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THURSDAY, SEPTEMBER 26, 1872

BALFOUR'S PALÆONTOLOGICAL BOTANY

Introduction to the Study of Palæontological Botany. By J. H. Balfour, A.M., &c., Professor of Botany, Edinburgh. (Edinburgh: Adam and Chas. Black, 1872.)

THE mastery of an alphabet by a child depends on his recognising and remembering the differences in form of the various arbitrary signs which we conventionally use to represent different sounds. Perhaps in the face of the alphabetic researches of Mr. John Evans we should withdraw the qualification—arbitrary. He may see a connection between the sign and the sound, and be able to give a reason for the various forms employed, and explain the influence of this horn or that loop superadded to the simple line in modifying the sound. We are, however, at a loss to discover what possible connection of affinity or even of analogy can exist between the signs O and Q and the sounds they represent. Whatever the recondite researches of the antiquarian may discover, the letters of the alphabet are practically recognised and universally received as arbitrary signs for particular sounds. The mastery of the alphabet is only the recognition and remembrance of the different signs and the sounds they represent.

This mnemonic education is not unfrequently the only education which some attain to, or perhaps are capable of. In science not a few are looked up to as masters whose extensive knowledge is nothing more than the faculty of discerning differences joined to a good memory. The rationale of the difference is another matter; perhaps they are mentally incapable of appreciating whether the distinguishing characteristics at once perceived by them are dependent on the absence of affinites or of analogies. The differences exist—that is their goal.

Many entomologists prosecute their labours on this low platform, and in botany the mere herbarium systematist occupies the same position. The one classes beetles, and the other plants as the lexicographer arranges words, by a method that exhibits their differences and permits easy reference.

But in no division of science is this class so common as in geology. The mere perception and memory of differences will give one a high class position as a palæontologist: and much useful work will such a one do—work necessary to the progress of science. True, it is not the highest class of work, and too frequently men who are experts at it aim at something higher. But deficient in the power of appreciating analogies, or recognising affinities in the points of resemblance, or their absence in the points of difference; deficient also in the exact knowledge of zoology or botany, such men have grandiloquently proclaimed and ignorantly and of course obstinately defended the most absurd opinions. Confined to the useful work of separating and recording different forms their services to science are most valuable, but the interpretation of obscure structures, and the higher problems of science, must be left to others able to deal with them.

No greater source of the evils we deprecate exists than the common but erroneous practice of separating palæontology as a science by itself. Palæontology has no grounds for recognition as an independent science. The organisms of an existing geographical province might be excluded from zoology and botany on the same grounds that are employed to exclude the plants and animals of a geological period. Zoologists are far ahead of botanists in getting rid of this error. Extinct forms have their place in every philosophic estimate of the animal kingdom. In botany, on the other hand, the study of fossil forms has been recently characterised as a non-scientific pursuit engaging the attention of the "geologist"; extinct plants are excluded from systematic works, and if dealt with at all in class books, all reference to them is eliminated from the general text, and they are confined to a page or two, or relegated to an obscure appendix.

Great praise is due to Prof. Balfour for introducing a different order of things. His manuals have always to a greater extent than any others published in the English language dealt with fossil plants; and he has now given us a special introduction to botanical palæontology. From the position obtained by the study of structural and systematic botany he deals with the problems presented by fossil plants

The earlier pages of the work are occupied with preliminary considerations as to the difficulties which present themselves in attempting to determine fossil plants, the different conditions of preservation in which their remains occur in the stratified rocks, the accepted classification of the sedimentary deposits, and the like.

The plan of the work itself is to treat of the plants as they are associated in the different recognised formations. Looking at palæontology as a separate section of the science of botany this is no doubt the obvious method of treatment, just as in geographical botany we deal with the different provinces of the surface of the earth and the floras which characterise them. If, however, the stratigraphical aspects had been subordinated to the systematic, a more valuable and instructive exhibition of the past vegetation of the globe would have been before us. For example, had the Coniferæ been traced from the first known occurrence of Abietineous wood in the Devonians, through the anomalous Araucarian wood and the Taxineous fruits of the Carboniferous rocks, the last types represented by the Walchias of the Permians and the Voltzias of the Trias, up to the appearance of the still existing groups in the Oolitic and Cretaceous rocks, an exhibition of extinct forms would have been given which would have conveyed to the botanist a clear and comprehensive view of the Order. In this way important light would be thrown on the present geographical distribution of orders and even genera. Look, for instance, at *Araucaria* and *Sequoia*,—two genera limited in numbers as well as in geographical distribution. We find the first represented by several species in our Secondary rocks, entirely absent from the Tertiaries, and at length banished to the southern hemisphere; while *Sequoia* appears in the Cretaceous strata, persists through the lower Tertiaries, and is now limited to a small geographical province in Western North America.

Nevertheless much may be said for the method adopted by Prof. Balfour. So long ago as the year 1828, Adolphe Brongniart detected a correspondence between the great divisions of the vegetable kingdom and the great epochs of the earth's history. He correlated the predominance of Cryptogams with the Primary epoch, of Gymnosperms with the Secondary, and of Angiosperms with the Tertiary. The discoveries of the half century that have elapsed since Brongniart published his views have confirmed the broad truth of his generalisations. Recently they have been expounded and illustrated by one who worthily follows his illustrious countryman in this particular field of study,—by the Comte de Saporta in the preface to his Tertiary Flora of the South of France.

Accepting this classification, which is so far both systematic and stratigraphical, Prof. Balfour prepares the student for dealing with the more obscure fossil remains by introducing each epoch with a *résumé* of the leading characters of the great group of plants which are found in it, drawn from their living representatives. The fossils characteristic of the various formations then follow in detail. The most recent observations are given. Look, for example, at the illustrations and descriptions of the fructification of the Cryptogamic plants of the Coal measures—the ferns, club-mosses and mares-tails—brought together here for the first time.

The numerous woodcuts and the admirable plates greatly enhance the value of the volume.

WM. CARRUTHERS

THE BRITISH MUSEUM PHOTOGRAPHS

Photographs from the Collections of the British Museum. Taken by S. Thompson. 1st Series. (London: W. A. Mansell and Co.)

AMONG all the varied purposes to which the art of photography has been applied, there is perhaps none for which it has proved itself more valuable than for the reproduction of ancient works of art. It matters not whether it be the sublime conception of some ancient Greek sculptor, the thorny-looking inscription on a Babylonian brick, or the stone hatchet of some prehistoric troglodyte, in each case the reproduction by the camera, if executed by a competent operator, will give a more vivid and faithful idea of the original than any drawing by however skilful an artist.

In the case of inscriptions, of minute patterns, of delicacy of form, or of the distinctive character of an object, the merely mechanical process, though not entirely without its drawbacks, possesses a great advantage over the skilful artist, inasmuch as it is entirely free from prejudice. The artist, however conscientious, is always prone to draw incorrectly such details as he does not understand, and to attempt some improvement in force and effect in those which he fully appreciates. It is only in the case of coins and of other small objects which it is necessary to hold in more than one light in order fully to discern the details, that a good drawing is preferable to a photograph; and then the question arises, what is a good drawing?

For rendering available to students the contents of a museum, photography is invaluable. By it the objects which, in many instances, it is impossible to study at leisure in their repository, are, as it were, rendered portable, and made available for extended examination at home, and for reference at a moment's notice. It is with great satisfaction, therefore, that we see this series of nearly a thousand quarto photographs of objects in our national collections issued to the public by Messrs. Mansell and Co. It is divided into seven parts—ethnographical and pre-historic, Egyptian, Assyrian, Grecian, Etruscan and Roman, Mediæval, and Seals, and one great advantage to the student is that he is by means of a comprehensive catalogue enabled to make his own selection of such photographs as come within his own particular province.

That the choice of the objects to be photographed has been judicious may be inferred from the fact that it has been made by the aid of Dr. Birch, Mr. Charles Newton, Mr. A. W. Franks, Mr. Murray, Mr. George Smith, and Mr. Walter de Gray Birch, all well known for their labours in the departments which they represent. Four of these gentlemen have also prepared the catalogue.

The photographs themselves are remarkably well and clearly executed, the figures in all cases being sufficiently large to make the details visible. We have but one fault to find, which it is to be hoped may be easily remedied—the absence of any scale on the photographs, and of any dimensions in the catalogue. In the case of some of the pre-historic and ethnographical objects it would also be an advantage if further particulars were given as to the localities from which they were obtained.

The catalogue is accompanied by an interesting introduction from the pen of Mr. Charles Harrison, giving a good general view of the progress of human civilisation, which the objects photographed illustrate, and also giving the rationale of the whole series. We cordially concur in his hope that each local museum will have its objects photographed, and that the plates like these may be made accessible to the public at a fixed moderate cost. In the meantime we commend these illustrations of our rich national collection to the readers of NATURE.

OUR BOOK SHELF

Autumns on the Spey. By A. E. Knox, M.A., F.L.S. Author of "Ornithological Rambles in Sussex, &c. (London: Van Voorst.)

WE have seldom come across a book in which the *dulce* and the *utile*, science and amusement, are so happily combined as in the modest little volume before us. Mr. Knox's main object, apparently, in spending his autumn on the Spey, was to fish for salmon in that trying river; and some of his wonderful achievements in this exciting occupation are narrated in an almost fascinating, and certainly unpretentious manner, in a few of the chapters of his booklet. But it would be a great mistake to consider this merely a book of sport, and Mr. Knox nothing more than a genial "piscator;" he has already proved what is confirmed by this his most recent work, that his knowledge of British zoology, and especially ornithology, is extensive and thorough. To any one who desires to see the report of a trained and patient observer on the zoology, and even geology, of the basin of the Spey and and of contiguous districts, we would with confidence recommend Mr. Knox's work. It contains much that is valuable and interesting on these subjects, and a good deal that is new to many. Quite charming and very curious is his account of the *modus operandi* of a family of crossbills (*Loxia curvirostris*) which he watched while standing under a tree, a few inches above his head, busily engaged at their marvellous employment of splitting the fir cones and extracting the seeds. His observations on the habits of the water-ouzel, its procedure under water, and the food it seeks there, quite redeem that lively little bird from the imputation of being a destroyer of salmon-spawn, and prove him to be the salmon fisher's best friend. But, indeed, the charms of Mr. Knox's book are many, and will be deemed an acquisition by all who take an interest in British zoology; to those who are both fishers and naturalists it will afford a rich treat. The tail-piece to the book is a beautiful woodcut of a salmon, having underneath the punning legend, "*In spe vivo.*"

Physical Geography. By Sydney B. J. Skertchley, F.G.S., H.M. Geological Survey. (London: Thomas Murby.)

THIS is one of "Murby's Series of Science Manuals" intended for use in schools. It seems on the whole creditably done, the information conveyed is valuable, and in the main trustworthy, the author occasionally drawing on his own experience for illustration. Amid the many manuals on the same subject competing for favour this deserves to take a place, though the few illustrations introduced are wretched, and there is an occasional attempt at fine writing.

Révue Photographique des Hôpitaux de Paris. Publié par Bourneville et A. de Montméja. 4ème Année. Avril, 1872. (Paris: Delahaye.)

THIS enterprising little publication deserves success. The number before us, which, however, is only interesting to our medical readers, contains three photographs (about 4 in. by 3 in.), one of a calcified enchondroma, and two of a remarkable case of encephalocele. One of these exhibits the whole infant, the other the upper part of the trunk. The details are very clearly visible, and there is an account of the case by P. Budin. The *Révue* contains also a good report of recent anatomical physiological and surgical work.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

The Potato Disease

I FEEL very much interested in the attempt you have made to connect the potato disease with cosmical phenomena, and I quite agree with you that although the *Peronospora* may be the proximate cause, yet for the ultimate cause we may have to look to a very different set of circumstances.

The researches of Baxendell, Meldrum, Smyth, and others, go to show that the convection currents of the earth are influenced by the state of the solar surface. Now surely anything that influences the motions of our atmosphere may readily be supposed to influence the distribution and activity of those disease germs that are now believed to be present in the atmosphere. Are not various kinds of blight associated with the prevalence of certain winds?

In referring to the five great visitations of the sweating sickness you say quite truly that we have no means of ascertaining the condition of the surface of the sun during those years. Nevertheless, *indirectly*, we may, I think, come to some sort of conclusion more or less probable regarding the sun's surface in those years.

This may, perhaps, be done by means of records of the Aurora Borealis. I have not access at present to the great catalogue of M. de Mairan, and I will, therefore, confine myself to the list of auroral appearances given by Mr. Jeremiah in your columns for November 17, 1870. Very great and extensive auroral outbursts are known to occur during years of maximum sun-spots, and auroræ are phenomena which appeal too much to the imagination to remain unnoticed in an unscientific age.

If, therefore, we can tell the years of very great auroral outbursts, we can at least approximate to those of maximum sun spots.

Now (quoting from NATURE) "in 1574 Camden and Stow inform us that an Aurora Borealis was seen for *two* successive nights, viz., the 14th and 15th of November, with appearances similar to those observed in 1716, and which are not commonly noticed. The same phenomenon was twice seen in Brabant in 1575, viz. on 13th February and 28th September, and the circumstances attending it were described by Cornelius Gemma, who compares them to spears, fortified cities, and armies fighting in the air." This has every appearance of a widely extended and great series of outbursts, and we may, perhaps, suppose that the maximum was not far from 1575.

Again we learn that "on September 2, 1621, the same phenomenon was seen all over France, and it was particularly described by Gassendus in his 'Physics,' who gave it the name of the 'Aurora Borealis.' Another was seen all over Germany in November 1623, and was described by Kepler." Giving equal weight to these two appearances, we may place the maximum in the year 1622.

Again we learn that "in 1707 Mr. Neve observed one of small continuance in Ireland, and that in the years 1707 and 1708 this sort of light had been seen no less than *five* times." We may in this case place the maximum in the year 1708.

We have thus selected as years of maximum auroral disturbances the years 1575, 1622, and 1708, and if they correspond approximately with years of maximum sun spots, we should expect the distances between them to be divisible by 11·1, which Wolf as well as De La Rue, Stewart, and Loewy, agree in representing as the solar period. Now the difference between 1575 and 1622 is 47 years—a period not very different from four solar periods, or 44·4 years.

Again the difference between 1622 and 1708 is 86 years – a period not very different from eight solar periods, or 88·8 years. Furthermore the difference between 1575 and 1708 is 133 years —a period not very different from twelve solar periods, or 133·2 years.

Finally the difference between 1708 and 1816·8, the period of one of Wolf's well-ascertained spot maxima, is 108·8, which is not very different from ten solar periods, or 111·0 years.

Assuming, therefore, that 1575 is not far from a period of maximum sun spots, and going backwards by steps of 11·1 years, we are led to the following dates:—1552·8, 1530·6, 1519·5, 1508·4, 1486·2, as years of maximum spots, whereas the dates of sweating sickness were 1551, 1528, 1517, 1506, 1485, and the differences between the two sets are as follows: 1·8, 2·6, 2·5, 2·4, 1·2, the mean being 2·1 years, and the difference always in the same direction.

It is, of course, hazardous to place much confidence in these results; nevertheless, it is worthy of remark that the greatest difference between observation and calculation from hypothesis, recorded in the communication, is 2·8 years, whereas it might sometimes have been 5·6 years on the supposition that there is no truth whatever in the hypothesis.

I shall only remark in conclusion that when we have arrived at the position of being able to explain by a probable hypothesis the cause of spot variations, we may perhaps be able to test our conclusions by means of these early notices of the Aurora Borealis. B. STEWART

HAVING been from home, it is only now that I have read your very interesting article of Sept. 12, on the Potato Disease.

It is certainly most desirable that "an investigation into the origin, cause, and remedies" thereof by the ablest of our scientific men should be promoted; but it appears to me that this is a case for private contributions rather than an appeal to Government. I would, therefore, suggest that a fund be raised by subscription to supply the means of offering three prizes for the above object: the first I should hope would not be less than 500*l.*, the second and third 300*l.* and 200*l.*

These sums would offer an inducement to the ablest men to devote to the object a portion of that time and talent which, with many of them, forms the chief (sometimes the only) source from whence their income is derived.

The judges might be appointed by such of the subscribers as could meet at a given place after due notice.

The sum required, including expenses of advertising, &c., would not be large; there ought to be no difficulty in raising it when we consider what a large interest is at stake.

I should be happy to subscribe 5*l.*

Richmond, Surrey, Sept. 23 M. MOGGRIDGE

On the Substance Exhibited at the British Association, Brighton, by Mr. P. L. Sclater, and stated to be the Ossified Notochord of a Fish

NEARLY every one frequenting the Zoological Section at the British Association at Brighton must have seen and been puzzled by a substance exactly resembling in external appearance a slender willow twig when perfect, two feet or more in length, and pointed at both ends.

This substance was exhibited by Mr. Sclater, and pieces of it were freely distributed by him for examination.

