THURSDAY, JANUARY 8, 1874

VIVISECTION

THE question of the propriety of vivisection has ever and anon cropped up for the last two centuries, and learned and unlearned persons have not been found wanting to condemn the practice. Amongst the latter the term vivisection has been taken to mean the dissecting of animals alive, with no other motive than curiosity or a malignant desire to be cruel to animals.

This arises from the utter and entire ignorance, on the part of the great mass of the public, of the scope and nature of physiology or the laws of life. If the elements of this noble and most useful science were taught in our schools as they should be, the unmeaning outcry against the practice of "dissecting live animals," as it is called, would not be heard. People would then know that the wonderful knowledge now possessed by man of the functions of his body has mainly been acquired by experiments on living animals, and that by the practice of vivisection is not meant the dissection of living animals, but the performance of experiments by which the nature of the functions of living beings may be ascertained.

Whatever excuse may be made for the public on account of their ignorance, there ought not to be any for men belonging to the medical profession, who should know the history of the science of physiology and the dependence of all true practice of medicine and surgery on the laws of life, mainly gained by humane and careful experiments upon living animals. These men would be answerable for much human suffering and premature death if they compelled men of science to give up the practice of studying the laws of human life and arrest the hand of Science in investigating the functions of living animals by inspection and experiments.

We feel almost ashamed in the present age to have to speak of the grand results which have been reaped by mankind from the observations of our great physiological discoverers in experiments on living animals. To begin with Harvey, whose name is a household word amongst us, and one of the grandest on the long page of England's discoverers; it is no perversion of words to say that he could not have discovered or demonstrated the circulation of the blood without the aid of vivisection.

In his great work, "An Anatomical Disquisition on the Motion of the Heart and Blood in Animals," he heads the second chapter "Of the motions of the heart as seen in the dissection of living animals." In this work he gives detailed accounts of his experiments, and also of those performed before the noblest and most learned in the land, who did not object to Harvey's experiments, but felt they were witnessing the demonstration of a truth that would for ever be a benefit to mankind. Had public opinion, had the Government of the day, instead of encouraging Harvey proceeded to prosecute him for cruelty to animals, then mankind would have lost a discovery that has saved myriads of human lives from torture and premature death by disease.

The discovery of the circulation of the blood produced an immense revolution in the practice of medicine and surgery. Counting the pulse became an intelligent aid to the diagnosis of nearly all diseases. Operations for the relief of disease were undertaken with fearlessness and the greatest success. The nature of aneurism and its means of cure were now understood. This last disease was studied and the surgical operation for its cure almost perfected by experiments on living animals by John Hunter. This great anatomist also made most important contributions to our knowledge of the nature of venous absorption, by his operations on animals. Nearly all the advances that have taken place in the treatment of aneurism since the time of Hunter have been made by experiments on living animals, amongst others we may name those of Spence, of Edinburgh.

Only to mention names rising to the surface from the greatness of their discoveries, we refer to Sir Charles Bell, to whom we are indebted for a knowledge of the nature of sensationary and voluntary nerves and their double origin in the spinal cord. These discoveries were made by experiments on living animals, and belong to a series which cannot be performed by the aid of anæsthetics, as the very essence of them consists in demonstrating that whilst one set of nerves is devoted to the feeling of pain, the other is the means of producing locomotion.

Another almost equally important discovery, the nature of the excito-motory action of the nervous system, was demonstrated by experiments on living animals by Marshall Hall. To say that these discoveries of Bell and Hall have had no influence on pathology and therapeutics, is to deny the experience of every medical practitioner in the kingdom—is to proclaim that the science of medicine is now practised on the system pursued by physicians and surgeons previous to the time of the discovery of the circulation of the blood. Numerous are the discoverers who have made great advances in our knowledge of the functions of the nervous system, by observations on living animals, who still live to be honoured for the advances they have made in that science which leads to the amelioration of human suffering. We need but mention here the names of Brown-Sequard and Ferrier. No human mind could have guessed at the conclusions at which they have arrived, but they have done so by the sure and certain method of observing facts in the living organism.

We might go on and fill our pages with the memories of great men who have not hesitated, for the benefit of mankind and the advancement of Science, to sacrifice the life of the lower animals. Majendie was accused in Paris of cruelty to animals, but his experiments led to a more accurate knowledge of the influence of medicines on the animal frame, and the introduction of a number of new remedies, which are still in common use. Blake, by the introduction of saline substances into the blood of living animals, showed what was the action of these matters on the blood, and he produced a sensible effect on the practice of medicine.

To the instructed this will seem a meagre list; but we hope enough has been said to show that to deny the utility of experiments on living animals is to deny that medicine has advanced at all during the last two centuries and a half, and to admit that the guesses of uninstructed practitioners are as good as the practice of the most cultivated practitioners of medicine and surgery.

Against this proof of the benefits of vivisection it has been urged that man has no right to inflict pain on animals. The same argument has been urged against the destruction of the life of animals at all, and the adoption of a vegetarian diet has been the result. It is surely not needful to answer the last argument here, but in a degree the answer is the same against giving pain to animals; if we take animal life for the purpose of food, it is only taking the life we have given us for the purpose of our existence; and in giving a minimum of pain to animals we give it for the higher purposes of securing human life and freedom from pain. It is curious to see those who defend the cruel sports of fox-hunting, hare-hunting, and partridge and pheasant shooting exclaim against the cruelty of vivisection. Yet it could be clearly shown, we believe, that those physiologists who are in the habit of practising vivisection would not be found at Hurlingham taking part in pigeon-shooting, or meeting with the hounds in any part of the country. In fact, so far from producing a hardening effect on the mind, these experiments seem to engender in the mind of the observer a love and a care for the brute creation, that does not exist in the mind of an ordinary person. A celebrated entomologist, in answer to the objection made to the pursuit of his science, the destruction of the life of insects, made answer that his habit of observing insects had induced him at various times to save more lives of insects—as flies from the cream-jug and tea-cup—than he had ever destroyed to make his entomological collection.

The question still arises whether the experiments that resulted in the discoveries to which we have referred should be repeated for the instruction of a class, or be regarded as final? Many physiologists think that the renewal of the experiments in the form of a demonstration before a class is not necessary. This position, however, cannot be maintained, if regard is had to the good of mankind. He would be a poor chemist who did not re-perform the experiments of those who had gone before him; and the natural philosopher could not make progress in his science if forbidden to repeat the observations of his predecessors. It is not only necessary to make good practitioners of medicine, and surgery that these experiments should be repeated but it is necessary for the advancement of the science of physiology.

Of course all these experiments should be performed with the greatest attention to diminishing pain to the utmost extent. Happily, by the use of anæsthetics, we can now do this so that an animal does not suffer more than it would in passing out of existence in any other way. And we are glad to find whilst writing this, that Prof. Schiff, of Florence, who has been so unrighteously assailed for these experiments, in a letter to the *Times* completely refutes all the charges brought against him, never failing to administer anæsthetics in the performance of these operations.

THE RELATION OF MIND AND BODY

Mind and Body. The Theories of their Relation. By Alexander Bain, LL.D., Professor of Logic in the University of Aberdeen. (Henry S. King and Co., 1873.)

IN this volume, which forms one of the international scientific series, the thoughtful reader is once more called on to consider those leading positions in psychology for which Prof. Bain has so long and so ably contended. He has here succeeded in presenting his views in language as concise, clear, and popular as the nature of his subject will permit. Whoever attaches importance to the application of scientific method to mental phenomena must welcome this popular statement of doctrines, which, if not the whole truth, are immeasurably nearer the truth than are the superstitions to which not only the uneducated, but also the great mass of the learned, are subject.

It is already known that Prof. Bain has given his adhesion, more or less fully, to the doctrine of inheritance in the region both of intellect and emotion—a doctrine without which the "experience" philosophy was utterly inadequate to explain the known facts. We may therefore be allowed to regret that he has not in this volume given more prominence to a conception without which his own system is but a half truth plus something of positive error. We are disappointed, for we certainly expected more than grudging references to "the new theory."

