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THE TERTIARY HISTORY OF THE GRAND CAÑON DISTRICT

A Monograph by Capt. C. E. Dutton. Being Vol. II. of the Monographs of the United States Geological Survey. With Atlas. 4to. [Vol. I. is not yet published.] (Washington, 1882.)

IN a handsome quarto volume, with a large atlas of maps and coloured views, the recently-constituted United States Geological Survey begins its series of memoirs descriptive of the geological structure and history of the country. Most appropriately the subject selected for illustration is at once the grandest and most unique feature in the geology of the United States, and to which indeed there is no parallel elsewhere in the world. Ever since the early Report by Ives and Newberry, in which the marvels of the Rio Colorado of the West were first made known, there has been a strong desire among geologists to learn more of that region, to have accurate measurements and careful drawings, and to be told authoritatively the details and the history of what they could not but admit to be the most stupendous example of river-erosion on the face of the globe.

Major Powell's bold descent of the river and the charming volume in which he described it threw much fresh light on the wonders of the cañons. But he had no opportunity of properly exploring the surrounding regions, though we looked forward to his return to the scene of his exploits and the consequent elaboration of another memoir discussing the whole problem of the origin and history of the geological features of that remarkable area. Pressure of other duties has prevented him from realising this hope. But, though unable himself to resume this task, he deserves our best thanks for having induced the late Director of the Survey, Mr. Clarence King, to intrust the detailed survey of the Grand Cañon to Capt. C. E. Dutton, who had already done excellent service among the high volcanic plateaux farther north. Capt. Dutton unites some of the highest qualities of a geological explorer. He is an excellent stratigrapher, a good petrographer, an enthusiast in the study of rock-sculpture, writes clearly and pleasantly, has a physical frame capable of carrying him triumphantly through any amount of physical fatigue, and is the happy possessor of a bright, cheerful nature, that must lighten the hardships of camp-life in the remote West both for himself and for his companions. We can well imagine how such a man, wandering among the lofty plateaux of Utah that had been assigned to him for exploration, should have cast many a longing gaze southward to that strange wild desert region of rocky platforms and winding *mesas*, through which the gorges of the Colorado and its tributaries have been sunk; how he should have been unable to resist the temptation to stray into that wonderland; and how he must in some measure have almost welcomed the blasts of early winter that drove him down from the survey of the plateaux, and allowed him to ourney through the cañon country on his way back to the Mormon settlements and the nearest railroad.

When at last the task of actually exploring and describing that region was intrusted to him, he already possessed a general acquaintance with its character and with many of its details. A stranger who first finds the cañon scenery before him is so excited by its novelty and grandeur, that for a time he feels utterly bewildered. Only after his eye has in some measure recovered its power of grasping the broad effects, without being lost in the details, does he begin to realise what are the elements of this stupendous grandeur. But Capt. Dutton had gone through this preliminary training. He had been led to scrutinise the scenery in detail, to discover the relations of part to part, and to speculate upon the evolution of the whole. Yet no one can read his pages without feeling that this analytic process has in no way dulled his sense of the beauty and majesty of the scenery. His words glow with the light that floods those flaming precipices. The blue aërial perspective of chasm and cliff receding into the dim distance in the central gorge seems to rise before our eyes as we read. With no irreverent hand does he tear the mask off the face of Nature. Rather does he make us feel how deeply the mystery of the scene has entered into his soul, as he gently lifts the veil that we may see a little way within, even as far as he has himself been enabled to penetrate.

And this is the true spirit in which such scenery should be described and discussed. The man who could sit down and dissect these cañons in cold blood, and with as little emotion as he would show in cutting up a joint of beef, would be a creature not to be envied. Nowhere in this world does the scenery appeal so powerfully to the imagination. Among the Alps the rocks have been so stupendously crumpled that we may be pardonably at a loss to tell how far the outlines of a mountain are due to subterranean movements or to subsequent erosion. But among the western cañons there is no room for any such doubt. The rocks lie for thousands of square miles as flat as when they were laid down upon the floors of ancient seas and lakes, and their horizontal undisturbed beds may be followed by the eye, winding in and out from cliff to cliff, preserving the same breadth, colour, features, and serving as so many datum-lines from which to measure the amount of solid rock that has been removed from the gorges. In tracing back the origin of these landscapes, and seeking out the causes of their infinite variety of detail yet marvellous harmony of effect, the mind naturally compares them with the feeble illustrations of erosion with which alone we are usually personally familiar. Such a comparison, however, will almost suggest a doubt as to whether we ever before could have had any proper conception of what the power of running water actually is, so utterly beyond description is the impressiveness with which this power is now realised. Nor is one disposed to deny that nowhere else is the dominant influence of geological structure upon the ultimate contours developed by erosion so significantly displayed. On every part of the scenery the story of its origin is impressed in characters that cannot be mistaken. Yet these characters are on so colossal a scale that the dry prosaic language of ordinary geological description seems utterly incongruous when applied to them. It must be a difficult task to preserve the sober decorum of scientific treatment, and to convey at the same time an adequate impression of the infinite majesty of the subject.

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Capt. Dutton may be congratulated on having accomplished this task with as large a measure of success as probably was achievable. Without entering into stratigraphical details he addresses himself to the problem of the origin and history of the erosion that has converted the level rock-platforms of the Colorado River into their present profoundly trenched condition. Sketching briefly but clearly the general geographical features of the region and their relation to the underlying geological structure, he presents the reader with a series of pictures of the various types of scenery. He shows how everywhere the evidence arises of vast denudation. Not only have the wide valleys and deep gorges been excavated, but an enormous amount of material has been worn away from the broad rocky terraces. From the high plateaux of Utah the Mesozoic and Tertiary formations descend by a succession of broad terraces like a giant staircase to the platform of Palæozoic rocks. Capt. Dutton gives reasons for his belief that the strata which end at the cliffs of these successive terraces once extended over the whole of the Grand Cañon district, and he estimates the amount of rock thus removed to have averaged probably 10,000 feet in thickness over an area 13,000 to 15,000 square miles in extent. He bases this estimate partly upon the obvious continuity of the strata, and the improbability that they could have ended off upon the Carboniferous platform; partly upon the evidence of displacements whereby Palæozoic rocks, formerly buried at least 10,000 feet below the sea-level, under an accumulation of sediment of that depth, have been again uplifted into the lofty plateaux of the Colorado; partly upon an argument from the history of the drainage-lines of the district. In this last argument, developing the views so forcibly expressed by Jukes many years ago for the rivers of the south of Ireland, and more recently applied by Powell to the stupendous illustrations in the Colorado basin, he shows that the present courses of the rivers are so entirely independent of structural features, that their position is inexplicable save on the interpretation that when the streams began to flow these features had not revealed themselves. He thus smoothes over the faulted Carboniferous platform, piles over it a covering somewhere about two miles thick of Mesozoic and Tertiary strata, and makes the rivers begin their first erosion on the surface of this covering. The faulting, plication, and uplifting have taken place subsequently; but meanwhile the rivers have kept their courses, incessantly sawing their way downward into lower layers of rock, and across the dislocations and folds that subterranean disturbance might throw across their path. No thoughtful student of this subject can refuse his assent to the solution of the problem so well worked out.

In tracing the geological history of the cañon region, we find at the bottom of all the visible strata, a foundation of ancient crystalline Archæan rocks, and also crumpled and broken masses of stratified formations, referred with more or less confidence to the Silurian and Devonian periods. The disturbance and extensive denudation of the older Palæozoic masses had been effected before the lowest of the vast conformable series of formations in this region began to be deposited, for the latter lie upon the upturned edges of the former, as on a platform—an impressive feature in the scenery. Continuous sedimentation began some time in the Carboniferous period, and appears to have been carried on with no sensible break up to the close of the Eocene period, until a total depth of at least 15,000 feet of sediment had accumulated. The Carboniferous portion is estimated at a thickness of 4500 feet, the various Mesozoic formations at 9000 or 10,000, and the portion of Eocene lacustrine beds deposited were 1000 or 1200 feet.

Capt. Dutton calls attention to the remarkable uniformity and persistency of the lithological characters of each formation, while at the same time there is great diversity in those respects between the strata of different platforms. By far the larger proportion of the whole mass of conformable strata consists of sandstone, presenting on successive horizons the most extreme contrasts of structure and colour, for they consist along certain platforms of adamantine quartzite, in others of massive cross-bedded sand-rocks, while they graduate also into shales and these into marls. It is this alteration of strata, showing very different degrees of permanence, yet each retaining its normal characters over vast areas, that affords the key to much that is most characteristic in the scenery of the region. The limestones are almost wholly confined to the Carboniferous system, where they occur both in the lower and upper divisions.

Another significant feature brought out by the survey is the evidence that sedimentation went on nearly at sea-level during the whole of Mesozoic time throughout the Cañon province. As the Mesozoic strata are 9000 or 10,000 feet thick, it is obvious that the sediments which were at or near the sea-level at the beginning had sunk to that distance below it by the end of the period. We have here, therefore, a consecutive series of shallow-water deposits not much less than two miles in vertical thickness. The Cretaceous rocks which form the uppermost division of this series are from base to summit banded with seams of lignite or coal, and layers containing marine mollusca. They vary in different parts of the province from 3500 to 8000 feet in thickness. At the close of their deposition, those movements appear to have begun which have culminated in the elevation of the sea-floor into the elevated plateaux that now form so prominent a feature on either side of the watershed of the continent.

With the advent of Eocene time the shallow sea-floor, in which sedimentation had been so continuous during the whole of the Mesozoic ages, began to be converted into wide fresh-water lakes. The Tertiary history of Western America is in large measure a record of the formation, duration, and effacement of these lakes, as the land gradually increased in elevation. In the plateau country the Eocene lacustrine deposits range from 1000 to 5000 feet in thickness. Great as this accumulation is, it unquestionably took place in comparatively shallow water over an area that was generally rising, yet was locally sinking, so that the lake persisted, and remained shallow; for its depth was reduced by the deposit of sediment as fast as it was increased by subsidence. The waters appear to have dried up from south to north, and finally disappeared somewhere in the area of the Uinta mountains.

It was on the floor of this desiccated lake that the drainage system of the Colorado river began, somewhere about the close of the Eocene period. During the vast

succession of ages that have since elapsed, erosion has been continuously in progress, and the result is the scenery of the Cañon region. Capt. Dutton gives what appear to be good reasons for believing that the larger rivers flow along the same channels which they took at the beginning, but that the minor tributaries, where any exist (and they are conspicuously absent in some wide districts), are comparatively recent in origin, and have been determined by modern surface conditions. The excavation of the Grand Cañon of the Colorado has thus been going on ever since the Eocene period. During that enormous interval the climate of the region appears to have passed through successive oscillations. There is no more skilful feature of the volume before us than the way in which the scattered facts that bear on this question are marshalled to their places and made to tell their story. Ancient river-beds, which for ages have been dry and are partly filled up with debris, open on the edge of the great chasm. They doubtless discharged their waters into the main river at a time when rains were abundant and watercourses numerous. But their fountains have long since been dried up, and their fading channels are almost gone. But all the while the Colorado and its larger feeders, drawing their supplies from far well-watered uplands, have continued their task of erosion until they have sunk their channels in some places more than a mile below the level of the plateau across which they flow.

The process of the excavation of the Grand Cañon is treated at length, and much new information is given as to its varying conditions. The details of the erosion are described with great clearness. The two final chapters, wherein these subjects are discussed, contain much that is suggestive, and deserve careful perusal by all who take interest in questions of denudation. They are condensed pieces of reasoning which cannot be intelligibly summarised here, and which indeed one is hardly prepared to find in an official report. Like his colleague, Mr. G. K. Gilbert, Capt. Dutton properly lays great stress upon the influence of an arid climate as one of the chief factors in cañon excavation. He points out how the absence of vegetation exposes the surface of bare rock to the action of rain. But it may be doubted if the scanty rains of the region can do more than remove material already disintegrated. We have to account for the continuous lowering of the level of the plateaux, and the removal of so vast a depth of stratified rock from their surface. Capt. Dutton himself admits that most of the rain which falls upon the country is absorbed by the rocks, and gushes out in copious springs at the base of the cañon-walls, thereby notably increasing the volume of the river. But there is everywhere a perceptible disintegration of the rock at the surface. This decay cannot be attributed to frost, which in so dry a climate can have but small effect. It seems to be due in large measure to the superficial strain induced by a great daily range of temperature. And it is no doubt aided by the action of wind, which removes the loosened particles, and exposes a new surface to the same kind of disintegration.

In conclusion, reference must be made to the truly magnificent series of illustrations by which this monograph is accompanied. The maps of the atlas give the reader a clear mental picture of the general topography and geological structure of the region. But it is by the pictorial illustrations that he will be chiefly fascinated. These are scattered profusely through the text, and form an important feature in the atlas. Mr. Holmes, whose reputation for the accurate and artistic rendering of geological details is so well established, has here far surpassed all his previous efforts, and has produced the most impressive and instructive geological pictures that have ever been made. His large coloured views of the Grand Cañon are in themselves a series of lessons in geology far more interesting and effective than can be supplied in words. The United States may be heartily congratulated on this first of the monographs of their Geological Survey. Let us hope that Congress will continue in the same liberal spirit the annual appropriations that have enabled the Director of the Survey and his associates to produce such splendid results. ARCH. GEIKIE

CENTRAL ASIA

Travels and Adventures East of the Caspian during the Years 1879-81, *including Five Months' Residence among the Tekkés of Merv.* By Edmond O'Donovan. Two Vols. (London: Smith Elder, 1882.)

Wanderings in Baluchistan. By Major-General Sir C. M. MacGregor, K.C.B. (London: Allen and Co., 1882.)

MR. O'DONOVAN'S venturesome excursion to the Merv Oasis stands out conspicuously as perhaps the most romantic episode in the recent annals of Central Asiatic travel. Yet in proceeding eastwards his original goal was not the Mervli Turkomans, but their western kindred, the Akhal Tekkés of the Daman-i-Koh. Sent out as the Special Correspondent of the *Daily News* with the Russian expedition against those nomads in 1879, he was at first well received, and spent some profitable time during the progress of military operations on the Caspian seaboard. But after the death of General Lazareff, having been suddenly banished from Chikislar, his ramblings lay henceforth mainly within the North Persian frontier. Here he again went over the ground, with which we have been made tolerably familiar by V. Baker, Macgregor, Stewart, and other recent explorers. Nevertheless even of this region Mr. O'Donovan has much to tell us, which is both new and interesting. There is a freshness and a fulness of detail in his account of Meshed, Tehrán, Kuchan, Resht, Shahrúd, as well as of the people and scenery of Khorasán and Mazandarán, which lend a peculiar charm to the first of these brilliantly written volumes.

But the chief interest of the work naturally centres in the section devoted to the Merv Oasis and its Tekke Turkoman inhabitants, with whom the traveller passed a forced residence for over five months during the year 1881. How he eluded his Persian escort, crossed the border above Sarakhs, traversed the Tejend river valley, plunged boldly into the heart of the desert, safely reached the Murghab Oasis, allayed the suspicions of the Tekkés, who took him for a Russian spy, gradually gained their confidence, became in fact a "Tekke of the Tekkés" and head of a Turkoman triumvirate, finally, by a rare combination of tact, patience, and courage, again escaping from his too importunate friends, all reads far more like a wild piece of fiction than so much sober history.

Although Colonel Stewart had recently brought home some accurate information regarding the present state of the Oasis, this region had been actually visited by no European traveller since Abbot's expedition in 1840. Hence Mr. O'Donovan is here on comparatively new ground, and his graphic account of the place and its inhabitants will be read with deep interest, especially by those who have not seen the portions already published in the *Daily News*. Since the Russian occupation of the Akhal country, the Oasis has doubtless lost whatever strategical significance it may have hitherto possessed. Nevertheless its position in the desert midway between the Oasis and Caspian, its great fertility and dense population—estimated at about 500,000—its numerous antiquities and grand historic memories, must always ensure for the "Queen of the World" an exceptional importance in the eye of the statesman and historian. The student will here find ample details of its present social and economic condition, of its government and administration, of the organisation of the Toktamish and Otamish Tekke tribes,[1] their local institutions, the water system of the Murghâb, the remains of Bairam Ali, and other cities which successively bore the name of Merv, the home life of the Mervlis, their actual commercial and political relations and future prospects.

A very full description is given of the many ruins scattered over the Oasis, all of which were visited and sketched by the explorer. Of these the most extensive are Giaur Kala, the original site of Merv, destroyed about the end of the seventh century by the Arabs, and Bairam Ali, its successor, destroyed in 1784 by the Amir of Bokhara. A general plan is given of all these crumbling citadels, palaces, tombs, baths, and earthworks, "where now no living creature is to be met with, save an occasional Ersâri robber or treasure seeker. For here, as in almost every other part of the East, the popular imagination enriches these ruined vaults and foundations with secret treasures stowed away beneath them" (ii. 241).

From the frequent recurrence of the term *Kalassi*, supposed to be a corruption of *ecclesia*, some archæologists have scattered the remains of ancient Christian churches with a liberal hand over Western Turkestan. But Mr. O'Donovan suggests that there is here a confusion between *Killissi*, which really represents *ecclesia*, and *Kalassi*, a Turki form of the Arabic *Ḳal'a*, a fort or castle.[2] Hence Kara Kalassi, for instance, would mean, not the "Black Church," but the "Black Castle." In Armenia, a Christian country, *Killissi* certainly occurs; but in the Oasis Mr. O'Donovan "never came upon any structure which could possibly have been a Christian Church" (ii. 177).

Amongst the remains are some earthworks bearing the title of Iskander Kala, or "Alexander's Castle," the local tradition being that the Macedonian army encamped here on its way to India. But here again he pertinently remarks that "in these countries Alexander comes into every story connected with ruins of remote antiquity."

[1] The subdivisions of these two main branches of the Merv Tekkés do not correspond with those given by Mr. Marvin in his "Merv, the Queen of the World." Some of the discrepancies however may be reconciled by restoring to their proper form the names disguised in Mr. O'Donovan's peculiar orthographic system. Thus his *Karatchmet* appears by reference to Marvin's tables to stand for the *Kara-Ahmed* subdivision of the Otamish branch.

[2] At the same time this *Kalassi* would appear in many cases to be simply the Persian *Kalâsa*, a well, and especially the watering-places maintained at intervals in the desert for the convenience of caravans and pilgrims to Mecca.

Some points of resemblance are discovered between the Turkoman and Kelt, which are probably not intended to be taken seriously. But the description of the Turkoman type, coming from a shrewd and original observer, possesses sufficient ethnological value to deserve quoting:

"The usual Turkoman physical type, both male and female, is rough, rude, and vigorous, and quite in contrast with that of the frontier Persian, which is sleek, cat-like, feeble, and mean. The worst part of the Turkoman is his head, which is decidedly conical, the point being thrown somewhat to the rear. A phrenologist would say that firmness was very pronounced, conscientiousness wanting, and benevolence small. The features are not of that Tartar cast that one would be apt to suppose in denizens of East Caspian districts, and though here and there may be seen a suspicion of peeping eye, a tendency towards flattening of the point of the noise, and occasionally high cheek bones, on the whole the faces are more European than otherwise. In fact I have seen some physiognomies at Gumush Tepé which, if accompanied by an orthodox European dress, would pass muster anywhere as belonging to natives of the West. It is among the women that the absence of European features is most conspicuous. There are many of them who could fairly be reckoned pretty, though it is quite a different order of beauty from that to which we are accustomed. . . . It is among the men that the handsome individuals must be sought for, especially when there has been an admixture of Persian blood. The scanty beard of the pure Turkoman is then replaced by one of much more luxurious proportions, and of a darker tint; the nose assumes a more or less aquiline form, and the eye loses the cold grey expression so characteristic of the pure-blooded dweller on the Steppes" (i. 231-3).