I was, unfortunately, not present when he read his paper on the subject; but I gathered that he had said that the substance had been described to him by the person who sent it as occurring in the back of a fish, and that Mr. Sclater called it an ossified notochord. A drawing of the fish was exhibited.

I further heard that Mr. Gray, of the British Museum, regarded the substance as the axis of one of the Pennatulidæ, and that this opinion was held by several other naturalists also.

I first became acquainted with the substance at the Kew Herbarium, where a piece of it was shown me by Mr. Berkeley, it having been given to him by Prof. Thiselton Dyer; and I was told that Dr. Hooker had examined it with the microscope, and rejected it as certainly not vegetable.

It was almost impossible to conceive of the substance being the notochord of a fish. No fish's notochord is composed of longitudinal fibres, nor has a structure at all resembling that of the substance in question; and moreover a notochord in such a fish as a lamprey, in which it is persistent, is much thicker in proportion to its length than are these calcified rods. Further, the tendency is for a notochord to ossify peripherally, and form rings of bone, not a hardened central core.

On reaching Oxford from Brighton, I got Mr. Robertson to give me a specimen of *Funicularia quadrangularis*, one of the Pennatulidæ, which was preserved in spirits. I found it had a long slender flexible core, exactly similar in appearance to Mr. Sclater's substance, but quadrangular in section instead of circular. The core was about two feet and a half long, and pointed at both ends. Microscopical examination of longitudinal sections of the core, when treated with acetic acid, gave off carbonic acid in quantities, and showed a structure almost exactly resembling that observed under similar circumstances in Mr. Sclater's substance.

I then looked into the literature of the subject, which fully confirmed me in the opinion that the substance in question is the core of one of the Pennatulidæ. A few statements, culled from the two works I consulted, may be interesting to the readers of NATURE. The works were "Anatomisch-systematische Beschreibung der Alcyonarien," von A. Kölliker, Erste Abtheilung: Die Pennatuliden. Erste Hälfte (Frankfort: C. Winter, 1870). "Icones Histologicæ, oder Atlas der Vergleichenden Gewebelehre," herausgegeben von A. Kölliker. Zweite Abtheilung, Erster Heft. Die Binde-substanz der Cœlenteraten, p. 158 (Leipzig: W. Engelman, 1866).

The Alcyonariæ, a sub-order of polyps, are divided into three groups:—(1) Alcyonidæ; (2) Gorgonidæ; (3) Pennatulidæ.

The Pennatulidæ consist of hard and soft parts. The hard parts appear in most varieties in the form of an inner calcified axis, which in size and position is like that of the Gorgonidæ. It is to be considered as calcified connective tissue, is entirely and completely enclosed within the substance of the polyp colony, and is pointed at both ends.

The Pennatulidæ are thus divided:—

I. Pennatulidæ with polypbearers bilaterally symmetrical.
  A. Polypbearers feather-shaped in Pennatuleæ.
  B. Polypbearers leaf-shaped in Renillaceæ.

II. Pennatulidæ, with polyps arranged radially.

The Pennatuleæ break up into (1) Penniformes; Pennatuleæ with a well-marked feather-shape; (2) Virgularieæ; Pennatuleæ with a long, narrow polypbearer, and small leaves or polyps resting immediately on the axis. To this latter group belongs the genus *Funicularia*, and probably also the genus to which Mr. Sclater's specimen belongs.

With regard to the fine structure of the hard axis of Pennatulidæ, I have gathered the following from Kölliker's "Icones," p. 158.

The axes of Pennatulidæ consist of calcified horny substance, arranged in concentric lamellæ about a central core. The lamellæ are pierced by peculiar soft radial fibres, which, however, are well defined in certain species only. The organic basal substance shows an extremely well marked fibrillar structure. The axes are less firmly calcified than those of the Gorgonidæ, and are thus for the most part able to be cut with a knife and bent.

According to the analyses of Fremy (Ann. de Chimie, 1855, t. xliii. p. 98), the axis of *Pterocides spinosum* contains from 31 to 40 per cent. mineral matter, and that of *Pennatula rubra* from 45 to 48 per cent.

A drawing is given by Kölliker of a transverse section of the axis of a Virgularia (*Lygus mirabilis*) prepared by grinding, which shows a white central core, surrounded by a broad brownish cortex, which is marked with concentric and radial lines.

If the axis of a *Lygus* be treated with acetic acid, a development of carbonic acid takes place. It becomes soft, and allows the following structure to be made out:—

The bulk of the axis consists of a fibrous tissue which resembles ordinary fibrillar connective tissue in the most deceptive manner, and consists of very fine fibrillæ, which run parallel to one another in a wavy fashion, and which can be isolated from one another. On the surface of the axis is a yellowish cuticle.

Drawings are given of longitudinal sections of the axis of *Lygus mirabilis*. It shows the peculiar broad transparent radial fibres crossing the finer longitudinal ones. In another figure of a similar preparation from *Funicularia quadrangularis*, these radial fibres are less marked, but the cavities containing them appear as oval apertures in the section.

Reference is made to Quekett, Lectures on Histology, II., and Histological Catalogue, I., where the structure of the axes of *Pterocides*, *Lygus*, and *Funicularia* is described, but the radial fibres mistaken for canals.

I think any one who has examined Mr. Sclater's substance, and very many have had such an opportunity owing to his kindness in distributing pieces, will find that both in external characteristics and internal microscopical structure, it conforms very closely to the description given here from Kölliker of the axis of the Pennatulid. I have sent the Editor of NATURE some pieces of the axis of *Funicularia quadrangularis* in case any one cares to compare the two substances, and has not the material at hand. In the mean time I cannot but conclude that Mr. Sclater has been misinformed, and that we are very unlikely ever to see that very marvellous fish in the flesh.

H. N. MOSELEY

Ocean Currents

HAVING just returned from a sojourn of nearly two months amongst the White Mountains, I am now for the first made aware of the publication, both of my last note on Ocean Currents, and also of Mr. Croll's reply. I have not been disposed to enter into an extended discussion of this subject, knowing that it cannot be properly treated without the use of mathematics, in short essays suited to NATURE, and doubting whether the discussion could be made either acceptable to its Editor or edifying to its readers. In my last note, therefore, I endeavoured to be as brief as possible, and considered only the more simple form of the conditions of the problem, as expressed by differential equations, showing the relations between the forces, resistances, and the differentials of the motions, and showed that the deflecting force eastward exerted upon a pound of water or any body in moving toward the pole with a velocity of one mile per day, and which must be sensibly the measure of the resistance of friction, is of the same order near the parallel of 45° as the action of gravity on the same body upon a gradient of 6 ft. from the equator to the pole; and from tidal considerations it was inferred that the resistances to the slow motions of ocean currents may be very much less than the action of gravity upon any body apon a gradient of 6 ft. in the distance of a quadrant.

If, instead of considering the differential equations of any problem, and endeavouring to satisfy them directly, we adopt the less simple method, and consider the integrals of these equations, and endeavour to satisfy them directly, the method, though less simple, is entirely legitimate, and we should obtain the same results. This is substantially the method adopted by Mr. Croll; and from considering the problem in this way, he comes to the conclusion that the deflecting force eastward, which is the measure of the resistance, is at least 1,500 times greater than the action of gravity on a gradient of 6 ft. from the equator to the parallel of 60°; and as the velocity of the pound of water eastward, and that toward the pole, are probably about of the same order, and consequently the resistances, he justly infers that the resistance to the motion toward the pole must be overcome by some other force than that of gravity due to the assumed gradient. But it may be shown by the other method of considering the problem, by assuming a motion towards the pole of one mile per day, that the forces which overcome the resistances in both directions are about of the same order. This very great difference in the results obtained from the two methods of considering the problem, indicates that there is some great fallacy somewhere which needs looking after.

Mr. Croll is misled by adopting the erroneous principle that the amount of work performed by gravity upon a falling or descending mass is in all cases expressed by the mass multiplied into the height through which it descends, or that the foot-pound is a unit of work. The amount of work required to give velocity to any body, or overcome any kind of frictional resistance, is expressed by the intensity of the effort, regarded as constant, multiplied into the time of action. The intensity of the effort of any force, as of gravity, is explained by the mass multiplied into the velocity which such force can produce in a unit of time. If we, therefore, put g, m, t, and v for the force, the mass, the time, and the velocity respectively, we shall have, putting W for the amount of work performed by gravity,

$$(1) \quad W = mgt = mv,$$

that is, the amount of work performed is expressed by the momentum. Now this amount of work is stored away in the moving body, and remains until it is used in overcoming resistance of some kind, as friction or the inertia of other bodies, and W is exactly the expression of the working power which has been communicated to it. But if the moving body has been subject to resistances, as of friction, during the time t, then we shall have

$$(2) \quad W = mv + ft$$

putting f for the coefficient of friction, and supposing it to be constant. In this case mv expresses the amount of work left which has not been expended in overcoming the friction during the time t, and of course in this case we cannot get all the work back again which has been expended, at least mechanically.

If we now suppose a body to fall in a vacuum through the space s, if the amount of work performed by gravity upon it, and the working power communicated to it, is expressed by the mass multiplied into the height through which it has fallen, we shall also have

$$(3) \quad W = ms = \tfrac{1}{2}mgt^2 = \tfrac{1}{2}mvt.$$

Hence, comparing the preceding expressions of W, we have $mv = \frac{1}{2}mvt$, which is impossible; and therefore, if the former expression of W is correct, the latter is not.

Again, illustrating by a special case, if we suppose a body, of which the momentum is mv, to move upon a level plane without friction, and the plane to curve up in the direction of motion, and also suppose another body with half the mass and double the velocity, of which the momentum is $\frac{1}{2}m \times 2v = mv$, m and v being the mass and velocity of the first body, it must be admitted that the amount of labour expended in giving both bodies the momentum mv is exactly the same, in the latter case the intensity of the effort being half as great, but the time of action twice as long; but the momentum will carry the latter up the slope to a height four times greater than that of the former, and after descending again to the plane, both will have the same momentum, and the same amount of labour would be required to bring each to rest, and consequently both have the same power of doing work. But the mass of the latter multiplied into the height through which it has descended is double that of the former, and hence these products do not express the power of doing work which gravity has communicated to them.

In the case in which the descending body is resisted by friction, we have seen (2) that neither the mass multiplied into the height of descent, nor the momentum mv which the body has on arriving at the level plane, expresses the whole action of gravity, and the resistance fv may be so small as not to affect sensibly the amount of work expressed by mv, or it may be so great that mv may be neglected, in comparison with ft. The value of ft may also be so great that the value which mv would have (1) in the case of a free body falling through the space s, would be almost infinitely small in comparison with the whole expression of W (2). In the various cases, therefore, which may be supposed, in which friction may be either very small or very great, so that in the former case the effect of the resistance might be scarcely sensible, and in the latter it might take the action of gravity a long time to drag the body down through the space which it has to descend, we cannot suppose that in all these cases the whole action of gravity is expressed by the same number of foot-pounds, supposed to be units of work.

If, therefore, Mr. Croll's pound of water were moved from the equator to the parallel of 60° by the action of gravity without any resistance, the momentum which it would have on arriving there would express the work done by gravity upon it, and not the six foot-pounds, and the work would be done in a very short time in comparison with the time in the real case of nature; but when it is dragged down there through all the resistance which it suffers, at the rate of a mile per day, as we have supposed, the amount of labour which gravity performs is very many times greater than that expressed by the momentum which the pound of water would have on arriving there without resistance; and with regard to the six foot-pounds, we have seen that the work is no more comparable to them than a surface is to a solid.

Again, if we suppose the gradient upon which gravity acts to be only one foot in the distance instead of six, and the resistance to the pound of water to be as the velocity, then the water would move with only one-sixth of the velocity in the other case, and the water would be six times as long in reaching the parallel of 60°, but the energy of the action of gravity would be only one-sixth as much, and hence the work would be the same, being carried on six times as long in the latter case with one-sixth of the energy. But then the same work would be represented by one foot-pound, if that is a true unit of the work instead of six. In this case also the deflecting force eastward would be only one-sixth as much, but the time being six times as long, the same amount of work would be done, and this would be sensibly the same as that which would be required to give the pound of water a velocity of about 760 miles, as Mr. Croll has it, but really double that amount.

I am well aware that in the action of machines in which force is balanced against force, and consequently the times of action are the same, the amount of work may be expressed by the forces multiplied into the spaces through which they act; but in all cases in which the times differ, the amount of work cannot be expressed by any unit into which the element of time does not enter. The amount of work required to produce a velocity of 760 miles per hour is a function of the time, and proportional to the time where the force is constant, and cannot be measured by foot-pounds.

With regard to the argument based upon M. Dubuat's experiment, the matter briefly and fairly stated stands thus: according to the experiment, water will not flow unless acted upon by a force equivalent to that of gravity upon a given gradient, which makes the force required to move it about fifteen times greater than the horizontal component of the moon's disturbing force which produces the tides. But this force of the moon does move the water of the ocean, and therefore M. Dubuat's experiment is not applicable to water of great depth as of the ocean, and the argument fails. It is true, as Mr. Croll states, that the two cases of motion are somewhat different; in the case of the tides the water from top to bottom flows in the same direction, while in the other the upper and lower strata flow in contrary directions, and the resistances to the lower motions are no doubt greater. Six of the nine feet therefore of Mr. Croll's gradient should probably be given to the lower currents, and only three to the upper ones. But Mr. Croll admits that in the case of the tides a gradient of one inch is sufficient to move the water. A gradient of three feet only, therefore, ought to be sufficient to move the upper half, which would correspond somewhat to the case of tides in an ocean of half the depth. The observations of Col. Graham show that the water of Lake Michigan, about 700 feet deep, readily yields to the moon's disturbing force which causes a tide at Chicago with a range of nearly two inches.

Cambridge, Mass., Sept. 7 WM. FERREL

Spectral Nomenclature

IT seems almost absurd that a subject of such interest, and, as I think, importance, as that of Spectral Nomenclature should be discussed from opposite sides of the globe alone; so it may be hoped that it will not have been allowed to end with Prof. Young's remarks upon the one or two points in which he differs from me, but will have been taken up by others at a less distance. There is a great deal more to be said about it; but probably I should not have troubled you again just yet but for the obligation I feel to disclaim credit which he gives me for what is not mine.

Doubtless it is not necessary, but the obligation is the same; and a mistake into which Prof. Young has fallen is open to others.

The map which he wants—to be based on inverse wave-length or rapidity of vibration or pitch—is, I believe, in course of construction by Dr. Huggins, to whom, and not to me, is due, I think, the first idea and the proposal. Prof. Young has probably associated my name with it through a lecture delivered by my brother at Glasgow, in 1869, in which it was advocated.

I also wish to acknowledge that I was not aware of the fact which has now been so decidedly stated, that the coronal green line is certainly *practically* identical in position with 1474 (K), as tested by direct comparison. Indeed, I was ignorant that such a comparison was possible, having supposed that the line in question was only visible during eclipse. I ought, of course, to have referred to Prof. Young's "Preliminary Catalogue," and probably should have done so had I been in a house instead of in a tent, a few score of miles from the nearest station. But in truth it did not occur to me that there could be any certainty about the position of a line which, as a coronal line, had never been fixed by measurement. I may now venture to ask, What guarantee was there that No. 31 of the "Preliminary Catalogue" was "the coronal line," anterior to the Dodabetta measurement? I do not question it now, but I should like to know if the presumptive identity is supported by any characteristic difference between that line and those which are presumably due to the chromosphere. There is still a link wanting.

However, admitting the identity, and therefore the accuracy of the assigned position, we may still believe what Prof. Young says he would be glad to see proved, that "the apparent coincidence (with the iron line) is merely a very close juxtaposition." More than this: even were a very much higher dispersive power to show no resolution of the identity, should we not still be in nearly the same position as to any inference to be drawn therefrom? Evidence of physical relation between metals which present one or more lines common to both spectra may, indeed, eventually be shown (by the improbability of so frequent an accidental concurrence) to amount to proof. But this must be a prior step. To conceive it taken, and then to apply the like reasoning by analogy to the case of the single coronal line tallying with an iron line, seems to me speculation of the second order. Undoubtedly it would be matter for congratulation to be relieved from the liability to temptation of this kind by definite disproof. In the meantime, I cannot but regret that Prof. Young has half neutralised the good of a plain disavowal of belief in the ferrous interpretation of the coronal green line, by hazarding the query whether it may not "turn out" to be quasi-ferrous.

I am sorry—to return to the subject of nomenclature—that your respondent does not agree to my objection to "D_3." Is it not plain that such a designation is haphazard? The association of idea is with D_1 due to sodium, instead of with the origin or source of the line. It tells nothing beyond the position, roughly, in the spectrum, by reference to a position which we happen to be familiar with, but with the occupant of which it has no connection otherwise. The name, in short, has no foundation in principle; and that, I apprehend, is a lack of the first requisite in a scientific name.

