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THURSDAY, JUNE 24, 1875

CROLL'S "CLIMATE AND TIME" *

Climate and Time in their Geological Relations; a theory of Secular Changes of the Earth's Climate. By James Croll, of H.M. Geological Survey of Scotland. (London: Daldy, Isbister, and Co., 1875.)

II.

MR. CROLL'S own theory about the distribution of heat by means of ocean currents is in intimate connection with his ideas as to the variation of climate in past time. His theory may be summarised as follows:—The Gulf Stream and other warm or cold currents are due entirely to the prevailing system of winds, which force the water along the surface, or even make it take a lower course; the return of the colder water from the Arctic regions being assisted by the difference of level caused by driving up the waters into a narrow basin, such as he supposes those regions to be. The result of this theory is, that if one hemisphere is colder than the other, the trades on that hemisphere will be strongest, and the resulting warm current will flow into the warmer hemisphere; any difference, therefore, in the mean temperature of one hemisphere from that of the other is augmented according to this theory by ocean circulation, whereas on Dr. Carpenter's theory the latter would have a counteracting influence. When, however, we take both theories into account, and also the effect of the distribution of land and sea, which is remarkably manifested by the two facts of the South Atlantic being coldest and the North Pacific also coldest, we see that we are not in a position to estimate the effect, if any of much consequence, of the different forms of ocean circulation on the occurrence of a glacial epoch, but must look for the causes of the latter independently.

Now what are the known facts to be explained? They are well described in various parts of this book, and the proofs of the less known are carefully given. We have first the widespread indications of a sheet of land ice in the northern hemisphere, reaching in some parts far down into temperate, if not into tropical regions; secondly, similar indications in the southern hemisphere during the same geological period, but without any proof of their being contemporaneous even in centuries with those in the northern; thirdly, a much milder climate than at present prevailing in very high latitudes in comparatively modern geological periods, though anterior to the glacial epoch; fourthly, that these changes from more intense cold to more intense heat have been going on throughout the whole of geological time; and lastly, that in the midst even of the glacial epoch, warm interglacial periods occurred. No satisfactory theory of the cause of the glacial epoch can leave any of these facts unaccounted for, still less contradicted. Sir Charles Lyell's theory, referring it to an alteration of the distribution of land and sea, does not well adapt itself to the magnitude of the phenomena indicated above in the first and second facts, and requires very sudden and violent changes to account for the fifth; and, moreover, it is shown by Mr. Croll that the distribution he indicates would have the very opposite effect to that supposed; geologists are therefore driven, however reluctantly, to consider the action of cosmical causes. Four theories founded on such causes have been proposed.

The first, that the solar system was passing through a cold region of space, may be dismissed at once; the second is that the sun is a variable star, and therefore the amount of heat received from him is variable; the third is, that the glacial epoch was due to a great obliquity of the ecliptic; and the fourth, Mr. Croll's, is that it depended on an increased eccentricity of the orbit combined with aphelion winters. We will discuss the last theory first, and examine Mr. Croll's proofs of it. In order to show how the eccentricity has varied in past time, and to find the periods at which it was a maximum or minimum, Mr. Croll has calculated by means of Leverrier's formula what its amount has been or will be, from 3,000,000 years past time to 1,000,000 years in the future, for intervals of 50,000 years, and has given a diagram and tables to illustrate the result. This must have been a most laborious task, but we are sorry to say that the results require confirmation. We have repeated the calculations for two of the most remarkable dates, near which the change is represented by Mr. Croll as very rapid from a maximum to a minimum, viz., 850,000 and 900,000 years ago respectively, and find that at the former date the eccentricity was ·0697 instead of ·0747, and at the latter date was ·0278 instead of ·0102 as expressed in the table. To satisfy ourselves that the mistakes are Mr. Croll's and not ours, we have recalculated also one of Mr. Stone's and one of M. Leverrier's results which have been used by Mr. Croll for the completion of his table, and in both instances have exactly verified them. The fact that the eccentricity was large when he represents it so, and small when he makes it small, seems to indicate that some approximating progress has been followed, and that possibly his diagram may give a *rough* idea of the changes of eccentricity for past time, provided of course that we agree to Leverrier's formula being used for such remote periods.

Assuming, however, that at some past date the eccentricity of the earth's orbit approached its maximum value, and that at the same time the winter of one hemisphere occurred in aphelion, what would be the result? In the first place the total annual heat received from the sun, which varies inversely as the minor axis of the earth's orbit, would be slightly increased, but not sufficiently to have much effect upon climate. The more important result would be that the hemisphere whose winter was in aphelion would have it very rigorous, and its summers would be very hot, while the other hemisphere would be enjoying a perpetual summer. It is on this that Mr. Croll relies for producing a glacial epoch, and we see that it involves the statement that the two hemispheres were *not* glaciated at the same time, while the other theories assume that they were.

Our question therefore is: Will an extreme difference between the winter and summer temperature produce a glacial epoch? The actual amount of heat received by either hemisphere may easily be shown to be the same, whether there are great or little differences between summer and winter, whether as to their length or their intensity, so that a glacial epoch could not be the *direct*

* Continued from p. 123.

result, and we must look to the indirect effects. While agreeing in the existence of many of those pointed out by Mr. Croll, we cannot think it quite so settled a matter as he does, as they do not all act in the same way. In the first place, though the total amounts of summer and winter heat together are equal in the two hemispheres, yet, since a larger proportion of the greater summer heat is *available* than of the smaller winter heat, the more unequal these are, it follows generally that *more* heat must be obtained, and therefore the more uniformly heated hemisphere will be coldest; but secondly, as Mr. Croll states, we must consider the formation of snow, *i.e.* take into account the latent heat of water and other physical properties. Some of his arguments on this point are rather circular, for whatever amount of heat is rendered latent in the melting of ice, as much will be supplied to radiation in the freezing; and no *increase* of ice would arise from this. There are, however, two points that seem to be made out. First, that snow and ice are better reflectors of light than almost any other substance, and therefore less heat enters into them; and, secondly, that moist air is much less transparent to heat than dry, so that the vapour raised by the sun in summer would be an *opposing* influence, whereas the frozen vapour in winter when fallen as snow would leave the air above freer for radiation. This result would overbalance that spoken of in the first place, and be a powerful influence in the production of a glacial epoch. The vapour, too, that was raised in summer would come in a large degree from the warmer tropics, and therefore continue to add each winter to the mass of the snow and ice in the more polar regions.

These seem to us to be among the most convincing of Mr. Croll's arguments, and they are in agreement, as he shows, with the condition of the earth at the present time as regards the more glaciated condition of the southern hemisphere, and they agree with what has been pointed out by Prof. Tyndall, that heat, to bring the snow in form of vapour, is just as necessary for a glacial epoch as cold to freeze it when brought. It has been argued by Mr. Murphy that under exactly the same circumstances it would be the more equally heated hemisphere that would be glaciated, as the cool summer would melt less snow; but according to the above theory the summer of the other hemisphere, though naturally hotter, would also be rendered cool at the earth's surface. We see that the whole of this argument depends on the relation of the atmosphere to heat rays, and what has been stated above has been experimentally verified; yet we are far from being fully informed on this point, and the example of the planet Mars, which is almost exactly under the circumstances of great eccentricity and winter aphelion supposed above, and yet has not much glaciation, teaches us that this may depend on other combinations of circumstances beyond those we have considered above.

The glaciation, Mr. Croll thinks, would be assisted by the deflection of ocean currents, on which he accordingly spends his strength; but the vertical circulation of Dr. Carpenter, no less proved than the influence of the Gulf Stream, would be antagonistic to this, and we may safely leave the unknown residuum out of consideration.

Such is Mr. Croll's theory of the cause of the glacial epoch, to the illustration of which he brings forward many interesting facts. Among these are the proofs he gives of the occurrence of warm interglacial periods. Some of these proofs are collected from other writers, but many are from his own observations, and consist of the intercalation of beds of fossiliferous sand between two masses of boulder clay, the fossils being often of a southern rather than of a northern type. He also refers to the records of borings collected by him and already published, which showed, in several instances, three, four, or even five boulder clays in succession, separated by stratified sands. These interglacial periods are certainly more easily accounted for on Mr. Croll's theory than on any other, as, owing to the numerous terms on which it depends, the eccentricity of the earth's orbit is liable to rapid changes. Many of the instances, however, of interstratified fossiliferous sands seem too insignificant to require so vast an apparatus as a cosmical cause to account for them; rather are they evidences of the dependence of temperature on the atmosphere, whose changes are much more comparable to those of limited beds. Another set of facts adduced by Mr. Croll in illustration of his theory is the evidences we have of glacial conditions in former geological periods, of which he gives a very useful summary, though it seems to us he goes too far in taking proofs of a *warm* climate to indicate glacial epochs preceding and succeeding it, on the ground that all warm periods *must* be interglacial—this is *lucus à non lucendo* truly. Indeed, the warmth of North Greenland in the Miocene period seems to us one of those facts which are not satisfactorily accounted for by the theory—for the eccentricity has seldom been much less than now—and our northern winters are in perihelion.

He thinks he can identify the glacial period proper, and those of the Eocene and Miocene periods, with portions of past time when the eccentricity has been great and yet rapidly changing to small; and attempts thus to get a measure of the length of a geological period, and hence with the aid of other theories and supposed measurements to arrive at the total length of past geological time. These speculations may be ingenious, but they can give no assistance to the solution of a problem of which we really have not yet the data. The title of the book leads us to believe that all the discussion about the glacial epoch is engaged in only to lead up to this, but we must regard that as a much more manageable and therefore interesting problem, and turn now to examine the other theories that have been broached to account for it.

The theory of the sun being a variable star is not in such an advanced state as to warrant a complete discussion from this point of view, and we have seen that mere absence of heat can never cover the land with snow and ice, and this theory therefore may be dismissed.

The only remaining one is that which accounts for it by increased obliquity of the ecliptic. This theory, which has recently been broached in different forms by Lieut.-Col. Drayson and Mr. Thomas Belt, has been espoused by Mr. Woodward in his address to the Geologists' Association, whose paper has been deemed worthy of insertion in the "Arctic Manual." Col. Drayson's form of it, which imagines that the whole mass of ice was formed every winter and melted every summer, may be dismissed as absurd. Not so Mr. Belt's. There can be no doubt that an increase in the obliquity of the ecliptic would cause a greater difference in the seasons, and this difference we have seen to be the very basis of Mr. Croll's own theory; the results must be the same (and they are rightly seen by Mr. Belt), whatever may be the *cause* of the difference between summer and winter temperature. If this theory were the true one, it is plain that both hemispheres were glaciated at the same time, so that both theories cannot be true; but the matter of fact as to the synchronism or otherwise of the glaciation of two hemispheres can never in the nature of things be determined. But we have still left the question, Has there been or can there be any great change in the obliquity? Astronomers say no. Mr. Belt, however, thinks that the distribution of sea and land and similar causes *may* make it possible for greater changes to occur—a gratuitous supposition that Mr. Croll shows to be groundless. This cause, then, though it may have the general effect of lowering the temperature of temperate and Arctic regions, is not sufficient to cause a glacial epoch.

On the whole, then, there appear to be several independent cosmical causes which affect climate in a greater or less degree, and the probable truth is that a glacial epoch occurs when they all conspire to bring about the same result.

So far, by going from chapter to chapter, we have endeavoured to bring Mr. Croll's arguments into something like logical order. The remainder of the book scarcely admits of this; indeed, we think the author might well have bestowed more care in arranging his matter if it was intended to form a consecutive whole; as it stands, there is much that can only be called a miscellaneous collection of essays without any obvious connection. Among these are his accounts of observations on the North of England ice-sheet, and his speculations as to the direction of its motion. There are also two theoretical questions of great interest discussed—"The physical cause of the submergence and emergence of the land during the glacial epoch," and "The physical cause of the motion of glaciers." With regard to the first of these questions, there are undoubted proofs that great oscillations of the relative level of land and sea have taken place in recent geological times, and the question arises, Was it the land which sank and rose, or the sea which changed its level? It was rightly considered one of the grand discoveries of geology when it was first taught that the changeable sea was that which retained its constant level, and that the "eternal hills" had been but as yesterday beneath the waters; and this principle is not likely to pass away. By it all alterations of level have been ascribed to the motion of the land, and none to the rising of the sea. While agreeing, however, to the principle, we may doubt its universality, and may be prepared to entertain the question whether causes of limited extent may not operate to raise the level of the sea, and thus enable us to account more naturally for such rapid changes as are sometimes indicated. There can be no question but that any considerable amount of water which by the fact of freezing should be retained in either polar region, and form an ice-cap there, would correspondingly shift the earth's centre of gravity and draw the remaining water more over to the side on which the ice-cap lay; and the amount of elevation of sea-level might easily be calculated for any latitude, if we knew the extent of the cap and its manner of deposition, *i.e.* its shape; and the amount would be doubled if the ice-cap were first on one hemisphere and then transferred to the other. This calculation Mr. Croll attempts to make on the very ingenious method of approximation that supposes the ice-cap such as shall make the earth with the cap on one side a perfect sphere. The question can be worked out more directly, as has indeed been done, though with varying results, the mean of which indicates that the rise at one pole due to this cause would be about one-fifteenth of the thickness of the ice melted off the other. If, therefore, we want to account for an alteration of level of 500 feet in England, corresponding to about 600 feet at the pole, we should require to have somewhat less than two miles' thickness of ice on the antarctic regions now. While these figures represent data too far removed from the truth to be at all reliable, and there are, moreover, other causes that may affect the result, they serve to show the kind of thickness required—that it is not *twenty* miles, for instance. Are we prepared, then, to admit that there may be two or three miles of ice on the south pole? This does not appear to us at all an extravagant assumption, when icebergs have been met with 700 or 800 feet out of water, and which must therefore have been considerably more than a mile in total height. We do not think it therefore unreasonable to suppose that during the glacial epoch, or indeed at other times, when there was less ice at the south pole than now, the sea in our latitudes may have stood at a higher level, and that many of the elevated marine deposits and raised sea beaches are due to this cause, and not to depression of the land; for the latter we have no other evidence, and it would involve such vast changes in so recent times that we can scarcely believe would leave all the main valleys and hills as they were before the glacial epoch, and afford no evidences of post-glacial faults. This argument of course does not deny that there *have* been land oscillations during the period, but only that they are not the only ones.

This leads us to the last of the theoretical questions discussed by the author of this work—the physical cause of the motion of glaciers, the answer to which appears to depend upon what is the amount of the shearing force of ice. The remarks which Mr. Croll makes on the theory and experiments of Canon Moseley are very forcible. There is no doubt that the element of time enters largely into the amount of force required to shear ice, and that during this time heat is acting on the ice also, and consequently that satisfactory experiments can only be made on a glacier itself; and also that the theory of the dependence of glacier motion on *change* of temperature will not account for the greater descent in summer than in winter. But what is Mr. Croll's own theory? He, like Canon Moseley, calls in the agency of heat, and indeed, since heat obviously makes a difference in the amount of motion, we have only to find out *how* it makes this difference to determine the cause of the whole motion. He considers the motion of a glacier molecular, that the heat entering at one end melts the first molecule, which then descends by its weight and leaves room for the molecule above it to descend, when *it* melts. This may look very pretty at first sight, but the first molecule would never descend and *leave a vacuum behind it*; so the second molecule must melt at the same instant, and so on to the other end of the glacier, which is absurd; and besides, what is there in this theory to distinguish a glacier from a common piece of ice? which on this principle ought to flatten out and not retain its shape as it does. Why also are we to suppose the molecule alternately to melt and crystallise when the heat is continuous? The mistake on which this explanation is founded seems to be the confounding of radiation with conduction. It is radiant heat that passes through ice, which is a very bad conductor. Ice at 32° F., heated by conduction, would certainly melt on the outside; the interior can only melt by the *absorption* of radiant heat. We cannot either understand the statement "that ice at 32° cannot take on energy from a heated body without melting," unless it is the exact equivalent of what we have just said; but then no heat could be transmitted, as it would be consumed in melting the ice, and if it were otherwise, still any amount of heat short of the latent heat of water might be "taken on" by a molecule without melting it.

We fear, then, that the complete account of the descent of a glacier is still a desideratum. The various theories may contain elements of truth, but none are entirely satisfactory.

As far as definite results are concerned, it will appear that Mr. Croll's book does not do all he hopes it may, yet we welcome heartily his attempts at reducing complex questions to arithmetical issues, for we thereby gain clearer ideas as to whereabouts the truth may lie, and certainly have the questions put before us in a more definite form. The vast problems with which he deals, and for the suggestion and discussion of which science is so largely indebted to him, are waiting for solution, and every attempt is valuable, both as showing us where to look and where *not* to look for help.