The accompanying portrait of the author in oriental garb might be taken as an apt illustration of this description. There is also an excellent map of North Persia and the Trans-Caspian region, based on that of Colonel Stewart, but with numerous fresh details embodying the results of the explorer's observations in the Tejend Valley and Merv Oasis. But the spelling is as usual at variance with that of the text, and there is unfortunately no index. The appendix contains facsimiles of a number of letters from Turkoman Khans, one or two of which are fine specimens of the beautiful ta'lîk penmanship.

The reputation of an intelligent and enterprising explorer secured to General Macgregor by his "Journey through Khorasân (noticed in NATURE, vol. xx. p. 453), will be considerably increased by his "Wanderings through Baluchistan." The trip was made in company with the ill-fated Capt. R. B. Lockwood, of the 3rd Bengal Cavalry, on their return to duty in India, between the months of September, 1876, and March, 1877. During this period the western section of Makrân was thoroughly explored, and the problems connected with the drainage of the Mashkîd and Mashkel rivers at last cleared up. The Mashkîd was supposed by many geographers to flow through the Dasht to the Arabian Sea, while the Mashkel was sent northwards to the Zirreh or Sistân Hamun, that is, to the Helmand basin. But by actual survey the explorers have shown that (1) both of these rivers belong to the same hydrographic system; (2) this system is unconnected either with the Helmand or Arabian Sea; (3) the two streams, after their confluence above the romantic Tank Zorati pass in the Sianeh-kuh range, flow mainly north-west to the Mashkel Hamun in 28° 20′ N., 60° E.; (4) this swamp has no outlet, and is actually separated by another depression, the Kindi Hamun,

and by a range of hills, the Koh-Amir, from the Sistán Hamun. A ride performed under great difficulties across the Kharan desert to the neighbourhood of the Sistán swamp placed all these points beyond doubt, so that the drainage system of the hitherto almost unknown region along the Perso-Baluch frontier, from the Lower Helmand to the Arabian Sea, has now been satisfactorily determined. From Sistán the travellers made their way by two new and parallel routes right across North Baluchistan to Jacobabad in Sind. The numerous typographical points recorded both here and throughout West Makrán are embodied in the accompanying map, which is on a large scale, and which forms an important contribution to our knowledge of the south-eastern section of the Iranian plateau. In the appendix are given the directions, distances, and other useful details of no less than twenty-two routes in the same region. The relief of the land and its salient physical features are also further illustrated by numerous sketches made on the spot by General Macgregor. Much valuable matter regarding the Baluchi and Brahui tribes, and the present political situation of Baluchistan, is scattered over the pages of this pleasantly written volume. A. H. KEANE

PHYSICAL OPTICS

Physical Optics. By R. T. Glazebrook, M.A., F.R.S. (London: Longmans, 1883.)

THIS is the most recent volume of the well-known series of Text-books of Science published by Messrs. Longman. Mr. Glazebrook is already favourably known as an accurate experimenter and an able theorist in the subject of which this volume treats, and it is therefore unnecessary to say that the treatise under notice contains a large amount of authentic and interesting information on all branches of the subject. We must confess, however, to a certain feeling of disappointment after going through the book, arising chiefly from the fact that the author does not appear clearly to have made up his mind as to the class of readers to whom the book is to be useful. Those who have had any experience in real personal teaching of the artisans and students in science schools for whom the volumes of this series are stated to be intended, will soon perceive that Mr. Glazebrook has assumed an amount of mathematical knowledge and ability which very few of them possess. On the other hand results are occasionally assumed, the investigation of which would be quite within the reach of those university students who will probably form the larger part of the readers of the treatise. For instance, the investigation of the focal lines of a pencil refracted in a principal plane through a prism, and the condition of their coincidence in the position of minimum deviation, is settled by an "it may be shown," although the analysis required is certainly not more difficult than much that is given in the book, and the point to be elucidated is of considerable importance.

The author has intentionally introduced a large quantity of matter which is usually considered to belong to the kindred subject of Geometrical Optics, and although there will probably be a difference of opinion as to the advantage of this proceeding, there will be none as to the clearness of the explanations and the excellence of the diagrams employed. There does not seem to be quite so much "matter new to the text-books" as is hinted at in the preface, but on the whole the book furnishes a good account of the subject, comparable with Lloyd's well-known treatise on the Wave Theory of Light, and dealing with many points which have been investigated since the date of the latter work.

Where there is so much of good, it is a pity that it should not be made better, and there are a few points in which perfection has not been reached. In places there is a tendency to a slipshod and "high-falutin" method of expression which may be forgiven in University Extension lectures delivered extempore to popular audiences, but which is hardly suitable for a scientific treatise. On p. 2 we have a graphic representation of the author raising his arm and of the effect thereby produced on his own body. Later on in the book, quitting the solitary first person, he becomes more friendly to his readers, and speaks of "our apertures, our lens, our prism, our eye," and so on. He even presently hands the apparatus entirely over to the reader and directs him to perform the operations for himself. A more serious matter is the want of care in revising the proofs. For instance, on p. 202 an effect is spoken of as "that due to a single aperture multiplied by a number of apertures," which is nonsense, the author's meaning being "multiplied by *the* number of the apertures." Again, on p. 141, the sentence—"They are distinct from the coloured rings of thick plates discovered by Newton, and were described by him as follows," gives an almost opposite meaning to that which the author intended. It ought to read "which were described."

The proper names are not treated with the accuracy and uniformity which are desirable. We have Fraunhofer usually, but on p. 316 Frauonhofer. Huygens appears on p. 15, but more frequently the name is met with as Huyghens, while the possessive case assumes the different forms of Huyghen's, Huyghens', and on p. 226 Huyghens's. Defects of this kind mar the pleasure with which the book would otherwise be read, and seem to indicate that more care might have been advantageously bestowed on the original composition as well as on the revision of the proofs. Possibly the author wishes to leave something to be looked for in the second edition, for the speedy arrival of which he has our best wishes.

OUR BOOK SHELF

The Year-Book of Pharmacy, 1882. 8vo. Pp. 607. (London: Churchill, 1883.)

THIS volume contains a number of exceedingly interesting papers and extracts. The most interesting are those which relate to the artificial production of organic alkaloids, for when we obtain such a knowledge of the constitution of these bodies as will enable us to make them artificially, we may hope that a new era will commence in medicine, and that the results of the treatment of disease will be more definite and satisfactory than heretofore.

Prof. Ladenburg, who has been engaged for some time on researches into those alkaloids which dilate the pupil, is still continuing his researches, and has obtained very interesting results indeed. Atropia when heated with strong hydrochloric acid splits up into a base, tropine, and an acid, tropic acid. While pursuing his investigations upon tropine, the author came to the conclusion that this base contained an alcoholic hydroxyl group

which possessed the exceptional property of forming fresh alkaloids, when treated with certain acids in hydrochloric solution as in the preparation from it of atropine and homatropine. By acting upon secondary amines by chlorhydrines, he has succeeded in obtaining a series of bases analogous to tropine, and yielding, like it, other basic compounds, which resemble natural alkaloids in their properties and composition. For these bases, which perform the function of alcohols and amines, the author proposes the name of *alcamines*, and for the basic ethers derived from them, that of *alcameines*. That resulting from the action of phenylacetic acid, for instance, has a composition represented by the formula $C_{15}H_{21}NO_2$, and forms crystallisable salts. It is a powerful poison, acting on the respiration and the heart.

A curious relation has been discovered between theobromine, the active principle of cocoa, caffeine, the active principle of tea and coffee, and xanthine, a substance found in muscle, and largely contained in beef tea and Liebig's extract.

Dr. Fisher shows that xanthine may be converted into theobromine, and theobromine into caffein. The relation between these bodies seems to be that theobromine is dimethyl- and caffein is trimethyl-xanthine.

From the great importance of the cinchona alkaloids and their extensive use in medicine, it is exceedingly desirable that we should be able to make quinine artificially. This has not yet been done, but, with a view towards it, extensive researches are being made into the constitution of the cinchona alkaloids; and Skraup finds that all the four cinchona alkaloids—quinine, quinidine, cinchonine, and cinchonidine—when oxidised with potassium permanganate yield formic acid, and a base apparently related to phenol or carbolic acid. Other modes of oxidation exhibit a relation between quinine and cinchonine.

Cinchonine has been prepared artificially by treating a mixture of nitrobenzol, aniline, and glycerol with sulphuric acid. In its physiological properties it exhibits a certain relation to quinine, and, like it, reduces the temperature in fever and lessens or prevents putrefaction. It is said to differ from quinine in not producing giddiness, or ringing in the ears, and to have very little action on alcoholic fermentation.

An important paper by Plugge relates to the various strengths of aconitine. He finds that Petit's nitrate of aconitine has a poisonous action at least eight times greater than Meret's and 170 times greater than Friedländer's. Such differences as these between preparations bearing the same name have already led to fatal cases of poisoning—when aconitine has been prescribed medicinally; and Mr. Holmes considers that the only way to secure uniformity in the ordinary preparations of aconite is to prepare them only from plants grown in this country, and gathered while the plant is in flower.

In addition to many other interesting papers this volume contains a bibliography of chemistry, pharmacy, botany, and allied subjects.

Mémoires de la Société des Sciences Physiques et Naturelles de Bordeaux. 2e Série, Tome v. 1ie Cahier. (Paris, 1882.)

IN this number there are several interesting papers. We note "La Route d'Australie par le Thermomètre," with tables, by M. Hautreux; "Sur les Unités de Gauss," by M. Abria; "Le Téléphone à Bordeaux," by M. Auguste Bonel; "Notice sur les Communications Télégraphiques sous-marines," by the same; "Modification aux Machines à Force centrifuge," by M. O. de Lacolonge; "Températures et Densités de l'Eau dans l'Estuaire de la Gironde," by M. Hautreux; "Vérification expérimentale des Lois de Dalton relatives à l'Évaporation des Liquides," by M. E. Laval. M. P. Tannery contributes one of his useful critical notes, this time "Sur une Critique ancienne d'une Démonstration d'Archimède." The criticism is contained in § 36 of the fourth book of the "Collections" of Pappus (Hultsch's edition), and it charges Archimedes (in Prop. 18 of his "Spirals") with solving as a *solid* problem (*i.e.* with the aid of the conic sections) the following proposition:—*O M A* is a spiral, of which *O* is the pole, *O A* the axis, and *A C*, the tangent at *A*, meets the perpendicular to the axis through *O* in *C*, then *O C* is equal in length to the circumference described with *O A* as radius. M. Tannery supplies the gist of Archimedes' proof, and shows that Archimedes "fait appel simplement à l'intuition et au principe de continuité." Other points of interest turn up in the communication. Dr. Sigismond Günther is engaged upon an extended inquiry into the processes employed by the ancient mathematicians in the extraction of square roots, and in the course of his work has met with some interesting results. Some of these he puts forth in his paper "Sur la dépendance entre certaines méthodes d'extraction de la racine carrée et l'algorithme des fractions continués." The methods examined are those of Mollweide ("Commentationes mathematico-philologicæ tres," Lipsiæ, 1813), and of Alexeieff ("Sur l'extraction de la racine carrée d'un nombre," *Bulletin de la Soc. Math. de France*, t. vii. p. 167).

We have left to the last an article by Dr. Adolf Dux (translated from the *Pester Lloyd* for February 4, 1880), entitled "La Tombe du Savant." Bolyai was professor of mathematics and physics in the "Collège Réformée" at Maros-Vásárhely. No statue, nor marble mausoleum with sides covered with laudatory inscriptions, marks the spot where this *savant* lies; but the tomb, by its occupant's strict direction, is overshadowed by the boughs of an apple-tree, "*en souvenir des trois pommes* qui ont joué un rôle si important dans l'histoire de l'humanité, et il désignait ainsi la pomme d'Ève et celle de Pâris qui réduisirent la terre à l'esclavage, et la pomme de Newton, qui la replaça au rang dès astres." Strangely enough, when Dr. Dux visited the tomb there hung on the tree just three apples, "ni plus ni moins."

Bolyai was not only a mathematician, he was also a poet: hence he had not only in his room a portrait of Gauss (with whom he had been associated at Göttingen from 1797 to 1802), but also the portraits of Shakspeare and Schiller.

In 1855 he wrote his own *Necrologe*, and survived its completion about one year. In this "Adieu" occur passages, some grave, and some humorous; of himself he writes, "S'il a été mauvais, la terre est délivrée de lui; s'il a été bon, il est délivré de la terre." He burned his poetical writings, and collected the ashes in a wooden cup, on which he wrote the following lines from Horace:—

"Poesis
Si paulum a summo discessit, vergit ad imum."

This is now preserved as a relic in the library of the College. Here too is shown a photograph taken after Bolyai's death. "Une noble figure, au front et au nez puissants, illuminée par la majesté de l'esprit et du sublime repos, avec de longs cheveux lisses, descendant jusqu'aux épaules. Quand on a vu ces traits, on comprend mieux le sens du nécrologe que Bolyai a lui-même écrit et suivant le texte duquel un noble pommier a été planté sur sa tombe.'

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Natural Selection and Natural Theology

I HAVE just received last week's issue of NATURE from England, and find in it some remarks by Prof. Asa Gray on an

article of mine which appeared in the *Contemporary Review*. As he appears to solicit a further statement of my views, I shall supply a brief explanation of those passages in my article to which he refers.

This article, I must begin by observing, was written in reply to a criticism on my essay in the "NATURE Series" ("Scientific Evidences of Organic Evolution"), and, being intentionally limited to the ground covered by that criticism, it did not require to discuss all the points raised by Prof. Gray. But without reference to the original article, I shall now consider these points *seriatim*.

First, I am requested to state what I mean by urging that in my opinion there is no logical point of contact between natural science and natural theology. My answer is that natural science, *as such*, can only be legitimately concerned with the investigation of natural or physical causes, and that in whatever degree it presumes to pass beyond the territory of such investigation, it ceases to be natural science, and becomes ontological speculation. In other words, there is no point of logical contact between the methods and aims of natural science, as such, and the super-scientific conclusions which constitute the aim of natural theology. For it is the aim of natural theology to establish certain very definite conclusions with reference to the existence and the character of a "*causa causarum*," which is acknowledged to be supernatural at least to the extent of being inscrutable by any of the methods possible to science. But from this sufficiently obvious position it does not follow, as my critic seems to insist, "that because natural phenomena can be reduced to laws and sequences of cause and effect, no legitimate or rational inference can be made by the human mind to a *causa causarum*." Whether or not any such legitimate or rational inference can be made, from the data mentioned, *by the human mind* is not here the question, and therefore I decline to enter upon it. The only question before us is as to whether any such inference can be drawn by the human mind from the province of *natural science*, and I say that this question must be answered in the negative, seeing that there is, as I have just explained, an absence of logical contact between the sphere of natural science and the sphere of supernatural theology. Any inferences of the kind in question, be they legitimate or not, are drawn from the *general* order of Nature—*i.e.* from the *universal* prevalence of "laws and sequences of cause and effect"; therefore they are not really or logically strengthened by a mere enumeration of *particular* instances of such laws and sequences all similar in kind. The so-called law of causation as a whole being known, and its universality recognised, its true argumentative value to the theory of theism is not influenced by the explicit formulation of any number of its specific cases which the progress of science has been able, or may be able, to supply.

I allow, of course, that *the human mind* cannot avoid occupying itself with the most momentous of all questions—the nature of the First Cause and of its relations to the universe—or, in the words of Prof. Gray, that "such questions are inevitable"; I am only concerned with explaining why I conceive that such questions are not affected by any of the methods or results of natural science, which do not stand in any logical relation to them. And this introduces me to the next point in Prof. Gray's criticism. He says that I am inconsistent in first alleging that there is no point of logical contact between natural science and natural theology, and then proceeding to affirm that the theory of natural selection has proved destructive of the evidence of special design in organic nature. But I think this charge must have been made without due reflection; for, if a man believes that there is no logical connection between one thing and another, I do not understand why he should be deemed inconsistent merely because he endeavours to show the fictitious character of the logical connection which has been erroneously supposed to exist. Assuredly I think that "Darwin's theory need not, and legitimately should not, concern itself with natural theology"; but I also think that natural theology should not seek to obtain unreal support from natural science, and it is because natural theology has sought to do this in one conspicuous instance, and in that one instance has been as conspicuously met by Darwin's theory, that, as I explained in my article, it seemed to me desirable, both in the interests of science and of theology, henceforth clearly to recognise the logical gulf which is fixed between these two departments of human thought.

Next Prof. Gray observes, and quite correctly, that my view of the matter as a whole is fairly presented by the following sentences, which he quotes:—

"The facts of organic nature furnish no *evidence* of design of a quality other or better than any of the facts of inorganic nature." "Or, otherwise stated, there is nothing in the theory of natural selection incompatible with the theory of theism; but neither does the former theory supply *evidence* of the latter. Now this is just what the older theory of special creation did; for it would be proof positive of intelligent design if it could be shown that all species of plants and animals were created, that is, suddenly introduced into the complex conditions of their life; for it is quite inconceivable that any cause other than intelligence could be competent to adapt an organism to its environment *suddenly*."

Prof. Gray then asks: "Is the writer of this quite sure that any cause other than intelligence could be competent to adapt existing organisms to their environment *gradually*?" My answer is but too easy. I must leave to others the happy position of being "quite sure" about anything relating to the possibilities of supernatural causation. For aught that I know, or for aught that any living man can ever know, not only all existing organisms, but all existing atoms, may have depended from all time, and for all their changes, sudden or gradual, upon "intelligence," without which they may not have been able either to have lived or to have moved, or even to have had their being. But how does this necessary ignorance on my part affect my statement that "the facts of organic nature present no *evidence* of design of a quality other or better than any of the facts of inorganic nature"? I confess I do not see how this failure in the evidence of design is made good by telling me that, for anything to the contrary of which I can be "quite sure," there *may* have been a designer. For I cannot follow my critic where he argues that the element of a supposed sudden introduction of an organism to its environment makes no difference in the evidence of its adaptations to its environment having been designed. He asks: "How is this presumption [*i.e.* that of special design] negatived or impaired by the supposition of Darwin's theory, that the ancestors were not always like the offspring, but differed from time to time in small particulars, yet so as always to be in compatible relations with their environment?" The answer is, that if we suppose the sudden or special creation of organisms in manifold adaptation to their several environments, we can conceive of no cause other than intelligence as competent to produce the adaptations, whereas, if the adaptations have been effected gradually, and *by the successive elimination of the more favourable variations by a process of natural causation*, we clearly have a totally different case to contemplate, and one which is destitute of any evidence of special design. Assuredly "*gradualness* is in nowise incompatible with design," and I do not suppose that there has ever been any one so foolish as to imagine that it is; but all the same, the progressive adaptations of structures to functions by such a purely physical cause as natural selection when once clearly revealed must destroy all special or particular *evidence* of design, even supposing such design to exist. For under this point of view it was *only* those variations which were "in compatible relations with the environment" which were able to survive. Only if it could be shown that the variations always took place exclusively in the directions required for a development of the adaptations, so as to leave no room for the operation of the physical cause in question—only then would the evidence of design as deduced from the theory of evolution be comparable with that evidence as deduced from the theory of special creation.