The objection to Greek alphabet letters is of a different character, but not less easily answered. It is very true that, through the exertions of Prof. Young and others, "the whole Greek alphabet would not suffice to name one in three of the lines" already known; but it would nevertheless suffice (as in the somewhat analogous case of the stars) "to express as many as the memory would require to hold." There is ample precedent. The principal lines of the elements, like the principal stars of constellations, are known to some extent by Greek letters; and as for the difficulty in respect of order, there was a time when the "lucid" stars, though very many in number, and having no very clear claims to precedence, were only known individually by personal names. Yet no sooner did a Bayer rank and name them according to apparent brilliancy, by Greek letters (to say nothing of the Roman), than the advantage of a fixed nomenclature was recognised and his work accepted; although observation must have shown that the assigned order was not always strictly correct. So would it happen now if, the lines having been lettered, further knowledge should show that the established precedence was not quite all that could be wished. The evil of slight incorrectness of this kind would be felt to be trifling compared with that which would result from an unsettling of a nomenclature established solely for convenience and involving no theory.

This would not prevent nor conflict with, neither would it render unnecessary, a far more extended tabulation depending on refrangibility. On the contrary, the want of such a classification and means of indication would be felt as soon as precise tabulation should come to be undertaken. In Kirchhoff's solar chart we have, graphically, something like what is wanted in a much more general, numerical, and tabular form—an example of a catalogue of lines. Charts are very useful, but not most handy; and they are not susceptible of such ready improvement and extension. The accumulation of results of spectroscopic research must sooner or later take the form of a catalogue of lines, from all sources, arranged in order of refrangibility; designating individuals (for special reference) where possible, according to their parent element or compound, their physical source, their cosmical habitat, or other characteristic and distinguishing indication, implied under the system of nomenclature which may be adopted; upon which would follow such details as to character (including intensity, width, definition, complexity, variability, &c. &c.) as present knowledge or future research may represent as suitable material for incorporation.

So long as spectroscopic analysis is content to remain in its earliest stages—and it must be allowed to be still in its infancy, though a giant from its birth—the student and experimentalist may to a very considerable extent learn by heart or by practice such spectra as he needs; but this can never suffice for all purposes. Accumulation is continually going on, and products must be stored. Let that be once acknowledged and the task attempted, and it must follow that, no matter how rigorous and precise may be the system of tabulation, there will be not only room, not only gain, but a positive necessity for an intelligible use of that kind of descriptive indication which is only to be found in scientific classification and nomenclature.

A general catalogue such as I contemplate would command, if compiled with even moderate knowledge and care, a very general acceptance. Unquestionably it would be extended, modified, improved upon, by subsequent work; but, so far as nomenclature is concerned, it would probably undergo but slight alteration—the less the better. It would form a basis on which any number of special catalogues might rest, without interfering with its permanence as a catalogue of reference.

I do not pretend to say that the task is a simple one; quite the reverse. But, then, all the more honour to whoever accomplishes it. J. HERSCHEL

Bangalore, July 29

Jeremiah Horrox

I OBSERVED in a number of NATURE some three weeks ago an inquiry relative to J. Horrox, the astronomer. My wife is descended from Horrox, and I knew that one of her friends had his life, but have not been able to find it till now. The book has just been sent to me here.

The Rev. Jeremiah Horrox was born in Toxteth Park, near Liverpool, in 1619, and died in 1641, aged 22.

The life is by the Rev. A. B. Whatton, published by Wertheim, Macintosh, and Hunt, 24, Paternoster Row, 1859, and includes a translation of his discourse on the Transit of Venus.

HENRY HOLIDAY

Muncaster Castle, Ravenglass, Carnforth, Sept. 20

Millions of Millions

WHY do not Messrs. Ranyard and Co. adopt the late Benjamin Gompertz's most convenient notation of prefixing a circle to the first significant figure, or suffixing a circle to the last significant figure having therein a digit for the number of zeros employed?

Thus: ·⑤718 is ·00000718

And 718⑥ is 718000000

S. M. DRACH

74, Offord Road, N., Sept. 17

Analogy of Colour and Music

IN NATURE, No. 150, p. 393, a letter from Mr. G. C. Thompson is headed "Correlation of Colour and Music." As this letter refers to a paper of mine published some time ago, permit me just to say that Mr. Justice Grove has in your journal objected to the use of the word "correlation" employed in this sense. Entirely coinciding with the opinion of the eminent parent of this work, I wrote as follows in NATURE for February 17, 1870:—"*Analogy* is certainly far more appropriate to express what is merely a parallelism, and not a necessary or complementary relationship between light and sound." In the subsequent letter on this subject you adopted the word "analogy;" pardon, therefore, my pointing out an obviously accidental "reversion to the primitive type" which appeared in your paper Sept. 12.

Sept. 16 W. F. BARRETT

The Fringes on the Lighter Side of the Rainbow

AT the place referred to by Mr. Thompson in NATURE (No. 150, p. 393) I merely followed Sir John Herschel; expressing myself, it is true, not very accurately, in my anxiety to save space in NATURE at the end of a letter already too long. If Mr. Thompson refers to Sir John's "Meteorology," sections 219 to 224, and still thinks the point requires fuller elucidation, he may possibly supply the deficiency by devising an experiment to prove that the width of the fringes does not vary inversely as the diameter of the drops.

In answer to Mr. Thompson's concluding question, perhaps somebody else will furnish the latest intelligence. I do not know what has happened in the last five years, and I do not know what you call violet; but I believe that in 1867 the extreme rate of vibration for visible rays was about 801 million millions a second. C. J. MONRO

A Curious Phenomenon

A VERY curious phenomenon was witnessed here on Wednesday afternoon last, September 4, about three o'clock, in a westerly direction. A somewhat heavy thunderstorm, originating towards the south, had divided its fury before reaching this immediate neighbourhood, one branch passing N.E. towards the Pennine Hills, the other taking the N.W. course, that to the N.E., however, being more violent. As the storm was passing, a stream—*apparently* of water, and fully six inches in breadth—shot with considerable speed from the vicinity of a dark, fiery cumulus across a rain cloud of a very deep blue, murky tinge. Its passage, as witnessed by my boy from its commencement, was similar to that of a rocket, at firs assuming a quivering motion, then darting suddenly forward, for some distance horizontally, afterwards obliquely. Its apparent length would be fully twenty yards, being of a very light slate colour. After I saw it the phenomenon remained about two minutes; but its total duration would be not less than five, vanishing gradually during its whole length.

Whatever the phenomenon itself—or its cause, its upward course was certainly very striking, and to me unprecedented—the impression on some people's minds being that it was water drawn up from Lake Ullswater into the clouds by the lightning!! A terrific storm of thunder and lightning occurred on the previous evening at 9 P.M., when several fatal accidents were reported. T. F.

Blencowe School, Cumberland, Sept. 7

APPEAL FOR SKELETONS OF WILD SPECIMENS OF THE LARGER CARNIVORA FOR OUR MUSEUMS

NEITHER in the Museum of the Royal College of Surgeons nor in that of the University of Oxford is there a skeleton of a wild lion or a wild tiger, and it is probable that there is no such skeleton existing in England. The preparations in our Museums, illustrating the anatomy of the larger carnivora, are almost without exception derived from menagerie specimens.

Lions breed well in confinement, and hence an ordinary menagerie specimen may not only itself have been during its whole life confined in a cage, but its ancestors may have suffered a like fate. At all events it has been trapped whilst still young, and reared in confinement, as is usually the case with the menagerie tiger. Now an animal confined in a narrow space from its youth upwards never has free play for its muscles, and as its food is provided for it, is never called upon to exert them in a violent manner. The result is that the bony framework on which the muscles act never attains in such specimens its full development, and the ridges and inequalities on the bones corresponding to the origin and insertion of the muscles are not well marked. Moreover, menagerie animals, as is well known, very frequently suffer from diseases of the bones, and the marks of these diseases may be seen on many of the skeletons in every anatomical museum. Now, it is of great importance to possess perfect skeletons of adult wild large carnivora, both for general study, and more especially for comparison with the remains of similar carnivora which are to be found in the more modern geological deposits in Great Britain. Considering the number of tigers and lions which are annually killed by English sportsmen, it is surprising that this desideratum has not yet been supplied. The reason probably is that sportsmen generally do not know that it exists, or do not understand how a skeleton should be prepared. The sportsman is usually content with preserving the skin of his tiger or lion; but no doubt there are many who would gladly aid the cause of science by preserving the skeleton as well, if they knew how much the result of their labours would be valued at home. I propose here to give a few simple directions for the rough preparation of skeletons for transmission to England, merely premising that I trust that if any sportsman may be induced by reading these notes to send home a skeleton, that he will send it to the Oxford Museum, in which I am especially interested, and I hope some old University man may help us in this matter. Any packages should be addressed to Prof. Rolleston, Museum, Oxford. Skeletons of other wild animals are, of course, of great value, and will be most gladly received; they also are too frequently only to be got from menageries.

Directions for Preparing Skeletons.—The skin having been removed from the animal, the abdomen should be slit open, and all the viscera extracted. The limbs should then be severed from the body, the scapula or blade bone being left attached to the fore limb, the hind limb being removed at the thigh joints, and care being taken that the articular surfaces are not injured in the process. The flesh should now be removed roughly from each of the limbs with knives; the several bones which go to form each limb should if possible be allowed to remain attached to one another. On no account should the small bones of the hind or fore foot be separated from their attachments. Mr. Flower, indeed, advises that the skin be not removed from the feet at all. The limbs being thus roughly cleaned, they should be placed in water for several hours to allow the blood to soak out, and they should then be placed in the sun till dry. The head should be disjointed from the neck, and the flesh cut off it. It is most convenient to commence with the strong muscles of the jaw. After these have been cut through, the ligaments which hold the lower jaw in place may be divided, and it may be separated from the skull. The tongue may now be removed, and search must be made in its base for several small bones constituting the hyoid apparatus, which should be carefully taken out, and tied at once to the lower jaw for fear of loss. A considerable quantity of the brain may be removed by means of a spoon-shaped stick through the aperture at the back of the skull where it joins the neck. The rest may be removed by means of large shot put in at the hole, and shaken up with water. The neck may be cut off close to the trunk, and the tail close to the rump, and the flesh removed with the knife. The chest cavity should be left entire, the flesh being removed as well as circumstances will permit. The whole of the pieces should be treated with water, and then dried, as in the case of the limbs. The skull, limbs, tail, and neck may be conveniently placed inside the chest cavity for packing, and if it be necessary to get the skeleton into a short packing case the back bone may be divided behind the chest cavity, and the hinder vertebræ and hip bones laid along side of it. The tail may also be divided into segments. The skeleton should be well packed in dry hay or straw.

Precautions.—The bones should on no account be boiled or placed in hot water. They should not be allowed to remain in the sun after they are once quite dry. In severing the various portions of the skeleton from one another, great care should be taken that the knife passes between the bones through joints, and that the bones are not cut or injured in the process.

H. N. MOSELEY

*NOTICE OF A SUPPOSED NEW MARINE ANIMAL FROM WASHINGTON TERRITORY NORTH-WEST AMERICA**

SOME months ago Capt. D. Herd of the Hudson's Bay Company's service, sent me several specimens which at first sight appeared to resemble long thin peeled white willow wands more than anything else. These objects, of which I exhibit examples, are about a quarter of an inch in diameter at their thickest part near the base, and taper gradually to a slender apex. The base also narrows slightly and presents traces of corrugations. The longest are upwards of six feet in length. Capt. Herd merely stated that they had been brought from North West America, and asked me to find out what they were, promising an account of all he knew about them on a future occasion.

Expecting to see Capt. Herd very shortly, I did not myself make any very accurate examination of these objects, but I convinced myself that they were of animal origin, and was inclined to regard them as possibly bones of one of the gigantic Rays. I gave specimens of them to Prof. Flower, Prof. Milne-Edwards of Paris, and several other naturalists,† who visited the rooms of the Zoological Society, and who all said that the objects were new to them and that they did not know what they were, but were mostly inclined to regard them as the axes of an unknown Pennatulide animal.

Knowing that Prof. Kölliker of Würzburg had recently been engaged on a monograph of the Pennatulidæ, I likewise sent him a specimen, in reference to which he was kind enough to write to me as follows:—

"The object you sent me, found near Vancouver Island, is indeed the axis of an unknown Pennatulide, and agrees with none of those described in my monograph. It differs from all axes of Pennatulidæ investigated by me, in showing no radial fibres, not even the very short ones, described by me in *Funiculina quadrangularis* and *Halipteris* (*Virgularia*) *christii*, and may therefore belong to a new genus. Except in this respect the said axis agrees most with that of *Halipteris christii*, but there is also a difference, as the axis of *Osteocella*, as we may call it, is absolutely *quadrangular* in its lowest part for the length of about 3 centimetres.

"I put the four pieces you sent together and found a total length of 1·769 metres.

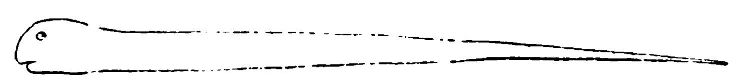
"The thickest part is found at the distance of about 0·210m. from the lower end, and measures 6·3mm. in breadth. Both ends are broken; the lower measures 1·8 mm. in breadth, and the upper 0·5mm. In general the axis is cylindrical and smooth but there are granulations and warty excrescences on the lower end for a length of about 0·20m. The axis is calcareous, and shows after the extraction of the earthy matter fine fibrils and lamellæ like the axes of all other Pennatulidæ.

"I may further add, that no Pennatulide of this size is known from the west coast of America."

Shortly after I had received this communication from Prof. Kölliker, I obtained from Capt. Herd the following account of the manner in which these objects had come into his possession.

"These rods are the back bones of a sort of fish found in great abundance at Barraud's Inlet, Washington Territory, North-West America, whence they have been brought by two Captains in our service. These animals are shaped like a Conger eel, but are quite transparent, their bodies being composed of a mass of jelly—they are about 8 inches in diameter. The head is like a shark's head; it is attached to the thick end of the rod—it has two eyes and a mouth placed low down. The back bone is also transparent in the living animal, but becomes hard when dried on the beach by the sun. These fishes swim about in shoals along with the dog-fishes.

The rods were brought by our ships *Prince of Wales* and *Princess Royal*, Capt. Anderson, who has made me the accompanying sketch of the fish itself."



Found at Barraud's Inlet, Washington Territory, amongst the Dog Fish.

A somewhat similar account of the origin of these objects is given in the subjoined extract from a letter of the Hon. Mr. Justice Crease, of Victoria, British Columbia, who has recently sent a specimen of the same object to the Royal Horticultural Society.

"I send you by this post a specimen, which Mr. Claudet (Superintendent of our Government Assay Office) has sent to me, to inquire what it was, of the bone of a fish taken frequently in Barraud Inlet, near New Westminster, Fraser river, by Messrs. Dick and Nelson at their Saw mills. There has been a great discussion here among brother ignoramuses as to whether it is vegetable or animal production. Though it has a singular breakage it answers to the test as lime. Claudet is a clever man, and thinks with me that it is bone. I have broken it in several pieces for convenience of transmission. Can you tell us what it really is? I have seen several like it and from the same place. Dick and Nelson are both respectable men and Claudet of course is beyond suspicion." (May 10, 1872).

Capt. Anderson being absent from England, I have not been able to ascertain whether the information above given was founded on his own observations or on the accounts given to him by the inhabitants of the district of Barraud's Inlet. Supposing the former to be the case, and that these objects are really derived from such an animal as is described and figured above, I can only suggest that they may be the hardened notochords of a low-organised fish, allied either to the Chimæroids or to the Lampreys, in which the notochord is persistent throughout life. It is quite certain I think, that they cannot be any part of the true vertebral column.

But whether this be the case or the Pennatulide view of their origin be the true one, it is certain that the animal that produces these curious rods is quite unknown to us, and it is highly desirable that specimens of it should be obtained. I have already requested Capt. Herd to communicate with Capt. Anderson on this subject, and trust that on his next return from Barraud's Inlet he will bring us the entire body of this wonderful creature preserved in spirits. I hope also that if any student of "NATURE" in Washington territory may chance to read this article he will not fail to exert himself and assist us in solving this somewhat puzzling zoological problem.

P. L SCLATER

* The substance of this paper was read before Section D at the meeting of the British Association at Brighton, August 20, 1872.

† A specimen given to Dr. Günther was handed by him to Dr. Gray, who to my great surprise without consulting me or even ascertaining correctly where I had obtained it, immediately described it in the "Annal of Natural History" (Fourth series, vol ix. p. 405) as a "new species" of his genus "*Osteocella*," whatever that genus may be, for its author considers it "very doubtful whether it belongs to the Pennatulidæ" and states that "it may be the long conical bone of a form of decapod cephalopod." !

A GIGANTIC "PLEASURING GROUND": THE YELLOWSTONE NATIONAL PARK OF THE UNITED STATES

II.