We have before now indicated our opinion that there is something wrong about Prof. Bain's celebrated theory of the Will; and we cannot now refrain from observing that in the present volume he seems to us to make the weakness of his position more manifest by placing alongside of his old theory some of the clearer and more thorough conceptions of recent development. "The distinguishing peculiarity of our voluntary movements," says Prof. Bain, "is that they take their rise in Feeling, and are guided by Intellect." Now our contention is, that there is no fact in nature corresponding to this description. Taking it for granted that "feeling" and "intellect" here mean facts of consciousness, and not physical facts—the objective activity of nerve cells and nerve fibres—we assert (1) that taken in the lump it is an expression of the popular notion, which Prof. Bain rejects, that the body is governed by the mind somewhat in the same way that the horse is governed by his rider; (2) that looked at closely it is a string of words making up a proposition that cannot be represented in thought. In support of the first point in our criticism it must suffice to show that Prof. Bain's teaching with regard to the will is relied on by the most thoughtful advocates of the doctrine of the soul—a belief against which Prof. Bain has been fighting all his life. A perfect example of the way in which Prof. Bain's theory is interpreted in favour of the hypothesis of a soul will be found in Mr. Lowne's "Philosophy of Evolution." We had recently occasion to make a few remarks on this essay, and we cannot now do better than quote part of what we then wrote:—"It is in studying the phenomena of volition (as understood by Prof. Bain) that Mr. Lowne finds the unmistakable evidence of a spiritual clerk employed in working the nervous apparatus. . . . Comparing the nervous system to a complex telegraphic system, he says:—'If the electric fluid became periodically liberated and affected all the instruments at once, or in a given succession, mechanism alone would account for the phenomena (reflex action); but if the electric current were always utilised according to ever-varying conditions which do not bear any direct relation to the manner in which the effect is produced—that is, which are themselves unable to alter the arrangement of the apparatus by which the effects are brought about—a guiding intelligence is needed (voluntary action). Such appears to be the condition of the nervous system in the higher forms of life; and we recognise such a guiding power, although we know of its existence only by its effects on the organic mechanism; and we speak of it as the mind or soul.'" It is for those who, holding Prof. Bain's theory of volition, reject the popular hypothesis that the body is endowed with a soul, to show the flaw in Prof. Lowne's argument. In saying this, however, we by no means wish to imply that there is not much in the writings of Prof. Bain quite inconsistent with this interpretation of his doctrine. Indeed we find set out with remarkable clearness in the volume before us some of the considerations which we urged, not against Mr. Lowne's argument, but against the theory of volition on which it is founded. "There is no warrant for the assumption (we said) that any movement of the kind called voluntary is not as completely and necessarily the result of purely physical antecedents, as are the movements of the planets or the spelling out of a telegraphic message . . . Whatever may be the link of connection between consciousness and nervous action, it seems both unnecessary and irrational to assert that either the amount or the direction of any vervous discharge depends in the slightest degree on the state of consciousness that preceded or accompanies it.

Sitting in his easy chair, Mr. Brown debates with himself how much he will give to the Mill Memorial Fund. Greed, small vanity, respect for Mr. Mill, the fear of being thought shabby, and perhaps a score of other mental states come and go, and at last he writes a cheque for 5*l*. Mr. Brown was aware of the mental side of his deliberations, while the corresponding physical changes in his nervous system were hidden from his observation. Hence the easy mistake of supposing that in writing out the cheque the fingers moved in obedience to spiritual direction." This view seemed, and still seems to us, to forbid every conceivable interpretation of the proposition that movements "take their rise in feeling and are guided by intellect." It would appear, however, that what we feel to be an incongruity, does not strike Prof. Bain as such. For he also, if we understand him aright, believes the physical chain to be at all points complete and sufficient within itself. At least we find it difficult to understand the following extract from the chapter "How are Mind and Body united?" in any other sense. "From the ingress of a sensation, to the outgoing response in action, the mental succession is not for an instant dissevered from a physical succession. A new prospect bursts upon the view; there is a mental result of sensation, motion, thought, terminating in outward displays of speech or gesture. Parallel to this series is the physical series of facts, the successive agitation of the physical organs, called the eye, the retina, the optic nerve, optic centres, cerebral hemispheres, outgoing nerves, muscles, &c. While we go the round of the mental circle of sensation, emotion, and thought there is an unbroken physical circle of effects. It would be incompatible with everything we know of the cerebral actions to suppose that the physical chain ends abruptly in a physical void occupied by an immaterial substance; which immaterial substance, after working alone, imparts its results to the other edge of the physical break, and determines the active response—two shores of the material with an intervening ocean of the immaterial." Now remembering that movements of all kinds are physical facts, have their place in the "unbroken material succession," we once more put the question—In what sense can a particular class of movements be said to take their rise in the mental series which runs parallel to, without forming part of, the physical series?

The truth or meaning of our assertion that the proposition, "movements take their rise in feeling," cannot be rendered into thought, may now be perceived by anyone who will attempt to picture to themselves a state of consciousness turning on, or in any way determining the direction of, a nervous discharge. But as some of our philosophers, strong in logic, can surmount psychological impossibilities with the same ease that our divines can rise above them on the wings of faith, the disciples of Mr. Mill and Prof. Bain may demur that the question is not one of conceivableness or inconceivableness, but of proof. Well, then, let them show, if they can, that they have any better ground for the opinion that voluntary movements take their rise in feeling and are guided by intellect, than a superficial observer ignorant of the construction of the steam-engine might have for a belief that the movements of a locomotive take their rise in noise and are guided by smoke. Should it be attempted to turn the point of the foregoing argument by aid of the curious description of a mental fact, that it is a "two-sided fact"—both body and mind—our difficulty only requires to be restated. In what sense can a movement called voluntary—the objective side of a "mental fact"—take its rise in feeling the subjective side of the same "two-sided fact"? Using Prof. Bain's own words, "it is, after all, body acting upon body."

In this work Prof. Bain does not advance his idealism; probably he may have concluded, and justly, that it would prove too metaphysical for the readers of the International Scientific Series. Throughout his language is that of a realist. Mind and Matter seem to be accepted as ultimate facts; and "the institution of two distinct entities" is spoken of as "not in itself a crushing dispensation." Not only so, in such expressions as "undivided twins," "one substance, with two sets of properties, two sides, the physical and the mental—a double-faced unity," we have, to say the least, very much of the ring of Mr. Spencer's hypothesis that nervous action and consciousness are the objective and the subjective faces of his Unknowable—the one Ultimate Reality. We do not say that Prof. Bain is attempting the dangerous experiment of trying to put new wine into old bottles, but we fear until he has explained more fully the modifications which, by changes or additions, he means to make in his system, his present deliverance will be apt to suggest this.

DOUGLAS A. SPALDING

THE ELEMENTS OF LOGARITHMS

The Elements of Logarithms. By J. M. Peirce. (Boston, U.S.A.: Ginn Brothers, 1873.)

IN the preface Prof. Peirce writes:—"Logarithms ought not to be comprised, as they often are, in the midst of a treatise on algebra. For, in the first place, they are not algebraic functions; and, besides this, the student is unlikely to form an adequate comprehension of their purpose, or to appreciate the importance of acquiring skill in the use of his tables if he takes them up in the course of a study to which they have no application. If logarithms must needs be combined with any other branch of mathematics, their true alliance, on grounds both theoretical and practical, is not with algebra but with trigonometry." In point of fact, logarithms are usually included in works on trigonometry; and we can see no reason why their principle should not also be explained, as at present, in treatises on algebra, to which the theory does really belong. Ordinary students of mathematics never learn to use logarithms properly, not so much owing to deficiency of explanation in the existing works as to the fact that they never meet in the course of their reading with anything requiring such a knowledge. Prof. Peirce's work contains 82 small octavo pages, and is intended for readers possessed of only a very trifling knowledge of algebra. It is simply what a chapter on logarithms in an ordinary algebra would become if printed separately, with the addition of copious examples and an appendix on their use in trigonometry. To show how limited is the range of the book, it is only necessary to state that all the rules have reference merely to three and four figure tables, and that the natural base e is not even alluded to, though it is stated that a chapter on the Napierian system will be added in another edition. There is little either to commend or blame in the book. It is partly intended for the entrance examination at Harvard, but it seems to us it would be most useful to computers who wished to obtain some notion of the reason for the rules they were in the habit of employing. In one respect the book is in advance of the time, viz., some paragraphs are devoted to the history of the subject. We believe the day will come when no scientific treatise will be considered complete that does not contain short historical notices relative to the discovery of the principal results.

Prof. Peirce defines the arithmetical complement as the complement from 10; we should much prefer to see it defined as the complement from zero, so that the arithmetical complement of a logarithm of a number should be the logarithm of the reciprocal of the number, viz. its cologarithm. We also hope the day will come when the addition of 10 to the mantissæ in our logarithmic trigonometrical canons will be abandoned, and the true negative characteristics printed and used. A complete seven-figure table with negative mantissæ was published at Paris by M. J. Dupuis in 1868, which was a step in this direction.

PEDIGREE AND RELATIONSHIP OF MAN

The Story of the Earth and Man. By J. W. Dawson, LL.D., F.R.S. (Hodder and Stoughton.)

Man and Apes. By St. George Mivart. (Hardwicke.)