Towards the close of his letter, Prof. Gray seems to have anticipated this obvious rejoinder, for he says that, in order to make the purely physical explanation tenable, "it must be shown that natural selection scientifically accounts for the adaptation,"—*i.e.* as I understand, it must be shown that there is not some influence of an intelligent kind guiding the occurrence of the variations in the requisite lines, which, having been thus intelligently caused to arise, are then seized upon by natural selection. If this is Prof. Gray's meaning, he is certainly wrong in attributing it to Mr. Darwin, and I cannot see that it is a meaning of any argumentative use. For the burden of proof lies with the natural theologian to show that there *has* been some such intelligent guidance of the variations, not with the evolutionist to show cause why there *may not* have been such guidance. The evolutionist may freely admit that natural selection has probably not been the only physical cause at work, and even that the variations supplied to natural selection may not have been wholly fortuitous, but may sometimes have occurred along favourable lines as "responses of the organisms to their

physical surroundings."[1] But such admissions would make no change in the logical aspect of the case; for, however many supplementary causes of this kind we may choose to imagine as possible, the evolutionist is bound to regard them as all alike in this—that they are of a physical or natural kind.

And this leads me to the core of the whole subject. Prof. Gray says:—

"What is probably meant is, that natural selection is a rival hypothesis to design, that it accounts for all adaptations in the organic world on physical principles, and so renders . . . the evidence of design from these adaptations of no other or better value than that from anything else in Nature." He then proceeds to object to this view, and says:—"If means and ends are practicable in inorganic nature at all, it is only by remote and indirect implication; while in organic nature the inference is direct and unavoidable. With what propriety, then, can it be affirmed that organic nature furnishes no other and no better evidence of underlying intelligence than inorganic nature? The evidence is certainly *other*, and to our thinking *better*."

This, I say, is the core of the whole subject. If once it is fully admitted and understood that organic nature is one with all the rest of the universe in the matter of physical causation, so that all the wonderful adaptations which we there encounter are the results of natural causes—survival of the fittest *plus* any number of other natural causes—then it appears to me, as I have said in the essay already alluded to, that all such cases of adaptation must fall into the same logical category, with reference to the question of design, as all or any other series of facts in the physical universe. For the only element of difference arises from the greater intricacy of the physical causation in the cases contemplated, rendering it more difficult to perceive the operation of the causes, at work, and therefore, as Prof. Gray truly asserts, rendering their operation more suggestive of design. But this element of difference does not really affect the question. For, *ex hypothesi*, the law of causation is everywhere and equally uniform, and for this reason the evidence of design in organic nature is certainly *not* other than it is in inorganic nature, nor, in view of the same reason, is it, to our thinking, better.

Florence, February 3 GEORGE J. ROMANES

THE letter of Prof. Asa Gray (NATURE, vol. xxvii. p. 291) contains a sentence which seems to me to contain the essence of the difference between the views of organic life, as held by the supporters of Natural Selection and Natural Theology. He says: "How is this presumption negatived or impaired by the supposition of Darwin's theory, that the ancestors were not always like the offspring, but differed from time to time in small particulars, *yet so as always to be in compatible relations to the environment?*" The italicised portion is just such a statement as "Design" would require, but cannot be held by scientific evolutionists, otherwise why are there so many extinct species? With "Design" there ought to be a perfecting of all species; whereas we know of so many which have been ruthlessly swept aside, owing to their having "differed (or owing to their not having sufficiently differed) from time to time in small particulars, yet" *not* "so as to be in compatible relations to the environment." Change is the evolutionist's view of life—change sometimes caused by the environment, sometimes beneficial, sometimes eventually detrimental: where beneficial, the species increases; where detrimental, other changes or extinction must ensue. Design would never have supplied us with a "Nature red in tooth and claw with ravine," nor would it have built up a system by the expensive and cruel mode of trial and error.

Cove Castle, Loch Long, N.B. J. B. HANNAY

Two Kinds of Stamens with Different Functions in the Same Flower

To the Melastomaceæ and Commelynaceæ mentioned in NATURE (vol. xxiv. p. 307, vol. xxvi. p. 386, and vol. xxvii. p. 30), may be added the genera *Mollia* (Tiliaceæ), *Lagerstræmia* (Lythraceæ), and *Heteranthera* (Pontederaceæ), for having differently coloured anthers. In several species of *Mollia*, according to Darwin ("Forms of Flowers," p. 168, footnote), the longer stamens of the five outer cohorts have green pollen, whilst the shorter stamens of the five inner cohorts have yellow pollen; the stigma stands close beneath the uppermost anthers. In a *Lagerstræmia* in my garden the six outer stamens have green pollen, and are much longer than the numerous inner ones, which have bright yellow pollen; the stigma stands on a level with the outer anthers. I have repeatedly seen bees alighting on, and gathering the pollen of, the inner anthers without noticing the outer ones.

In *Heteranthera reniformis* there is one long stamen (belonging to the outer whorl) having pale bluish pollen, and two short stamens (of the inner whorl) with bright yellow pollen. The stigma stands generally on a level with the anther of the long stamen. When the white flower opens, pistil and long stamen diverge, the pistil bending (almost without exception) to the right, and the stamen to the left; at the withering of the flower, they again approach each other, so that the stigma may be fertilised by the pollen of the long stamen. Visiting insects are attracted yet more to the yellow anthers of the two short stamens by their being placed close to a yellow spot, surrounded by a violet border, at the base of the upper petal.

Thus it may be safely assumed that in all these flowers, as well as in the above-mentioned Melastomaceæ and Commelynaceæ, fertilisation is almost exclusively effected by the pollen of the longer stamens, whilst the shorter stamens serve only to attract pollen-gathering or pollen-eating insects. It is far from surprising that the pollen of these latter stamens, though often produced in large quantity, should tend to degeneration. Darwin long ago came to this conclusion with respect to some Melastomaceæ with differently-coloured anthers, of which he had raised seedlings from pollen both of the longer and shorter stamens ("There is reason to believe that the shorter stamens are tending to abortion."—"Cross- and Self-Fertilisation," p. 298, footnote). The *Lagerstræmia* in my garden being self-sterile, I fertilised some flowers with green, and others with yellow pollen of a different variety (or species?) growing in other gardens; both produced fruits with apparently good seeds, but only some of those from the green pollen have germinated.

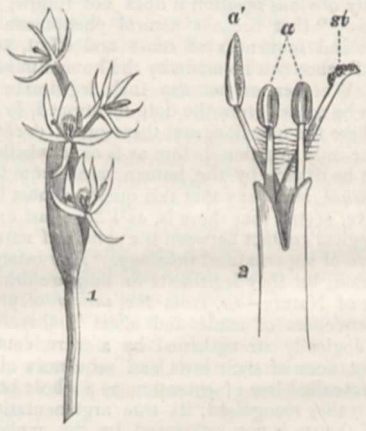


FIG. 1.—Flower-spike of *Heteranthera reniformis* (natural size). FIG. 2.—Upper end of the flower-tube, seen from behind. *a'*, the one anther of the outer whorl, with pale bluish pollen; *a*, the two anthers of the inner whorl, with bright yellow pollen; *st*, stigma.

As in all the flowers above-named, with differently-coloured anthers, the dull colour of those of the longer stamens evidently serves to make them less visible to insects, may not the green colour of the anthers of the long stamens of the mid-styled and short-styled flowers of *Lythrum salicaria* also protect them against the attacks of pollinivorous insects, to which, from protruding far from the corolla, they would be more exposed than those of the shorter stamens?

Even without being differently coloured, the stamens of the same flower may be divided into different sets with different functions. Thus in a species of *Cassia* the visiting humble-bees gather the pollen of the four intermediate stamens (the three upper ones being pollenless), which are short and straight, whilst the three lower ones are very long and curved in such a way that their pollen is deposited on the back of the humble-bees. The pistil is of the same length and curved in the same way as the longer stamens. Another very striking instance has been carefully described by Prof. J. E. Todd of Tabor (Iowa) in

[1] In my "NATURE Series" essay I expressly stated that natural selection is probably not the only cause of organic evolution, and therefore I think it might have been well if my critic had taken the trouble to refer to this essay before indulging in the general proposition at the close of his letter with reference to exactitude.

a plant of a very different family, viz. *Solanum rostratum* (*American Naturalist*, April, 1882, p. 281): one stamen and the pistil are very long and strangely curved; four stamens are short and straight, and serve only to furnish pollen to the visiting insects; all the anthers, as I am informed by Prof. Todd, are of the same dull yellow colour. FRITZ MÜLLER

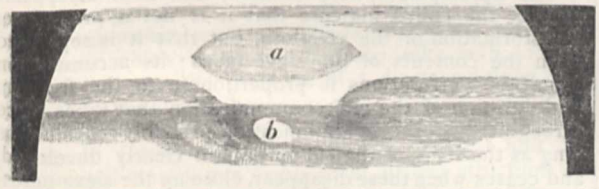
Blumenau, Santa Cattarina, Brazil, December 27, 1882

The Markings on Jupiter

AFTER heavy storms of hail on January 30, the sky cleared and the night was exceptionally fine. I observed Jupiter with my 10-inch reflector about 11h. 30m., and watched the chief markings pass the central meridian of the planet. The well-known equatorial white spot came to transit at 11h. 44m., and it was followed 5 minutes later—at 11h. 49m.—by the great red spot. These objects, therefore, must have been in conjunction on January 30, at 2h. 47m., as the greater velocity of the white spot enables it to gain 13m. 24s. on the red spot daily.

In NATURE, vol. xxv. p. 225, I stated that during the 400d. 0h. 20m. elapsed between 1880, November 19, 9h. 23m. and 1881, December 24, 9h. 43m., the white spot had completed 9 revolutions of Jupiter relatively to the red spot; the number of rotations performed by the former being 976, and by the latter 967. Since 1881, December 24, I have continued to watch the anomalous velocity of these curious markings, and find that between that date and 1883, January 30, the white spot has completed 9 further revolutions of Jupiter. From 1881, December 24, 9h. 43m., to 1883, January 30, 2h. 47m., is 401d. 17h. 4m., during which the white spot has rotated 980 times, while the number for the red spot is 971. In fact my observations since 1880, November 19, show that up to 1883, January 30, the white spot had performed 1956 rotations, as against 1938 by the red spot in the interval of 801d. 17h. 24m.

On January 30, when I last saw these markings, the red spot was remarkable on account of its great faintness. On the other hand, the equatorial white spot was extremely brilliant and conspicuous, and formed one of the most noticeable features on the planet. Observers should now keep a close watch on the red spot, as it seems likely to be on the point of disappearance, though this disappearance need not necessarily be of final character. It fortunately happens that a curious irregularity in the formation of the great southern belt will probably enable the exact position of the spot to be watched for a considerable time. This particular region of the planet, as I drew it on January 30 at midnight, was as follows:—



1883, January 30, 12h. The region south of the equator of Jupiter. *a*, The red spot; *b*, white spot.

The sketch shows that the great south belt is now double, and a very conspicuous object on Jupiter. The south half of this belt is bent abruptly to north, and runs into the other half exactly north of the preceding and following ends of the red spot. There is some explanation to this interesting feature, though it is at present involved in mystery; in any case it may possibly serve as a very accurate indication of the place of the red spot long after that object has become obliterated altogether. W. F. DENNING

Bristol, January 31

Meteor of November 17

I THINK now that more observations of the remarkable phenomenon of November 17 have been brought forward, that we cannot but candidly acknowledge that the evidence is extremely contradictory and impossible to reconcile, that is as applying to one and the same object. Altogether there is something mysterious about it. It is evident that since it appeared to reach the greatest apparent length of about 30° at York, then from all places further south it ought to have attained a length exceeding this, the more so the further south they are. The ends of the beam appeared very well defined from here, and there was very little room for estimates varying according to the observer's sensitiveness to light. If we take the observations made from Clifton, Cirencester, East Clevedon, Woodbridge, and Windsor, as they nearly all agree in estimating the length as *over* 30°, some considerably over, then these may all relate to the same object. But its appearance from York is flatly contradicted by Mr. Batson's observation from Hungerford, that from Halstead, Essex (which seems to agree with Mr. Batson's), also those from Lincoln's Inn Fields, Greenwich, and Cambridge. All these agree in contradicting the others named above, by assigning a much *smaller* angular length. Mr. Batson describes a sudden foreshortening which the meteoroid underwent when passing the moon, and since I saw it pass below the moon at practically the same time, then (on the supposition that we beheld the same object) the same shortening ought to have been visible to me; but there was not the slightest trace of any such thing. I noticed that it very gradually shortened in length (after allowing for perspective) in its journey towards the west, which is significant, and explainable if we suppose the body to have been encountering resistance to its momentum. It is impossible to reconcile all the observations, and yet most extraordinary that no single observer is known to have witnessed more than one such phenomenon at about that time except Mr. Worthington, who says he saw two *at once*. I have reason to believe that a rather similar thing was seen below the moon at about 5.30 on that night from here. I see that from Zierickssee, in Holland, a similar phenomenon was seen to transit α Pegasi (which would be at about 50° altitude, and on the magnetic meridian from there). If this was the one that I saw, then at the time that it was seen to transit α Pegasi, from Holland, it would appear to me to be just forming in the south-east, where it appeared to be about 10° above the horizon, it which case it would have to be under seventy miles high when over Belgium. But it is almost certain that it attained a height of over 150 miles during the latter part of its course. As yet (figuratively speaking) the spectra of these auroral phenomena have not thrown as much light on these things as that which enters the narrow slip of the spectroscope to print its uncertain record on the retina. I only hope that some one with a clear head and much patience will succeed in unravelling the tangled skeins of evidence which surround the mysterious meteoroid of November 17, 1882. H. DENNIS TAYLOR

Heworth Green, York, February 11

Aino Ethnology

IN an article on "Aino Ethnology" which appeared in NATURE, vol. xxvi. p. 524, and which I happened to read only a few days ago, Mr. A. H. Keane makes the following statement:—"Until the appearance of Herr Rein's large work on Japan, one of the most universally-accepted of these conclusions was that, whatever be their affinities, the Ainos must certainly be separated from the Mongolic connection. No little surprise was accordingly produced by Rein's attempt to affiliate them to the surrounding members of the yellow race. But it was soon seen that his arguments, apparently inspired by a love of paradox, were sufficiently refuted by the very illustrations of the Aino type introduced into his work."

I submit that one who has read my work upon Japan will decide, with me, that the spirit of the matter quoted is unfair, in so far as it charges me with "attempts" at affiliation, and with being "inspired" otherwise than by a love of truth—this motive being, as stated in my preface, that which induced me to write.

It is repeatedly mentioned in my book that I had never been in the island of Yezo, and those who have carefully read the whole work—including Mr. Keane, if he has done so—cannot reasonably fail to observe that I speak of the Aino tribe as one who had never visited them in their proper home, nor made them a special subject of study in any respect. My remarks upon their probable racial affinities were based upon good, and the then latest, authorities, whose names I was careful to mention. Thus, on p. 444, occurs a passage of which the following is a rendering:—"Dœnitz and Hilgendorf have made thorough investigations of their (the Ainos) physical peculiarities, and have published the results thereof in the *Mittheilungen der Deutschen Gesellschaft Ostasiens*. It appeared as an undoubted fact 'that the Ainos are Mongolians, who are separated from the Japanese in a perhaps less degree than the Germans from the Romans.'"

In the comments which I added to the views of these eminent observers, wherein I mentioned those circumstances that seemed to me to tend in the direction of their support, I was, of course, unable to include, as I would if possible have done, a statement of the opinions of such noted authorities as Dr. Steube and Herr von Siebold, the results of whose investigations have so recently been given to the press, and which are cited in the article that elicits this letter. J. J. REIN

Marburg, Germany, February 8

Hovering of Birds

I REGRET that I did not notice until to-day that Mr. Airy, in his letter published in NATURE, vol. xxvii. p. 294, specially referred to "hovering with perfectly motionless wings" as being that for which an upward slant of wind is, as he believes, absolutely requisite to enable the bird to do so.

Is the term "hovering" applicable to the examples given by Mr. Airy of gulls and hawks floating as it were with motionless wing along hillsides and cliffs?

I have always associated "hovering" with the flapping or fluttering of the wings, as is invariably noticed when terns or hawks are looking for their prey either over land or water.

February 10 J. R.

Intelligence in Animals

IN his letter in NATURE, vol. xxvii. p. 337, Mr. J. Birmingham does not mention what kind of bear it is that throws down pieces of rock "in order to catch the bareins," as told by the Kamtschadales; but the Eskimos have a somewhat similar story of the white bear, when attacking the walrus, the largest of which, with their formidable tusks, Bruin generally avoids.

The circumstance was told me by an eye-witness, a very truthful and honest Eskimo of Repulse Bay. He said: "I and two or three other Innuit were attempting to approach some walrus in winter, lying on the ice close to the water, kept open by the strong current, in Fox's Channel. As we were getting near we saw that a large white bear was before us. He had reached in the most stealthy manner a high ridge of ice, immediately above where the walrus were lying; he then seized a mass of ice[1] in his paws, reared himself on his hind legs, and threw the ice with great force on the head of a half-grown walrus, and then sprang down upon it."

The Eskimos then ran up, speared the bear, and found the walrus all but dead, thus securing both animals. I should add that the bear threw the ice as if he was "left-pawed."

Kensington, February 10 J. RAE

WHILE spending the late winter months at Paignton, in Devon, I frequently watched, through a telescope, shore birds of various kinds stalking game on the low-tide sands. These abound with sand-eels, which lie, perfectly concealed, about an inch below the surface, and are caught in the following way by the gulls.

Standing close to the water's edge, the birds tread the wet sand into soft puddles by rapid alternate movements of their feet, and when a sand-eel, thus disturbed, makes a dart for the sea, he is instantly taken by a skilful but leisurely-looking snap of the beak.

Sand-eels bury themselves without leaving any marks on wet sand, and the gulls were always seen steadily and tentatively beating over the ground in the way I have described. They took, each, a fish a minute, perhaps, and impressed me with the idea that some thoughtful ancestral gull had deserved well of his race for the invention of such an easy logical way of picking up a living. D. PIDGEON

Holmwood, Putney Hill, February 7

The Sea-Serpent

ON reading the letter of W. Steadman Aldis in NATURE (vol. xxvii. p. 338) yesterday, I was reminded by a person present that some years ago, when in Orkney, I pointed out an appearance that most people unaccustomed to witness it might have taken for a great sea-monster. This was no thing more or less than some hundreds of cormorants or "scarps" flying in a continuous line close to the water, the deception being increased by the resemblance of a head caused by several "scarps" in a cluster *heading* the column, and by the "*lumpy*" seas of a swift tideway frequently intervening and hiding for an instant part of the black lines, causing the observer to—not unnaturally—imagine that the portions so hidden had gone under water. The speed of the cormorant on the wing may be fairly estimated at thirty miles an hour or more. J. RAE

Kensington, February 10

[1] It may be questioned how the bear could find a lump of detached ice. The strong current mentioned is constantly breaking up the ice into small pieces.

