and commence on a new system. Perhaps the Water Companies may have a word to say on this point.

The concluding paragraph of Dr. Frankland's letter seems to have been written under some misapprehension. I distinctly stated that "if we refer to the headings of Organic Carbon and Organic Nitrogen there can be little doubt of the superiority of the Kent Company's water." I may, however, be under some misconception as to the statistics under the awful heading "Previous Sewage or Animal Contamination," in which, possibly, I do not stand alone. What I ventured to suggest was that the Commissioners on the Water Supply of the Metropolis, within whose proper sphere this question lay, were not altogether wrong in reporting, that with perfect filtration and efficient measures taken for excluding from the rivers the sewage and other polluting matter, the Thames and Lea would afford water which would be perfectly wholesome, and of suitable quality for the supply of the metropolis.

If this proved impossible, then I ventured to point out that there was already in London a sufficient supply of water of the kind recommended by the other body of Rivers Commissioners.

I must not, however, waste your space and your reader's time, but will in a few words mention my principal reason for taking up this subject, which, however, apart from any such reason, I considered would be of interest to geologists.

It was this, that in an otherwise admirable and exhaustive public report, measures were advocated involving in all probability great inconvenience and loss to large tracts of country, without, so far as I could see, one single reference to such loss and inconvenience. With the advocates of a private scheme such a disregard of injury to others would be reprehensible, though possibly not uncommon, but some greater consideration of the interests involved might fairly be expected from a public document. JOHN EVANS

Nash Mills, Hemel Hempsted, March 18

Evidences of Ancient Glaciers in Central France

MANY lovers of natural history who have not the opportunity of seeing foreign scientific periodicals, may learn the advantage of taking such a paper as NATURE in the correspondence which was published between Dr. Hooker of Kew and the late Mr. Poulett Scrope, on the evidences of ancient glaciers in Central France.

The objections raised by Mr. Poulett Scrope, and the pleasure of examining such evidences as are adduced by Dr. Hooker, have induced me to accept the invitation of friends, who also enjoy such researches, to again visit Auvergne for the purpose of examining the Mont Dore valley for glacial traces, and I would gladly avail myself of any observations made by other geologists in that region, if they would do me the favour of sending me the notes of any localities to the address below.

In the meantime M. A. von Lasaulx, of Breslau University, claims the priority over Dr. Hooker in describing glacial traces in the *Ausland* periodical, in 1872, as occurring at the entrance of the "Gorge d'Enfer." I have also before me, as I write, a travelling note-book of Sir Wm. Guise, President of the Cotswold Naturalist Field Club (date, June, 1870), in which he refers to observing "moraines," "ice-action," "boulders," and "*bloc perchés*" in the same region.

My object in sending these lines to NATURE is to ask for notes of localities where glacial traces may be seen, as an aid to those who hope to examine more closely into the glacial phenomena of Central France. W. S. SYMONDS

Pendock Rectory, Tewkesbury, March 25

Metachromism

A FEW words of explanation may seem necessary after Mr. Ackroyd's observations (NATURE, vol. xiii. p. 385) on my previous letter regarding the above subject.

The question as to whether a change of composition can be said to *produce* or to *accompany* changes of physical properties, is a matter of words which the chromium series does not affect, as the relative number of atoms of the two elements is the test of arrangement followed.

With regard to the two colour scales—one co-existent with alterations of composition, the other with alterations of temperature—I never wished to "criticise" Mr. Ackroyd's results, but solely to point out a resemblance which I had observed a few years ago, and which I was not aware that that gentleman had noticed. The two series need not necessarily be similar; and, whatever other reasons may exist for placing white in the ultra-violet, the question in hand is not whether the ultra-violet rays produce the same sensation on our eyes as a mixture of all the colours, but, Do the white compounds in question, when spectroscopically examined, *only* show the ultra-violet, leaving the rest of the range in darkness, or do they show a complete spectrum? If the first, then of course their place of classification would be in the ultra-violet; but if they give a whole spectrum (as the compounds do to which I referred), then they must be classed as having an average refrangibility greater than yellow light (because they have blue in addition to it), and less than blue light (because they have yellow also), for the centre of luminosity (on each side of which the total of light rays is balanced) falls in the green.

If we had only to deal with monochromatic substances, then of course the usual *pan-spectral* white would not need to be considered, and green (as Mr. Ackroyd says) would be the only appearance to be classed between blue and yellow.

Thus "the assertion that white comes between yellow and blue" does not "rest upon the colour relation found to obtain between the oxides of the alkali metals," though it is in accordance with the rule given on p. 347, in the six sets of the oxides and chlorides there mentioned; the sole case not agreeing with it being that of the chromium chlorides, which, however, may be accounted for.

As to the orange colour of Na_2O_2, as Miller does not mention any colour, Turner was referred to; and if he is in error, that one instance may be laid aside; in any case it does not affect the relative natural order of blue and white.

Bromley, Kent W. M. FLINDERS PETRIE

Socotra

WHEN I wrote the letter to the *Times* about Socotra, alluded to in NATURE, vol. xiii. p. 414, I was not acquainted with the excellent topographical memoir on this island by Lieut. J. R. Wellsted, published in the Geographical Society's Journal for 1835 (Journ. R. Geog. Soc. v., p. 129). After perusing it I am more than ever of opinion that Socotra is well worthy of the attention of the naturalist, and may probably possess many most interesting indigenous plants and animals. Unless matters are very different from what they were in 1834, there can be little difficulty in exploring the island, and if, as we are told, it has really become British property, I trust we may not have to wait much longer for some information about its zoology and botany. "Socotran Aloes" and "Dragon's Blood" are at present almost its only known natural products, and Lieut. Wellsted mentions but one native animal—a species of Civet.

P. L. SCLATER

11, Hanover Square, W., March 27

Coloured Solar Halos

SOLAR Halos such as described by Dr. Frankland (NATURE, vol. xiii, p. 404), may be seen on about seventy-five or eighty days in the year, here, and are commonest in the spring, but it is extremely rare for them to be brightly coloured. I speak of the ordinary solar halo of about 22° radius, but the great halo of about 46° radius, is always distinctly coloured, though not a common phenomenon. It is not the "murky atmosphere" of London that hides the colours of the ordinary halo; they usually do not exist, except dull red and orange, and perhaps a faint tinge of blue. This is owing to the great breadth of the halo, which causes the colours to overlap and mix together; here it is very seldom that the halo is narrow and the colours consequently bright, as they seem to have been when seen by Dr. Schuster (p. 394). I doubt whether the name "parhelia," which he gave them, is correct; I understand that term to mean mock suns (or a bright small portion of a halo), a phenomenon visible here on thirteen days in a year on the average.

I may add that though I am rather easily dazzled, I find no difficulty in seeing halos with the naked eye.

Sunderland, March 28 T. W. BACKHOUSE

"Euclid Simplified"

MR. MORELL'S defence is a curious one, and amounts to this: "If my book is a bad one I am not to be blamed, because I have copied from Amiot, Legendre, and others. If I have made blunders in derivations, &c., again I am not to blame, but to be pitied, because I could not employ better printers." As in our former notice we limited our remarks to a few only of the objectionable features in "Euclid Simplified," so, in our present notice, we shall select a few only of the points put forward in Mr. Morell's letter, though we may observe in passing, that we see no reason to retract any of our previous comments. We think that our readers will agree with us when we state our belief that Mr. Morell has utterly failed in most, if not in all cases, to appreciate the force of our objections. Mr. M. correctly quotes Dr. Wormell (pp. 78-81), but fails to see that his own statement is widely different; had he written "perpendicular to the straight line A A′ through its centre" (p. 41), "perpendicular to A B through its middle point" (p. 42), we should not have found fault with him. Again, the reference to Mr. Gerard (p. 310) is not to the point; we can understand what is meant by a "segment capable of a given angle," but we still object to the term "capable angle." The revised definition of a *parallelogram* is now (see text and letter), "a quadrilateral of which the opposite sides are equal and parallel!" We did not object to the term *lozenge*, which is a well-known one, but to the way in which it was introduced.

We turned to Dr. Wormell's definition of *circumference* with some curiosity, and found that (with the exception of "plain" being printed for "plane") it was perfectly right, and that Mr. Morell had again failed to see the point in our citation of the schoolboy's definition. We contend that Amiot's sentence, as quoted by Mr. Morell, does not mean what Mr. M. makes it to mean. Dr. Wormell's use of G. C. M. is perfectly legitimate, but does not warrant, so far as we can see, the use of *R* for *right angle* (seeing it is conventionally applied to another purpose) unless, indeed, it be explicitly stated in the text that *R* is so used.

We said (p. 204) that in Theorem VI., p. 148, the reasoning is defective. Mr. Morell replies it "only errs by excess of proof." We will reproduce the "proof," and leave the decision to our readers. "The area of a trapezium A B C D is equal to the product of its height B E by the half sum of its bases A C and B D. Drop the perpendicular B E on A F, and *bisect it by line G H.* Produce the base A C to F, making C F = D B. Then the two triangles D H B and F H C which have for bases the base D B of the trapezium or C F = D B, and which have also the same height, ½ B E, are equal. The area of triangle F H C = ½ D B or F C × ½ B E; that of triangle D H B = ½ D B × ½ B E. These triangles, having equal angles, are therefore equal. But," &c. Upon this we remark, we are not told *how* G H is drawn—the pupil is to infer that it is parallel to B D. Now we must suppose H connected with B and F, and cannot assume that B H F is a straight line, hence, though triangles H F C, B H D are equal, it does not follow that angles F H C, B H D are equal, hence too we cannot assume A B F to be a triangle. But really we must apologise for taking up space with such elementary details. For Mr. Morell's benefit we give the following:—Produce A C to F, making C F = B D, join B F, cutting C D in H, then triangles C H F, B H D are equal, and triangle A B F = A B D C, &c.

Enough has been written on this, in its present form, objectionable book. At any rate we hope that any one who has thought of introducing the work into school use on the strength of one or two hastily written commendations of it, will be induced in consequence of what we have written, to examine the work for himself. We feel confident that any competent geometer who opens the book at almost any page, will endorse our criticisms, and say "the half was not told." In brief, the definitions are faulty, the enunciations are faulty, the proofs are faulty, and the typography is faulty; if these things do not make a bad book we do not know what does. The defence is, "if the enunciations are loosely and inelegantly worded, Amiot must bear the blame which attaches in a greater degree to our translations of Euclid." Alas! poor Amiot! this is an unkind cut, Mr. Morell! R. TUCKER

March 6

Bullfinches and Primroses

I HAVE a bullfinch which was hatched last summer after primroses were over. They were therefore quite new to him when I offered him the first I could get this season. He pulled it to pieces quite indiscriminately, biting stalk, flower, or calyx quite indifferently, and the same with a few more which were given to him at the same time. But since then he has often had a few at a time, perhaps twenty or thirty in all, and he now almost always bites out the lower part of the calyx, as described in NATURE, vol. ix. p. 482. Sometimes he bites a little too high up, but almost instantly tries again with better success. When that part is eaten he attacks the stalk rather than the corolla.

Last spring I offered primroses to four bullfinches belonging to friends. Not one seemed to pull the flower to pieces according to any method. Two of them I saw only once. Another (an old bird and somewhat shy), after being supplied with the flowers for several days, seemed as unskilful in picking out the tit bit as he was at first. The fourth was a young bird. His mistress was called away before she had heard what was the peculiarity for which I was watching. A few days later she told me she had been giving him primroses in the meantime, and had noticed that he ate only the green part. In those few days he had learnt the art of primrose eating, not indeed quite perfectly, but wonderfully well considering how little practice he had had. C. A. M.

Seasonal Order in Colour of Flowers

IT seems that Mr. Thiselton Dyer has thought fit to conclude the different observations made on this interesting subject by copying a part of Sachs's "Text-book." He will, I hope, allow me to point out to him the latest researches respecting the influence of light on the colour of flowers, published by E. Askenasy, in the *Botanische Zeitung*, 1876, Nos. 1 and 2. This author made experiments with several flowers which had sufficient food at their disposal, and found that some of his flowers changed their colour when placed in the dark, while it was not so with others. Therefore it cannot be said that light has no influence. The cause of this difference, observes the author, has as yet not been explained; other experiments will have to be made to clear up this point.

I think the colour of most flowers is a thing that by continued inheritance during a very long lapse of time has become almost constant, and cannot be changed in a few weeks or months. Long-continued experiments with the same flowers and their offspring would, perhaps, show more considerable changes than Askenasy found.

So much for the point referred to by Mr. Dyer.

As to the seasonal order itself, a continuation of Mr. Alexander Buchan's observations would be necessary, and probably also experiments with the several parts of the spectrum to which the flowers are to be exposed.