J. F B.

SPRAGUE'S ELECTRICITY

Electricity; its Theory, Sources, and Applications. By John T. Sprague. (London: E. and F N. Spon, 1875.)

THE author tells us in his preface that this book is "written chiefly for that large and increasing class of thinking people who find pleasure in the study of science, and seek to obtain a full and accurate scientific knowledge for its own sake, or as part of the necessary mental preparation for many of the departments of modern life." Our examination of the book itself would lead us to an opposite conclusion. We very much question whether any one of the class to whom the author refers will ever have the patience to read through this volume. Certainly they will have but sorry pleasure and anything but full and accurate information. The book abounds in foolish conceits advanced with a show of knowledge that cannot but repel every intelligent reader.

That we are justified in these strictures will be seen from one or two quotations. Here, for example, are some statements taken from chapter ii. in this book. At the outset the author asserts that the fundamental facts relating to frictional electricity given in "one of our standard electrical works (and it is just what all say) . are received as absolute truth by electricians . . and yet there is scarcely a truth in them which is not overweighted by an error, and the simplest facts even are erroneously stated" (p. 17). Mr. Sprague, so far as we are aware, has never done anything to prove that he is able to sit in judgment on the intellectual giants among modern men of science. Mere off-hand condemnation of the laborious work of men like Sir W. Thomson and Prof. Clerk-Maxwell cannot for one moment be tolerated. Mr. Sprague seems to us to be like a child trying to turn one of the pryamids of Egypt upside down because he imagines it has been built the wrong way up. The best teaching is to let him try. This is how the author proceeds in his bold attempt. It is not true, he states, that bodies similarly electrified repel each other; "the repulsion is only apparent; the real cause of the motion is to be found in the attraction exerted by surrounding bodies" (p. 19). And with regard to the electrophorus, "that the dish forms the conductor from the dielectric to the earth, as all electrical books tell us, is an error which will come up for examination by and by" (p. 15).

According to Mr. Sprague the common explanation of induction is all wrong; "the real explanation is" given by him (p. 49). The rubber of an electric machine "is seldom made upon true principles" (p. 33); and as for the earth-connection to an electric machine, we are assured that it is merely imaginary; what we must do is to lead a chain to the floor or gas-pipe, and "hence the idea that we make a connection with the mass or surface of the earth" (p. 29). And further on (p. 40) we read—still concerning the machine—that "because both the poles are insulated and the circuits limited, we are freed from the *ignis fatuus* of the earth-connection." We presume the author does not mean the earth-connection is an *ignis fatuus*, but that the usual explanation is such; it is evidently so to him, for it has landed Mr. Sprague in a quagmire of crudities where we will not attempt to follow him. In these early chapters everything is attributed to "polarisation," a word which has for the author a consoling sound like that "blessed word Mesopotamia." We are told that it is for a similar cabalistic reason electricians employ the term "potential." Not understanding the term, and yet finding it necessary to say something about it, this is how the author discusses the subject: "The word [potential] is always used in place of tension or electro-motive force, because there is something full and smooth sounding about it; but the idea which really does belong to it is a pure mathematical abstraction which only highly trained minds can apprehend" (p. 154).

In another part of this book we meet with dark hints upon "Sprague's patent universal galvanometer," an instrument that is to "do for many purposes, without other instruments and without calculations, the work which at present requires the Wheatstone's bridge and expensive resistance coils, as well as many calculations." But, beyond exciting our curiosity, the author declines to go further, and so we cannot give our readers the benefit of this wonderful galvanometer, which combines "Pyscho" and "George Bidder" in one.

Notwithstanding the grave defects that quite spoil the early chapters in this book, it is only just to the author to point out that the latter part of the volume has considerable merit. Much useful practical information is to be found in the chapters on electro-metallurgy, a subject that is discussed with great detail, too much so, however, for a general treatise. The author has evidently been at no little pains to collect the numerous tables he gives, and in some instances they are the results of his own experiments. There is also a freshness and originality in the treatment of the sections on resistance and electromotive force that make us regret Mr. Sprague did not submit his theoretical views to some scientific friend before sending his work to the press. If the author had confined himself to the practical part of current electricity we should gladly have recommended his book to our readers.

OUR BOOK SHELF

Anales del Museo Publico de Buenos Ayres para dar a conocer los objetos de Historia Natural nuevos o poco conocidos conservados en este establecimento. Por German Burmeister, M.D., vol. ii. (Buenos Ayres and London: Taylor and Francis.)

In previous numbers of NATURE (vol. iii. p. 282, and vol. vii. p. 240), we have given some account of the important work which the well-known German naturalist, Dr. Burmeister, is now carrying on at Buenos Ayres.

The number of the *Añales* now before us completes the second volume of this remarkable work, and gives us additional proof of the extraordinary richness of the extinct Mammalian Fauna of the Argentine Republic, to which Dr. Burmeister has devoted so much attention. The Monograph of the Glyptodonts, or extinct gigantic fossil Armadillos, which is now brought to a conclusion, is certainly one of the most valuable contributions to palæontological science that has been produced of late years, and deserves the hearty commendation of all naturalists. This is more especially the case when we consider the difficulties under which the work has been carried on—in a new country, where every man *avidus lucri* is striving to advance his own material interests, and science and all that pertains to it are at an utter discount. On one occasion, we have been told, when one of the most perfect of these Glyptodont skeletons came into the market, the authorities of the National Museum were unwilling or unable to raise the necessary funds to secure it, and it would have left the country and been lost to Dr. Burmeister and his Monograph, had not an English friend found the money. Then, again, the necessity of having the plates lithographed in Europe must add greatly to the difficulties of the undertaking. Under these circumstances we may fairly congratulate Dr. Burmeister and science on the occasion of the second volume of the Annals of the Public Museum of Buenos Ayres having been brought to a successful conclusion.

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.*]

Peculiarities of Stopped Pipes, Humming-tops, and other Varieties of Organ-pipes

The peculiarities of a stopped organ-pipe as compared with an open organ-pipe are many and suggestive, and are of the utmost importance to the investigator both to know and to interpret. Without entering deeply into the principles of the craft of organ-building, there are certain matters of fact very necessary to be known before the full bearing of a theory can be estimated or its consistency be judged with true understanding.

By far the greater portion of an organ consists of pipes of the structure called "flue-pipes," or, as here named, "air-reed" pipes, and these are of two classes, the open and the stopped; also they are of two kinds, wood and metal. We have to notice how differently these two kinds are constructed to attain the same ends. In the metal pipe every part is to all appearance immoveable. In the wooden pipe the under-lip, or technically the "cap," is the only adjustable part, and is fixed in position by two or more screws. Within the mouth there is a platform filling the space beyond the windway; it is called the "languid," and it is by varying relatively the level of the edge of the cap to the edge of the languid that the direction of the stream of air is determined; if the cap is set low the angle of flow outward is increased, contrariwise it is lessened, and the art of the voicer decides to the finest degree what is requisite for the quality and speech of each particular pipe. If the wind is much thrown outwards, the speech is slow; if more inward, the speech is quickened; if too much inward, the octave sounds instead of the ground-tone; if too much outward or inward, the pipe will not speak at all. One more power of adjustment remains—the width of the narrow slit through which the wind issues is capable of being varied by alteration of the inner surface of the cap; a wide windway gives a stiffer air-reed, a fine windway gives a thinner one. In a metal pipe we have precisely the same capability of variation, only that we effect our purpose by pressure; the languid is moved higher or lower, not the cap. By means of a rod introduced at the foot or at the top of the pipe, we tap or press the languid into the desired relation to the edge of the underlip. We can also press the upper lip forward or backward; we can, by a like process, reduce the windway or enlarge it as easily. Very simple, yet very beautiful, compensations. In the variations of construction, nothing is done without purpose, nor can you make any one of these minute changes without causing at the same time a flattening or sharpening of the pitch, or a diversity in intonation or quality.

The above details all tend to one point, which I wish to press upon your attention; one distinctive feature belongs to the stopped pipe: the languid is lower than in an open pipe, else the pipe does not attain its proper speech. Consider it well, for it is a fact full of meaning. A necessity of an opposite kind exists in the nature of an open pipe; its demand is that the current shall have determination more to an outward flow. The cause of so essential a distinction between the two classes of pipes will be explained in another paper.

Stopped pipes when they are deep-toned are called "Bourdons," the name the French give to the Humble Bee for its "drowsy hum." Our plaything, "the humming-top," is a true bourdon, is a revolving organ-pipe, has a vibrating air-reed, its principle of action is "suction by velocity," the abstraction of air particles by velocity of *rotation* causing a partial vacuum just as in the stationary organ-pipe by velocity of *passage* of a current of wind.

Bearing in mind the working power of the air-reed, we are brought to consider the effects of the dimensions of the pipe and consequently of the form as well as the extent of the air-column whereon this power is impelled to act, and it is necessary to recur in passing to the question of length. Scientific writers affirm that the length of an organ-pipe for a given note corresponds to the length of the wave in air with an absolute relation, thus expressed. Prof. Tyndall says: "The length of a stopped pipe is one-fourth that of the sonorous wave which it produces, whilst the length of an open pipe is one-half that of the sonorous wave." Prof. Balfour Stewart says, in his "Elementary Lessons in Physics": "In an organ-pipe of this kind, the upper end closed, the primary note is that of which the wave length is twice the length of the pipe . the wave length of the sound produced by an open pipe is equal to the length of the pipe, so that it is only half of that produced by a shut pipe of the same length." (One is curious to know why there is this difference of statement from two leading teachers of men; perplexing to the student in want of a leader). Prof. Tyndall demonstrates his affirmation, showing that a stopped tube resounds to the note mid C of 256 vibrations per second; the wave length in air of this note he states to be fifty-two inches; then in proof he measures the jar or tube, and says, "by measurement with a two-foot rule I find it to be thirteen inches, precisely a fourth of the wave length." He then proceeds to affirm the same of organ-pipes, and proves it by tuning-forks and by sounding the pipes to the same note, and believes he has justified his assertions. His hearers do him that justice, and go home believing also. The proof is, however, altogether illusive. No speaking organ-pipe of that length ever gave the note of that pitch. Let us put the assertion to the test. My object lies beyond the rectification of a philosopher's misapprehension, and is meant to show not only that an organ-pipe behaves itself in a manner different to that with which it is accredited, but also why it does so; to show how important a matter in the nature of its action is this neglected difference, and how wide its bearing on the whole system of musical instruments. Here at my hand is a stopped pipe sounding mid C. I measure it interiorly from languid to stopper; it is eleven inches in length, and has a diameter of one-and-a-half inches. Here is an open pipe, same pitch, same diameter, and its length is twenty-three inches. Observe, our stopped pipe is half an inch less in length than half that of the open pipe; yet again notice, it is longer than that pipe would be if severed at the true nodal distance from the languid. How can we read eleven as precisely thirteen, and twenty-three as twenty-six inches? Under the strange notion that it is no matter if there is a difference, this has been done, and the truth of facts lost sight of or disguised in the convenient phrase, "approximately correct." The phrase assumes that there is a standard claiming nature's allegiance. We want to know, not what is correct, but what is true? Further, remark that if you stop the same open pipe at the top, the note obtained will not be an octave deeper, it will be nearly a tone sharper than that; if you stop the pipe at the centre, the note will not be the same as the open one, it will be considerably flatter; in neither case a good tone, since for its proper sounding in such condition the lip would require to be cut higher, mouth a little narrower, perhaps curved, and languid lowered. Every detail we come upon tells plainly of the working power of the reed affecting variably the results in pitch, and I think the reason for these distinctive sounding lengths will be discerned when we reach the consideration of the question of periods of vibration in pipes as tempered by rests.

The fundamental importance of the recognition that pipes of the same pitch varied between themselves as to lengths, was not perceived, nevertheless a qualifying condition was admitted that pitch was "affected by *depth* of the pipe, that is, its distance from front to back, but *width* does not affect pitch." As regards "depth," in no work within my knowledge does there exist any attempt at a solution of the problem how such a result ensues that depth interferes with pitch. It seems to be taken account of only as a disturber of the harmony of things, yet see how significant it is under the new theory of the working abstracting reed. The actual law operating admits of most precise statement when this generating power is acknowledged, viz., the difference of pitch in pipes of varied diameter (other things being equal) is proportional to the difference existing between the area of the cross-section of the pipe and the area of the mouth; the difference in pitch is greatest when the depth from front to back is greatest. It should be observed that increase of depth always flattens pitch, and tends to deprive the pipe of harmonic force. As regards the further assertion that "width" is without effect on pitch, this also is inexact and misses the very point which should have led to closer investigation. It is not true, because the same amount of wind acting over a wider area cannot do the larger extent of work with the same energy. The pitch of every pipe is affected by the width of mouth *relatively*, that is to say, its proportion to the diameter of the pipe. Apart from the ordinary rectangular and cylindrical pipes there are others of so-called "irregular shapes," which are usually viewed as monstrosities, out of the pale even of law padded with exceptions; yet these we shall find are the best evidence to us of the uniformity of the principle of action set forth in these papers, and of the consistency of a theory which recognises no exceptions.

Cylindrical pipes, notwithstanding their symmetry, differ greatly among themselves. The law by which flue-pipes differ has never yet been noticed, which is singular, since it is very striking when the pipes are thoughtfully observed, and gave the first clue to the theory of an aeo-plastic reed. A student well read in all that the best text-books in acoustics can teach, coming to the practical study of organ-pipes, and seeing in a grand organ so multitudinous an array of pipes, the unison pipes of the several stops conspicuous for diversities of diameter as well as of length, would naturally expect that here, if anywhere, he would find confirmation of Reynault's law, "The velocity of propagation of a wave of the same intensity in straight lines *is less according as the section of the tube is less.*" No! this small comfort is denied him; he is in a world of contrarieties; the law is abrogated; he will find the organ world *de facto* governed on principles the exact opposite, "*The velocity is greater as the section is less.*" Investigating further, he will find that, although in length the octaves of particular flue-stops examined are each very closely upon half the length of the other, yet their diameters do not follow a similar rule, for instead of octave or double octave being in that ratio, he must from the pitch note count to the seventeenth pipe before he will arrive at a pipe half its diameter. For other seeming anomalies, let him proceed to the stops called bassoon, trumpet, and tuba, and he will find that here increase of diameter demands not less length, but greatly increased length, to accompany increase of scale. Books of latest authorities will tell him that in an organ-pipe with a metallic reed "the note produced depends upon the length of the pipe rather than upon the length of the reed. In fact, when the note is established the reed obeys the impulses it receives from the air in the tube. Its use is accordingly rather to economise air and to give certainty and percussion to the striking of the note." Alas, it is inference by theory without test. Remove the whole of the eight or nine feet of the tube, leaving but the few inches of cup or socket, and you will have altered the pitch not more than a semitone.

All organ-pipes having metallic reeds act in conformity with Regnault's law, and the same holds good of wind instruments—trumpets, bassoons, and the like. All organ-pipes possessing air-reeds, flutes also, and some whistles, not all, display an opposite law. The musical tones of all in both these systems are the result of "suction by velocity," and the distinction is that in the former the intermittence is produced by suction under a *propulsive current*, and in the latter by suction under an *abstracting current*. The fact announces the law and leads to its explanation.

HERMANN SMITH

Faults and the Features of the Earth

MY attention has been drawn to an article in NATURE, vol. xii. p. 93, on an exploring party of the Geological Class of the University of Edinburgh to trace out a long fault in Scotland. In this it is stated that particular attention was devoted by the party to the connection between dislocations and valleys, and they came to the conclusion that not a single main valley ran along the fault they were tracing out. As an advocate of the theory *that faults or other breaks greatly induced the present features of the earth*, perhaps you may allow me to say a few words on the subject.

Fault-rock may be friable or hard; the first is inclined to induce valleys, the second peaks or ridges. Faults are of different ages, and therefore the features due to them are liable to be obliterated. Pre-Silurian features may be obliterated by the subsequent deposition of Silurian rocks, and so on upwards until we find many preglacial features obliterated by the glacial drift. In Ireland and Scotland we find more faults in the metamorphic rocks than in the overlying Silurians, in the Silurians than in the overlying Carboniferous and Old Red Sandstone, and in the Carboniferous than in the drift, while each newer accumulation obliterated, or perhaps, more properly, obscured the features in the older.

The fault examined by this party, from the brief description, seems, first, to have had a hard fault-rock, and second, its age to have been far from recent. Consequently, by the first, if the fault induced any features at all in the present surface, they ought to have been peaks or a ridge like that formed by the great Slieve-muck fault in Tipperary, Ireland; while if the second is correct, this fault ought not to form surface features, as any features due to the original fault were long since obliterated; also, the fault has been cut up and displaced by the more recent movements. If a valley chances to run along the line of an ancient fault, it probably was not induced by that fault, but by a much more recent break that for a greater or less distance coincided with the line of the older fault.

