

THURSDAY, DECEMBER 21, 1876

## GRAHAM'S RESEARCHES

*Chemical and Physical Researches.* By Thomas Graham, D.C.L., F.R.S. Collected and printed for presentation only. (Edinburgh, 1876.)

IT is but seldom that science owes a work like this to private munificence. All, therefore, to whom Mr. Graham's memory is dear will be specially grateful to Mr. James Young for choosing, as the second monument he has raised to his friend, the publication of this splendid volume.

It is fortunate also that its compilation has been undertaken by Dr. Angus Smith, who has done more than collect the scattered writings, for he has added an analysis of the contents of the volume which cannot fail to be of use, and reminds us, in a careful preface, of Graham's claim to a place in "that chain of eminent thinkers which has been represented by such as Leucippus, Lucretius, Newton, Higgins, and Dalton." As the work is printed for private circulation only, it may be well to give a brief summary of this preface which is headed "Graham and other Atomists."

A sketch is first given of the nature of Indian and Greek thought as regards atoms, and, passing to Leucippus, Dr. Smith points out that "in the mind of this early Greek, the action of the atom as one substance taking various forms by combinations unlimited, was enough to account for all the phenomena of the world." Leucippus told us that all was motion. "Graham conceived the idea that the diversity in the motion was the only basis of the diversity of the material, or that an atom constituted an element of a special kind," according to the velocity or nature of its movements. After Leucippus few men seem to have devoted much attention to the subject until modern times. A quotation from Lange gives the position assumed by Democritus, "The difference of substances arises from the difference in the number, size, shape, and arrangement of the atoms. The atoms have no internal conditions; they act by pressure and percussion only."

Dr. Smith considers that the views of Lucretius deserve attention, as he was the only full expositor of the theory of atoms to the ancients. To Lucretius atoms are "solid and eternal, with some unalterable motion." "They are made of parts, which parts cannot exist by themselves." "Motion is to him everything that can be found in life and thought, which are only the clashing of atoms." This theory allows of any shape of molecules, even hooked ones, which, as Dr. Smith somewhat dryly adds, "are spoken of as explaining combination both in Lucretius and more modern writers." Space will not permit Newton's words to be given at length, but he held that the primitive particles of which matter is composed are incomparably hard and incapable of wear, for otherwise "water and earth composed of old worn particles, would not be of the same nature and texture now with water and earth composed of entire particles in the beginning." He is thus forced to the conclusion that "the changes of corporeal things are to be placed only in

the various separations and new associations and motions of these permanent particles."

A sketch is then given of the next important stage, namely, the motion of gaseous molecules, beginning with Daniel Bernoulli, and passing to Davy, Rumford, and Herapath—to whom, by the by, Graham asserted in 1863 the merit of reviving Bernoulli's hypothesis in modern times is fairly due. As is well known, the theory of gases now generally received makes them consist of small bodies continually impinging on one another, and on the walls of the inclosing vessel, their elasticity increasing with the temperature, and the pressure of the gas being due to the impact of the particles against any surface presented to them, an hypothesis which Joule investigated experimentally.

Now, as Dr. Smith shows, "it was the object of Graham's life to find out what the movement of an atom was. . . . He avoided picturing the most primitive motion in all its character, but he seems to indicate one of revolution, as he brings in the similarity to the orbit of a planet," and he advances still further, "when he adopts the theory of one kind of matter, each atom being distinguished by the extent of its motion," there being an initial impulse for each kind. "These atoms are believed to be congregate . . . and equal volumes can coalesce and form a new atomic group." Indeed the whole force of Graham's intellect was patiently and persistently devoted to this study of molecular movement, and as Dr. Smith claims him to be as "strict an anatomist as perhaps can be found," it may be interesting to gather from his writings some of the passages in which his views are expressed.

His earliest paper, on the Absorption of Gases by Liquids, was published in Thomson's "Annals of Philosophy," in 1826. In it he considers that "gases may owe their absorption in liquids to their capability of being liquefied," and that when gases appear to be absorbed by liquids, they are simply reduced to that liquid inelastic form, which otherwise (by cold or pressure) they might be made to assume; their detention in the absorbing liquid is owing to that mutual affinity between liquids which is so common. In his last paper in the *Phil. Trans.*, published forty years afterwards, he refers to the liquefaction of gas in colloids in much the same terms, for he alludes to the "general assumption of liquidity by gases when absorbed by actual liquids or by soft colloids," and he states that those gases penetrate (india-rubber) most readily which are easily liquefied by pressure, that gases undergo liquefaction when absorbed by liquids and such colloid substances as india-rubber, and finally, that the complete suspension of the gaseous function during the transit through india-rubber cannot be kept too much in view.

The *Quarterly Journal of Science*, 1829, pp. 74-83, contains his first paper on the diffusion of gases. In it he states that the diffusiveness of gases is inversely as some function of their density, apparently the square-root of the density; he also considers it conceivable that orifices of excessive minuteness may be altogether impassable by gases of low diffusive power, but defers these theoretical considerations to a future paper. This promise was fulfilled by a paper in 1831, the object of which was to establish the following law of the diffusion of gases. "The diffusion or spontaneous intermixture of two gases



in contact is effected by an interchange in position of indefinitely minute volumes of the gases, which volumes are not necessarily of equal magnitude, being in the case of each gas inversely proportional to the square root of the density of that gas." He speaks of diffusion "being effected by a force of the highest intensity," and insists that diffusion takes place between the *ultimate particles of gases*, and not between sensible masses. In a later paper, *Phil. Trans.*, 1863, he states that molecules only of gas can pass the pores of graphite, "and they may be supposed to pass wholly unimpeded by friction." He showed that a gas may pass into a vacuum in four ways, first by effusion, a movement which affects *masses* of gas only, second, by diffusion which affects *molecules*, third, by transpiration through capillary tubes, "the transpiration ratios forming a class of phenomena remarkably isolated from all else at present known of gases," and lastly by a previous absorption in the walls of the septum which divides the gas from the vacuous space, as was so beautifully shown in the papers published in the years 1866-69. In one of these, in considering the passage of gas through metallic septa, he recognises "an intermolecular porosity due entirely to dilatation at a high temperature," and thus apparently hoped to ascertain the ultimate size of molecules, for he says that this "species of porosity, if it exists, might well be expected to throw light on the distances of solid molecules at elevated temperatures."

His views are very clearly defined in a paper published in 1863 entitled "Speculative Ideas respecting the Constitution of Matter." He is of opinion that the various kinds of matter now recognised as different elementary substances may possess one and the same ultimate or atomic molecule existing in different conditions of movement. Were this ultimate atom at rest, the uniformity of matter would be perfect; but it always possesses motion, due to a primordial impulse, and, as differences in the amount of this motion occasion differences of volume, matter only differs in being lighter or denser matter. The gaseous molecule is composed of a group of the preceding inferior atoms following similar laws and is thus a reproduction of the inferior atom on a higher scale. Chemical combination consists in equal volumes of the different forms of matter coalescing and forming a new atomic molecule, and is therefore directly an affair of weight; and the combining weights differ because the densities, atomic and molecular, differ. Graham is further careful to point out that liquefaction and solidification probably only involve a restriction of the range of the atomic movement.

In this brief sketch it has not been possible to touch on his views as to states of matter, such, for instance, as the "colloidal condition which intervenes between the liquid and crystalline states," or to the more purely chemical portion of his work, of which his theory of polybasic acids is probably the most remarkable.

Widely as the value of Graham's work was recognised during his lifetime, there is no doubt that the appreciation of it is increasing, and cannot fail to be stimulated by Mr. Young's liberality, which has set forth the researches in such a manner as to impress us with their coherence and strength.

W. CHANDLER ROBERTS

### THE ANDES AND THE AMAZON

*The Andes and the Amazon; or, Across the Continent of South America.* By James Orton, A.M., Professor of Natural History in Vassar College, U.S., &c. Third Edition, Revised and Enlarged, containing Notes of a Second Journey. Maps and Illustrations. (New York: Harper Brothers, 1876.)

AS is indicated in the title this work contains accounts of two separate journeys, to a considerable extent over the same ground, the first undertaken in 1867, the second in 1873. A narrative of the former was published several years ago both in America and in England, we believe; the second half of the volume is quite new and is essentially supplementary to the former. The results of the journey of 1867 are given in the form of a personal narrative, those of 1873 are arranged systematically in a number of chapters on the various features of the Amazon and its surroundings. The main scientific results of both expeditions have been described in the *Proceedings* of various scientific societies and in scientific journals in America and in England, and the present volume is therefore perfectly free from any details that would prove unattractive to the general reader.

In the journey of 1867 Prof. Orton and party landed at Guayaquil in Ecuador, mounted the Andes to Quito, proceeded by Papallacta, Baeza, and Archidona, still among the Andes, to the Napo river. Floating down this river they reached the Amazon, took steamer at Pebas, and enjoyed a splendid sail to the mouth of the river at Pará. In the second journey, that of 1873, Prof. Orton landed at Pará and sailed up the great river to Yurimaguas, thence over the Andes to the Pacific Coast and down to Lima, with a side-excursion to Lake Titicaca by way of Arequipa.

Prof. Orton tells his story in most attractive style. He is in danger sometimes, no doubt, of degenerating into the florid, but from beginning to end of his large volume he never ceases to be attractive, amusing, and instructive. He writes on people and things in the wonderful region of the Amazon with great piquancy, genuine humour, and full knowledge; he frequently becomes absolutely eloquent, if not poetic. Few features of the towns and the country through which he passed have escaped his attention. In describing his first journey, he lingers at Quito for several chapters, describing the city, giving hints and comments on its history, touching off the appearance and character of its easy-going people, giving an account of the country of which it is the capital, Ecuador, the flora and fauna and primeval inhabitants of the Valley of Quito, rising thence to an eloquent dramatic sketch, à la Hugh Miller, of the geological history of South America, the rise of the Andes, and the creation of the Amazon, devotes two interesting chapters to the volcanoes of Ecuador and its earthquakes, and before leaving, gives several details about a few of the Indian tribes in "the Province of the Orient." So on his way down the Napo and the Amazon, he paints vividly and picturesquely the scenery, the people, the animals, the plants, and the geology of one of the most interesting regions in the world. He chats pleasantly and piquantly of all he comes across, never gives the reader a chance of feeling wearied, and leaves him, if he has been a faithful



listener, with a fuller and clearer knowledge of the Amazon and its tributaries, its basin, its products, its people, its cities, and fragments of towns, its industries, and its probable future, than he could get from reading many other books. The second part especially, containing the results of the journey of 1873 systematically arranged, will be found extremely handy and valuable by anyone who desires in brief space a general view of the physical geography, natural history, ethnology, industrial resources, commerce, prospects, and scenery of the vast Amazonian region. Prof. Orton has evidently supplemented his personal knowledge of the region by an extensive study of the contributions of others who have written on the subject, so that while the classical works of Bates and others, as well as the special papers of Prof. Orton himself, will be resorted to by those who desire to make a thorough study of the Amazonian basin, we know of no single work containing a fuller, more brilliantly written, and at the same time more trustworthy general account of the basin of the Amazon and its many wonders. The following extract on the density of population in the Amazonian valley will give our readers some idea of the style of the work :—

“The valley of the Amazons is probably the most thinly-peopled region on the globe, save the great deserts and the polar zones. There are not 40,000 souls along the banks of the rivers in the whole province of Amazonas and the Lower Marañon. Many of the towns marked on the maps do not exist, or are represented by a solitary palm-hut. The visible population is almost confined to the circumference of the valley; as at Pará, near the mouth of the river; at Moyobamba and Taramoto, on the oriental side of the Andes; and at Trinidad, Santa Cruz, Cochabamba, and La Paz, on the head-waters of the Madeira. The great basin is filled with a continuous, dark, primeval forest, rarely disturbed by the hand of man, and into which daylight seldom enters. Yet imagination peoples this pathless wilderness with uncounted swarms of savages. There are, it is true, numerous clans (we can hardly call them tribes) of Indians, distinct in language, and often hostile toward each other. But many of these so-called tribes, though dignified with separate names, are insignificant in numbers, barely mustering a hundred; while the Mundurucú, the largest known tribe in the valley, does not exceed 8,000—men, women, and children. Nor are there any remains of ancient walls to indicate a bygone civilisation, or even shell-heaps in memory of a more primitive race.

“Until the close of the Tertiary age the waters prevailed over this heart of the continent; and since then vegetation has had the mastery, leaving little chance for animal life. And until there is a decided change in the physical geography of the valley, a large part of it must remain unfit for permanent settlement, on account of the annual floods; for a rise of 40 feet in the river drives the inhabitants from their summer resorts on the margin of the streams to the higher *terra firma* within the forest. In this way nomadic habits are engendered or perpetuated, and poverty is almost inevitable, for half the year (flood-time) it is hard work to get a living. Furthermore, this regular inundation of the country and the lack of grassy campos (except on the Lower Amazons and the Beni region) prevent the raising of domesticated animals, which, if it does not lie at the foundation of agriculture, certainly does aid in the transition from the savage to a semi-civilised state. In this respect the natives of Central Asia and Africa, as well as the maize-eating tribes of the Andes, have an advantage over the mandioca-eating Indians on the Amazons.”

While minute criticism might find many statements

and hypotheses in Prof. Orton's work to challenge; while some of his chapters may be considered by the lover of severity of style as intolerably florid; while in short anyone who has a mind to might find something to object to, we are sure that all into whose hands the work may fall will agree that few more attractive and at the same time more instructive works of travel have been written. Prof. Orton seems to anticipate that ere long the Amazon will become a highway for tourists, as it well might—even now it has a considerable service of steamers—and therefore gives many hints, directions, and statements of expense that render his work valuable as a guide-book. Not the least attractive feature are the many well-executed illustrations of places, people, scenery, and animal and plant life that enrich the volume. Two large maps, one of the Marañon and its tributaries and the other of Equatorial America, add to the value of the work, which will doubtless be brought within reach of the English reading public by some enterprising publisher.

#### OUR BOOK SHELF

*The Secret of the Circle, its Area Ascertained.* By Alick Carrick. Second Edition. (H. Sotheran and Co., 1876.)

*The Impossible Problem.* By James Alexander Smith. Printed for the Author's Use. (Shaw and Sons, 1876.)

THE only difference we have been able to detect between this edition and its predecessor are that the last lines of pp. 34 to 38 of the first edition are the first lines of pp. 35 to 39 of the second edition, with the corresponding changes of the other lines of the several pages, that a date has been omitted on p. 39, and fig. 2 on p. 41 slightly modified. With our copy we were favoured with a number of *The Welshman* (Sept. 29, 1876) containing a very long notice of it, supplied to the editor of the paper by an enthusiastic admirer of the work. An extract or two will sufficiently illustrate the article. “Don't let the reader run away with the idea that this is a prelude to any long, complicated calculations, understandable only by the initiated. As simple as truth itself, when ascertained, the solution of this problem is as easy and capable of absolute proof as any ordinary sum in addition and subtraction.” “This beautiful problem and mystery that has tempted, attracted, and defeated the skill of thousands of the most subtle and far-seeing minds for thousands of years is found, when looked at in the right way, to be as easy and as simple as the alphabet.” “It will not suffice for mathematicians to endeavour to show by any fallible and inadequate system of computation now in vogue that this result cannot be. The reader can judge for himself.”

We in our former notice pointed out what we considered defective in Mr. Carrick's proof. The work is a posthumous one, hence it is that the second edition has experienced no revision at the author's hands.

Mr. Smith, in his pamphlet (8 pp.), arrives at the same result as Mr. Carrick, viz., that  $\pi = 3\frac{1}{2}$ , or the area of the circle equals  $\frac{3}{8}$  of the square of the diameter  $+ \frac{1}{28}$  of that square. The roads pursued are different.