As this letter was written, I read that of Mr. Wm. Ackroyd (NATURE, vol. xiii., p. 366); doubtless every one will expect with great interest his following note. J. C. COSTERUS

Amsterdam

Plant Fertilisation

SOME short time since I observed a rather curious case of plant fertilisation through the medium of insects, and thinking—as the subject is one which is attracting much attention from botanists at present—it might be interesting to some of your readers (more especially perhaps as occurring in this remote part of the world), I take the liberty of forwarding you the particulars in the hope that you can find a corner for them in your valuable journal.

Growing rather abundantly just on the coast here is a small shrub belonging, I believe, to the sub-order Coffeæ, having numerous small greenish flowers, the interior of the corolla tube filled with silky white hairs, and the style bent in a peculiar manner, so as to bring it to one side of the tube. I observed the anthers delusce before the flower buds open covering the stigmatic surface (which is simply a thickened continuation of the style) with pollen. I noticed that all the individuals of this species of shrub were visited by a kind of ant in large numbers, and as soon as a flower opened they began pulling out the hairs, lining the corolla tube, and often biting off the stamens also, in order to clear a way down to the nectar contained at the bottom of the tube. In doing so they often support themselves by clinging to the pollen-covered style with their posterior legs. The bend in the style which brings it to the side of the corolla tube prevents it from being an obstruction while they are obtaining the nectar, although, so eager are they to get it even to the last drop, that in a few old flowers I noticed even the style removed. The pollen keeps dry for a considerable time, so that cross-fertilisation is effected by the removal of pollen from the *stigma* of one flower to that of another.

We have here, therefore, several adaptations of structure and habit to ensure that end. The deluscence of the anthers while in the bud removes the pollen from a part of the flower where it would in all probability be wasted (when the ants bite off the anthers) to another part, where by a peculiarity in its structure, viz., the bend in the style, it is protected and transferred to other flowers. The hairs in the corolla-tube, by rendering the approach to the nectar difficult, and thus making the use of the style as a support needful, also increase the chances of cross-fertilisation. M. S. EVANS

Durban, Natal, South Africa, Jan. 25

The Visibility of Mercury

PERHAPS some of your readers may, like myself, have been struck with the remarkable brilliance of Mercury to the naked eye on the evening of January 26. I scarcely ever remember to have seen the planet so well deserving the epithet στίλβων. Since April, 1858 I have noticed it twenty-one times with the naked eye at its evening apparitions. It seems difficult to reconcile the lament of Copernicus that he would die without seeing Mercury with the accounts of his life. The common reason given is, that it was always enveloped, to him, amid the vapours of the Vistula. But he did not pass all his life in that part of Europe. At one time he went to Bologna and stayed with Dominic Maria, a professor of astronomy in that place. After this he proceeded to Rome, where he was made professor of mathematics, and where we find him actually engaged in making observations about the year 1500.

The amateur may look out for Mercury near the western horizon, after sunset, about the following dates:—1877, Jan. 10, April 29; 1878, April 10; 1879, March 26; 1880, March 7; 1881, Feb. 20; 1882, Feb. 2; 1883, May 6; 1884, April 18; 1885, March 31; 1886, March 15.

Tycho Brahe, who could not have enjoyed a very favourable latitude for picking up the planet, gives us the following notes in his "Historia Celestis":—

1585, Nov. 15.—"Apparuit hoc tempore matutino ☿ tanquam rubricunda quædam stella secundæ magnitudinis et mediæ, quasi 2 et 1 magnitudinis."

1590, March 1.—"☿ adamodum apparenter videbatur, instar stellarum primæ magnitudinis, adeo ut eam, quæ in dextro humero Orionis est, magnitudine visibili repræsentaret. Si ☿ diametrum visibilem feceris 2¼′, non inconvenienter se habebit."

1596, March 15.—"Erat hâc vesperâ apprimè serenum et mediocriter tranquillum. ☿ hâc vesperâ satis fuit conspicuus quippe cujus quantitas stellam inter primæ et secundæ magnitudinis referebat."

Measurements of the diameter of the planet are best obtained when it is seen in transit on the sun, of which there will be a very favourable opportunity for several hours on May 6, 1878. After this, it is doubtful whether we shall see Mercury on the sun again this century in England, as he passes off the solar disc on May 10, 1891, about half-an-hour after sunrise, and on Nov. 10, 1894, the ingress of the planet is only a few minutes before sunset. For a transit to be seen thoroughly from this country we must wait till Nov. 12, 1907, and Nov. 6, 1914, both of which will be visible throughout here.

SAMUEL J. JOHNSON

Upton Helions Rectory, Crediton, Feb. 21

How Typhoid Fever is Spread

THE case recently reported (NATURE, vol. xiii. p. 331) by Prof. Frankland from a Swiss village, where the poison of typhoid fever is said "to have filtered through a mile of porous soil, but which had nevertheless lost none of its virulent properties," is certainly so striking that some further reference to authorities seems requisite. If it can be satisfactorily proved that this was the most likely and reasonable origin of the case, it will give additional weight to every endeavour for preserving our water supply from every conceivable impurity; but unless based on the opinion of a competent and skilled investigator of such cases, it will lay us open to the charge of receiving any similar statement that favours our view, however rash. As medical officers of health it is our duty frequently to trace the origin of cases of this disease, and my anxiety to have further information of this case will thus seem reasonable, and I hope will meet with some reply from the distinguished professor. J. MITCHELL WILSON

Rochdale, March

The Ash Seed Screw

MR. ALFRED GEORGE RENSHAW would like to know (NATURE, vol. xiii. p. 367) whether the pitch of the ash seed screw is that which would give most power to the propeller of a steamer.

There is no screw on the samara of the ash (*Fraxinus excelsior*) while it is green. The pitch of the screw, at the same date, differs on different trees, and also on different seeds of the same tree, and also on different parts of the same seed.

Why the wing generally becomes twisted as it dries is a very interesting question. But what seems to me the most remarkable fact about this phenomenon is, that in every case, and on all trees alike, the thread of the screw is in one direction; that direction being the same as in a cork-screw, or ordinary screw-nail.

All varieties of the wild oat (*Avena fatua*) and the fly oat (*Av. sterilis*) have long awns, which also in the green stage are straight, but which in ripening become twisted. And in these also, the direction of the screw is uniform and the same as in the ash seed; but the pitch of the screw is variable.

Are these facts the same in other parts of the country? Are the screws left-handed on the south of the equator?

Summerhill, Aberdeen, March 20 A. STEPHEN WILSON

OUR ASTRONOMICAL COLUMN

THE COMPANION OF SIRIUS.—Mr. Wentworth Erck, of Sherrington, Co. Wicklow, in a communication dated March 21, notes the fact of a considerable diminution in the angle of position of the small star accompanying Sirius, which was detected by Alvan Clark in January, 1862—since the earlier measures, and adds that he cannot now estimate it at more than 55°. This retrograde motion is a consequence of the theory of Dr. Auwers, supposing the *comes* to be the cause of the anomalous proper motion of Sirius, which has formed the subject of several elaborate memoirs by this eminent astronomer. His last elements of the presumed disturbing body, adapted to the form of double star orbits, are as follow:—

T	1843·275	Excentricity	0·6148
Node ...	61° 57′·8	Semi-axis	2″·331
λ	18° 54′·5	Period	49·399 yrs.
Inclination	47° 8′·7	Motion, retrograde.	

The following angles of position and distances are given by these elements:—

		°		″			°		″
1862·0	Pos.	85·4	Dist.	10·10	1874·0	Pos.	65·5	Dist.	10·95
1865·0	,,	79·9	,,	10·78	1876·0	,,	62·1	,,	10·59
1868·0	,,	75·0	,,	11·15	1878·0	,,	58·4	,,	10·05
1871·0	,,	70·3	,,	11·20	1880·0	,,	54·2	,,	9·33

Dr. Auwers gives a comparison of his calculated angles and distances of the centre of gravity, with those of Clark's companion to 1867, the agreement being pretty close throughout. But there can be no doubt that the calculation has given the angle too great since that year. The Washington observations show the following differences:—

1872·25 ...	Position $(c-o)$	+ 5°·6 ...	Distance $(c-o)$	− 0″·40
1874·18 ...	,,	+ 6 ·3 ...	,,	− 0 ·46
1875·24 ...	,,	+ 7 ·2 ...	,,	− 0 ·72

In the above orbit the limits of distance are 2″·31 at 1841·84 for a position of 302°·5, and 11″·23 at 1870·13 for position 71°·7, and Auwers remarks that under the former condition the angle changes one degree in ten days, while under the latter condition 233 days are required for the same diminution.

Our correspondent mentions that Lassell's "new star" (Mem. R. Astron. Soc., vol. 36, p. 18), "though exceedingly faint, was distinctly visible. Pos. circa 130°, distance circa 75″," with 7½ inches aperture power, 200. Lassell, 1865, January 14, found the position 127°·0, distance about one minute.

Dr. Gylden, from the meridian-altitudes of Sirius, observed by Sir Thomas Maclear at the Cape of Good Hope during the years 1836 and 1837 found for the annual parallax of the star 0″·193 (*Bulletin de l'Acad. de St. Petersbourg*, t. vii.). Adopting this value, Dr. Auwers finds for the mean distance between the companion and Sirius, 37 times the earth's mean distance from the sun, and for the masses of Sirius and companion 13·76 and 6·71 respectively, in units of the sun's mass. The parallax 0″·193 corresponds to 1,068,700 times the sun's distance from the earth.

D'ARREST'S COMET.—M. Leverrier's *Bulletin International* of March 18, contains the definitive elements obtained by M. Leveau for the next return of this comet to perihelion in 1877, with an ephemeris for every 20th day throughout the year, which sufficiently defines the circumstances of the next appearance. The whole of his long-continued investigations relating to the motion of this comet have been conducted by M. Leveau with extreme care and minuteness, so that by his investigations, in continuation of those commenced by M. Villarceau, the theory of D'Arrest's comet has been placed upon a similar footing of accuracy to that, upon which the theory of the periodical comet of Faye now stands through the labours of Dr. Axell Möller of Lund.

The next perihelion passage of D'Arrest's comet is found to occur 1877, May 10·339 M.T. at Paris, and the comet appears to attain its maximum intensity of light about a fortnight subsequently. Other elements of the orbit which apply to 1877, Jan. 14, are:—

	°	′	″	
Longitude of the perihelion	319	9	15	M. eq. 1880·0
,, ,, Ascending Node ...	146	9	28	
,, ,, Inclination	15	43	9	
,, ,, Excentricity	0·6278048			
Logarithm of perihelion distance ...	0·1199444			

Motion direct.

The elements usually found in catalogues of cometary orbits are here substituted for others given by M. Leveau.

The semi-major-axis of the orbit is 3·54139, to which corresponds a sidereal revolution of 2434·2 days.

When this comet was last observed by Herr Julius Schmidt at Athens, on December 20, 1870, the intensity of light was 0·154; the greatest intensity attained in 1877, about May 26, is only 0·22, and it is certain that large telescopes will be required for the proper observation of the comet. When theoretically brightest, it rises at Greenwich less than two hours before the sun, and as this difference increases towards the end of the summer, the intensity of light is diminished by one-half. The comet will be nearest to the earth in the middle of October (distance = 1·40). M. Leveau promises in a subsequent communication a precise ephemeris for the whole year, and, it may be remarked, it very rarely happens that there is any necessity for predictions extending over so long a period.

PROF. HUXLEY'S LECTURES ON THE EVIDENCE AS TO THE ORIGIN OF EXISTING VERTEBRATE ANIMALS [1]

III.

IT will be necessary to preface our remarks as to the origin of the next highest group of Vertebrates—that of Reptiles—by some account of the distinction between them and the Amphibia, and by some observations on what zoologists mean by the terms "higher" and "lower" as applied to animals or groups of animals.

In external form there is little difference between such a reptile as a lizard, and such an amphibian as a newt, and there seems, at first sight, to be no reason why they should be placed in different primary groups. In former times, as a matter of fact, the essential difference between reptiles and amphibians was not seen, and the two were united into a single class; but modern researches have shown that, beneath this external similarity, lie great and important differences, the chief of which we must now consider.

In the first place, no reptile, at any period of its life, possesses gills, and, in consequence, the breathing of air dissolved in water becomes impossible. Nevertheless, reptiles, in common with all the higher animals, have, at one period in their existence, slits leading from the throat to the exterior, in precisely the same position as the branchial clefts of an amphibian, but functionless.

Secondly, certain organs, known as "fœtal appendages," are developed in connection with the young animal before it leaves the egg, and serve a temporary purpose in its economy. In the possession of these appendages, as well as in the absence of gills, reptiles agree with birds and mammals, and differ from fishes and amphibians.