G. H. KINAHAN

Wexford, June 18

Salaries in the British Museum

AMONG your notes of last week is a favourable announcement of my promotion as an assistant in the Geological Department of the British Museum; but whilst thanking you, allow me to point out that it contains a grave misstatement as to the amount of remuneration I receive for my services (as a reference to the Parliamentary Returns will demonstrate); a misstatement alike unjust to the trustees and to myself.

May I venture to ask you to insert this, and so correct the erroneous impression which the paragraph conveys, as to the small amount of the pay received by myself and others in a similar position on the establishment.

British Museum, June 15

WM. DAVIES

OUR ASTRONOMICAL COLUMN

THE DOUBLE STAR Σ 2120.—In the notes to the last catalogue of measures of double stars of the late Rev. W. R. Dawes, he remarks with reference to Σ 2120, or, as it has been frequently called, Herculis 210 (Bode): "This object discovered by Struve is undoubtedly a binary system—the position varying in a retrograde sense, and the distance diminishing." Notwithstanding this positive opinion as to physical connection of the components by so high an authority in this department of practical astronomy, an examination of the path of the companion up to the latest published measures of the Baron Dembowski towards the end of 1870, or, it should be stated, through a period of observation fifteen years longer than that upon which the above opinion was expressed, does not support the presumed binary character of the object, but on the contrary, when the apparent fixity of the principal star is considered, shows pretty decidedly that the variation of angle and distance must be owing to proper motion of the smaller one. In fact we may represent the measures, from Struve's earliest in 1829, to Dembowski's in 1870, by the following expressions:—

$$d \sin P = -1''{\cdot}6806 - 0''{\cdot}12044\,(t - 1850{\cdot}0)$$
$$d \cos P = +1{\cdot}7259 - 0{\cdot}10250\,(t - 1850{\cdot}0)$$

which formulæ imply a secular proper motion of the small star amounting to 14″·75 in the direction 226°·0; they may doubtless be somewhat improved by complete discussion of all the measures, and perhaps some one of the astronomical readers of NATURE may be able to say how measures in the present year are represented.

D'Agelet, Bessel, and Struve have meridionally observed this star.

THE "MIRK-MONDAY" ECLIPSE, 1652, APRIL 7–8.—The following elements of this long-remembered eclipse are founded upon the same system of calculation which furnished so satisfactory an agreement between computation and observation in the eclipse of 1715, lately detailed in NATURE:—

Conjunction in R.A. April 7, at 23h. 4m. 33s. G.M.T.

	° ′ ″
R.A.	17 42 14
Moon's hourly motion in R.A.	33 0
Sun's ,, ,, ...	2 18
Moon's declination	8 24 17 N.
Sun's ,, ...	7 31 40 N.
Moon's hourly motion in Decl.	17 5 N.
Sun's ,, ,, ...	0 56 N.
Moon's horizontal parallax	60 16
Sun's ,, ...	9
Moon's true semidiameter	16 25
Sun's ,, ...	15 57

The sidereal time at mean noon April 8 was 1h. 9m. 20s., and the equation of time 1m. 38s. subtractive from mean time. The middle of general eclipse April 7, at 22h. 21m. 30s.

Hence the following points upon the central line in its track over the north of Ireland and Scotland:—

Long. 8° 25′ W.,	Lat. 50° 21′ N.	Long. 4° 8′ W.,	Lat. 55° 3′ N.
,, 7 50	51 0	,, 2 47	,, 56 28
,, 6 3	53 0	,, 1 20	,, 57 54
,, 5 26	53 42		

At Carrickfergus, where Dr. Wyberd observed the eclipse as described in the "Philosophical Transactions," totality began at 22h. 8m. 34s. according to the above elements, and continued only forty-four seconds. At Edinburgh it commenced at 22h. 22m. 55s., the duration being 2m. 47s. with the sun at an altitude of 39°; and at Arbroath at 22h. 26m. 23s. with the same duration, these being local mean times. Probably there may be other accounts of this eclipse in existence than those commonly quoted when "Mirk Monanday" is referred to.

DIAMETERS OF THE PLANETS.—We give the following values of the apparent diameters of planets reduced to the mean distance of the earth from the sun and of their true diameters in English miles, as being perhaps as reliable as any that can be assigned from existing data. They are founded in every case upon the measures which from observational circumstances appear to deserve the greatest weight, and in the reduction to true values the solar parallax is taken 8″·875, and Clarke's diameter of the earth's equator is adopted. It would of course be idle to attempt to offer final numbers, where the difficulties attending observations and the differences between the results of the most experienced and favourably-circumstanced observers are so considerable.

	″	Miles.	
Mercury	6·35 ...	2,850	
Venus	16·95 ...	7,550	
Mars	9·305 ...	4,150	
Jupiter, Equat....	197·47 ...	88,200	Compression $\frac{1}{15{\cdot}54}$
,, Polar ...	184·76 ...	82,500	
Saturn, Equat....	166·82 ...	74,500	Compression $\frac{1}{9{\cdot}10}$
,, Polar ...	148·50 ...	66,300	
Uranus	68·57 ...	30,600	
Neptune	67·26 ...	30,050	

In fixing upon the apparent diameters of the bright planets it has been desired to adopt values which shall represent the actual arc values that are presented by the true diameters at the earth's mean distance. Many observations would assign larger values, but undoubtedly less trustworthy for computing real dimensions. As is well known, preference in such case is to be given to double-image over wire-micrometer measures, yet even if we confine ourselves to the former mode of observation we by no means secure great consistency of results.

SOLAR HEAT AND SUN-SPOTS

THAT the rainfall of certain parts of the earth tends to vary periodically with the sun-spots has been shown with considerable probability by Messrs. Meldrum and Lockyer, and Prof. Köppen * has detected a similar tendency in the temperature of the atmosphere, most distinctly shown (as might have been anticipated) at stations in the tropical zone. † These discoveries indicate that there is at least some ground for the truth of Sir W. Herschel's surmise, that the heat emitted by the sun undergoes a periodical increase and decrease, concurrently with the varying disturbance of the solar atmosphere, as evidenced by the number of spots and prominences on his surface. But except Mr. Joseph Baxendell, who has published two papers on the subject in the fourth volume of the Transactions of the Literary and Philosophical Society of Manchester (new series), I am not aware that anyone has attempted to investigate the more direct evidence afforded by observations of the black-bulb thermometer.

Mr. Baxendell's work was based on the observations of the Radcliffe Observatory at Oxford, and a series made at Eccles by Mr. Mackereth; the two series extending over the years 1859-66. He did not attempt to institute a direct comparison of the recorded radiation temperatures with the number of the sun's spots, a proceeding which would probably have failed to lead to any definite result in English latitudes and in so cloudy a climate; but took as the first term of his comparison the ratio existing between the excess of the maximum radiation over the maximum air-temperature in the shade, and that of the mean air-temperature over that of the wet bulb, or the dew point deduced therefrom. His conclusion was to the effect that the sun's heat undergoes a distinct periodical variation, coinciding with and directly as that of the spots.

* Zeitschrift der Oesterr. Gesellschaft für Meteorologie. Vol. viii. Nos. 16 and 17.

† It is to be noticed as a remarkable fact that Prof. Köppen finds the epochs of maximum and minimum temperature to correspond (not as might be expected from the results of the present investigation with the maxima and minima of sun-spots respectively), but approximately with the opposite phases; the *maximum* of temperature (in the tropics) preceding the *minimum* of the sun-spots by 0·9 of a year, and the minimum of the former the maximum of the latter by 0·1 year.

It is evident that India offers far greater advantages for investigating the variations of the solar heat than any European country can do, and as observations of the black-bulb thermometer *in vacuo* have now been registered at several stations during the last six or seven years, I have lately examined a portion of these, to see if they afford any direct evidence of a periodical graduated variation in the intensity of the radiation. The result is to me very striking, and if not absolutely conclusive as to the direct variation of the sun's heat with the number of the spots and prominences, certainly, as far as it goes, strongly confirms Mr. Baxendell's conclusions, drawn from indirect evidence.

It is unfortunate that owing to the fragility of the instruments employed and the necessity of exposing them freely, they are very frequently broken; and, as a consequence, the longest series of observations made with one and the same instrument extends over only five years. This is at Silchar in Eastern Bengal. The place is situated in lat. 25°, therefore beyond the tropic; and the climate being very damp and more cloudy than most parts of Bengal, it is not, perhaps, so favourably circumstanced for the present purpose as some other stations.

The means of the maximum sun-temperatures registered on clear days (that is, on days when the proportion of clear sky estimated at 10 A.M. and 4 P.M. did not average less than three-fifths) are given in the following table. The months of the S.W. monsoon are omitted, since in some cases they do not furnish a single clear day according to the above definition, and as a rule such days are too rare to contribute much evidence of value. I give for each month the number of clear days that have contributed to the mean.

TABLE I.—*Average maximum temperature of solar radiation on clear days at Silchar.*

	Days.	1870	Days.	1871	Days.	1872	Days.	1873	Days.	1874
January	24	124·8	25	127·1	27	122	21	121	19	121
February	19	130·4	20	130·9	20	125·8	19	128·2	8	128·2
March	15	137·2	19	135·7	23	133·8	17	132·4	10	134·3
April	12	142·6	17	139·1	13	140·5	12	134·5	5	139·8
May	10	144·7	15	142·8	14	143·8	5	140·6	6	146·5
October	16	140·7	19	136·7	9	141·3	7	140	5	146·4
November	23	132·2	27	126·3	15	131·7	20	127·7	10	143·1
December	29	124·7	25	121·3	18	121·5	23	121·2	14	136·7
Year	148	134·6	167	132·5	129	132·5	124	130·7	77	137

Did this table stand alone, the evidence of any periodical variation would be very doubtful. But we shall presently see that the irregularities that it exhibits are all but completely neutralised by the registers of other stations. It is easy to suggest their explanation, grounded on the fact to which all the registers testify, that the highest sun-temperatures occur, not on days registered as cloudless, but on those on which there is a considerable proportion of cloud, and frequently rain. Such days were numerous in 1874; while in 1871 (the year of sun-spot maximum) days without visible cloud predominated. Leaving the discussion of this question, however, as unnecessary in this place, I will give the combined results of Silchar and eight other Observatories variously situated, some in, and others beyond the tropical zone. These are:—

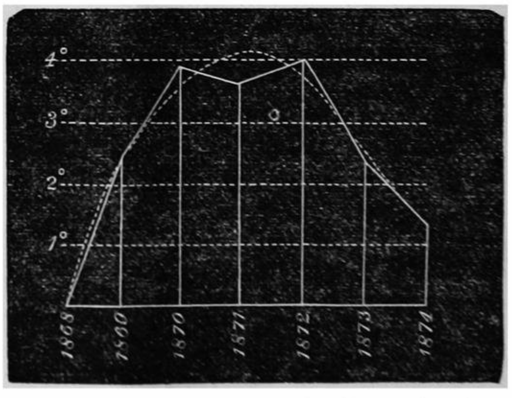
Port Blair, in the Andamans ... ... lat. 11° 41′ N.
Cuttack, in Orissa ... ... „ 20 29 „
Chittagong, on the Arakan coast ... „ 21 39 „
Jessore, on the Gangetic delta ... „ 23 9 „
Dacca, also on the delta ... ... „ 23 43 „
Hazaribagh,* elev. 2,000 ft. in Western Bengal „ 24 0 „
Berhampore,* on the Gangetic delta ... „ 24 6 „
Roorkee, elev. 900 ft. in the N.W. Prov. „ 29 52 „

Since the radiation-thermometers originally in use at most of these stations have been broken and replaced by other instruments, and since these thermometers (furnished by the best London makers) sometimes differ to the extent of many degrees when placed under the same conditions of exposure, it would be only misleading to compare together the registers of different years recorded with different instruments at the same station. In order to avoid this source of error, and at the same time to bring in evidence as much as possible of the registers, I have taken for each station separately the difference (rise or fall indicated respectively by + and −) of each pair of homonymous months in consecutive years, omitting all cases in which the instrument has been changed in the interval; and then the mean of all the differences thus obtained for the same pair of months. The results are given in the following table, additional columns being added to show how many stations have contributed to the mean of each pair of months. As in Table I., the mean temperatures compared are those of clear days only; but with the exception of Port Blair, I have admitted as clear days those only on which at least four-fifths of the sky on an average was estimated as unclouded at 10 A.M. and 4 P.M. In the case of Port Blair it was necessary to admit days with only one half of unclouded sky.*

TABLE II.—*Annual variation of mean maximum readings of black-bulb thermometers on clear days.*

	Station.	1868 1869	Station.	1869 1870	Station.	1870 1871	Station.	1871 1872	Station.	1872 1873	Station.	1873 1874
January	2	− 0·9	3	+ 0·5	4	+ 0·4	6	− 3·7	4	+ 0·4	8	− 5·3
February	2	+ 1·9	3	− 1·5	4	+ 0·6	6	− 3·6	4	+ 2·1	8	− 3·6
March	2	+ 3·8	3	+ 1·2	4	+ 2·6	6	− 2·4	5	− 0·7	8	− 1·7
April	2	+ 7·1	3	+ 1·5	7	− 0·5	5	+ 0·7	6	− 2·6	8	− 0·2
May	1	+ 14·2	3	+ 0·9	7	− 1·9	4	+ 2·6	7	− 0·7	8	− 2·5
October	2	− 4·3	3	+ 8·2	6	− 1·5	4	+ 4·0	8	− 4·7	8	+ 2·7
November	3	− 2·7	4	+ 0·4	6	− 0·6	4	+ 3·5	8	− 3·6	8	+ 0·8
December	3	− 1·0	4	+ 1·8	6	− 1·3	4	+ 2·2	8	− 2·9	8	+ 0·9
Year		+ 2·3		+ 1·6		− 0·3		+ 0·4		− 1·6		− 1·1

If these differences be plotted as the increments of a series of ordinates, and the curve thus marked out be corrected for its small irregularities *liberâ manu*, its resemblance in general character to the sun-spot curve will be distinctly apparent. (See figure.)



I have been unable to ascertain (here in Calcutta) the number of spots observed during the last few years; but this datum can readily be supplied at home.

Calcutta, May 28 HENRY F. BLANFORD

* The registers of these two stations taken alone give a curve nearly approximating to the resultant of all the stations, but it is of doubtful validity owing to the thermometers having been twice renewed at both stations.

* I have ascertained by direct comparison that any difference thus introduced is inappreciable, the results being treated comparatively, and not for absolute values.

LECTURES AT THE ZOOLOGICAL GARDENS†

VIII.

Mr. Sclater on the Pheasants.

IN that Birds possess a high temperature of the blood, they agree more with the mammalian than with other vertebrated animals; the balance of anatomical evidence

† Continued from p. 130.

is, however, in favour of their more intimate reptilian affinities. They are characterised externally by their covering of feathers, and by the fore limbs being developed to form wings. These wings, though primarily constructed for flight, in some birds perform other functions. In the Penguins they are employed for swimming, in the Ostrich to assist in running, whilst in the Apteryx and the Cassowary their condition is so rudimentary that they can be of no service to their owners. In the Night Parrot and the Weka Rails the wings are very much diminished.