THESE two works possess some points in common. Neither of their authors accept Darwinism in its entirety, the former absolutely rejecting it. They both treat of the relations of man to the lower animals, and both find the chasm of the human mental and moral phenomena the great drawback against bringing man into the same category with the apes. The manner in which the subject is treated, and the facts employed, are however not the same, while the results arrived at are very different, as will be seen from the following remarks.

Dr. Dawson is very much irritated by the manner in which many of the biologists of the present day, without feeling any necessity for giving the reasons for their belief, are in the habit of writing and talking as if the evolution hypothesis were fully proved, and established as a fundamental principle of nature. "That in our day a system destitute of any shadow of proof, and supported merely by vague analogies and figures of speech, and by the arbitrary and artificial coherence of its own parts, should be accepted as a philosophy, and should find able adherents to string upon its thread of hypotheses our vast and weighty stores of knowledge, is surpassingly strange,' remarks our author in a spirit which we are surprised to meet in one who thinks that "in the present state of natural science in Britain this evil (of regarding geologic facts from an evolutionary point of view) is to be remedied only by providing a wider and deeper culture for our young men." In the same dogmatic and unscientific spirit all the theoretical questions which are discussed, are written for the perusal of the readers of a popular journal, and such being the case, it is hardly surprising that false notions are so common as to the direct bearing and tendency of the greatest theory of modern times.

"We need not stop to mention the usual inaccuracies as to facts" is the way in which a criticism of a paragraph in one of Mr. Herbert Spencer's works is commenced, and as might be almost predicted from so self-satisfied an author, it is in the criticism only in which the inaccuracy is to be found. On the following leaf we are astonished to learn with reference to the Ascidian, that its "resemblance to a vertebrate animal is merely analogical, altogether temporary and belonging to the young state of the creature, without affecting its adult state or its real affinities with the mollusks." The author can hardly have studied Kowalevsky's memoir on the subject, in detail.

In his anatomical structure, man, according to Mr. Dawson's distorted view, presents differences from all the apes which are at least of ordinal importance, distinctions "mainly dependent on grade or rank, and not to be broken down by obscure resemblances of internal anatomy having no relation to this point, but to physiological features of very secondary importance." When, in association with this, we are told that it is merely begging the question to say that "the fact that the human skeleton is constructed on the same principles as that of an ape or a dog, must have some connection with a common ancestry of these animals," we think it hardly necessary to make further comment on the work in question, except to hope that it will not fall into the hands of commencing biological students, who would find it difficult to shake off the false associations that, in it, surround the facts which are discussed.

Mr. Mivart treats his subject in a very different manner. His object is "to investigate by the unimpassioned process of enumeration and weighing facts of structure, what is the teaching of Nature as to the affinities of various apes to man." In doing this, after a rapid review of the clasification of Mammalia generally, and the geographical distribution of the apes and lemurs, or half apes, as they are termed, the peculiarities of the osteology of the Primates, and their soft-part anatomy, are entered into in detail. From the facts thus obtained, especially from the peculiarities of the liver and brain, it is shown that the, at present accepted, notion that the Gorilla is man's nearest ally, is not borne out by anatomical investigation, and that the Chimpanzee, the Orang, or the Gibbon can either of them claim a closer relationship. The recapitulation of the many different points in which man in some one or other point resembles the various higher and lower apes, leads the author to think that the laws of affinity form a "network" or "tangled web" rather than a "ladder," from which it is only possible to infer that in the course of development there has been blood relationship established between the different species of apes, after their differentiation into distinct species, which is hardly compatible with our notions of the definition of a species. No decided opinion is given as to which ape does stand nearest to man, the various points of similarity in each being considered as fairly balanced. However, there are two structural features at least that are not mentioned, which, when added to those noted, go strongly to support the placing of the Orang-Utang, as the nearest ally to the human race. The first of these is an osteological one; in man and the orang the postero-internal angles of the orbital plates of the frontal bone do not meet and blend behind the cribriform plate of the ethmoid bone, as they do in the gorilla and chimpanzee. The second is in the soft part, the penis of the orang being very similar in general proportions to that of man, whilst in the chimpanzee at least, it is decidedly different, being smaller proportionately, and with a button-shaped glans.

Mr. Mivart adds the weight of Gratiolet's bold attempt to classify the monkeys by their cerebral convolutions, to show more demonstrably that the gorilla is anything but as high as he has been placed in the scale. Though Gratiolet may have been correct in so displacing the gorilla, nevertheless it is difficult to believe from their general appearance, structure, and geographical range, that, as he thought, the baboons and cercopitheci are far separate from one another, that the Asiatic true macaques and the African chimpanzee, are most closely allied; and that the affinities between the Entellus monkey and the orang are very intimate. These somewhat shake our faith in the results that have as yet been arrived at from the study of the cerebral convolutions.

No animal seems more difficult to depict correctly than the monkey, man alone excepted, which may in itself be considered to indicate a point of affinity; the illustrations accompanying Mr. Mivart's work, are, however, of the poorest description, many not being the least worthy of their author; old, and imperfect, inaccurate in not in any way giving the expressions or correct attitudes of the originals, we should have preferred to see them omitted.

OUR BOOK SHELF

Geology. By Prof. Geikie. Science Primers. (Macmillan and Co. 1873.)

THIS is a charming little book of 128 pages. It is well arranged, well written, and well illustrated, and is thoroughly well adapted for its purpose. Of course the geology in it is unexceptionable, and therefore it follows that the only thing with which a reviewer can quarrel at all is the selection of subjects for omission. Among small omissions he might mention that of Darwin's theory of coral islands as a "proof that a part of the crust of the earth has sunk down." This is so beautiful an instance of an explanation of the curious phenomena of coral islands, that it never fails to interest boys. To lead them up to this theory, and then test it as Darwin tested it, is an excellent exercise in that peculiar kind of reasoning about past causation which is of the essence of geology. A greater omission is perhaps that of the history of geological science. A sketch of this in half a dozen pages would greatly interest boys; it would show them how science grows; and they would infer that geology is not yet completely mastered, but that there is something left for them to do. It strikes one also as an omission, of a very grave kind, to say nothing at all about stratigraphical geology, a few pages of it with a general description of the stratigraphical structure of England would increase the value of the book, and what is more, inspire the reader with a desire to learn more. And lastly, one cannot but desiderate some sketch of the sequence of life on the earth as the result of palæontology, for the same reasons. If all these things were put in, the book would still be small, and would really introduce the reader to the whole of geology, and excite his curiosity. J. M. W.

Jahrbuch der kais. kön. geologischen Reichsanstalt. Band xxii. Nos. 3 and 4.

IN the first of these numbers, perhaps the most interesting paper to an English geologist is one by von Theodor Fuchs, "On Peculiar disturbances in the Tertiary Formations of the Vienna Basin, and on a self-evident Movement of Unconsolidated Earth-masses," which is accompanied by a number of illustrations and sketch sections, taken chiefly from the cuttings of the railway at Marchegg. The writer thinks that the contortions and displacements witnessed in superficial deposits, and which have been variously accounted for—some geologists supposing them to be due to subterranean action, others to glacial action, and so forth—have been induced by causes, which have hitherto been either overlooked or treated as insufficient. His studies have led him to conclude that these superficial confusions and displacements are brought about by a movement amongst the earth-masses themselves, which, as a rule, beginning with some local slip of the beds, becomes eventually converted into a movement of the whole. The motion of the earth-masses, now rolling, now gliding, can only be compared to the flow of a mud-stream or that of a glacier. After the author's paper was written, he became aware that he had been preceded in his general conclusions by Mr. R. Mallet, whose paper in the Journal of the Dublin Geological Society ("Some Remarks on the Movements of Post-tertiary and other discontinuous Masses," vol. v. p. 121) will no doubt be known to many of our readers. It is not likely, however, that glacialists will ever be got to believe that their boulder-clays, &c., and scratched rock-surfaces have been produced by the continuous or intermittent slipping of loose material which is in daily progress around all the existing coasts. The other papers in this number are, "The Mountain-land of South Glina in Croatia," by Dr. Emil Tietze, and "On the so-called gas-shales of Nyran and their flora," by von Ottokar Feistmantel. Number 4 opens with the second part of Professor Hochstetter's interesting paper "On the Geology of the eastern parts of European Turkey." This part is accompanied by a geological map of Central Turkey, which shows the distribution of the rock-masses, while several diagram sections scattered through the paper enable us to understand more clearly their succession and relative position. Amongst primary rocks the author enumerates gneiss, amphibolgneiss, mica-schist, talc-schist, phyllite, granite, syenite, amphibolite, serpentine, and crystalline limestone. Under the mesozoic division, he gives red sandstone, quartzite, and conglomerate, which he considers to be of Triassic age, and compact limestone and dolomite, which may be either of Triassic or Jurassic age, or both. Above these come deposits of chalk and marl of middle cretaceous age. The tertiary and quaternary deposits consist of miocene lacustrine beds with lignites, post-miocene diluvium or fluviatile gravels, and alluvium. Amongst eruptive rocks he enumerates quartz-porphyry, augite-porphyry, pyroxenic tuff and conglomerate, trachyte, trachyte-conglomerate, pumaceous tuff, &c. The only other geological paper in this number is an explanation of Sheet iv. (East Carpathia) of the Geological Survey's map of the Austro-Hungarian empire. Both numbers of the *Jahrbuch* are accompanied by the usual mineralogical communications, which contain a number of papers, amongst them one by von Johann Rumpf upon "Kaluszite," a new mineral, the chemical formula of which is given as $CaK_2 (SO_4)_2 +$ aq. or in another way as $CaO, SO_3 + KO, SO_3 +$ aq. An illustrative plate accompanies the description.—Prof. Tschermak furnishes some account of the meteorites in the imperial mineralogical collection up to October 1872, giving a table that shows in a condensed form the names of the places where the meteorites were found, the hour of the day, the day of the month, and the year in which the stone fell, &c.—Special mention must also be made of a paper by Fuchs on the Island of Ischia, which is geological and historical, as well as petrological—a paper which will well repay perusal by those who are engaged in the study of igneous geology.