The young reptile is produced from an egg of relatively large size, and consisting of a considerable mass of yolk, surrounded by a quantity of transparent "white" or albumen; the whole being invested by a hard or soft shell. The yolk does not divide as a whole, but the process of division is confined to a small patch on its surface; in fact, the reptilian egg answers to the amphibian egg, *plus* a quantity of additional matter, called accessory, or food-yolk, which is unaffected by the process of yolk-division. It is the small superficial patch, answering to the whole amphibian egg, which is converted into the body of the young reptile, the accessory yolk becoming gradually smaller and smaller, as its substance is used up in the nourishment of the embryo; in the meantime it forms a bag attached to the umbilicus of the embryo, and hence called the *umbilical vesicle* or yolk-sac; it is the first of the fœtal appendages, and the only one which occurs in any vertebrate below a reptile, being possessed by certain fishes.

After the embryo has attained a certain size, and has come to lie, like an inverted boat, on the yolk-sac, a fold grows up, all round it, from the surface of the yolk, and, the edges of the fold coming together above, a bag is formed enclosing the embryo into the interior of which a watery fluid is secreted, in which the little creature lies. This natural water-bed is called the *amnion;* it is the second of the fœtal appendages, and no trace of it is to be found in any fish or amphibian.

The third and last of these curious embryonic appendages, the *allantois*, grows out from the tail-end of the embryo as a pear-shaped body, solid at first, but soon converted into a sac, which extends round the embryo and yolk-sac, immediately beneath the membrane of the shell. The cavity of the allantois acts as a receptacle for the nitrogenous waste of the embryonic body, but its chief function is as a respiratory organ; for this purpose it is supplied by blood-vessels which form a close network over its outer layer, and the blood contained in these coming into close relation with the external air, through the porous shell, readily exchanges its carbonic acid for the atmospheric oxygen.

As the embryo grows, the yolk-sac becomes smaller and smaller, and is eventually completely drawn into the interior of the body of the young reptile, which by this time completely fills the shell. In many cases a horny knob is developed on the nose, and, with this, the now ripe embryo breaks the shell from the interior; the amnion and other membranes are burst, the allantoic circulation is stopped, the first inspiration is taken, and the little creature is born.

There are several minor points in which reptiles are distinguished from amphibia, amongst which we will only mention the articulation of the skull to the first vertebra by one condyle instead of two, the presence of a bone called the *basi-occipital* in the hinder part of the skull floor, and the fact that the branchial apparatus is reduced in the adult to the small *hyoid* bone or cartilage, which supports the tongue.

In what respects is a reptile a higher organism than an amphibian? When one animal is said to be higher than another, one of two things may be meant: its structure may be more complicated, as a carved platter is higher than a simple trencher; or its parts may be so arranged as to form a more complicated mechanism. The mere repetition of parts does not raise an animal in the scale; a worm with a hundred segments is no higher than one with ten, any more than a mill with ten pairs of stones is a higher kind of machine than one with a single pair. But if, instead of multiplying the number of millstones, two pairs only were used, one of which was adapted for coarse, the other for fine grinding, a machine of a far higher order would be produced, and it is a similar differentiation of parts for special uses and co-adaptation of structures to given purposes which raises an animal above its fellows.

Judged by this standard, a reptile is a decidedly higher animal than an amphibian; its skeleton, for instance, is a better piece of work, the joints being more neatly finished, and the whole mechanism much more perfect.

A third test is based on the facts of development. We saw that a frog, in the course of its development, went through a stage in which it was, to all intents and purposes, a fish, and that it was only after passing through this stage, as well as that of a branchiate amphibian, that it attained its higher adult character. Now the reptile stands in just the same relation to the amphibian, with regard to its development, as the amphibian to the fish. During the earlier stage of its growth it presents certain amphibian characters, such as the presence of gill-clefts; but these lower stages are passed over; the reptile goes beyond the highest amphibian in its development, and is therefore, in this respect also, to be considered as a higher animal.

At the present day there are four types of reptiles: the lizards (*Laartilia*), snakes (*Ophidia*), turtles and tortoises (*Chelonia*), and crocodiles (*Crocodilia*). We will now direct our attention to the first of these groups.

Most existing lizards have four well developed limbs, a long tail, a scaly armour, sometimes supplemented with plates of bone, and teeth, not set in distinct sockets, but firmly fixed to the jaw. The skull is so constructed that the hinder nostrils open far forwards into the mouth. The vertebræ have a peculiar and characteristic form, their articular surfaces being concave in front and convex behind, except in the Geckos or wall-lizards, and that remarkable New Zealand genus *Hatteria* or *Sphenodon*. The heart is composed of three chambers, two auricles and a single ventricle, the latter being again partly divided into two, and thus showing a slight advance on the amphibian heart, in which the ventricle is quite single.

Lizards are very abundant, especially in hot climates;

[1] A course of six lectures to working men, delivered in the theatre of the Royal School of Mines. Lecture III., March 13. Continued from p. 412.

most of them are land animals, a few only being inhabitants of fresh water, and one—the genus *Amblyrhynchus* of the Galapagos Archipelago—lives on the seashore, and, if hard pressed, takes to the sea.

Through the whole of the Tertiary epoch the lizards are essentially the same as those now existing. Some of the Secondary species, also, have the same characters, but in the chalk are found, in addition, strange marine lizards, such as the genus *Mosasaurus*, which attained a length of 30 feet. As far back as the Purbecks, the lizards have vertebræ like the existing kinds, but on descending to the Solenhofen slates we find abundant remains, which present the lower character of bi-concave vertebræ, and the same is true of all the still older forms, such as the *Telerpeton* of the Triassic sandstones of Elgin and the Permian *Protorosaurus*.

Thus the older lizards have a slightly simpler structure than those of the present day, but resemble them, on the whole, so closely, that we must conclude our existing forms to have been derived from the ancient ones, and have no need whatever to assume their special creation. Lizards, then, offer another example of what is meant by a persistent type.

A remarkable instance of this persistence is afforded by a case of quite the same order as that of Ceratodus, described in the first lecture. The Hatteria, mentioned above, differs from all other lizards in many particulars. Its jaws are armed with a horny beak, and its upper jaw has two rows of teeth, one on the maxillary, the other on the palatine bones; the teeth of the lower jaw bite between these, like a pair of scissors with a double upper blade. The vertebræ are bi-concave, and, along the belly, are placed a number of bony plates.

No other existing form whatever is known presenting these characters, but, about the year 1858, a number of fossils were discovered in the sandstone of Elgin, and amongst them the remains of a large lizard with bi-concave vertebræ, abdominal plates, a horny beak, a double row of upper jaw teeth, and, in fact, altogether like the existing Hatteria.

The crocodiles are the only other reptiles the history of which it will be possible to notice in this course. Two of the most important characters by which they are distinguished from lizards are, the lodgment of the teeth in distinct sockets and the position of the hinder nostrils or posterior nares. The maxillary, palatine, and pterygoid bones are so disposed as to form a remarkable shelf or partition in the roof of the mouth, thus bringing the posterior nares to the hindermost part of the throat. The soft palate forms a veil in front of these apertures, and hangs down so as to rest on the back part of the rudimentary tongue, and thus, except when the animal is swallowing, entirely shuts off the cavity of the mouth from the air passages. This arrangement has been prettily explained by the crocodile's habit of killing its prey by drowning; it is said that it can hold a captured animal under water, while its own nostrils—placed at the end of the long snout—are just above the surface, and thus is enabled to breathe freely, the air passing through the posterior nares, behind the veil of the palate, and so to the lungs, while its prey is being suffocated. This is an admirable explanation as far as the crocodile is concerned, but, unfortunately, it is probably untrue, for precisely the same arrangement is found in the Gavial and other crocodilians which live upon fish.

(*To be continued.*)

PHYSICAL SCIENCE IN SCHOOLS

PROF. ROSCOE has tak n the right view when he says that science teaching in schools will remain unsatisfactory as long as it does not receive the same range and time as the subjects which at the present time preponderate so greatly. Granting the necessity of devoting more time to science, it follows, almost as a matter of course, that science teaching ought to begin at an earlier age than now. For else, where is the time to come from? The other alternative—to add a couple of years to the time required to pass through the present curriculum of a public school—would be accepted by very few parents. But there is no need for this alternative. The teaching of the elements of physical and chemical knowledge is most beneficially begun in early years. Some of the foremost thinkers of the scientific world assert and support this view, as may be gathered, for instance, from the Sixth Report of the Royal Commission on Science Teaching. I would mention, in addition to this, that Liebig strenuously advocated ("Chemische Briefe," Leipz. und Heidelb., 1865, 50th letter) the teaching of such elementary chemistry in *village schools* as bears upon the constitution of air, water, the ash of plants, and explains the process of combustion. The great German philosopher would hardly have done so, without being sure that the pupils will profit by the teaching. The average age of such a pupil is, I believe, twelve years, and he receives, as far as I am aware, no preparatory instruction in algebra or geometry.

It is surprising to find a man of the educational eminence of Mr. Wilson battling against early science teaching. I am inclined to ascribe his opinion on this matter to an incomplete view taken by him of the true significance science teaching has. Mr. Wilson considers the study of physical science as a means of developing merely the reasoning faculty of a boy, leaving out of sight the equally important function of calling forth and sharpening the faculty of *observation*. As for reasoning alone, certainly, the languages, and still more, mathematics, afford at least an equally good basis. It is just the circumstance that a sound teaching of science shows to the young mind the difference between evidence as resting (wholly, or to the greatest extent) on the *teacher's statement*, and evidence based on *facts put actually before the pupil*, which makes the study of science so valuable from a general educational point of view.

Early beginning of science teaching suggests itself for yet another reason. Everyone, with but the least experience in educational matters, knows that in order to be successful in instruction, one must repeatedly go over the same ground during the curriculum of a boy's education, and gradually expand the subject in the repetition. Why, then, shall not science, if it is to enter organically into the education of a boy, and not be merely tacked on to him, receive the same treatment? Let a boy at the age of ten or eleven begin with witnessing all the experiments which are usually performed in illustrating those sections of Chemistry and the science of Heat, that are required from the candidates of the London University Matriculation Examination. Let the boy become thoroughly acquainted with the *facts*, and let at this time as little theory be placed before him as possible. After such a course, which might be made to fill up two years, there should be a pause in the study of these branches of science for a year, or even two, before allowing the pupil to resume the same in a fuller and more theoretical way. The hours gained might be given to mathematics. Of course it would be out of place to give here anything like a programme of how the above idea should be realised; I must just content myself with throwing out the hint. After the initiatory course the pupil will be in better condition to follow later the theoretical parts than he is under the present system, where he has to overcome simultaneously the novelty of the facts and the difficulties of the theory.

Science acquired in this way will be very different from that which is hastily got up in the last six or eight months of a boy's stay in the school, and mostly, too, under the pressure and anxiety which accompany the preparation for some examination, say the London University Matriculation.

The scientific culture which pervades the educated classes of Germany is usually ascribed to the efficiency of the Universities; I believe it would be more correct to put it down to the sound education in scientific matters which the boys receive in the public schools, for it is only the smaller number who proceed to the Universities: the majority go after commercial and industrial pursuits.

R. GERSTL

University College School, London

ON DETERMINING THE DEPTH OF THE SEA WITHOUT THE USE OF THE SOUNDING LINE[1]

THIS is the title of a paper which has been presented to the Royal Society, and Mr. Siemens gave at the meeting of the 24th ult., a description of the instrument which he has designed with this object. He commenced by giving a mathematical statement of the effect of local attraction, to a certain depth, on a body placed at the surface of the earth, assuming it to be of uniform density, spherical in form, and unaffected by centrifugal action. For small values of depth (h), this attraction is $2\pi h$, the original formula from which this is adduced is :—

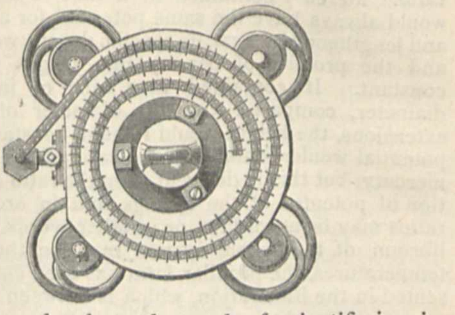
$$2\pi h\left(1-\frac{2}{3}\sqrt{\frac{h}{2R}}\right),$$

and by substitution of $2R$ for h in this, Newton's statement of the total attraction $\frac{4}{3}R\pi$ is obtained.