LEAVING the Yellowstone Basin, and crossing in a westerly direction the range which divides the drainage of the Yellowstone and the Madesin, we come into the great Geyser Basin of the Firehole river, a branch of the Madesin Forth. Travelling in this region is attended with great difficulties on account of the immense quantity of fallen timber. The uplands, as well as the lowlands, are covered with a dense growth of pines, the majority of which have a trunk not over 6 in. to 12 in. in diameter, but run up to a height of 100 ft. to 150 ft., as straight as an arrow. In crossing this shed the source of the east fork

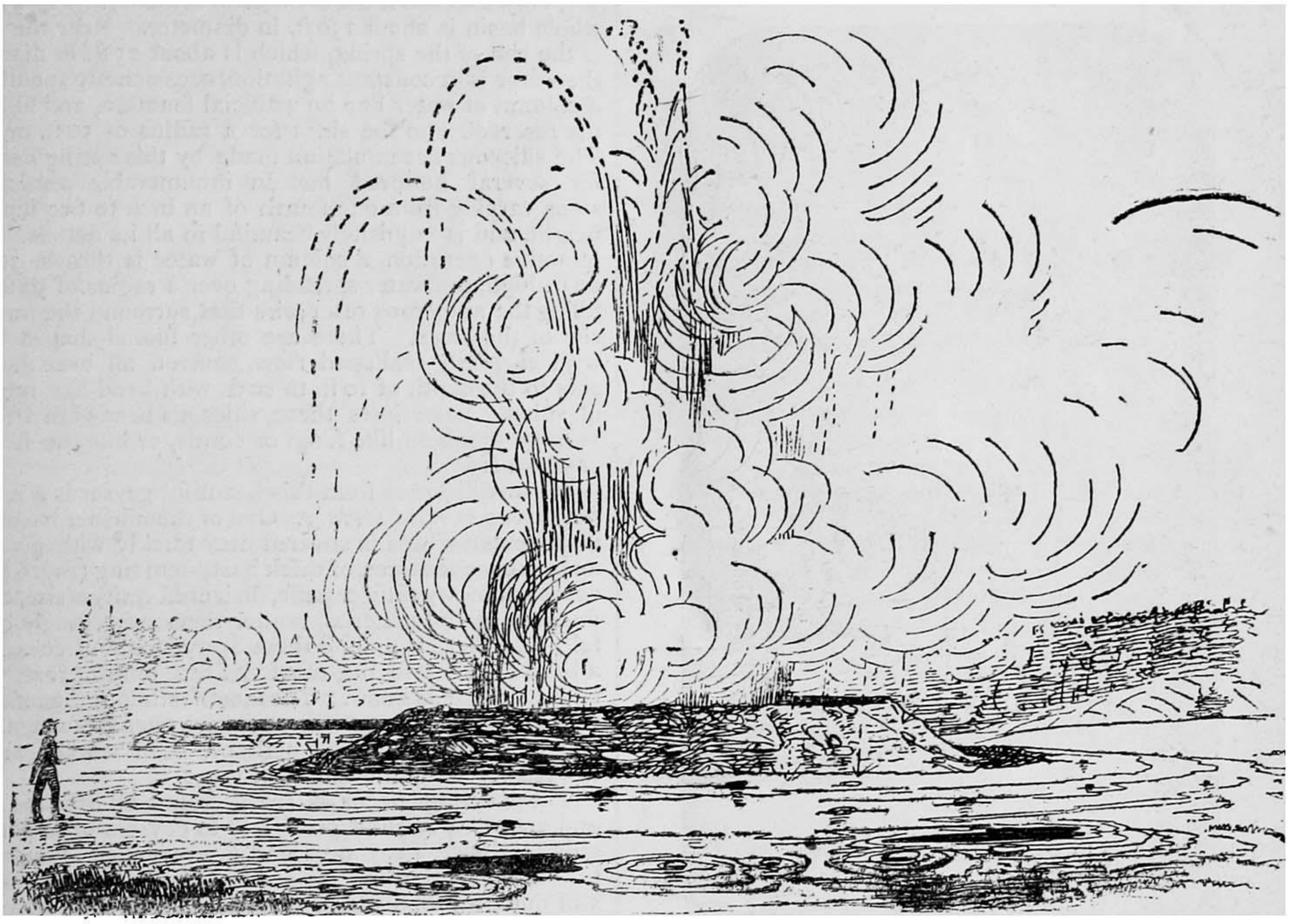


FIG. 5.—Architectural Fountain.

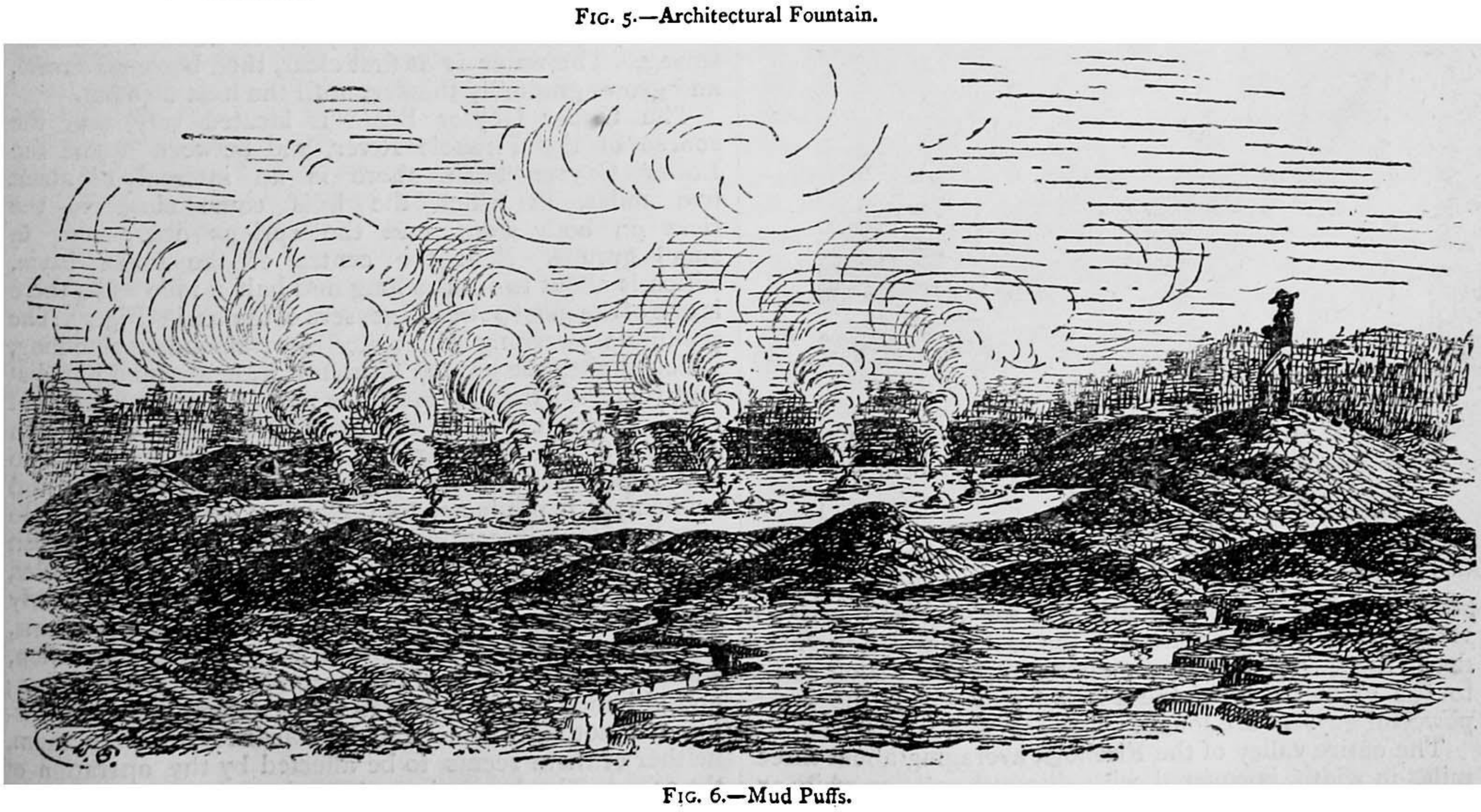


FIG. 6.—Mud Puffs.

of the Madesin is first struck, and every few miles a group of dead or dying springs is met with. In the distant view the appearance of the whole country may be not inaptly compared to a vast limekiln in full operation. The last branch of the Madesin is almost entirely fed by water from the hot-springs, and its temperature is 60° or 80° all the time. The vegetation that grows along the banks, and in the stream itself, is a marvel of luxuriance. The mountains that enclose the valley on either side are com-

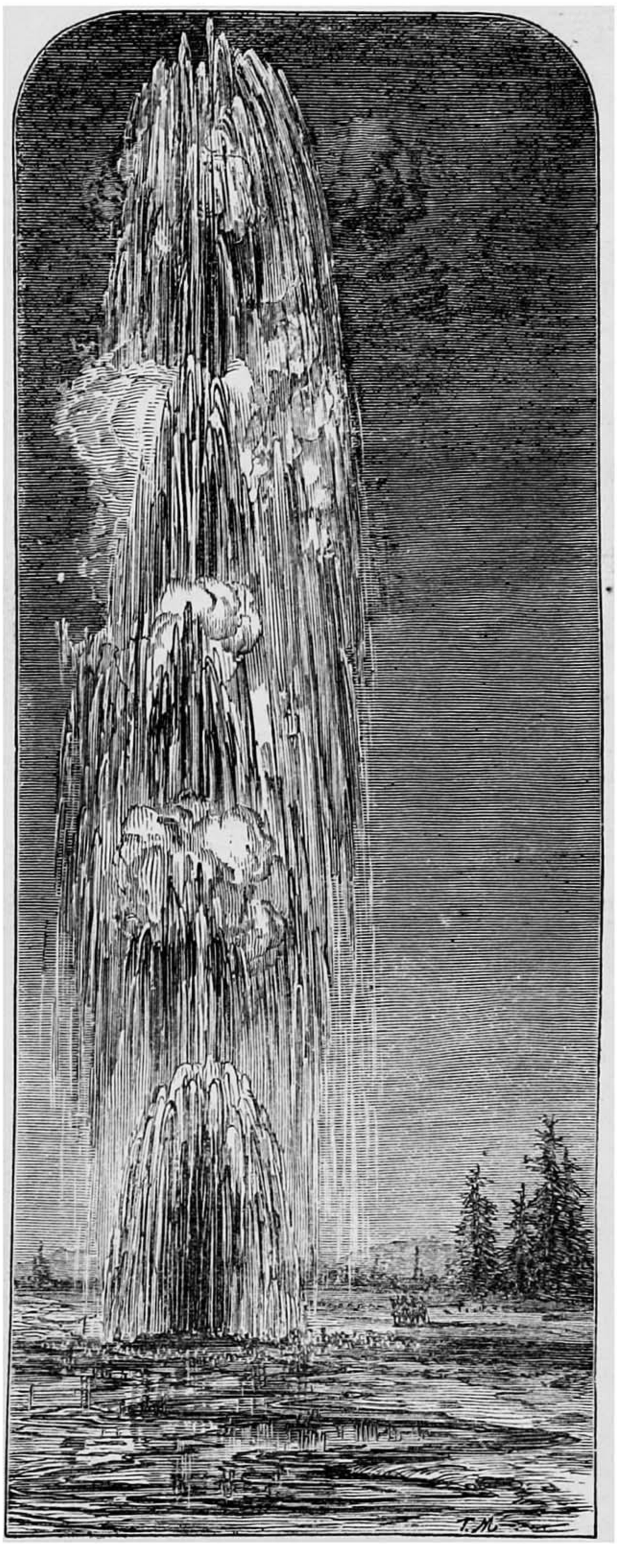


FIG. 7.—Giant Geyser.

posed of basalt and obsidian. As we proceed down the valley, toward the junction of the east fork with the Madesin, the springs grow more abundant, and we soon come to the great basin of the Firehole, in which the most powerful geysers are found.

The entire valley of the Firehole, averaging about three miles in width, is covered with siliceous crust as white as snow. The elevated mounds and numerous columns of steam reveal where the most important groups of springs and vents are located. In the Lower Geyser Basin, though there are many groups of most interesting springs, none of them can rank as geysers of the first class. Over an area of about three miles there must be at least a thousand active, quiet, dead and dying springs. One of the most remarkable of the springs in this lower basin has built up for itself a cistern, which is so elaborately beautiful that Mr. Hayden calls it the *architectural* fountain (Fig. 5). The whole basin is about 150 ft. in diameter. Near the centre is the rim of the spring, which is about 25 ft. in diameter; the water is in constant agitation, occasionally spouting up a column of water like an artificial fountain, and filling up the reservoir and the sides for a radius of 50 ft. or more. The siliceous accumulation made by this spring descends for several hundred feet in innumerable semicircular steps varying from one-fourth of an inch to two inches in height, and is exquisitely beautiful in all its details. When in active operation, a column of water is thrown 30 ft. to 60 ft. high, the water spreading over a radius of 50 ft., and filling the numerous reservoirs that surround the immense rim of the basin. There are other funnel-shaped basins with elegantly-scalloped rims, covered all over the inner side to the depth of 10 ft. to 20 ft. with bead-like tubercles of silica. Sometimes these siliceous beads are arranged in large numbers like fungi or corals, or like the heads of cauliflowers.

A short distance from this beautiful geyser is a remarkable group of *mud springs*. One of them has a basin 50 ft. in diameter, which is covered over thickly with puffs, like an immense cauldron of thick hasty-pudding (Fig. 6.) The exact symmetry of these puffs, their uniformity of size, and the fineness of the material, render them exceedingly beautiful; and there is among them every shade of colour from a bright scarlet to the most delicate pink or rose, with a base as white as snow. The most fastidious manufacturer of porcelain would go into ecstacies over this magnificent bed of mortar, that has, perhaps, been worked and re-worked for many thousands of years.

These springs occur in small groups all over the basin, and are often in close proximity to geysers or to perfectly quiet springs. They are found in every stage, from simply turbid water, through all grades of consistency, to thick stiff mud, through which the gases force themselves with a suppressed thud-like sound. Each of these mud springs probably commenced as a geyser, or at least a boiling spring. The water is at first clear, then becomes turbid, and grows gradually thicker, until the heat dies out.

The Upper Geyser Basin is located very near the source of the Firehole River, and between it and the Lower Geyser Basin there is an interval of about five miles, in which the hills come close to the river on both sides, and the springs occur only in small groups. Near the centre of the upper basin, which is about two miles long and half a mile wide, there is one of the most powerful geysers of the basin (Fig. 7). The preliminary warning is indicated by a tremendous rumbling, which shakes the ground all round with a sound like distant thunder; then an immense mass of steam bursts out of the crater as from an escape-pipe, followed by a column of water 8 ft. in diameter, and rising by steady impulses to the height of 200 ft. Mr. Hayden compares the noise and excitement it produced to that of a battle charge. He says the fountain continued to play for the space of fifteen minutes, when the water gradually subsided, and settled down in the crater about 2 ft., and the temperature slowly diminished to 150°. There are here two separate basins, one of which is in a constant state of violent agitation, while the other plays only at intervals of thirty-two hours; and although, so far as the eye can detect, there is a partition of not more than 2 ft. in thickness between them, neither of them seems to be affected by the operation of the other. The decorations about these springs are beyond conception beautiful; the most delicate embroidery could not rival them in their wonderful variety and complexity. The surface within and without is covered over with little tubercles of silica, which have a smooth, enamelled appearance like the most delicate pearls; down on the sides of the basin are large rounded masses like corals, formed entirely of silica. There are probably from twenty to fifty geysers of greater or less importance in this valley.