Though we cannot agree with Mr. Smith, we have read his work with some interest, for there are some neat little pieces of simple construction in it. His equations on p. 5 may be put into the more general form :—

$$w = (3 + \rho) x, y = (\frac{3}{2} + \rho) x, z = (\frac{3}{2} + 2\rho) x.$$

We have not been able to see if other values would satisfy his equations besides the one he has selected, which leads him to draw the conclusion he does,



*Concise Instructions in the Art of Retouching.* By Burrows and Colton. (London: Marion and Co, 22 and 23, Soho Square, 1876.)

FOR some years past a conviction has been growing amongst the better class of professional photographers that their art alone, even under the most perfect conditions, is unable to produce an artistically perfect portrait, a proposition, by the way, for which the true artist has all along contended in the face of the constant assertion of the converse by fanatical advocates of "sun-painting" *pur et simple*. The victory having at last rested with the artists, a number of books on retouching have been published, each professing to give the true method of at once producing artistic pictures.

We are glad to see that in the present little work the authors disclaim any such intention, but, on the contrary, proceed solely to instruct their pupils in the work before them, which we may here inform the uninitiated is no less a one than that of restoring, so to speak, on a photographic negative those injuries to a face which may have been caused by imperfect lighting and defects, such as dust, &c., in the film; or disease or physical injury to the face itself. In fact, the art of the re-toucher is to convey to a photograph a certain amount of that idealisation always manifest in the works of the painter, the want of which is the unknown cause so often producing a feeling of dissatisfaction even with the best of photographs.

To carry out their aim, the authors give two very good lithographs of the muscles of the face and head, with two more of the same model covered with the flesh. Two negatives on a flexible film (apparently taken by Warnerke's process) are also added as examples of the work to be done. The descriptive matter is concisely put, and is clear and to the point. We have little doubt that the book will be of service to many amateur and professional portrait photographers.

R. J. F.

### 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.]

#### Sea Fisheries

I HAD hoped that Mr. Holdsworth, in the rejoinder which he told me he was preparing to my former letter (*NATURE*, vol. xv. p. 55) would have confined himself to defending the assertions he had before made, or at most to rebutting the evidence I had adduced in reply to them. In this case I should have gladly left the matters at issue between him and me to the judgment of the public. Unfortunately he has thought it needful for the sake of the cause he adopts to introduce some new assumptions and charges, conveyed in language of a rather vigorous kind, so that out of regard to the good opinion of your readers, I am driven again to trespass on their forbearance and yours. But in doing this I shall try to be as brief as possible, and however much my friend may have exceeded the limits of a rejoinder, not to follow his example.

All will fully agree with Mr. Holdsworth that an "index is not a *précis*," but few will deny that an index is a valuable aid to mastering the contents of a book. If he says that this particular index is a bad one, I must leave him to settle with the maker of it. If it is neither bad nor good it may be misleading unless the user of it looks pretty carefully into the text. But if it is good, as I believe, it gives the reader the best of all help in acquainting himself with the huge volume, and by its help nobody need fear falling into dangerous mistakes. It does not seem to me that I have fallen into such. The errors which my friend asserts I have made are, if errors at all, very trivial, and as one tells for, and the other against, his views, they may be safely paired off to the detriment of neither side. As to the figures set in the last column of my table, against "Cod and Ling," they ought to have been "33" instead of "38"—a mistake in copying or printing which escaped my observation till now. I freely give Mr. Holdsworth the benefit of it. The next two paragraphs of his letter have afforded me some merriment, though chastened by

the thought that he must have a very low opinion of me if he seriously supposes I am ignorant of the notorious reputation of the dog-fish. Whether "predatory fishes," however, are necessarily "mischievous," so that the two epithets should be closely linked together, as though one was the consequence of the other, is a large question, upon which I shall not enter. But surely it is obvious that the prevalence of predatory fishes is more or less a measure of the prevalence of their prey, and as the blind man judged of the value of the field by asking how many thistles grew on it, so may we judge of the abundance or scarcity of other fishes by the abundance or scarcity of the dog-fish. Might I here apply to Mr. Holdsworth an expression of his own to myself, and say that, from these paragraphs, I am justified in believing him unable to comprehend one of the simplest relations of animal life?

As to herrings, I pretend to no greater knowledge of their natural history than other people do. I do not see why I should be accounted more ignorant, or attempting to conceal that ignorance, by any mysterious evolutionary process from my inner consciousness or elsewhere. The herring is admittedly not ubiquitous in the sea, *i.e.*, it has, like other animals, its more or less definite range. It therefore has "borders," though even Mr. Holdsworth cannot lay them down exactly. My friend is pleased "to doubt very much" whether I "had given five minutes' attention to the practical study of the habits of the herring—to its life-history" before I wrote my Glasgow address. That rather depends on what may be called "practical study." Has Mr. Holdsworth ever heard of a "water-telescope"—an instrument of which I can find no mention in his book—or has he ever looked through one? If, when the days get a little longer and the steamers are running conveniently, he will cross to Norway and follow the coast to the Lofoden Islands (perhaps he need not even go so far), he will possibly appreciate the value of these remarks, and will be doing what I did more than twenty years ago.

The next five paragraphs of Mr. Holdsworth's rejoinder seem to contain very fair comments on what I had urged, and, though I do not thereby assent to them, I may say that had the rest been of apiece with them I should not now be troubling you. I must, however, express my disappointment that in what follows no definite information is given as to the sea-fishes which are so often said to be devoured by sea-birds. Here is room for almost any amount of new and interesting observations, whether those observations affect his argument or not. He, not I, introduced the topic, for reasons I suppose known to himself, but not to be guessed at by me. He now seems to consider it, as I did, irrelevant.

Then as to Prof. Baird's reports. Far be it from me to find fault with my friend for fishing out the two passages which, as he thinks, tell in his favour. But these relate to two particular kinds of fishes<sup>1</sup>—the alewife and the cod—the former mainly as furnishing food to the latter, and I never said that to over-fishing *only* was the diminution in every case due. The decrease of the cod is ascribed by Prof. Baird to the decrease of the alewife, and this, he says, is caused by the erection in the tidal rivers of impassable dams or of weirs by means of which every fish ascending the river to spawn was caught. Surely this was "over-fishing." In the first of his reports this question is considered far more generally and closely than in the second, from which Mr. Holdsworth's extracts are taken, yet there is nothing in the latter really to contradict the conclusions arrived at in the former. Hence I infer that they are still upheld by their author, and their nature may be seen by the following citations from his "General Summary of Results" (Part I., pp. 38, 39):—

"I. The alleged decrease in the number of food-fishes in these waters within the last few years has been fully substantiated.

"II. The shore-fishes have been decreasing during the past twenty years, gradually at first, but much more abruptly from about the year 1865, the reduction by the year 1871 being so great as entirely to prevent any successful summer-fishing with the hook and line, and leaving to the traps and pounds the burden of supplying the markets. This statement applies also, but perhaps to a certain extent, to the blue-fish. The decrease in their numbers first manifested itself about ten years ago, and is going on quite rapidly until now.

"III. This period of decrease represents the time during which the traps and pounds have been well established, their operations

<sup>1</sup> The menhaden and the mackarel are indeed mentioned, but incidentally and with some uncertainty.



increasing year by year, and their catch, especially in the early spring, being always very great.

"VI. The decrease of the fish may be considered as due to the combined action of the fish-pounds or weirs, and the blue-fish, the former destroying a very large percentage of the spawning fish before they have deposited their eggs, and the latter devouring immense numbers of young fish after they have passed the ordinary perils of immaturity."

As Prof. Baird goes on to remark that there are no measures at command for destroying the blue-fish, even if that were desirable, and as the blue-fish was once far more abundant than it is at present, while other fishes were also more numerous, I cannot see that I made any mistake in stating that "over-fishing" was unquestionably assigned as "the chief cause" of the decrease in American sea-fisheries.

Lastly, Mr. Holdsworth says that the question lies between the late Royal Commissioners and myself. It was under this belief, holding him as their secretary to be their mouthpiece, that I took some trouble to reply to his first letter. Had any one not in that position challenged my remarks I should, perhaps, have not felt myself bound to give my reasons for the faith that is in me. He asserts that I have "no practical acquaintance with the subject." Possibly he considers that qualification limited to those who have been named in a Fishery Commission. In such case I certainly have none. He further charges me with using the Index to the Evidence as my "sole guide." Here I must venture to contradict him. I have used that Index, indeed, but much as Norwegian fishermen use the "water-telescope"—to look into the teeming depths of evidence below, unobstructed by the surface ripple of a Report.

To sum up. Your readers are aware that I originally treated of the Fisheries question as part of a much wider subject on which I felt constrained to speak my mind at a fitting opportunity. I have yet to learn that the Report of a Royal Commission is beyond the reach of fair and cool criticism, or that it is obligatory on all men to accept that Report as a revelation from supreme intelligence. My criticism of this Report was, I venture to think, not unfair, and it was not made in hasty warmth. Some ten years had passed since I adopted the opinions I hold, and the time had come when, as I thought, I could not help uttering them, nor does it seem to me that an unfitting occasion was offered by a meeting of the British Association. The decision of the question whether there is and has been "over-fishing" or not is hardly helped by the reiteration of the passage with which my friend ends his rejoinder.

Magdalene College, Cambridge,  
December 15, 1876

ALFRED NEWTON

### Ocean Currents

AGREEING in the main with Mr. Digby Murray's argument on the subject of ocean currents in NATURE (vol. xv. p. 76), I am the more disposed to criticise some of the statements with which it concludes, as put forward too strongly, to say the least.

I would ask for the "absolute proof" which Mr. Digby Murray supposes to exist, that (1) the upper-current return-trades "flowing from the equator descend again to the surface of the ocean on the polar sides of the calms of Cancer and Capricorn," and (2) "that these equatorial currents, subsequent to their descent on the polar sides of the calms of Cancer and Capricorn, are known as the westerly winds of the temperate zones." That these statements represent the prevailing opinion on the subject I readily admit, but I have ever looked in vain for any convincing arguments in their favour.

As regards the hypothesis that the trades cross one another in the region of equatorial calms, I may perhaps be permitted to quote some remarks of my own, made two years ago (*Symons' Met. Mag.*, vol. x. p. 37), since subsequent study has tended to confirm the doubts which I then expressed:—

"Maury's hypothesis, that the surface trade-wind of one hemisphere becomes the upper-current return-trade of the other ('Physical Geography of the Sea,' sec. 122 to 139) was in all probability originally suggested by the well-known fact that over the southern portion of the N.E. trade a S.E. upper-current prevails, and over the northern portion of the S.E. trade a N.E. upper-current, though he lays most stress on the arguments which he draws from the greater rainfall of the northern hemisphere (sec. 169 to 186), and from Ehrenberg's examination of the African air-dust (sec. 266 to 296).

"A seaman on approaching the doldrums, commonly notices a current overhead blowing at an angle of about 90° with the surface-trade; he is aware that this upper-current coincides in direction with the trade on the other side of the doldrums, and that in the calm belt itself, there is an upward motion of the atmosphere. It is, therefore, not unnatural that he should conclude that the upper-current which he observes is a poleward extension of the opposite trade in the higher regions of the atmosphere. It may also, I think, be admitted that the rapid and suddenly shifting cloud-currents, often observed over the region of the doldrums, are somewhat in keeping with Maury's idea of 'eddies,' or alternate strips of air.

"I would suggest that this hypothesis (which many subsequent writers have been surprisingly ready to adopt) may, perhaps, be subjected to a crucial test, if an answer can be given to the following query:—When the south-east trade draws so far to the north as to be deflected into a south-west surface wind, what is the prevalent direction of the upper-current over the southern portion of the north-east trade? If it runs from south-west it will be difficult to resist the conclusion that Maury is right; if from south-east it will appear probable that the upper-current is (principally at least) the north-east trade, deflected in the first part of its return course towards the north-west, just as it is in the subsequent part towards the north-east.

"Perhaps some meteorologist can give a definite answer to this question. The published data for its solution appear rather scanty; but, so far as my own limited information goes, the observations are generally rather adverse to Maury's theory."

I would now ask what proof exists that the upper currents from the polar depressions and those from the equatorial depression cross one another in the calms of Cancer and Capricorn so as subsequently to become the trades and anti-trades respectively? Since these upper-currents are understood to meet at the belts of tropical calms and there to descend, it is surely "more reasonable to suppose that their currents intermingle and that their mixed volume is then drawn off north and south, as required to restore the equilibrium of the atmosphere." These are Mr. Digby Murray's words in reference to the equatorial calms, and I fail to see why they will not apply to the calms of Cancer and Capricorn.

The whole question of the cause of the prevailing south-west and north-west winds of the north and south temperate zones, and the relation which these bear to the polar areas of barometric depression, may be regarded as fairly solved by the researches of Mr. Ferrel, Prof. J. Thomson, and others. As regards the great intensity of the Antarctic, as compared with the Arctic depression, and the superior force of the westerlies on its border, there is surely *prima facie* ground for believing that these are mainly due to superior evaporation in the water-hemisphere generally. (I say "mainly," because it seems probable that the comparative absence of surface-friction experienced by the atmospheric currents in that hemisphere tends to intensify the Antarctic depression.) That the evaporation from the warm surface-water of the North Atlantic is in excess of that from the relatively cold surface-water of the South Atlantic, may be readily admitted; but the Atlantic represents, after all, only a small portion of the surface of the globe. Will anyone maintain that the evaporation from the whole continent of Asia is equal to that which takes place from the corresponding area of the South Indian Ocean?

W. CLEMENT LEY

### Solar Physics at the Present Time

HAVING now read the *Astronomical Register's* more extended account of the November meeting of the Royal Astronomical Society, I found it very confirmatory of NATURE's shorter, but more quickly produced, summary of November 23, especially in what was said in the discussion upon Prof. Langley's (United States) paper on Sun-spots and Terrestrial Climate. Will you kindly allow me to remark:—

I. I am extremely glad that Sir G. B. Airy is now finding from the deep-soil thermometer observations at Greenwich that, whatever may be the interior temperature of the earth, and the terrific manifestations of it in some special volcanic localities abroad, yet all the remarkable changes and occasional abnormal elevations of temperature in the Greenwich soil *come from without*; for, Sir, that is precisely one of the earliest conclusions which I deduced for the Edinburgh soil, from the longer series of similar deep-soil thermometers there, and which I had the honour



of communicating to the Royal Society of London six years ago.

2. I am also very glad that Prof. Langley has proved by his most refined and crucial observations on the direct radiating powers of spot, penumbra and photosphere surface, combined with the continuous register of the amount each day of the spotted portion of the sun, that sun-spots have no sensible power in themselves for producing any notable change on terrestrial climate; because, Sir, in that paper of six years ago, and still in the hands of the Royal Society, I deduced that sun-spots were consequences, rather than causes, of the great periodical waves of heat which come upon the earth from without; and I proved that conclusion three times over, or for three successive cycles of the eleven-year sun-spot period.