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 Largest Amphipod.—Willemoesia (Deidamia).

IN a paper which was read at the Royal Society this year, I described the anatomy of a female amphipod caught in the Atlantic, and remarkable for its large size and the absence of the second pair of antennæ. This female had a length of 84 mm., not of 14 mm., as has been stated in NATURE and in other periodicals which have reprinted my abstract from the Proc. Roy. Soc. We have since also caught males of this interesting amphipod, which were still larger, more than 3 in. long. A description of these has been added to the above-mentioned paper, so that now the anatomy of both sexes will be known. This amphipod, which, as we have discovered, lives on the surface, is, thus, by far the largest one known. Some figures representing the male and parts of the mouth, which at first could not be dissected, and therefore not well be seen, will appear in a larger paper on some of the remarkable deep-sea and other crustacea caught during the *Challenger's* cruise in the Atlantic.

This amphipod, however, which was supposed to be new, and to which I gave the name of *Thaumops pellucida*, has been already described by Guérin-Méneville under the name of *Cystosoma neptuni*. The distinguished French naturalist has described this species from a single specimen caught in the Indian Ocean. This I found out only when I got Mr. Spence Bates's catalogue of amphipods sent out to me, in which the original figure has been reproduced. The first description of this species seems, however, to be so incomplete, that some additional knowledge about its structure will be welcome, I hope, to zoologists.

The geographical distribution of certain amphipods seems to be a very wide one, for we have not only caught several specimens of Cystosoma in the Atlantic, but also a species of the genus Oxycephalus, which hitherto seems to have been found in no other but the Indian Ocean.

With regard to *Willemoesia* (*Deidamia*), Mr. Grote has been kind enough to point out in vol. viii. p. 485 of NATURE, that the name Deidamia has been used already for a genus of Sphingidæ, by Dr. Clemens, and honoured me by calling the two blind crustacea, which are so closely allied to the fossil Engonidæ, by our family name; I am very much obliged to the curator of the Buffalo Museum for this information, and will always be glad, during the time of our cruise, to receive communications of this kind. For though we have a good library on board, mistakes like these cannot always be avoided, when it is necessary to give a name to those animals which I describe, not because they are new, but because they furnish interesting additions to our knowledge of the morphology of lower animals.

R. V. WILLEMOES-SUHM

H.M.S. *Challenger*, Simons Bay, Cape of Good Hope, Nov. 24, 1873

Physiological Effects of Ozone

LONG before Schönbein discovered ozone, electricians who had been in the habit of employing Franklinic or statical electricity as a therapeutic agent, had discovered that the electrical aura, as they termed it, or the current of air proceeding from an electrified point, possessed decided physiological properties, and the effect appeared to be the same whether, on the single fluid hypothesis, the electrical current or breeze passed from the point to the animal surface, or *vice versâ*. The physiological effects principally noticed were the power of this breeze to allay chronic inflammatory actions in delicate organs, such as inflamed eyes, or to relieve pain arising from a decayed tooth; but they were most remarkable in the curative effect produced on obstinate ulcers, when the electrified current or aura was daily thrown upon their surfaces for some minutes. The *rationale* of this process was not understood, and electricians were contented to accept the facts without being able to explain them.

About 45 years ago I employed Franklinic electricity as a collateral branch of my electrical pursuits pretty extensively as a therapeutic agent, and had abundant opportunities of noticing these physiological actions. In addition to these a fact gradually developed itself during the course of my electrical investigations, namely the effect which an electrified atmosphere had upon the mucous lining of the throat and bronchial tubes. It was no uncommon thing, after a day's continuous use of an electrical machine in a close room, to feel a considerable amount of irritation over the respiratory tract very similar to that experienced when recovering from an attack of influenza, and I found that I could often produce the same irritation by removing the prime conductor from the cylinder of an electrical machine and holding my face in such a position as to breathe the copious ramifications of electricity that were thrown off from it.

These effects are all now referable to the development of ozone, and the interesting experiments of Mr. Dewar and Dr. M'Kendrick, recorded in NATURE (vol. ix. p. 104) open up a field of inquiry, the extent and importance of which can scarcely be estimated. Of late years ozone has, by a kind of *post hoc propter hoc* reasoning, been designated the scavenger of the atmosphere, since raging epidemics have been suddenly checked in their course after the occurrence of a good rattling thunderstorm, and hence the old notion, not without good foundation, that lightning clears the air. So much importance has been attached to the supposed value of this antiseptic agent, that not a few, and myself amongst the number, have recommended various forms of apparatus for the development of ozone within the precincts of fever hospitals, but the experiments of Mr. Dewar and Dr. M'Kendrick seem to show that there is a limit beyond which it would not be prudent to ozonise an atmosphere destined for respiratory processes.

The further investigation of its physiological effects will therefore be looked forward to with no small interest. The examination of the subject, however, must not end with its effects upon animal physiology.

From experiments which I have made on the extraordinary electrical conditions which are suddenly induced in an atmosphere forming the extended di-electric of a thunderstorm, I can trace an intimate relation between the copious development of ozone and a corresponding effect upon delicate vegetable organisms, which may lead to the discovery of the proximate causes of blight so frequently the accompaniment of thunderstorms. Some years ago I extended a small atmospheric exploring wire between my own house and the cupola of a chapel 400 ft. off. One end of this wire was brought into my study, and connected with an electrical battery containing about 12 square feet of internal surface; a discharging apparatus, which also served the purpose of a lightning-conductor to the ground, was connected with this wire. The changes which this wire indicated as occurring in the stratum of atmosphere with which it was in contact, were of a most extraordinary character.

Simultaneously with the occurrence of a flash of lightning, even at a mile distance (the battery being disconnected), a torrent of sparks rushed between the exploding balls, presenting the appearance of a thick bundle of brilliant sparks, with a noise similar to that of suddenly breaking a hard fibrous stick. When the battery was in connection with the atmospheric wire, the quantity of electricity brought in by it was sufficient to charge and discharge the battery, over an interval of a quarter of an inch several times so rapidly, that it was impossible to count the discharges, the cracking noise being correspondingly loud. This effect is now accurately imitated by the bundle of sparks passing between the terminals of a powerful induction coil, having an electrical battery connected with it. When it is considered that the earth's surface in immediate connection with this electrified stratum is acted upon electrically by it, one can easily see the influences which such actions are likely to produce upon the delicate vegetable organisms which have not only to act as conductors in these electrical changes, but are exposed to the actions of a sudden development of an enormous amount of ozone. It will therefore be an interesting matter to know how vegetable life will be influenced by an ozonised atmosphere, especially as the conditions necessary for artificial experiments will not be difficult to obtain.

Plymouth, Dec. 29, 1873 J. N. HEARDER

Photographing the Transit of Venus

THE following is the result of some experiments recently made on photographic irradiation :—

If, as is generally supposed, photographic irradiation is caused by the reflection of light from the back surface of the plate, then photographs taken on non-actinic coloured glass ought to be free from irradiation, because the light would be quenched in the glass, and therefore no reflection could take place. Photographs of a model transit were taken on yellow, orange, and red glasses; but in all cases the irradiation was nearly as bad on the coloured glasses as on the clear glass.