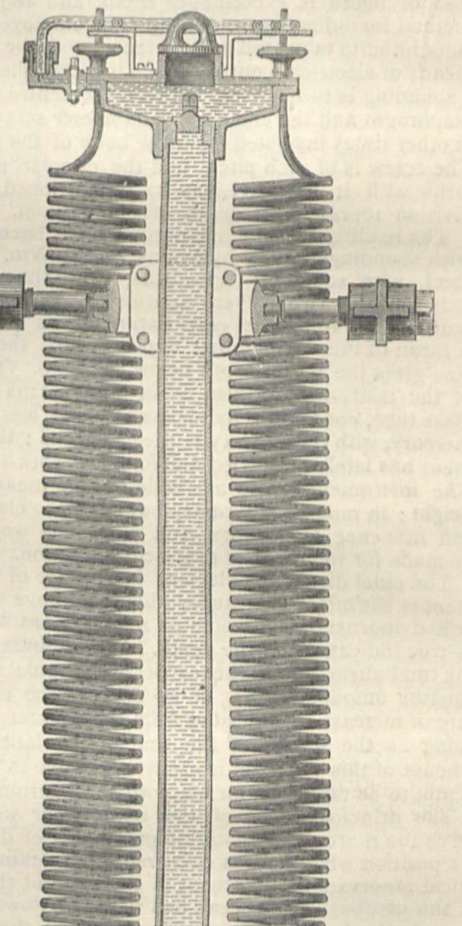
Now, if in place of the solid substance which forms the exterior crust of the earth, whose density may be taken to be the mean density of superficial rock, water, a material of less density is substituted, it is shown that the total attraction must be diminished, and the measure of this diminution is a measure of the depth of light substance which has been substituted for heavy. If we were in possession of the exact mean density of the earth, of that of the surface-rock, and of sea-water, a scale could be calculated beforehand, to show what depth would agree with a certain diminution of the measured effect of gravitation. Such an approximate calculation was made in designing the instrument, but Mr. Siemens has preferred to compare the readings of the instrument with actual soundings, in order to obtain a scale.

The instrument which is called a bathometer is represented in the accompanying illustration, and consists of the following parts: a weight being a column of mercury affected by variation of gravitation, a counterbalance being springs unaffected by variation of gravitation, and an arrangement by which the variations in gravitation can be read as depth in units. The column of mercury is maintained in a vertical steel tube having cup-like extensions, the lower portion being closed by a corrugated diaphragm of thin steel plate, and the upper portion containing an aperture for filling the instrument, having a screw stopper. The internal diameter of the tube is reduced at the upper portion, in order that the vertical oscillations of the mercury produced by the motion of a vessel in a sea-way, may be reduced to a minimum, and the instrument is suspended in a universal joint above its centre of gravity, so that it may always hang in a vertical position at sea, and is enclosed in an air-tight casing so that it may not be under the influence of atmospheric changes. The weight of the column of mercury is balanced at the centre of the diaphragm by the elasticity of the steel springs, and the *modus operandi* of the instrument is evident; as the mercury diminishes in potential through the effects of diminished attraction, the action on the springs diminishes, and these shorten upon themselves.

There are some peculiarities in the mechanical arrangement of the instrument which repay examination. Both ends being open to the air, its indications are not affected by variations of atmospheric pressure. With regard to temperature, the instrument is *parathermal*.



Its peculiar form has been the result of scientific inquiry. It was first discovered by experiment that well-tempered



steel springs diminished in potential with rise of temperature in a constant ratio, it was therefore necessary that

[1] By C. William Siemens, D.C.L., F.R.S.

the potential of the mercury should diminish in the same ratio. Mercury contained in a tube of uniform section would always have the same potential, for as it expanded and lengthened by heat, its specific density would diminish, and the product of density into height would remain constant. If contained in a tube of infinitely small diameter, compared with the diameter of the cup-like extensions, the height would remain constant, whilst the potential would diminish in the ratio of the expansion of mercury, but this is different from the ratio of the diminution of potential of the springs, and in order that these ratios may be accordant, or in other words that the equilibrium of the whole system may be the same at all temperatures, the peculiar form has been employed represented in the illustration, which is between the two forms already referred to.

The amount of the variation of gravitation with variation of depth is exceedingly small, and requires some method for indicating it. The method employed in the results hitherto tabulated and presented with the paper is by means of electrical contact, which is established whenever a sounding is to be taken, between the centre of the steel diaphragm and the end of a micrometer screw, which is at other times insulated from the body of the instrument. The screw is of such pitch, and the circular plate which turns with it has divisions so proportioned, that each division represents a depth of one fathom.

The readings of the instrument have been compared with soundings taken by means of Sir Wm. Thomson's steel wire sounding apparatus, and the accordance between the two is very satisfactory, especially as the bathometer, from the very nature of its action, gives a mean of the surrounding depths, whilst the sounding-line gives the actual depth below the ship. The reading of the instrument is also effected by means of a spiral glass tube, connecting by means of liquid, less dense than mercury, with the mercury in the upper cup; this arrangement has lately been tried and found to work successfully. The instrument is also available for the measurement of height; in mountain ascents, however, the elevated land will influence its readings, and allowance would have to be made for the effect of this local attraction.

The chief disturbing element in the use of this instrument is the effect of latitude, which will have to be ascertained approximately before its readings can be accepted as true indications of the depth. The difference between the total attraction of the earth at the pole and on the equator amounts to $\frac{1}{180}$ of its effect at the equator, the rate of increase in travelling from the equator to the pole being as the square of the sine of the latitude. The amount of this variation is easily calculable in fathoms of depth, to be tabulated for use with the instrument.

The principal value of the bathometer would be to serve the mariner as an additional means of determining his position when he was debarred from taking astronomical observation on account of the state of the weather. If the contour of the ocean bed were laid down on charts more perfectly than it is at present, and if these were in the hands of the mariner, he would be able to tell from his bathometer what was the depth of ocean below him, and whether that depth was increasing or decreasing in pursuing his course; he could also observe the rate of increase or decrease of depth, and in consulting his chart he would be enabled to determine his actual position with considerable accuracy, and thus be forewarned of the approach of danger.

PHOTOGRAPHY OF THE RED AND ULTRA-RED END OF THE SPECTRUM

IN 1874 Dr. Vogel communicated to the scientific public that he had been able to photograph the least refrangible rays of the solar spectrum by using what are known as the Uranium Dry Plates prepared by the Uranium Dry Plate Company. At the same time he experimented with other bromide plates, using dyes to give them distinctive tints. He then enunciated that the sensitiveness of the plates for the hitherto unphotographed portion of the spectrum was due to the colours employed, and apparently all his efforts have been diverted in elaborating this idea. Last summer I commenced a series of similar experiments to see whether the discovery could be made of practical use in photography; and have arrived at the conclusion, that the colouring matter gives this extended sensitiveness owing to the compound of silver formed and not to the colour itself, in fact, that the tint given to the film necessitates a very prolonged exposure. The additions of resins, nitro-glucose, and other similar compounds containing carbon to the bromized and bromo-iodized collodion soon convinced me that it was the organic salts of silver to which we must look for sensitiveness in the yellow red and ultra-red rays of the spectrum. Nearly every resinous body seems to prolong the photographic spectrum towards the ultra-red, and one or two in particular, also when the film is least colourless when viewed by transmitted light, that then it is probably in the most impressionable condition. Another point which is worthy of notice is, that a film dried from moisture, taking, in fact, the form of a dry plate, is always most readily acted upon by the ultra-red end of the spectrum. Probably this is due to the absorptive qualities possessed by the silver nitrate solution, and not really from an increased sensitiveness of the compound salts when dry.

If ordinary pyroxylin be employed for the collodion it is generally less suitable than if it contain a certain quantity of nitro-glucose, or other similar body; and it frequently happens if this be absent entirely that the photographic spectrum will stop short near *b*.

Taking a collodion made with pyroxylorin prepared at high temperatures and using the ordinary solvents (in the alcohol of which suitable resin and bromides are dissolved), it will be found in general that when the silver salts formed through them by the sensitizing bath, or by emulsifying them by direct addition of silver nitrate to the collodion, are presented to the action of the spectrum, the whole of it will be impressed with a developable image. With three prisms of 60° and one of 45° twenty minutes is sufficient exposure to give when the slit is nearly closed, a lens of four feet focus being used as the objective.

When this is overcome it will be possible (and I hope shortly to do so) to present a complete photographic map of those lines which lie beyond A to a distance at least equal to D − A, a point beyond which I have not as yet been able to obtain an image. The great difficulty to be encountered is that of finding a sharp focus for the different points of the invisible spectrum, the change in length from one point to another being very rapid.

Uranium and iron salts have also furnished me with spectra which are well worthy of notice. With the latter salt more especially the action of the heat-rays is very decided, though at present it seems to me that the exposure must be very prolonged.

I propose at a later date to give details of all the most interesting of these experiments sufficient to enable anyone to repeat them who may desire to do so.

W. DE W. ABNEY

P.S.—It may be as well to state that the best results with resin plates have been obtained when a modification of alkaline development has been adopted.

RAOUL PICTET'S SULPHUROUS ACID ICE-MACHINE

THE countries between the 40th degrees of N. and S. latitude have in general too temperate winters to admit of natural ice being obtained in any quantity; and yet these are the countries in which it is most required.

The high price at which it is sold prevents many people from buying it. It is for the purpose of rendering the supply of this useful and healthful article abundant and cheap that ice-making machines have been devised. These are of three classes, of which we shall give a brief account in order to show the advantages of the new invention, due to the ingenuity of a young Genevese physicist, M. Raoul Pictet.

The first class comprises the Ammoniacal Machines. These are based on the principle, applied by M. Carré, of the solution of ammonia in water. A saturated solution of ammonia is introduced into a boiler which is heated to 140° or 150° C. The ammonia is disengaged under strong pressure, and is liquefied in a condenser washed by a current of ordinary water. The liquid ammonia flows into the refrigerator, where it is evaporated and it returns into the liquid in a gaseous form. The evaporation is the source of the cold, which may be made very intense.

In order to work an apparatus of this kind, it is necessary that the first operation be effected, *i.e.*, the liquefaction of the ammonia in the condenser. For this purpose it is necessary that the pressure in the boiler correspond to the tension of ammonia vapour, at the temperature of the water of condensation, 30° C. at least in warm countries. This pressure is raised then to fifteen or eighteen atmospheres, an enormous pressure, liable to great danger.

Under so great tension the gas traverses cast-iron walls of two centimetres, the joints leak, not being air-tight, and the ammonia rapidly escapes from the apparatus. The alkaline solution must then be constantly renewed, which is very costly. There is another inconvenience. The fire under the boiler causes deposits, which, increasing day by day, give rise to the danger of an explosion. The fear of such a catastrophe demands constant watchfulness.

In countries less warm, with a water of condensation at 20°, the tension of the ammonia hardly exceeds eight atmospheres. In these circumstances the Carré machine works well and produces economical results. But in such countries natural ice is abundant, and the service rendered is consequently of less value.

The second class includes the Ether Machines. Sulphuric ether was first employed for the manufacture of ice in England. A large pneumatic pump draws up the vapours of ether, which are formed in a tubular refrigerator, and compresses these vapours in a condenser bathed in spring water. The evaporation may also freeze the water contained in the tanks, while the compression of the vapours and their condensation in the condenser transfer to the spring-water all the heat freed from the water in the tanks, transformed into ice. A pipe allows the condensed ether to return to the refrigerator, and be again subjected to volatilisation.

These machines, more simple than the ammonia ones, are, however, less workable. Ether is a liquid of small volatility, and gives only weak tensions. At – 4° or – 5° C., this tension varies from 4 to 8 centimetres of mercury according to the quality of the ether. The cylinder of the pneumatic machine must then be of considerable dimensions in order to draw up a small weight of ether and produce a limited amount of cold. The whole of the first half of the machine works with an almost complete vacuum, but if the joints, the walls of the tubes, and the caulking of the cylinder are not perfectly hermetic, atmospheric air will enter the apparatus ; and should the manometer show that it is present to the extent of one or two centimetres, the evaporisation of the ether will be arrested. With the smallest amount of air present the machine becomes unworkable ; a hole no larger than a pin-point is sufficient to paralyse the half of its normal product.

Another inconvenience arises from the fact that ether is not a body perfectly constant in its chemical characteristics. Under the action of frequent changes of condition, of frequent volatilisations and condensations, it becomes acidified and transformed into less volatile isomeric bodies. On this account it is necessary frequently to change the active liquid, which is very troublesome and expensive.

Lastly, the greases with which the piston of the cylinder is lubricated form a close mixture with the ether, a mixture which is carried into all the apparatus, and which also tends to trammel its regular working.

The third kind, Compressed Air Machine, is the least workable of all, and has invariably failed on trial. Its principle is as follows :—Air is compressed in a large cylinder to nearly three atmospheres. This compression raises the temperature of the air to about 150° C. A current of water cools this air, which enters cold into a second cylinder, where it expands from three atmospheres to the ordinary pressure. This work which it produces upon a second piston is equal to the deduction of the original work, for the two cylinders are joined *en suite*, and are worked by the same rod. The air which is expanded lowers the temperature, and gives the second cylinder about 60° of cold. This air may be utilised to manufacture ice, or to cool cellars, apartments, &c.