Birds are divided into from seventeen to twenty well-marked groups, of which the Gallinæ, the order which contains the Pheasants, forms one which is more important in an economical point of view than any of the others, as it contains most of the domesticated species of birds, the ducks and pigeons being exceptions. The Game Birds, as the Gallinæ are commonly termed, may be divided into the following seven sections:—1. The *Pteroclidæ*, or Sand Grouse, birds which inhabit Africa and Western Asia. By some naturalists they are grouped with the Pigeons; they, however, differ from them and agree with the fowls in laying coloured eggs, at the same time that the young ones run about directly they are hatched. There is one species, found in the steppes of Tartary, in which, unlike its allies, the hind toe is absent. In the year 1863 a flock of Sand Grouse spread over all Western Europe. Prof. Newton tell us, in the "Ibis," that not less than seven hundred individuals must have appeared. A few stragglers were seen for a short time afterwards. 2. The *Meleagridæ*, or Turkeys, are unfortunately so called, as they are in their wild state confined to Northern and Central America. Only three wild species are known, the most northern of which is said to be the parent stock of our domesticated form, although some of the evidence is in favour of the latter having sprung from the Mexican species. The Ocellated Turkey, from Honduras, is a particularly handsome bird. 3. The *Numididæ*, or Guinea Fowl, are represented in Guinea by one species. The four or five others are all confined to Africa; of these, the elegant Vulturine Guinea Fowl, of which several specimens have been presented to the Zoological Gardens by Dr. Kirk, comes from Zanzibar. 4. The *Cracidæ*, or Curassows, are the representatives of the Game Birds in Central and South America. They will not nest in captivity here, perhaps because, as they are arboreal in their habits, it is not possible to give them suitable abodes. They are said to have been first introduced into Europe by the Dutch, from the island of Curassow, in the West Indies. In some species the cock and hen are identical in plumage; in others very dissimilar. 5. The *Megapodidæ*, or Megapodes, are confined almost entirely to the Australian region. They are nearly allied to the Cracidæ. Their eggs are laid in the middle of a mound composed of earth and grass, where they are left to be hatched. Many eggs are laid, and the young ones are able to fly within twenty-four hours of leaving the egg. Their breeding habits have been well described by Mr. Bartlett, from examples which have laid in the Society's Gardens. By one species the mound constructed is as much as 15 ft. high and 60 ft. in circumference. The habits of one peculiar species, the Maleo of Northern Celebes, have been well described by Mr. Wallace. 6. The *Turnicidæ*, or Hemipodes, much resemble quails. They are mostly African, one species occurring in Andalusia. Their anatomy is somewhat peculiar. 7. The *Phasianidæ*, or Pheasants, are constituted by (*a*) the *Tetraonidæ*, or Grouse, inhabitants of the mountainous regions of Europe and Northern Asia. In all the species the legs, and in some the toes, are feathered. They do badly in captivity. The best known of them are the Prairie Fowl, Capercailzie, Black-cock, and Ptarmigan. (*b*) The *Perdicidæ*, or Partridges, are found in every part of the Old World. The Snow Pheasant of the Himalayas is one of the largest species. The Impeyan Pheasant, from the same locality, is a closely allied form. These birds are represented in America by (*c*) the *Odontophoridæ*, or Colins, sometimes called toothed Partridges, because the bill is slightly toothed. They are much more arboreal than their Old World representatives, and none of them attain a great size. (*d*) The *Phasianidæ*, or Pheasants proper, form about forty species, arranged in seven genera. The story runs that the common Pheasant was first brought from Colchis by the Argonaut, whence its scientific name, *P. colchicus*. The genera include the *Crossoptilons*, or Eared Pheasants of Northern Asia, of which there are four species: the true Pheasant, preserved in this country; the *Thaumalea*, or Gold Pheasant, with its superb ally, the Amherst Pheasant of Central Asia, first made known from a specimen brought over by the Lady Amherst when returning from an embassy to the King of Ava. Further facts respecting its distribution have been obtained by Dr. John Anderson and Mr. Stone. The *Euplocami*, or Kaleeges, are represented by twelve species. They are intermediate between the Pheasants and the Fowls. A new species has been quite recently obtained by Mr. Gould from the interior of Borneo (*Lobiophasis*). *Gallus* is the name given to the genus which includes the Fowls, of which there are four species. The Jungle Cock of India is the wild ancestor of the domesticated bird; others are inhabitants of Ceylon and Java. *Ceriornis* includes the Tragopans, which are peculiar in having horned appendages to the head. There are five species in this beautiful group. (*e*) The *Pavonidæ*, or Peafowls, are natives of the forest jungles of India, and such being the case it is strange that they so well resist the winters of our own country. *Polyplectron*, or the Peacock Pheasant, is an allied form; it is aberrant, however, in that it is monogamous and lays only two eggs. The Argus Pheasant also belongs to the same family.

THE PROGRESS OF THE TELEGRAPH*

VIII.

MORE daring inventors, as we have seen, entered the field—Nott and Gamble, with a letter-showing telegraph; Edward and Henry Highton, who produced an array of signal apparatus, in some cases evading the Cooke and Wheatstone patents by the use of nickel for the electro-magnet in place of soft iron. But formidable beyond all other competitors was the talented Alexander Bain, the Edinburgh watchmaker, who has contributed largely to the improvement of the telegraph by his singularly beautiful adaptations and chemical printing arrangements. Expensive litigation speedily followed, and the directors in most cases compounded with their opponents. Alexander Bain was made a director of the Company, and at the same time received 12,000*l.* for his chemical printer, and most of the other opposing patents became the property of the Company by special arrangements with the inventors. By means such as these a monopoly for a time was secured, even though it was purchased at an exorbitant price. Monopoly at that time represented commercial gain, and every aspiring inventor was sooner or later run off his feet by the powerful and wealthy corporation. Such is the early history of the introduction and opening of the Electric Telegraph as a means of the transmission of inland intelligence. The telegraphic connection of Great Britain with the Continent of Europe at this time was scarcely developed, the extent of electrical communication by the continental land lines being circumscribed.

This, however, thanks to further applications of science, is no longer the case. The planet is now girt by telegraphs. First, there is the "Great Northern,"

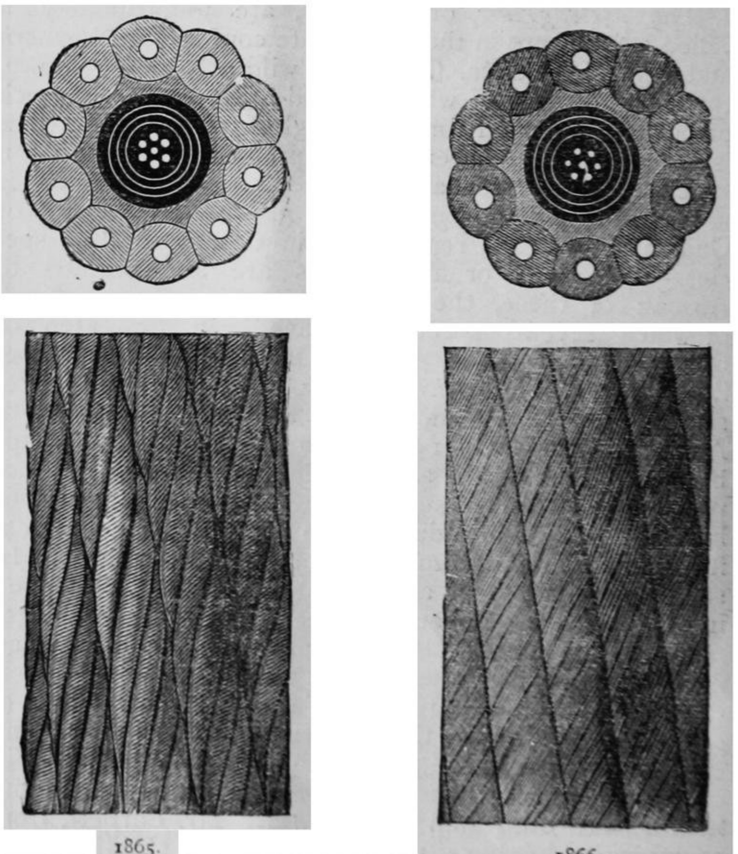
* Continued from p. 113.

stretching from London, the telegraphic centre of the world, by land and submarine circuits into Denmark, Norway, Sweden, and Russia in Europe, thence across the wilds of Siberia in Asiatic Russia to the Japanese Sea, and on to Japan, terminating within the tropics, at Hong Kong. Secondly, the "Eastern Telegraph," which, crossing the Bay of Biscay, reaches Lisbon, and thence threading its way under the dark blue waters of the Mediterranean Sea to Suez, reaches India by the Red Sea and Indian Ocean, and on to Ceylon (Point du Galle), joining the "Great Northern" at Hong Kong *viâ* Singapore. Thus by means of these two great systems a complete circuit of the continents of Europe and Asia is effected, the one within the limits of the tropics, the other bordering upon the Arctic circle, reaching as it does to 62° of north latitude. At Singapore the circuit is divided, a branch extending south to Sumatra, Java, and the continent of Australia,—Sydney, Melbourne, and Adelaide being reached; New Zealand being about to be included. Thirdly, there is the vast stretch of the South Atlantic Ocean traversed by the circuits of the "Brazilian Submarine," connecting Great Britain, *viâ* Lisbon, with Madeira, St. Vincent, and the continent of South America to Pernambuco. There it joins the coast submarine circuits of the "Western and Brazilian," extending north to Para and south to Bahia, Rio Janeiro, Rio Grand do Sul, and Monte Video in the River Plate, at which station, in connection with the local lines of the River Plate Company, it reaches Buenos Ayres, thence by means of the wires of the Argentine Republic, crosses the Andes into Chili and Peru. From Para the electric circuit is extended (Para and Demerara being now under completion), by way of the West India Isles, Jamaica, and Cuba, to Florida, there joining the extensive system of the United States Trunk lines; to San Francisco, west, and Newfoundland, east; and thence, by the circuits of the "Anglo-American" and "Direct United States" cable, crossing the Atlantic Ocean to Great Britain. Thus the New World is also encircled by two great systems, the one almost equatorial, the other within the higher degrees of northern latitude.

In dealing with submarine circuits the electrician has several matters to consider and accurately adjust, some of which will be more fully considered hereafter. First, there is the copper-conducting wire, its capacity according to the length of the circuit. Too small a conducting wire on a circuit of a given length would offer too great a resistance; too large a conducting wire would be equally faulty, induction increasing in greater proportion from its large superficial surface than its increased sectional area augments the speed. The exact sectional area of the wire has therefore to be determined; then for insulation, the best relative proportion in weight, and sectional measurement between the wire and that of the insulating material. Insulation, as is well known, may be obtained by a mere film of a non-conductor surrounding the wire. This is illustrated by the simple experiment of passing a weak voltaic current of electricity through an extended fine metallic wire immersed in a trough of water. Under ordinary circumstances it is but natural to suppose (water being a conductor) that there would be no insulation; not so; by the action of the current through the wire decomposing the water, a fine non-conducting film of hydrogen is developed surrounding the wire, which, with a strength of current adjusted to the resistance of the wire, will separate the water from the metallic conductor, perfect insulation being maintained. Destroy the balance between the current and the wire, and the hydrogen, evolved too rapidly by reason of electrical decomposition, accumulates upon the surface of the wire and, passing off in the form of small bubbles, destroys the insulation. This simple experiment demonstrates that insulation in the abstract sense may be obtained by a very thin covering of a non-conductor. It is, however, in practice mechanically unsafe to rely upon mere tissues of insulating material surrounding the conducting wire; a certain thickness is absolutely necessary for security. Every insulated core to be used for submarine purposes, to ensure integrity of manufacture, should be tested under pressure, so as to break down all mechanical imperfections in the coating of the insulating medium, before the cable is submerged. The determination of the dimensions of the insulator influences also

FIG. 34.—First Atlantic Cable, 1857 (natural size).

materially the inductive effect of the circuit; and when it is remembered that the best insulating material represents a cost of about 6*s*. per pound weight upon the wire, the close connection between science and pounds shillings and pence becomes at once apparent. The variations in weight per nautical mile of copper and insulation in some of the recent important cables are here given. The Atlantic main cables of 1865 and 1866: copper 300 lbs., insulation 400 lbs.; lengths each about 1,900 nautical miles. French Atlantic main cable, 1869: copper 400 lbs., insulation 400 lbs.; length about 2,600 nautical



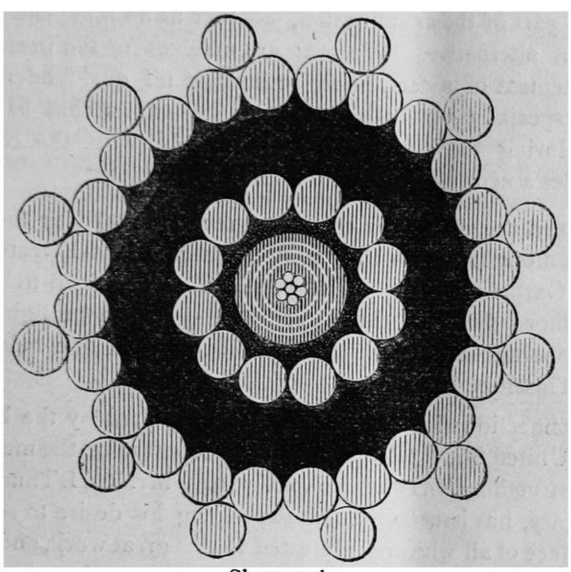
1865. 1866.

FIG. 35.—Atlantic Cables laid in 1865 and 1866, between Valentia and Newfoundland (natural size), weight per naut. 1·75 tons.

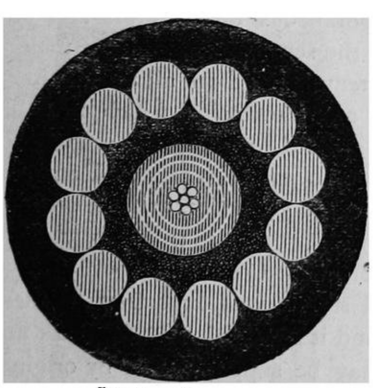
miles. Falmouth and Lisbon, 1870: copper 120 lbs., insulation 175 lbs.; length about 800 nautical miles. Anglo-Danish Cable, 1868: copper 180 lbs., insulation 180 lbs.; length, 365 nautical miles. Hong-Kong—Shanghai, 1870: copper 300 lbs., insulation 200 lbs.; length, 1,100 nautical miles. China Telegraph, 1870: copper 107 lbs., insulation 140 lbs.; length, 1,632 nautical miles. British India Extension, 1870: copper 120 lbs., insulation 175 lbs.; length, 1,448 nautical miles. Eight important submarine circuits have here been summarised, and in six it will be found that the proportions in the weight per nautical mile between the copper and insulation vary in an extreme degree. Thus there is found copper and insulation in the respective proportions by weight of 1 to 1, also 3 to 4, also 3 to 2, also 2 to 3, and also in the irregular proportion of 11 to 14. By these figures it appears that there is no accepted ratio, and every new cable seems to be constructed according to the electrical views of the designer, in some cases at an enormous cost, as compared with others of similar length and equal efficiency in transmitting power. Thus, by reducing the weight of material per nautical mile into an average money value, assuming for copper 1*s.* 2*d.* per lb., and insulation 6*s.* per lb., we obtain the following ratios :—

1,100	nautical miles :	copper	£16	0	insulation	£60
1,632	,,	,,	6	5	,,	42
2,600	,,	,,	23	10	,,	70
2,000	,,	,,	16	0	,,	70

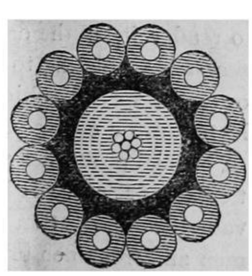
With such indiscriminate specifications there is certainly something left to discover, and the next few years may



Shore end.



Intermediate.



Main.

FIG. 36.—French Atlantic Cable laid between Brest and Island of Saint-Pierre, 1869.

determine with some degree of accuracy the true proportions by weight to be adopted between the conducting wire and the external thickness of the insulator, to obtain the best practical results at the least expenditure of capital on a circuit of given length, worked with one of the sensitive recording instruments already brought under notice. As an example of the augmentation of speed upon a submarine circuit, according to the delicacy of the recording instrument employed, upon the Great Northern cable between England and Denmark, 365 nautical miles in length, with the most improved submarine morse, an average of seventeen words per minute was obtained; with the Wheatstone's automatic thirty words, and with the Thompson syphon recorder fifty words per minute are practically reached.

For many years there has existed a divided opinion as to whether a light submarine cable, combining economy of construction with mechanical facilities of laying, is not the right system to adopt as against the heavy and more expensive form of iron covered cable. The light cable theory may be said to be based upon the opinion of the late Lieut. M. F Maury, who through every opposition adhered in principle to light cables. His argument may be expressed in his own words: "You may snap a taut rope, but you cannot break a slack line." This remark may nautically be quite true, but electrically far from correct, for the following reasons. In submerged cables, speed is greatest upon the shortest line. Now, in deep-sea telegraphy, in the only circuits upon which a light cable could possibly be employed with any security against mechanical interruptions, two or three points come into play. Supposing a light cable were to be used over, say, a circuit of 2,000 miles, with an average depth of 1,500 fathoms, or about 1¾ miles. First, take the specific gravity of the light cable as compared with water, at what rate will it sink to the bottom? if not so adjusted as to sink at about one mile per hour (looking to the enormous sweep between the paying out steamer and the bottom of the ocean at the depth of 1¾ miles), it is more than probable that although you cannot break a "slack line," it may be so twisted and contorted by surface-currents and under-currents moving at various velocities or even in opposite directions as it slowly sinks to the bottom by reason of low specific gravity, that a very great length of cable may be paid out (as a slack line). Secondly, the cost of this increased mileage must be taken into account as compared with that of the heavier iron-sheathed cable upon which a mechanical strain can be placed to ensure more or less a "Bee" line. Thirdly, the speed of transmission through a submarine cable is inversely as the square of the length. Now, if this is practically correct, it is easy to determine whether the best commercial results will be obtained from a light cable with increased electrical resistance, although it may be carried out at a less original outlay, or from a shorter cable more costly per mile from increased strength and weight of iron, but with greater transmitting speed, and in consequence dividend earning capacity. But of equal importance with any of the previous points is the impossibility of grappling a light cable from any considerable depth in cases of injury affecting the insulation. To raise a cable from a depth of 1¾ miles involves a great strain upon the cable, and unless the breaking strain has been calculated to meet such an emergency, any successful attempt at restoration must be abandoned, and the entire line is rendered useless and the capital lost. Every submarine cable should be laid with a certain percentage of slack, regulated according to depth of water and surrounding circumstance. The average slack is from 8 to 14 per cent.