3. As we are now on the commencement of another of those cycles, I must regret that the chief speakers seemed to intimate that almost all their idea of further investigation into the origin of those mysterious heat-waves received by the earth from without and apparently from the sun, centred in causing to be made more sun-spot observations; for, Sir, not only did it sound very much like proposing to lock the stable-door as soon as it should be announced that the horse is no longer therein, but there are further features accompanying those occasional great heat-waves, showing that they must originate in something much more intense, violent, and complicated than those comparatively harmless little phenomena, the dark spots. PIAZZI SMYTH, Edinburgh, December 5 Astronomer Royal for Scotland

Radiant Points of Shooting Stars

BETWEEN October 13 and November 28, watching for forty-nine hours, I observed 367 shooting stars, 306 of which were well seen and their paths registered. On going carefully over them some thirty-five radiant points are shown, about twenty-five of which are good positions, while the remainder are open to more or less doubt. The following are the twelve principal ones:—

Dates.	Position of Radiant Point.		No. of Meteors.	Notes.
	in	at		
1. Oct. 21–Nov. 20 ...	Taurus ... ..	R.A. Decl. 60 + 19	25	Many fine meteors, mean of three showers.
2. Sept. 17–Nov. 8 ...	Pisces ... ..	15 + 11	17	Mean of two showers, very slow meteors.
3. Sept. 20–Oct. 25 ...	Musca ... ..	46 + 26	19	Small rapid meteors.
4. Oct. 13–Nov. 23 ...	Lynx ... ..	125 + 47	17	White rapid meteors, mean of two showers.
5. Sept. 18–Nov. 8 ...	Cassiopeia ... ..	15 + 52	20	Small meteors, mean of two showers.
6. Oct. 25–Nov. 8 ...	Gemini ... ..	110 + 22	11	Mean of two showers, very swift meteors.
7. Nov. 20–23 ... ..	Leo Minor ... ..	155 + 36	21	A fine A. M. shower.
8. Nov. 20–28 ... ..	Ursa Major ... ..	208 + 43	12	Just before sunrise, small, short meteors
9. Nov. 7–8 ... ..	Camelopardus... ..	69 + 66	10	An evening shower.
10. Sept. 17–Nov. 20 ...	Lacerta ... ..	345 + 43	10	White rapid meteors, mean of two showers.
11. Nov. 22–28 ... ..	Auriga ... ..	78 + 43	9	White rapid meteors.
12. Oct. 17–25 ... ..	Orion... ..	88 + 17	8	A well-known Oct. shower.

Several of these positions are the mean of two showers, one seen in October the other in November, and evidently identical. No. 7 in the list is possibly new, and was a very active shower from near Leo; the meteors were rapid and white, leaving phosphorescent streaks. No. 8 is quite new, and three other radiants found on the mornings between November 19 and 28 are new. They are:—

- |                             |       |        |                        |
|-----------------------------|-------|--------|------------------------|
| Position.                   | R.A.  | Decl.  |                        |
| 1. Near $\gamma$ Leonis ... | 170 + | 4 ...  | 6 meteors, very swift. |
| 2. In Sextans ...           | 153 S | 1 ...  | 6 meteors.             |
| 3. $\alpha$ Boötis ...      | 212 + | 18 ... | 7 meteors.             |

The positions given are in most cases very accurate, and each of them represents a well-marked shower. The new radiants are visible preceding sunrise, and this may account for their having previously escaped detection.

Generally the meteors of October–November were very small. The magnitudes of the 306 registered were:—

X or =	1st mag.	2nd mag.	3rd mag.	4th mag.	5th mag.	6th mag.	Total.
19 ...	49 ...	61 ...	107 ...	65 ...	5 =		306.

I have lately found meteors very much more frequent after midnight than before it. In November, 13 $\frac{3}{4}$  hours watching, P.M., gave 79, while 12 hours watching, A.M., gave 133. Thus I have noted about double the number in the mornings than in the evenings. I found a similar difference in October, though have made no special comparisons to find if it has also been shown in the other months of the year. I usually find meteors show a progressive increase in numbers as the night advances, being at a minimum early in the evening hours and at a maximum just before the morning twilight.

Ashleydown, Bristol

WILLIAM F. DENNING

The Atlantic Ridge and Distribution of Fossil Plants

IT has occurred to me that the discovery of the narrow belt of suboceanic highlands extending in a sinuous course down the length of the Atlantic, as shown in the *Challenger* chart, removes a difficulty that has been present to students of fossil botany. When the area was land these hills would probably form a ridge sufficiently high to have a temperature cool enough to explain the migration across the tropics of plants living in a temperate or even cooler climate.

M.

Antedon Rosaceus (Comatula Rosacea)

THE letters of your correspondents with reference to the above, seem to me to fail to prove that there is any public recorded instance of its capture in the stalked (brachial) form at Torquay before the instance noted by the Birmingham Natural History and Microscopical Society in 1873. Of course if Prof. Allman took a specimen in the pre-brachial stage there in 1863, and Mr. Gosse the adult animal in 1864, the stalked form (brachial) must have been there as well, but was probably overlooked.

As to the change of name to which Mr. Thomas R. R. Stebbing objects, I certainly think that Dr. Carpenter, in his monograph before referred to, has deduced ample reasons for the substitution of *Antedon* for *Comatula*, "on the grounds of priority, in accordance with the rules of zoological nomenclature, and in concurrence with the views of Dr. J. E. Gray, Sir Wyville Thomson, and the Rev. A. Merle Norman." Birmingham, December 10 W. R. HUGHES

"Towering" of Birds

My experience goes to show that the towering action, although most frequent in the gallinaceous birds, is by no means confined to them. In the first case which came under my notice the bird was the common godwit. It was feeding on the border of a marsh, and I being very young at the time, committed the unsportsmanlike act of firing at it on the ground. Immediately on being hit the bird rose perpendicularly to a height of about 30 feet, then turned over on its back and fell dead almost on the spot from which it started.

Since then I have seen the same movement in the dunlin and some other species of *Tringa*, in the sanderling, the whimbrel, and, if I recollect rightly, in the lapwing plover, but in no other birds, excepting of course those mentioned by Mr. Romanes.

I have never seen a towering shore bird, after being struck, fly any considerable distance before towering, and those which have towered directly on being hit have always received a slight wound at the base of the brain, but there is little doubt that pulmonary hemorrhage is the principal cause of this curious action.

December 12

F. W. MILLETT

THE SPECTRUM OF THE NEW STAR<sup>1</sup>

NOTWITHSTANDING the bad weather and the feeble light (4th to 5th mag.), I have been able to investigate pretty completely the spectrum of the light

<sup>1</sup> Sur le spectre de l'étoile nouvelle de la constellation du Cygne. Note by M. A. Cornu, read at the Paris Academy of Sciences, December 11.



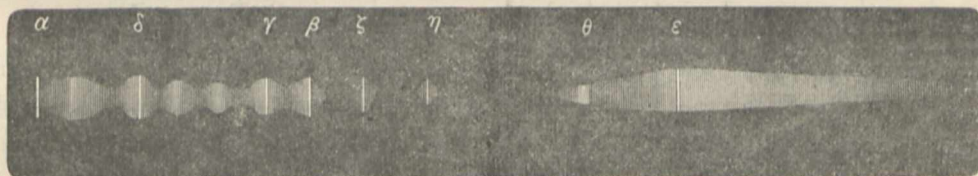
of this new star with the Eastern Equatorial of the Paris Observatory. On the first evening of observation I was only able to establish the presence of bright lines in the spectrum, two days after the atmospheric conditions enabled me to make a more thorough examination, and to take measurements as exact as the feeble light of the star permits. The following is the result of the spectroscopic investigations:—

The spectrum of the star is composed of a certain number of bright lines standing detached on a sort of luminous background, almost completely interrupted between the green and the blue, so that at first sight the spectrum appears to consist of two separate parts. In order to study it qualitatively, I made use of a spectroscopic eye-piece, specially constructed, which utilises the greatest portion of the light, and allows us to vary its concentration. For the measurements I employed a Duboscq direct-vision spectroscope, fitted with a scale visible by means of lateral reflection. The accompanying sketch gives an idea of the appearance of the spectrum, and represents the position of lines measured according to the readings of the auxiliary scale, in the most complete series of measurements.

I have only noticed bright lines; the dark lines, if they exist, must be very fine and must have escaped me on account of the very feeble light of the star. The order  $\alpha\beta\gamma\dots\theta$  is that of their intensity, taking into account the visibility of the colour. The following figures are the divisions of the scale which define their position:—

$\alpha$	$\delta$	$\gamma$	$\beta$	$\zeta$	$\eta$	$\theta$	$\epsilon$
30	44	60	66	73	81	100	113

The flame of a spirit lamp, observed immediately after, gave the line D at the division 42; but a slight obliquity



sodium, and  $\beta$  with the triple line  $b$  of magnesium. The feeble dispersing power of the spectroscope used did not enable me to distinguish whether the bright line was single, double, or triple, for any of the three cases may occur (*Comptes Rendus*, t. lxxiii., p. 332).

But the most curious coincidence, which I give here with much reserve, but which it will be interesting ultimately to verify, is the coincidence of the line  $\gamma$ , very bright in the spectrum of the star, with the green line  $\lambda = 532$  (1474 of Kirchhoff's scale), observed in the spectrum of the solar corona and in the chromosphere; the feeble band  $\theta$  corresponds also to a band  $\lambda = 447$  of the chromosphere; one is thus led to think that the line  $\delta$  corresponds rather to the bright line of the chromosphere  $\lambda = 587$  (helium), than to that of sodium, 589. If this interpretation be accurate, the bright lines of the spectrum of the star comprehend exclusively the brightest and most frequent lines of the chromosphere. The following, in fact, according to Young's Catalogue of the Chromospheric Lines (*Phil. Mag.*, November, 1871), is the designation of the brightest lines and their relative occurrence:—

Wave-lengths ... ..	656(C)	587	532	517( $\delta$ )	486(F)	447	434
Relative frequency ...	100	100	75	15	100	75	100

All the other bright lines have a relative frequency lower than 10, with the exception of the fourth bright line of hydrogen  $\lambda = 410$  ( $H$ ), to the extreme violet, whose frequency is represented by 100. I believe, moreover, that I have often seen this line without always being able to measure it.

To sum up, the light of the star appears to possess exactly the same composition as that of the solar envelope known as the chromosphere. Notwithstanding

of the slit relatively to the lines of the scale, introduces a constant difference of one to two divisions in the direction of the re-establishment of the coincidence with the line  $\delta$ .

The sky being covered soon after that measurement, I left the spectroscope in position without touching it, and next morning I compared the position of the Fraunhofer lines visible with the light of the clouds:—

C	D	$b$ (mean)	F	G
31	43.5	65.3	79.5	116

This is the spectroscope which I employed to observe the spectrum of the Aurora Borealis of February 4, 1872. The relative distance of the lines C, D, F, was the same, 21, 33, 69. There may easily be deduced from these data the correspondence of the divisions of the auxiliary scale with the scale of wave-lengths. The following are the results calculated for the bright lines observed, as also a table of bright lines of various elements expressed in millionths of a millimetre:—

	$\alpha$	$\delta$	$\gamma$	$\beta$	$\zeta$	$\eta$	$\theta$	$\epsilon$
Observ. ...	661	588	531	517	500	483	451	435
Hydrogen ...	656(C)	—	—	—	—	486(F)	—	434
Sodium ...	—	589(D)	—	—	—	—	—	—
Magnesium...	—	—	—	517( $b$ mean)	—	—	—	—
Line in Solar Corona ...	—	—	532	—	—	—	—	—
Line in Chromosphere..	—	587	—	—	—	—	—	447—

This comparison shows that if we take into account the small apparent displacement caused by the obliquity of the slit (which makes all the numbers a little too large), and the inevitable uncertainty presented by measurements of such feeble lights, it may be admitted that the lines  $\alpha, \eta, \epsilon$ , coincide with that of hydrogen,  $\delta$  with that of

the great temptation there exists to draw from this fact inductions relative to the physical condition of this new star, its temperature, the chemical reactions of which it may be the seat, I shall abstain from all comment and all hypothesis on this subject; I believe the facts necessary to arrive at a useful conclusion are wanting, or at least at a conclusion capable of verification. Whatever attractions these hypotheses may have, it is necessary not to forget that they are unscientific, and that, far from serving science, they greatly tend to trammel her.

### JUST INTONATION

DR. STONE'S lectures on "Sound and Music," just published, lead one to expect that notwithstanding the formidable appearance of some of the key-boards exhibited at South Kensington, the cause of scientific music and of just intonation in particular will be materially advanced by the Loan Exhibition.

Certainly we may look for something practicable and little short of perfect in the "Natural Finger-board" of Mr. Colin Brown, the Euing Lecturer on Music in the Andersonian University, Glasgow. As supplementary to the descriptions given by Dr. Stone and by Mr. Brown himself, I trust the following remarks will help to elucidate the construction of the instrument, and to make still more obvious its simplicity and "naturalness."

The vibration numbers of the diatonic scale being represented by—

$$1, \frac{9}{8}, \frac{5}{4}, \frac{4}{3}, \frac{3}{2}, \frac{5}{3}, \frac{15}{8}, 2,$$

if we build the scale upon the dominant  $\frac{3}{2}$  the vibration numbers will be—



$$1, \frac{9}{8}, \frac{5}{4}, \frac{45}{32}, \frac{3}{2}, \frac{27}{16}, \frac{15}{8}, 2,$$

and if we built it upon the subdominant  $\frac{4}{3}$  the vibration numbers will be—

$$1, \frac{10}{9}, \frac{5}{4}, \frac{4}{3}, \frac{3}{2}, \frac{5}{3}, \frac{16}{9}, 2.$$

Or, more generally, if C, D, E, F, G, A, B, 2C, be taken to represent the vibration numbers of the so-named notes in the scale of C, then the vibration numbers of the scales on G and F will be—

$$C, D, E, aF, G, \beta A, B, 2C,$$

$$C, \frac{1}{\beta}D, E, \frac{1}{a}F, G, A, \frac{1}{a}B, 2C,$$

where

$$\alpha = \frac{135}{128}, \beta = \frac{81}{80},$$

$\alpha$  and  $\beta$  being respectively the chromatic semitone and the comma. Here the law of the formation of the relative scales is so obvious that they can be written down successively at sight.