This machine, in order to work well, requires large cylinders and close-fitting pistons with very little friction, a perfectly regulated introduction into the expansion cylinder, and orifices, valves, and flaps without a flaw. But these conditions are almost impossible to realise in practice. A piston of large size, travelling in a cylinder under a temperature from – 50° to – 60° C., is an abundant source of friction, for it is only imperfectly lubricated by glycerine. A thick coating, produced by the solidification of the vapour of water in the expansion cylinder, is also a cause of accident and trouble in the working. Lastly, the smallest derangement in the aspirating or compressing valves of the first cylinder puts an entire stop to the production of cold. These machines, therefore, are absolutely unequal to the practical solution of the problem.

A machine capable of easily performing the work must comply with the following five essential conditions :—1. Too great pressure must not occur in any part of the apparatus. 2. The volatile liquid employed ought to be so volatile that there will be no danger of air entering ; *i.e.*, the pressure must be at least one atmosphere in order to be in equilibrium with the external pressure. 3. It is necessary to have a system of compression which does not require the constant introduction of grease or of foreign materials into the machine. 4. The liquid must be stable, it must not decompose by the frequent changes of condition, and it must not exert chemical action on the metals of which the apparatus is constructed. 5. Lastly, it is necessary, as far as possible, to remove all danger of explosion and of fire, and for this reason the liquid must not be combustible.

If we examine the series of liquids investigated by M. Regnault, only one will be found to satisfy these essential desiderata ; this liquid is sulphurous anhydride, SO_2.

In fact, this body is liquid under the atmospheric pressure at a temperature of – 10° C., and it does not give pressures higher than four atmospheres at a temperature of 35°. This liquid does not act on metals or fats ; it is not combustible, and is the least expensive of all known volatile liquids. By the process of manufacture discovered by M. R. Pictet, it costs less than sulphuric ether.

Thus, by taking advantage of the general principle of the evaporation of a volatile liquid to produce cold, and utilising sulphurous acid, we can obtain a machine which gives results constant in every country, and which acts in a perfectly mechanical and normal manner in all latitudes. The following is a brief description of a typical machine, manufacturing 250 kilogrammes of ice per hour:—

A cylindrical tubular copper boiler has a length of 2 metres and a diameter of 35 centimetres; 150 tubes of 15 millimetres traverse its entire length, and are soldered by their extremities to the two ends. This first boiler is the refrigerator. It is placed horizontally in a large sheet-iron vat, which contains 100 tanks of 20 litres each. An incongealable liquid, salted water, is constantly circulating in the interior of the refrigerator by means of a helix. This liquid is re-cooled to about $-7°$ in a normal course, and it licks on its return the sides of the tanks which contain the water to be frozen.

In the space reserved between the tubes of the refrigerator, the sulphurous acid liquid is volatilised, its vapours are drawn up by an aspirating force-pump, which compresses them without the condenser. This condenser is a tubular boiler, the same as the refrigerator; only a current of ordinary water passes constantly into the interior of the tubes to carry off the heat produced by the change of the gaseous into the liquid state of the sulphurous acid, and by the work of compression. A tube furnished with a gauge tap, adjusted by the hand once for all, permits the liquefied sulphurous acid to return into the refrigerator to be subjected anew to volatilisation.

Sulphurous acid has the exceptionally advantageous property of being an excellent lubricant, so that the metallic piston which works in the cylinder of the compressing pump requires no greasing. Thus the introduction of foreign matter into the apparatus becomes entirely impossible.

The work necessary to manufacture 250 kilogrammes of ice per hour is at the most seven-horse power.

A cold of 7° in the bath is amply sufficient to obtain in the tanks a rapid and in every way economical congelation.

With these mechanical arrangements the following important advantages are realised:—1. The pressure never exceeds four atmospheres. 2. There is never any entry of air to fear, the pressures, as far as $-10°$ C., being always above that of the atmosphere. 3. The volatile liquid employed is perfectly stable, undecomposable, and without chemical action on metals. 4. All greasing in the machine is dispensed with. 5. The volatile liquid is obtained at a very low price, and it is accompanied by no danger of explosion or fire. 6. The cost of production of the ice approaches infinitely near to the theoretic minimum: it is about 10 francs per ton of ice.

By means of all these advantages the practical problem of the manufacture of ice may be considered as solved for all climates, and the process of M. Pictet will not fail to be speedily adopted in all warm countries as soon as it becomes known; it is in such countries that its happy results will be specially utilised and appreciated.

A small specimen of M. Pictet's machine will be shown at the forthcoming Loan Exhibition of Scientific Apparatus at South Kensington.

APPARATUS FOR DEMONSTRATING THE TRANSFORMATION OF FORCE

IN a recent number of the *Journal de Physique*, M. Crova describes a convenient apparatus for showing the relations between heat, electricity, and mechanical force. The arrangement is as follows:—

Two of Clamond's thermo-electric generators are connected in surface, and put in communication with a Gramme machine in such a way as to set this in action. In the circuit is inserted a sort of electric lamp, in which a platinum wire placed in the centre of a small globe (which protects it from agitation of the air) can be raised to incandescence. The only difficulty of the experiment consists in so regulating the length and diameter of the platinum wire as that it may be raised to a red heat, while the thermo-electric current retains sufficient intensity to drive the Gramme machine. A circuit entirely metallic then is obtained, with which the following transformations can be effected:—

1. The Gramme machine being excluded from the circuit, a portion of heat, transformed into electricity by the thermo-electric pile, reappears in the state of heat in the platinum wire.

2. The platinum wire being excluded from the circuit, and the Gramme machine introduced, a portion of heat, transformed into electricity in the pile, produces mechanical work in the machine, which acts as a motor.

3. The platinum wire and the machine being included in the circuit, a part of heat, transformed in the pile into electricity, produces heat in the wire and work in the motor. If we then stop the motion of the Gramme machine, we find the incandescence of the platinum wire increased. The machine being liberated, on the other hand, is set agoing again, and the incandescence of the platinum wire diminishes in proportion as the motion is accelerated. In this way is rendered sensible to the eye the expenditure of heat necessary to develop an increasing quantity of mechanical work.

4. Taking the handle of the machine, we turn it *in the direction* of the rotation the current produces, but with an increasing velocity. In this way a velocity is reached such that the incandescence of the wire *completely disappears*.

5. If the handle be turned in a direction opposite to that of the rotation the current communicates, there is considerable resistance, and the incandescence of the wire *increases* rapidly; on turning more quickly, the wire is fused. Thus, in the metallic circuit under consideration, the circulation of a given quantity of energy may appear exteriorly in the form of heat or of mechanical work, the one of these quantities being the complement of the other. If by an exterior force we introduce into the circuit an additional quantity of work, the increase of the quantity of energy put in circulation is rendered visible by the incandescence of the wire; any communication outwards from the circuit, of a certain quantity of energy which circulates in it, appears, on the other hand, in diminution, or even disappearance, of the incandescence.

NOTES

LORD SALISBURY, on Monday, named the following as Commissioners under the Oxford University Bill:—Lord Selborne (Chairman), Lord Redesdale, the Dean of Chichester, Mr. Mountague Bernard, Sir Henry Maine, Mr. Matthew White Ridley, and Mr. Justice Grove. The feeling among scientific men is one of intense disappointment, leading to the conclusion that it is useless any longer to consider whether Oxford will ever be in a position to do anything for the promotion of science.

THE report of the Cambridge Board of Mathematical Studies to the Studies' Syndicate contrasts with the reports of most of the other boards in the paucity of its suggestions for improvement. They do not seem to think that very much is required in order to perfect the system of mathematical teaching. They believe in the probable stability and development of the system of inter-collegiate lectures, but say very little to assist its development, and they say nothing about the present vehement competition by means of private tuition, and the defective method of study that it induces. In answer to the question how University teaching may be organised so as to give the greatest encouragement to the advancement of knowledge, "the Board offer no suggestions under this head." Is this quite what might have been expected in a report bearing the signatures of Stokes, Cayley, Adams, Clerk-Maxwell, Sir W. Thomson, Tait, Lord

Rayleigh, and James Stuart? May there not be some unobvious explanation of this phenomenon? The whole report consists of only forty-one lines.

THE *Daily News* of Thursday last contains a letter from its *Challenger* correspondent, giving an account of the voyage from Valparaiso to Monte Video between December 10 and February 16. Most of the work of this cruise was done among the islands on the South American coast between the Gulf of Penas and the Straits of Magellan, and at the Falkland Islands. A considerable number of soundings were taken, and successful hauls made; the naturalists landed on several islands and made collections of specimens. This is the first part of the last section from Valparaiso to England, when the work of the ship will be completed.

MR. W. Spottiswoode, F.R.S., has been elected a corresponding member of the French Academy of Sciences, in the Geometrical Section.

ON the 21st inst. a fine bronze statue of the late Lord Rosse, erected in the principal street of Parsonstown, was unveiled. It is described as one of the latest and best of Foley's works.

GEOLOGICAL Time was the subject of Prof. McKenny Hughes' lecture at the Royal Institution on Friday evening. He told the audience that "A wise man before building a structure first examined the nature of the foundation," and he then proceeded to inquire into the nature of the evidence upon which the calculations have been made, as to the rate of denudation of valleys, the wearing back of sea coasts, the growth of peat-mosses, and the deposition of alluvium. He endeavoured to show by means of examples of exceptional phenomena, happening at long intervals and lasting for indefinitely short periods, that such calculations were utterly fallacious, and concluded by comparing our position to that of a "man in a cockle-shell boat, trying to sound an almost unfathomable ocean, firstly, with too short a line; secondly, with too heavy a weight; and lastly, with a weight so light that he was perfectly unaware when he touched the bottom." "To doubt," a great biologist tells us, "is the first principle of modern science," but in life, as in art, there are lights as well as shadows, and geological time is so intimately connected with the history of life upon our globe, and with the knowledge we possess of past denudations that have produced those missing leaves in the chapters of the earth's life history that geologist's call unconformities, that we venture to think that though we may not have reached the bottom of the deeper oceans, yet we have certainly sounded some of the most shallow seas. We therefore look forward with pleasure to hearing another lecture from Prof. Hughes, in which drawing on his abundant resources, he will leave the negative for the positive, the unknown for the known, and show us step by step the lights modern science has already thrown on the great cosmical, physical, and biological changes that are involved in the term "Geological Time."

AT the meeting of the Vienna Geological Society, on March 7, Mr. Hauer read a letter dated Manila, Jan. 11, 1875, from Dr. Drasche, who has been staying at the Philippine Islands since December 8 last, and intends to remain there for about six months longer for the purpose of investigating the active volcanoes and obtaining some knowledge of the geological composition of the island of Luzon. Four considerable excursions have been already made—1. To the plain of Pampanga, with the ascent of the Arayat and the Cordillera of Zambales. 2. The southern shore of the Laguna de Bey, ascent of the extinct volcano Maquilin, and a visit to the solfatara Tierra Blanca. 3. Ascent of the volcano Tael. 4. River Poray and Cueva de San Mateo. The wide fertile plain in which Manila lies is composed of clay strata recently raised from the sea, abounding with such species of fossils as are still living in the neighbouring sea. The plain is bordered by a range of hills, consisting of pumice-tuff. On the higher mountains behind these there are found trachytes and andesites, besides the tuffs. Of special interest among these is an amphibole-andesite, containing a great deal of olivine, that composes the Arayat. It is the most basic eruptive rock that Dr. Drasche has noted as yet on the isle of Luzon, and belongs probably to a more ancient volcanic period. On the Arayat, which had been hitherto considered as an extinct volcano, neither eruptive matters nor any other signs of distinct volcanic action are to be discovered. Near the River Poray there were found, besides greenstone-trachytes and conglomerates, large masses of a white limestone, partly crystalline and containing fossils, chiefly fragments of corals. When Dr. Drasche despatched his letter he was about to undertake a journey of two months to the wild northern regions of Luzon that have hardly ever been visited by geologist as yet.

DR. PARKES, whose death at the comparatively early age of fifty-six years we announced last week, was a man whose loss will be felt in many circles of society; he had connections with many scientific bodies, and, we believe, was universally known, beloved, and admired in the medical profession, for the scientific advancement of which he did so much. He served the State in various capacities throughout his life, but is specially known in connection with the Army Medical School, which owes a great part of its efficiency to his exertions and example. He had had a good training in scientific investigation, and his application of the principles of science to the conduct of research in his own department led to valuable results. Dr. Parkes was elected a Fellow of the Royal Society in 1861, and his contributions to its proceedings have been numerous and of high value. Among these we may mention his three papers on the Effects of Diet and Increase in the Elimination of Nitrogen during Muscular Action; in 1870, 1872, 1874, he also contributed papers on the Effects of Alcohol on the Human Body. Dr. Parkes, indeed, seems to have been a model of what a scientific physician should be, and to this he joined a character that attracted the love of all who knew him.