The "Zoological Record"

I SHOULD like to point out a slight error in the last impression of NATURE (p. 311). In your notice of the *Zoological Record*, 1881, it is stated that no separate paper seems to have appeared in 1881 exclusively devoted to the group *Octactiniæ*. I should mention that Prof. Nicholson's book on "Monticulipora," his paper on the skeleton of "Tubipora," and Mr. Wilson's paper on the development of "Renilla," all appeared in 1881, and were duly recorded by me. SYDNEY J. HICKSON

Anatomical Department, Museum, Oxford, February 5

SIEVE-TUBES

A CAREFUL examination by E. Russow (*Ann. Sci. Nat.* xiv. 1882, Nos. 3 and 4) of the structure and development of sieve-tubes leads him to the following general conclusions.

In all vascular plants examined, the sieve-tubes exhibit a remarkable agreement in structure, always expressed by the presence of callus. The sieve-punctation appears to be wanting in *Isoetes*, and possibly also in the Marattiaceæ. It is not, when present, confined to the sieve-tubes, but occurs also in the parenchyma of the secondary liber. It is often difficult to decide whether these punctations are actually perforated; but this is clearly the case wherever the sieve is traversed by callose cushions or striæ, or by connecting filaments; the presence of callus is not of itself sufficient to indicate perforation, for its formation certainly precedes the perforation of the membrane. In conifers the punctations between the sieve-tubes and the cells of the medullary rays are provided with callose cushions only on the side of the sieve-tubes, and the punctations remain closed.

The development, accumulation, and final disappearance of the callus indicate that it is not a product of transformation of the cellulose, but that it is separated from the contents of the sieve-tubes; its accumulation round the perforations is proportionate to the freedom and duration of the intercommunication that takes place through them; this communication probably continues as long as the striæ of the callus remain clearly developed, and ceases when these disappear, close up the sieve-pores, and end the function of the sieve-tubes.

In gymnosperms and vascular cryptogams, mucilaginous filaments are never to be seen traversing the callose cushions, although there is always a certain amount of communication between the elements of the sieve-tubes. The special function of the sieve-tubes is probably always maintained wherever striæ cross the callose cushions. The large number of plants in which the sieves are traversed, both in summer and winter, by mucilaginous filaments, and the large number in which no such filaments are at any time observable, contradicts the idea that the function of the callus is to close the sieve-pores during the dormant season.

Much less callus is deposited in the sieve-tubes of closed fibrovascular bundles, especially in permanent organs, than in those of open bundles which increase in thickness from the activity of their cambium. This difference corresponds to a difference in the nature of the contents, and in the duration of the activity of the sieve-tubes. While in gymnosperms and dicotyledons the active period of the sieve-tubes rarely exceeds two years, in monocotyledons and vascular cryptogams it lasts as long as the organ itself. A stem of *Alsophila*, at least

twenty years old, had all the sieve-tubes at its base still in a state of full activity. In a stem of *Yucca aloifolia*, about fifteen years old, the sieve-tubes of all the fibro-vascular bundles, even the innermost, were active, and had their sieves covered with callus; but this was no thicker in the oldest than in the youngest tubes. In a stem of *Dracæna draco*, at least twenty years old, the callus had nearly or entirely disappeared from many of the sieve-tubes; but the plant was otherwise in bad health.

The callus is not a reserve-substance; for in gymnosperms and dicotyledons it often remains unchanged for years in the dead sieve-tubes, and even in leaves which have fallen in the autumn, and in aërial branches which die in the winter. It behaves rather like a secretory product; and this view is confirmed by the study of its development. The organised structure which the callus sometimes exhibits is not a sufficient objection to this view.

Under which class of organic compounds the callus should be placed cannot at present be determined with certainty. Its behaviour to iodine-reagents and to aniline blue appears to indicate an alliance with proteinaceous substances, and especially with nuclein; in this respect it differs altogether from the solid carbohydrates, such as cellulose and starch.

All sieve-tubes resemble one another in their contents, at least as far as relates to the parietal protoplasm and water. The mucilage, which is undoubtedly a non-granular protoplasm, only exists in large quantities in dicotyledons; no mucilaginous threads can be detected in monocotyledons or vascular cryptogams; in some monocotyledons there is simply an accumulation of mucilage in the sieve-tubes. The sieve-tubes of these two classes contain, on the other hand, a large quantity of smaller or larger refringent globules, which are also proteinaceous. Similar globules have been observed in the closed vascular bundles of *Hippuris vulgaris*.

Although starch is almost always present in the sieve-tubes of open vascular bundles, it is seldom to be met with in those of closed bundles. The diameter of the starch-grains is always greater than that of the canals which are clothed with callus, which renders it impossible for them to pass from cell to cell as long as the sieve-tubes are in an active state. The reddish-violet or brick-red colour which these starch-grains take with iodine reagents indicates the presence of a diastase among the contents of the sieve-tubes.

A series of observations on the same organs by E. Janczewski (*Ann. Sci. Nat.* xiv. 1882, Parts 1 and 2) was directed mainly to a comparison of their structure in the different primary groups of the vegetable kingdom.

In vascular cryptogams the elements of the sieve-tubes are not much larger than those of the parenchymatous tissue. They have no nucleus, and contain proteinaceous globules, adhering to the parietal protoplasm, and collected below the pores. Both the lateral and terminal walls have a larger or smaller number of pores. The membrane of these pores is never perforated, and prevents the intercommunication of the contents of adjoining elements; it is sometimes (as in *Pteris aquilina*) pierced by callose cylinders. The time of year exercises no influence on the sieve-tubes, which remain in the same condition through the whole of their existence.

In gymnosperms the life of the sieve-tubes may be divided into two periods, *evolutive* and *passive*. During the first period the pores in the walls of the young tube produce callose substance, and are transformed into sieves covered and closed by the callus; the elements of the tubes contain, at this period, parietal protoplasm. During the second period the tubes entirely lose their protoplasm, and become inert; but at its very commencement the sieves also lose their callus, and free communication is established between adjacent elements.

In dicotyledons the structure of the tubes is still more complicated; their life may be divided into four periods: *evolutive*, *active*, *transitional*, and *passive*. During the first period the cambial cell is not transformed immediately into an element of the tube, as in gymnosperms; it divides longitudinally, and produces on one side an element of the tubes, on the other side one or two cells of the liber-parenchyma. In the elements thus separated, the pores of the walls, or the entire horizontal septa, become covered with callus, and perforated into true sieves composed of a delicate network of cellulose and a callose envelope. The tubes now enter the second or active period, characterised by the sieve-structure and the free intercommunication of the protoplasmic contents of adjacent elements. It may last for months or years. In some cases the sieves are closed before winter by a fresh formation of callus, and open again in the spring. During this period the tubes contain protoplasm, a larger or smaller quantity of a mucilaginous proteinaceous substance, and sometimes starch. During the transitional period the tubes gradually lose their contents; the sieves are closed by callus, and reopen again by the complete absorption of the callose substance. They have now entered the passive period; they are completely inert, and contain no organic matter; the sieves are reduced to a delicate network of cellulose.

The development and behaviour of the sieve-tubes of monocotyledons resemble that of dicotyledons, and their life may be divided into the same four periods. But from the fact of the vascular bundles being closed, and having no cambial zone capable of forming fresh tubes, the active period of the tubes may last as long as the life of the organ which contains them requires it. The passive period is, in fact, rarely manifested. In our climate the sieve-tubes have the power of closing their sieves in autumn, and reopening them in spring. The elements of the tubes contain no starch or mucilaginous substance; and their parietal protoplasm only contains proteinaceous particles which seem to disappear in the spring, and to add to the density and refrangibility of the protoplasm.

CASSELL'S NATURAL HISTORY[1]

WITH the sixth volume, this well-illustrated account of the natural history of the animal kingdom is brought to a close, and the six handsome volumes leave nothing to be desired, so far as good covers inclosing excellent paper and beautiful typography are concerned. Indeed, the general get-up of the series is quite unexceptional, and as to the average value of the scientific contents we feel fully justified, on the strength of such contributors as Parker, Sharpe, Carpenter, Dallas, Sollas, &c., in strongly recommending the series to the majority of our readers.

From a purely scientific point of view, we regret the title selected by the Editor. He should not have launched so important a book in these days upon the sea of science under an obviously wrong title. The "Historia naturalis" embraces, as the Professor of Geology in King's College, London, well knows, something more than an account of the members of one of nature's kingdoms, and of their distribution in space and time. It is therefore certainly not scientific, and we take it as against modern culture to adhere to such a style. If, indeed, the eminent firm of publishers were to extend this natural history so that in another half-dozen volumes we should have an account of the equally interesting, and even more important vegetable kingdom, the title of the series would the more approach exactness.

Although in the title of his work the Editor has followed in the footsteps of the mere compiler, he has by no means

[1] "Cassell's Natural History." Edited by P. Martin Duncan, M.D. Lond., F.R.S., Professor of Geology, King's College, London. Volumes 1 to 6, illustrated. Volume 6. (London, Paris, and New York: Cassell, Petter, Galpin, and Co., 1883.)

followed this example in the direction of writing on all the groups of the animal kingdom with his own pen, but has been fortunate in getting together a number of contributors, whose very names command respect for their contributions. The table of contents of the just published volume shows that the subjects of the Insects, Myriopods, and Arachnids have been written by Mr. W. S. Dallas, with the exception of the Lepidopterous Insects written about by Mr. W. F. Kirby, the Crustacea are described by Mr. Henry Woodward, the Echinoderms by Mr. Herbert Carpenter, the Sponges by Prof. Sollas, the Rhizopods by Prof. Rupert Jones, and the Worms, Zoophytes, and Infusoria by the Editor.

We have been greatly struck by the immense amount of information given to us by Mr. Kirby in Chapter IX., which treats of the characteristics of the order of Lepidoptera, gives an account of the evolution of these insects from the egg to the perfect state; describes the imago condition; gives a condensed but very clear account of their anatomy, food, and geographical distribution, and concludes with a few hints on collecting, killing, and setting. Among the statistics of lepidopterous life, we note that the present census gives about 10,000 species of butterflies, and 40,000 moths; but then Mr. Kirby adds: "Hundreds of new species are being added to our lists every year." The abundance of species in a district would seem to be in proportion to the variety of the vegetation, which latter is intimately connected with variety of elevation, and so it is "that Lepidoptera are far more numerous in Switzerland than in the peninsulas of Italy and Spain:" but is it not possible that the mountainous regions of Spain will still yield many as yet unknown forms? The illustrations in this portion of the volume are often very beautiful, and comparatively new. Of the next order, Diptera, says Mr. Dallas, "it is not easy to arrive at any trustworthy estimate of the total number of species, yet allowing Dr. Schiner's estimate of 9000 species as European, it has been calculated that the total fly population of the world would be from 150,000 to 160,000. Only a very few of this great army could be of course alluded to, but the information given about the gnats, midgets and crane flies is very full and interesting. Many of these forms are injurious to our crops, as well as irritating to ourselves. The Gall Midges (Cecidomyidæ) are among the most delicate species of all these gnat-like Diptera. The larvæ of these elegant little insects feed

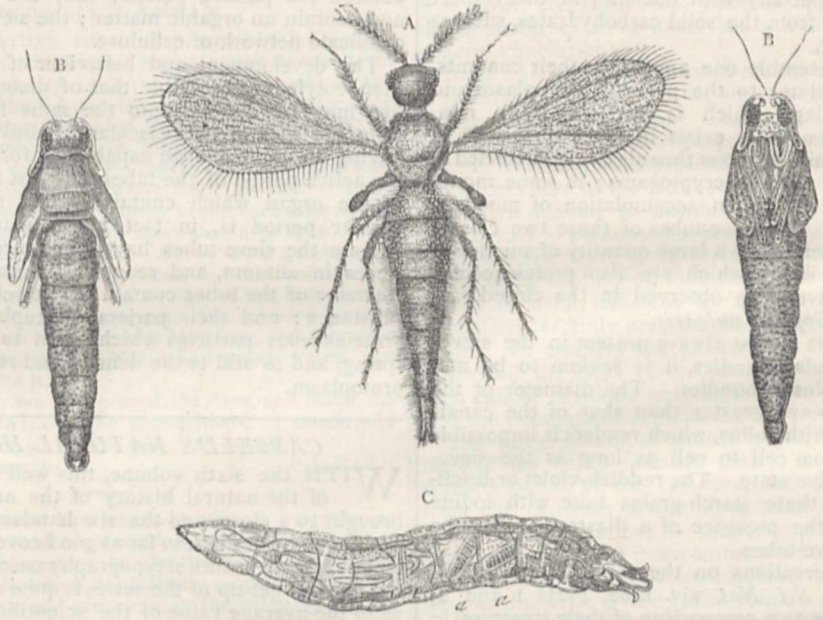


FIG. 1.—Cecidomyid with viviparous larva. A, adult insect; B, B, pupæ; C, larva, showing young larvæ at *a a*.

upon various species of plants. The number of species is very considerable, about 100 being recorded as European. Many of them by attacking useful plants, frequently do much mischief. Among these may be mentioned the Hessian Fly (*Cecidomyia destructor*), which has done so much damage to the grain crops of the United States, and which received its name from a belief that it was introduced into the States with the baggage brought by the Hessian troops in the pay of the British Government about the year 1776. The Wheat Midge (*C. tritici*) is an enemy of the wheat crops in this country, sometimes doing much damage; several other species form the flower-like galls oftentimes found on willows.

In 1860 Dr. Nicolas Wagner, of Kasan, made the startling discovery that in certain of these Cecidomyids the larval stages could give rise by a kind of budding, to several small larval-like forms, and that when these latter got free, they in their turn produced still other larval forms in the same curious fashion, and so one generation succeeds another throughout the autumn, winter, and spring. In the summer the last generation undergoes a change to the pupa state, and from these pupæ the perfect winged males and females emerge. The latter lay eggs in the bark of trees, and the larvæ produced from these commence once more a fresh series of organic broods. This strange circle of development is in part represented in the accompanying illustration, which will serve as a fair example of those which abound in this volume. All the families of the flies, ending with that of the flea, which, however, is placed in an order by itself, are well and judiciously treated.

The chapter on the Rynchota is also, despite its subject, a very interesting one, and a great deal of useful information is crowded into a small space. Mentioning the noise produced by the male Cicadæ, the author says: "During the heat of the day they sit concealed amongst the foliage of the trees and shrubs, and sing incessantly;" but is it not rather their wont to select the end of some dead twig, or the extremity of some vine pole, and there out in the full glare vibrate violently. A little space might have been spared for an account or figure of the vine phylloxera.

The chapter on the Orthoptera begins with the crickets and ends with the springtails. Among the Myriopods

we find the orders Pauropoda and Onychophora, the latter for Lansdowne Guilding genus Peripatus, which, by the way, he referred in his original description to the mollusca, and not, as here stated, to the worms.

The chapter on the Arachnida includes the Scorpions and their allies the Spiders, Mites, Tardigrades, and Pantopods; in the sketch of the latter a half page might usefully have been devoted to recent researches on the distribution of this extraordinary group of marine forms in the depths of the sea.

The class of Crustacea is well illustrated, and in the

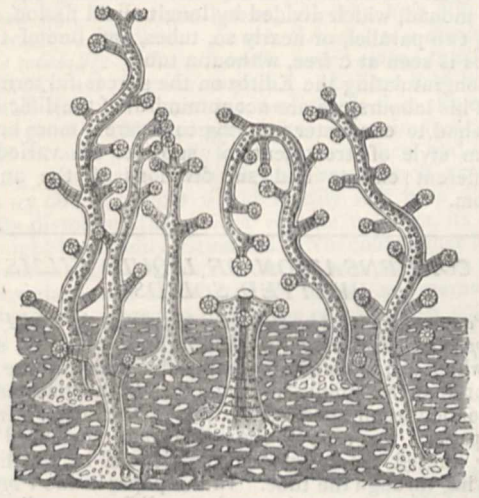


FIG. 2.—Millepora, showing expanded zooids.

introductory chapter we have an excellent account of the general anatomy and strange development of the group. Even Fritz Müller's account of the metamorphosis of Penæus is given, with figures of the Nauplius, Zoea, and Mysis stages. The typographical arrangements of the headings of the orders of the Crustacea seem faulty. The Editor's eye has failed him here, and though the sense is in no ways altered by the want of uniformity in the type used for the headings Brachyura (p. 197), Anomoura (p. 202), Macroura (204), yet there is a utility in the case of a classification of appealing to the eye. The

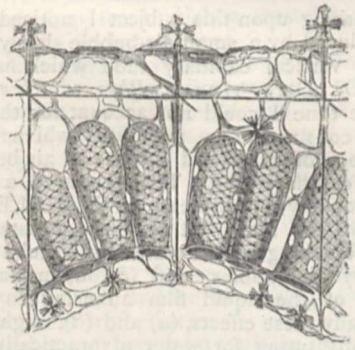


FIG. 3.—*Euplectella aspergillum*, structure.

King Crabs are placed as usual among the Crustacea, but the joint authors (Messrs. Dallas and Woodward), in their concluding remarks on the Arthropods write: "the structural relations of these to the scorpions would seem to be very close, and certainly raise a difficult problem, one which is rendered still more interesting by the fact that, according to the researches of Dr. Jules Barrois, a Limuloid, or King Crab-like stage occurs in the development within the egg of certain true spiders. For the present, this and many other such questions must, however, remain open. In all biological problems relating to the past developmental history of the organic world, we must for a long time yet expect to come continually upon obscure and puzzling points which only a more extended knowledge of minute details can clear up."