Now let the fifteen scales be written down from C to C $\sharp$  in the one direction, and to C $\flat$  in the other, when it will be immediately apparent that the symbols are arranged in groups of threes and fours, and if we draw straight lines horizontal and vertical so as to enclose these groups each in a rectangle, we have at once the properly so-called "Natural Fingerboard," the rectangles being the digitals, of which the larger are white and the smaller coloured

Major Keys.	$\alpha\beta C$	$\alpha\beta D$	$\alpha\beta E$	$\alpha\beta F$	$\alpha\beta G$	$\alpha\beta A$	$\alpha\beta B$	$2\alpha\beta C$
C $\sharp$	$\alpha\beta C$	$\alpha D$	$\beta E$	$\alpha\beta F$	$\alpha\beta G$	$\alpha\beta A$	$\beta B$	$2\alpha\beta C$
F $\sharp$	$\alpha\beta C$	$\alpha D$	$\beta E$	$\alpha\beta F$	$\alpha G$	$\alpha\beta A$	$\beta B$	$2\alpha\beta C$
B	$\alpha\beta C$	$\alpha D$	$\beta E$	$\alpha\beta F$	$\alpha G$	$\beta A$	$\beta B$	$2\alpha\beta C$
E	$\alpha C$	$\alpha D$	$\beta E$	$\alpha\beta F$	$\alpha G$	$\beta A$	$\beta B$	$2\alpha C$
A	$\alpha C$	D	$\beta E$	$\alpha F$	$\alpha G$	$\beta A$	$\beta B$	$2\alpha C$
D	$\alpha C$	D	$\beta E$	$\alpha F$	G	$\beta A$	$\beta B$	$2\alpha C$
G	C	D	$\beta E$	$\alpha F$	G	$\beta A$	B	2C
C	C	D	E	F	G	$\alpha A$	B	2C
F	C	$\frac{1}{\beta}D$	E	F	G	A	$\frac{1}{\alpha}B$	2C
B $\flat$	C	$\frac{1}{\beta}D$	$\frac{1}{\alpha}E$	F	$\frac{1}{\beta}G$	A	$\frac{1}{\alpha}B$	2C
E $\flat$	$\frac{1}{\beta}C$	$\frac{1}{\beta}D$	$\frac{1}{\alpha}E$	F	$\frac{1}{\beta}G$	$\frac{1}{\alpha}A$	$\frac{1}{\alpha}B$	$2\beta C$
A $\flat$	$\frac{1}{\beta}C$	$\frac{1}{\alpha\beta}D$	$\frac{1}{\alpha}E$	$\frac{1}{\beta}F$	$\frac{1}{\beta}G$	$\frac{1}{\alpha}A$	$\frac{1}{\alpha}B$	$2\beta C$
D $\flat$	$\frac{1}{\beta}C$	$\frac{1}{\alpha\beta}D$	$\frac{1}{\alpha}E$	$\frac{1}{\beta}F$	$\frac{1}{\alpha\beta}G$	$\frac{1}{\alpha}A$	$\frac{1}{\alpha\beta}B$	$2\beta C$
G $\flat$	$\frac{1}{\alpha\beta}C$	$\frac{1}{\alpha\beta}D$	$\frac{1}{\alpha\beta}E$	$\frac{1}{\beta}F$	$\frac{1}{\alpha\beta}G$	$\frac{1}{\alpha}A$	$\frac{1}{\alpha\beta}B$	$2\alpha\beta C$
C $\flat$	$\frac{1}{\alpha\beta}C$	$\frac{1}{\alpha\beta}D$	$\frac{1}{\alpha\beta}E$	$\frac{1}{\alpha\beta}F$	$\frac{1}{\alpha\beta}G$	$\frac{1}{\alpha\beta}A$	$\frac{1}{\alpha\beta}B$	$2\alpha\beta C$

Here all the scales are true and adjacent according to their relationship, the fingering obviously the same for all. The relative minors are provided for by a round

digital in the corner of each coloured digital, bearing to it the vibration ratio of 15 : 16.

The intervals on the board as seen above express themselves : moving from flat towards sharp keys the interval from a white to a coloured digital is  $\alpha$ , and from coloured to white  $\beta$ .

Between digitals related as  $\alpha G$  ( $G\sharp$ ) and  $\frac{1}{\alpha}A$  ( $A\flat$ ), that is, between every pair similarly related in mutual azimuth (to borrow a term) and distance, the schisma occurs. The keyboard gives us the value of the schisma by inspection; we may either take the route—

$$\frac{1}{\alpha}A \text{ to } A = \alpha : A \text{ to } G = \frac{9}{10} : G \text{ to } \alpha G = \alpha.$$

giving schisma =  $\frac{9}{10}\alpha^2$ . Or thus—

$$\frac{1}{\alpha}A \text{ to } A = \alpha : A \text{ to } \beta A = \beta : \beta A \text{ to } \alpha G = \frac{15}{16},$$

giving schisma =  $\frac{15}{16}\alpha\beta$ . This expression shows that the schisma also exists between any round digital, and the coloured digital next below. Or we may get an entirely numerical value thus :—Since from white to white is 8 : 9, then from  $\frac{1}{\alpha\beta}D$  diagonally to  $\beta A$  is  $(8 : 9)^4$  and from  $\beta A$  back horizontally to  $\alpha C$ , a minor sixth, is 8 : 5, the interval of  $\alpha C$  and  $\frac{1}{\alpha\beta}D$  is

$$\text{Schisma} = \frac{5}{8} \left(\frac{9}{8}\right)^4.$$

Again, the "comma of Pythagoras" being the excess of twelve fifths over seven octaves is expressed by—

$$\left(\frac{3}{2}\right)^{12} \frac{1}{2^7} = \left(\frac{9}{8}\right)^6 \frac{1}{2},$$

that is, it is the excess of six major tones over one octave.

The keyboard shows this immediately; from  $\frac{1}{\alpha\beta}D$  diagonally to  $2\alpha\beta C$  is six major tones, thence back horizontally to  $\alpha\beta C$  is one octave, therefore  $\alpha\beta C$  differs from  $\frac{1}{\alpha\beta}D$  by the "comma of Pythagoras." And every pair of white digitals (or coloured, as  $\alpha D$  and  $\frac{1}{\alpha}E$ ) similarly related in azimuth and distance have the same interval. Obviously, by mere inspection of the board, the "comma of Pythagoras" is equal to comma + schisma. In fact this keyboard will well repay a very careful study.

Turning to the practical aspect of the subject, the harmonium on this principle must be considered "un fait accompli," judging by the highly appreciative interest shown by the South Kensington audience, who remained long after the close of the lecture to listen to the instrument which was exhibited, and played on.

When we count the number of wires that would be necessary for a piano, the prospect would be somewhat alarming, did we not remember that on account of the number of harmonics and sub-harmonics, or combination tones that would be called into play, probably one wire to each digital instead of two or three would suffice.

The original account of this key-board is given in the second of two small pamphlets entitled "Music in Common Things," by Mr. Colin Brown. In these the numerical basis of the diatonic scale as derived from harmonics is laid down with remarkable perspicuity, and amongst other things the reader will see, for the first time probably, that although the fourth and sixth of the scale are wanting as harmonics to the tonic, yet they come out in the diatonic scale as harmonics to the fourth of the scale. Indeed the seven notes of the scale come out successively as harmonics to Fa in the fifth and sixth octaves.



ANATOMY OF THE TEETH<sup>1</sup>

TO the histologist, the zoologist, and the human anatomist the teeth are organs of considerable interest, from the points of view of their minute structure, their development, together with their variations, as well as from the diseases to which they are subject; and yet our literature has been deficient in a work on "Dental Anatomy, Human and Comparative." Text-books, such as Quain and Sharpey's "Anatomy," give us full information with reference to their structure, form, and development in man, whilst in Owen's superb monograph on "Odontography," as in his "Anatomy of Vertebrated Animals," their zoological aspect is treated of in detail. Mr. Charles S. Tomes has filled the deficiency in the volume under consideration, in a manner so satisfactory that we feel assured that his work will take a high place among Messrs. Churchill's valuable "Manuals."

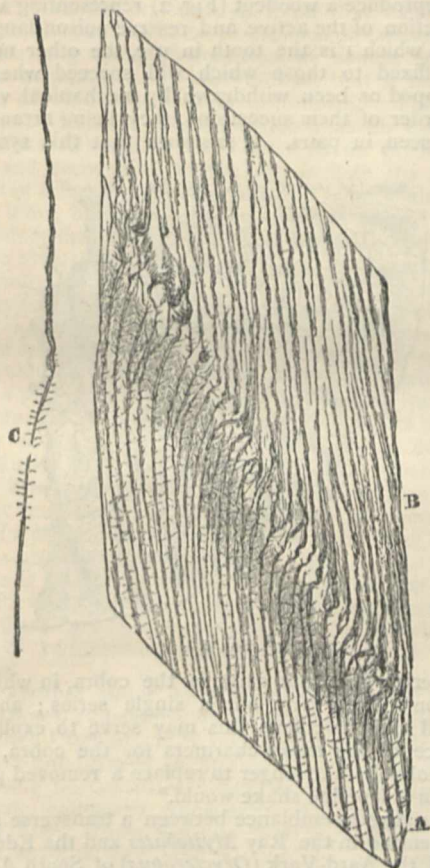


FIG. 1.

For some years past Mr. Tomes, following his father's footsteps, has been carefully investigating the structure and development of the teeth, not so much in man as in the lower vertebrata; and his results have been published in the *Proceedings of the Odontological Society*, the *Quarterly Journal of Microscopical Science*, the *Transactions of the Royal Society*, and elsewhere. In the work before us these results, many of them of considerable importance, are incorporated in the sections to which they refer, the book itself being an excellent epitome of our present knowledge of the development and structure of the teeth in man and the lower animals.

It is evident to all that a thorough acquaintance with comparative odontology cannot be obtained by anyone not familiar with the structure and development of the

<sup>1</sup> "A Manual of Dental Anatomy, Human and Comparative." By C. S. Tomes, M.A. (J. and A. Churchill, 1876.)

teeth. In the same way the facilities for investigating histological and embryological odontology are much enhanced by a knowledge of the comparative anatomy of the organs studied, as is the human dentition, although we would be among the last to make it a *sine qua non* that our dentist should be thoroughly informed on this large view of the subject. It is, therefore, on these accounts that Mr. Tomes's Manual will appeal to more than one class of student.

The work commences with an account of the teeth of

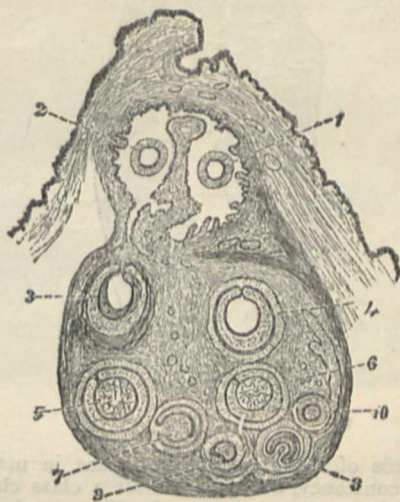


FIG. 2.

man and of the bones with which they are associated, "as being the standard with which the student is likely, consciously or unconsciously, to compare those other forms with which he afterwards becomes acquainted." The dental tissues are next described, followed by the development and eruption of the teeth, the last two hundred pages being devoted to the zoological aspect of the subject.

The human teeth have been studied so thoroughly that Mr. Tomes has nothing special to tell us with reference

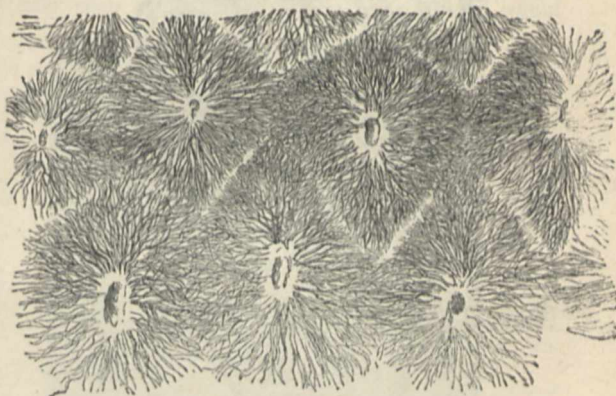


FIG. 3.

to them. As to their arrangement, his experience confirms the fact that, from the parabolic curve in which they are arranged in the typical human jaws, tending to squarish in the lowest races, "a deviation in the opposite direction is daily becoming more common in the most highly civilised races, resulting in a contour to which in extreme cases the name of V-shaped maxilla is applied."

On the subject of the histology of the dental tissues we notice several interesting points. With reference to the presence of enamel in the lower vertebrata, we are told



that a thin layer of this material is to be found in snakes, and that an enamel organ is always present, as in the frog and armadillo, where in the fully-formed tooth this tissue cannot be detected. We cannot refrain also from quoting a sentence with reference to a point of zoological interest which is frequently overlooked, for, says the author—"as was pointed out by my father, the passage of the dentinal tubes into and through a great part of



FIG. 4.

the thickness of the enamel, takes place in marsupials with such constancy, as to be almost a class characteristic." This condition is seen in Fig. 1, where A is the dentine, B the enamel, and C one of the dentinal tubes which enters the latter at the point where its course is most bent, and the smaller lateral ramifications cease.

It is with regard to the development of the teeth that Mr. Tomes gives us most valuable information, especially

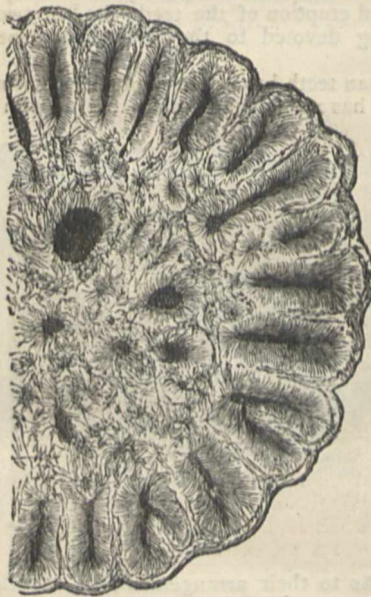


FIG. 5.

among the fish, amphibia, and reptiles, and his argument as to their homologies, deduced from their condition in the sharks, is so well stated that we cannot refrain from quoting it. We read that "if a young dog-fish just about to be hatched be examined, it will be found that it has no distinct under lip, but that its skin turns in under its rounded jaw without interruption. The skin outside

carries spines (placoid scales), and these spines are continued over that part of it which enters the mouth and bends over the jaws; only they are a little larger in this latter position. If the growth of the dog-fish be followed, these spines of the skin which cover the jaws become developed to a far greater size than those outside, and the identity and continuity of the two become to some extent masked. No one can doubt, whether from the comparison of the adult forms or from a study of the development of the parts, that the teeth of the shark [and dog-fish] correspond to the teeth of other fish, and these again to those of reptiles and mammals; it may be clearly demonstrated that the teeth of the shark are nothing more than highly developed spines of the skin, and therefore we infer that all teeth bear a similar relation to the skin. This is what is meant when teeth are called *dermal appendages*, and are said to be perfectly distinct from the internal bony skeleton."

We reproduce a woodcut (Fig. 2) representing a transverse section of the active and reserve poison-fangs of a viper, in which 1 is the tooth in use, the other numbers being affixed to those which will succeed when that has dropped or been withdrawn by mechanical violence in the order of their succession, they being arranged, as can be seen, in pairs. It is shown that this system of

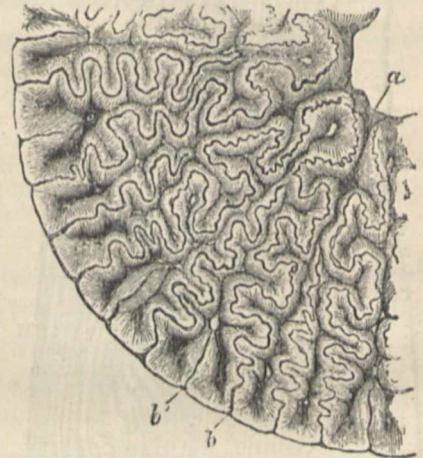


FIG. 6.

paired series does not exist in the cobra, in which the successional teeth form but a single series; and it is suggested that "perhaps this may serve to explain the preference of the snake-charmers for the cobra, which would probably take longer to replace a removed poison-fang than a viperine snake would."

The striking resemblance between a transverse section of the dentine in the Ray *Myliobates* and the Edentated mammal, the Aard-Vark (*Orycteropus*) of South Africa is well illustrated in Fig. 3; as in Figs. 4, 5, and 6 are the steps, through the Monitor Lizard (Fig. 4), and the American Bony Pike (Fig. 5), by which a tooth, apparently so elaborate as that of the extinct Labyrinthodon (Fig. 6)—consequently so named by Prof. Owen—may be assumed to have been arrived at by a gradual increase in the number and the depth of the numerous inflections of the pulp-papilla.

Mr. Tomes takes an extremely broad view of the mammalian tooth series, and, instead of following the but too frequent method of specialists, is willing to admit that similarity in dentition is not always associated with classificational affinity. As an instance of this we read that "it is very easy for us to see how a *rodent* type of dentition is beneficial to its possessor by rendering accessible articles of food wholly unavailable for creatures which have no means of gnawing through a shell or other hard body. Now it happens that in three regions of the world,



pretty completely cut off from one another, three animals, in parentage widely dissimilar, have arrived at dentitions of rodent type. Thus in Australa, a region practically wholly monopolised by marsupials, a marsupial, the Wombat, has a dentition very much like an ordinary placental rodent. In the island of Madagascar, which is, with the exception of a few mice, without indigenous rodents, a lemurine animal, the *Cheiromys* [Aye-aye], has a dentition modified in a similar direction; and elsewhere, scattered over the world, we have the ordinary rodents. In fact, three creatures, as widely different in parentage as they well could be, have been modified by natural selection until they have dentitions, not identical, but for practical purposes not unlike."