LETTERS received in Sydney from Signor D'Albertis, the Italian naturalist, we learn from the *Times*, who has been for some time resident on Yule Island, on the coast of New Guinea, give further accounts of the belt of coast land, twenty to twenty-five miles in width, of which he is able to speak, and so much of the land beyond this limit as was visible from the summit of a hill about 1,200 feet high. From this eminence he saw a large extent of plains, indented with lagoons, with the River Amama (the Hilda of the *Basilisk*) flowing downward from a northerly direction to its junction with the Nicura, which discharges its waters into the sea. Apparently, this stream is deep enough to be navigable far into the interior, but its channel is seriously obstructed by fallen timbers. He ascended the Nicura River for a distance of eighteen or twenty miles, and found it fringed with mangroves for the first ten miles, after which these gave place to splendid thickets of the Nipa palm, while the eucalyptus and the grass tree flourish at some distance from the stream. He crossed the Amama several times, and describes it as flowing through a large and fertile valley, apparently uninhabited, and well adapted for pastoral purposes. Nowhere did he find the natives possessing any knowledge of gold, silver, or any other metal. He confirms what has been said by Mr. Wallace and other travellers as regards the island being peopled by two races, the one mentally and physically superior to the other; the invaders having driven the indigenous tribes into the interior. The earlier inhabitants of New Guinea have darker skins than their conquerors, are shorter in stature, and their countenances are more prognathous than those of the coast tribes. The western side of New Guinea appears to be chiefly inhabited by the indigenous Papuans, and the eastern by a superior race.

THE mail steamer *Congo* arrived at Madeira on Saturday with Lieut. Cameron on board; his health is perfectly restored. The *Congo* left Madeira the same afternoon, and is expected to arrive in England during the present week. Sir H. Rawlinson announced to the Geographical Society on Monday that it is proposed that Lieut. Cameron should appear at the next meeting of the Society, and as it is expected that the audience will be unusually large, the meeting will be held in St. James's Hall on Tuesday week, April 11.

AT the last meeting of the Geographical Society Capt. Anderson, R.E., read a paper on "The North American Boundary from the Lake of Woods to the Rocky Mountains." Capt. Anderson was chief astronomer of the North American Boundary Commission.

THE most important paper in the last (February) part of the *Bulletin* of the French Geographical Society is the first part of Dr. Nachtigal's account of his travels in Central Africa in 1869-74. M. H. Duveyrier has a paper on Lieut. Cameron's trans-African journey, and the account of Abbé David's second exploring journey in the West of China, and M. J. Codine's paper on the Portuguese discoveries on the Western African coast in 1484-8 are continued.

WE have received the first number of *La Revue Géographique Internationale*, whose proposed fortnightly appearance we announced recently. It is a well-printed quarto of sixteen pages, and starts with a very promising programme. The most notable paper is on Ancient Geographical Monuments of the Tenth and Eleventh Centuries, by M. E. Cortambert, being a notice of the principal maps of that time which have reached us. Under the heading of "Courriers de l'Extérieur" letters from correspondents in various parts of the world are published.

INTELLIGENCE from Kasan announces that the German Exploring Expedition to Western Siberia has arrived there.

THE state of Mount Vesuvius was reported by the *Daily News*' correspondent on Sunday to have been unchanged. Prof. Palmieri wrote from the Observatory on Saturday: "Smoke is still abundant. There is a reflected glare at intervals from the fire within the crater. No lava has yet made its appearance." No immediate eruption is, however, expected.

THE annual meeting of the Iron and Steel Institute was commenced on Tuesday in London, and will be concluded on Friday. The Bessemer Medal for 1875 was presented to Mr. R. F. Mushet. To-day the Council will be entertained at dinner by the Lord Mayor, at the Mansion House.

M. FRIEDEL, an able mineralogist, has been appointed Professor to the Museum of Natural History of Paris, to fill the place vacated by the retirement of M. Delafosse. It is to M. Delafosse that is due the admirable arrangement of the Gallery of Mineralogy in the Museum.

THE Lords of the Committee of Council on Education have given directions for a course of instruction in Botany to be delivered at South Kensington, commencing about the middle of June, 1876. This course will be given by Prof. Thiselton Dyer, M.A., B.Sc., &c. It will consist of a daily lecture, with practical instruction in the Laboratory, and will extend over about eight weeks. A limited number of Science Teachers, or of persons intending to become Science Teachers, will be admitted to the course free of expense. They will also receive their travelling expenses to and from London, together with a maintenance allowance of 30*s*. per week while attending the course. The hours of attendance will be from 10 A.M. to 4 or 5 P.M.

A "VICTORIA and Albert Palace Association" has just been formed. It is intended, if the consent of the Government can be obtained, to build a palace on the banks of the Thames, near Battersea Park, for the "health, recreation, and instruction of the metropolis," combining "the amusements of the Crystal Palace, the pleasures of the Albert Hall, with the instruction and benefits furnished by the Kensington Museum." It is hoped that the palace will be opened on May 1, next year.

AN International Exhibition is to be held in Paris in 1878 or 1879 at latest.

THE *Bulletin de la Société des Sciences d'Alger* for 1875 contains interesting papers on the ethnology of the Barbary races, by M. J. A. N. Perrier; and the geography, ethnography, geology, zoology, and archæology of Algeria, by Prof. Jourdan. Meteorological tables are appended, giving the observations made from three to five times daily, the barometric observations being made unfortunately with an aneroid, by which their value is much lessened.

A COURSE of Twelve Lectures on Geology, free to the public, will be delivered in the large hall of the London Middle Class School, Cowper Street, Finsbury, on Tuesday and Friday evenings, at eight o'clock, commencing April 4, by Dr. W. B. Carpenter, C.B., F.R.S.

PROF. RUBENSON has published in the *Transactions* of the Royal Academy of Sciences at Stockholm, a discussion of the rainfall of Sweden, with five plates, from the observations made at twenty-nine stations from 1860 to 1872. From this discussion we learn that Göteborg is the wettest, and Kalmar the driest station; that in advancing from S.W. to N.E. the line of maximum precipitation passes from Göteburg to near Gefle, and that as regards seasonal distribution, the maximum is assimilated to that of continental Europe, occurring generally in July and August, and the minimum to that of the eastern part of Great Britain, south of the Grampians, occurring in March and April. Two valuable tables are added, one giving the monthly means at places at which long-continued observations have been made, and the other the annual averages for the twelve decennial periods, beginning with 1751.

IN the *Bulletin International* of the Observatory of Paris, P. Denza gives an interesting notice of a comparison of the barometers at fifty-five of the Italian stations, made by him during 1870-75. The comparison was made in each case with the normal barometer of the Observatory at Moncalieri, whose error had been ascertained by comparison with the standard of the Paris Observatory through that of the Observatory at Turin.

WE have received the meteorological observations made at the Naval Hydrographic Office at Pola for January last. This number is the first of a new series giving the hourly observations of the barometer, thermometer, and anemometer, including both direction and force of wind, together with the daily and hourly averages for the month. The position of Pola on the comparatively confined basin of the Adriatic gives a peculiar value to these hourly observations.

IN the *Quarterly Journal of the Meteorological Society* appear, among other matters, a paper on the rainfall at Calcutta for the twenty-eight years ending with 1874, by Mr. R. Strachan, in which the main facts are carefully summarised and tabulated in a useful form; a description of a self-regulating atmometer, by Mr. S. H. Miller; and a short paper by Mr. William Marriott, on the reduction of barometric observations, with a table for combining the corrections for index error, temperature, and altitude. The table will facilitate the work of reduction, and is sufficiently exact for most practical purposes for which such tables are required, and may be used in preparing observations and means for the press, provided the observations and means themselves be also printed uncorrected for height.

EXPERIMENTAL RESEARCHES ON THE EFFECTS OF ELECTRICAL INDUCTION, FOR THE PURPOSE OF RECTIFYING THE THEORY COMMONLY ADOPTED[1]

THE theory generally adopted in treatises on Physics and Electricity to explain the fundamental fact of electrical induction in an insulated cylinder A B, acted on by an electrified body C, is as follows:—It is admitted that on the extremity B of the induced cylinder, that is, the extremity next to the inductor G, is found only the electricity opposite to that of the inductor; while on the rest of the same cylinder is found only electricity similar to that of the inductor. But these two opposite electricities are both supposed to be endowed with tension, consequently they ought to be divided by a neutral line. When it is wished to represent in a graphic manner the electrical distribution indicated, obtained by induction upon the cylinder A B, this is done by means of Fig. 1. In this figure A B represents the induced and insulated cylinder, C the positive inductor, *a b* the neutral line, *a p b* the negative induced electricity, which may also be called induced electricity of the first kind, and *a q b* the positive electricity, or induced electricity of the second kind.

It is obvious that the explanation commonly adopted for electric induction cannot hold for the following three reasons:—1. Because, as the two opposite electricities possess tension, they ought on that account to neutralise each other as they are present on a conducting cylinder. 2. Because in putting into communication with the earth the extremity B of the induced cylinder, upon which extremity it is admitted that the homonym of the inductor is *not* found, nevertheless that only is dissipated, while the opposite electricity, which is found on the same extremity, remains there entirely, notwithstanding its communication with the earth.

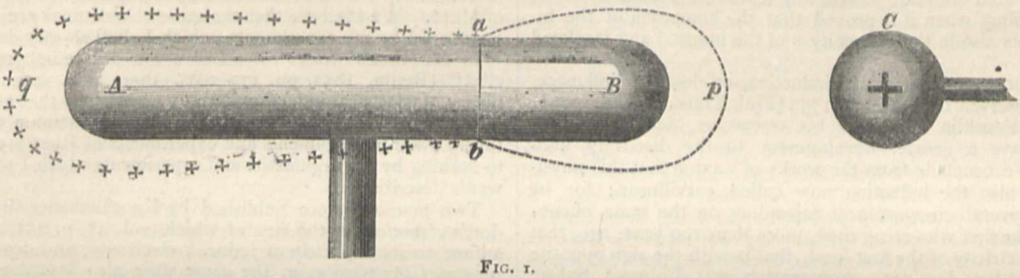


FIG. 1.

3. Because of the two kinds of electricity which coexist upon the induced insulated body, only the homonym of the inductor is dissipated by contact with the air. The experimental proofs upon which this old theory is based do not prove the facts indicated while the action of C is being exerted, contrary to what is stated in treatises on Physics. But in what follows we shall give other experimental and irrefragable proofs to demonstrate that the explanation indicated is not admissible.

Melloni[2] having discovered that the above-mentioned explanation was altogether erroneous, proposed another, which is as follows:—Upon the induced and insulated cylinder A B is found *everywhere* electricity of the same kind as that of the inductor, *i.e.*, induced electricity of the second kind; but to a much greater degree at the extremity A, furthest from the inductor and much less at the extremity B, nearest to the same inductor. As to induced electricity of the first kind, it does not possess any tension, *i.e.*, it is entirely latent (dissimulated) on the extremity B, nearest to the inductor, and proceeding from this extremity B towards the extremity A, furthest from the same inductor, it always goes on diminishing. It is for this reason that upon the induced insulated body there is a section in which the induced electricity of the first kind, entirely concealed, will be equal to that of the second kind, entirely free. Also these two opposite electricities may coexist upon the induced and insulated cylinder, without neutralising each other.

In order to represent graphically this electrical distribution, obtained by induction upon the cylinder A B, and regarded as true by Melloni, let us make use of Fig. 2. In this figure A B represents the induced and insulated cylinder, C represents the positive induction, *a b* the section of electricities equal to each other but of opposite kinds; *m, a, p, b, n*, the induced of the first kind, and *a, q, b, h* the induced electricity of the second kind, *i.e.* the homonym of the inductor.

The new theory of Melloni, the truth of which I have proved by means of the experiments afterwards described, does not complicate the explanation of the facts which depend upon it. On the contrary, it tends to present them all in a unique and invariable aspect, the only one which is really natural and conformable to observation.