The first Atlantic cable, 1857, between Valentia and Newfoundland, is shown in elevation and section at Fig. 34. This cable, from imperfect construction, remained electrically sound for a very limited period, and very few messages were successfully passed through the conducting wire. It, however, became the pioneer to success, and elucidated several important points in connection with the design of the 1865 and 1866 Atlantic cables shown at Fig. 35. The covering of these cables consists of ten strands of Manilla hemp, each containing a homogeneous steel wire. The French Atlantic iron-sheathed cable between Brest and Saint-Pierre, laid in 1869, is shown at Fig. 36.

			Tons.
The weight of the main cable per naut is			1·652
,,	intermediate	,,	... 6·246
,,	shore ends	,,	... 20·447

(*To be continued.*)

SCIENCE IN GERMANY

(From a German Correspondent.)

SINCE Darwin first gave the signal for a complete rupture with the old tradition of the morphology of animals, Germany has zealously continued working in the new direction, trying to bring anatomical, embryological, and biological facts into causal connection with each other by the comparative method. Darwin's theory remains the basis, and it has been principally Haeckel who, in advance of all its supporters, deduced further important consequences; the antagonists of the theory have confined themselves to a purely negative criticism. At one time the whole theory with all its suppositions and deductions was rejected by them; at another, the theory of descent was accepted in principle, but the further representation of its connection with the anatomy and the development history of animals was refuted; in all cases they either returned to the old views openly or they were satisfied with simple contradiction, leaving it to the future to fill up the gaps thus produced in the theory. In a work that has lately been published, the attempt has been made to consider the whole science of the morphology of animals from a different point of view. This work is: "Die Entwicklungsgeschichte der Unke als Grundlage einer vergleichenden Morphologie der Wirbelthiere" (the History of Development of *Bombinator igneus* as the basis of a Comparative Morphology of Vertebrata), by Dr. Alexander Götte, Professor of the University of Strassburg.

From a careful examination of the individual history of the development of Vertebrata and comparative consideration of the lower types, Götte tries to determine the morphological laws for the individual species, and from this to deduce their causal connection; he thus arrives at certain general theorems which, according to his view, form the basis for a conception of the origin of new animal species, totally different from Darwin's view. On the one side Götte does not look upon the animal ovum as a cell, nor in fact as a living organism at all; this of course is different from all other theories hitherto published. According to his view the cells, which are the basis of the formation of the ovum, only produce a conglomeration of a certain material (yolk) in a certain arrangement, but are themselves dissolved sooner or later, so that the complete ovum is a peculiar body, not living, but endowed with properties that enable it to be converted into a living organism under certain conditions. He maintains that this capacity for development is not the simple consequence of the chemical composition of the yolk, but that it only contains the motive force which is freed by chemical processes, and can do very different work according to the physical conditions under which it happens to be. The result may therefore as likely be the destruction of the ovum as its further development. For the latter, perfectly certain conditions of form are necessary, which have already been initiated during the formation of the ovum, and cause the force in question to work in a direction just as determined and certain as they are themselves. The results in that case are self-divisions of the yolk, when the parts are either of equal or of different sizes, and produced at different intervals. The former separate very soon and form separate individuals, which therefore consist only of one element and represent the lowest type (Protozoa); the ova of Matazoa, which are unequally divided according to a certain law, remain whole. Their coarser formation is brought about in a purely mechanical way, each division causing a displacement.

Thus Dr. Götte finds the basis of the fundamental structure, the type of each animal species, in the differences originating through the laws regulating the first divisions of the yolk.

NOTES

AN Exhibition of 50*l.* a year, tenable for four years, was recently devoted by the Endowed Schools Commission for annual competition between the four schools of Taunton, Tiverton, Exeter, and Sherborne. The details of the competition were left entirely to local trustees, whose names we do not know, but whom we understand to be gentlemen of the county of Somerset. The regulations issued by the trustees are before us. They very properly order that the examination shall be conducted by the Oxford and Cambridge Schools Examination Board. The subjects proposed by that Board include four groups, of which Science is one, and all candidates, whether choosing to take up Science or not, are permitted, if they please, to substitute Botany for Latin Verse, and Physical Geography for Greek Prose Composition. The scheme of the Somersetshire trustees includes all the subjects named by the Universities *except those which come under the head of Science*, refusing to permit any branch of science to form part of the examination, whether as an independent topic or as an alternative. We content ourselves for the present with the statement of a fact likely to interest all our readers, those more especially who are aware of the efforts that have been made during the past six years to establish in the county of Somerset a centre of first-rate scientific teaching.

AMONG the additional estimates recently voted by the House of Commons is one for the salary of an Assistant-Director to the Royal Gardens at Kew. Everyone will be rejoiced to hear that the arduous duties of the Director are likely to be lightened by this appointment, which has been filled up by the selection of Prof. Thiselton Dyer.

IN connection with the Commission appointed by the President of the United States to experiment and report upon the metals used in construction (NATURE, vol. xii. p. 94), Mr. R. H. Thurston, the Secretary, has issued circulars expressing his desire to secure the assistance of all who are interested in this great work, and through them to obtain all information available as the result of the labours of earlier or of contemporaneous investigators and observers. The circulars indicate the scope of the labours undertaken by this Commission, and request aid from all in a position to render it, in the collection of all information which may be accessible, relating to either the general work of the Commission or to the special subjects assigned to its committees. Data collected in the course of ordinary business practice, and the records of special researches previously made or now in progress, are particularly desired. It is expected that the Commission will receive valuable information and useful suggestions, both from business men and from men of science, and it is hoped that the work undertaken by the Commission may be supplemented by original investigations made by both these classes. The great importance of this work justifies the expectation of an earnest and effective co-operation. Part of the work of the Commission is the investigation of the methods and effects of Abrasion and Wear of metals in engineering and mechanical operations. Valuable data for the purpose could be furnished by railway engineers and others in regard to the wear of rails, wheels, axles, journals under heavy loads or at high velocities, the wear of tools, and other points, and we hope that all in this country who have it in their power will lend what aid they can to this important Commission. Another part of the Commission's work is a series of determinations of the effects of carbon, phosphorus, silica, manganese, and other elements, upon the strength, toughness, elasticity, and other qualities of iron and steel. Mr. A. L. Holley, Chairman of the Committee on Chemical Research, issues a circular giving detailed instructions as to the specimens and kind of information wanted. We should advise all interested to apply to Mr. R. H. Thurston, Stevens Institute of Technology, Hoboken, N.J., for detailed information; and we think the

Board would do well in sending circulars to the engineers of our principal railways, as well as to all others who are likely to be able to give them help in their laudable and valuable work.

WE are informed that H.M.S. *Challenger* will have completed her cruise and be back in this country by April of next year.

THE library of Audubon, the ornithologist, was destroyed by fire in April last. It was in the house of Mrs. Bakewell, the sister-in-law of Audubon, at Shelbyville, Ky.

THE twenty-fourth annual meeting of the American Association for the Advancement of Learning will be held in Detroit, Mich., beginning on Wednesday, August 11 next, under the presidentship of Mr. J. E. Hilgard, of Washington.

DR. HORNER, the medical officer on board the *Pandora*, which left England yesterday for the Arctic seas, will take upon himself all the meteorological duties of the expedition. Lieut. Banyan, of the Dutch navy, will act as scientific officer, it being intended that botanical and marine research will form a prominent duty of the expedition. Hall's Esquimaux Joe also accompanies the expedition, and altogether there are thirty-two souls on board. Capt. Allen Young hopes to get as far north as Carey Island, at the entrance of Smith's Sound. On this island a "post-office" or cairn has existed for many years, and, accordingly, all the letters Capt. Allen Young takes with him for the *Alert* and *Discovery* will be deposited here, unless he falls in with one of those vessels. The commander of this new expedition will push to the north-west after leaving Carey Island, and if the *Pandora* succeeds in forcing a way through the north-west passage, as Capt. Allen Young hopes, she will be the first steamship to accomplish the marvellous feat. She may possibly return in November next.

IN Part X. of the *Deutsche Rundschau* there is an excellent review of Capt. Lawson's wonderful book, "Wanderings in the Interior of New Guinea" (which we noticed in our issue of the 3rd inst., vol. xii. p. 83). The review is by Prof. A. B. Meyer, director of the Zoological Museum at Dresden. Like every sensible man, Prof. Meyer points out the absurdities with which this book is crammed. Indeed, he owns that he was almost of opinion that it was the author's intention to write a satire on modern narratives of travel, and that on the last page of the book the reader would be told of this; "but, unfortunately, Capt. Lawson is constantly in earnest; indeed, he left no stone unturned to make the book attractive, and to pass off its contents as real facts." Prof. Meyer dwells at some length on Capt. Lawson's marvellous mountain ascents, on his wonderful hunting feats, and his most surprising discoveries in the animal and vegetable world. He points out that with regard to the quadruped fauna it is well known that tigers are not found further eastward than Java, monkeys not further than Timor, and deer not further than Halmahera, and that it is incredible that these species, besides buffaloes, foxes, and hares, exist in New Guinea. Prof. Meyer, in conclusion, thinks it rather surprising that shortly after the publication of this wonderful book of fiction a deputation led by the Duke of Manchester should have waited upon the Colonial Secretary with a view to induce the Government to annex New Guinea. He asks, "Was this a consequence of the marvellous description of the distant country, or has the sensation novel been manufactured to order?"

A NEW steering balloon by Smitter is being exhibited, suspended in the middle of the Alcazar in Paris. The measurement is only 6,000 cubic feet, but the balloon is so light, that when filled with pure hydrogen it must float. A considerable sum of money has been invested in it, and great ability has been displayed in the construction. Although no practicable result in open air may be hoped for, it is a wonderful piece of clockwork. In connection with this subject it is stated that for several months past a firm of engineers have been experimenting privately at the Crystal Palace with an aërial steamer of a novel and promising character, weighing 160 lbs. Experiments are stated to have proved the capability of two vertical screws, each 12 feet diameter, to raise a weight of 120 lbs.; the steam-engine, with water and fuel, forming part of the weight so raised to the extent of 80 lbs. The power exerted by it is equal to two-and-a-half horses. The communication of motion is given by a vertical axis emanating from the car.

AT a Congregation held on Friday, the report of the Cambridge Syndicate recommending the purchase by the University of the collection of models, instruments, and tools used by the late Prof. Willis was confirmed.

CAPT. R. F. BURTON writes to the *Times* stating that the Italian African Expedition, under the Marchese Antinori, is reported to have for its ultimate object the wholly unvisited section to the south-west of Christian Abyssinia and the Abai River, "connecting known countries with the so-called Victoria Nyanza Lake."

WE regret to announce the death of M. Le Besgue, oldest Correspondent to the Geometry Section of the Paris Academy of Sciences. He died on June 12, at Bordeaux.

ON Tuesday a deputation from the Highland and Agricultural Society of Scotland waited upon the First Commissioner of Works, to ask the Government to proceed with the Survey of Scotland, which had been for some years in abeyance, and also to allow it to be done on a 25-inch scale of maps. A memorial was handed in to show that the opinion of the Scotch people was that the Survey should be at once carried out. Lord Henry Lennox promised to give the subject his best consideration, and remarked that the applications for the same object from different parts of the United Kingdom made it difficult to obtain from the Treasury any grants for the purpose.

AN important Report of a Committee of Council appointed to consider the requirements of Oxford University, as amended and adopted by Council, has been circulated for the information of members of Convocation. The "Requirements of the University" may be conveniently divided into Provision for Buildings and Institutions, and Provision for Professors and Teachers. Under the head of Buildings and Institutions, it is stated that with reference to the Botanic Garden, if it is to remain where it is, the lease being renewed, considerable amount of reconstruction is required, estimated at 4,000*l*. If it is to be removed to the Parks, a much larger outlay will be required. With regard to the University Museum, the heads of the three chief departments (Chemistry, Biology, Physics) report that additional buildings are required in each of the three, roughly estimated in all at 30,000*l*. Under the head of Provision for Professors and Teachers, the Committee find many demands which it is difficult to meet at once; one of their principal suggestions is the appointment of a Board for the following purposes:—1. To appoint lecturers from time to time to deliver lectures in the University on any subject that may seem to the Board to claim attention, and to assign payment to such lecturers. 2. To make occasional grants to individuals for the purpose of carrying on special work in connection with the studies or institutions of the University. 3. To appoint Readers for limited periods, not exceeding ten years, in subjects in which public teaching within the University may seem to the Board to be desirable; and to assign the stipends to such Readers; such appointments and the stipends being subject to the approval of Convocation. The

Board also, under certain conditions, might be entrusted with the duty of appointing Professors for life. It appears, however, that several additions to the permanent staff of Professors will be required. These must be provided for, the Report states, from time to time, by statute. Thus, for example, the following suggestions have been made with regard to the chief departments of study pursued in the Museum :—1. In the department of Chemistry it is stated that an additional professorship is required. 2. In the department of Physics also it is stated that an additional professorship is required. 3. In the department of Biology it is proposed—(*a*) That the present Linacre Professorship should become a Professorship of Human Anatomy and Ethnology. (*b*) That the Hope Professorship of Zoology should become a Professorship of Zoology and Comparative Anatomy. (*c*) That the Clinical Professorship of Medicine should become a Professorship of Physiology and Public Health.

A CORRESPONDENT sends us the enclosed cutting from *Le Français* as an illustration of how they do things in France :—"On sait que sur la proposition de M. de Cumont, ministre de l'instruction publique, des cultes et des beaux-arts, l'Assemblée a voté, le 18 juillet dernier, une pension annuelle et viagère de 12,000 fr. à M. Pasteur, membre de l'Institut, professeur à la Faculté des sciences de Paris, à titre de récompense nationale. Un nouveau décret, rendu par M. le maréchal de Mac-Mahon sur le rapport de M. Wallon, contre-signé par M. Leon Say, vient d'accorder une nouvelle pension de 6,000 fr. à M. Pasteur, indépendamment de celle de 12,000 fr. qui lui avait été donnée précédemment. De telles mesures ne peuvent qu'encourager nos hommes de science et stimuler l'esprit de découverte. Cette pension permettra donc d'assurer d'une manière digne de lui les jours d'un homme qui compte près de trente-trois années de services dévoués, et que les fatigues d'un travail assidu ont mis dans l'impossibilité de continuer à exercer ses fonctions de professeur."

LAST Thursday, in the House of Commons, in reply to a question by Sir John Lubbock, the Chancellor of the Exchequer said he would be ready to consult with his colleagues in the course of the autumn to see whether the object of preserving the ancient monuments of the country could in any way be carried out. Sir J. Lubbock, considering this a favourable answer, said he would withdraw his Ancient Monuments Bill.

A SPECIMEN of a sturgeon, eight feet in length, has been added to the Manchester Aquarium. Several examples of the Wolf, or Cat Fish, and three of the Monk, or Angel Fish, each five feet long, are also to be seen in the same building.

DURING this season the *Morning Post* has made a speciality of noticing the proceedings of some of the learned societies. The lectures at the Royal Institution have generally occupied half a column, and some of the popular lectures of the Zoological Society have been given at equal length. In a notice of one of the ladies' lectures of Prof. Bentley at the Botanic Society is this passage :—"Future historians of the social condition of the people of England at our period will have to make constant reference to the daily press, and it is therefore but right, alongside of the notices of the culture of music and the sister fine arts, to record each attempt to spread the knowledge acquired by men of science." We are glad the *Morning Post* has set so good an example.

PROF. NORDENSKJÖLD'S expedition left Tromsö for Novaya Zemlya on June 8. The expedition is undertaken on board the Norwegian Arctic sea-yacht *Prøven*, Capt. J. N. Isaksen, who has been to Spitzbergen and Novaya Zemlya a great many times previously. On the southern coast of the latter island the party expect to meet with Samoyedes; they intend then to move in an easterly direction, towards the rivers Obi and Yenesei. Prof. Nordenskjöld will then leave the ship to continue the expedition by boat.