In one instance we think that Mr. Tomes has gone a little too far in his generalising proclivities, and this is with reference to the canine teeth, when we are told that "it would practically be very inconvenient to abolish the term canine, but it should be borne in mind that its significance is merely equivalent to *caniniform premolar*, and that in describing the dog's dentition we should be less liable to be misinterpreted were we to say that it has five premolars, of which the first is caniniform." This unwillingness to recognise the canine tooth as an element of the dental series makes Mr. Tomes, as do M. Milne-Edwards and some other naturalists, include the lower "canines" of the ruminant ungulata, and lemurs, with the incisors. This, however, is quite opposed to the well-supported doctrine that in placental mammals there are never more than three incisors in each side of each jaw, and if extended to its logical consequences must render it necessary that the lower incisors of all mammals should be termed *incisiform premolars*, a very awkward predicament. We all accept it as a fact that the definition of a "canine" tooth is not established upon so distinct a footing as a premolar and a molar, or an upper incisor; but any argument which attempts to annihilate its entity does away with the lower incisors also. In an animal like the Musk Deer (*Moschus moschiferus*), where the premolars are gradually reduced from behind forwards, how is it, it may be asked, that the upper canine tooth does not, if a premolar, participate in the reduction? Conversely it is immensely exaggerated in size.

Attention is drawn to an important fact recently arrived at by M. Pietkewicz, that, contrary to the statement of Goodsir, there are no traces, even in the youngest examples, of rudimentary upper incisors in the true Ruminantia.

There is another minor point in which we would differ from Mr. Tomes. Speaking of the Perissodactylate and Artiodactylate Ungulata, it is said that "the distinction between the two groups is strongly marked, if living animals alone be considered; but, as Prof. Huxley has pointed out, increasing knowledge of fossil forms is tending to break down the line of demarcation." Our experience is otherwise, and we cannot see between *Coryphodon* and *Anoplotherium* any nearer affinities than between the Tapir and the Hippopotamus. It is quite beyond our comprehension that an animal with the axis of the limb running through the middle of the median digit should be allied to a similar creature in which the axis runs between the third and fourth digits, except in times when no such special axis of support existed; that is, before the Ungulata came into existence as such.

In conclusion we cannot do better than recommend this valuable work by Mr. Tomes to students, not only to those who make the diseases of the teeth their special study, but also to others who are endeavouring to obtain reliable information on the comparative anatomy of these organs, which from their variations and complexity in different animals, have yielded and for a long time yet to come will continue to yield, so large a field for zoological investigation.

### FORMATION OF RAINDROPS AND HAIL-STONES<sup>1</sup>

WHEN the particles of water or ice which constitute a cloud or fog are all of the same size, and the air in which they are sustained is at rest or is moving uniformly in one direction, then these particles can have no motion relatively to each other. The weight of the particles will cause them to descend through the air with

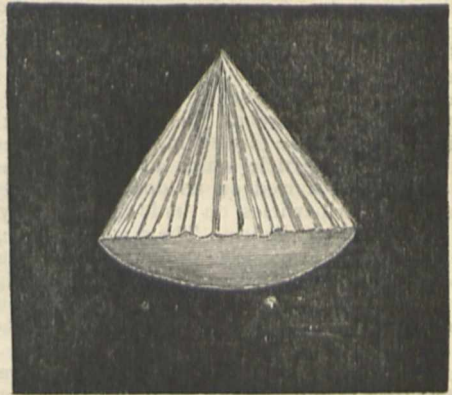


FIG. 1.—Perfect Hailstone.

velocities which depend on their diameters, and since they are all of the same size, they will all move with the same velocity.

Under these circumstances, therefore, the particles will not traverse the spaces which separate them, and there can be no aggregation so as to form raindrops or hailstones.

If, however, from circumstances to be presently considered, some of the particles of the cloud or fog attain a larger size than others, these will descend faster than the others, and will consequently overtake those immediately beneath them; with these they may combine so as to form still larger particles which will move with greater velocity, and more quickly overtaking the particles in front of them will add to their size at an increasing rate.

Under such circumstances, therefore, the cloud would

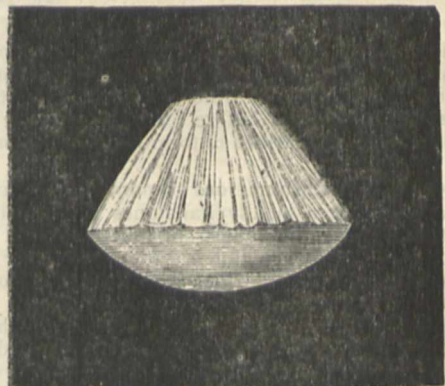


FIG. 2.—Broken Hailstone.

be converted into rain or hail according as the particles were water or ice.

The size of the drops from such a cloud would depend simply on the quantity of water suspended in the space swept through by the drop in its descent, that is to say,

<sup>1</sup> Abstract of paper "On the Manner in which Raindrops and Hailstones are Formed," by Prof. Osborne Reynolds, M.A., read at the Literary and Philosophical Society, Manchester.



on the density and thickness of the cloud below the point from which the drops started.

The author's object is to suggest that this is the actual way in which raindrops and hailstones are formed. He was first led to this conclusion from observing closely the structure of ordinary hailstones. Although to the casual observer hailstones may appear to have no particular shape except that of more or less imperfect spheres, on closer inspection they are seen all to partake more or less of a conical form with a rounded base like the sector of a sphere. In texture they have the appearance of an aggregation of minute particles of ice fitting closely together, but without any crystallisation such as that seen in the snow-flake, although the surface of the cone is striated, the striæ radiating from the vertex. Such a form and texture as this is exactly what would result if the stones were formed in the manner described above. When a particle which ultimately formed the vertex of the cone, started on its downward descent and encountered other particles on its lower face, they would adhere to it, however slightly. The mass, therefore, would grow in thickness downwards; and as some of the particles would strike the face so close to the edge that they would overhang, the lower face would continually grow broader, and a conical form be given to the mass above.

When found on the ground the hailstones are generally imperfect; and besides such bruises as may be accounted for by the fall, many of them appear to have been imperfect before reaching the ground. Such deformities, however, may be easily accounted for.

The larger stones fall faster than those which are smaller, and consequently may overtake them in their

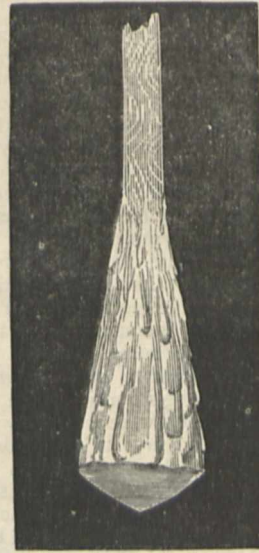


FIG. 3.—Imitation in Plaster of Paris.

descent; and then the smaller stones will stick to the larger and at once deform them. But besides the defor-

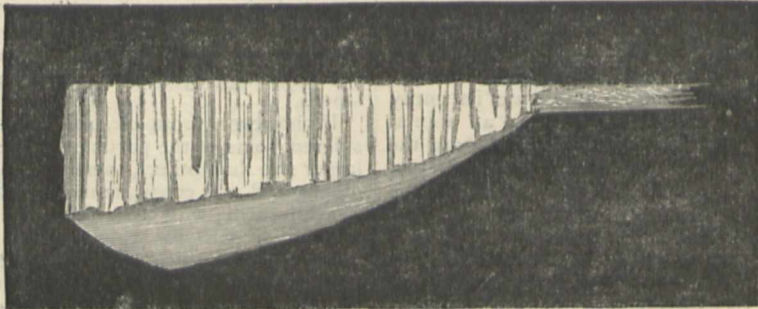


FIG. 4.—Imitation in Plaster of Paris.

mation caused by the presence of the smaller stone, the effect of the impact may be to impart a rotary motion to the stone, so that now it will no longer continue to grow in the same manner as before. Hence we have causes for almost any irregularities of form in the ordinary hailstone.

It appears from the numerous accounts which have been published that occasionally hailstones are found whose form is altogether different from that described above. These, however, are exceptional, and to whatever causes they may owe their peculiarities these causes cannot affect the stones to which reference is here made.

Again, on careful examination it is seen that the ordinary hailstones are denser and firmer towards their bases or spherical sides than near the vertex of the cone, which latter often appears to have broken off in the descent. This also is exactly what would result from the manner of formation described above. When the particle first starts it will be moving slowly, and the force with which the particles impinge upon it will be slight, and, consequently, its texture loose; as, however, it grows in size and its velocity increases; it will strike the particles it overtakes with greater force, and so drive them into a more compact mass. If the velocity were sufficient, the particles would strike with sufficient force to adhere as solid ice, and this appears to be the

case when the stones become large, as large as a walnut, for instance.

An idea of the effect of the suspended particles on being overtaken by the stone, may be formed from the action of the particles of sand in Mr. Tilghman's sand-blast, used for cutting glass. The two cases are essentially the same, the only difference being that the hailstone is moving through the air, whereas in the case of the sand-blast, the object which corresponds to the stone is fixed, and the sand is blown against it.

By this sand-blast the finest particles of sand are made to indent the hardest material, such as quartz or hard steel; so that the actual intensity of the pressure between the surface of the particles of sand and that of the object they strike, must be enormous. And yet the velocity of the blast is not so much greater than that at which a good-sized hailstone descends. It is easy to conceive, therefore, that the force of the impact of the suspended particles of ice if not much below the temperature of freezing on a large hailstone, would drive them together so as to form solid ice. For the effect of squeezing two particles of ice together, is to cause them to thaw at the surface of contact, and as soon as the pressure is relieved they freeze again, and hence their adhesion.

It is then shown that hailstones, such as those described, can neither be formed by the freezing of rain-drops, nor by



the condensation of vapour on a nucleus of ice; and that it is impossible that the particles of ice can have been drawn together by electrical attraction—their conical shape, and the increase in their density towards their thicker sides clearly showing that the particles have aggregated from one direction, and with an increasing force as the size of the stone has increased.

The possibility of making artificial stones is thus considered:—If a stream of frozen fog were driven against any small object, then the frozen particles should accumulate on the object in a mass resembling a hailstone. Not seeing his way to obtain such a stream of frozen fog, the author thought it might be worth while to try the effect of blowing very finely powdered plaster of Paris. He therefore introduced a stream of this material into a jet of steam, issuing freely into the air (which he hoped would moisten the powdered plaster sufficiently to cause it to set firmly into whatever form it collected). The jet was directed against a splinter of wood.

In this way masses of plaster very closely resembling hailstones were obtained. They were all more or less conical, with their bases facing the jet. But as might be expected, the angles of the cones were all smaller than those of the hailstones. Two of these figures are shown in the sketches annexed:

The striae were strongly marked, and exactly resembled those of the hailstone. The bases also were rounded. They were somewhat steeper than those of the hailstone; but this was clearly due to the want of sufficient cohesive power on the part of the plaster. It was not sufficiently wet. Owing to this cause also it was not possible to preserve the lumps when they were formed, as the least shake caused them to tumble in pieces.

Similar masses were also obtained by blowing the vapour of naphthaline, but these were also very fragile. Whereupon it is remarked:—At ordinary temperatures the powdered naphthaline does not adhere like ice when pressed into a lump. No doubt at very low temperatures ice would behave in the same way, that is to say, the particles would not adhere from the force of impact. Hence it would seem probable that for hailstones to be formed, the temperature of the cloud must not be much below freezing-point.

That the effect of the temperature of the cloud exercises great influence on the character of the hailstones cannot be doubted. And if, as has been suggested by M. L. Dufour, the particles will sometimes remain fluid, even when the temperature is as low as 0° F., it is clear that as they are swept up by a falling stone, they may freeze into homogeneous ice either in a laminated or crystalline form.

The author then proceeds to show that raindrops are probably formed in the same way as hailstones; that although the raindrops have no structural peculiarities like the hailstones, the aggregation of the particles of water by the descent of the drop through the cloud is the only explanation which will account for them. He shows that, as Mr. Baxendell had previously pointed out, the amount of vapour which a cold drop could condense before it becomes as warm as the vapour would be inappreciable when compared with the size of the drop, and since, in order that there might be condensation, the air must be warmer than the drop, the drop could not part with its heat to the air. He also shows that during the time of descent of a large drop, the heat lost by radiation would not account for the condensation of sufficient vapour to make any appreciable difference in the size of the drop. Whereas if we suppose all the vapour which a body of saturated air at 60° F. would contain over and above what it would contain at 32° to be changed into a fog or cloud; then if a particle, after commencing to descend, aggregated to itself all the water suspended in the volume of air through which it swept, the diameter of the drop after passing through 2,000 feet would be more

than an eighth of an inch, and after passing through 4,000 feet a quarter of an inch, and so on. So that in passing through 8,000 feet of such cloud, it would acquire a diameter of half an inch.

The fact that raindrops never attain the size of large hailstones is explained as being due to the mobility in the case of large drops of the surface tension of the water, by which alone the drop retains its form, to withstand the disturbing force of the air rushing past; when the drop reaches a certain size, therefore, it is blown in pieces like the water from a fountain.

The origin of drops and stones is then discussed—why some of the particles in a cloud should be larger than the others, as it is necessary for them to be in order that they may commence a more rapid descent. A cloud does not always rain; and hence it would seem that in their normal condition the particles of a cloud are all of the same size and have no internal motion, and that the variation of size is due to some irregularity or disturbance in the cloud.

Such irregularity would result when a cloud is cooling by radiation from its upper surface. The particles on the top of the cloud being more exposed would radiate faster than those below them and hence they would condense more vapour and grow more rapidly in size. They would therefore descend and leave other particles to form the top of the cloud. In this way we should have in embryo a continuous succession of drops.

Eddies in the cloud also form another possible cause of the origin of drops and stones.

#### D'ALBERTIS'S EXPEDITION UP THE FLY RIVER, NEW GUINEA

RECENT letters from Sydney announce the successful results of Signor L. M. D'Albertis's expedition up the Fly River, and that he was shortly expected back in Sydney.