In the light of this new theory we see clearly (1) why the two opposite electricities coexisting on the induced and isolated cylinder do not neutralise each other; (2) if the extremity B,

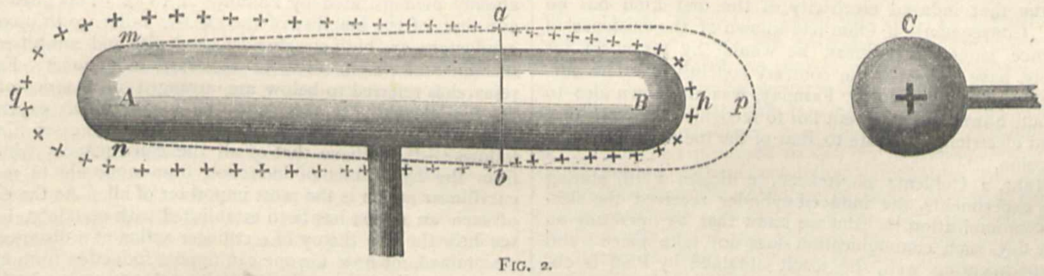


FIG. 2.

the nearest to the inductor G, is put into communication with the ground, the homonymous electricity of the inductor alone is lost, and not the opposite electricity; (3) why, of the two kinds of electricity which are found upon the insulated induced body, there is lost, by contact with the air, only the homonym of the inductor.

The cylinder A B, insulated and subjected to the influence of C, develops in a state of tension only the electricity homologous to that of the inducing body. The opposite electricity is completely latent, and becomes sensible only after the suppression of the inductive force.

When a proof-plane applied to the extremity B of the induced and insulated cylinder is subtracted from its influence, there is always seen the resultant of the two electricities which are found on the same extremity, both of them having become free, on the same proof-plane subtracted from the induction. This resultant may be either positive or negative, or even *nil*, relatively to the extremity B; but in each of these three results, we are bound to admit the pre-existence of two component electricities, opposite to each other, one completely latent, the other completely free, upon the same extremity.

If any element whatever of the section *a b* (Fig. 2) can be removed when insulated from the induction it will give a *nil* resultant.

Melloni, in the communication above referred to, confesses

[1] An Exposition of the Two Theories of Electric Induction. By M. Paul Volpicelli.

[2] "Comptes Rendus," t. 39, p. 177 (July 24, 1854).

that he is not able exactly to assign the true cause of certain facts relative to electric induction, in the phenomenon in question. We shall see in what follows, that nothing remains obscure in the theory developed by Melloni, when we reflect on the existence of what is called curvilinear induction, which was discovered by Faraday, and which is now acknowledged as a reality by all those who have kept themselves abreast of electrostatic science.

All that we have indicated in order to render clear the explanation given by Melloni of the electrostatic fact of which we have spoken, will be proved with a much greater amount of evidence by the experiments which follow. But, before describing them, it will be useful to give a brief historic sketch of the researches of different physicists as to the effect of electrical induction. The greater part of these researches are favourable to the theory of Melloni, while some are opposed to it. This theory will be acknowledged as true when it is proved that the induced electricity of the first kind does not possess any tension, or, what comes to the same thing, when it is proved that the homonym of the inductor exists also in the extremity B of the induced and insulated cylinder.

HISTORICAL.—Electrostatic induction, or electrical influence, was first observed by Canton in 1753 (Phil. Trans. vol. 48, part i., p. 350). Franklin continued his researches, but Wilke and Æpinus gave a greater development to the discovery indicated.[1] We conclude from the works of Canton that this physicist knew also the induction now called curvilinear; for he indicates several circumstances depending on the same phenomenon. The first who recognised, more than 100 years ago, that induced electricity of the first kind, that is with the sign opposite of the induction, does not possess tension, was Æpinus.[2] Subsequently Lichtenberg clearly announced that induced electricity of the first kind has no tension.[3] De Luc was also of the same opinion,[4] as also the celebrated Volta.[5] This Italian physicist[6] admitted the want of tension in the induced electricity opposite to that of the inductor, and admitted moreover that the electrical influence is exercised by means of a *partial* dissimulation of the inducing electricity, and the entire dissimulation of the induced electricity with opposite sign, a fact which is always verified. It seems that the question whether or not induced electricity of the first kind can have tension, was discussed for the first time by Lord Mahon and Volta, about 1787, to judge from what De Luc says.[7]

Among notable physicists who afterwards admitted that induced electricity of the first kind has no tension—before that doctrine was reproduced in a more developed form by Melloni, July 25, 1854—we must also reckon Fischer. This will be seen in reading Fischer's "Mechanical Physics," translated by Biot (4th ed., Paris, 1830, p. 238-242). The physicist Pfaff admitted completely the want of tension in induced electricity of the first kind.[8]

The celebrated Ohm, in a paper "On an unrecognised property of latent electricity," criticises Pfaff, and concludes that it is not true that induced electricity of the first kind has no tension.[9] Consequently if Ohm had known of the existence of the influence named curvilinear, he would, by means of his experiments, have arrived at the contrary conclusion. The curvilinear influence discovered by Faraday, was unknown also to Melloni, but, however, he did not fail to recognise the truth that the induced electricity opposite to that of the induction does not possess tension.

C. F. Mohr, a Coblentz pharmacist, criticises Pfaff, stating that in his experiments, the induced cylinder received the electricity by communication.[10] But we know that by operating on a very dry day, such communication does not take place; and yet by experimenting well, the result obtained by Pfaff is obtained, which triumphantly refutes Mohr.[11]

[1] Fischer's "History of the Arts and Sciences," Göttingen, 1804, vol. v., p. 726.

[2] "Tentamen theoriæ electricitatis et magnetismi," Petersburg, 1759 § 42, No 2

[3] See Erleben's work "Elements of Physics," sixth edition." Göttingen, 1794., p. 519.

[4] "Ideas on Meteorology," vol. i., second part, p. 334, § 360-1 (Paris, 1787).

[5] See his collected works, vol. i., part i. Florence, 1816, p. 258, line 4.

[6] Ibid, p. 200, line 6 from bottom. p. 260, line 14, and pp. 222-277.

[7] "Ideas of Meteorology," vol. i., part i., p. 292, § 324-5.

[8] Zehler's "Physikalisches Wörterbuch," vol. iii., p. 311 (Leipsic, 1827, p. 1).

[9] "Neues Jahrbuch der Chemie und Physik," by Schweigger Seidel, vol. v., p. 129 (1832).

[10] "Pogg. Ann. der Phys. u. Ch.", vol. xxxvi., pp. 224-8 (1835).

[11] "Op. cit. Val. 44, p. 332, and p. 334, line 1 (1838).

M. Riess gives a general *résumé* of the question in the "Repertorium der Physik" (vol. ii., p. 29; Berlin, 1838). He believes that by adopting the vertical position of the induced cylinder, instead of the horizontal position commonly adopted, we may be convinced that the pith balls, or even the gold leaves, diverge by the tension of the induced electricity—opposite to that of the inductor—which they possess. But this is not altogether true, since the *chief* cause of this divergence consists in curvilinear induction, which is not impeded in the vertical position of the inducted cylinder. Moreover, we cannot at all understand the choice of an induced cylinder placed vertically, to which, without any good reason, M. Riess has given the preference for the purpose of proving the phenomena of electrical induction, since these phenomena are always the same, and are equally well explained in an induced cylinder, whether it be vertical or horizontal. M. Riess, in his memoir "On the power of propagation of induced electricity,"[1] produces some observations against the memoir which Pfaff published in reply to that of Mohr. We shall see that the same observations are evidently overturned by my experiments, which I shall shortly describe.

In M. Riess's work "Die Lehre von der reibungse Electricitat" (Berlin, 1853, pp. 177-207), there is a very elaborate theory of electrical induction entirely opposed to that announced by Melloni, and agreeing with the old and commonly adopted theory; but the arguments and experiments of Riess are reduced to nothing by the arguments and experiments which I shall afterwards describe.

Two memoirs were published by Knochenhauer in Poggendorf's *Annalen*, in the first of which (vol. 47, p. 455, 1839) the author treats explicitly of induced electricity, and denies that it possesses any tension, at the same time also asserting that the electrical influence cannot traverse the conductors; all this agrees with our point of view. In the second memoir (Pogg. Ann., vol. 51, p. 125, 1840) he treats of the power of induction of the *Coibents*, an argument which has a close connection with electrical induction. We ought to observe here that Fischer, long before Pfaff and Knochenhauer, asserted that induced electricity had no tension. It is really extraordinary that neither Fechner nor Riess ever sought to examine the physics of Fischer in connection with the subject of electrical induction.

Knochenhauer, in a memoir in Pogg. Ann., 1843, vol. 58, p. 31), replies to Fechner, maintaining against him that induced electricity of the first kind must be regarded as entirely latent.

The physicist Petrina, in a memoir the object of which is to prove the erroneousness of the hypothesis that the electric influence can traverse a conductor,[2] shows himself favourable to the absence of tension for the induced electricity opposite to that of the inductor, and concludes that Fechner had by no means refuted the experiments of Knochenhauer which admit this absence of tension.

According to the inferences to be drawn from the memoir of Petrina above referred to, it appears that this physicist was one of the first to recognise, in 1844, curvilinear electrical induction, already demonstrated by Faraday in 1839. This phenomenon, and that of the inability of the electric influence to traverse the conductors, are both closely connected with and comprised in the fundamental phenomenon of electrical induction.[3] Faraday's researches referred to below are arranged in a series of thirty. In the eleventh of this series he speaks of his experimental researches on curvilinear induction,[4] and expresses himself as follows:—"I believe that of all the consequences which flow from the hypothesis of induction from molecule to molecule, curvilinear action is the most important of all. As the existence of such an action has been established with certainty, I do not see how the old theory of rectilinear action at a distance can be maintained, or how anyone can oppose induction from molecule to molecule." It is really astonishing that in no modern treatise on Physics or on Electricity do we find any mention of curvilinear induction, which may easily be tested by repeating the experiments of Faraday, as also the other experiments which I have published.[5] We must, however, except the treatise of De la Rive and that of M. Gavarret; in the latter there is a paragraph entitled, "Induction through dielectrics can be exerted in a curved line."

(*To be continued.*)

[1] "Pogg. Ann.," vol. 44, p. 624.

[2] "Pogg. Ann.," 1844, v. 61, p. 116.

[3] See Faraday's "Experimental Researches—Electricity," and "Catalogue of Scientific Papers," vol. ii. (Lond., 1868).

[4] See also "Pogg. Ann.," 1839, vol. 46, p. 537.—De la Rive, "Traité d'Electricité" (Paris, 1854), t. 1, p. 138–9.

[5] "Comptes Rendus," 1856, t. 43, p. 719.