The two kinds of deposits in these regions, the calcareous and siliceous, have been mentioned in the preceding description. According to analysis by Dr. Peale, Chemist of the U. S. Geological Survey, the springs on Gardener's River, known as the White Mountain Springs (Fig. 8), deposit carbonate of lime mostly. There is present also sulphate of magnesia, chloride of calcium, sulphate of soda, and a little silica. In the deposits of the Firehole Basin

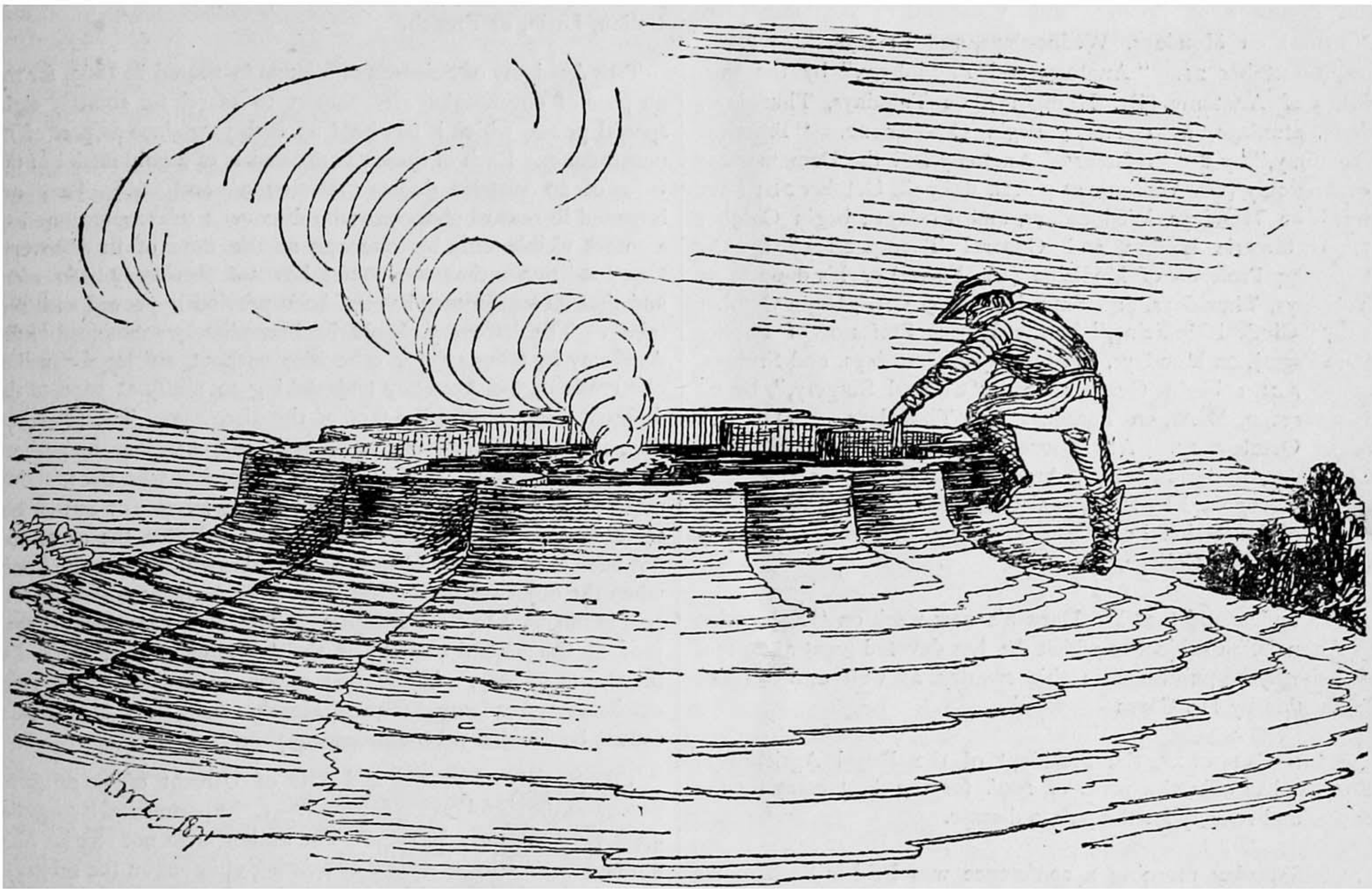


FIG. 8.—Wh te Mountain Springs.

not a trace of lime could be detected, but about 85 per cent. of silica, 11 per cent. of water, and the remainder mostly chloride of magnesium, and only a slight trace of lime has been found in the water. There are, scattered over the great area, a few patches of the sedimentary rocks, and it is most probable that underneath the deposit of this small group of springs there are portions of the carboniferous limestone.

So far as Mr. Hayden and party could ascertain, in all the deposits of the Yellowstone Basin proper, and the Firehole Basin, silica is the dominant constituent. The springs are, with very few exceptions, near the borders of the streams below beds of limestone. It is possible that underneath the vast masses of volcanic material which compose the mountains on every side, the sedimentary rocks exist; but Mr. Hayden is disposed to believe that they occur only in isolated and much restricted patches, if at all.

It may therefore be stated, in general terms, that the great hot-spring region of the Yellowstone and Missouri rivers is covered with rocks of volcanic origin, of comapratively modern date.

NOTES

AT their Statutory Quarterly Meeting on Monday, the Trustees of Anderson's University, Glasgow, elected Mr. George Forbes, B.A., F.R.S.E., of St. Catherine's College, Cambridge, to the vacant professorship of Natural Philosophy. There were originally eleven candidates for the appointment, but one of them died before the day of election arrived. Prof. Forbes is by inheritance, as well as by inclination and education, a man of science. The son of the late Principal, James D. Forbes, F.R.S, of St. Andrews, he received his university training at St. Andrews, Edinburgh, and Cambridge, and has since had the benefit of practical training in physical science at the Royal Observatory under the Astronomer Royal, and on the Continent. In making the appointment, the Andersonian trustees acted on the advice of Sir Wm. Thomson, Sir G. Biddell Airy, K.C.B.; Prof. P. G. Tait, Edinburgh; Prof. Balfour Stewart, Prof. A. S. Herschel, Profs. W. H. Miller and J. C. Adams, Cambridge; Prof. Grant, Glasgow Observatory, and other eminent physicists. It is understood that this professorship is to be established on a more extensive basis than formerly. This would offer to the professor a wider field of useful employment by developing the resources of the professorship, not only through the extension of the system of lecturing, but also by the establishment of a physical laboratory for students, which, it is understood, is in contemplation. Mr. Alexander Lindsay, of Glasgow, has also been elected Professor of Medical Jurisprudence. It was intimated to the meeting that Mr. J. Tennant, of Rollox, has given a donation of 1,900*l.* to the University.

THE following lectures on subjects connected with Physical and Medical Science will be delivered at the University of Cambridge in the Michaelmas Term :—"Heat and Electricity," by the Professor of Experimental Physics (Mr. Maxwell), on Mondays, Wednesdays, and Fridays, at 12 M.; begin October 23. "Chemistry," by the Professor of Chemistry (Mr. Liveing), on Mondays, Wednesdays, and Fridays, at 12 M.; begin October 21. "Practical Chemistry," by the Professor of Chemistry, on Mondays, Wednesdays, and Fridays, at 1 P.M.; begin November 4. "Zoology and Comparative Anatomy," by the Professor of Zoology and Comparative Anatomy (Mr. Newton), on Mondays, Wednesdays, and Fridays, at 1 P.M.; begin October 21. "Anatomy and Physiology," by the Professor of Anatomy (Dr. Humphry), on Tuesdays, Thursdays, and Saturdays, at 1 P.M.; begin October 22. "Practicar Anatomy," by the Professor of Anatomy and the Demonstrator of Anatomy (Dr. Wilson), at 9 A.M. daily till October 21; after wards on Mondays, Wednesdays, and Fridays; begin October 7. "Materia Medica and General Therapeutics," by the Downing Professor of Medicine (Dr. Fisher) or his deputy, on Tuesdays, Thursdays, and Saturdays, at 9 A.M.; begin October 22. "Clinical Medicine," by the Regius Professor of Physics (Dr. Paget), on Mondays, Wednesdays, Thursdays, and Fridays, at 10 A.M.; begin October 10. "Clinical Surgery," by C. Lestourgeon, M.A., on Tuesdays and Thursdays, at 11 A.M.; begin October 22. Attendance on the lecture on Botany, Chemistry, Materia Medica, Anatomy, Physiology, and Dissections is recognised by the Royal College of Surgeons, England, as one of the sessional courses required by the regulations of the Council of that College.

WE understand that Mr. Darwin's new work on "Expression in Animals," a subject to which he has devoted great attention, will shortly be published in this country, as well as a German translation by Dr. Carus.

LORD CATHCART, the President of the Royal Agricultural Society, has offered a prize of 100*l*. for the best essay on the causes and remedy for the potato disease.

ON Saturday morning a conference was held in St. Thomas Charterhouse Schools to inaugurate a system of Science Teaching for Elementary School Teachers. The Rev. Evan Daniel, Principal of Battersea Training College, was voted to the chair, and there was a very large attendance of teachers of both sexes. The chairman spoke strongly of the necessity of immediately starting an institution for the efficient teaching of science. On all sides, he said, there was an outcry for it. All provisions hitherto made by the Government for the teaching of science were wholly inadequate. The school is to be known as the St. Thomas Charterhouse School of Science, and its programme for the ensuing session, which commenced on Monday evening last, shows that, in addition to the evening classes, there will be day classes for general students. The subjects include chemistry, geology, mathematics, animal physiology, acoustics, vegetable anatomy and physiology, magnetism and electricity, botany systematic and economic, plane and solid geometry, music, &c. Several professional lectures will be given during the session, and biology students will have opportunities of making microscopical observations. Any communications or inquiries should be addressed to Mr. C. Smith, the organising teacher.

THE Royal Microscopical Society will hold its first meeting for the session on Wednesday, Oct. 2, at King's College, at 8 P.M.

CAPTAIN BURTON, who has been exploring the unknown interior of Iceland, has returned to England. His collections, &c., we believe, are at present left in the care of the Anthropological Institute, London, as he had to leave England at once in order to take up his appointment as Consul at Trieste, to which he was appointed on the death of the late Mr. Charles Lever.

THE Academy of Sciences of Bologna has resolved to give in 1874 a prize of 1,200 francs *(prix Aldini)* for the best scientific experimental essay on Galvanism or Dynamic Electricity. The competition remains open till June 30, 1874; the works may be either written or printed, but in the latter case they must not be published before 1874; and then they may be written either in Italian, Latin, or French.

THE Academy of Sciences of Vienna instituted in 1869, for the purpose of encouraging astronomers to search for comets, eight special prizes, which it has kept up each year since as part of its programme. Each of these prizes consists of a gold medal of the value of 20 Austrian ducats (between 9*l*. and 10*l*.) They are intended to reward observers who discover a telescopic comet, or a comet visible only by telescope at the time of its discovery. One condition is that the comet has not previously been seen, and that its appearance has not been previously proved with certainty. The discovery should be immediately announced to the Academy by telegraph or otherwise without waiting for further observations, the Academy undertaking to notify at once to the different observatories the fact of the discovery. The place and time of the discovery ought to be indicated, as well as the position of the comet and its orbit as exactly as possible with the first intimation; the data should be completed at leisure by further observations if it be possible to make them. When the comet has not been seen by other observers, the prize will be presented only when the observations of the discoverer have been sufficient to enable the orbit to be determined. The prizes are decided each year at the general meeting of the Academy held at the end of the month of May. If the first announcement of the discovery reaches the Academy between March 1 and May 31, the prize cannot be decided till the following year.

THE Society of Science and Arts of Utrecht offers prizes for papers on the six following subjects:—1. Experimental Researches upon the Inhibitory Nerves. The author must not restrict himself to a mere critical review of existing opinions on the subjects; he must elucidate them by new experiments. 2. Researches on the development of one or more species of invertebrate animals whose history is not yet known; the paper must contain all the illustrations necessary to the understanding of the text. 3. Researches upon the influence which slight variations of external circumstances may exercise upon the evolution of the embryo of one or more species of invertebrate animals. 4. Description of the larva and nymph of the common cockchafer, to complete the monograph on that insect in its perfect state by Strauss-Durckheim. This must be accompanied by the necessary figures. 5. Researches to determine the normal variation of the temperature of at least thirty-five places in the northern regions of Europe. The monthly means of old observations ought to be reduced so as to agree with the time at which the observations are actually made. 6. To investigate and point out how the waters of the rivers which traverse the Netherlands could be purified so as to become drinkable without detriment to the public health; at the same time to indicate the probable expense of their application on a large scale. Each of the prizes will consist of a medal of the value of about 27*l*. sterling, or its equivalent in money. The papers may be written in French, Dutch, German (Roman letters), English or Latin. They ought to be sent to the Secretary of the Society, M. N. F. Van Nooten, before December 1, 1873, the name and address of the author ought to be attached to each memoir in a sealed envelope.

DEMERARA papers record the death, in the early part of the summer, of Mr. C. F. Appun, an enterprising naturalist who had done much to explore both the physical features and the natural productions of the colony. He had previously travelled through Venezuela, Brazil, and the Amazons Valley, and had sent considerable collections of plants both to this country and to Germany. His journal has been printed in the *Georgetown Gazette*, and we hope to give extracts from it on a future occasion.

THE Council of the Vienna Exhibition have decided on having a permanent aquarium erected in that city, and the plans of Mr. H. Driver, C.E., who erected the aquarium at the Crystal Palace last year, have been approved.

WE understand that it is not the intention of the Board of Managers of the London Institution to fill up the vacancy occasioned by the decease of Mr. J. C. Brough, F.C.S. until after November.

THE *Newcastle Chronicle* states that some gentlemen connected with the mining interest have for several days been prosecuting their inquiries in the neighbourhood of Waterford with reference to the existence of coals in that portion of county Kilkenny which lies between the Suir and the Barrow, and has a communication with both of these important rivers. The geological maps give no indication of coals in this locality, but the result of inquiries prosecuted with much intelligence has led to the discovery of a coal bed of immense dimensions in this district, about two miles from Waterford, and within easy access to the river Suir. The coal seam to the thickness of 10 ft. lies immediately under the Old Red sandstone, the lower strata being a very fine outcrop of silicate of magnesia. The coal shales come to the surface at the cross road, about half a mile beyond the chapel of Sheverne. The arrangements are in a state of great forwardness for an immediate start, and a number of English miners are daily expected. If the hopes of these parties—and they appear to be well founded—are realised, it will afford a vast amount of employment, and will give the south of Ireland an almost unlimited supply of fuel. We need hardly point out that our Newcastle contemporary must have fallen into an error in speaking of the coal-seam being found *under* the Old Red sandstone.

WE are glad to see that Professor Piazzi Smyth, Astronomer Royal for Scotland, has at last got the reward of his twenty years' persevering and creditable importunity in the shape of a new equatorial telescope for the Royal Observatory on the Calton Hill, Edinburgh, for which Government last year granted 2,300*l*. Hitherto, as the *Scotsman* justly remarks, the Metropolitan Observatory of Scotland has been in the position of the meanest appointed Government astronomical institution in Scotland. To accommodate the new telescope, it is necessary to raise the dome of the Observatory, and Mr. James Fergusson, author of the "History of Architecture," who was consulted on the matter, has decided that the new dome could not be raised more than fourteen inches—that being the largest increase that could be æsthetically allowed in conjunction with the rest of the Observatory, which, viewed as a piece of architecture, is considered to be the very gem of the works of the late William Playfair. In consequence of this the Astronomer Royal has had a difficult task in endeavouring to arrange a form of equatorial instrument which would give a greater amount of power within a smaller line or compass than was ever attained before. The new telescope will have an aperture of two feet upon a focal length of only ten feet—a larger diameter in proportion to focal length than any astronomical telescope yet introduced into any observatory; and it will no doubt be by far the most powerful instrument ever erected under so small a dome. Although the telescope will be much more powerful than any ever before placed in the Observatory, it is still not such as was desired or considered almost necessary in the present state of science. The instrument, which is being built by Mr. Howard Grubb of Dublin, is to be constructed on M. Foucault's comparatively new principle of having the speculum of glass coated with pure silver. The instrument is to be employed both for photography and spectroscopy. Both these classes of research require the seemingly impossible accompaniments—that the telescope must have the utmost amount of firmness, and also have the most accurate possible movement at the same rate at which the stars change their position in the sky. Notable features in the new telescope will therefore be the remarkably perfect clockwork apparatus, the several devices connected with the prisms of the spectroscope, the means by which not merely celestial objects will be kept in view, but those by which the rays of chemical flames will be brought into comparison with the light of the stars. The extremely delicate measuring apparatus to be applied to the respective subjects as they appear on the spectrum will also be noteworthy. December next is the time fixed for the completion of the new instrument, but meanwhile active preparations are being made in the Royal Observatory for its reception. The new dome, it is expected, will be erected in the course of this month, while the weather is yet fine. This dome, which is also being built by Mr. Howard Grubb, will be of iron instead of wood, and that for two reasons: first, because it will afford a greater amount of space in the interior of the instrument room; and second, because it will enable such an arrangement being made for the shutter as will allow of an opening several feet in breadth, whereas the opening in the old dome was only a few inches wide. Although the arrangements of the Observatory are, during operations, necessarily somewhat upset, observations continue to be made by Prof. Piazzi Smyth and his assistants.