The following letter from him to Dr. George Bennett has been published in the Sydney newspapers of October 13:—

“DEAR DOCTOR,—I have written a letter to the Committee, necessarily very brief, as I have but little time and a very scanty supply of paper. I am satisfied with the collection I have made, not for the number, but for the quality. I have four species of birds of paradise (*Paradisea*), the *P. raggiana*, the *P. apoda*, the twelve-wired bird of paradise (*Seleucidés alba*), the king bird of paradise (*Cincinurus regius*), and the rifle bird (*Epimachus magnificus*). I got a perfect adult specimen of a cassowary, which I think is *Casuarius bicarunculatus*; also the *Dasyptilus pecqueti*, a new genus of *Ptilotis*, and a splendid new species of *Gracula*, and several other small but very interesting birds. I have seen many birds which are not included in the avifauna of New Guinea, as the *Pelecanus conspicillatus*, the Jabiru (*Mycteria australis*) and the pygmy goose (*Nettapus pulchellus*). Among my fishes I have some fine and large species. Of reptiles I have very few except a water snake, which I hope will be something extraordinarily new. Among my insects I have some fine Coleoptera, but the season was not very favourable for them. I expect to have about five hundred species of dried plants and between twenty and thirty of living plants, collected far in the interior, many of which I did not get afterwards. I hope Mr. C. Moore will be satisfied, as I have some fine crotons and palms among them, also some ferns with variegated leaves, orchids, and several other plants with variegated or spotted foliage, &c., from the very centre of New Guinea. I hope Mr. Moore has sent to Somerset some Wardian cases, so that the plants may not be destroyed by the sea breezes during the passage to Sydney. I much regret that I cannot send you any specimens, but I have not a box to pack them



in ; the few boxes I have are filled with earth and living plants. My ethnological collections are very extensive indeed ; I have literally cleared all the houses, and I have the best collection of the stone implements of New Guinea ever seen, of every kind and description. I have also the ornaments used when dancing and when engaged in war, paddles for their canoes, &c., &c. I procured dresses of various patterns, some petticoats made of human hair, others of grass, both of the natural colour and dyed ; stone implements, finished and unfinished ; painted and carved skulls, stuffed human heads, arrows pointed with bone, artistically worked, and the cement used in fixing the points. I am very anxious to show you everything, and see your surprise at the beauty of my collection, as you can so well appreciate it. I hope my plan of the Fly River will be correct. I have noted mile by mile, and every day I landed I recorded the nature of the soil, &c. I hope the Government, Committee, and subscribers to the Expedition will be satisfied with the confidence they placed in me, and, more so, when I have time to publish my notes *in extenso*. I have investigated science in all its various branches, more especially anthropology. The presence of the great bird of paradise (*Paradisea apoda*, Linn.) in the centre of New Guinea, but at the same time in almost the same latitude as Aru Island, is of the greatest importance after what Lesson has asserted, and which has been denied by Wallace. I have got specimens in every stage of plumage, and of both sexes, and I have no doubt it is the *P. apoda*, and not the *P. papuana*. It is, nevertheless, much smaller than all the specimens I have seen in the British Museum, and in the collections of Mr. Beccari and Mr. Cockerell, and if with this distinction, when compared, any other difference may be perceptible, then it will probably prove to be a new species. For the present I believe it to be the *Paradisea apoda*. I have two beautiful male birds in full plumage, and also of the *P. raggiana*. I hope the Committee will be pleased with the short report I have sent, but at the same time must ask them to suspend their judgment of all that I have done until they receive a more extended and minuter account of the expedition from me." [Mr. D'Albertis concludes his letter with a few lines addressed to Mrs. Bennett, in which he says] : "I am in good health and spirits, and remember your kindness, for I bought bananas, when I was starving, with the red worsted ribbons adorned with pearl shells you gave me to traffic with the natives. I also enjoyed the large plum cake for a month, and finished it in the true centre of New Guinea, and wished I had another. The flag was the admiration of the natives of Moatta, and I bore in remembrance the ladies who presented it to me."

P. L. S.

#### OUR ASTRONOMICAL COLUMN

THE NEW STAR IN CYGNUS.—It is stated by Prof. Littrow (*Bulletin International*, December 12) that this star, which on December 1 appeared of about the same brightness as at the time of discovery on November 24, was of the fourth magnitude on December 2, and two days later had descended to the fifth.

Comparisons with neighbouring stars, using the magnitudes of the "Durchmusterung" on December 13, showed that the estimate we gave last week was somewhat too low, being doubtless influenced by the unfavourable conditions on the previous night ; the star was found to be 5.8—6.0, but as before, without trace of colour.

The following position for 1876.0 will be rather closer than the one given last week, R.A. 21h. 36m. 50.41s., N.P.D. 47° 43' 21".5, to which correspond annual precession in R.A. + 2".361s., in N.P.D. - 16".27. We give M. Cornu's spectroscopic results in another column.

REMARKABLE STAR SPECTRUM.—D'Arrest, writing in November, 1873, refers to the spectrum of the star XX. 1396, of

Weisse's second catalogue from Bessel's zone observations ; he says "sein Spectrum ist das merkwürdigste unter einigen tausenden, die ich bislang untersucht habe," and thinks the star may eventually prove to be variable. Has this object been examined by any British spectroscopist ? It was observed by Bessel as an eighth magnitude, October 26, 1823, and his position reduced to 1877.0 is in R.A. 20h. 43m. 24".2s., N.P.D. 67° 27' 36".

THE MINOR PLANET, No. 169.—This last discovered member of the group of small planets which was detected at Paris on September 28, has been named "Zelia." M. Leverrier's *Bulletin* of December 12, contains ample materials for the determination of its orbit.

NEWCOMB'S CORRECTIONS TO HANSEN'S LUNAR TABLES.—Part III. of papers published by the United States Commission on the transit of Venus, just received, contains an important investigation, by Prof. Newcomb, of the corrections required by Hansen's lunar tables, for the purpose of rendering the lunar ephemeris available for accurate determination of the longitudes of stations not telegraphically connected with well-ascertained positions.

Remarking that determinations of longitudes from moon culminations have been found by experience to be subject to constant errors which there is difficulty in allowing for, Prof. Newcomb refers to its having been a part of the policy of the American Commission to depend rather upon occultations. An occultation of a star is a sudden phenomenon, and the time at which it occurs can be fixed by observation within a small fraction of a second ; wherefore, if the ephemeris of the moon is exact and her figure a perfect circle, the longitude could be determined from such observations with a similar degree of precision. The inequalities of the lunar contour form a source of error that it is impossible to avoid, but may be considered to be eliminated from the mean of a large number of observations, and the star's position admitting of being fixed by the meridian instruments with any required exactness, there remain only the errors of the lunar ephemeris to be diminished as far as practicable, and it is the object of Prof. Newcomb's paper to reduce these errors to a minimum.

The material principally relied upon is the series of meridian observations of the moon at Greenwich and Washington from 1862 to 1874, but in order to verify the most striking and unexpected result of the investigation, the comparison of Hansen's tables with the Greenwich observations during the twelve years 1847-1858 has also been utilised. The result alluded to is the irregularity in the moon's longitude represented by

$$1''.50 \sin (56^{\circ}8 + 13^{\circ}.12413t)$$

where  $t$  is reckoned in days from Greenwich mean noon of 1850, January 0. The period of this inequality is 27.4304 days.

Prof. Newcomb remarks that "it would perhaps be premature to introduce so purely empirical a term as this into lunar tables for permanent use," but in the particular case to which his researches apply, where it is requisite to obtain the corrections of the tables with all possible accuracy for a limited period only, he considers the evidence in favour of the existence of the inequality sufficiently strong to justify its introduction. He further observes that the only apparent cause for this term is "the attraction of some one of the planets."

Prof. Newcomb finds some support to a correction of the tabular longitude of node, as already suggested by Hansen in the *Darlegung* in connection with his discussion of ancient eclipses. The entire corrections to the moon's longitude given by his investigation are given at p. 37, supplemented by auxiliary tables for facilitating the calculation of the corrections required by the tables as published, the arguments in which extend from 1850 to 1890.



It will be remembered that Prof. Newcomb communicated his principal result to the Royal Astronomical Society last summer (see "Monthly Notices," vol. xxxvi. p. 358).

PROF. FÖRSTER'S SCIENTIFIC LECTURES.—Under the title *Sammlung wissenschaftlicher Vorträge*, there has lately appeared a series of seven lectures on astronomical subjects by the director of the Royal Observatory at Berlin. It includes an address on "The Astronomy of Antiquity and the Middle Ages in Relation to Modern Development," notices of Copernicus and Kepler and their works, &c.

### CHEMICAL NOTES

VARIATIONS IN THE CRITICAL POINT OF CARBON DIOXIDE IN MINERALS, AND DEDUCTIONS FROM THESE AND OTHER FACTS.—Mr. W. N. Hartley has continued his experiments on this subject, and gives in a paper read lately before the Chemical Society further conclusions as to the existence of the expansible fluids in mineral cavities. He concludes it to be carbon dioxide from the spectrum produced by the electric spark in a tube containing such gas as was liberated by the decomposition of the minerals; and the turbidity produced by crushing quartz under baryta water (Vogelsang and Geissler, 1869); the rate of expansion of the liquid in sapphire compared with that of carbon dioxide (Sorby and Butler, 1869) and the determinations of the critical point made by himself in 1875-76. To determine the critical point he uses small thermometers made specially for the purpose, one having a range from  $-20$  to  $140^{\circ}$  F., the other graduated to register tenths of a degree from  $25^{\circ}$  to  $33^{\circ}$  C. The following table shows all the variations noticed in the critical point of carbon dioxide existing in various minerals:—

	Critical point.
Topaz ... ..	$28^{\circ}$ C.
Topaz ... ..	$28^{\circ}$ C. and $26^{\circ}5$
Topaz ... ..	$27^{\circ}55$
Tourmaline ... ..	$27^{\circ}27$
Tourmaline ... ..	$26^{\circ}9$
Sapphire ... ..	between $30^{\circ}5$ and $31^{\circ}$
Sapphire ... ..	between $25^{\circ}5$ and $26^{\circ}$
Sapphire ... ..	$29^{\circ}5$
Rock crystal ... ..	$30^{\circ}95$
Rock crystal ... ..	$30^{\circ}95$
Rock crystal ... ..	$32^{\circ}5$
Rock crystal ... ..	$33^{\circ}7$
Rock crystal ... ..	$39^{\circ}$
Rock crystal ... ..	$30^{\circ}95$
Beryl ... ..	$30^{\circ}92$

He discusses, from his conclusions, certain ideas with regard to the formation of diamonds, and believes that it is difficult to suppose that they are entirely formed by a process in which unoxidised forms of carbon are intermediate products, otherwise they would occur not unfrequently in the neighbourhood of coal formations. The theory that diamonds are produced by reducing agents on carbon dioxide very highly compressed and acted on at temperatures much above its critical point, introduces a condition of things highly suggestive of further speculation, and of experiments subject to conditions under which no chemical reactions have ever been made in the laboratory.

THERMO-CHEMICAL RESEARCHES.—Julius Thomsen has found in some recent investigations that gold presents allotropic modifications according to the nature of the solutions from which it is obtained, and the reagent with which it is precipitated. The modifications he has examined are gold precipitated from solution of the chloride and bromide respectively by sulphurous acid, and that precipitated from the sub-chloride, sub-bromide, and sub-iodide. These modifications differ in the amount of heat evolved by each in similar reactions. As the energy shown by the gold precipitated from solution of the chloride by sulphurous acid is less than in the other cases, this amount is taken as the

standard. The energy of the gold precipitated from the bromide is greater by  $3\cdot200$  heat units, and that precipitated from the sub-chloride, sub-iodide, or sub-bromide by  $4\cdot700$  heat units per atom.

THEINE IN TEA.—As the amount of theine varies in various kinds of tea (according to different analyses) from one to six per cent., the question naturally arises whether the quality of tea does not depend upon the amount of theine it contains. Some time ago M. Claus arrived at the conclusion that the inferior kinds of tea contain altogether more theine than the higher, pointing out especially that the cheapest, the so-called brick-tea used in Mongolia and Siberia, and prepared from all kinds of refuse as dead leaves, stalks, &c., contains far more theine ( $3\cdot3$  to  $3\cdot6$  per cent.) than the higher qualities ( $1\cdot0$  to  $1\cdot3$  per cent.). M. Markovnikoff, of Moscow, now arrives at different results. Having made a series of analyses of one kind of tea by the various analytical methods used until now, for ascertaining their comparative values, he proves the deficiency of most of these methods. Ether, for instance, extracts but one-third of the whole amount of theine, and benzole, one-fourth. Using, then, a more perfect method, and analysing six kinds of tea, selected from the highest and from the lowest qualities, he arrives at the result that the amount of theine in them varies but very little, from  $2\cdot08$  to  $2\cdot44$ , and that it regularly increases, with one exception, with the quality of tea, whilst the amount of ash given by each kind regularly decreases from  $6\cdot1$  to  $5\cdot7$  per cent. The differences being, however, very small, M. Markovnikoff supposes that the quality of tea does not depend, or depends very little, upon the amount of theine, and far more upon the quantity of tannic acid and aromatic oils it contains, but that on the whole the teas made from younger leaves contain more theine than those made from older leaves.

INFLUENCE OF PRESSURE ON COMBUSTION.—Some interesting observations have been recently made by M. Wartha, on the influence of pressure on combustion. He observed the burning of six stearine candles in free air, and in an iron case under a pressure of  $1\cdot95$  atmospheres. They burned under this pressure with a flame 9 to 12 cm. long, and gave much smoke; their luminous power diminished, while the flame assumed a yellowish-red colour. The decrease of weight after one hour of burning was found to be less than in burning in free air. This last result is opposed to the observations of Frankland, who has affirmed that the consumption of the burning material of a candle, or the like, is not perceptibly dependent on the pressure of the medium in which the combustion occurs. It is supposed that the difference of pressure in Frankland's experiments (on Mont Blanc and at Chamounix) was not sufficiently great to give a distinct difference in consumption of the burning matter. M. Wartha further put a candle to burn under an air-pump receiver, with special apertures, and, with increasing rarefaction, the flame was seen to enlarge, and its luminous power to diminish. At a pressure of 90 mm., the greatest rarefaction produced, the luminous power was quite gone, and the flame, which had now assumed threefold size, appeared to consist of three parts, an inner bluish-green cone with a violet sheath, and a weakly violet mantle. The diminution of the luminous power in this case M. Wartha explains by the fact that under less pressure less of the products of combustion are separated in the form of soot.

### BIOLOGICAL NOTES

THE AMERICAN BISONS.—An important quarto memoir on the living and extinct Bisons of America, from the pen of Mr. J. A. Allen, has just been issued from the University Press of Cambridge, Massachusetts. It is illustrated by twelve plates and a map of North America, in which the distribution of the bison



at different dates is indicated by various colours. The material for the account of the fossil forms, namely, *Bison latifrons* and *B. antiquus*, was obtained by Mr. N. S. Shaler, the Director of the Kentucky Geological Survey during 1868 and 1869, at Big Bone Lick. Independent of its scientific interest the description of the chase and rapid diminution of the existing animal will be found well worthy of perusal.

**COLORATION OF WATER BY SALPÆ.**—Coral reefs, as is well known, give a greenish appearance to the surrounding water when they do not lie far below the surface of the sea. The captains of merchant-vessels are accustomed to report the observation of such spots in the ocean when they are not marked on the charts. It has, however, frequently occurred, that subsequent navigators are entirely unable to find at such points any trace of hidden reefs. Baron von Schleinitz, commander of the German vessel *Gazelle*, in the report on his late exploring expedition, mentions that he encountered ten such places in the meridian 177° E. and 31° S. The lead was sunk to distances varying from 400 to 600 feet, at all these places, without reaching bottom. The green water presented an appearance as if an oily liquid was constantly rising to the surface, and a quantity of it was secured for examination. Investigation showed the cause of the phenomenon to be the presence of a great number of small, transparent, spherical salpæ. These were joined together in double rows of seven each, and by simultaneous expansion and contraction maintained a regular and comparatively rapid motion.

**THE EVOLUTION OF THE CAMELIDÆ.**—Prof. Cope traces an acceleration of the process of ossification of the cannon bones in Camelidæ, by which the three constituent metapodials become united at earlier and earlier periods of life. In the oldest genus, *Pœbrotherium*, these bones are permanently distinct; they are long distinct in *Procamelus*; and in *Auchenia* the bones are united before birth. There has been a concurrent reduction of the incisor teeth. In the Soup Fork genus *Protolabis* the three superior incisors persisted throughout life. In *Procamelus occidentalis* the second of these teeth persisted without being protruded till nearly adult age; the first incisor was very small, and, with its alveolus, was early removed. In the existing Camelidæ the second incisor disappears in the same way. In ruminants other than Camelidæ the third or external incisor has undergone the same process, while in the Bovidæ the canines have also been atrophied.