The various classes of the "grand division" of the worms are treated rather unevenly. This grand division is, no doubt, a somewhat heterogeneous one. "Thus it is found that an animal does not exactly correspond with one of the articulate groups, and another resembles in certain points, but not in all, an Infusorian. They are then placed with the Vermes [worms] because of the existence of certain fundamental structures." There is a good deal of minute anatomical detail given about the Leeches and Rotifers, while the Land Planarians are dismissed with the following:—"They have eyes, no tentacles, a proboscis, and a narrow body. They are found in the United Kingdom and generally in Western

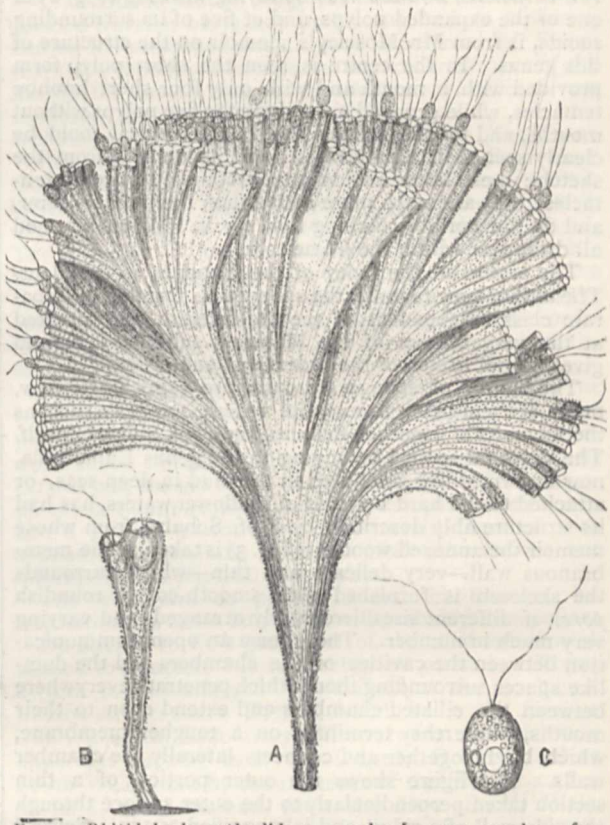


FIG. 4.—*Rhipidodendron splendidum*. A, colony; B, two monads; C, free monad.

and Central Europe. They have been found in America and on continental as well as on oceanic islands. Moseley says that they are nocturnal in their habits, when in the light getting under leaves. Some contain chlorophyll and seek the light, but die in the sunshine. They eat small snails, worms, and flies. An American kind secretes a mucous thread, and suspends itself in the water, and another lets itself down from the leaves by one."

If we were introduced to the Worms as a Grand Division, we are told that the Echinoderms form a Sub-Kingdom of the Animal Kingdom, but there is nothing to guide us to this in the heading of the portion of this volume in which Mr. Herbert Carpenter so well, though succinctly describes this important group, an account that we would have wished to have been much more detailed. The classes of this group getting about three pages of text to each, and several of the pages are devoted to new and excellent woodcuts.

The group of the Zoophyta embraces the Hydrozoa and

Actinozoa. These "are distinct from the Spongida, although some synthetic-minded morphologists classify all together as Cœlenterata." In treating of the freshwater Hydra we notice the "old story" repeated that "if the body be turned inside out, the old ectoderm [why the adjective?] takes on the digestive power and the former endoderm that [takes on the function] of the skin."

The order Hydrocorallina is placed as the last of the Hydrozoa, with the families Milleporidæ and Stylasteridæ, as indicated by Moseley, to whose researches and those of Agassiz we are indebted for all we know about the order. *Millepora alcicornis* was obtained by Moseley at Bermuda. The calcareous tissue of the coral is very hard and compact, and the polyps are extremely small. It is very difficult to prevail on the polyps to protrude themselves from their cells, but Mr. J. Murray, of the *Challenger* Expedition, succeeded in procuring them in this state on two occasions, and the accompanying drawing (Fig. 2) of one of the expanded polyps, and of five of its surrounding zooids, is from Mr. Moseley's memoir on the structure of this genus. In the centre is seen the short polyp form provided with a mouth and with only four short knobby tentacles, while grouped around are the five polyps without mouths, and for the sake of letting the central zooid be clearly seen a sixth mouthless zooid is omitted from the sketch; these latter zooids have from five to twenty tentacles; they are much more active than the mouth-bearer, and do the work of catching food for it. When alarmed all disappear within the framework.

The article on the group of the Sponges is excellent. The author now regards the sponges as forming a separate class independent of the Cœlenterata, and situated at the very bottom of the Metazoic sub-kingdom, and gives a brief sketch of the orders and sub-orders.

The figures of sponge structure are refreshingly new, many of them being from quite recent sources—such as the memoirs of Haeckel, Schulze, and Prof. Sollas himself. The beautiful sponge belonging to the genus Euplectella, now known to live anchored in the mud in deep seas, or attached to the hard bottoms of shallower waters, has had its structure ably described by Prof. Schulze, from whose memoir the annexed woodcut (Fig. 3) is taken. The membranous wall—very delicate and thin—which surrounds the skeleton is furnished with smooth-edged roundish *pores* of different sizes, irregularly arranged, and varying very much in number. These form an open communication between the cavities of the chambers and the duct-like spaces surrounding them, which penetrate everywhere between the ciliated chambers and extend even to their mouths, where they terminate on a tougher membrane, which binds together and connects laterally the chamber walls. The figure shows the outer portion of a thin section taken perpendicularly to the outer surface through the side wall of a ridge, and is magnified × 150. Several of the ciliated chambers are seen.

Although the Rhizopods are described as standing "first in the scale of animal organisation," we find them treated of in a chapter before that relating to the Infusoria, and we are told in the same paragraph that "they have in a great degree the same simple constitution as several other kinds of animalcules which are grouped by naturalists as Protozoa." We venture to think that such a description will be apt to lead the general reader astray; nor was it quite fair of the Editor to allow the writer of the article on the Rhizopods to go somewhat out of his way in his forty-ninth paragraph to give a view of the organisation of the Sponges which will be apt to puzzle the reader who has perused the more accurate account of the sponge structure given by Prof. Sollas.

As an example of the beautiful illustrations of the Infusoria, which are for the most part taken from Saville Kent's excellently illustrated Manual of Infusoria, we give the woodcut of *Rhipidodendron splendidum*. There are few workers with the microscope who devote themselves to the study of the Infusoria but must be familiar with the stems of that group of animalcules, which gravitate about the well known *Anthophysa vegetans* of Müller; the attached colony stocks putting one in mind of some minute fucoid stem. Of this group the species figured after Stein is one of the most remarkable, originally described and most beautifully illustrated in Prof. Stein's great work. This freshwater form has apparently not yet been found in this country, but a nearly allied species, *R. Huxleyi*, has been met with in South Devon. The figure shows the compound colony stock at A, the quite young colony stock at B, which latter was built up by a single monad, which divided by longitudinal fission, producing two parallel, or nearly so, tubes, and one of these monads is seen at C free, without a tube.

In congratulating the Editor on the successful termination of his labours, we are not unmindful of the difficulties he has had to encounter in trying to secure a more or less uniform style of treatment of subjects so varied as the different classes and sub-divisions of the animal kingdom.

THE CONDENSATION OF LIQUID FILMS ON WETTED SOLIDS

IN *Poggendorff's Annalen* for 1877, and in the *Philosophical Magazine* for 1880, I have recorded some facts which are satisfactorily explicable only on the supposition that the liquid in contact with the glass undergoes condensation upon the surface of the latter. In the latter paper I was able to show that this condensed film visibly altered the resistance experienced by the liquid in flowing through the tube. In the paper in the *Poggendorff's Annalen* it was shown that a difference of potential was set up between the two ends of a capillary tube through which water was forced, and that the effect of leaving the water in contact with the tube was that this difference of potential rapidly diminished. No doubt this finds its explanation in the effect of the condensation of the liquid on the sides of the capillary tube, causing the friction of the water against the tube to become less and less, whilst the friction of the water upon the condensed water-film becomes progressively greater, as the latter adheres more strongly to the glass. Probably simple drying would suffice to restore to the tube the originally observed difference of potential between its ends.

Whilst working upon this subject I noticed the large E.M.F. produced by a small air-bubble slowly ascending through the vertical capillary tube which was full of water (see Dr. Dorn, *Ann. d. Phys. u. Chem.* 1880, S. 73). At the time I could not account for this, but not long ago I constructed an apparatus which allowed of alternate drops of water and bubbles of air being driven through the capillary tube. This produced a very large E.M.F. Probably this increase in the E.M.F. is dependent upon (α) the increased electrical resistance consequent upon breaking up the water in the tube into drops separated by air-bubbles, and (β) upon an increased disturbance of the liquid film adhering to the glass. Experimentally these effects, (α) and (β), might be separated by substituting for water a (practically) perfectly insulating liquid.

Another and very interesting illustration of a liquid condensed on the surface of a solid is probably to be seen in the familiar fact that water will not clean a greasy sheet of glass.

As is well known to all workers on surface tension, almost the only way of getting a *physically* clean surface of glass is by heating the glass in concentrated sulphuric acid, to which a little nitric acid has been added, and then heating, after washing in pure water to remove the acid. Such a glass surface exposed to the air for a short time is generally imperfectly

wetted by water. This no doubt arises, partly at least, from the condensation of *gases* on the surface, which Quincke has shown will produce this effect to a remarkable extent under certain conditions described by him. To this, also, Barrett and Stoney have referred certain modifications of Leidenfrost's phenomena; and the floating shells, &c., of Hennessey are due to the same general cause. But a very minute trace of oil on a physically clean surface produces the familiar greasy surface. Why is this? Oil is not insoluble in water, and when the quantity of water used is sufficient to dissolve the quantity of oil placed on the glass, it ought to wash off. Every one knows, however, how difficult it is to wash oil off glass. *Is this then due to a diminution in the solubility of the oil in the water owing to its* CONDENSATION *on the glass surface?* I believe it to be very probable that this is the case, and think that the experimental proof would be possible by placing estimated quantities of oil on a physically clean glass surface, and subsequently washing in quantities of water, such as under ordinary circumstances would readily suffice to dissolve it. By dissolving the oil in a volatile medium, its quantity might be readily estimated. No doubt other liquids of somewhat greater and better known solubility might be advantageously substituted for the oil, and perhaps, as Dr. Japp has suggested to me, by employing a coloured liquid the result might be rendered evident to the eye.

My inability to complete these experiments at the present time, and the great interest attaching to a determination as to whether the condensation experienced by the liquid-film alters the physical or chemical properties of the liquid must be the excuse for the publication of incomplete results, which I much hope may be taken up by others.

J. W. CLARK

THE STOCKHOLM ETHNOGRAPHICAL EXHIBITION[1]

DR. STOLPE was asked to arrange and describe the Ethnographical Exhibition of Stockholm in the year 1878. This exhibition was brought together from all, or at least nearly all, Swedish public and private collections; no less than 217 exhibitors with about 10,000 objects participated, the King himself opened the galleries, and general interest was raised by an ethnographical exhibition as indeed no other country has realised till now. We took occasion to visit the exhibition, and were astonished to see so rich a material, as well as a thoroughly scientific arrangement.

Both works named below are a result of the meritorious undertaking. The second was partly a guide through some parts of the exhibition, especially China and Japan, with a general introduction, and many valuable and interesting special remarks, partly, in its second volume, a determination of all, about 6200 numbers of the exhibition, arranged after the exhibitors. The first-named work illustrates in geographical order the more important objects of the exhibition, partly in groups, but chiefly in single representations. There may be represented in all about 1500 objects, and we hear that a fourth supplementary volume is in the press.

The first volume of this album contains, on 84 plates, Australia, Oceania, Malaysia, Madagascar, Malayo-Chinese, and Tibet; the second, on 116 plates, China, Japan, Samoyedes, and Turks; the third, on 78 plates, America, Africa, Circassia, Persia, and India. Japan and China, as well as Oceania, are relatively best represented; among the last-named division figures the fine collection from the Savage Islands, which the expedition of the *Eugénie* brought home in the year 1853.

[1] H. Stolpe, "Exposition ethnographique de Stockholm, 1878-1879." Photographies par L. F. Lindberg. 3 vols. 4to. 36 pp. 278 plates. (Stockholm, 1881).—"Den allmänna etnografiska utställningen, 1878-1879" (The General Ethnographical Exhibition). 2 vols. 8vo. 80 pp. 1878-1879, and viii. 112 pp. 1880.

This photographic album must be regarded as the best existing ethnographical atlas; it gives, notwithstanding the inequality in the representation of the single countries, a good idea of a high-class ethnographical museum. The editor has had a full appreciation of the problem which was to be solved, and no ethnologist who works scientifically can do well without this album. It was therefore right that the International Geographical Congress of Venice in the year 1881 should bestow a prize on this beautiful work. Copies of the album are, we believe, only printed to order, and may be obtained direct from Herr Lindberg, R. Archæological Museum, Stockholm.

A. B. MEYER

BARON MIKLOUHO-MACLAY

LETTERS have been received from Baron N. de Miklouho-Maclay from the Suez Canal, the distinguished traveller being now on his way back to Australia. During his prolonged and arduous experience of eleven years' life amongst Melanesian and other savages of the Pacific his health has, we are sorry to say, suffered very seriously, and he returns to Sydney mainly on this account, since he finds that the climate of New South Wales suits him best. He intends to call at Batavia on the way out, where he left a part of his collections in 1878, in order to convey these to Sydney, where the main bulk of the gatherings of his many journeys is already stored. The Emperor of Russia, with enlightened liberality, has promised to defray the cost of the publication of the scientific account of Baron de Maclay's results, and the collections have been brought together at Sydney in order that they may be available for the preparation of the work for the press there.

Baron de Maclay hopes to be able to get ready the whole of his numerous diaries, notes, and papers for publication in about two years' time. The complete work to be issued by him will, if his present plan be carried out, consist of an anthropological and ethnographical section, a section treating of comparative anatomy, and a general narrative of his travels, together with appendices containing meteorological observations and information on physical geography.

The work will be published first in Russian, but translations in other languages will probably soon follow.

He intends to do a good deal of the anatomical work needed to complete his researches on animals collected by him in Australia and New Guinea at the Zoological Station at Watson's Bay, of which he is the founder. This Zoological Laboratory at the very first received most important support from the Linnean Society of New South Wales, and by the influence of this Society a grant of land was obtained from the New South Wales Government for the erection of the building. Scientific men in other colonies, and notably in Victoria, recognising the great importance of the establishment to the progress of biological research, have come forward nobly to support the enterprise, and the Australian Biological Association has been formed, a Society including men of science of all the Australian colonies and some distinguished European naturalists, the object being to support biological stations in Australia. It is very gratifying to find so enlightened a sympathy with scientific progress developed, and that the different colonies are able to work together in so excellent a cause. We hope to refer shortly again to the constitution and aims of the Australian Biological Association.

NOTES

WE can only for the present express the deep regret with which we learn of the death, on the 9th inst., of Prof. H. J. S. Smith, of the Savilian Chair of Geometry at Oxford, at the

comparatively early age of fifty-six years. We hope next week to refer to his career and work in detail.

THE death is announced at Basle of Prof. Peter Merian, the Nestor of Swiss geologists. He was born on December 20, 1795. His first important work, on the Jura of the Canton of Basle, was published in 1821, followed a few years later by a geological account of the Southern Schwarzwald. In 1821 he was appointed Professor of Physics and Chemistry in his native University, and at a later period he accepted the Chair of Mineralogy and Geology, which he held for half a century. He was more than once chosen rector of his University, and throughout his life not only continued his geological activity, but took an active interest in scientific work of all kinds as well as in public affairs.

WE learn that Dr. Oscar Dickson arrived in Christiana on the 9th inst. to confer with King Oscar, who is sojourning there, as to an Arctic expedition to be despatched this year, under the command of Baron Nordenskjöld, to North Greenland.

M. JANSSEN, as leader of the French Eclipse Expedition, will embark on March 6 for Panama. He will cross the isthmus by rail, and the *Éclaireur* will be ready at Colon to take him to Sable Island, near Caroline Island in the Marquesas group.

MR. DEANS COWAN, well known for his explorations in Madagascar (see *Proc. R. Geogr. Soc.*, vol. iv. p. 521), has now fully settled to return there, in order to explore the southern part of the island. Of this district little is known, and it may be fully expected that Mr. Cowan will get valuable additions to our knowledge of the natural history of the country. Mr. Cowan calculates that his journey will occupy about two years. His plan is to begin at Ambahy on the south-east coast, and to proceed inland to the most southern point reached when he made his survey of the Bara-land, working southward amongst the Tausay and Taudroy people, thence westward towards the district of the Mahafaly tribe, and on to the River Onylahy. This will occupy one year. From the Onylahy the route will be nearly north through western Bara-land and the Sakalava country, ending at Mojanga. As these journeys will be amongst the aborigines, and even in different geological formations to that from which nearly all our Madagascar specimens are obtained, Mr. Cowan expects that the results will be of a most valuable character, and help to a solution of many interesting questions in regard to Madagascar.

COLONEL ÉMILE GAUTIER has been appointed Director of the Observatory of Geneva, in succession to the late Prof. E. Plantamour, who had filled the position from 1839.

ON Tuesday, February 6, a large number of scientific men and social and political notabilities assembled at the Northern Station, Paris, to witness the transmission of energy by an iron wire used like an ordinary telegraphic line, and extending to Sevran, near Le Bourget, and returning to the station, thus completing a distance of 20 kilometres. The primary engine was moved by a force of about 5 horse-power, and the force of the secondary was said to be 2½. No precise measure was taken. The experiment was in continuation of the much spoken of Munich transmission of energy from Mierbach, according to the Marcel-Deprez system. It is not believed that this new experiment will put an end to the controversy. Many papers have reported enthusiastically on the proceedings, and letters have been written by electricians claiming to have executed more successful trials. It is stated that new experiments on the Marcel-Deprez system of transmission of energy will take place under the superintendence of M. Tresca, with the same machines as on February 6. The *Lumière Électrique* states that the percentage of force is about 37½ per cent., and that the dynamometer for measuring the motive power of the primary machine was not in order at the time of the first experiment.

PROF. FLOWER will commence, at the Royal College of Surgeons, on Monday, the 26th inst., a course of nine lectures upon the Anatomy of the Horse and its allies. In the first three lectures the general position of the horse in the animal kingdom, and its relations to other existing and extinct species will be treated of; the remainder of the course being devoted to a more detailed account of the osseous, dental, muscular, nervous, and other systems, as compared with those of the generalised Mammalian type, the allied forms of Ungulates, and Man. The lectures will be given on Mondays, Wednesdays, and Fridays, and are free to all who take an interest in the subject.

IT seems that the season of 1882 has, on account of the state of the ice in the Arctic seas, undoubtedly been one of the most adverse on record. Thus while the Norwegian walrus and white fish hunters were unable to get to the north of Spitzbergen and the Swedish Meteorological Expedition to Mossel Bay, no vessel succeeded in reaching the Siberian rivers. It appears from information just to hand that the summer along the coast of Siberia has been unusually cold, while incessant north-east winds have accumulated drift-ice on the shores to such an extent that the estuaries of the Yenissei and the Obi were not once navigable in the season. Thus the small steamer *Dallmann*, of Yenisseisk, belonging to Baron Knop, was quite unable to get from the Yenissei into the Obi, and all she accomplished during the year was to transfer a few thousand poods of grain from Mr. Sibiriakoff's depôt, where it had been lying for some years, to Baron Knop's, to be eventually forwarded to Europe. When it is remembered that this was the state of the ice in the eastern and southern parts of the Arctic seas, and we remember the reports of Mr. Leigh Smith and Sir Henry Gore Booth of open water north and east of Novaya Zemblya, it becomes apparent that some other part of the Polar basin must have been very free from ice during the summer. It seems to be the opinion of several authorities, as for instance Baron Nordenskjöld, that any vessel which had attempted to penetrate by way of Behring Strait would, no doubt, have demonstrated the practicability of navigating the Siberian seas every summer from one end or the other. This year fresh attempts will be made by Mr. Sibiriakoff, Baron Knop, and Dr. Oscar Dickson to open up a trade route with Siberia from Europe; those however acquainted with the Arctic seasons would not be surprised to see the ice in the summer of 1883 as adverse to Arctic voyaging as it was in 1882.