IN accordance with the decision of the Scientific Association of France, systematic and simultaneous observations on shooting stars have been made during August in various places in France, Italy, and at Alexandria. The chronometers of the various stations were compared by telegraph, the signals being given from Bordeaux, Lyons, Marseilles, and Paris. The results of the simultaneous observations at the various stations are recorded in the *Bulletin Hebdomadaire*, from which we learn that on the nights of August 9, 10, and 11, observations were made at twenty-two stations; Alexandria and Moncalieri being those from which the greatest number of shooting stars were seen, 2,042 having been noticed during the three nights at the former, and 2,049 at the latter place. It will be remembered that in November last the whole of the shooting stars did not come from Leo, as it was expected they would, and that the observers noticed radiant points in Taurus, Gemini, &c. Somewhat similar eccentricities, though upon a less scale, appear to have occurred during the August shower. At Genoa, about the half of the stars came from various directions; M. Stephan, at Marseilles, intimates that on the third night the radiant point was in Cygnus; and at Paris, M. Tremeschini found that on the third night the majority of the stars did not come from Perseus. MM. Le Verrier and Wolf, who have been charged with the arrangement of the various observations, have presented to the Academy the results of the work done last November, and expect to be able to do the same for that of August, soon after they receive detailed reports from the various observers. For the discussion of the common observations, the astronomers at their meeting at Montpelier decided to employ the method proposed by Colonel Goulier, which however can be applied only when the stars are at least 10° above the horizon; when otherwise, the methods followed by MM. Lespiault and Stephan will be used. M. Goulier has been charged with the construction of the charts of his system; M. Lespiault with the preparation of the method to be followed when the trajectories are at a small elevation above the horizon; and M. Wolf with the final arrangement of the complete results.

BIELA'S COMET

MR. J. R. HIND has addressed the following letter to the *Bulletin* of the *Association Scientifique de France*:—

You will probably think me rather sanguine in supposing for a moment that there is a chance of re-discovering either nucleus of Biela's comet this year, when in the ordinary course of things a perihelion passage would be due. I look at the matter in this light. We know that in February 1846 a very remarkable alternation of brilliancy took place: that the second nucleus, barely discernible at first, so greatly increased its light as to surpass what I will term the parent-nucleus, and continued thus several days, when it gradually faded. Again in September 1852, M. Otto Struve's drawings show the same remarkable interchange of light between the 20th and 25th. Whatever the cause may be, each nucleus appears to have a re-vivifying power, so to term it, and I think it is just possible this may be exercised at one time or other to such an extent as to bring the comet again within our grasp, though its condition in 1865-1866 may have been such as to render it quite invisible from the earth. In this idea of the subject, I have prepared sweeping ephemerides for September and October, part of which (that applying to next absence of moonlight and longer) I now do myself the honour to send you. The mean motion in Dr. Michez's orbit from 1866 would bring the comet into perihelion 1872, October 6·4, and I have calculated places on this supposition, also with variations of ± 8[d] in perihelion passage. Clausen carried forward the perturbations of both nuclei in 1866, and his elements for that year would indicate (of course, neglecting perturbation 1866-1872) the following times of perihelion passage.

Nucleus I . .	1872 Oct. 4·776	Greenwich
Nucleus II . .	,, Oct. 7·256	,,

and hence these differences of R. A. and N. P. D. between the two nuclei, which, if one is only found, might be useful in bringing to light the other.

	R. A. (I—II).	N. P. D. (I—II).
	° ′	° ′
Sept. 12 . . .	+ 1·59 . . .	+ 0·55
,, 20 . . .	+ 1·50 . . .	+ 0·57
,, 28 . . .	+ 1·42 . . .	+ 0·55
Oct. 6 . . .	+ 1·34 . . .	+ 0·50
,, 14 . . .	+ 1·26 . . .	+ 0·44
,, 22 . . .	+ 1·18 . . .	+ 0·37

Perhaps there may yet be time to interest some of your many correspondents in a search for Biela during the first ten days of October. Here the weather has been exceedingly unfavourable, and, though I have watched assiduously, there has not been a single opportunity of examining the eastern heavens in the morning during the last period of absence of moonlight.

I remain, etc.

J. R. HIND

Observatory, Twickenham, near London, Sept. 17

SWEEPING EPHEMERIDES FOR BIELA'S COMET

1872 9h 36m Greenwich.	Perihelion. September 28,4 R.A.	δ	Perihelion. October 6,4. R.A.	δ	Δ	Perihelion. October 14,4. R.A.	δ
	h m	° ′	h m	° ′		h m	° ′
Sept. 28	10 4·0	+6 27	9 43·0	+9 15	1·345	9 20·3	+12 22
30	10 13·0	5 21	9 52·4	8 6		9 30·2	11 9
Oct. 2	10 21·9	4 15	10 1·7	6 56	1·365	9 40·0	9 56
4	10 30·7	3 9	10 10·9	5 46		9 49·7	8 42
6	10 39·3	2 4	10 19·9	4 37	1·388	9 59·2	7 28
8	10 47·8	+1 0	10 28·8	3 28		10 8·6	6 15
10	10 56·2	−0 3	10 37·6	2 20	1·414	10 17·8	5 2
12	11 4·5	1 4	10 46·2	1 13		10 26·9	3 50
14	11 12·6	2 5	10 54·8	+0 7	1·442	10 35·8	2 38
16	11 20·6	3 4	11 3·2	−0 58		10 44·6	1 27
18	11 28·5	4 2	11 11·4	2 2	1·472	10 53·3	+ 0 18
20	11 36·3	4 59	11 19·6	3 4		11 1·8	− 0 50
22	11 44·0	5 55	11 27·6	4 5	1·503	11 10·2	1 57
24	11 51·5	6 49	11 35·5	5 5		11 18·5	3 3
26	11 59·0	−7 42	11 43·3	−6 3	1·535	11 26·6	− 4 7

Mr. Hind, in a letter with which he has favoured us, states that M. Stephan, the Director of the Observatory of Marseilles, will employ the large Foucault mirror of that establishment in a search for the comet.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

THE session of this Association opened at Dubuque, Iowa, on August 21. At the end of last year's meeting it was expected that the Association would meet this year at San Francisco, which place it was hoped Prof. Agassiz would have reached in the *Hassler* by the time of the opening, and added an unusual brilliancy to the proceedings by an account of some of his discoveries in the Southern seas. The great naturalist, however, as we know, has not yet reached the Californian coast; and partly on this account, but mainly, we believe, on account of the great expense to most of the members of a journey to San Francisco, that city has not this year been honoured with the presence of the associated *savans* of the United States. Indeed, if we may judge by the somewhat diminished attendance, and the unusually small number of papers read, even Dubuque seems to be too far away from the homes of many of the members. Some of the prominent members of the American Association were absent this year, the many, however, who did attend, met with a very liberal reception from the citizens of Dubuque.

The retiring president of the last year, Prof. Asa Gray, of Cambridge, Mass., made, as is the custom, a valedictory address. He took the opportunity to sketch his recent experience at the West as a botanist, for, strange to say, until the present summer he had not seen the Mississippi nor set foot upon a prairie. Prof. Gray touched lightly upon the history of the Sequoias; that their age must be counted by hundreds of years we cannot doubt; but also we cannot doubt that they did not antedate the glaciers whose icy expanses have left their indisputable evidences everywhere around. The main portion of Prof. Gray's address was devoted to showing the probability that certain trees of the present day, the *Sequoias* of California, the *Taxodium*, or Bald Cypress of the Atlantic, and the *Glyptostrobus*, a Chinese tree, the only ones of their tribe at present existing, were not only closely allied in structure and general characteristics, but were the lineal successors by gradual modification of the fossil trees of geological ages; and that all our existing vegetation was a continuation of that of the Tertiary period. Three hypotheses are open to account for their present existence: either they were created in the detached regions where they are now found; or they are the first members of a new and increasing race; or they are nearly the last representatives of a once powerful and widely-spread tribe. After discussing the first two alternatives, Prof. Gray gives his reasons, which are supported by palæontological evidence, for believing that the "big trees" of California are the last of their race, and that a little further drying up of the climate, which is now in progress, will speedily precipitate their doom. The oldest of the trees now standing he considers to possess an antiquity of about 2,000 years.

A paper was read by Prof. E. S. Morse "On the Oviducts of the Brachiopods." He brought forward for the first time a few facts regarding the development of a species of Brachiopod from the coast of Maine. The various stages in their development, he believed, fully supported the position he maintained, that the Brachiopods were true worms and not molluscs.

Prof. Benjamin Peirce, Superintendent of the United States Coast Survey, gave an exceedingly interesting account of the measures taken by that Bureau with reference to stations for astronomical observations at great heights, such as Sherman, on the Rocky Mountains. Prof. Young, of Dartmouth College, has been appointed to examine and give an opinion as to the effect of taking observations from the Rocky Mountains and Sierra Nevadas. Prof. Peirce's opinion was that observatories should be placed on the summits of both, if not permanently at least temporarily, the Rocky Mountains being favourable to some, the Sierra Nevadas more favourable for other observations. In a higher position you get rid of absorption of light by getting rid of half the atmosphere. Prof. Young reports the whole number of lines in the chromosphere seen from Sherman as 150, which is three times as great a number as have been observed before. This fact alone shows that higher points should be resorted to for astronomical observations. The next element of success depends upon the steadiness of the atmosphere. It can be said in reference to this, that a star has been recognised at these high altitudes as having a companion, or being a double star, not previously known as such. An observer on the Pacific coast reports to Prof. Peirce that he can see the companion of the star Polaris from a high point on the Sierra Nevada. It is well known that this is a test of great nicety, requiring the utmost purity of atmosphere. The best work in astronomy is done in the few best nights at any place, and by these alone the value of the position must be determined.

Prof. Peirce showed that the necessities of the Coast Survey extended its operations to all parts of the United States. To prove the paths by which vessels can best traverse the ocean, to test the best methods by which 2,000,000,000 dollars of values shall be carried from the West to the East, from the East across the Atlantic, or from the shores of China and Japan to the Pacific coast, and thence across the country, was the business of the Coast Survey. All the United States is deeply interested in every part of the subject. Every ship that is lost by imperfectly surveyed harbours is a loss to the whole country. If the coast survey were thorough, and maps were fully representative of ascertained facts, a pilot would scarcely be necessary, but yet never could be entirely dispensed with, especially in bad weather, The pilots discovered that by putting down every rock that they knew of, they made maps that frightened the captains of vessels into employing them. Hence these practical observers have added immensely to the number of facts accumulated by the Coast Survey.

Prof. Peirce explained why he considered it unnecessary to carry out at present so thorough a survey of the Pacific as has been made of the Atlantic coast. The needs of the commerce of the coast is the standard by which the work of the survey is determined. He took occasion to mention that the *Hassler* expedition was at the expense of private individuals, principally of Boston, and was not at the expense of the Government. A general geodetic connection has been effected in these observations, so that the whole United States will benefit by them; and the points will be taken so as eventually to procure a complete survey of the whole continent, passing through each State and the large cities. It is a work that may take a century. It is the hope of Prof. Peirce that this survey will not only be the best in the world, but that its details will be such that before long there will be no necessity for railroad surveys—the facts will be spread everywhere. As to the higher operations of the Coast Survey, their ultimate expression will be an accurate determination of the figure of the earth. Observations in America he thinks are more successful and free from local irregularities than in Asia or Europe. Yet there are some such local irregularities here—notably one near Boston, where there is some strange deviation of density from the surrounding country.

Prof. J. W. Foster, of Chicago, read a paper on what he considers a new species of fossil elephant, called by him *Elephas Mississippiensis*. He presented to the association a fossil tooth found in Indiana, and which he regards as differing specifically from that of any other fossil elephant found in America or on the Continent. The differences are so great that he holds them sufficient to constitute the new species.

Prof. J. E. Parry, who had been in a good position for studying the subject, made a few remarks upon the glacial deposits of Northern Ohio. Prof. Foster took exception to several of his positions.

A very interesting paper was read "On the Mounds of Dubuque and vicinity."

A somewhat warm discussion took place on the 23rd between Dr. F. H. Day of Wisconsin and Prof. Morse of Salem, occasioned by the former sending a paper to prove that the trilobite not only resembled the lobster in some respects, but was actually closely related to that crustacean, and must have been in structure and movements almost similar. Prof. Morse stoutly maintained that the congener of the trilobite at the present day was the *Limulus*, not the lobster.

On the same day Prof. F. W. Putnam of Massachusetts, editor of the *Naturalist*, read a brief and well illustrated paper on certain "Stone Carvings of the New England Aborigines."

Prof. E. B. Andrews of Ohio, the State Geologist, presented a paper on Coal, of both scientific and practical interest. The universal law of the accumulation of coal strata seems to be on horizontal lines, determined by water levels. The proof of this is in the marine organic remains found in the coal itself and its associated slates. In addition, there is a distribution through seams of coal of horizontal layers of fine sedimentary matter which constitute the clay partings of the seams. The subsidence which carries down the area of the coal-marsh, if it may be so called, is wide-spread in its character and equable in its movement, having a tendency to a continental rather than a local character. The result of this gives a horizontal parallelism to all the seams of coal at the time of their formation; and this is equally true where one coal-marsh is formed over another, making a series of coal measures. So far as Prof. Andrews's observation goes, a coal seam has never been accumulated on high grounds or ground above water-level; and such formations could not take place, because it would be impracticable to establish the conditions of accumulation on the side of a hill. Hence it is impossible in the nature of things that two distinct seams of coal could ever coalesce, since their subsidences must take place in parallel lines. To suppose otherwise would involve a very unequal subsidence over very limited area, amounting, indeed, to a convulsion of nature, which is almost incredible.

It has hitherto been to some extent conceded that the solidification of coal is an exceedingly slow process; and the popular notion is that there has been a complete accumulation of a series of layers of coal, &c., long before thorough hardening takes place. Prof. Andrews has, however, found instances indicating that the process of solidification, making complete coal, is comparatively rapid. Thus it appears that where a gulley has been torn out of a coal seam by a rapid current of water, the small boulders washed by it over the covering stratum of sand a few feet above, are complete coal, having an angular fracture, some being still sharp on the edges and some being slightly water-worn. These boulders in turn have again been covered by subsequent depositions, and are found at considerable depths, near the base of the coal measures.

There are three leading varieties of bituminous coal; the ordinary resinous or caking coal, the splint, and the cannel coal. These pass into each other by almost imperceptible gradations. The resinous coal seems to be the normal condition which the buried vegetation first assumes, and splint and cannel are modified forms, the cannel coal having lost all trace of structure, and containing no organised forms, except stigmaria, which is very abundant. The ash of coals is the original inorganic matter of vegetation, often increased by sedimentary matter in the marsh during the formation of coal.

Prof. C. A. White, of Iowa City, gave a general sketch of the geology of the State of Iowa, in which he stated that the extent of its coal measures was greater than the entire area of Massachusetts. The occurrence of quartzite in the north-eastern portion of Iowa he regards as of the Potsdam formation. There is a remarkable area of drift in the north-western part of the State, covering an area of at least 20,000 square miles so deeply that no rock crops through. Prof. Swallow thinks that the waters receding from this State went to the Pacific.

In the discussion which took place next day, on Prof. Andrews' paper, several geologists controverted his position that coal seams seldom or never diverged from each other within small areas, adducing instances to prove their statements. Prof. Andrews did not doubt that there were occasional instances of the sort, but in general he did not believe that there were frequent cases where seams separated widely. Seams of coal, as a rule, with rare exceptions, were of parallel levels from uniform subsidences.

On the evening of the 23rd, Prof. Morse delivered to a large and delighted audience a popular lecture on "The Locomotion of Animals," beginning with the lowest forms of animal life, and working his way upwards through all grades to man, illustrating his statements by a series of clever and rapidly-executed illustrations on the black-board.

On the 24th, the Association passed a resolution with regard to what is known in America as "the Chinese Indemnity Fund," the name given to a large sum which China overpaid in settling for damages to United States citizens, and which, when informally tendered to the Chinese Government, was declined, with the intimation that pressure on the subject would be hurtful to Asiatic pride. This fund now amounts to 450,000 dols, and a bill is now pending in Congress which proposes to appropriate this surplus for the education of Americans and Chinese "in the languages, literatures, and sciences of the respective nations; to facilitate commercial, diplomatic and scientific intercourse between the two peoples; and for the increase and diffusion of knowledge among men." It is apparently intended to accomplish this purpose by establishing an American college in China, and the resolution of the Association "heartily endorses the purpose of the aforesaid Bill."

In expatiating upon the propriety of this resolution, President Smith alluded to the insecurity of the present position of the fund in the hands of the United States Government, since it offered a premium for trumped-up claims, and already 37,000 dols. had been thus abstracted from it; that if it remained much longer unappropriated, Japanese as well as Chinese claims would be introduced against it, and it might eventually suffer the fate of the Smithsonian funds, which the Government had to make good after the loss of a considerable portion.

During the course of the same day a scene of intense excitement and disorder took place on the reading of a paper by Prof. B. C. Swallow, the Missouri geologist, entitled "Good Wine a Social and National Blessing,' which was devoted principally to the details of wine making. It was, however, made an opportunity for the discussion of the causes of drunkenness, and the best means for preventing it, which had far more of a social than of a scientific interest.