THE so-called tobacco-meal, the *Kölnische Zeitung* says, has been successfully used in agriculture for the destruction of noxious insects, but it has not yet been applied largely on account of its high price, which is caused by heavy import duty. The Prussian Minister for Agriculture has just addressed a letter to the Minister for Commerce with a view to reduce this duty or to take it off entirely. The only obstacle lies in the fact that the meal might be used for the manufacture of snuff. A Hamburg firm is said to have a stock of over thirty tons of this meal.

WE regret to learn that Mr. Alexander Agassiz, director of the Anderson School of Natural History, has been unable to make arrangements for a third session of this establishment during the present summer. He announced some time ago that, in view of the expense of the enterprise and the limited funds at his command, it would be impossible for him to proceed unless a sufficient number of students could be found willing to pay fifty dollars for the course. This appeal not proving effectual, he has given notice that the school will not be opened during 1875.

THE French Minister of Public Instruction has established a new commission to report on the state of meteorology and the improvements to be introduced in the system of observations, as hitherto practised at the Observatories of Paris and Montsouris, and other public establishments.

COL. MONTGOMERIE, the representative at the International Geographical Congress of the Royal Geographical Society and the Indian Survey Office, has arrived in Paris. A representative of the English Admiralty is expected very shortly. It is hoped that the Admiralty will send to Paris one of the magnificent yachts of the English navy for exhibition during the Congress. An immense quantity of goods for exhibition is stated to have already arrived from London.

THE death of Mr. Thomas Baines, the African traveller, is announced.

AT the last meeting of the Edinburgh Botanical Society, the *British Medical Journal* states, Dr. T. A. G. Balfour reported some interesting experiments on the *Dionæa muscipula*, which he considered a carnivorous plant. He showed that the irritability under which the leaf contracts is resident in six delicate hairs, so placed on the surface of the leaf that no insect could avoid touching them in crawling over. Chloroform dropped on a hair caused the leaf to close immediately; water had no such effect. Contraction only lasted for a considerable time when any object capable of affording nutrition was seized, when it lasted for about three weeks, and the interior of the leaf gave out a viscous acid secretion. A number of interesting points were made out with regard to the secretion, digestion, and absorption performed by the plant.

THE additions to the Zoological Society's Gardens during the past week include two Dorsal Squirrels (*Sciurus dorsalis*) from West Mexico, presented by Mr. John G. Haggard; a Yellow-shouldered Amazon (*Chrysotis ochroptera*) from South America, presented by Miss Amelia Grove Grady; a Grison (*Galictis vittata*) from South America, a Hobby (*Hypotriorchis subbuteo*), European, a Humboldt's Lagothrix (*Lagothrix humboldti*) from the Upper Amazon, purchased; ten Summer Ducks (*Aix sponsa*), seven Spotted-billed Ducks (*Anas pæcilorhyncha*), four Temminck's Tragopans (*Ceriornis temminckii*) bred in the Gardens.

RECENT PROGRESS IN OUR KNOWLEDGE OF THE CILIATE INFUSORIA *

II.

THE reproductive process was lately followed by myself through some of its stages in a very beautiful Vorticellidan obtained abundantly from a pond in Brittany.† The zooids which form the colonies in this Infusorium are grouped in spherical clusters on the extremities of the branches. They present near the oral end a large and very obvious contractile vesicle, and have a long cylindrical nucleus curved in the form of a horseshoe.

In the internal protoplasm are also imbedded scattered green chlorophylloid granules. No trace of the so-called nucleolus was present in any of the specimens examined.

Among the ordinary zooids there were usually some which had become encysted in a very remarkable way, and without any previous conjugation having been noticed. These encysted forms were much larger than the others and had assumed a nearly spherical shape; the peristome and cilia-disc had become entirely withdrawn, the contractile vesicle was still obvious, but had ceased to manifest contractions; brownish spherical corpuscles with granular contents, probably the more or less altered chlorophylloid granules of the unencysted zooid, were scattered through the parenchyma, and the nucleus was not only distinct, but had increased considerably in length. Round the whole a clear gelatinous envelope had become excreted.

In a later stage there was formed between the gelatinous envelope and the cortical layer of the body a strong, dark-brown, apparently chitinous case, the surface of which in stages still further advanced had become ornamented by very regular hexagonal spaces with slightly elevated edges. In this state the chitinous envelope was so opaque that no view could be obtained through it of the included structures, and in order to arrive at any knowledge of these it was necessary to rupture it. The nucleus thus liberated was found to have still further increased in length, and to have become wound into a convoluted and complicated knot. Along with the nucleus were expelled multitudes of very minute corpuscles with active Brownian movements.

In a still further stage the nucleus had become irregularly branched, and at the same time somewhat thicker and of a softer consistence; and finally, it had become broken up into spherical fragments, each with an included corpuscle resembling a true cell nucleus in which the place of a nucleolus was taken by a cluster of minute granules.

In this case the original nucleus of the Vorticellidan had thus become broken up into bodies identical with the so-called eggs of Balbiani, but this was unaccompanied by any conjugation or by the formation of anything which could be compared to spermatozoal filaments.

What I believe we may regard as now established in the phenomena of reproduction in the Infusoria is, that besides the ordinary reproduction by spontaneous fission of the entire body, the nucleus at certain periods, and after more or less change of form has occurred in the Infusorium body, becomes broken up into fragments, each including a corpuscle resembling a true cell nucleus; and that this takes place without necessarily requiring the influence of conjugation or the action of spermatozoa; that these fragments after their liberation from the body of the Infusorum become developed—still without the necessity of spermatic influence—directly or indirectly into the adult form.

Whether proper sexual elements ever take part in the life history of the Infusoria remains an open question.

Everts ‡ has given an account of observations which, with the view of testing the statements of Greeff, he made on *Vorticella nebulifera*. Greeff, as we have seen, followed Claparede and Lachmann in attributing to the Vorticellæ a true cœlenterate structure; and Everts, by his own investigations, has convinced himself of the untenableness of this view, and has been led to regard the Vorticellæ as strictly unicellular.

He recognises the distinction between the cortical layer (which forms not only the periphery of the body but the whole of the stalk on which this is supported), and the central mass in which the nutriment is deposited, collected into pellets and digested; but instead of regarding this central mass as chyme, he looks upon it as an integral constituent of the entire body, like the central portion of an Amœba. The nucleus is imbedded in the inner side of the cortical layer, which is itself differentiated into certain secondary layers. He describes the deeper part of the cortical layer as exhibiting a rotation of its granules independent of the rotation which occurs in the central parenchyma, and moving in a direction opposite to that of the latter. Everts's account of the structure of Vorticella is thus in accordance with the conception of it as a cell with a parietal nucleus; a cell, however, in which differentiation is carried very far without the essential character of a simple cell being thereby lost.

Everts regards the external wall as corresponding with the ectoderm, and the internal softer body-substance with the endoderm of higher animals. If by this the author meant to indicate a homological identity between the structures thus compared, it is plain that he would have taken an entirely mistaken view based on a misconception of the essential nature of an ectoderm and endoderm. These membranes are essentially multicellular, and are always results of the segmentation of the vitellus in a true ovum. They can therefore never be attributed to a unicellular animal, in which no true segmentation process ever takes place. In his rejoinder, however, to an elaborate criticism of his memoir by Greeff, he explains that he intended to compare the two layers of the Infusorium body analogically, not morphologically, with an ectoderm and endoderm.

The same author has further made some interesting observations on the development of Vorticella. He has noticed that reproduction is here ushered in by a longitudinal cleavage, in which after division of the nucleus the body of the Vorticella becomes cleft into two halves, still seated on the common stalk. Each of these develops near its posterior end a wreath of vibratile cilia, while the peristome and the cilia-disc over the mouth are entirely withdrawn, and then breaks loose from its stem and swims freely away. These free-swimming Vorticellæ now encyst themselves, the cilia disappear, and the contents of the encysted animal acquire a uniform clearness with the exception of the nucleus, which persists unchanged. In the next place the nucleus breaks up into eight or nine pieces, and then the wall of the cyst becomes ruptured and gives exit to these fragments, which now appear as spontaneously moving spherules. These increase in size, develop on one end a cilia wreath, within which a mouth makes its appearance, and the free-swimming nucleus-fragment becomes gradually changed into a form which entirely agrees with the *Trichodina grandinella* of Ehrenberg.

These Trichodinæ now multiply by fission, first developing a posterior wreath of cilia, and then dividing transversely between the anterior and posterior wreaths. After this each fixes itself by the end on which the mouth is situated; a short stem becomes here developed, and the cilia wreath gradually disappears. Then upon the free end the peristome and cilia disc make their appearance, and the growth of the stem completes the development.

Everts remarks that in this process we have an example of alternation of generations. There is one point, however, in which he has overlooked its essential difference from a true alternation of generations, namely, the absence of any intercalation of a proper sexual reproduction.

Ray Lankester * has subjected to spectrum analysis the blue colouring matter of *Stentor cæruleus*. This occurs in the form of minute granules in the cortical layer of the animal, and Lankester finds that it gives two strong absorption bands of remarkable intensity, considering the small quantity of the matter which can be submitted to examination. He cannot identify these bands with those of any other organic colouring matter, and to the peculiar pigment in which he finds them he gives the name of *stentorin*.

He has also examined the bright green colouring matter of *Stentor Mülleri*, and finds that instead of giving the stentorin absorption bands, it gives a single band like that of the chlorophylloid matter of *Hydra viridis* and of Spongilla.

Ray Lankester † has also described, under the name of *Torquatella typica*, a remarkable marine Infusorium, which, though quite destitute of true cilia, can scarcely be separated from the proper Ciliata. With the general structure of the ciliate Infusoria, the place of a peristomal cilia wreath is taken by a singular plicated membrane, which forms a wide, frill-like, very mobile appendage, surrounding the oral end of the animal, and projecting to a considerable distance beyond it. The author regards *Torquatella typica* as the type of a distinct section of the Ciliata to which he gives the name of *Calycata*.

Of all the authors who since Von Siebold have applied themselves

* Anniversary Address to the Linnean Society, by the President, Dr. G. J. Allman, F.R.S., May 24. Continued from p. 137.

† British Association Reports, 1873.

‡ Everts, Untersuchungen an *Vorticella nebulifera*. Sitzungsberichte der Physikalisch-Medicinischen Societat zu Erlangen. 1873.

* Quart. Journ. Mic. Sci., 1873.

† Ibid. 1874.

to the investigation of the Infusoria, Haeckel must be mentioned as the one who has brought the greatest amount of evidence to bear on the question of their unicellularity. In a very elaborate paper which has quite recently appeared,* and which is remarkable for the clearness and logical acuteness with which the whole subject is treated, Prof. Haeckel, resting mainly on the observations of others, and partly also on his own, argues in favour of the unicellularity of the Infusoria from the evidence afforded both by the phenomena of their development and by the structure of the mature organism. He confines himself chiefly to the Ciliata—which, indeed, he regards as the only true Infusoria—while he considers the unicellularity of the Flagellata as too obvious to require an elaborate defence. The value of this paper will be obvious from the analysis of it which I now propose to give.

In stating the argument derived from development, Haeckel does not accept as established the alleged sexual reproduction of the Infusoria, and he believes it safest to regard as non-sexual "spores" the bodies (*Keimkugeln*) which result from the breaking up of the nucleus, and which Balbiani regarded as eggs.

These bodies consist of a little mass of protoplasm usually destitute of membrane, and including a nucleus within which one or more refringent granules admitting of comparison with a true nucleolus may sometimes be witnessed—characters which are all those of a simple genuine cell. From this spore the embryo is developed by direct growth and differentiation of parts; but however great may be the differentiation, there is never anything like the formation of a tissue.

The development of the Infusoria is thus entirely in favour of the unicellular theory. This theory, however, is just as strongly supported by the study of their mature condition; and here Haeckel gives an admirable exposition of the structure of the true or Ciliate Infusoria.

The parts which are common to all Ciliata and which first differentiate themselves in the ontogenesis or development of the spore, are the cortical layer, the medullary parenchyma, and the nucleus, which is situated on the boundary between the two. The differentiation of the protoplasm of the naked spore into a clearer and firmer cortical substance, and a more turbid, granular, and softer medullary substance, corresponds entirely with what we see in the parenchyma cells of higher animals. These two products of differentiation are designated by Haeckel "exoplasm" and "endoplasm."

The exoplasm is originally a perfectly homogeneous and structureless, colourless hyaline layer distinguishable from the turbid granular soft protoplasm of the internal body mass, by containing in its composition less water, by absence of included granules, and by its high independent contractility. All the mobile appendages of the body, the cilia, bristles, spines, hairs, hooks, &c., are nothing but structureless extensions of this exoplasm and participate in its contractility. In this respect they entirely correspond to the cilia and flagellæ of the cells which form the ciliated epithelium of multicellular animals.

In many Ciliata we find this cortical layer or exoplasm itself subsequently differentiated into distinct strata. In the most highly differentiated Ciliata four layers may be distinguished as the result of this secondary differentiation of the exoplasm. These are: (1) the cuticle layer, (2) the cilia layer, (3) the myophan layer, (4) the trichocyst layer.

The *cuticle* is nothing but a lifeless exudation from the surface. In the majority of Ciliata there is no true cuticle, and in those which possess it, it presents itself under various forms, as seen in the thin, chitine-like, hyaline homogeneous pellicle of Paramæcium and Trichodina, the outer elastic layer of the stem of the Vorticellinæ, the protective sheath of Vaginicola, the chitin-like cases of the Tintinnodeæ and Codonellidæ, the beautiful lattice-like siliceous shells of the Dictyocystidæ, and many other shells, cases, and shield-like protections.†

The *cilia layer* occurs in all Ciliata; it lies immediately beneath the cuticle where this is present, and the whole of the cilia and other mobile appendages are its immediate extensions. These must therefore perforate the cuticle or its modifications when such protective coverings exist.

The *myophan layer* is identical with that which most authors describe as a true muscular layer. It has been demonstrated in most of the Ciliata. It appears as a system of regular parallel fine striæ in the walls of the body, and in the Vorticellidæ occupies also the axis of the stem, where it forms the characteristic "stem-muscle" of these animals. There can be no doubt that these striæ represent contractile fibrils, which, by their contraction, effect the various form changes of the animal. They are thus *physiologically* analogous to muscles. From a *morphological* point of view, however, we must regard them as only differentiated protoplasm filaments. In the morphological conception of true muscle, its cell nature is absolutely indispensable. The so-called muscle-fibrils of the Infusoria never show a trace of nucleus. They can be viewed only as *parts* of a cell due to the differentiation of the sarcode molecules of its protoplasm; and as they are thus only sarcode filaments, Haeckel designates them by the term "myophan," as indicating a distinction from proper muscle.

The *trichocyst layer* occurs also in many Infusoria, but not in all. It is a thin stratum of the exoplasm lying immediately on the endoplasm, and including in certain species the trichocysts. The presence of these bodies, which possess a striking resemblance to the thread-cells of the Cœlenterata, has, as we have already seen, been urged as an argument in favour of the multicellularity of the Infusoria. But, as Haeckel argues, no evidence of multicellularity can be derived from this fact. The thread-cells of the Cœlenterata are themselves the products of a cell, and we often find many of them originating in a single formative cell quite independently of the nucleus; the formative cell may in this respect be compared with the entire body of the Infusorium.

It is the endoplasm, or internal parenchyma of the Infusoria that has given rise to the most important differences of opinion, and in his account of this part of the Infusorium-organism Haeckel chiefly directs his criticism against the views advocated by Claparede and Lachmann, and by Greeff.

These authors, as we have already seen, compare the Infusoria with the Cœlenterata, and regard the endoplasm not as a real part of the body, but merely as the contents of the alimentary canal—as a sort of food mash or chyme contained in a spacious digestive cavity whose walls are at the same time stomach wall and body wall, and into which the mouth leads by a short gullet. As Haeckel urges, however, it needs only a correct conception of the intestinal cavity throughout the animal kingdom and of its distinction from the body cavity, in order to show the untenableness of this position. The main point of such a conception lies in the fact that the intestinal cavity and all extensions of it (gastro-vascular canals, &c.) are always originally clothed by the endoderm or inner leaflet of the blastoderm, while the body cavity is always formed on the external side of the endoderm, and between this and the ectoderm or outer leaflet of the blastoderm. The body cavity and intestinal cavity of animals are thus essentially different; they never communicate with one another, and always arise in quite different ways.

Again, the contents of a true intestinal cavity consist only of nutritious matter and water, in other words, of chyme; while the fluid which fills the body cavity is never chyme, but is always a liquid which has transuded through the intestinal wall, and which may be called chyle, or blood in the wider sense of the word.

Haeckel has thus taken, I believe, the true view of the intestinal and body cavities of animals. He had already advocated it in his work on the Calcareous Sponges. It necessarily in-

* Haeckel, "Zur Morphologie der Infusorien." Jenaische Zeitschr., Band vii heft 4, 1873.