SOCIETIES AND ACADEMIES

LONDON

Geological Society, March 8.—Pro. P. Martin Duncan, F.R.S., president, in the chair.—W. J. Chetwood Crawley, Walter Keeping, Joseph Thompson, and William Walker, were elected Fellows of the Society. The following communications were read:—1. On the influence of various substances in accelerating the precipitation of clay suspended in water, by Mr. Wm. Ramsay Principal Assistant in Glasgow University Laboratory. Communicated by Prof. Ramsay, F.R.S., V.P.G.S. The author referring to the fact that clay when suspended in water in excessively minute particles, settles more rapidly when the water contains salts in solution, noticed the opinions expressed by previous writers on the subject, and gave the results of experiments made by him, from which it would appear that the rapidity of precipitation is proportionate to the amount of heat absorbed by the salts in process of solution. By another series of experiments he found that the fluidity of the respective solutions had apparently no influence on the rapidity of deposition of the clay. He also found that clay is deposited less quickly in acid solutions than in solutions of salts, and more rapidly in a solution of caustic soda than in one of caustic potash. In solutions of common salt of different strengths he found that clay settled in the inverse order of their specific gravities. From all these results the author is inclined to attribute the varying rapidity of the settling of clay suspended in saline solutions to the varying absorption of heat by the solutions. When water containing suspended clay was heated, the rapidity of the settling of the clay was proportionate to the heat of the water. The author suggests that the increased rapidity of settlement may be due to the greater amplitude of vibration of the molecules of water when heated; the vibrations being performed in equal times, particles descending at right angles to the plane of vibration will experience less resistance from the molecules of water. A note by Prof. Ramsay, briefly indicating some of the geological bearings of these results, was appended to the paper.—2. On some Fossiliferous Cambrian Shales near Carnarvon. By Mr. J. E. Marr. Communicated by Prof. T. McKenny Hughes, F.G.S. With an Appendix, by Mr. Henry Hicks. Tl e shales described by the author extend from about three miles S.W. of Carnarvon to Bangor, running nearly parallel to the Menai Straits. They are faulted against Lower Cambrian to the east, and disappear against a dyke on the west. The shales vary from greyish black to bluish black in colour, and are generally sandy and micaceous, but in places chiefly clayey. Fossils were obtained from three places on the banks of the Seiont, namely, near Point Seiont (where the beds are concretionary in structure), along the old tramway from Carnarvon to Wantle, and near Peblig Bridge. The first-named locality is richest in fossils; and here there is a greenstone dyke, parallel to the bedding of the rock, and altering the shales for a distance of about four yards from the edge of the dyke. The fossils seem to indicate that the deposit belongs to the upper part of the Arenig group. Mr. Hicks pointed out that the fauna clearly showed that these beds belong to the Arieng group, many of the species being identical with those found in the upper part of that group at St. David's Shelve, and in Cumberland. The new species found by Mr. Marr are a *Caryocaris* (*C. Marrii*) and an *Æglina* (*A. Hughesii*). The other fossils were *Didymograptus indentus*, *D. bifidus*, *D. Murchisoni*, and the var. *furcillatus*. Species of *Barrandea*, *Trinucleus*, *Lingula*, *Obolella*, *Discina*, &c., and *Orthoceras caeresiense*. The rock in its general character is extremely like that at the same horizon in the succession at St. David's Shelve, and in Cumberland, and indicates, therefore, the prevalence of similar physical conditions when deposited. The rock is such as would be formed over an even sea-bottom at some considerable distance from land and in moderate deep water. Mr. Hicks looked upon this discovery as of considerable importance, since it clearly proved the position of beds hitherto imperfectly known, and moreover shows that similar conditions prevailed over extensive areas at the time these beds were deposited. It also furnished further evidence in support of Mr. Hicks's opinion that no break occurs anywhere in the Welsh area between the Cambrian and Lower Silurian rocks. —3. On the occurrence of the Rhætic Beds near Leicester. By Mr. W. J. Harrison, Curator of the Town Museum, Leicester. The sections described by the author are shown in brick-pits in the Spinney Hills, forming the eastern boundary of the town of Leicester, and in the Crown Hill on the eastern side of a valley excavated by the Willow Brook. In the latter locality they are capped by Lower Lias. They have a slight dip to the south-east. The brick-pits show a thickness of about 30 feet of Rhætic beds above the Triassic red marl, to which their stratification is parallel. The lowest bed is a light-coloured sandy marl about 17 feet thick, traversed by three or four courses of harder, whiter stone, and containing crystals of selenite, pseudomorphs of salt, and numerous small fish-scales. A single insect wing was obtained from it. This bed extends across the valley of the Willow Brook, and forms the base of Crown Hill. Above it comes the Bone-bed, from 2 to 3 inches thick, containing numerous small teeth, bones, and scales of fishes and Saurians, including large vertebræ of *Ichthyosaurus*, ribs probably of *Plesiosaurus*, and some bones of Labyrinthodont character. Two species of *Axinus* also occur. The Bone-bed is followed by about 2½ feet of coarse black shales, overlaid by a very thin band of hard reddish sandstone, with casts of *Axinus*, and this by about 2 feet of finely laminated black shales containing *Cardium rhæticum*, *Avicula contorta*, and a Starfish (*Ophiolepis Damesii*). Above these come about 5 feet of shales with sandy partings, the lower foot rather dark and containing *Avicula contorta*, *Cardium rhæticum*, *Ostrea liassica*, and a new *Pholidophorus*; the remainder light-coloured, but with the same shells. The topmost bed in the section is a band of nodular limestone 6 inches thick. The same sequence is observed in Crown Hill. There are indications of the existence of a second nodular limestone and of beds of light-coloured clay and sand, but obscured by drift, in which, however, blocks of limestone occur with *Monotis decussata* and *Anoplophora musculoides*. The author indicates other localities where traces of the Rhætic beds are to be seen, and states that wherever the true junction of the Trias and Lias is exposed, the Rhætics appear to be invariably present. The paper also included some particulars with regard to borings in the Trias near Leicester.—4. Hæmatite in the Silurians. By Mr. J. D. Kendall. The author referred to a former paper in which he showed that direction of the hæmatite deposits in the Carboniferous Limestone of Cumberland and Lancashire is parallel to that of the meridional divisional planes, or nearly north and south; while the deposits in the Silurians are in two directions, some parallel to one set of divisional planes and some to the other. In the present paper he describes a deposit of hæmatite at Water Blean, in the parish of Millom in Cumberland, in Coniston Limestone, which appears to be altogether unlike those referred to in his former paper. The Silurians here are all conformable, with a strike about 65° N.E. and S.W. and a dip of about 80° to N.W., but their order is inverted. The hæmatite occurs in the Coniston Limestone in the form of short veins, varying in width from a few inches to 9 feet, running in the direction of the strike, and having the same dip as the limestone, their deposition having taken place along the bed-joints of the rock. The author accounts for this difference in the deposits by the fact that in the Coniston Limestone at Water Blean the bed-joints are much more persistent than the divisional planes, which are very irregular and not at all so strong and open as the bed-joints.

Zoological Society, March 21.—Dr. E. Hamilton, vice-president, in the chair.—Mr. Sclater exhibited and made remarks on a series of skins of the parrots of the Fiji Islands, obtained by Mr. E. L. Layard belonging to the collection of Lord Walden, and called special attention to a new species of the genus *Pyrrhulopsis* of Reichenbach from the Island of Taviuni, which Mr. Layard had proposed to call *P. taviunensis*.—Mr. A. G. Butler read a paper containing descriptions of some new Lepidoptera from the collection of Lieut. Howland Roberts.—A communication was read from Mr. Andrew Anderson, containing corrections of and additions to a former paper of his on the Raptorial Birds of North-Western India.—Mr. Howard Saunders read a paper on the *Stercorariinæ* or Skua Gulls, in which he revised and corrected the synonymy of several species, and traced their respective ranges so far as they were known. He considered that *Stercorarius chilensis* (Bp.), although more nearly allied to the Northern form *S. catarrhactes* than to *S. antarcticus*, was perfectly distinguishable from either by its constant rufous coloration of the underparts and axillaries; its range as at present known being restricted to the West coast of South America. The range of *Sterc. pomatorhinus* was shown to extend from S. lat. 82° N. to about 30° S., and that of Richardson's Skua, to which he restored the original, but lately disused name of *Stercorarius crepidatus* reached from 82° N. to more than 40° S., on the coast of New Zealand: *S. spinicauda* (Hardy), from the African coast, being regarded as merely this bird in winter dress.

Anthropological Institute, March 14.—Col. A. Lane Fox, president, in the chair.—Mr. Stanbridge, of Daylesford, Victoria, exhibited and presented a collection of stone implements from Australia. It consisted of some axe heads, a mounted stone spear head, some wallongs, or grinding stones, and a Yowiwi, or large flat stone, on which the Nardoo seed is ground. A large stone implement, supposed to be for digging, was also lent. The president considered this last was an unfinished tool which would have been reduced in size if finished; but it had been used apparently in its present state, one of the ends being much rubbed.—Capt. Melford Campbell, President of Nevis, exhibited some stone implements. One of these, a knife or dagger, from Honduras, is 10½ inches long, and made of a thick flake of buff coloured chert of a fine amber hue; similar but smaller specimens from the same place are already in the Christy collection. Three polished Celts, from Turk's and Cairo Islands were shown by Capt. Campbell.—Mr. H. H. Howorth read a paper on the Samatæ, which was followed by a discussion.—Mr. H. Dillon, the Director, read a translation by Capt. R. F. Burton, of two letters from H. B. M.'s Vice-Consul at Lissa, H. Topich, on some human remains recently found in the Island of Pelagosa.

Meteorological Society, March 15.—Mr. H. S. Eaton, M.A., president, in the chair.—R. Trout Hawley Bartley, M.D., John Wuford Budd, Lieutenant-Colonel George E. Bulger, W. Brown Clegram, M. Inst. C.E., J. Sanford Dyason, John Eunson, Assoc. Inst. C.E., Thomas W. Grindle, Assoc. Inst. C.E., Major F. Bonnycastle Gritton, Junius Hardwicke, F.R.C.S., Alfred O. Walker, and the Rev. E. William Watts, M.A., were duly elected Fellows of the Society.—The following papers were then read:—On the Rhé-electromètre of Marianoni, by Robert James Mann, F.R.A.S.—On the variation of errors in hydrometers, by R. Strachan.—On the deduction of mean results from meteorological observations, by L. F. Käntz (translated from the *Repertorium für Meteorologie*, by J. S. Harding).—Summary of observations made at Stanley, Falkland Islands, during 1875, by F. E. Cobb.—Contributions to the Meteorology of West Australia, by R. H. Scott, F.R.S.

Victoria (Philosophical) Institute, March 20.—A paper was read upon the flint implements found in Brixham Cavern, in which the author, Mr. Whitley, alluded to the statements of Mr. Pengelly, whose active superintendence of the exploration of the cavern under the auspices of the Royal and Geological Societies was deserving of the warmest thanks of all geologists. Mr. Whitley complained that the Report of the Royal Society and the specimens had been allowed to lie by for fifteen years before being published and rendered accessible to the public. The consequence was, that for a long time theories having no foundation in fact had been promulgated as to these specimens, and several statements in regard to Brixham Cavern and its contents had been made in well-known geological works, which did not accord either with the recent Report of the Royal Society or Mr. Pengelly's subsequent one.

Institution of Civil Engineers, March 21.—Mr. Geo. Robert Stephenson, president, in the chair.—The paper read was descriptive of the hydraulic canal lift at Anderton, on the River Weaver, by Mr. Sidengham Duer, B.Sc., Assoc. Inst. C.E.

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Academy of Sciences, March 20.—Vice-Admiral Paris in the chair.—The following papers were read:—On the first method of Jacobi for integration of equations with partial derivatives of the first order, by M. Bertrand.—On the inferior limit which should be set to admission of steam into a steam-engine, by M. Resal.—On the storms called *foehn* in Switzerland, by M. Faye. These occur in certain parts, when a cyclone from the south-west meets the Alps; instead of showers and fall of temperature, the wind blows hotly and dryly; there is also a marked barometric depression. Around the special region the tempest produces its ordinary effects. The facts are explained by the gyratory descent of a mass of air, deprived by the mountains of the cirrhus which whirlwinds formed in the upper regions usually carry downwards. Now it is objected against M. Faye's theory of storms that a descending current should give a barometric maximum; but here, with manifest descent, there is a minimum. The barometric depression (both in the *foehn* and the ordinary storm) is a simple consequence of the gyratory movement, not the index of a strong suction excited from above on the inferior layers.—Note on an apparatus for determining the intensity and the law of development of pressures in the bore of guns with reference to the time, by M. Morin. The gaseous pressure is caused to press out a metallic jet of tin, the length of which increases with the pressure with sufficient regularity (each millimetre corresponds to about 237 kilogrammes of pressure per square centimetre). A piston is in contact with the tin cylinder, and a pencil at the end of its rod gives a tracing on a chronometric apparatus.—Determinations of nitrates and of ammonia in the water of the Seine, made on 18th March, 1876, above the bridge of Austerlitz, by M. Boussingault.—On the volume of the Seine, and the flood of 17th March, 1876, by M. Belgrand. This flood is the third highest in this century (the highest was in 1802). The Seine, in its greatest flood, gives fifty-two times more water than at low water.—On the spectrum of calcium, by Mr. Lockyer.—Actinometric measurements on the summit of Mont Blanc, by M. Violle (he will shortly give his results).—On the next hatching of winter eggs of Phylloxera; note by M. Balbiani.—Physiological action of *Amanita muscaria*, general phenomena of the poisoning; effects on organs of circulation and respiration, and disorders of calorification, by M. Alison, *inter alia*, the lowering of temperature by this substance, and the restoration to normal temperature with atropine are important.—On the means employed for education and instruction of deaf mutes, by the method of articulation, by M. Magnat.—Impossibility of the equation $x^7 + y^7 + z^7 = 0$, by M. Pepin.—On the behaviour of chronometers, by M. Rouyaux.—Geometrical solution of the problem of determining the most probable place of a ship by means of any number of straight lines of altitude greater than 2, by M. Bertot.—Influence of temperature on magnetisation, by M. Gaugain. The new facts given are briefly these:—When a steel bar in contact with a magnetic pole is gradually heated to a blue tint, the magnetism first increases, reaches a maximum, then decreases. The bar being allowed to cool while in contact, the total magnetism increases all the time, so that, when the bar has cooled to the surrounding temperature, it has much greater magnetism than before heating. The total magnetism of the bar, brought back to ordinary temperature, is greater the more the bar has been heated (at least under the temperature giving a blue tint). After breaking contact of the cooled bar for a few seconds it loses a part, but not all, of the increase of magnetism that resulted from heating.—On a rock intercalated in the gneiss of the Mantiqueira, Brazil, by M. Gorceix.—Reply to two *critiques* by M. Faye, by M. Hildebrandsson. He calls attention to three facts. It is rare that the form of isobars is circular. Synoptic charts show that the air moves spirally towards the centre of a minimum. The anterior and the posterior parts of a squall are quite different, so that after passing the centre you have a sudden change in the weather; and this is explained if the motion of the air have an upward component, for then other air unceasingly flows in from different regions. The facts cited are in opposition to M. Faye's theory.

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