A very interesting and valuable paper was that of Mr. C. V. Riley of St. Louis, entitled "Insects Shaped by the Needs of Flowers," with especial reference to the fructification of the American Yuccas. Dr. Engleman of St. Louis had this year discovered that these plants must needs rely on some artificial agency for fertilisation. The flowers are peculiarly constructed, so that it is impossible for the pollen to reach the stigma, it being glutinous and expelled from the anthers before the blossoms open. Prof. Riley discovered that there was a small white moth that did the work, and demonstrated on the blackboard how wonderfully well the insect was adapted to the purpose. This little moth, which he calls *Pronuba Yuccasella*, has been unknown to entomologists, and forms the type of a new genus. It is most anomalous from the fact that the female only has the basal joint of the maxillary palpus wonderfully modified into a long prehensile-spined tentacle. With this tentacle she collects the pollen and thrusts it into the stigmatic tube, and after having thus fertilised the flower she consigns a few eggs to the young fruit, the seeds of which her larvæ feed upon. He stated that the Yucca was the only entomophilous plant known which absolutely depended for fertilisation on a single species of insect, and that insect so remarkably modified for the purpose. There was a beautiful adaptation of means to an end, and a mutual in-t· ·pendence between the plant and animal; and Mr. Riley ¢ ·ed how on Darwinian grounds, even this perfect adapta- as doubtless brought about by slow degrees. He alluded, in ·ing, to a practical phase of the subject. The plant and its fructifier are inseparable, under natural conditions, and the latter occurs throughout the native home of the former. In the more northern portions of the United States, and in Europe, where our Yuccas have been introduced and are cultivated for their showy blossoms, the insect does not exist, and consequently the Yuccas never produce seed there. The larva of *Pronuba* eats through the Yucca capsule in which it fed, enters the ground and hibernates there in an oval silken cocoon. In this stage the insect may easily be sent by mail from one part of the world to another, and our transatlantic florists may by introducing it soon have the satisfaction of seeing their American Yuccas produce seed without any personal effort on their part.

Among other papers read at subsequent meetings of the Association was one by Dr. J. W. Foster on the "Bird-shaped Skulls found in Indian Mounds," in which he tried to show that the peculiar shape of these skulls was not produced by compression, as in the case with the heads of many modern Indian tribes, but really belonged to a very early and comparatively low type of man intellectually.

Another paper was by Prof. Daniel Kirkwood on "Binary Stars with Extraordinary Orbits," with special reference to Mr. Wilson's communication to the Royal Astronomical Society, as to the orbits of the stars constituting Castor.

Mr. W. W. Wheildon of Concord, Mass., advanced in opposition to what is known as the Gulf Stream theory an atmospheric theory to account for ameliorations of climate and an open sea in Polar regions. He thought that there could be no question that if the whole Arctic region were of open water that fact would not account for all the atmospheric phenomena and warmth of temperature experienced by polar navigators. The theory of Prof. Wheildon is that open water, melting ice, rain after snow, and other phenomena of the sort in Arctic regions, are not caused by winds warmed by an open sea, but by a circulation of air in which warm winds descend from upper atmospheres; being a circulation by which winds heated at the equator reach the poles. The brief discussion which followed this paper did not indicate much difference of opinion on the subject.

Of course, there were the usual numbers of excursions to places of interest, including one to the curious "Picture" or "Calico" Rocks near the town of Macgregor, which are composed of Potsdam sandstone. Far up in a narrow glen, outcropping sandstone rocks, partially denuded, exhibit in narrow stripes and patches, but principally in linear horizontal directions, the greatest variety of colours. Red is predominant, but black, blue, yellow, and intermediate shades are not wanting, each being distinct and not unfrequently contrasting sharply with the adjoining stripe, or with a gray that is almost white. Probably the original sand was white; the colours indicate varying admixtures of iron oxides and carbonates. Another was to the Mississippi, apparently for the purpose of seeing and collecting specimens of the large lily or lotus of that river (the *Nelumbium luteum*), closely allied to the Egyptian lotus. The last excursion was to Sioux City, and was to last for three days.

Altogether this year's meeting seems to have given general satisfaction, and the hospitality of the Iowans was unbounded. Next year's meeting is to take place at Portland, Maine, on the third Wednesday of August, Prof. Joseph Livesing, of Harvard University, being the president-elect.

THE FRENCH ASSOCIATION MEETING AT BORDEAUX

THE first session of the French Association for the Promotion of Science, closely modelled after that at Britain, was held at Bordeaux from the 5th to the 12th of September, and seems in all respects to have been successful and satisfactory. As is usual at similar meetings everywhere, the citizens of Bordeaux lavished their hospitality upon the members, who well deserved this as well as the gratitude of the French generally for inaugurating a movement to spread among that nation a knowledge of and love for science, and thus inform and temper their often misleading enthusiasm; in the words of M. Quatrefages, "to renovate our country by the scientific spirit and scientific studies." The meetings were well attended both by French and foreign *savans*, though the only two English ones whose names we notice were Prof. Odling and Dr. Gladstone. The Society already numbers 800 members, and, as will be seen by M. G. Masson's paper, its finances are in a flourishing condition. The first general meeting was presided over by M. de Quatrefages (the president-elect for next year), in room of M. Claude Bernard, the state of whose health prevented him from attending.

The opening address of M. de Quatrefages, as acting president, was a very stirring and noble one, full of sound sense as to the recent humiliation and present condition of France, enthusiasm towards science, and faith in it as one of the most powerful regenerators of the country. "Science is at present supreme," he said; "she is becoming more and more the sovereign of the world;" and he believes that it is only when all ranks and classes of the people, rulers and ruled, are thoroughly imbued with the scientific spirit, and are guided by scientific knowledge, that France will ever again take and maintain the supreme place in the world which she ought to hold. M. de Quatrefages concluded with a graceful allusion to "our elder sister, the British Association."

After an enthusiastic speech from the Mayor of Bordeaux, M. Cornu gave a brief sketch of the history of the Association. The first idea of the Association, he tells us, originated among a group of French Alsatian students gathered around M. Combes, the late lamented director of the School of Mines of Paris, and it has been nourished by a patriotic desire to contribute to the moral condition of the dejected country. After the death of M.

Combes, M. d'Eichtal came to the assistance of the embryo association, and on January 17 of this year a provisional council was appointed, with M. C. Bernard as president, and on April 22 the French Association was constituted, and already numbers upwards of 700 members. The British Association when it started in 1831 had only 370 members, whereas now it numbers many thousands, and can spend 2,000*l.* yearly for the progress of science. M. Cornu hopes that the French Association may become similarly prosperous, as indeed it seems to bid fair to do, for already, as M. Georges Massen, the treasurer, intimated, it has a capital of 140,000 francs, and can dispose of an annual revenue of 16,000 francs.

M. Laussedat gave an interesting lecture on "The Services which Modern Science can render to the Art of War," in which the starting sentence is that "Germany triumphed over France by numbers, discipline, and science." This was followed by a paper from M. Le Fort, on the Reform of Military Surgery. In the meetings of sections which followed, medical subjects received permanent attention, and called forth many papers of great value.

In the Anthropological Section, M. Lagneau read a careful paper on "The Ethnology of the Populations of the south-west of France, particularly of the basin of the Garonne and its affluents," M. Paul Lopenard one on "Prognathism," in which he gave some new measurements, and described a new method of measuring skulls; M. Parrot gave some details of the Cave of Excideuil (Dordogne), the variety of bones and other remains in which are of the greatest interest to anthropologists. Most of the other papers in this section were on subjects similar to the last, the prehistoric remains of various kinds found in caves, dolmens, &c., a department of Archæology to which much more has been done in France than in this country. M. Cartailhac went the length of saying that two populations belonging to prehistoric times could be distinguished as inhabiting the south of France; one warlike and given to the chase, who knew the beds of flint, and could cut it to perfection, and which was armed with the bow; the other pastoral, seldom feeding on wild animals, ignorant of the arrow and the flint weapon, and which used quartz, ophites, and other stones, but seldom flint for making axes.

But little seems to have been done in the Botanical section, one of the most valuable papers having been that of M. Van Tieghem, expounding the result of his researches into "Germination," which had for their object to determine experimentally the connection existing between the various organs of the embryo, and the degree of dependence of the latter upon the albumen. Another elaborate paper was by M. Baillon, "On the Floral Organisation of the *Amentaceæ*, and especially the Hazel."

Some of the most valuable and interesting papers of the meeting were read in the Section of Physics; the first, and one of the most attractive, was by M. A. Lallemand, President of the Section, "On the Polarisation and Colouration of the Atmosphere." The researches of M. Lallemand have been conducted on the basis of a theory which explains at once the origin of aërial polarisation, the formation of the neutral points indicated by Arago and Babinet, and the blue colour of the atmosphere. According to him, atmospheric illumination is only a particular instance of the phenomena of illumination of transparent bodies by a pencil of non-polarised solar rays. The generation of neutral points is explained simply by the intervention of the dust and atmospheric corpuscles which abound in the lower layers of the atmosphere, in the centre of which the observer finds himself placed. This paper was followed by another by M. Soret, of Geneva, "On the Influence of the Atmosphere upon the Intensity of Solar Radiation." The new experiments of M. Soret have been made by means of two calorimeters made upon the same principle and of analogous construction. The first, which is described under the name of "Actinomètre Transportable," is of small size, and consists simply of a metallic tube open at one end surrounded by a second concentric envelope. The interval between the two is filled with ice. At the bottom of the tube is fixed a thermometer, whose stem crosses the two envelopes, and upon which the sun's rays are allowed to fall. The thermometer, zero at the outset, rises in proportion as the quantity of heat which it receives equals that which it loses by radiation. The temperature which it finally reaches enables us to deduce the intensity of the solar radiation, after a certain number of corrections, the principal of which relate to barometric pressure. The Fixed Actinometer differs from the preceding only in its greater size, which allows of four thermometers being used instead of one: the inclosed interval is maintained at a constant temperature by a current of water. The experiments made by means of these instruments have enabled M. Soret to ascertain, in the first place, that the more moisture there is in the air, the less is the intensity of solar radiation for the same height of the sun and the same atmospheric pressure.

In the same section, M. Cornu presented a detailed sketch of the dark rays in the ultra-violet part of the solar spectrum, following the scale of the wave-lengths adopted by M. Angström in his memoir on the normal spectrum of the sun. These sketches have been made from the micrometric measurement to the microscope of photographic plates, forming two series. The first series has been obtained with the assistance of Nobert's network (of 1801 lines), of which the second spectrum was very perfect and very luminous; the proof obtained contained the rays G, H_1, H_2, L, M, N, O, P, and even Q, although the object glasses of Gonionde were of crown and flint glass. M. Cornu has verified the correctness of the measures of M. Mascart; the comparison of the results has always shown four common figures in the numerical expression of the wave-lengths. The second series, intended to furnish details of the rays of the photograph, proceed from a spectrum much dispersed and of great delicacy; more than twenty-five were counted between the two rays H_1 H_2. This spectrum was obtained with a flint prism of 60°; the moist collodion had taken an impression even up to the ray O, in spite of the absorbing power of the two object-glasses. M. Cornu strongly recommends the use of these ordinary achromatic object-glasses for obtaining photographs of the ultra violet region, when it is desired to go no farther than ray O; the inconvenience of the absorbing power is largely compensated for by the ease of setting in position and by the angular size of the region where the lines are clearly distinguished. With regard to the process itself, it differs but little from that of M. Mascart. M. Cornu, however, advises the adoption of a small dark chamber exterior to the telescope, in place of the photographic camera of M. Mascart. The advantage of employing collodionised surfaces sufficiently large to make room for the spots and other defects, arises from the thinness of the plate. M. Cornu further indicates upon his sketches the principal lines of magnesium, lime, manganese, and iron, which, as is known, compose the greater part of the groups, L, M, and N.

M. Saint-Loup followed with a paper on a proposed modification of Holz's machine. Other papers of interest in this department were by M. Potier on "The Elliptical Polarisation produced by Vitreous Reflection;" by M. Petit on a Modification of the Ordinary Telegraphic System, by means of which the telegraph may be made to print directly; M. Descloizeaux a very curious paper on some of the Optical Phenomena of Crystals; and M. Gariel on the Distribution of Magnetism in Magnets.

In the Chemical Section, presided over by M. Balard, M. Berthelot read a paper of considerable practical value "On the State of Bodies held in Solution," in which he brought much discrimination and research to bear upon some of the ordinary problems of Chemistry, especially upon molecular mechanics. Other papers in this section were by M. Jungfleisch "On the Transformation of Tartaric Acid into Racemic Acid," and by M. Filhol "On the Nature of the Sulphuric Composition of the Mineral Waters of the Pyrenees."

In the Section of Zoology, under M. Soubeiran, various interesting papers were read, including one by M. Chatin, "On the Odoriferous Glands of some Mammiferous Animals;" one by M. Jobert, "On the Organs of Touch in Fishes;" one of considerable interest by M. Soubeiran, on the Oyster Culture at Arcachon; and one by M. Pouchet on the Colourisation of Fishes.

The Geological Section was presided over by M. Raulin, Professor at the Faculty of Sciences at Bordeaux, and in the section devoted to Mathematics, Astronomy, Geodesy, and Mechanics a paper was read by Madame Hureau de Villeneuve "On the Mechanism of the Flying Apparatus of Birds." She was preceded by M. Respighi, who read an able paper on the Scintillation of Stars. In accordance with a long series of observations made by him, he showed that the changes in brightness and colour presented by stars near the horizon are caused by momentary and successive deviations of luminous rays of different colours, by which these rays are carried outside of the object-glass of the telescope or the eye of the observer. These deviations are produced by extraordinary refractions or irregularities in layers of air condensed or rarified, placed at great distance from the observer, and at the precise spot where by atmospheric dispersion the rays of the different colours directed by the object-glass are separated from one another, so as to be only partly contained in the irregularly refracting layers of air. The most important result of M. Respighi's observations is this:—the layers of heterogeneous air are not reached by the luminous rays of different colours by means of the internal movement of atmospheric masses, but by their general movement caused by the rotation of the earth; which shows that the rotation of the earth is one of the principal elements in causing the twinkling of the stars. M. Respighi next described a very ingenious zenith telescope by means of which he can obtain the zenith distance of stars in their passage across the meridian.

In the Section of Geography, Political Economy and Statistics, various papers were read on methods of education.

The first session of the Association lasted eight full days, during which excursions were made to Arcachon, and to the Troglodyte Caves of Eyzies. On the condition and civilisation of the people whose remains they contain, M. Broca gave a very interesting lecture, in which he concluded that these men were savages, but in a state of partial civilisation, having at their disposal abundant food, and consequently leisure, applying themselves to the arts, and already exhibiting the perfectibility of the race. Another excursion was made to the Pointe de Grave, and one, which lasted three days, to the Industrial and Scientific establishment of Landes as far as Bidassoa. The Monday, Wednesday, Thursday, and Friday were devoted to *séances*, the morning for the sections, and the afternoons for general meetings. These public meetings were well attended, especially the evening lectures of MM. Broca and Cornu, who had audiences numbering about 800. Much interest was also manifested in the narratives of MM. Janssen and Respighi, who recounted the results of their researches into the constitution of the sun, and of their visit to India last year.

The reports of the Congress speak in lavish terms of the hospitality and considerateness in all respects of the Bordelais, whose city seems to be one of the foremost in France in respect of educational and scientific institutions. There can be no doubt about the success on the whole of the first meeting of the French Association; and we only hope that by the time it re-assembles at Lyons next year it will have advanced to the same ratio as it has done from its foundation till now, and that ere very long it will have taken as firm root as a recognised and universally beneficial French institution, as the British Association has done among ourselves.

ON PULSE FREQUENCY AND THE FORCES WHICH VARY IT *

THE circulation of the blood is an uniform circulation, the pulsations being neglected, and a uniform circulation is one in which the quantity of fluid flowing through all segments of the circulating system is the same; otherwise there would be a tendency for the fluid to accumulate at certain points, which is contrary to the premises.

To arrive at precise conclusions respecting the circulation there are two points which must be considered—1st., The laws which regulate the flow of fluids through capillary tubes. 2nd., The variations in the capacity of the circulating system under different pressures. These will be considered separately. Poiseuille found that the flow of fluids through capillary tubes varies directly as the pressure and as the fourth power of the diameter of the tubes. The author has verified the former of these results on the vessels of the animal system by a different method. Respecting the capacity of the arteries and ventricles under different blood pressures, it is evident that the capacity of the former must depend on the pressure only, for they are simple elastic tubes, and must be more capacious under high than under low pressures; reasons are given below for a more precise statement of this relation. To maintain a uniform circulation with a pulsating motor, like the heart, it is evident from the above considerations that variations in the resistance at the small arteries must produce variations in pulse-rate; and that unless the capacity of the arteries and heart vary directly as the pressure, variations in blood pressure must be also attended with change in pulse frequency. That the capacity of the ventricles is dependent on the arterial blood pressure can be proved by the varied amount of opening up of the ventricular cavities which follows different fluid pressures in the coronary arteries.