† In the same number of the *Zeitschrift*, Haeckel ("Ueber einige neue pelagische Infusorien") describes some highly interesting Infusoria which spend their lives in the open sea and are distinguished by the possession of variously formed shells. His attention was first directed to them by finding their elegant empty shells in the extra-capsular sarcode of Radiolariæ. These pelagic Infusoria appear to belong to two different groups, which stand nearest to the Tintinnodea of Claparede and Lachmann. He designates them as *Dictyocystidæ* and *Codonellidæ*.

The family of the Dictyocystidæ is based on Ehrenberg's *Dictyocysta*, and is characterised by the possession of a siliceous perforated lattice-like shell so closely resembling that of many Radiolariæ, that Haeckel at first mistook it for the shell of one of these. The shell is in all the species bell-shaped or helmet shaped, and the body of the animal, which is fixed to the fundus of the bell, and can be projected far beyond its margin, has a wide funnel-shaped peristome on whose edge are two concentric wreaths of strong cilia. He describes four species, distinguishing them by characters derived from their siliceous latticed shell.

The family of the Codonellidæ, based on the genus *Codonella*, Haeckel, is also provided with a bell-shaped case, but this, instead of being formed of a siliceous lattice work, consists of a chitine-like organic membrane, through which siliceous particles are scattered. The family is, however, chiefly characterised by the peculiar form of its peristome This is funnel-shaped and provided on its margin with a thin collar-like expansion. The free edge of this collar is serrated, and each tooth carries a stalked lobe of a piriform shape, regarded by Haeckel as probably an organ of touch. At some distance behind the circle of piriform lobes is situated a ring of long, strong, whip-like cilia, which form powerful swimming organs. The three species described are distinguished by the form of their chitinous cases.

volves a belief in the homological identity of organisation between very distant groups of the animal kingdom, a belief which all recent embryological research has only tended to confirm.

(*To be continued.*)

SCIENTIFIC SERIALS

American Journal of Science and Arts, June.—The original articles in this number are :—Results of dredging expeditions of the New England Coast in 1874, by A. E. Verrill. More than 100 species new to the fauna of southern New England were secured. Most of these are northern species, but many are undescribed. A table giving nature of bottom and temperature at the surface and bottom of the sea is given.—Mr. Fontaine's paper on the Primordial Strata of Virginia is continued and concluded. At the end is given a comparison with the metamorphic crystalline rocks of the Blue Ridge.—On the occurrence of the Brown Hematite deposits of the Great Valley, by Frederick Prime, jun.—Note on some new points in the elementary stratification of the Primordial and Canadian rock of south central Wisconsin, by Roland Irving. The order for the Lower Silurian strata of Wisconsin has been generally accepted as (beginning from below) 1. Potsdam sandstone ; 2. Lower magnesian limestone ; 3. The St. Peter's sandstone ; 4. The blue and buff limestones ; 5. The Galena limestone ; 6. The Cincinnati group. The succession as now made out is (beginning from below) 1. The Lower or Potsdam sandstone ; 2. The Mendota limestone ; 3. The Madison sandstone ; 4. The main body of limestone ; 5. The St. Peter's sandstone. A table of correlation is given with the Mississippi Bluffs and the Minnesota River.—On the application of the horizontal pendulum to the measurement of minute changes in the dimensions of solid bodies, by Prof. O. N. Rood.—On diabantite (a chlorite), by G. W. Hawes.—Re-discovery of double star H.I. 41, by S. W. Burnham. It is about 46′ north of the well-known double star ψ' Draconis, and is easily found without an equatorial mounting.—On the distribution of electrical discharges from circular discs, by C. J. Bell.—Examination of gases from the meteorite of Feb. 12, 1875, by A. W. Wright.—On limonite with the colour and transparency of göthite, by Prof. Mallet.—Under the head "Scientific Intelligence," the original notes are :—On the surface geology of Ohio ; On the Prototaxites of Dawson ; On the Crustaceans of the caves of Kentucky and Indiana, together with several reviews.

Fourth and Fifth Annual Reports of the Wellington College Natural History Society, Dec. 1872 to Dec. 1874.—We are gratified to see that this Society is in a much more hopeful condition than it was when we noticed its last Report, the tone of which was almost despairing. The attendance has been very much better, and the interest taken in the Society by the boys is evidently increasing. Judging from the lists a fair amount of field-work in natural history has been done, and the Society is gradually forming good collections. But, as the preface to one of the Reports hints, there is still much room for improvement in the subjects and character of the papers read at the meetings. Except in the case of lectures by outsiders, the majority of the papers are the result of reading and not of observation or experiment, and not many of them can strictly be called scientific. Now, however useful such exercises as these may be to the boys, this is scarcely the sort of work one looks for from members of a Natural History Society. We think this Society might well take a leaf out of the Rugby Society's Report, and go in much more extensively for organised field-work, encouraging the boys to use their eyes and their hands on nature as well as on books, and to bring forward papers embodying the results of their observations, papers of a character similar to the interesting one of the president, the Rev. C. W. Penny, on "Natural History in the Christmas Holidays." Not only would the members thus reap much benefit, both in the way of discipline and instruction, but we are sure a greater interest in the Society would be created in the School. The Society has evidently got a good second start, and we trust that the next Report will show as great an advance on the two under notice as these do on the previous one.

Riga Society of Naturalists.—Nos. 8 and 9 of this Society's publications contain three papers of importance, besides meteorological reports and notes of smaller interest. The more important papers are : On some theories of earthquakes, by Prof. Schweder.—On the changes in the Düna estuary, by M. Gottfried.—On the fauna of Spitzbergen, by Prof. Nordenskjöld, showing that this fauna consists of 15 species of quadrupeds, 23 of birds, 23 of fishes, 64 of insects, 100 of Crustaceæ, and 130 of sea molluscs.—There is also an obituary notice of the late Dr. Ernst Nauck, who died at Riga on Jan. 26 last.

SOCIETIES AND ACADEMIES

London

Royal Society, June 10.—"Experiments on Stratification in Electrical Discharges through Rarefied Gases," by William Spottiswoode, M.A., Treas. R.S.

In the stratified discharges through rarefied gases produced by an induction-coil working with an ordinary contact-breaker, the striæ are often unsteady in position, and apparently irregular in their distribution. Observations made with a revolving mirror, which the author hopes to describe on another occasion, have led him to conclude that an irregular distribution of striæ does not properly appertain to stratification, but that its appearance is due to certain peculiarities in the current, largely dependent upon instrumental causes.

The beautiful and steady effects obtained by Mr. Gassiot with his Leclanché battery, and also more recently by Mr. De la Rue with his chloride-of-silver battery, have abundantly shown the possibility of stratification free from the defects above mentioned ; but it must be admitted that the means employed by those gentlemen are almost gigantic. The present experiments were undertaken by the author with the view of ascertaining, first, how far it was possible to approach towards similar results with instruments already at his command ; and secondly, whether these would afford other modes of attack, beside the battery, on the great problem of stratified discharges.

The induction-coil used was an "18-inch" by Apps, worked occasionally by six large chloride-of-silver cells, kindly lent the the author by Mr. De la Rue, but more usually by ten or by twenty Leclanché cells of the smallest size ordinarily made by the Silvertown Company. He has also, in connection with the same coil, 120 of the latter cells, connected in twenties for quantity, and forming six cells of twenty times the surface of the former. These work the coil with the ordinary contact-breaker very well, giving 11-inch sparks whenever required. A "switch" affords the means of throwing any of the three batteries in circuit at pleasure.

Having reason to think that the defects in question were mainly due to irregularity in the ordinary contact-breaker, he constructed one with a steel rod as vibrator, having a small independent electromagnet for maintaining its action. The details of construction of this contact-breaker are described.

With a contact-breaker of this kind in good action, several phenomena were noticeable ; but first and foremost was the fact that in a large number of tubes (especially hydrocarbons), the striæ, instead of being sharp and flaky in form, irregular in distribution and fluttering position, were soft and rounded in outline, equidistant in their intervals, steady in proportion to the regularity of the contact-breaker. These results are, the author thinks, attributable more to the regularity than to the rapidity of the vibrations. And this view is supported by the fact that, although the contact-breaker may change its note (as occasionally happens), and in so doing may cause a temporary disturbance in the stratification, yet the new note may produce as steady a set of striæ as the first. And not only so, but frequently there is heard, simultaneously with a pure note from the vibrator, a strident sound, indicating that contacts of two separate periods are being made, and yet, when the strident sound is regular, the striæ are steady. On the other hand, to any sudden alteration in the action of the break (generally implied by an alteration in the sound) there always corresponds an alteration in the striæ.

The author then attempts to show the extreme delicacy in action of this kind of contact-breaker, or "high break," as it may be called.

The discharges described above are usually (although not always) those produced by breaking contact ; but it often happens, and that most frequently when the strident noise is heard, that the current produced by making contact is strong enough to cause a visible discharge. This happens with the ordinary as with the high break ; but in the latter case the double current presents the very remarkable peculiarity, that the striæ of one current are so arranged as to fit exactly into the intervals of the other. And further, that any disturbance affecting the column of striæ due to one current affects similarly, with reference to absolute space, that due to the other, so that the double column moves, if at all, as a solid or elastic mass. And this fact is the more remarkable if we consider, as is easily observed in a revolving mirror, that these currents are alternate, not only in direction, but also in time, and that no one of them is produced until after the complete extinction of its predecessor. And it is also worthy of note that this association of striæ is not destroyed, even when the two currents are separated more or less towards opposite sides of the tube by the presence of a magnetic pole. There seems, however, to be a tendency in that case for the striæ of one current to advance upon the positions occupied by those of the reverse current, giving the whole column a twisted appearance. But as there is no trace, so far as the author's observations go, of this association of alternate discharges when produced by the ordinary break, we seem led to the conclusion that a stratified discharge, on ceasing, leaves the gas so distributed as to favour, during a very short interval of time, a similar stratification on the occurrence of another discharge, whether in the same or in the opposite direction. An explanation of the fact that the striæ of alternate discharges occupy alternate and not similar positions is not obvious, and probably demands a better knowledge of the nature of the striæ than we possess at present.

The column of striæ, which usually occupy a large part of the tube from the positive towards the negative terminal, have hitherto been described as stationary, except as disturbed by irregularities of the break. The column is, however, frequently susceptible of a general motion, or "flow," either from or towards the positive pole, say a forward or backward flow. A similar phenomenon was observed by Mr. Gassiot in some tubes with his large battery, but the author is not acquainted with the exact circumstances under which it was produced. This flow may be controlled, both in velocity and in direction, by resistance introduced into the circuit, or by placing the tube in a magnetic field. The resistance may be introduced in either the primary or the secondary circuit. For the former arrangement the author successfully employed a set of resistance-coils, supplemented by a rheostat. For the secondary current, as well as for the Holtz machine, he has used an instrument devised and constructed by his assistant, Mr. P. Ward, to whose intelligence and skill he is much indebted throughout this investigation, intended for fine adjustment. Wherever the resistance be introduced the following law appears to be established by a great number and variety of experiments, viz., that, the striæ being previously stationary, an increase of resistance produces a forward flow, a decrease of resistance a backward flow. The author has generally found that a variation of 3 or 4 ohms, or, under favourable conditions, of 1 or 2 ohms, is sufficient to produce this effect. But as an alteration in the current not only affects the discharge directly, but also reacts upon the break, the effect is liable to be masked by these indirect causes. The latter, so far as they are dependent upon a sudden alteration of the resistance, may be diminished by the use of the rheostat; but when the striæ are sufficiently sensitive to admit the use of this delicate adjustment, some precautions are necessary to ensure perfect uniformity of current, so as to avoid disturbances due to uneven contact in the rheostat itself.

When the striæ are flowing they preserve their mutual distances, and do not undergo increase or decrease in their numbers. Usually one or two remain permanently attached to the positive electrode; and as the moving column advances or recedes, the foremost stria diminishes in brilliancy until, after travelling over a distance less than the intervals between the two striæ, it is lost in darkness. The reverse takes place at the rear of the column. As the last stria leaves its position, a new one, at first faint and shadowy, makes its appearance behind, at a distance equal to the common interval of all the others. This new one increases in brilliancy until, when it has reached the position originally occupied by the last stria, when the column was at rest, it becomes as bright as the others. The flow may vary very much in velocity; it may be so slow that the appearances and disappearances of the terminal striæ may be watched in all their phases, or it may be so rapid that the separate striæ are no longer distinguishable, and the tube appears as if illuminated with a continuous discharge. In most cases the true character of the discharge, and the direction of the flow, may be readily distinguished by the aid of a revolving mirror. In some tubes, especially in those whose length is great compared with their diameter, the whole column does not present the same phase of flow; one portion may be at rest while another is flowing, or even two conterminous portions may flow in opposite directions. This is seen also in very wide tubes, in which the striæ appear generally more mobile than in narrow ones. But in all cases these nodes or junction-points of the flow retain their positions under similar conditions of pressure and current; and it therefore seems that, under similar conditions, the column in a given tube always breaks up into similar flow-segments.

These nodes will often disappear under the action of a magnetic pole. Thus, if the first segment, measured from the positive terminal, be stationary and the second be flowing backwards (*i.e.* from − to +), a magnetic pole of suitable strength, placed at the distant end of the latter, will stop its flow, and the whole column will become stationary throughout. An increase in the strength of the magnet, or a nearer approach of it to the tube, will produce a general forward flow of the column.

The phenomena of the flow, as well as others of not less interest, are capable of being produced with the Holtz machine. It is well known that stratified discharges, similar to those produced by an induction-coil working with an ordinary break, may be produced by such a machine, provided that it be furnished with the usual Leyden jars, and a high resistance (usually a piece o. wetted string) be interposed in the circuit. The absence of either of these conditions was supposed to destroy the striæ and to render the discharge continuous. Experiments which the author has recently made, but do not describe on the present occasion, tend in part, but only in part, to confirm this view. They show that for the production of striæ both quantity and resistance are necessary, that the discharge must occupy a certain short, perhaps, but finite time, or, as it may also be expressed, that a continuous current is an essential element.

Now, seeing that every tube must offer some resistance, and also that by adjusting the height of the vertical condensors of the machine (or length of air-spark interposed in the circuit) we had the means of altering the quantity in the discharge, it seemed worth while to try whether, by a suitable adjustment of the parts, phenomena similar to those brought out by the coil and high break might not be produced by the machine. And this proved to be very easy of attainment in tubes which had been successfully used by the coil; and not only so, but the character of the flow therein shown confirmed in a very striking and simple manner the effects of resistance described above.

The connections being made in the usual way, and no air-spark being admited into the circuit, a vacuum-tube of carbonic oxide, about 60 centims. in length and 4·5 centims. in outside diameter, gave, when the plates of the machine revolved at about six times per second, a rather confused discharge. As the speed was increased a rapid forward flow of the striæ was readily discerned; and on a still further increase to about ten revolutions per second, the flow, first in one part and then throughout nearly the whole length of the tube, slackened its pace and stopped, and ultimately reversed its motion. An increase of speed is equivalent to an overcoming or a diminution of resistance in the circuit, a diminution of speed to an augmentation of resistance. Hence the phenomena of flow produced by the machine agree with those produced by the coil.

The author concludes by referring to the effects obtained with sulphurous acid and other tubes, and by describing the resistance-coil used for the secondary current.

Chemical Society, June 17.—Prof. Abel, F.R.S., in the chair.—Notes on the chemistry of tartaric and citric acid, by Mr. R. Warrington, gives many important particulars connected with the manufacture of these acids; and also detailed accounts of the methods of analysis—many of them novel—of the various raw materials from which they are made.—After this the Secretary read a communication on the action of nitric acid on copper, mercury, &c., especially in the presence of metallic nitrates, by Mr. J. J. Ackworth.—Dr. Gladstone then gave a short account of the decomposition of water by the joint action of aluminium and aluminium iodide, bromide, and chloride, including instances of reverse action, by himself and Mr. Tribe.—The other papers were on nitrosyl-bromide and on sulphuro-bromide, by Mr. M. M. P. Muir.—On achrematite, a new molybdo-arseniate of lead from Mexico; and on certain new reactions of tungsten, both by Prof. J. W. Mallet; and on the action of chlorine on acetamide, by Dr. Prevost.