Photographic irradiation may possibly be caused, either by the bright light producing an intense state of chemical activity, which has the power of spreading itself, or what seems more probable, the parts of the collodion on which the bright light is falling become luminous, and reflect light to the surrounding parts of the sensitive film, and thus extend the chemical change in each side of the true optical boundary line. If this is the explanation, then we can correct photographic irradiation by allowing only sufficient light to fall on the plate to produce the necessary chemical change, so that there shall be no surplus to be reflected; or we may make the sensitive film of such a nature that it cannot reflect the actinic ray. There are two ways of carrying out the first of these plans. We may either "stop" down the lens by means of a diaphragm, or we may pass the light through a non-actinic coloured screen. The first should be the best plan, but was not found practicable with the Dallmeyer "triplet" lens used in the experiments. Screens of glass and coloured solutions were then tried, and photographs of the model transit taken perfectly free from irradiation, and not to be distinguished from photographs of the model taken against a dull sky, which required 15 seconds' exposure. Experiments were then made to make the sensitive film incapable of reflecting actinic rays. This was done by adding red aniline to the collodion, till the colour was found by experiment to be deep enough. Photographs taken in this way were also quite free from irradiation. After the photographs were developed and fixed in the usual way, they were treated with chlorine gas, which destroyed the red colour and left the photographs on a clear film.

Ocular irradiation is also, in all probability, in part caused by the reflection of light in the eye. But in addition to this cause there is another of considerable importance—namely the "persistence of the image" combined with the unconscious motion of the eye—as the impression received by the brain is not only that of the light on the part of the retina where the image at the time is, but also that of where it was a short time before, the mental impression must therefore be larger than the image on the retina. Ocular irradiation can also in all probability be corrected, by reducing the amount of light falling on the eye, to the minimum necessary to give a distinct impression. The reflection in the eye will then be less. The image not being so bright will not "persist" so long—and the light not being so brilliant, the stimulus to the unconscious motion of the eye will not be so great. Diaphragms will of course be preferred for this purpose. When screens are used it is probable that neutral tinted ones will be found to suit best.

JOHN AITKEN

The New Marine Animal

IN NATURE, vol. viii. p. 488, under the heading "New Marine Animal from Washington Territory," Mr. P. L. Sclater announces the description by Mr. Stearns of the *Verrillia blakei*, the long-sought-for owner of the wand-like rod named by Gray, *Osteocella septentrionale*.

I write to say that the nationality of the Polyp is altogether British; Burrard's Inlet—the only place it has yet been found—is in British Columbia, close to the north mouth of the Fraser, and the first description of it would have been British too, but for unavoidable postal delays in the transmission of my paper, the receipt of which by the Zoological Society Mr. Sclater mentions. EDWARD L. MOSS

Royal Naval Hospital, Esquimalt, B.C., Nov. 26, 1873

The Potato Disease

IN NATURE, vol. ix. p. 161, it is stated by Mr. W. G. Smith that the bodies referred by Dr. Montague to Artotrogus are possibly no other than *Volutella ciliata*. Nothing can be more common on decaying potatoes than *V. ciliata*, but I can state most positively that Montague's fungus, whatever its nature may really be, had nothing to do with *V. ciliata*. It is very important that attention should not be drawn off from Dr. Montague's, or rather Dr. Rayer's curious observation by a supposition which is entirely without foundation. A reference to the figures in the *Journal of the Horticultural Society* (vol. i. tab. 4, figs. 27, 28, 29), and the characters of Artotrogus, apart from the specimens submitted to myself, and the occurrence within the cellular tissue, ought to be quite sufficient.

Jan. 3 M. J. BERKELEY

Specific Gravity of Sea-water

IN Prof. Wyville Thomson's work "The Depths of the Sea" there appears to me a curious discrepancy between two statements of the specific gravity of the sea, to which it may be useful to direct general attention. At p. 505, Mr. W. L. Carpenter states that the average specific gravity of surface-water, at a sufficient distance from land to be unaffected by local disturbances, was 1·02779. At p. 513, Dr. Frankland gives the specific gravity of four samples of surface-water, the mean of which is only 1·0267, even less than the minimum value as given by Mr. Carpenter. Both results are said to be for temperature 60° F. I should have expected Dr. Frankland's determination to have been the higher, from possible loss by evaporation. The difference may probably be due to want of identity of indication between the instruments used. From whatever cause it may arise, the difference is so considerable, as to leave no doubt whatever that it ought to be accounted for in some way; and the error wherever it lies fully exposed. R. STRACHAN

Meteorological Office

Optical Phenomenon

A SHORT time ago I was lying, during the heat of the day, in a darkened room in a house at one of the hottest stations in India. There was a great glare of sunlight outside. All at once I became aware of figures moving about on the opposite wall. On examination they proved to be the inverted images of the servants of the establishment who were walking about in the performance of their several duties in the gravelled court-yard outside the house. The white colour of their clothes, the dark colour of their skin, and the red colour of their sashes or turbans, were distinctly reproduced, and every servant was recognisable without difficulty. The images were produced by rays passing through three or four holes in the Venetian shutters; and while they all remained open there was a large penumbra round the images, but on closing all but one hole, this was very much reduced. The holes were of the size of a shilling or half-crown, and made in an outer door as well as the shutter, having been constructed to admit of a punkah rope passing through. The explanation appears to be this:—The sun was above and slightly behind the house. The solar rays falling on the objects in the court-yard were transmitted through the shutter holes. There being no other light in the room, and the rays being strongly scattered by the rough whitewashed wall, the rays were sufficiently powerful to produce an image on the retina of an observer in whatever part of the room he might be; the room became, as it were, the box of a large camera.

On intercepting the rays with a smooth oval looking-glass, they were not, of course, scattered, and no image was visible on the glass, but the image could be reflected from the looking-glass to any part of the wall which contained the shutters through which the rays passed. The appearance produced when a servant was made to stand in the required position, was singular. A full-length (inverted) coloured figure appeared in an oval frame of bright white light, much larger, of course, than the looking-glass. The white light was produced by the glare from the gravelled yard, shadows on which were reproduced.

A dog-cart and horse were imaged on the wall most clearly, the chesnut colour of the horse being very distinct. The whole phenomenon was always producible at any time when the sun was in the proper position above the house.

Are not mirages of one class, *i.e.* the appearance of inverted images in clouds, produced in a similar way? The rays from a figure might pass through an opening in one cloud to the face of another otherwise uniliuminated, and be thence scattered. I believe I have seen this explanation given somewhere, but I cannot remember where.

E. C. BUCK

N. W. P., India

ON TEMPERATURE CYCLES *

SINCE the discovery of an eleven years' period in the phenomena of solar spots, several corresponding periods (it is now well known) have been demonstrated in terrestrial phenomena, more especially in those of magnetism, auroras, cyclones, and rainfall. With regard to weather changes, it has been thought by Dove, that the tracking of a cycle in these could not, theoretically, be made an object of research; and that while some indications of a periodicity might appear, a great part of the complicated changes named must be, from the nature of the case, quite unperiodical. The series of observations by Dove on the subject led him to the conclusion (1) that divergences from the normal, especially those of temperature, are not local, but spread over large surfaces; but (2) that negative divergences, in one region of the earth, are compensated by positive in another; and conversely. That the compensation is perfect, and that the quantity of heat annually given by the sun is constant, has been affirmed also by Maury and others.

The data on which this conclusion is based are limited. They appeared quite insufficient to a German physicist, Dr. W. Köppen, who has recently been led to undertake a wider investigation of the subject. He has communicated to the Austrian Society for Meteorology a preliminary notice of his inquiries and results (*Zeitschrift*, Aug. and Sept. 1873), which will be found of considerable value.

We may first note here his materials and method. He furnishes a long list of places from which observations (more or less extensive) have been had; and in his first table he gives the divergences of temperature of individual years (1820—71) from the average temperature, and for the following regions: India, Tropical America, Temperate South America, South Africa, Australia, China, and Japan, Mediterranean region, Southern United States, Western U.S., Western Central Europe, Austria, South Russia, South-West Siberia, East Siberia, Central part of U.S., Atlantic Stat s, British Islands, North Germany and Netherlands, North-West Russia, North-East Russia and Ural, North-West America, North East America, Iceland, Northern part of Europe. [The particular towns, &c., are given, and the author's purpose partly is, that the list may be supplemented by other series of observations (which he has not been able to see), being sent to the Central Physical Observatory at St. Petersburg, where he has chiefly been prosecuting this research.] The periods of observation ranged from three to thirty years; the average was taken from several years' observations. In many observation-series, the yearly average had to be calculated for the first time. Series of six years' length were the shortest admitted, and such short series only by way of completing the longer. The original sources of Prof. Dove's material were consulted.

A second table shows the divergences of temperature in various regions for the years 1768—1819. By way of condensing, a third table is given, in which the material from 1820—71 is arranged in five series, one of which represents the tropics, and the four others four successive ex-tropical zones. The zones are not bounded by determinate parallels of latitude, but it was sought to combine approximately equal material of observation and earth surface.