Next, the different forces which vary the pulse-rate must be considered. It can be shown that any change in the resistance to the flow of blood through the capillaries varies the pulse-rate, increased resistance rendering the pulse slower and the reverse. As instances of these effects may be given, the pulse-slowing effects of stripping in a cold air, of a cold bath, and of compression of large arteries; the pulse-quickening effects of a hot bath, whether air or water. Numerous experiments by the author prove that the effect of copious blood-letting is not to modify the pulse-rate at all, thus showing that the law given by Marey respecting pulse frequency is not correctly based. The above points, namely the law of Poiseuille, the dependence of the capacity of the arteries and ventricles on the pressure of the blood, the dependence of the pulse-rate on the peripheral resistance and its non-dependence on the blood pressure, can all be correlated by only one theory, namely, that the heart always re-commences to beat when the tension or pressure in the arteries has fallen at invariable proportions, which also assumes that the capacity of the heart and arteries varies directly as the pressure. The facts that the arteries are generally empty after death, and that the cavity of the heart is sometimes found to be obliterated on *rigor mortis*, show that absence of pressure and capacity go together.

This theory explains the known peculiarities in pulse rate attending change in position, by showing that while standing all the pressure of the body weight is borne by non-compressible rigid tissues and so the circulation is normal, but while lying, the soft parts are compressed and resistance introduced into the circulation, reducing the rapidity of tension-fall, and therefore the frequency of the pulse; an intermediate condition attends the sitting posture. The pulse quickens during inspiration, and becomes slower during expiration; for during the former act the reducing pressure in the chest lowers the aortic blood pressure, and makes the tension-fall more rapid, while in expiration the reverse occurs.

This theory also is the only one which throws light on the cardiograph law published by the author (see *Journal of Anatomy and Physiology*, 1870–73), which may be thus stated —For any given pulse-rate the first part of the heart's revolution has a constant length, but it varies as the square root of the length of the complete pulsation. The pulse-rate not depending on the blood pressure, and the length of the first cardiac interval not varying when the rate is constant, its length also does not depend on the blood pressure. The first cardiac interval may be divided into the systole and the interval between that and the closure of the aortic valve (the diaspasis); these combined not varying as the blood pressure, it is almost certain that separately they do not do so either; so it may be said that neither the length of the systole nor of the diaspasis depends on the blood pressure. But the fall of tension between the pulse beats being but small, and the diaspasis length not depending on the blood pressure, there is no reason why it should vary in length with different pulse-rates; and assuming this in connection with the measured diaspasis length in a particular case (·00183 of a minute), it can be deducted from the above cardiograph law, that the systolic length varies as the square root of the diastolic. From these facts the relation of the nutrition of the heart to the time of heart nutrition (diastole); and to the blood pressure, may be deduced; for the systolic length not varying with the blood pressure when the pulse rate is constant it is evident that the cardiac nutrition must vary directly as the blood pressure in the aorta; and the systole varying as the square root of the diastolic time, shews that the nutrition of the heart varies as the square of the time of nutrition (diastole), for with a quadruple resistance to the peripheral circulation, the heart would be four times

* Paper read before the British Association at Brighton in Section D, Department of Anatomy and Physiology,) by A. H. Garrod.

the time in emptying itself, but it is only double that time, which demonstrates the statement.

A complete logical explanation of the action of the pneumogastric can be given on this theory, by assuming that its function consists in diminishing the calibre of the small arteries of the coronary system, and always keeping them somewhat contracted.

PHENOMENA OF COAGULATION IN FROG'S BLOOD *

I WAS endeavouring in the autumn of last year, at Prof. Sanderson's instigation, to demonstrate upon the frog some of Brücke's fundamental experiments on the coagulation of the blood, which he performed on the tortoise; I was surprised at the apparent failure of some of them. For instance, having tied a glass tube into the animal's aorta and allowed it to fill with blood, I expected that which was in the tube speedily to coagulate, that which remained in the heart to continue liquid for a considerable time. But no such contrast was observable, both portions of blood remained perfectly fluid for an indefinite time. I say apparently, for, in fact, on subsequently turning out the blood, a slight film of coagulated fibrin was observable attached to the walls of the tube. Of course the corpuscles being the heavier gravitate to the bottom, and the blood thus becomes divided into two portions, a clear fluid above and a mass of red corpuscles below, with a thin filmy stratum of white again on the surface of the latter.

To show that the clear fluid is plasma and not merely serum, that is to say, that it fully retains its coagulability, it is sufficient to take a little up into a very fine, almost capillary, glass tube. The extent of surface to which it is thus exposed very quickly determines its coagulation.

Following up the subject still further, I found the same thing to happen when the blood is allowed to drop into a glass vessel, the whole remaining fluid, except that portion in immediate contact with the sides, the corpuscles subsiding as before, and the supernatant liquid being readily coagulable in a capillary tube.

But frog's blood does not always behave in this manner. It is not unfrequently the case, especially at this season of the year, that the blood of these animals behaves to all appearance precisely as we are in the habit of expecting that blood should behave, that is to say, the commencing subsidence of the corpuscles is arrested, the fluid solidifies, seemingly throughout. And, indeed, in rare instances, the coagulation is complete to the centre, and the mass soon separates into clot and serum, which latter, in these cases, never yields a coagulum in a capillary tube. More frequently, however, on breaking the surface with a knife, the interior of the coagulated mass is seen to be occupied by still fluid blood.

In either case, the coagulated fibrin soon begins to contract; and this contraction proceeds to such an extent that not only is the serum of the blood expressed from it, but it comes to pass that there is no longer room in its meshes for the involved corpuscles, which consequently begin to be squeezed out and to fall to the bottom of the glass. This diminution in volume of the clot may proceed so far that in the course of a few hours the blood may present an appearance precisely as if it had not undergone coagulation at all, there being a mass of corpuscles at the bottom of the vessel, and a clear supernatant fluid. The contracted remains of the clot may however be always found, although often obscured by the liberated corpuscles. Now, this disappearance of the clot of frog's blood under certain circumstances was noticed some years ago by v. Reclinghausen, and ascribed by him to a re-liquefaction of the fibrin; and not un naturally, if we consider the astonishing diminution in bulk which it undergoes, and the fact that the serum in such cases is frequently found to yield a further coagulum.

But in every case of the latter kind, *i.e.* in every case in which the supernatant fluid yields a coagulum in a capillary tube, it will have been found that the primary coagulation was incomplete, *i.e.* that the central parts of the blood remained fluid, whereas on the other hand it is certain that when the primary coagulation has been complete, no further coagulum is *ever* obtainable, although, in this case also, the clot may have contracted to a relatively exceedingly small bulk, in fact, may have almost disappeared.

A further proof, if one were needed, that the diminution of the clot is due merely to contraction and not re-liquefaction of fibrin, is to be found in the examination under the microscope, using an immersion objective, of the process as it occurs in a very thin walled and fine capillary glass tube.

The phenomena here observed are wholly those of contraction; first simply serum, then white corpuscles, and finally red corpuscles being expressed, until a mere thread of fibrin remains, almost obscured by the corpuscles and still including a few.

Throughout the whole process, however, there is no trace of a re-liquefaction of fibrin; this would of course involve the dropping away of the corpuscles from the sides; on the contrary, they are most evidently squeezed out, some of them being actually ruptured in the passage and appearing on the exterior of the clot as small reddish spheroids. The facts then, briefly, are these: that frog's blood, especially if taken during the winter months, exhibits but very little tendency to coagulate, with the exception of the portion in immediate contact with a foreign surface; that, when apparently coagulated throughout, the central portions are very apt to remain fluid, and to impart coagulability to the expressed serum; that the clot when formed frequently tends to attain a relatively very small bulk; and, finally, that this diminution in bulk is due to contraction merely, not reliquefaction of the fibrin.

* [Paper read before the British Association at Brighton in Section D. (Department of Anatomy and Physiology), by E. A Schäfer, M.B.

PHYSICS

Acoustic Experiments on the Seine during the Siege of Paris

IN the experiments made by Colladon and Sturm on the Lake of Geneva, in 1827, to determine the velocity of sound in water, the source of sound was a bell, weighing sixty-five kilogrammes, fixed to a boat immersed in the water near Rolle. Another boat, moored near Thonon, carried the observers, who employed a long acoustic tube made of metal, one extremity of which, widened and closed with a membrane, was thrust into the water. The distance from Rolle to Thonon is about 13,500 metres, so that the range of the sound was considerable. The water in that part of the lake is of great depth.

During the siege of Paris, the idea arose of establishing an acoustic telegraph by means of the Seine, between the invested city and provinces that had not been invaded. The Geneva experiments appeared to favour the proposal.

M. Lucas was charged by the Minister of Public Works to make some experiments on the subject, which he accordingly did in November, 1870. He gives an account of these to the Paris Academy.

In the first series, a bell weighing forty kilogrammes was lowered by a windlass from the bow of a barge, to a position twenty or thirty centimetres from the bottom. It contained a clapper, which was moved by means of wires carried up to the barge. Two workmen were charged to ring the bell at certain fixed intervals, while the observers, in another boat, marked the effect at different distances, being carried along by the current. The acoustic tube employed was 1·50m. long, and the membrane of its orifice, immersed in the water, was turned towards the bell. At the distance of a few metres, a dull sound was heard (like that of a drum beat with a drum-stick), at each stroke given to the bell. The intensity diminished with the distance, and the sound ceased to be perceptible at about 1,800 metres. The result was constant for experiments repeated at different parts of the river.

In a second series of experiments, a bronze bell, weighing 354 kilogrammes, was used. This was hung in a wooden frame weighing 446 kilogrammes, constructed in the form of a quadrangular pyramid. The hammer of the bell weighed sixteen kilogrammes, and was moved by wires, as in the other case. The frame and bell were suspended by chains from the four corners, between two barges, and then lowered into the water. The mode of observation was the same as in the former case.

A few metres from the barge a slight metallic sound was heard, doubtless from the acoustic tube vibrating with the membrane. The sound soon became dull, its intensity decreased rapidly with the distance; at 1,400 or 1,500 metres there was no perception of it.

Comparing these experiments with those of the first series, we have the unexpected result that the very intense sound of a bell weighing 354 kilogrammes has a less range than the weaker sound from a bell of forty kilogrammes.

In a third series, a small bell, twelve centimetres diameter, was sounded in the water alternately with the bell of forty kilogrammes; the range of the latter extended, as before, to 1,600 or 1,800 metres; that of the little bell was small, it exceeded one kilometre, however. M. Lucas concludes from the experiments that the range of sound in a river, even in the direction of motion of the water, is much less than that of sound in a lake; and that by increasing considerably both the intensity and the gravity of the sound, the range is but little increased, and may even be diminished. It further appears that, with equal intensity, the range of sound in a river will increase with its acuteness. If so, a considerable range ought to be obtained, he thinks, with a blast of compressed air for the sonorous source. A. B. M.

SCIENTIFIC SERIALS

THE *Geological Magazines*, Nos. 97—99 (July to September 1872).—One of the most important contributions contained in these three numbers is a translation of Prof. Nordenskiöld's account of his expedition to Greenland, which runs through the whole of them, and is not yet completed. With a good deal of general matter, this paper includes a vast amount of interesting geological information, and it must be welcome to English geologists, few of whom could make much out of it in its original Swedish dress. This translation is illustrated with the original plates, maps, and woodcuts.—Another highly important memoir, which appears in two parts in the July and August numbers, is Dr. H. B. Holl's essay on those most puzzling objects, the fossil sponges.—In the August number Prof. Allman gives us a note on a fossil *Hydractinia* from the Coralline Crag, the *Cellepora echinata* of Michelin.—Mr. S. V. Wood, jun., has some further remarks on Mr. Geikie's correlation of the glacial deposits; and Mr. J. Lucas a paper on the Permian Beds in Yorkshire, one portion of which calls forth a note by Mr. J. Clifton Ward, on rock-staining, in the September number.—The latter further contains a notice of the occurrence of the genus *Cupressocrinus* in the English Devonian, near King's Teignton, the first part of a paper, by Mr. Alfred Tylor, on the formation of Deltas, and some other papers, among which we may mention especially Mr. Woodward's description of a new species of Phalangiiform Arachnide (*Architarbus subovalis*) from the Lancashire Coal Measures, which is especially interesting from its generic identity with a North American form.

THE *Memoirs of the Natural History Society of Bremen* for 1872 (Abhandlungen herausgegeben vom naturwissenschaftlichen Vereine zu Bremen, Band iii. Heft 1) contain some exceedingly valuable papers for the study of zoological and botanical geology. In the former department we have Dr. O. Finsch's contribution to the ornithology of North-western America, from which inhospitable region the author records about 120 species of birds, many of them possessing a very wide geographical range. Singularly enough the author does not describe a single new species, but of some variable forms detailed descriptions are given, and with regard to many others we find valuable synomymic and descriptive remarks.—The flora of high northern latitudes receives a contribution in the shape of a list of the vascular plants of Spitzbergen and Bear Island, by Dr. T. M. Fries.—The student of the geographical distribution of European plants will find most valuable information in M. C. Nöldeke's Flora of the East Frisian Islands (including Wangeroog) which treats of the vascular plants in a most exhaustive fashion, and is supplemented by a short notice of the mosses of the islands, by M. C. E. Eiben.—From Drs. Buchenau and Focke we have an important paper on the *Salicorniæ* of the Baltic coasts of Germany, which includes a discussion of the views of previous authors on the species of that genus.—Dr. Buchenau has also a paper on the variation of the curious bracts of the lime tree.

SOCIETIES AND ACADEMIES

SAN FRANCISCO

Academy of Sciences, May 20.—Dr. Blake, on presenting some stone implements that had been found in stratified rock, at an elevation of 1,700 feet, observed there can be no doubt that up to the present time the earliest traces of man's existence on the earth have been found in this country. The skull found in the Pliocene deposits in Table Mountain is certainly the oldest human skull that has yet been discovered. Many stone mortars have also been taken out from the same deposits. I have examined the spot where one of these mortars was taken out ten feet beneath the surface of the Pliocene gravels on the Sierrasr and as this was some six hundred feet above the valley and near the top of a rolling hill, there was no possibility of the strata having been disturbed. The objects I now have to present to the Academy furnish important evidence on this point. They are two implements cut in serpentine, and evidently fashioned by the hand of man, or of some animal capable of using its anterior extremities so as to fashion objects to meet its wants, and apparently possessed of sufficient intelligence to use lines or nets for catching fish, as it would seem that these instruments must have been used as sinkers. They are cut in serpentine, the surface of which is slightly weathered to the depth of about $\frac{1}{20}$ of an inch. One is pear-shaped, $\frac{3}{4}$ in. long, and $5\frac{1}{2}$ in. in circumference at its largest part; near the smaller end is a hole passing through it, and immediately above this is a notch passing across the end. The other instrument is cylindrical, thicker in the middle than at the ends; it is $5\frac{1}{2}$ in. long, and $3\frac{1}{2}$ in. in circumference at its thickest part. There is a hole through it at about an inch from one end, and above the hole is a notch passing across the end. The spot where they were found is on a rolling hill within a few feet of the summit of the coast range, and at an elevation of about 1,700 feet. They were met with in digging away the side of the hill, at about eight feet from the surface, four feet being alluvium, and four feet argillaceous shales, in which they were found embedded. These, and four other instruments of the same form, were found in a space of about two square feet. A great deal of alluvial soil had been removed whilst levelling the ground for a house, but no instruments had been found until the shales were dug into. In company with Prof. Whitney I visited the spot, and we have not the slightest doubt but that they were taken out at the place indicated. As to the geological age of the rock in which they were embedded, this is to a certain extent undetermined. It certainly cannot be later than the Pliocene, and Prof. Whitney is of opinion that it is still older. Dr. Blake then made some remarks as to the absence of anything like a rim to the Great Basin, north of the Puebla Mountains. In going north from the valley of the Humboldt near Mill City to the Puebla Valley, the highest elevation crossed between the Rattlesnake and Vicksburg ranges did not exceed three hundred feet, whilst the country to the east of this, between the Rattlesnake and Umsham ranges, is at a still lower level. North of the Puebla Buttes nothing but some low ranges are found until we reach the head waters of the Owhyee, a tributary of the Columbia, so that there can be no doubt but that the larger part of the waters of the Great Basin must have escaped through the valley of the Columbia. The quantity that was left for evaporation did not probably exceed from 100 to 150 feet, as he had not found concretionary deposits at a greater height than 100 feet above the present level of the Humboldt.

BOOKS RECEIVED.

ENGLISH.—The Vegetable World L. Figuier, English edition (Cassell and Co.).—A Manual of Microscopic Mounting: J. H. Martin (Churchill).

FOREIGN.—(Through Williams and Norgate).—Thesaurus Ornithologiæ, 1er Band, 2te Heft: Dr. C. G. Giebel.

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