THE first news has been received at St. Petersburg from the Russian Lena Expedition. Lieut. Harder, who was searching for the remains of the victims of the *Jeannette* disaster, met Dr. Bunge and Jürgens on October 3. He found the members of the Lena Expedition in excellent health, and already comfortably settled in their winter quarters.

M. WOLF, chief of the Physical Department in the Paris Observatory, delivered last Saturday evening a lecture at the Sorbonne, on the Methods employed in Astronomical Physics, before a very large and enthusiastic audience. M. Wolf insisted upon the three methods employed by astronomers, viz. ocular inspection with telescopes, spectroscopic analysis, and photography. He dwelt upon the difficulties of vision with instruments possessed of a great magnifying power, and he tried to oppose the popular delusion that any description of celestial phenomena could be photographed with advantage. He explained that this method should be almost exclusively confined to the sun and moon. The lecture was illustrated by many experiments and projections.

AN important advance is in course of realisation in the use of telegraphy for French newspapers. The *Reforme* has hired a

direct cable from London to Paris. The instruments are in the London and in the Paris office of the paper, so that the transmission is instantaneous. According to circumstances, the *Reforme* telegraphists use the Calais, Boulogne, or Dieppe cables. None of these gives a sensible retardation through crossing the sea; but it is remarked that, contrary to expectation, the Dieppe cable is the best of the three. The transmission is made with an ordinary Hughes apparatus.

THE following are the subjects of the lectures to be given at the Royal Institution by Prof. Robert S. Ball, the Royal Astronomer of Ireland, on the Supreme Discoveries in Astronomy:—"The Scale on which the Universe is Built," "The Sun no more than a Star, the Stars no less than Suns," "The Law of Gravitation," and "The Astronomical Significance of Heat." The first lecture will be given on Tuesday, February 20.

PROF. JOHNSTRUP, Rector of the University of Copenhagen, in a paper on "The Glacial Phenomena manifested in Denmark," has shown that the Cyprina-mud deposits overlying the gravel in many parts of the Danish territories afford evidence that an interval of lesser cold must have followed the great glacial period. He moreover regards the presence of the shells of *Cyprina islandia*, and other boreal forms of similar habit, as a proof that the climate in this intermediate period must have been similar to that of the North Sea and the Cattegate in the present day. His views of the connection between these Cyprina deposits and the varied manifestations of glacial action are based on the hypothesis that the ice, which advanced from the interior of Scandinavia and covered Denmark and Northern Germany, must have been driven back, and that on its disappearance, the Cyprina mud was deposited in horizontal layers. On the recurrence of another glacial period these deposits were crushed, dislocated, and often thrust into a vertical position by drifting ice-fields, which had ploughed and broken up the sea-bottom in their advance. This view is in opposition to the opinion more generally held by geologists, that Denmark was twice completely buried under one connected ice-pall, which owed its origin to the continental ice of the Scandinavian peninsula. The direction of the striations and scouring lines in the island of Bornholm, and in some parts of Iceland, which are now being carefully investigated, are, however, admitted to be favourable to the views advanced by Prof. Johnstrup.

MR. GEORGE STALLARD, B.A., of Keble College, Oxford, at present Science Master of St. Paul's School, has been appointed Science Master at Rugby, in place of the Rev. C. M. Hutchinson.

THE Council of the Meteorological Society have determined upon holding at the Institution of Civil Engineers, 25, Great George Street, S.W., on the evening of March 21 next, an Exhibition of Meteorological Instruments which have been designed for, or used by, travellers and explorers. The Exhibition Committee invite co-operation, as they are anxious to obtain as large a collection as possible of such instruments. The Committee will also be glad to show any new meteorological apparatus invented or first constructed since last March, as well as photographs and drawings possessing meteorological interest.

WITH reference to the recent explosion of a zinc-plate oxygen gasometer, described by Herr Pfaundler in *Wiedemann's Annalen*, Dr. Loewe states (*Wied. Ann.* No. 1) that to protect oxygen or atmospheric air from admixture of carbonic acid or acid vapour from the air of the laboratory, he has for many years placed them over lime-water. Some 20 to 30 gr. freshly slacked lime, in a powdered state, is placed in a strong linen bag, which is tied with cord just above the contents, and hung near the outflow tube of the water vessel of the gasometer. This ensures that all carbonic acid and acid vapours which the water of the gasometer may in time absorb from the air, are neutralised by lime-hydrate, and rendered innocuous. There is the further advantage, for elementary analysis, that the potash or soda lye, which is preferred for washing the gases, remains long quite caustic, and thus serves—as it ought to do—less for purification of the gas than as an indicator of the gas current. After long use the linen bag of lime-powder is renewed.

THE February part of Hartleben's *Geographische Rundschen* contains an interesting account of Potanin's journey through Mongolia in the years 1876–77; also some well-written articles on the Samoa Islands, on Eastern Africa, and the European census of 1881, together with a memoir of Count Hans Wilczek.

SOME time ago we announced the commencement of the publication of an "Elektro-technische Bibliothek," by Hartleben, of Vienna. The second volume of this series has just appeared, entitled "Die elektrische Kraftübertragung und ihre Anwendung in der Praxis," by Eduard Japing.

WE are requested by the Council of the Society of Telegraph Engineers and of Electricians to state that an International Electrical Exhibition will be held in Vienna under the patronage of the Austro-Hungarian Government, in the months of August, September, and October next. At the request of the Austrian Minister of Commerce, and of the Managing Committee of the Exhibition, the Council of the Society have appointed a Committee for the purpose of receiving applications for space from intending British exhibitors, and for promoting generally the formation of a British section. Application should be made as early as possible, and should be addressed to the Secretary of the Society of Telegraph Engineers and of Electricians, 4, The Sanctuary, Westminster, S.W.

THE Municipal Council of Paris is proposing to the administration to organise a medical service for the inspection of the eyes, ears, and teeth of the pupils of the public schools, in order to see how to cure constitutional or chronic diseases by which they may be affected.

A LOCAL committee has been established in Annonay for the erection of a statue to commemorate the invention of the *Montgolfier* balloon.

UPWARDS of one hundred Palæolithic implements from the collection of Mr. Worthington G. Smith have been transferred to the collection of Mr. John Evans at Nash Mills, Hemel Hempstead.

A "SECOND London edition" has been issued by Macmillan and Co. of Prof. Newcomb's "Popular Astronomy." The principal additions relate to Dr. Draper's investigation on the existence of oxygen in the sun; Janssen's conclusions from his solar photographs; Prof. Langley's investigation on the solar spectrum on Mount Whitney, California; the satellites of Mars; the supposed intra-Mercurial planet; preliminary results from the late (1874) transit of Venus, and other recent methods of determining the solar parallax; the transit of Venus on Dec. 6, 1882; the great telescopes completed within the last four years; and recent developments in cometary astronomy. The preface is dated Washington, July, 1882.

A DEPOSIT of remains of mammals from the diluvial period has been laid bare by the waves of the Wolga on the bank of that river between Zarizyn and Sarepta. M. Al. Knobloch, of Sarepta, has made a valuable collection of the bones found, which belonged to *Elephas primigenius*, *Bos priscus*, *Elasmotherium*, *Camelus Knoblochi*, besides several antelopes, stags, &c.

On the evening of January 24 an aurora was observed at Geestemünde, which was remarkable both for its duration as well as for the intensity of its light. The sky was quite clear and the moon shining brightly, when about 7.30 p.m. a semicircle of light appeared in the north-east. Soon afterwards long rays shot out from this across the sky, forming an immense fan of light; the middle one of these rays crossed the sky right down to the south-west, and remained visible in the same brightness for two hours. The size and brightness of the other rays changed constantly. The light was perfectly white.

A violent earthquake is reported from Freiburg-im-Breisgau January 24, at 5.30 a.m., accompanied by loud subterranean noise. At the same time two strong shocks were felt at Bischoffingen. On the same date, at 7.58 a.m., an earthquake was observed in Herzegowina. It lasted for four seconds, and its direction was from north to south.

During the coming summer a Fine Art and Industrial Exhibition will be held at Huddersfield in connection with the opening of the New Technical School.

The additions to the Zoological Society's Gardens during the past week include two Macaque Monkeys (*Macacus cynomolgus*) from India, presented respectively by Mr. T. W. Davidson and Miss M. Sutton; two Common Marmosets (*Hapale jacchus*) from Brazil, presented by Mr. A. Pariss; an Oak Dormouse (*Myoxus dryas*) from Russia, presented by M. A. Wrzesniowski; a Common Marmoset (*Hapale jacchus*) from Brazil, presented by Mrs. Lynch; two Common Gulls (*Larus canus*), British, presented by Mr. W. K. Stanley; two Herring Gulls (*Larus argentatus*), British, presented by Capt. C. R. Suckley; a Brant Goose (*Bernicla brenta*), European, presented by Mr. J. C. Robinson; a Black Lemur (*Lemur macaco*) from Madagascar, four Impeyan Pheasants (*Lophophorus impeyanus* ♂ ♀ ♀ ♀) from the Himalayas, a Black-necked Swan (*Cygnus nigricollis*) from Antarctic America, deposited; two Philantomba Antelopes (*Cephalophus maxwelli*), a Crowned Hawk Eagle (*Spizaetus coronatus*) from West Africa, four Snow Buntings (*Flectrophanes nivalis*), two Brant Geese (*Bernicla brenta*), European, a Red-throated Diver (*Colymbus septentrionalis*), British, purchased; a Schomburgk's Deer (*Cervus schomburgki*), from Siam, received in exchange; two Hybrid Peccaries (between *Dicotyles labiatus* ♂ and *D. tajaçu* ♀), five Ring-hals Snakes (*Sepedon hæmachates*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

The Comet of 1771.—The comet discovered by Messier at Paris on April 1, 1771, and last observed by St. Jacques de Silvabelle at Marseilles on July 17, has long been mentioned in our treatises on Astronomy as undoubtedly moving in a hyperbolic orbit. This inference was first drawn by Burckhardt, who considered that of all the comets calculated up to the time he wrote (*Mémoires présentés par Savans étrangers*, 1805) that of 1771 was the only one of which it could be stated with some degree of certainty that the orbit was hyperbolic. Encke considered the case worthy of further investigation; remarking that from the nature of the conditions it might be demonstrated that a comet could not rigorously describe a parabola, and that experience so far rather gave the preference to the ellipse over the hyperbola, he insisted that a comet, whose track could not be represented completely except by hyperbolic motion, merited the greatest attention. He accordingly reduced anew the six observations employed by Burckhardt, and after their careful discussion found that the most probable elements were hyperbolic with eccentricity = 1·00937, which is almost identical with Burckhardt's value (1·00944). Nevertheless he did not regard the decided superiority of the hyperbola in the representation of the six places as an indubitable proof of the necessity of admitting motion in that curve; the positions used were not normal positions, but the results of single and isolated observations, and as such, the errors exhibited by a parabolic orbit had not so great a preponderance in his opinion as to enforce such necessity. He concluded that the subject still required examination by a combination of all the observations, and especially if the originals of those at Marseilles could be found. On this point Zach stated, in a note to Encke's communication (*Correspondance Astronomique*, t. v.), that during a recent visit to Marseilles he had searched in vain amongst the papers of St. Jacques de Silvabelle for these originals.

Lately, the orbit of the comet of 1771 has formed the subject of two memoirs, containing very rigorous discussions of the observations, the first by Mr. W. Beebe, in the *Transactions* of the Connecticut Academy of Arts and Sciences, vol. v.; the second by Dr. H. Kreutz, published in the *Proceedings* of the Vienna Academy. Mr. Beebe gives also a hyperbolic orbit, accompanied by the most probable parabola for comparison. The investigation by Dr. Kreutz, a very complete one, gives perhaps a more definite result. He is led to a parabolic orbit for the closest representation of the comet's path, and though the original observations at Marseilles had again been sought for unsuccessfully, he does not think their recovery would affect the conclusion at which he had arrived. The elements of the definitive parabola are as follow:—

Perihelion passage, 1771, April 19·14144 M.T. at Paris.

Longitude of perihelion ...	104° 1′ 21″·7	M. Eq. 1771·0
,, ,, ascending node	27 53 11·7	
Inclination	11 15 53·1	

Logarithm of perihelion distance, 9·955127

Motion—direct.

The Cassini Division of Saturn's Ring.—At the January meeting of the Royal Astronomical Society, Prof. J. C. Adams made a very interesting communication on William Ball's observations of Saturn, upon which much confusion and misapprehension have existed. Attention has been directed to the subject lately by several astronomical contemporaries, mainly with the view to show that William Ball was not, as he has been considered, the discoverer of the chief division of Saturn's ring. Prof. Adams has carefully examined letters from Ball preserved in the *Archives* of the Royal Society, Huyghen's *Opera Varia*, &c., and remarks: "I find no evidence that Ball, any more than Huyghens, had noticed any indication of a division in the ring." This statement may be accepted as conclusive that the impression of several English writers as to Ball's claim to the discovery of a double ring is a mistaken one, and the credit of the discovery rests with Cassini. The announcement of it made by the French astronomer to the Academy of Sciences is in the following terms:—"Après la sortie de Saturne hors des rayons du soleil l'an 1675 dans le crépuscule du matin, le globe de cette planète parut avec une bande obscure semblable à celles de Jupiter, étendue selon la longueur de l'anneau d'orient en occident, comme elle se voit presque toujours par la lunette de 34 pieds, et la largeur de l'anneau étoit divisée par une ligne obscure en deux parties égales, dont l'intérieure et plus proche du globe étoit fort claire, et l'intérieure un peu obscure. Il y avoit entre les couleurs de ces deux parties, à-peu-près la même différence qui est entre l'argent mat et l'argent bruni (ce qui n'avoit jamais été observé auparavant), et ce qui s'est depuis vû toujours par la même lunette, mais plus clairement dans la crépuscule et à la clarte de la lune que dans une nuit plus obscure. Cette apparence donna une idée comme d'un anneau double, dont l'inférieur plus large et plus obscur fût chargé d'un plus étroit et plus clair." In two figures attached to this announcement the ring is shown with the outer half shaded and the inner half white, and there is a central band across the globe.

ON THE CHEMICAL CORROSION OF CATHODES[1]

THIS paper contains a description of the influence of various circumstances upon the chemical corrosion of metallic cathodes in different liquids.

Several preliminary experiments are described by means of which it was found that in some cases the chemical corrosion of a metal is increased, and in others decreased, by making the metal a cathode. Also, that the loss of weight of a cathode in an electrolyte is dependent upon several conditions, such as difference of metal, of liquid, or of strength of liquid, some of

[1] By G. Gore, LL.D., F.R.S. Abstract of paper read before the Birmingham Philosophical Society, December 14, 1882.

which tend to increase, and others to decrease the corrosion. In a solution of potassic cyanide pure silver is always protected by being made a cathode. The influence of variations of strength of acid was tried in several cases.

The results, which at first were apparently contradictory, were found to depend upon a number of conditions, and it would require an extensive research to determine the limits of those conditions, and what the proportions are, in which all those separate influences participate in producing the effect. Unequal capillary action is one of them, and its effect is described in a separate paper entitled, "The Electrolytic Balance of Chemical Corrosion." Another is unequal corrodibility of the metal itself. This was investigated, but how it arose was not clearly ascertained. Traces of certain kinds of soluble impurity in the liquid was also a disturbing circumstance. The altered chemical composition of the liquid around the cathode, caused by substances set free or formed by electrolysis, was another influence; this was investigated in the case of a silver cathode in a solution of potassic cyanide, and the protective influence of the current upon the cathode was found to be partly due to the formation of potassic hydrate; the current, however, operates also in some other manner. The effect of temperature was also examined, and it was found that the current exercised a greater protective power when the liquid was hot than when it was cold; the corrosive effect without a current was also greatest (as might have been anticipated) in the hottest liquid. The effects were further influenced by the degree of strength of the current; the greatest strength of current exercised the most protective power, and a large number of experiments were made expressly to test the question whether difference of electro-motive force alone, independently of difference of strength of current, affected the rate of corrosion, but the difficulty of insuring perfect uniformity in all the other conditions which affected the corrosion was so great that sufficiently decisive results were not obtained.

THE MOVEMENTS OF AIR IN FISSURES AND THE BAROMETER

FROM time to time attention has been called to the property exhibited by certain wells in different parts of this country of maintaining an active and permanent circulation of air. It was observed that currents alternately entered or issued from fissures in the sides of the wells, and though in some cases the first emission on sinking the well consisted of choke-damp, the gas subsequently passing consisted of no more than atmospheric air. While it was clear that the currents were not due to the evolution of any gas by chemical action in the rock or the water, an explanation of the phenomenon was found in the fact that the changes in the direction of the circulation coincided precisely with the changes of movement of the barometer, the current being outwards with a falling glass, inwards when the barometer was rising, and ceasing altogether when no change in the atmospheric pressure was taking place. The strength of the currents moreover was found to be proportionate to the rapidity of the barometric movements.

The name of Blowing Wells has come to be applied to such wells in consequence of these properties. From their extreme sensitiveness to changes in the atmospheric pressure, they have been found to give useful indications of the approach of bad weather. Their warnings are rendered audible by fixing horns or whistles in an air-tight covering, in such a way as to sound readily to the outward current, or to give a different note for an outward or inward movement of the air.

The first blowing well of which we have an account appears to have been of an entirely artificial origin. A well was sunk at Whittingham, near Preston, to a depth of eighty feet, and being afterwards abandoned, was covered with a large flagstone pierced by a small hole. Currents of air were observed to enter or issue from this hole, according as the barometer was rising or falling, and a tin horn fixed in it became audible at a considerable distance. Similar phenomena were exhibited by a cesspool, intended to receive offensive residue from some chemical works. The pool was arched over, a small hole being left for the passage of the refuse; a fall in the barometer was made unpleasantly evident by the issue of offensive vapours.[1]

Subsequently it was noticed that three wells in the New Red Sandstone, in the neighbourhood of Northallerton exhibited the same peculiarity. The wells "blow" through fissures in the sandstone just above the water-level. The changes in the direction of the currents coincide precisely with the movements of the barometer, and the outward current is made to blow a "buzzer," which is said to be audible at a mile distance.[1] In the years 1879-80 a series of interesting experiments on one of these wells, situated near Solberge, three and a half miles south of Northallerton, was made by Mr. Thomas Fairley, F.R.S.E.[2] After stating that the water has a composition similar to that coming from chalk or limestone, and that, though on the first opening of the fissure a violent outburst of choke-damp had taken place, the gas subsequently issuing did not differ appreciably from common air, Mr. Fairley gives a detailed account of observations made on the volume of air passing. The currents passed through fissures in the sandstone at a depth of forty-five feet from the surface of the ground, and just above the level of the water. The measurements were made firstly by a vane-anemometer, and subsequently by two large dry meters, constructed to pass 3,000 cubic feet per hour; these had been substituted for two of the largest meters in the possession of the Leeds Corporation, which had been thrown out of gear by their incapacity to pass the air fast enough. As a result of these experiments it was found that a fall of the barometer of 0·26 inch was accompanied by an outflow of 83,900 cubic feet of air, and by an application of Boyle's law it was calculated the total capacity of the fissures must amount to nearly 10,000,000 cubic feet.