On comparison of the curves of Table III. with the sun-spot curves (according to Wolf), a striking correspondence at once appears, as far as the year 1854. In the tropics, the maximum of heat occurs $\frac{1}{2}$—$1\frac{1}{2}$ years *before* the spot-minimum; in the ex-tropical zones, on the other hand, it occurs *after* the minimum; in some cases (in the forties, *e.g.*) as much as three years after. The regularity and extent of the variations diminish from the tropics to the poles.

It is further noticeable that as the interval from maximum to minimum of the spots is always greater than that from minimum to maximum, a corresponding inequality occurs in the temperature changes.

On these results Dr. Köppen remarks that, while there is evidently some connection between the two kinds of phenomena, the sun-spots do not act directly by darkening a part of the solar disc; for, as the temperature of the earth's surface is a function of the solar radiation, the change in the former must follow that in the latter; but the opposite occurs, as we have seen, in the tropics. It is probable that the temperature of the sun's surface is (from some unknown cause), at its highest one or two years before the minimum of the sun-spots. That the spots (if we suppose them to be solid bodies) take so long to melt that their minimum only occurs after the maximum temperature of the earth's surface, is not remarkable, considering their size.

If we consider the period 1800—71, we find a section of about 40 years, with marked periodic variation, 1815—54, and two periods, before and after, showing great disturbances, (say) 1792—1815, and 1854—66. Whether in 1865 we have again entered (as the curve would seem to indicate) on a time of distinct periodic variation, will doubtless appear in the next ten years.

The observations before 1800, again, show such anomalies in the temperature, that we should almost doubt the existence of connection with the sun-spots were it not for the convincing evidence of the years 1815—54. We find all possible cases, from complete indifference of the temperature in contemporaneous change of the sun-spots (1750—71), and a short correspondence of both (1772—77), to a well-marked and regular variation of temperature (1777—90), which stands to the sun-spot curve, in exactly the opposite relation to that found in 1816—54. True, the observations here are only from a small fraction of the earth (West Europe and the New England States); but the continuance of the same curve shows the normal variation in 1816—54 quite distinctly. The estimation of the spots previous to 1826 is somewhat arbitrary, but an error such as that the maximum is put in the place of the minimum cannot be supposed. And lastly, if it be urged that the turning points

* Abstract of paper by Dr. W. Koppen in the Austrian *Zeitschrift fur Meteorologie.*

of the temperature curve (1779 maximum and 1785 minimum) are precisely where, according to the mean length of the sun-spot period of 11·1 years, they must be; that there may, perhaps, be an 11 years period in the temperature independent of the sun-spot period, and that, in the present case, a displacement which the spot period has experienced is not shared by the temperature period; we have to remember that the correspondence of the temperature changes in 1815—54, does not merely extend to the average length of the periods, but that all peculiarities and disturbances in the sun-spot curve are, in these 30 or 40 years, reflected in the temperature curve. Further observation is needed to explain this phenomenon. Possibly (the author suggests), we have here the interference of a number of quite independent periodical actions; and (without laying stress on the fact, in default of causal evidence), he notices that the greatest negative anomalies occur, for a considerable time, in a series which progresses by multiples of 9, and in such a manner that an interval of 27 alternates with one of 18 years. Thus—

$$1740 = 1767 = 1785 = 1812 = 1830 = 1857$$
$$+27 \quad +18 \quad +27 \quad +18 \quad +27$$

The first four agree; there is merely the quite isolated cold year 1794 intermediate. Going further, we find divergence; for the table shows a strong negative anomaly about 1836; but we have, again, the well-authenticated negative anomaly of 1856—57 conforming to the rule. Renou has assigned, for the return of the cold winter of south-western Europe, a period of 41 years; the author asks whether the time 27 + 18 = 45 years does not better agree with the phenomenon. On this view, the first winter, reckoning back from 1740 is 1695, and this is recorded as having been one of excessive cold. Between these two occurs one winter of extraordinary cold, 1709, but it is quite isolated, the neighbouring years having been warm. If we go still further back, the periodicity cannot be ascertained with any certainty. If the rule is correct, and its validity between 1740 and 1857 not a mere accident, *i.e.* the expression of quite other laws, we have to look for a very cold year in 1875 (being 1857 + 18).

Dr. Köppen proposes, in a future communication, to treat of hydro-meteors, and to examine the influence of periodic weather changes (at several years' interval) on some phenomena of organic nature.

LAVOISIER'S WORK IN THE FOUNDATION OF THE METRIC SYSTEM

SINCE the publication of the article on the Metric System, in NATURE, vol. viii. p. 386, my attention has been drawn to some recent information showing the important part taken by the celebrated Lavoisier in the scientific operations for establishing the basis of the metric system of weights and measures in France. Lavoisier's name has hitherto been little noticed amongst those of the men of science who were prominently engaged in this work; but it is now clearly proved that up to the period of his being guillotined on May 8, 1794, when he fell a victim to the revolutionary fury during the reign of terror, no one took a more active or serviceable part in the scientific labours for founding the Metric System than Lavoisier.

This information is contained in a "Notice historique sur le Système Metrique," by General Morin, lately published in the "Annales du Conservatoire des Arts et Métiers." It is derived from original documents left by Lavoisier, and now in the possession of the Académie des Sciences. These documents have since been submitted to my inspection by M. Dumas, and full details of them will soon be given to the world in the fifth volume of the works of Lavoisier, which M. Dumas is now completing.

Although Lavoisier's name does not appear in the list of the original Committee of Weights and Measures in France, yet it is shown that he was very actively engaged in making the arrangements for their meetings and in preparing the minutes of their proceedings, as appears from papers and letters in his own handwriting. It was through his personal agency that funds were provided at Paris for continuing the measurement of the arc of the meridian in Spain by Méchain. And more particularly, all the actual comparisons for determining the length and dilatation of the standard measures used by Méchain and Delambre for measuring the basis, and known as the *Règles de Borda*, were made, not by Borda, but by Lavoisier. The subsequent computations only were made by Borda. Lalande has expressly stated that the work of preparing them was executed by Lavoisier and Borda, but that the construction of the measures of platinum and brass, forming metallic thermometers, and of the comparing apparatus used, was carried out under Lavoisier's directions. The published report upon the construction and verification of these measures in 1792 is contained in the "Base du Système Metrique," vol. iii. p. 313. It was drawn up by Borda, but Lavoisier's name is not mentioned in it.

Another very important part of the work, the determination of the weight of a cubic decimetre of water, was carried out, in the first instance, chiefly by Lavoisier. This branch of the operation had been specially entrusted by the Committee to Lavoisier and Haüy. The necessary apparatus was constructed under Lavoisier's directions, and all the requisite measurements and weighings of the cylinder were made by Lavoisier and Haüy. Hitherto few details of the actual processes of this scientific determination have been given to the public, and the whole credit of determining the weight of a cubic decimetre of water, upon which the kilogram, the unit of metric weight, was based, has been attributed to Lefèvre-Gineau, to whom, in conjunction with Fabbroni, the work was entrusted after Lavoisier's death. In point of fact, Lefèvre-Gineau appears to have repeated, in the winter of 1798-9, all the observations made by Lavoisier and Haüy five years before, using the same instruments and obtaining nearly similar results.

The facts are stated as follows by Bugge, the Danish member of the Commission, in the thirtieth of his letters describing his visit to Paris, and published in 1800:—

"The final results of the labours of this special commission, consisting of Lefevre-Gineau and Fabbroni, to whom Van Swinden and Trallès were afterwards joined), was that the true kilogram, the weight of a cubic decimetre of water at its maximum density, or at 4° C., was 18,827 French grains of the old French pound, *poids de marc.*

"By the laws of August 1, 1763, and April 7, 1795, the kilogram is determined to be 18841 grains of the old French pound, *poids de marc*, in accordance with the experiments of Lavoisier and Haüy. This determination was adopted by the Chief Office of Weights and Measures in France, and the Standards have been hitherto made for the Departments accordingly. So that there now exist two kinds of kilograms, the legal or provisional, and the scientific or true kilogram. The difference between them is fourteen old French grains."

The difference is partly attributable to Lavoisier's determination having been made at the temperature of melting ice, instead of that of the maximum density of water adopted for Lefevre-Gineau's determination. The unit of Metric weight, the Kilogramme des Archives, appears to have been based on the later observations of Lefevre-Gineau, and to have been legalised by the law of Dec. 9, 1799, after Bugge's letter was written.

H. W. CHISHOLM

THE COMMON FROG*

VIII.

THE skeleton of the ankle as developed in the frog's class presents us with some characters, which, more than even those of the wrist, suggest the passage of the line of affinity directly from Batrachians to mammals, leaving both reptiles and birds on one side.

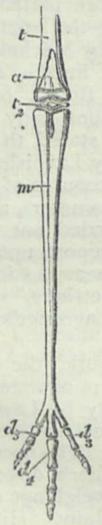


FIG. 51.