Geological Society, June 9.—John Evans, V.P.R.S., president, in the chair.—The following communications were read:

—On *Prorastomus sirenoïdes*, Owen. (Part II.), by Prof. Owen, F.R.S. The author has submitted the skull of a Sirenian from Jamaica, described by him in 1855 under the name of *Prorastomus sirenoïdes*, to a careful re-examination; and in this paper notices the characters revealed by further removal of the matrix, and discusses the bearings of the facts thus ascertained upon the relations of the animal and of the Sirenia generally. The parts which have been brought to light are the base and roof of the cranium, the zygomatic arches, the hind half of the mandible, with the articular part of the condyle, and the greater part of the atlas. The characters presented by these parts are described in detail, and the characters of the genus are compared with those presented by other genera of Sirenians, both living and fossil, especially *Manatus* and *Felsinotherium*. The dental formula of *Prorastomus* is given as—

$$i.\ \frac{3-3}{3-3?},\ d.\ \text{or}\ c.\ \frac{1-1}{1-1},\ p.\ \frac{5-5}{5-5},\ m.\ \frac{3-3}{3-3} = 48:$$

thus, as in *Manatus*, showing an excess in the molar series over the type of the terrestrial herbivorous mammalia, whilst the incisors and canines retain the common type as to number and kind, and have not been subjected to so great a degree of suppression or of individual excess of development as in existing Sirenians. The presence of these small subequal incisorsin both jaws of *Prorastomus* is the most marked feature in which *Prorastomus* adheres to the normal mammalian type, while showing the essential characters of the marine Herbivores; but a similar tendency is shown in other parts of the skull. The author regards the Sirenia as essentially monophyodont. *Halicore* and *Felsinotherium* depart further from the type than *Halitherium* and *Manatus*, and these than *Prorastomus*. *Rhytina*, with a better developed brain and with the jaws edentulous when adult, is an extreme modification of the Sirenian type. The rudimentary femur in *Halitherium* is to be regarded as the result of degeneration through lack of use, from better-limbed prototypal mammals. With respect to the genealogy of the Sirenia, the author remarks that Häckel derives the Sirenia, Zeuglodontes, and Cetacea, together with the Artiodactyla, from the branch Ungulata, and the Perissodactyla from the branch Pycnoderma of the Mammalian trunk; but that while *Halitherium* and *Felsinotherium* show the molar pattern of *Hippopotamus*, *Prorastomus* exhibits that of *Lophiodon* and *Tapirus*, to which *Manatus* also adheres rather than to any Artiodactyle type. The author suggests that both Ungulates and Sirenians diverged at some remote period from a more generalised (cretaceous?) mammalian gyrencephalous type? and that the marine Herbivora in the course of long Eocene and Miocene eons were subjected to conditions producing modifications of their molars, leading on one side to an Artiodactyle and on the other to a Perissodactyle character. As *Prorastomus* by its more generalised dentition and shape of brain represents a step nearer the speculative starting-point than any other Sirenian, it acquires a great interest, and the determination of the precise age of the (supposed Eocene) bed from which its remains were derived is very much to be desired.—On the structure of the skull of *Rhizodus*, by L. C. Miall, F.G.S. In this paper the author described a large skull of *Rhizodus* from the coal-shale of Gilmerton, near Edinburgh. The characters described show that *Rhizodus* is a Ganoid fish, and that its position in the order is not far from *Holoptychius* and *Megalichthys*. The author referred it to the cycloidal division of the family Glyptodipterini.—Appendix to a note on a modified form of Dinosaurian Ilium, hitherto reputed Scapula, by Mr. J. W. Hulke, F.R.S.—This paper contained a notice of the pubis of *Iguanodon*, which proves to be identical with the smaller of the two specimens figured by the author in a former paper (Quart. Journ. Geol. Soc. xxx. pl. xxxii. Fig. 1). When inverted, its long slender process is easily identified with that of the pubis of the nearly allied *Hypsilophodon*, and this slanted downwards and backwards parallel to the ischium, the little process of its posterior surface, meeting a corresponding process of the ischium, and converting the upper end of a long narrow obturator space into a foramen. The pubis of *Iguanodon* contributed largely to the formation of the acetabulum, thus resembling that of existing Lacertilia, as also in its possession of a broad ventral extension, probably united with that of the opposite side by a median symphysis. The specimens described in this paper were collected in the Isle of Wight by the Rev. W. Fox.—Notes on the Palæozoic Echini, by Mr. Walter Keeping, of the Woodwardian Museum, Cambridge; communicated by Prof. T. M'Kenny Hughes, F.G.S. The author alluded to the interest excited by the discovery of Echinoderms with flexible tests; and having pointed out the difference between the more modern and the Palæozoic forms (their plates imbricating in opposite directions), gave a description of the following forms:—(1) *Perischodomus*; (2) *Rhœchinus*, g.n., sp. *R. irregularis* (Keeping); (3) *Palæchinus* (?) *intermedius* (Keeping); (4) *Palæchinus gigas* (McCoy); (5) *Palæchinus sphæricus* (McCoy); (6) *Archæocidaris Urii* (Fleming). In conclusion, the author proposed a new method of classification for the *Echinoidea*. He also noticed the existence in the Museum of the Royal School of Mines of a British fossil which appears to belong to the group of Echinoidea with numerous ranges of ambulacral plates, represented in America by the genera *Melonites*, *Oligoporus*, and *Lepidesthes*.—On some fossil Alcyonaria from the Australian Tertiary deposits, by Prof. P. Martin Duncan, F.R.S. In a former communication in 1870 the author described some fossil corals from the Tertiary strata near Cape Otway, in the province of Victoria. In one, which he called the "Upper Coralline bed," the equivalent of the Polyzoan limestone of Woods, he found specimens which he did not then describe, as they were not true corals. Belonging to the Isidinæ, and not being of great interest, he retained them until the receipt of some similar specimens from New Zealand, described in the following paper. The Australian forms described by the author were shown to be nearly allied to the recent *Isis hippuris* and the fossil *I. corallina*.—On some fossil Alcyonaria from the Tertiary deposits of New Zealand, by Prof. P. Martin Duncan, F.R.S. The New Zealand fossils referred to in the preceding paper were sent to the author by Capt. F. W. Hutton, F.G.S.; they were derived from the Awawoa Railway cutting, and were from the upper part of the Oawaru formation. They consisted of fragments of species of the genus *Isis* and of *Corallium*. These were compared with those from the Australian Tertiaries, and the author inferred that both deposits were formed under similar conditions, and that they were at least homotaxial, whatever their precise geological age might be.—On some fossil corals from the Tasmanian Tertiary deposits, by Prof. P. Martin Duncan, F.R.S. The author described a new species of *Dendrophyllia* possessing very unusual characters, the epitheca replacing the true wall, and giving the specimen a marked Palæozoic appearance. The fossil was obtained from a Tertiary deposit, and was associated with *Placotrochus deltoideus*, a well-marked coral, characteristic of a definite geological horizon in Victoria, namely, the lower beds of the Cape Otway section, belonging to the Lower Cainozoic period. For this coral he proposed the name of *Dendrophyllia epithecata*. A much worn reef-coral was found associated with the above.

Meteorological Society, June 16.—Dr. R. J. Mann, president, in the chair.—The following papers were read:—On a white rain or fog bow, by Mr. G. J. Symons.—On a proposed form of thermograph, by Mr. Wildman Whitehouse, F.R.A.S.—On the rainfall at Athens, by Prof. V. Raulin (translated by Mr. R. Strachan). These observations were made by M. Julius Schmidt, director of the Greek Observatory, and embrace a period of twelve years and a half, viz., from August 1859 to December 1871. The average yearly fall is 15·83 inches, and the average number of wet days ninety-three. The wettest year was 1864, when 28·30 inches fell, and the driest 1862, with 9·63 inches.—On the barometric fluctuations in squalls and thunderstorms, by the Hon. Ralph Abercromby. There are two classes of storms in this country: in one the barometer rises, in the other it falls. The author in the present paper only refers to the former. After mentioning some of the phenomena which accompany storms of this class, he proceeds to give two instances as typical of their general character. In conclusion he makes the following remarks on their origin:—Though in this country squall-storms are almost always associated with primary or secondary cyclones, those in India and Africa are not connected with cyclones, and hence the source of the barometric rise cannot be due to any special phenomenon of cyclone motion. Since the rise is always under the visible storm, it is propagated at the same rate and in the same manner as thunderstorms. Enough is known of the course of the latter to be certain that they are not propagated like waves or ripples, and hence these small barometric rises are not due to aërial waves, as has sometimes been suggested. Since the general character of the rise is the same whether there is thunder or not, it is evident that electricity, even of that intensity which is discharged disruptively, is not the cause of the rise. If we look at a squall from a distance, we always see above it cumulus, which is harder and more intense in the front than in the rear of the squall. Since cumulus is the condensed summit of an ascensional column of air, it is evident that the barometric rise takes place under an uptake of air. If we consider further that a light ascensional current would give rise simply to an overcast sky, a stronger one to rain, while a still more violent one would project the air suddenly into a region so cold and dry that the resulting electricity would be discharged disruptively as lightning, the foregoing observations show that the greatest rise is under the greatest uptake. Some meteorologists attribute the low pressure at the equator to the ascending current formed at the junction of the trades; while others attribute the 10 A.M. maximum of the diurnal range of the barometer to the reaction of an ascending column of air due to the increasing heat of the day. The above observations tend to strengthen the view that an ascending column of air gives rise to a reactionary pressure downwards, and more generally to the idea that though the total pressure shown by the barometer is principally statical, or due to the weight of a definite column of air, a small portion is dynamical, or due to the reaction of air motion in that column. —Notes on solar radiation in its relation to cloud and vapour, by Mr. J. Park Harrison.—Mr. Scott also exhibited and described Lowe's graphic hygrometer.

Zoological Society, June 15.—Prof. Newton, F.R.S., V.P., in the chair.—A letter was read from Dr. A. B. Meyer, of Dresden, stating that having inquired into the statement made by Mr. Bruyn (P.Z.S., 1875, p. 30), that he had specimens of four species of Birds of Paradise alive in his possession at Ternate, he had ascertained that the foundation for this statement was that Mr. Bruyn expected to receive specimens of other species, but had only actually obtained examples of one of them (*Paradisea papuana*).—Mr. George Dawson Rowley exhibited and made remarks on some specimens of two diminutive Parrots from New Guinea (*Nasiterna geelvinkiana* and *N. pygmæa*).—Sir Victor Brooke exhibited and made remarks on two original drawings by Mr. Wolf of the two species of Koodoo, *Tragelaphus strepsiceros* and *T imberbis*. The latter was taken from a specimen received direct from the Juba River, Somali. The exact habitat of this species had not before been determined.—Prof. Owen, C.B., read a paper in which he gave the description of some bones of *Harpagornis moorei*, sent to him by Dr. Haast, which had been found in the turbary deposits of Glenmark, a locality about forty miles from Christchurch, New Zealand. This paper formed the twenty-first part of Prof. Owen's series of memoirs on the extinct birds of the genus *Dinornis* and its allies. —Mr. G. E. Dobson communicated the descriptions of some new species of bats of the genus *Vesperugo*.—A communication was read from Mr. George Gulliver, F.R.S., containing observations on the sizes and shapes of the red corpuscles of the blood of Vertebrates. These observations were accompanied by a series of drawings of these objects, and by extended and revised tables of measurements.—A communication was read from the Rev. S. J. Whitmee, of Samoa, respecting the changes he had observed in the habits of feeding, roosting, and building of the *Didunculus strigirostris*.—A second paper by Mr. Whitmee gave an account of the times of appearance of the Edible Marine worm (*Palola viridis*) in the islands of the Samoan group, together with observations on its habits.—A communication was read from Dr. J. S. Bowerbank, containing the fourth of a series of memoirs on the Siliceo-fibrous sponges.—Sir Victor Brooke, Bart., and Mr. A. Basil Brooke read a joint paper on the large Asiatic Wild Sheep or Argalis. Of these animals they recognised eight species, viz.: *Ovis ammon*, from the Altai between the Sea of Baikal and Thian Shan; *O. karelini*, from the Thian Shan; *O. poli*, from the Pamir; *O. hemsii*, from the Alexandrian Mountains; *O. nigrimontana*, from the Karatau; *O. hodgsoni*, from Little Thibet; *Ovis nivicola*, from the Stanovoi Mountains and Kamschatka; and *Ovis brookei*, of which the habitat was unknown.—Mr. Sclater read a paper on the Rhinoceroses now or lately living in the Society's Menagerie.

Victoria (Philosophical) Institute, June 21.—The Rev. Isaac Taylor, M.A., read a paper on the Etruscan language. After stating the causes which had made this language so long a mystery, the lecturer gave an account of the origin of the Etruscan alphabet, and of the information as to the nature of the language which is supplied by the bilingual inscriptions. He then gave an account of the inscribed dice, which he held to be the key to the Etruscan secret. He fully explained the Etruscan system of numeration, and showed that the numerals, the vocabulary, the grammar, and the mythology of this people all pointed to a Turanian origin.

PARIS

Academy of Sciences, June 14.—M. Frémy in the chair.—The following papers were read:—On the discovery of the two minor planets (144) and (145) by Director Peters, and (146) by M. Borrelly.—A note by M. Chevreul, on the explanation of numerous phenomena which appear as a consequence of old age.—Researches on solar radiation (continuation) by M. P. Desains.—On the synthesis of camphors by the oxidation of camphenes, by M. Berthelot.—On the water-spout which occurred near Caen in 1849, by M. Faye.—Some remarks, in complement to his note read before the Academy in May 1873 by M. Weddell, on the part played by the substratum in the distribution of Lichens inhabiting rocks.—A note by MM. E. Belgrand and G. Lemoine, on the probable decrease of flowing water in the basin of the Seine during the summer and autumn of 1875.—Report of the Commission which was appointed to examine a proposed new method in the construction of lightning conductors for powder magazines.—On the theory of revolution surfaces which, by way of deformation, can be superposed on one another, and each on itself in all its parts (second paper), by M. F. Reech.—A note by M. Sekowski, on a system of distribution in steam-engines.—On the synthesis of terpilene or carburetted camphene, by M. G. Bouchardat.—A note by M. Barthelemy, on a process to measure the co-efficient of the absolute dilatation of mercury.—A note by M. A. Rivière on the appearance of sedimentary formation in the granitic rocks now used for the pavements in the Paris streets.—A note by M. E. Jourdy on the shape of bays in the Algerian district.—A memoir by M. L. V Turquan, on the integration of the equation with partial derivatives of the third order, and two independent variables.—A note by M. Lecoq de Boisbaudran, on the theory of dissolution and of crystallisation.—Report of the falling of two meteoric stones in the United States, by M. J. Lawrence Smith, of Louisville (Ky.). The author gives a minute description and an analysis of these two meteorites.—On the influence of forests upon the climate, and on the variation of temperature with the phases of vegetation, by M. L. Fautrat.

BOOKS AND PAMPHLETS RECEIVED

FOREIGN.—Annales del Museo Publico de Buenos Aires.—Annalen des Physikalischen Central Observatoriums for 1873: H. Wild (Russia).—Morphologisches Jahrbuch. Eine Zeitschrift für Anatomie und Entwickelungsgeschichte: C. Gegenbaur (Leipzig, W. Engelmann).—Die Neue Schöpfungsgeschichte: Arnold Dodel (Leipzig, F. N. Brockhaus).—Handbuch der Zoologie. 3 vols.: J. Victor Carus and C. E. A. Gerstaeker (Leipzig, W. Engelmann).—Boletin de la Academia Nacional de Ciencias Exactus existante en la Universidad de Cordoba, Buenos Aires.—Jahrbücher für Wissenschaftliche Botanik: Dr. N. Pringsheim (Leipzig, Wm. Engelmann).—Sie Alcundi Principj di Elettrostatica. Serie di Esperienze del Prof. G. Cantoni (Milan, F. Vallardi).—Salla Polarizzione dei Coibenti: Prof. G. Cantoni.—Efficacia dei Vapori nell 'Interus dei liquidi: Prof. G. Cantoni.—Sul limite di resistenza nei Coibenti Elettrica: Prof. G. Cantoni.—Importante Osservazioni di C. B. Beggaria sui Condensatori Elettrica: Prof. G. Cantoni.—Sie Talune particolari Forme di Cirri: Prof. G. Cantoni.—Sperienze d'Elettrostatica (two parts): Prof. G. Cantoni.—Nuvoa Serie di Sperimenti su l'Eterogenia: Prof. G. Cantoni.—Verdhandlungen des Vereins für Naturwissenschaftliche Unterhaltung zu Hamburg, 1871–74: J. D. E. Schmelz (Hamburg, L. Friederischen und Co.)—Jahrbuch der k. k. Geologischen Reichsanstalt. No. 1, 1875 (Wien).—Uber die Palæozoischen Gebilde Podoliens und deren Versteinerungen: Dr. Alois v. Alth (Wien).—Uber die Triadischen Pelecypoden: Gattungen, Daonella und Halobia: Dr. E. M. v. Mojsvár (Wien).—Die Culm Flora des Mahrisch Schlesischen Dachechieffers: Dr. Steer (Wien).

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