**SELF-FERTILISATION OF PLANTS.**—Mr. Thomas Meehan, continuing his observations on this subject, has found that the flower of *Campanula pulcherrima*, when confined in fine gauze bags, seeded perfectly; the method of this self-fertilisation he has not discovered. He has observed the self-fertilisation of chicory, which has rather large white pollen grains. The whole process takes about two hours. About six o'clock in the morning the pistil with closed stigmatic lobes elongates, pushing through the mass of pollen, and carrying quantities with it. About an hour after the stigmatic lobes expand, and the pollen falls into the cleft and on to the stigmatic surface. The flowers close entirely by nine or ten o'clock of the same day. No doubt pollen-eating insects visited the flowers, but when these were carefully excluded, all the flowers had pollen on their stigmatic surfaces, nevertheless. (*Proc. Acad. Nat. Sci., Philadelphia, 1876, p. 142.*)

**AN UNUSUAL CASE OF NATURAL SELECTION.**—The usual causes for the origin and increase of secondary sexual characters do not exist among gasteropods; there is no struggling between the males for possession of the females. Mr. E. S. Morse has described (*Proc. Boston Soc. Nat. Hist., 1876, p. 284*) a curious case in which in a limited area the shells of the males of *Buccinum undatum* scarcely equalled half the length of the

female shells, and there was no doubt about their maturity. The rocky ledge on which they lived was at all times washed by impetuous currents, and the specimens of *Buccinum* were always found hid in nooks and concealed in cracks and crevices. Only the smallest males could work their way into such constricted quarters to the females; thus a diminution of the normal size of the male had arisen from a singular secondary sexual cause.

**GELATINE IN RELATION TO NUTRITION.**—Proceeding from the supposition that the processes of digestion are merely decompositions under the influence of water, which furnishes smaller and more diffusible molecules, that are afterwards compounded into the constituents of the body, M. Hermann recently (*Naturforscher*) proposed the question whether it might not be possible to employ gelatine for synthesis of albumen in the system (its decomposition products being very similar to those of albumen), merely adding tyrosin to it, the products of albumen-decomposition which are wanting. He accordingly requested M. Escher to make certain experiments on the subject. For a number of days the same food, containing gelatine but no albumen, was given to animals, and their weight and urine were determined. Then, for a similar number of days, the same amount of food was given, with a small quantity of tyrosin added, and weight and urine again determined. If the above supposition were correct, the body-weight should diminish during the first period, and the excreted urine correspond to the gelatine taken, plus some albumen of the body; in the period in which tyrosin was given, the body-weight should decrease less quickly or not at all, or even increase, and the urine be diminished so much as would correspond to the quantity of gelatine retained in the body together with tyrosin, like albumen. The experiments (nine series of them) made on pigs and dogs gave the following results:—1. Gelatine and tyrosin are absorbed in the intestine; they do not appear again in the excrement. 2. In food containing no albumen, gelatine alone cannot sustain the animal organism; the weight diminishes. 3. The same holds for tyrosin in food that is without albumen. 4. In food without albumen, gelatine and tyrosin may together sustain the organism; the weight of this remains stationary, or even increases. 5. The addition of tyrosin to food containing gelatine, but no albumen, diminishes the excretion of urine, so that less nitrogen is excreted than taken.

## NOTES

NEXT month a new mineralogical journal will appear in Germany to be called *Zeitschrift für Kristallographie und Mineralogie*. The editor is Dr. Groth, Professor of Mineralogy in the University of Strassburg. The most eminent German and foreign mineralogists have promised their co-operation.

The *Nation* states that the Trustees of the Johns Hopkins University are prepared, if convinced of the want of such a periodical, to assist in the publication of an American *Journal of Pure and Applied Mathematics*. A circular to elicit an expression of views on this subject has been issued under the signatures of Professors J. J. Sylvester, Simon Newcomb, Henry A. Rowland, and William E. Story.

MESSRS. MACMILLAN AND CO. are about to publish the first volume of a "Treatise on Chemistry" by Prof. Roscoe and Dr. Schorlemmer. The aim of the authors in this work has been to furnish a concise but at the same time complete treatise, which they hope will serve as a standard for the use of those who desire to obtain a more extended knowledge than can be derived from the various excellent smaller manuals that exist. The authors endeavour to give as complete and accurate an account as possible of purely chemical phenomena, and a clear description of the chief chemical processes. In the case of each element and of the chief compounds a short historical statement of the



growth of our knowledge is prefixed to the description of their chemical properties, while in all cases of importance references to original memoirs are given. Special care has been bestowed upon the illustrations.

THE following are the arrangements for the Friday evening lectures at the Royal Institution:—January 19, Prof. Tyndall, LL.D., F.R.S., M.R.I.; January 26, Prof. Huxley, LL.D., F.R.S.; February 2, Prof. Osborne Reynolds, "Vortex Motion;" February 9, Francis Galton, F.R.S., M.R.I., "Typical Laws of Heredity;" February 16, Prof. F. Guthrie, F.R.S., "Solid Water;" February 23, J. F. Moulton; March 2, Sir John Lubbock, Bt., M.P., F.R.S., M.R.I., "Ants;" March 9, Frederick J. Bramwell, F.R.S., M.R.I.; March 16; March 23, Prof. Gladstone, Ph.D., F.R.S., M.R.I.

M. LEVERRIER is setting up, at the door of the Paris Observatory, a public clock for the use of clock-makers, who have been in the habit of calling at a special room to see a chronometer regulated for their use. He has also issued a circular announcing that every commune which purchases a public aneroid barometer, places it for inspection in a public building, and enters into an engagement to send regularly weather telegrams, will receive warnings, either directly from the Observatory, or through a departmental office established in each chief centre.

A LARGE aneroid barometer has been placed in the most prominent part of the Paris Halles for the use of the country people who come daily to sell their garden produce. The dial is 1 metre 50 in diameter, and is lighted at night.

AT the last meeting of the Netherlands Zoological Association (held on Saturday, November 18, in the Zoological Garden of Rotterdam) the Committee of the Transportable Zoological Station (see NATURE, vol. xv., p. 118) presented their first annual report, which was received with enthusiasm by the members of the Association, and will be printed in its *Bulletin*. Next year Flushing will see the station erected in its neighbourhood. With the addition of a fourth member, the Committee of the past year was re-elected for the year 1877, and consists now of Prof. Hoffmann (Leiden University), Dr. Hock (Assistant at the Leiden Zootomical Laboratory), Dr. Hubrecht (Conservator Leiden Natural History Museum), and Dr. Horst (Assistant at the Utrecht Zootomical Laboratory).

NO less than twenty-nine medical men have been elected members of the French Chamber of Deputies, and seven members of the Senate. One of these, Dr. Henry Liouville, suggested the establishment of an extra Parliamentary Conference of all his colleagues. The idea was carried into execution, and the conference of medical legislators has produced much useful work. Various important bills have been already prepared and presented to the Chamber of Deputies by these medical members. The object of the Conference is merely to deliberate upon medical matters, all political subjects being excluded. Members of all parties are admitted if they belong to the medical profession. The meetings of the Medical Conference are held weekly at the residence of Dr. Henry Liouville.

A PARISIAN optician recently commenced to observe daily the exact times at which a radiometer in his shop began and ceased to rotate. The results are published daily in the *Temps*, and although different radiometers cannot well be compared with each other, these results are already interesting. Since December 1, when the first notice was published, the radiometer stopped twice entirely during the daytime—on the 8th, during a thunder-storm, which lasted from 1.30 to 3 P.M., and on the 13th, from 10 to 10.45 A.M., during an obscuration produced by fog. The exact time of the first move varies from 8.15 to 10.25 A.M., accord-

ing to the purity of the atmosphere. The time of stopping is far less irregular, having varied only from 3.30 to 4 P.M.

THE hall in which the Academy of Sciences meets seems to be one of the worst ventilated rooms in Paris. If the windows are closed the members are stifled with heat and foul air, and many of the members have a horror of open windows. An illustrious physiologist is specially remarkable for his aversion to a current of fresh air. At a recent sitting the following colloquy on this subject occurred:—"M. Bouley: Nous sommes plongés dans un air irrespirable; ce n'est pas tenable; et, au lieu du gaz, je désire qu'on nous rende les anciennes bougies. M. Leverrier: J'ai réclamé l'éclairage par le gaz; mais j'avais réclamé aussi un autre mode d'aération. Or, rien n'a été changé sous ce rapport. Cependant nous avons le général Morin, et en huit jours des appareils convenables de ventilation seraient installés, si l'on voulait. M. Morin: Ah! en huit jours! Il y a dix ans que leur installation est décidée en principe!!! M. Leverrier: L'état actuel est vraiment honteux! Il n'y a pas de salle aussi mal ventilée que la *salle de l'Institut!*" If M. Leverrier would enter the meeting-room in the magnificent new buildings of the Royal Society during a meeting of that learned body, perhaps he would be inclined to modify his statement. It is a curious commentary on the progress of science that in Paris and London the most unscientifically constructed buildings are those in which the leaders of science carry on their deliberations.

BARON VON HOFMANN, president of the Oriental Museum in Vienna, has received a letter from the Austro-Hungarian Consul at Khartoum giving some details of the travels of Emin Effendi (Dr. Schnitzer). He has visited Uganda, Usoga, and Unyora, and stayed for some time with King Mtesa, whom he speaks of as a most wonderful man, with a considerable proportion of Abyssinian blood in his veins. This mixture of Abyssinian and negro blood Dr. Schnitzer is inclined to think accounts for the varied traits of character which travellers tell us Mtesa exhibits. He speaks of him as a man of high intelligence, but like all negroes, a child with tiger instincts; his liking for Christianity may possibly be an outcome of his Abyssinian blood. Dr. Schnitzer states that if he gets safely to the end of his journey through Nasindi, Magango, over the lake to Duffla and Lado, he will set about the arrangement for publication of his well-filled diaries. Dr. Noll of Frankfort will publish the zoological results of his travels.

A COMMISSION for the investigation of the possibility of making a canal across the Isthmus of Darien, left St. Nazaire last month. If their report is favourable it is stated that the work will be commenced next year.

A SPECIAL society for sending a scientific and commercial expedition to the unexplored parts of Asia is about to be formed at St. Petersburg.

MR. WILLIAM JOLLY, H.M. Inspector, has reprinted the paper he read at the B.A. Glasgow Meeting, on Physical Education and Hygiene in Schools. Kempster and Co., London, are the publishers.

IN the last session of the Berlin Anthropological Society Prof. Virchow presented a communication from Dr. Maclay on the results of his journey through the Malay peninsula. In his zigzag course through the mountainous region he repeatedly encountered savage tribes, displaying many points of resemblance with the Nigritos of the Philippine Islands. Two very interesting physical peculiarities of these savages attracted the traveller's attention. The first was the unusually prominent development of the so-called third eye-lid, a feature by no means uncommon among many families of animals; and the second a remarkable inclination sideways of the three outer toes. This



latter peculiarity has been noticed in several members of the ape family. In the course of his trip Dr. Maclay reached what he regarded as the highest mountain of the peninsula. At its foot he found the most numerous remains of the prehistoric Milanese inhabitants, and encountered the dreaded "Bru," an ape above the human size. His accounts possess a special interest, as they supply the first accurate description of what are probably the only continental representatives of the insular tribes of the Malay Archipelago. Prof. Virchow exhibited also a bronze three-wheel vehicle, excavated near the River Spree, which was ornamented with oxen heads and birds. Among a large number of anthropological objects shown to the Society were a collection of outlines of the feet of negroes on the Loango coast, showing in the most striking manner, by comparison, with the feet of Europeans, the crippling effects of modern costume.

THE South African Exhibition is to be opened at Cape Town on March 15, 1877. We believe that Messrs. R. S. Newall and Co., the well-known lightning-conductor manufacturers, have received the contract for supplying their copper-rope lightning-conductors to the buildings.

A MEETING was recently held in Birmingham of the Council in connection with the projected aquarium for that town. We are glad to see that the arrangements for carrying the scheme into execution are well forward, and a Committee was appointed at the meeting to make all necessary preliminary arrangements. The proposed plan of the aquarium seems to us all that could be desired, and we are glad to see that Mr. Hughes, and other speakers at the above meeting, showed a laudable desire to make the institution serve important educational purposes; we hope, at least, that it will not degenerate into a second-rate music-hall and miscellaneous rendezvous.

THE French government established many years ago at Athens a school to which pupils are sent yearly at its expense, to study, *in situ*, Greek archaeology. M. Jules Simon, when Minister of Public Instruction, established a similar school at Rome, for Italian archaeology. Both have been highly successful. The number of works sent to France by them has been so great that it was stated at the last sitting of the Academy des Inscriptions et Belles Lettres, that M. Waddington has determined to issue a special periodical to be called *Annals of Archaeology* in order to publish the contributions of the Fellows of the schools and of other French archaeologists.

A CORRESPONDENT writes that on Wednesday, about ten minutes to 5 P.M., while at Blackwater, near Yorktown, Hants, he suddenly saw, not sixty yards off, what appeared to be a small ball of fire falling from the heavens. It was of a pale yellow colour at first, changing to a lovely bluish green; then, as it approached nearer to the earth, it became brilliant red, with a tail of fire of the colour already mentioned; and finally, when about a yard from the ground, it seemed to go out. There was no explosion, no hissing sound. There were thin clouds in the sky at the time, but no wind; and very few stars had come out. It shortly became too dark to ascertain if any body had actually fallen, or if the exploding carbon, or whatever it may have been, had left any mark on the grass, which was partly under water. Several friends had noticed an aërolite proceeding from north to south in the direction of our correspondent's post, about the time when he saw the fire-ball fall.

THE intensity, colour, and polarisation of the diffuse light of the sky in different parts has recently been a subject of study by M. Wild, of the St. Petersburg Academy, who has endeavoured to measure it with a somewhat complicated instrument devised by him and named a "uranophotometer." Referring to the Academy's *Bulletin* for the account of this instrument, we may here give briefly the results at which M. Wild has arrived:—  
1. Proceeding from the sun in a vertical circle northwards, it is

found that the colour of the light gradually changes from the red end of the spectrum towards the violet, and at about 80° distance from the sun, it reaches about midway between the Fraunhofer lines C and D (corresponding to a wave-length of 0.000628 mm.); from there on to the horizon the colour gradually goes back towards the red end of the spectrum. Thus, in St. Petersburg at the time of equinox, the sun having about 60° zenith distance, the colour tone of the diffuse light at 80° angular distance (in a vertical circle) is mostly pure blue, and passes on either side into green. 2. The saturation of the colour appears to reach its maximum at 90° distance from the sun; where also the degree of polarisation is a maximum. On either side of this maximum the degree both of polarisation and of saturation of colour decreases pretty regularly. 3. The intensity of the diffuse reflected light of the sky appears, on the other hand, to be at its least at about 80° distance from the sun, and from there it increases less quickly towards the horizon than towards the sun. Thus, while at 140° distance from the sun the intensity is about five times greater than at 80°, at 20° distance it is over seven times greater. Southwards from the sun the intensity is considerably greater than northwards for the same distance. Thus, at 20° distance southwards it is nearly twice as great as the same distance northwards. These researches are being further prosecuted by the author.