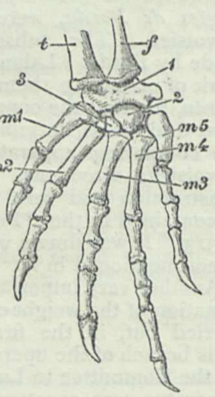


FIG. 52.

FIG. 51.—Right foot of Emeu. *a*, astragalus; d_2—d_4, second, third, and fourth digits; *m*, metatarsals anchylosed together except at their distal ends; *t*, tibia; t_2, distal tarsal element.

FIG. 52.—Left foot of a Monitor Lizard (*Varanus*). *f*, fibula; m^1—m^5, the five metatarsals, m^1 being that of the hallux; *t*, tibia; 1, astragalo-calcaneum; 2, cuboides; 3, ecto-cuneiforme.

In the first place we meet in the frog with certain extra ossicles in the inner side of the foot, which present the appearance of a broad rudiment of an extra digit on the inner side of the great toe. Now we find a structure very

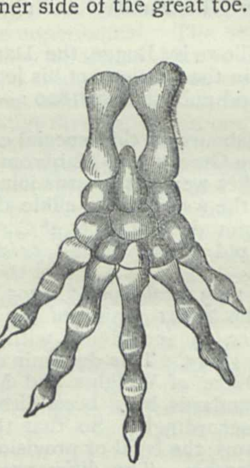


FIG. 53.

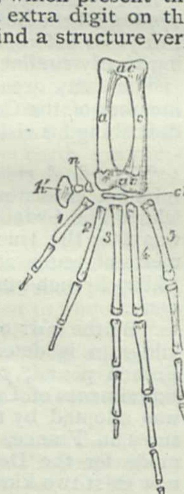


FIG. 54.

FIG. 53.—Skeleton of posterior extremity of an eft.

FIG. 53.—Bones of foot of Frog.—*a*, astragalus; *c*, os calcis; *ac*, united portions of these bones; *li*, extra ossicle of inner side of foot; *cb*, ossicle representing cuboid and other tarsal bones—1, 2, 3, 4, 5—the five metatarsals.

similar in form in animals remote enough from Batrachians, yet rarely do we find such in any intermediate kinds. Thus in certain tree-porcupines the ankle is furnished in like manner—another instance of the independent origin of strikingly similar structures.

* Continued from p. 150.

There are other matters, however, more important than this. It has been remarked that the wrist shows an

FIG. 55.—The Maholi Galago.

amount of resemblance to the same part in beasts which

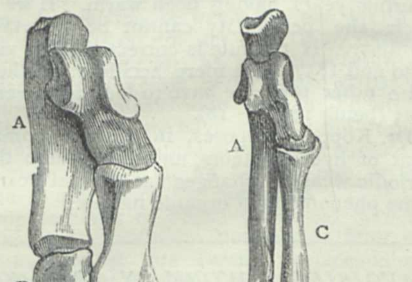


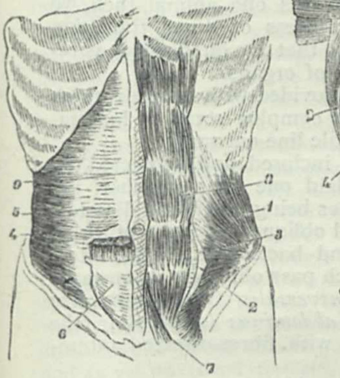
FIG. 56.—Elongated tarsus of Lemuroids. Left-hand figure, tarsus of *Cheirogaleus*; right-hand figure, tarsus of Tarsius. *A*. calcaneum; *B*. cuboides; *C*, naviculare.

is wanting in most reptiles and in all birds. The same

FIG. 57.—Tadpole of Bull Frog, partly dissected, to show the muscles of the tail and the branches of the 8th nerve or the *vagus*. *a*, great lateral branch giving off—*b*, a dorsal branch, and *c*, the lateral branch (or *nervus lateralis*); *d*, branches descending and passing along the branchial arches. The descending branches seen behind the branchial nerves on the side of the belly are not branches of the vagus at all, but spinal nerves, which come out from beneath the muscles and pass down under the *nervus lateralis*, and without having any communication with it.

observation may be repeated with far greater force as regards the ankle.

In all beasts, as in man, the motion of the leg on the foot takes place by means of a joint between the shin-bone of the leg and the small bones of the ankle; and though in some beasts (as in the orang) there is considerable power of motion between the first and the second row of ankle bones; this is small compared with the mobility of the foot and ankle taken together, upon the leg



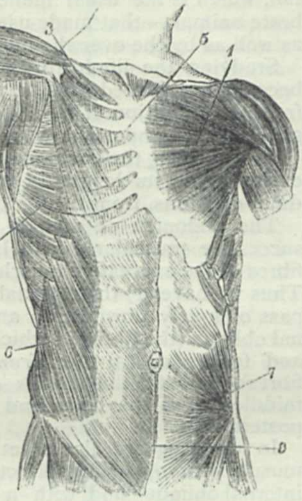


FIG. 58. FIG. 59.

FIG. 58.—Anterior muscles of the Trunk: the pectoralis major of the right side and the left external oblique being removed. 1, pectoralis major; 2, pectoralis minor; 3, subclavius; 4, serratus magnus; 5, internal intercostals; 6, external oblique; 7, internal oblique; 8, linea alba.

FIG. 59.—Deeper Abdominal Muscles—the external oblique being removed from the left side of the body, and the internal oblique and part of the rectus also, from its right side. 1, the internal oblique; its outer tendon (2) is cut and reflected from the outside of the rectus to show its deeper tendon (3), which passes within the rectus except towards the pubis; 4, transversalis; 5, its fascia; 6, sheath of the rectus—near the pubis, the conjoined aponeuroses of the abdominal muscles pass in front of that muscle; 7, pyramidalis; 8, rectus of left side, showing the tendinous intervals, or *lineæ transversæ*.

In all birds, on the contrary, not only is there no motion between the ankle-bones (as a whole) and the shin-bone, but the two rows of ankle-bones actually anchylose respectively with adjacent parts—the row nearer the leg coming to form one with the shin-bone; the second row coming to form one with the bones of the foot. Thus in birds the motion of the foot on the leg takes place not

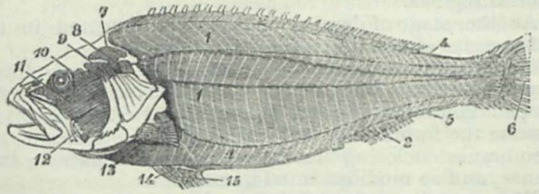


FIG. 60.—Superficial Muscles of the Perch. The fin-rays of all the fins are cut off. 1, great lateral muscle, showing the numerous vertical tendinous intersections slightly but variously inflected; 2, small superficial muscles inserted into the fin-rays of the dorsal and ventral fins; slender longitudinal muscle running (in the interval of the summits of the two great lateral muscles) between the dorsal and caudal fins; 5, similar muscle on the ventral margin, which also appears between the anal and ventral fins; 6, small radiating muscles of the caudal fin; 7, part of the great lateral muscle inserted into the skull; 8 and 9, elevators of the operculum; 10, elevator of the palato-quadrate arch; 11 and 12, muscular mass by which its contraction closes the jaws; 13, superficial muscles of the pectoral fin; 14 and 15, muscles of the ventral fin.

between the ankle and the shin-bone but between the two rows of ankle-bones.

The same thing to a less degree takes place in reptiles; the ankle-bones do not indeed *anchylose* with the shin-bone and foot respectively, but they nevertheless unite with those parts so firmly that motion takes place between the bones of the ankle and not between the whole ankle and the leg.

Now in the frog's class, *e.g.* in the order *Urodela*, we meet with a condition which is mammalian rather than reptilian or avian. Motion takes place freely between the leg and the whole tarsus. Moreover, the number and proportions of the ankle-bones themselves far more closely agree with the condition of the same parts exhi-

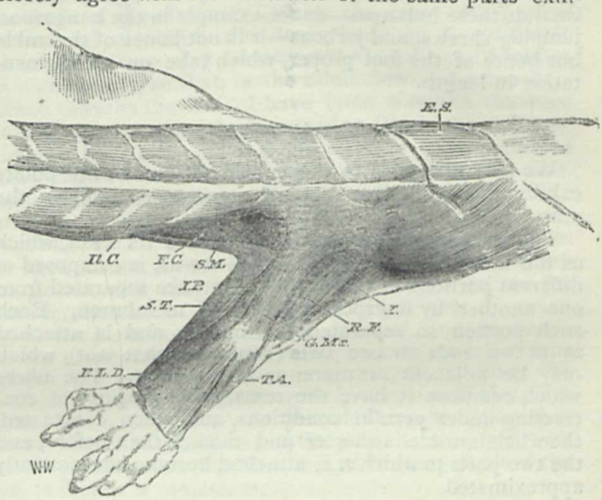


FIG. 61.—Superficial Muscles of Extensor Side of Leg and of parts of Trunk and Tail of Menopoma. *ES*, erector spinæ—directly continued into dorsal half of tail; *ELD*, extensor longus digitorum pedis; *FC*, femoro-caudal; *GMx*, probably rectus femoris; *I*, muscle resembling iliacus; *ILC*, ilio-caudal; *IP*, ilio-peroneal; *RF*, part of great extensor of thigh; *SM* and *ST*, muscles like the semi-membranosus and semi-tendinosus.

bited to us by certain beasts than it does with that which is possessed by any bird or of most reptiles.