The existence of currents obeying the same laws is equally obvious in a well at Langton at a few miles distance. The well has been long disused, and the water is exceedingly foul, notwithstanding which a candle burns clearly at the bottom. A third instance occurs at Ornhams near Boroughbridge, where the roar of the air-currents passing into the crevices of the rock has been compared by a workman to that of the water in a mill-race. No observations, further than those necessary to prove the existence of the currents, have yet been made on these wells.

At Hopwas a well has been sunk for the supply of Tamworth to a depth of 168 feet, the water standing naturally a depth of 129 feet. The shaft passes through alternations of shale and sandstone, one of the beds of the latter, met with at a depth of ninety-six feet eight inches, being described as "light fissured sandstone thirty feet four inches."[3] From a fissure in this bed, at 115 feet from the surface, there issued a violent rush of atmospheric air, which soon spent itself, and was succeeded by currents showing variations coincident with the barometic changes. The currents have been noticed in one fissure only, an irregular opening, of two and a half inches in height by one inch in width, in a nearly close-sided vertical joint. Experiments on the amount of air traversing this fissure are now in progress.

The same properties are exhibited in an equally well-marked degree in a well belonging to Mr. A. Potts at Hoole Hall, near Chester. The well is eighty-one feet deep and contains ten feet nine inches of water; it is sunk through glacial deposits, consisting of a tough clay overlying a sand of variable thickness, into the New Red Sandstone, but, being an old well and lined with brick to the water-level, the exact nature of the strata and the position of the fissures is unknown. Communicating with the interior of the well by pipes, are two whistles of a different tone, and a pressure gauge; the deeper-toned whistle sounds to an inward, the shriller-toned to an outward current, and were they allowed to act freely during unsettled weather, these whistles would render sleep in the adjoining house impossible. It is stated by Mr. Potts that changes in the atmospheric pressure are shown more rapidly by the pressure gauge of the well than by a mercurial barometer, and that whenever there is a sudden change for rain, the water in the well becomes agitated and slightly discoloured. An appearance of ebullition was noticed also in the Solberge well, but has been attributed by Mr. Cameron to the falling of fragments of mortar. The movements of the water in the Hoole well are being made the subject of experiment by Mr. Potts. Similar, though less powerful, currents have been observed in two other wells within a distance of 500 yards of Hoole Hall. The wells are in a situation where a similar sequence of glacial deposits probably exists, but further particulars are at present wanting.

The fissures from which the currents in blowing wells issue occur usually near, but just above, the water-level. Above them there is provided an air-tight covering in the glacial clays,

[1] J. Rofe, F.G.S., *Geological Magazine*, vol. iv. p. 106, 1867.

[1] A. G. Cameron, *Geological Magazine*, vol. vii. p. 95, 1880.

[2] *Proc.* York Geol. and Polyt. Soc., N.S., vol. vii. p. 409, 1881.

[3] Mr. H. J. Marten, Eighth Report on the Circulation of Underground Waters to the British Association, 1882.

or in beds of shale interstratified with the sandstone, cutting off communication with the open air in this direction. Fissures traversing a dry sandstone in such a situation constitute an air-chamber which may clearly be of great capacity. On cutting one of a system of connected fissures, the first effect is frequently to liberate a quantity of pent-up air or choke-damp, as at Solberge; subsequently the opening becomes the sole channel by which equilibrium is preserved between the enclosed air and the atmosphere. It would however seem as probable that the opening should occur below the water-level as above it. In such a case the first effect of an expansion of the pent-up gases would be to force out water, and raise the level of the water in the well. The agitation of the water noticed in the well at Chester is probably due to the openings being partly above and partly just below the surface of the water. That they not infrequently are wholly below appears probable from observations on springs and wells, for it has been noticed that in certain chalk-springs there is an increase in the amount of water flowing when there is a rapid fall in the barometer, though no rain may have fallen, and that under the same circumstances water recommences to flow from land-drains and percolation gauges. The gaugings of deep wells in the chalk have confirmed these observations and show that there is a rise in the water-level under a decrease of atmospheric pressure. These movements have been attributed to the expansion of the dissolved gases.[1] It is probable that the gases when given off by the water, rise into and occupy cavities from which there is no escape upwards.

It is noticeable however that five certainly, and two probably, of the blowing wells described above derive their properties from fissures in the New Red Sandstone; no case is known in either chalk or limestone, though these are soluble rocks peculiarly liable to contain caverns or widened joints. It is not improbable that the fissures are too numerous in these rocks, so that wherever large hollows occur, there are also communications upwards with the open air. In sandstone on the other hand large hollows are of extremely rare occurrence, and in view of this difficulty it has been suggested that the Magnesian Limestone which underlies it about Northallerton, at a depth of about 400 feet, and is known to be extremely cavernous, may have given way in places, and led to the formation of hollows in the sandstone. This explanation however is not applicable to the wells at Tamworth or Chester, where the sandstone is not underlain by limestone. It seems more probable that the strength of the air-currents should be taken in connection with the copiousness of the water-supply as indicative of the great extent of small ramifying fissures in some of the triassic sandstones. That the united capacity of such fissures must be very great to account for the phenomena is undeniable. The volume of air contained in the cavities at the Solberge well was estimated at about 10,000,000 cubic feet, or as much as would fill a chamber measuring 217 feet each way, length, width, and height.[2] In making this estimate no allowance was made for aqueous vapour, or for air held in solution in the water, both of which would come off in increased quantities with a decreasing pressure. The former was known by the state of the meter to have been present in large quantities. But making every allowance for these causes of error, it is impossible to escape the conclusion that the fissures, small as they are individually, must in the aggregate form a reservoir of immense capacity.

In concluding these remarks we may refer to the practical application of the knowledge of these properties in fissures. It has been noticed that the drains of large works begin to smell on the approach of rain, and there can be little doubt that this is partly due to the setting up of an outward current corresponding to a fall in the barometer. In fact every network of covered drains, and every covered cess-pool, where special provision for ventilation is not made, must constitute a natural blowing well. It is not our intention to discuss here the engineering details of drainage. It is sufficient to point out that by a faulty system of ventilation, or by the derangement of a system originally good, sewer-gas might be forced into a house with every fall of the barometer.

Lastly we would allude to the effect of the barometer on the escape of fire-damp from coal-seams. Coal is a rock subject to jointing; seams are not only broken through and displaced by faults, but for some distance from the main fracture are traversed by joints and smaller shifts resulting from the general strain. A brief visit to a fiery portion of a mine is sufficient to show the part played by these small clefts. On every side is heard the monotonous hissing or bubbling of the escaping gas, often accompanied by the deeper note of a "blower," or one of those larger channels often observed in connection with faults. The gas is continously given off as a result of a slow decomposition taking place in the coal, and the amount that comes off indicates a great extent of connected fissuring. For though cavities charged with gas under pressure and liable to exhaustion are found, yet large "blowers" commonly continue active for years, and must therefore drain a large area of the seam. While the movement of the gas in the blower differs from that of the air in sandstone fissures, in being always in one direction, namely outwards, it is at the same time evident that the same cause which induces an outward current in the well would cause an increase in the outward current from the coal. The increase would be proportional to the capacity of the fissures; a fall in the barometer from thirty to twenty-nine inches for example would cause $\frac{1}{30}$th of the body of gas stored in the fissures to be added to the ordinary outflow. The liability to explosion with a falling glass has long been a subject of observation. When it is considered that a wide margin is usually allowed in the ventilation to ensure the sufficient dilution and removal of fire-damp, and that a number of other contingencies may bring about an explosion, it becomes evident that a powerful cause must be operating to make the influence of the barometric changes perceptible.

A. STRAHAN

[1] Baldwin Latham, Report of the British Association for 1881, p. 614.
[2] *Proc.* York Geol. and Polyt. Soc., *op. cit.*

SOCIETIES AND ACADEMIES

LONDON

Royal Society, January 25.—"Internal Reflections in the Eye," by H. Frank Newall, B.A.

The author in this paper describes the appearance and investigates the cause of a faint light seen under certain circumstances now to be related:—Stand opposite a uniformly dark wall in a darkened room. Direct the eye to any point in front, and keeping the eye fixed, and being ready to perceive any appearance out of the line of direct vision without moving the eyes towards it, hold up a candle at arm's length, and move it to and fro over about two inches on a level with the point fixed, and a little to the right or left of it. The faint light may be seen moving with a motion opposite to that of the candle on the other side of the point of direct vision.

Near inspection of the light shows it to be an inverted image of the candle, about equal in size, very faint.

Reasons related in the paper lead the author to offer the following explanation: the physical cause of the faint light or "ghost," is light which, proceeding outwards from the image of the candle, formed on the retina by the lens, is reflected back on to the retina by the anterior surface of the lens. This second image is "referred" outwards, and seems as if produced by a faint source of light outside the eye.

The effects of alterations of the state of accommodation on the appearance of the ghost are described; the question as to whether the retina is to be regarded as a screen or as a *regular* reflector is discussed; and the results of calculations based on numbers given by Helmholtz for his schematic eye are noted as forming a difficulty in the explanation.

If the candle be replaced by sunlight, further observations are to be made: (1) signs of the faulty centering of eye-surfaces, as shown by the fact that the sun and its ghost do not arrive at the centre of the field of vision together; (2) signs of oblique reflection at a concave mirror, as shown by the fact that the ghost is circular in only one state of accommodation, whilst in other states it is extended either in a horizontal direction for near focus, or in a vertical direction for distant focus.

To about four out of fifteen persons the author has failed to show the ghost; but no relation is as yet observed between the visibility of the ghost and the kind of sight of the observer, as defined by the ordinary terms, long- and short-sightedness.

A second "ghost," probably due to reflections entirely within the lens, is referred to in the paper: but this, on account of its indistinctness, has not been investigated, except to establish the fact that its motion is the same in direction as that of the candle in the circumstances above related.

February 1.—On the Electrical Resistance of Carbon Contacts, by Shelford Bidwell, M.A., LL.B.

The experiments described in the paper were undertaken with the object of investigating the changes of resistance occurring in carbon contacts under various conditions.

A short movable carbon rod was placed across and at right angles to a similar rod which was fixed in a horizontal position, and arrangements were made for varying and accurately measuring the pressure of the one upon the other, for varying and measuring the current passing through them, and for measuring the resistance at the points of contact. The following are the most important results arrived at:—

1. *Carbon Contacts.*—Changes of pressure produce proportionately greater changes of resistance with small pressures than with great pressures. Thus, when the pressure was increased from ·25 to ·5 grms., the resistance fell from 16·1 to 11·0 ohms., the difference being 5·1 ohms; whereas, when the pressure was increased from 25 to 50 grms., the resistance fell from 2·1 to 1·8 ohms., the difference being only ·3 ohms.

Changes of pressure produce proportionately greater changes of resistance with weak currents than with strong currents. Thus when the pressure was increased from ·25 to ·5 grm., the resistance fell from 9·27 to 8·45 ohms with a current of ·1 ampere; and from 25·50 to 17·75 ohms with a current of ·001 ampere.

Changes of current, the pressure remaining constant, produce greater changes of resistance with small currents than with large currents, and with light pressures than with heavy pressures.

When the resistance of a carbon contact has been reduced by an increase of pressure, it will, on the removal of the added pressure, rise to approximately its original value.

The passage of a current whose strength does not exceed a certain limit, depending upon the pressure, causes a permanent diminution in the resistance (so long, of course, as the contacts are undisturbed), and the stronger the current, the greater will be such diminution.

When the strength of the current exceeds a certain limit, the resistance is greatly and permanently increased (generally becoming infinite). The greater the pressure, the higher will be such limit.

Unless special means are adopted for maintaining a constant current, the fall in the resistance which attends increased pressure is greater than that which is due to increased pressure alone being partly due also to the increased current.

It is not proved that the diminished resistance which follows an increase of current is an effect of temperature.

2. *Metallic Contacts.*—For the sake of comparison, a few experiments were made with metals. The metal principally used was bismuth, which was selected on account of its high specific resistance, but experiments were also made with copper and platinum.

In the case of bismuth, and probably of other metals:—With a given pressure, the weaker the current the higher will be the resistance. This effect is most marked when the current is small. Thus, with a pressure of ·1 grm. the resistance, with a current of ·1 ampere, was 2 ohms; with ·01 ampere it was 16·92 ohms; and with a current of ·001 ampere it was 143·3 ohms. With a pressure of ·5 grm., the resistance with the same currents as before was 1·45, 1·47, and 3·8 ohms.

The passage of a current, even when very small, causes a permanent adhesion between metallic contacts. This effect had been previously observed by Mr. Stroh.

Increase in the current is accompanied by a fall of resistance, and if the current be again reduced to its original strength, the resulting change in the resistance will be small, and it will in no case return to its original value.

Diminution in the strength of the current is followed by a small fall in the resistance if the metal is clean, and by a small rise in the resistance if the metal is not clean.

Increased pressure produces a greater fall in the resistance with small pressures than with great pressures, and with weak currents than with strong currents.

The resistance, after having been reduced by increased pressure, does not return to its original value when the added pressure is removed.

3. *Reasons for the Superiority of Carbon over Metal in the Microphone.*—The above observations may perhaps furnish an answer to the question, Why does carbon give far better results than any metal when used in the microphone? The mere fact that a current causes delicately-adjusted metal contacts to adhere to each other seems sufficient to account for the superior efficiency of carbon. In addition to this phenomenon of adhesion, and probably connected with it, are the facts that metallic contacts, unlike those of carbon, do not even approximately recover their original resistance when once it has been reduced by increased pressure or increased current, unless indeed complete separation occurs; and even the initial effect of pressure upon resistance is in general much more marked with carbon than with metals.

Lastly, it is to be noticed that in the case of carbon, pressure and current act in consonance with each other: pressure diminishes the resistance, and in so doing, increases the strength of the current; and the current thus strengthened effects a further diminution in the resistance. In the case of metals, on the other hand (or at least in the case of clean bismuth) pressure and current tend to produce opposite effects. The resistance is diminished by pressure, and the current consequently strengthened; but by reason of the increased strength of current, the resistance is *higher* than it would have been if the current had remained unchanged. The effect of this antagonism is not very great, but it seems sufficient to give a material advantage to carbon.[1]

The paper contains fifteen tables, four curves, and three diagrams, illustrative of the apparatus used.

February 8.—"Note on Terrestrial Radiation." By John Tyndall, F.R.S.

On Hind Head, a fine moorland plateau about three miles from Haslemere, with an elevation of 900 feet above the sea, I have recently erected a small iron hut, which forms, not only a place of rest, but an extremely suitable station for meteorological observations. Here, since the beginning of last November, I have continued to record from time to time the temperature of the earth's surface as compared with that of the air above the surface. My object was to apply, if possible, the results which my experiments had established regarding the action of aqueous vapour upon radiant heat.

Two stout poles about 6 feet high were firmly fixed in the earth 8 feet asunder. From one pole to the other was stretched a string, from the centre of which the air thermometer was suspended. Its bulb was 4 feet above the earth. The surface thermometer was placed upon a layer of cotton wool, on a spot cleared of heather, which thickly covered the rest of the ground. The outlook from the thermometers was free and extensive; with the exception of the iron hut just referred to, there was no house near, the hut being about 50 yards distant from the thermometers.

On November 11, at 5.45 p.m., these were placed in position, and observed from time to time afterwards. Here are the results:—

6	p.m.	...	Air 36° Fahr.	...	Wool 26° Fahr.
8.10	,,	...	,, 36	...	,, 25
9.15	,,	...	,, 36	...	,, 25

air almost dead calm, sky clear, and stars shining.

November 12, the wind had veered to the east, and was rather strong. The thermometers, exposed at 5 p.m., yielded the following results:—

5.15	p.m.	...	Air 38°	...	Wool 33°
5·45	,,	...	,, 38	...	,, 34
6.45	,,	...	,, 38	...	,, 35
9	,,	...	,, 39	...	,, 36

During the first and last of these observations the sky was entirely overcast, during the other two a few stars were dimly visible.

On November 13, 25, and 26, observations were also made, but they presented nothing remarkable.

It was otherwise, however, on December 10. On the morning of that day the temperature was very low, snow a foot deep covered the heather, while there was a very light movement of the air from the north-east. Assuming aqueous vapour to play the part that I have ascribed to it, the conditions were exactly such as would entitle us on *a priori* grounds to expect a considerable waste of the earth's heat. At 8.5 a.m. the thermometers were placed in position, having left the hut at a common temperature of 35°. The cotton wool on which the surface thermometer was laid was of the same temperature. A single minute's exposure sufficed to establish a difference of 5° between the two thermometers. The following observations were then made:—

8.10	a.m.	...	Air 29°	...	Wool 16°
8.15	,,	...	,, 29	...	,, 12

Thus, in ten minutes, a difference of no less than 17° had established itself between the two thermometers.

Up to this time the sun was invisible: a dense dark cloud,

[1] In April, 1882, the author communicated this observation to Mr. Preece, who referred to it in a paper read at the Southampton meeting of the Brit. Assoc., on "Recent Progress in Telephony."

resting on the opposite ridge of Blackdown, virtually retarded his rising.

8.20 a.m.	...	Air 27°	...	Wool 12°
8.30 ,,	...	,, 26	...	,, 11
8.40 ,,	...	,, 26	...	,, 10
8.45 ,,	...	,, 27	...	,, 11
8.50 ,,	...	,, 29	...	,, 11

During the last two observations, the newly-risen sun shone upon the air thermometer. As the day advanced, the difference between air and wool became gradually less. From 18° at 8.50 a.m., it had sunk at 9.25 to 15°, at 9.50 to 13°, while at 10.25, the sun being unclouded at the time, the difference was 11°; the air at that hour being 31° and the wool 20°.

In the celebrated experiments of Patrick Wilson, the greatest difference observed between a surface of snow and the air 2 feet above the snow, was 16°; while the greatest difference noticed by Wells during his long-continued observations fell short of this amount. Had Wilson employed swansdown or cotton wool, and had he placed his thermometer 4 feet instead of 2 feet above the surface, his difference would probably have surpassed mine, for his temperatures were much lower than those observed by me. There is, however, considerable similarity in the conditions under which we operated. Snow in both cases was on the ground, and with him there was a light movement of the air from the east, while with me the motion was from the north-east. The great differences of temperature between earth and air, which both his observations and mine reveal, are due to a common cause, namely, the withdrawal of the check to terrestrial radiation which is imposed by the presence of aqueous vapour.

Let us now compare these results with others obtained at a time of extreme atmospheric serenity, when the air was almost a dead calm, and the sky without a cloud. At 3.30 p.m., January 16, the thermometers were placed in position, and observed afterwards with the following results:—

3.40 p.m.	...	Air 43°	...	Wool 37°
3.50 ,,	...	,, 42	...	,, 35
4 ,,	..	,, 41	...	,, 35
4.15 ,,	...	,, 40	...	,, 34
4.30 ,,	...	,, 38	...	,, 32
5 ,,	...	,, 37	...	,, 28
5.30 ,,	...	,, 37	...	,, 30
6 ,,	...	,, 36	...	,, 32

These observations, and especially the last of them, merit our attention. There was no visible impediment to terrestrial radiation. The sky was extremely clear, the moon was shining; Orion, the Pleiades, Charles's Wain, including the small companion star at the bend of the shaft, the north star, and many others, were clearly visible. On no previous occasion during these observations had I seen the firmament purer; and still, under these favourable conditions, the difference between air and wool at 6 p.m. was only 4°, or less than one-fourth of that observed on the morning of December 10.