THE "photography of tones" has been accomplished by Dr. Stein in a way which he describes in *Poggendorff's Annalen* (No. 9, 1876). One variety of his method consists in fixing a tuning-fork horizontally with its branches in vertical planes; there is a hole bored through the upper branch and a horizontal beam of light of somewhat larger section than the hole is directed on this from a heliostat. Part of the beam passes through to a sensitised plate in a case, which plate is made to move rapidly in a horizontal direction by means of a spring, or the like. Thus the luminous circle (on the plate) which, when the fork is vibrated and the plate at rest, gives a vertical line, gives a horizontal sinusoidal line when the plate is put in motion. The rate of motion of the plate being fixed, there will be a different number of undulations in a given space for each fork of different pitch. The curve has some interesting features; thus, it is much brighter at the bends than at the intermediate parts, the motion having been slower at the points of turning. The gradual retardation and acceleration are clearly shown. Dr. Stein finds it possible to photograph all ordinary musical tones, and even those vibrations which are above the upper limit of audition. He applies his method to strings also; fixing on these, with light supports, small square discs of blackened mica with a hole for admission of the light. Several cords in a row may have their periods photographed together on the same plate, the mica discs rising one above another.

THE additions to the Zoological Society's Gardens during the past week include a Sykes's Monkey (*Cercopithecus albogularis*) from West Africa, presented by Mr. R. Payne; a Vervet Monkey (*Cercopithecus lalandii*) from South Africa, presented by Mr. Thos. Mash; a Rhesus Monkey (*Macacus erythronus*) from India, presented by Mr. J. H. Ivory; a Hoffmann's Sloth (*Cholopus hoffmanni*) from Panama, presented by Mr. L. R. Dickinson; a Silver-backed Fox (*Canis chama*) from South Africa, presented by Mr. Richard Ladd; a Common Paradoxure (*Paradoxurus typus*) from Bengal, presented by Dr. J. B. Wilson; a Black-headed Gull (*Larus ridibundus*), European, presented by Mr. James Smart; two Jardine's Parrots (*Psephenus gularis*) from West Africa, two Indian Robins (*Geocichla citrina*), two Red-crowned Jay Thrushes (*Garrulax ruficeps*), a Horsfield's Mountain Thrush (*Mycophonus horsfieldi*) from India, purchased; a Red Coati (*Nasua nasica*) from South America, seven Double-spurred Francolines (*Francolinus bicalcaratus*) from West Africa, deposited.



## SCIENTIFIC SERIALS

*Memorie della Società degli Spettroscopisti Italiani*, for September, 1876, contains a paper on the absorption spectrum of the colouring matters of several molluscs, by Antonio and Giovanni De-Negri. The authors show that the colouring matter of the *Elysia viridis* gives the same absorption spectrum as chlorophyll, and that the colouring matter of other molluscs is identical vegetable colouring matters, or their derivatives.—A table of the solar spots and faculæ observed at Palermo in August and September. On at least half the days there was an absence of spots. The spectroscopic image of the sun's limb as seen at Rome in January and February, 1875, accompanies this number.—Prof. Serpieri continues his paper on the observations of the zodiacal light, by G. Jones.

October.—This number contains a very quaint drawing of the sun taken from Kircher's "Mundus Subterraneus." It represents a spherical body covered with flames, bright spots of light, and puffs of smoke giving the appearance somewhat of facula, spots, and prominences.—Spectroscopic and direct observations of the sun made at Palermo in August and September last, by Prof. Tacchini. He remarks that the height of the chromosphere, about 12", seems greater than is usual at the minimum sunspot period where hitherto a diminution has been noticed, the eruptions were smaller in number. Prof. Serpieri continues his paper in this number.

*Poggendorff's Annalen der Physik und Chemie*, No. 10, 1876.—On the heat conduction of gases and vapours, and the dependence of their specific heat on temperature, by M. Winkelmann.—On the absolute changes of phase in reflection of light, and on the theory of reflection, by M. Wernicke.—The electric conductivity of chlorhydric, bromhydric, and iodhydric acid, and of sulphuric, phosphoric, oxalic, pyrotartaric, and acetic acid in aqueous solutions, by M. Kohlrausch.—On the transverse vibrations of liquid films, by M. Melde.—On the photography of the less refrangible parts of the solar spectrum, by MM. Vogel and Lohse.—On the number of electric materials, by M. Neumann.—On anhydride sulphuric acid and a new hydrate of sulphuric acid, by M. Weber.—Remarks on M. v. Oettingen's paper on temperature and adiabata, by M. Clausius.—On the smallest deflection in the prism, by M. Lommel.—On the momentary illumination in observation of light wave streaks, by M. Mach.—On a radiometer experiment, by M. Krüss.—Safety arrangement for hydrogen-making apparatus for avoidance of oxyhydrogen explosions on mixture of the gas, by M. Rosenfeld.

## SOCIETIES AND ACADEMIES

## LONDON

Royal Society, December 14.—"Note on the Photographic Spectra of Stars." By William Huggins, D.C.L., LL.D., F.R.S.

In the year 1863 Dr. Miller and myself obtained the photograph of the spectrum of Sirius.

"On January 27, 1863, and on March 3 of the same year, when the spectrum of this star (Sirius) was caused to fall upon a sensitive collodion surface, an intense spectrum of the more refrangible part was obtained. From want of accurate adjustment of the focus, or from the motion of the star not being exactly compensated by the clock movement, or from atmospheric tremor, the spectrum, though tolerably defined at the edges, presented no indications of lines. Our other investigations have hitherto prevented us from continuing these experiments further; but we have not abandoned our intention of pursuing them."<sup>1</sup>

I have recently resumed these experiments by the aid of the 18-inch speculum belonging to the Royal Society's telescope in my possession. Considerable delay has arisen from the necessity, for these observations, of a more uniform motion of the driving clock. For this purpose Mr. Howard Grubb has successfully applied to the clock the control of a seconds pendulum in electric connection with a sidereal clock. This system works quite satisfactorily.

The prisms employed are made of Iceland spar, and the lenses of quartz. After an extensive trial of different photographic processes, preference has been given to dry plates.

The apparatus is so arranged that a solar or electric spectrum can be taken on the same plate, for the purpose of comparison,

with the spectrum of the star. Spectra have been obtained of Sirius, Vega, Venus, the moon, &c.

I do not purpose in this preliminary notice to describe in detail the arrangements of the special apparatus which has been constructed, nor to offer in their present incomplete state to the Royal Society the results of the experiments. Still I venture to hope that even in this early stage of the inquiry the enlarged copy of the spectrum of Vega (*α* Lyrae) which accompanies this note, may not be regarded as altogether unworthy of attention.

After exposure to the light of Vega the dry plate was allowed to remain in the instrument until the following morning, when a solar spectrum was taken upon it through the half of the slit which had remained closed when the instrument was directed to the star.

The photograph shows seven strong lines all of them slightly shaded at the sides. The two lines which are least refrangible coincide with two known lines of hydrogen in the solar spectrum.

It is expected, by means of an apparatus now in the course of construction, to obtain also any finer line which may be present in the spectrum of this star, as well as to extend the photographic method to stars which are less bright.

I need not now refer to the many important questions in connection with which photographic observations of stars may be of value.

Anthropological Institute, December 11.—Col. Lane-Fox, president, in the chair.—The following new members were announced:—Rev. A. H. Sayce, G. Tippets, and T. F. Peacock. The following paper, by H. H. Howorth, was read:—On the ethnology of the Geruans; Part I., The Saxons of Lower Saxony. The author contended that the Saxons north of the Elbe were immigrants, and of the same race as those south of that river, and that the Saxons were not indigenous to Hanover or Westphalia, but colonists or invaders. This he proved by the topography of those districts and by the names of men, things, &c. He pointed out also the strong differences between the old Saxons and the Saxons who invaded England. He referred to Spruner's Historical Atlas for the definition of the ancient limits of the Saxon peoples.—In the discussion which followed Mr. Hyde Clarke opposed the author's views, and considered that in these matters we had no better authorities than those heretofore accepted.—A paper on the Javanese, by M. Kiehl, who had resided there for some time, was also read, and interesting accounts of the religion, customs, agriculture, &c., of the Javanese were given. Mr. Campbell also spoke on the subject.

Physical Society, December 2.—Prof. G. C. Foster, president, in the chair.—The following candidate was elected a member of the Society:—G. Waldemar von Tunzelmann.—M. Janssen made a brief communication, in French, with reference to a method which he has proposed to the Académie des Sciences for ascertaining whether planets really exist between Mercury and the Sun. After mentioning the importance of photography from an astronomical point of view he explained his reasons for hoping that a series of solar photographs, taken regularly at intervals of about two hours at a number of places on the earth's surface would enable us to determine this question. As it is necessary that such observations be made at several places and in several countries, M. Janssen hopes that other countries besides France, will ere long arrange to have such a series of observations taken, and he considers that in a few years the circumsolar regions would thus be explored with a certainty which could not possibly be attained by any other method. He exhibited some of the original photographs taken in Japan of the transit of Venus, and explained the advantage of placing a grating in the focus of the camera, in order to eliminate distortion.—Mr. Crookes showed the spectrum of a small specimen of chloride of gallium which he had received from its discoverer, M. Lecoq de Boisbaudran. The discovery of this metal is of peculiar interest, as M. Mendelief had previously, from theoretical considerations asserted it to exist, and had also correctly given some of its chemical and physical properties. The most prominent line in the spectrum was a bright line in the blue somewhat more refrangible than that of indium.—Mr. Lodge briefly described a model which he has designed to illustrate flow of electricity, &c., and he showed how similar considerations can be applied in the case of thermo-electric currents. The model in its simplest form consists of an endless cord passing over four pulleys, and on one side of the square thus formed it passes through a series of

<sup>1</sup> *Phil. Trans.* 1864, p. 423.



buttons held in their positions by rigid rods or elastic strings, according as they represent layers of a conducting or non-conducting substance. When considered in connection with thermoelectricity, the buttons are assumed to oscillate on the cord, and if they move in one direction with greater velocity than in the other, the cord will tend to move in the former direction. Now at a junction of copper and iron an unsymmetrical oscillation of the molecules must ensue, and the cord, or electric current, will advance when two junctions are at different temperatures. Mr. Lodge showed experimentally that for a given difference of temperature the maximum thermo-electric current is obtained when one of the junctions is at 280° C., and beyond this point the amount of deflection decreases. This fact led Sir W. Thomson to discover the convection of heat by electricity; that is, if we have a circuit composed of copper and iron and one of the junctions be at the above temperature, the current in passing from hot to cold in the iron, or from cold to hot in the copper, absorbs heat. This fact was experimentally illustrated by Mr. Lodge. A strip of tin plate is symmetrically bent so as to nearly touch the two faces of a thermopile and is heated at the bend by steam passing through a brass tube on one side (not end) of the thermopile and kept cold by a current of water on the other side. As the arrangement is symmetrical no current is found to pass through the thermopile, but when a powerful voltaic current passes through the strip of metal, a distinct deflection of the needle is observed in accordance with the above law.

PHILADELPHIA

Academy of Natural Sciences, June.—Prof. Cope's fourth contribution to the history of the existing Cetacea, describes new species of *Globiocephalus*, *Phocæna*, and *Lagenorhynchus*. He also described (July 25) a new fossil genus of *Camelidæ*, *Protolabis*, with three upper incisors on each side.

PARIS

Academy of Sciences, December 11.—Vice-Admiral Paris in the chair.—The following papers were read:—Theorems relating to couples of segments making a constant length, taken the one on a tangent of a curve, and the other on a normal of another curve, the two curves being of any order and class, by M. Chasles.—On the composition of glass and crystal among the ancients, by M. Peligot. He proves by quotation and chemical analysis that the glass of the ancients differed importantly in composition from the ordinary glass of to-day; the proportion of lime was much smaller (our good glasses contain 12 to 15 per cent. of their weight of it). Further, no test or analysis proves that the true crystal, the English *flint glass*, was known to the ancients. The honour of its invention belongs to us.—General method of analysis of the tissue of plants, by M. Fremy. He finds the principal tissues of plants, after exhaustion by neutral solvents, to be formed by organic association of (1) cellulose bodies (cellulose, paracellulose, metacellulose); (2) vasculose; (3) cutose, (4) pectose, (5) pectate of lime, (6) azotised substances, (7) various mineral matters. He indicates the reagent for each.—On a polymer of oxide of ethylene, by M. Wurtz.—Results obtained on phylloxerised vines by their treatment with sulpho-carbonates, manures, and compression of the ground, by M. Marés.—M. de Lesseps presented a report by M. Roudaire on the results of his exploration of the Tunisian Chotts. An extract from an Arabic manuscript found at Nafta shows that the sea at one time reached to that place. M. Roudaire points out the advantages of the new project.—On the laws of impulse (*entrainement*) in plants, by M. Baillon.—A new chapter added to the history of hybrid *Agilops*, by M. Godron.—Researches on the structure, the modes of formation, and some points relative to the functions of the urns in *Nepenthes distillatoria*, by M. Faivre. The urn is a special formation, *sui generis*, belonging histologically to the foliaceous type, derived from the peduncle, which itself is a continuation of the median nervure of the foliar lamina. The complex liquid of the urns proceeds from the plant itself; and liquid poured into the urns is partly absorbed.—On the caries of bones, by M. Brame.—Researches on the vitality of the eggs of phylloxera (third communication), by M. Balbiani. The upper limit of temperature, at which the eggs are rendered sterile, is 45°. (The experiments were made with hot water.) The eggs and insects brought out of the ground die quickly on being exposed in a dry place. The eggs by reason of the chitine of their outer envelope present much greater resistance to destructive agents than the individuals fully developed; and the germ or embryo is less surely affected by large doses of poisonous vapour acting for a short time, than by small doses, acting slowly and con-

tinuously.—On some processes indicated by Florentinus, for preservation of the vines and for the manufacture of wines, by M. Paulet.—On the geometric construction of the pressures which are borne by several plane elements which cross at the same point of a body, by M. Boussinesq.—Note on the integration of the equation—

$(xdy - ydx)(a + bx + cy) - dy(a' + b'x + c'y) + dx(a'' + b''x + c''y) = 0$ , by M. Allégret.—On the spectrum of the new star of the constellation of Cygnus, by M. Cornu. [See separate article.]—Note on the correction of variations in the working of astronomical pendulums arising from differences of atmospheric pressure, by M. Redier. Above the bob is fixed an aneroid barometric case, and a mass connected with the flexible wall of this rises or falls, thus compensating the effects of the pressure.—Note on the theory of the radiometer; extract from letter of Mr. Crookes to M. du Moncel.—Liberation of ammonia observed after rupture of certain bars of steel, by M. Barré. This was verified with red litmus and yellow turmeric paper; which applied to the moistened fracture, changed to blue and brown respectively. Bubbles of gas rose from the moistened surface during a quarter of an hour. The effect is got in Bessemer steels, and in steels obtained from the Siemen's furnace. M. Daubrée remarked that M. Fremy had observed the liberation of ammonia from steels in the cold state in presence of steam. M. Boussingault had discovered nitrogen in all the meteoric irons he had examined, and M. Cloëz had found ammonia in the state of chlorhydrate and carbonate, in the meteorites of Orgueil. He thinks the gaseous bubbles indicated also the presence of a gas less soluble in water than ammonia.—Researches on the urea of the blood, by M. Picard. There are in arterial blood two different substances, both decomposed by Milon's reagent; the one, very destructible, disappears almost completely in the capillaries; the other is fixed, resistant, and exists in venous blood in the same quantity as in arterial (it is probably the urea of the blood).—On the fixed cells of tendons, and their lateral protoplasmic expansions, by M. Renant.—On a dust shower at Boulogne on October 19, and on the mode of formation of earthy rains in general. The dust was mostly of *débris* of various microscopic algæ, with grains of silica and lime; it was probably raised from the beach. He thinks the organic matter in such showers is not, as often supposed, derived from the air.—The height of the quaternary glacier of La Pique, at Bagnères-de-Luchon, by M. Piatte.

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