We have here, I submit, a very striking illustration of the action of that invisible constituent of the atmosphere, to the influence of which I drew attention more than twenty-two years ago. On December 10 the wind was light from the north-east, with a low temperature. On January 16 it was very light from the south-west, with a higher temperature. The one was a dry air, the other was a humid air; the latter, therefore, though of great optical transparency, proved competent to arrest the invisible heat of the earth.

The variations in the temperatures of the wool recorded in the last column of figures are, moreover, not without a cause. The advance of temperature from 28° at 5 p.m. to 32° at 6 p.m., is not to be accounted for by any visible change in the atmosphere, or by any alteration in the motion of the air. The advance was due to the intrusion at 6 p.m. of an invisible screen between the earth and firmament.

As the night advanced the serenity of the air became, if possible, more perfect, and the observations were continued with the following results:—

6.30 p.m.	...	Air 36°	...	Wool 31°
7 ,,	...	,, 36	...	,, 28
7.30 ,,	...	,, 35½	...	,, 28
8 ,,	...	,, 35	...	,, 26
8.30 ,,	...	,, 34	...	,, 25
9 ,,	...	,, 35	...	,, 27
10 ,,	...	,, 35	...	,, 28
10.30 ,,	...	,, 35	...	,, 29

After this last observation, my notes contain the remark, "Atmosphere exquisitely clear. From zenith to horizon cloudless all round."

Here, again, the difference of 4° between the temperature of the wool at 8.30 p.m., and its temperature at 10.30 p.m., is not to be referred to any sensible change in the condition of the atmosphere.

The observations were continued on January 17, 23, 24, 25, and 30; but I will confine myself to the results obtained on the evening of the day last-mentioned. The thermometers were exposed at 6.45 p.m., and by aid of a lamp read off from time to time afterwards.

7.15 p.m.	...	Air 32°	...	Wool 26°
8 ,,	...	,, 31	...	,, 26
9.30 ,,	...	,, 31	...	,, 27

During these observations the atmosphere was very serene. There was no moon, but the firmament was powdered with stars. The serenity, however, had been preceded by heavy rain, which doubtless had left the atmosphere charged with aqueous vapour. The movement of the air was from the south-west and light. Here again, with an atmosphere at least as clear as that on December 10, the difference between air and wool did not amount to one-fourth of that observed on the latter occasion.

The results obtained on February 3 were corroborative. The thermometers were exposed at 6.15 p.m.

7.15 p.m.	...	Air 34°	...	Wool 28°
8.25 ,,	...	,, 34	...	,, 30

Here again, the difference between air and wool is only 4°, although the sky was cloudless, and the stars were bright. The movement of the air was from the south-west and light.

On the forenoon of this day there had been a heavy and persistent rain storm. Heavy rain and high wind also occurred on the night following. The serene interval during which the observations were made lay, therefore, between the two storms. Doubtless the gap was well filled with pure aqueous vapour.

Further observations were made in considerable numbers, but they need not here be dwelt upon, my object being to illustrate a principle rather than to add to the multitudinous records of meteorology. It will be sufficient to say that, with atmospheric conditions sensibly alike, the waste of heat from the earth varies from day to day; a result due to the action of a body which escapes the sense of vision. It is hardly necessary for me to repeat here my references to the observations of Leslie, Hennessey, and others, which revealed variations in the earth's emission for which the observers could not account. A close inspection of the observations of the late Principal Forbes on the Faulhorn proves, I think, that the action of aqueous vapour came there into play, and his detection of this action, while unacquainted with its cause, is in my opinion a cogent proof of the accuracy of his work as a meteorologist.

Postscript.—In the *Philosophical Transactions* for 1882, Part I. p. 348, I refer to certain experiments executed by Prof. Soret of Geneva. My friend has recently drawn my attention to a communication made by him to the French Association for the Advancement of Science in 1872. It gives me great pleasure to cite here the conclusions at which he has arrived.

"The influence of humidity is shown by the whole of the observations; and it may be stated generally that, other circumstances being equal, the greater the tension of aqueous vapour the less intense is the radiation.

"In winter, when the air is drier, the radiation is much more intense than in summer, for the same height of the sun above the horizon.

"On several occasions a more intense radiation has been observed in dry than in humid weather, although the atmosphere was incontestably purer and more transparent in the second case than in the first.

"The maximum intensity of radiation, particularly in the summer, corresponds habitually to days exceptionally cold and dry."

Such are the results of experiments, executed by a most excellent observer, on the radiation of the sun. They apply word for word to terrestrial radiation. They are, moreover, in complete harmony with the results published by General Strachey in the *Philosophical Magazine* for 1866. Meteorologists will not, I trust, be offended with me if I say that from such outsiders, fresh to the work and equipped with the neces-

sary physical knowledge, they may expect efficient aid towards introducing order and causality among their valuable observations.

Mathematical Society, February 8.—Prof. Henrici, F.R.S., president, in the chair.—Capt. P. A. MacMahon, R.A., was admitted into the Society.—The following communications were made:—On the Sylvester-Kempe quadruplane, by Mr. H. Hart.—On curves obtained by an extension of Maclaurin's method of constructing conics, by Mr. S. Roberts, F.R.S.—A generalisation of the "nine-point" properties of a triangle, by Capt. P. A. MacMahon.—On the use of certain differential operators in the theory of equations, by Mr. J. Hammond.—A method for reducing the differential expression $dt/\sqrt{\{t-\alpha, t-\beta, t-\gamma, t-\delta\}}$ to the standard form, by Mr. J. Griffiths. The "nine-point" property was the following:—If through the centre of the circle ABC, and the ortho-centre of the triangle ABC, lines be drawn making angles α and $\pi-\alpha$ with the sides of the triangle, twelve points will be obtained on the sides, and these lie six and six on two circles of radius $\frac{1}{2}R$ cosec α. Each circle also passes through six other points, and they are inscribed circles of the two three-cusped hypocycloids, which are the envelopes of the two tangents, equally inclined to the axis (at angles α), to a parabola inscribed in the triangle ABC. Of course, when $\alpha = \frac{\pi}{2}$, the circles become the ordinary nine-point circle of ABC.

Linnean Society, February 1.—Sir John Lubbock, Bart., F.R.S., president, in the chair.—Messrs. F. W. Burbidge and Joseph Johnson were elected Fellows of the Society.—Dr. W. C. Ondaatje called attention to examples of red coral from Ceylon.—Mr. W. T. Thiselton Dyer exhibited a model of the fruit of the Double Cocoa-nut (*Lodoicea Seychellarum*, Lab.), of an unusual form, obtained from Major-General C. G. Gordon, R.E.—A series of microscopic sections of coal-plants were shown on behalf of Mr. J. Norman.—The following paper was then read:—On the structure, development, and life-history of a tropical epiphyllous lichen, by H. Marshall Ward. The author's observations lead him to believe that the epiphyllous cryptogam in question supports the view that a lichen is a compound organism composed of an alga on which an ascomycetous fungus has become more or less intimately affixed and dependent. It is developed on the leaves of many plants, but it has been more closely watched on *Michelia furcata*. The lichen presents four types, orange-red stellate patches, greyish-green blotches, clear grey spots, and white shining circles, but these pass imperceptibly into one another, and vary in size from a speck to a quarter of an inch in diameter. The reddish spots of the earlier stages is an alga of which the radiating filaments are in part reproductive organs, and in part barren hairs. It subsequently passes into the grey and green stages, and by a modification of growth the invasion of a fungus mycelium succeeds. The white matrix of the complete lichen consists of the same algal thallus invested by dense masses of the fungus hyphæ, which produce shining black dots, viz. the fruit bodies. The author describes in detail the peculiarities of growth and reproduction of the alga and fungus, and formation of the lichen. He alludes to and criticises Dr. Cunningham's account of *Mycoidea parasitica*, which latter is evidently closely related to that described by himself. Assuming that Mycoidea and Ward's Alga are generically the same, either Cunningham discovered a female organ of reproduction which becomes fertilised and produces zoospores, or he confounded these with certain fertile hair organs described by Ward. As regards the systematic position of the alga, a comparison with *Coleochæta* suggests that there is very little in common beyond mode of growth of the disc-like thallus, and the production of zoospores from certain cells. The genus *Chroolepus*, moreover, presents features which agree in several important points, viz., orange-red oily-cell contents, habitat, production of zoospores in ovoid cells developed terminally and laterally. The structure of the thallus, and relative positions of the main masses of fungal and algal portions, agree with what occurs in heteromerous crustaceous lichens, as Graphidea; but the perithecia indicate an angiocarpous alliance, bringing this form nearer such families as Pertusaria and Verrucaria, to the latter of which it may ultimately be referred.—A paper was read by F. Maule Campbell, on the pairing of *Tegenaria Guyonii*, and description of certain organs in the male abdominal sexual region. Two cases are related in which during confinement the males killed the females after union, and an instance is also given of an attempt to impregnate an immature female which was also destroyed by the male. In neither case could hunger have been the cause of the attack. The writer explains the occurrences, and also the accounts of females destroying males after union, on the ground "that those instincts which are habitually practised throughout the far greater portion of the life of the species, and on which it is dependent, would scarcely be suspended for a longer period than necessary for the sexual union." Some of the habits of spiders, and especially of this species, are mentioned as bearing on these sexual conflicts, and the specific benefits which would arise from them are referred to. The paper concluded by a note on some glands (probably for spinning) situated on the convexity of the abdominal sexual region. The ducts are considerably convoluted, and open through transparent tubular spines which are arranged transversely to the axis of the body of the spider. Two papilla-like processes below the opening of the genital sinus are described.

Zoological Society, February 6.—Prof. W. H. Flower, LL.D., F.R.S., president, in the chair.—A letter was read from Mr. F. C. Selous, dated from the Matabele Country, on the possibility of obtaining a White Rhinoceros.—Extracts were read from a letter received from the Rev. G. H. R. Fisk, C.M.Z.S., of Cape Town, giving an account of the habits of some reptiles which he had had in captivity.—A communication was read from Messrs. Salvin and Godman, containing the description of a new species of Pigeon of the genus *Otidiphaps* from Ferguson Island, one of the D'Entrecasteaux group, which they proposed to call *O. insularis*.—Mr. Sclater read some further notes on *Tragelaphus gratus*, and exhibited drawings of both sexes of this antelope, taken from specimens living in the Menagerie of the Jardin des Plantes, Paris.—A communication was read from Mr. E. W. White, F.Z.S., containing some supplementary notes to a former paper on the birds of the Argentine Republic.—A communication was read from the Rev. G. A. Shaw, containing some notes on the habits of an Aye-Aye which he had had in confinement for several months, and other information respecting this animal.—Mr G. A. Boulenger, F.Z.S., read a paper containing the description of a new species of Lizard of the genus *Enyalius* from Peru, which he proposed to name *E. palpebralis*.

Berlin

Physiological Society, January 12.—Prof. du Bois Reymond, in the chair.—Dr. Falk read a contribution upon the phenomenon lately demonstrated by experiments on animals, that great oedema of the lungs can be produced in a very short time, even in a quarter of an hour, by compressing or otherwise interrupting the function of the left side of the heart; whereas a similar action on the right side does not produce such an effect. Dr. Falk has had an opportunity in two post-mortems of proving the correctness of these observations in respect to man. In one of these cases, a strong, healthy man died in consequence of a discharge of shot, and the post-mortem showed that the cause of death was the penetration of a shot into the wall of the left ventricle. The lung of this previously healthy man exhibited a high degree of oedema. In the second case, a healthy railway-workman was killed by a blow of a buffer upon the chest. The post-mortem showed a rupture of the right ventricle to have been the cause of death; the lungs which were carefully examined, did not show a trace of oedema.—Dr. W. Wolff described the structure of the tactile corpuscles, according to his researches, they contain no nerve-branchings, but consist of a rugose sheath, granular contents, and the free ends of the entering nerve-fibres. In opposition to other histologists he further found the epithelium-cells which he studied in the corneæ of small mammals to be devoid of nerves; and in agreement with this he has always found gland-cells to be without nerve. The sympathic nerve-fibres which enter into the glands according to Dr. Wolff always end in unstriped muscles.—Prof. Kronecker reported on experiments of Dr. Meiss, upon the irritability of the heart under abnormal conditions of nutrition. During experiments, undertaken to study the comparative effects of concentrated and diluted blood upon the frog's heart, and which established the occurrence of a more energetic activity by nutrition with concentrated blood, certain remarkable deviations occasionally occurred from the general law that frog's hearts (always) respond to every stimulation with maximal contractions. These deviations consisted in the occurrence of smaller submaximal beats between the maximal beats. Further investigation of this appearance led to the conclusion that this was a

result of abnormal conditions of nutrition which was not easily or certainly to be produced. These sub-maximal contractions and the irregular pulse, were chiefly observed when passing a current of asphyxiated blood through the heart, but they always disappeared on supplying the heart with fresh blood.

Physical Society, January 19.—Prof. Roeber in the chair.—Prof. Schwalbe supplemented his former communications to the Society on ice-caves, wtth additional facts he had recently come to know through literature. He noted, as a most interesting phenomenon, that the occurrence of ice-caves was not confined to limestones, basalts, and lavas, but that they have also been observed in gypsum-hills. Thus, in the gypsum-hill Illetzkaja Satscha, in the Southern Ural, is an ice-cave which Murchison visited in August; situated in the street, it was closed with a simple wooden door, and it was utilised by the inhabitants of the district as a store-room. Its temperature was so low that the drinks kept in it only a short distance from the mouth were frozen. And as with all other caves distinguished by their low temperature and ice-formation, it was stated of this one, that in winter the air in it was very warm, so that people slept in it at night without requiring their sheep-skin furs. Murchison had applied to Sir John Herschel for an explanation of the phenomenon, and Herschel had offered the hypothesis, that it was a case of cold and heat waves, which, penetrating inwards from the surface of the earth, were retarded, and so caused the low temperature in summer and the warmth in winter. Prof. Schwalbe, however, has convinced himself that this explanation is inadequate; for the summers in which ice-caves have been visited and found filled with ice, have been preceded by very mild winters, *e.g.* the winter 1881-2 was very mild, yet the ice-cave in Bohemia, which he had himself visited, was covered with ice; besides, the retardation of cold several months is very improbable. Before a sufficient explanation of these peculiar conditions of temperature can be reached, continuous scientific observations must be made, for a long time, of the course of the temperature throughout the year. As to the warmth of air in the caves in winter, and the melting of the ice in winter, there are only observations by lay-persons, which, however, strikingly agree, even in the assertion that the ice-formation regularly takes place on a large scale in the month of May. With regard to the immediate cause of the ice-formation no one (according to the author) can be in doubt, who has visited an ice-cave, and seen how the dropping water from the roof solidifies directly on falling. Water that has trickled through is over-cooled, and solidifies just as after falling to the ground, even when it meets a different solid body; the ice, further, is only met with where water drops. The strong cooling of the water and of the rock through which it has trickled, is, perhaps, connected with the process of filtration through the earth-strata. On this point experimental research must decide, following up the investigation made by Jungk in 1865, who observed a lowering of temperature in filtration of water through porous partitions. Such laboratory experiments, exact long-continued temperature-observations in accessible ice-caves, and topographical examinations of as many as possible of these caves (which are not rare), will surely bring about a solution of this still obscure natural problem.

PARIS

Academy of Sciences, February 5.—M. Blanchard in the chair.—The following papers were read:—On the physical and mechanical constitution of the sun (third and last part), by M. Faye. He deals with the depth of spots, the movements of hydrogen and their effects, the height of protuberances and an illusion attending them, the clouds of the photosphere, &c.—M. Hirn presented an analysis of a *brochure* by himself and M. Hollauer. "Refutations of a second critique of M. G. Zeuner." It relates to the theory of steam-engines.—On the spherical representation of surfaces, by M. Darboux.—On the functions satisfying the equation $\Delta F = 0$, by M. Appell.—On the displacement of the sodium lines, observed in the spectrum of the great comet of 1882, by MM. Thollon and Guy. From the displacement observed with a single-prism spectroscope, he had estimated the rate of recession of the comet at from 61 to 76 km. per second. This is confirmed by M. Bigourdan, who, from a determination of the trajectory of the comet, estimates the velocity at the time in question (3 p.m. on September 18) at 73 km. The spectroscope is thus shown to be reliable for the purpose referred to.—Magnetic action of the sun and the planets; it does not produce secular variation in the great axes of orbits: note by M. Quet.—The distribution of energy in the solar spectrum and chlorophyll, by M. Timiriazeff. Prof. Langley finds, with his bolometer, the maximum of radiant energy in the orange, precisely that part of the spectrum which corresponds to the characteristic band of chlorophyll. M. Timiriazeff is studying the quantitative relation between solar energy absorbed by the chlorophyll of a leaf and that stored up through chemical work produced. It appears that the leaf can transform into useful work as much as 40 per cent. of the energy absorbed.—On some combinations of sulphite of magnesia with alkaline sulphites, by M. Gorgeu.—On hydraulic silica; reply to M. Le Châtelier, by M. Landrin.—On the mutual displacements of bases in neutral salts, the systems remaining homogeneous, by M. Menschutkine.—The microbes of marine fishes, by MM. Olivier and Richet. In all the fishes they examined (about 150) there were, in the peritoneal liquid, the lymph, the blood, and consequently in the tissues, microbes more or less numerous, having all the characters of terrestrial microbes, and reproducing similarly. Besides direct observation, the authors had recourse to experiments (1) of cultivation, (2) of occlusion. (In the latter case the fishes, or parts of them, were put into melted paraffine, which, after solidifying, was coated with several layers of collodion and Canada balsam, to protect from atmospheric germs. In a few weeks microbes were always abundantly developed.) The organisms were mostly *Bacillus*.—On the reaction-time of olfactory sensations, by M. Beaunis. He gives numerical results for this quantity (which is the time between sense-excitation and the moment when the person indicates by a signal that he has experienced the sensation) in the case of ten substances: ammonia, acetic acid, camphor, &c. They range from 37 to to 67 hundredths of a second. The time is longer than that for tactile, visual, and auditory sensations (in the author's case shorter than for tactile sensations).—On the respiration of aquatic plants or submerged aquatic-aërial plants, by M. Barthélemy. He considers the phenomena brought forward as proof and measure of the chlorophyllian function merely exceptional, and produced by the mode of experimentation. Under normal conditions, the special respiration of green organs cannot have the universal importance attributed to it.—Note on the morphological nature of the aërial branches of adult *Psilotum*, by M. Bertrand.—Influence of temperature on the production of wheat, by M. Duchaussoy. He gives in a table the yield of wheat in the department of Cher, and the mean temperature of spring and summer, from 1872 to 1881. The descending scale of the yield is nearly that of the mean temperature of summer. The years 1873 and 1876 are exceptions, and their small yield is explained by the dryness of the summer.

CONTENTS