The frogs and toads, however, differ from the *Urodela* and present us with a peculiar condition of the ankle-bones, in that the two which represent the bones of the first row are so greatly elongated as to give to the limb an additional segment—as it were two "long bones" more.

We should search in vain through every other order of the Batrachian class, through every known group of birds and reptiles, both living or fossil, to find any analogous structure. None of the lowest mammals, no marsupial, no rodent, no insectivorous or carnivorous beast, no hoofed mammal, presents us with anything of the kind. Nevertheless, at almost the other end of the series, in the very

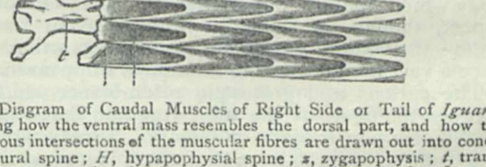


FIG. 62.—Diagram of Caudal Muscles of Right Side of Tail of *Iguana*, showing how the ventral mass resembles the dorsal part, and how the tendinous intersections of the muscular fibres are drawn out into cones. *N*, neural spine; *H*, hypapophysial spine; *z*, zygapophysis; *t*, transverse process; 1, dorsal series of cones; 2, upper lateral series of cones; 3, lower series of cones; ventral series of cones.

highest order, that to which man himself belongs, we actually find a very similar development.

Amongst the very peculiar beasts which inhabit the island of Madagascar, there are certain small creatures, "Half-Apes," belonging to the genus *Cheirogaleus*, in which two of the ankle-bones are elongated in a manner similar to that of the frog. The same character is more marked in an African genus of half-apes (*Galago*), and still more so in a third half-ape (*Tarsius*), from the island of Banca. Now it is absolutely impossible to believe that a special genetic affinity connects together by a peculiarly common descent, Half-Apes and Frogs! We are then driven to the conclusion that we have here again a striking similarity of structure in two instances which are quite independent in their origin.

That the power of rapid and prolonged "jumping" does not carry with it as a necessary consequence the elongation of ankle-bones, is demonstrated by the fact that in other animals which, to say the very least, jump no less than do these half-apes—as for example in the kangaroos, jumping shrews, and jerboas—it is not bones of the ankle but bones of the foot proper, which take on an augmentation in length.

The Muscles of the Frog

We may now pass to the consideration of some points exhibited by another set of structures—namely, the muscles.

The muscles of an animal constitute its flesh, which as the most ordinary inspection shows us, is composed of different portions of soft fibrous substance separated from one another by interposed layers of membrane. Each such portion, so separated, is a muscle, and is attached at its two ends to two parts (bones or what not), which may be adjacent or more or less distant. The fibres which compose it have the remarkable property of contracting under certain conditions, and, when contracted, the whole muscle is shorter and thicker than before, and the two parts to which it is attached become consequently approximated.

Muscles may be large expanded sheets of flesh (as in the abdomen) or long and more or less narrow, as in the limbs.

Muscles are said to be "inserted," or to "take origin from" the parts to which they are attached, and they may be so inserted either by their own muscular fibres or by the intervention of a tough membrane or a dense fibrous cord called a "tendon."

All the motions of an animal are produced by means of the contractions of its muscles pulling the bones, which act as so many levers (of different kinds according to circumstances), and so effecting locomotion.

These muscular contractions are in life produced by the agency of certain of the nerves proceeding from the nervous centres, *i.e.* from the brain and spinal marrow, and which carry an influence outwards to the muscles. Other of the nerves so proceeding convey an influence inwards to the nervous centres from an irritated portion of the body's surface.

The muscles, however, especially in the frog may, for a time, be made to contract after death by direct irritation of the nerves themselves.

After the skeleton, it is the muscular formation of the body which mainly determines its general form and aspect, though occasionally—and often in the Frog's order—the voluntary inflation of the lungs will alone produce a vast modification in an animal's appearance.

The curious and grotesque resemblance which exists between the figure of the adult frog and that of man has been a common subject of remark. It may then be less surprising to some to learn that there is a great degree of resemblance between the muscles of the Rational and of the Batrachian animals; though the much greater gulf which separates the Batrachian than the Reptilian class from mammals may lead others to anticipate a greater divergence than in fact exists.

The frog, however, in its immature stage of existence, is widely different from the adult in its muscular (or myological) furniture, and this from one obvious reason.

"Muscles" are, as we have shown, *par excellence*, "organs of motion," and the motions of the tadpole are essentially different from those of the frog.

The frog, as all know, progresses on land by jumps, and swims through the water by a series of movements which are in fact aquatic jumps. This action is familar to many of us, not only from observation but also by imitation (the frog being a swimming-master given us by nature), but it is none the less a mode of swimming which is very exceptional indeed.

The tadpole progresses through the water in a very different manner, namely, by lateral undulations of its tail, which is the usual mode of swimming among vertebrate animals—that made use of by sharks and porpoises, as well as by the overwhelming majority of fishes.

Studying the life-history of this one animal, then, we become acquainted with a process of direct transition from the condition of a fish to that of a quadruped, as regards a most important group of organs.

In ourselves, the back is provided with muscles which extend along its length in a complex series of longitudinal divisions, from the middle line outwards.

The abdomen of man is inclosed and protected by successive muscular layers laid one upon another, the fibres of the successive muscles being differently directed. Thus we have (1) the external oblique (the fibres of which pass obliquely downwards and backwards, (2) the internal oblique (the fibres of which pass obliquely downwards and forwards), (3) the *Transversalis* (with transverse fibres), and (4) the *Rectus abdominus* (situated in the middle line of the body, and with fibres directed antero-posteriorly).

In the frog we also meet with the vast sheets of muscle with oppositely directed fibres (the external and internal oblique) and with a median, antero-posteriorily directed rectus muscle.

A very different condition exists in fishes, where there is indeed a median antero-posteriorly directed rectus, but where the abdomen and tail are encased with a mass of muscular fibres not arranged in superimposed sheets, but as a series of narrow segments separated from each other by layers of membrane. The edges of these membranous layers, when the skin is removed, appear as a successive series of undulating lines proceeding from the back to the belly.

Now the tadpole exhibits a muscular condition (Fig. 56) quite similar to that of the fish, and in the great persistent larva the axolotl, we find no truly oblique abdominal muscles, but only as it were a hypertrophied rectus.

In other species of the frog's class which retains a tail throughout life, the marked superimposed lamellæ are distinctly developed, but more or less distinct traces are also retained of the successive membranous partitions separating the muscular segments of both the dorsal and ventral regions.

Another stage of development may be detected in the tail-muscles of certain reptiles.

Here the membranous partitions have become drawn at short intervals from above downwards out into a formal shaped condition, so that the muscular fibres enclosed, assume the forms of cones. Moreover, the apices of the membranes enclosing the cones, become denser in substance, and so modified into ligaments.

We come thus to have a key to the process of development, by which the muscles of the back may be conceived to have arisen.

The muscles of the back may be conceived as having arisen through increasing obliquity, conical prolongation, and partial detachment (from muscle) of the separating membranous lamellæ; the produced ends becoming condensed with firm tendons directed more or less obliquely forwards.

The muscles of the abdomen may be conceived as having arisen through atrophy, in that region, of the separating membranes and subsequent splitting up of the muscular mass into super-imposed sheets of differently-directed fibres.

This filiation between piscine and mammalian myology could hardly have been detected but for the remarkable series of gradations which the frog's class exhibits—

gradations both between species, and between different ages and conditions of one and the same species.

ST. GEORGE MIVART

(*To be continued.*)

BEES VISITING FLOWERS

ON the cliffs at Llwyngwril, near Barmouth, *Lathyrus sylvestris* grows in large patches, and is freely visited by humble-bees. Where a plant grows in considerable masses, a great number of bees are naturally attracted, and the competition among them becomes severe. In this case the flowers are not sucked in the usual manner, but the bees bite holes through the corolla, and obtain in this way illegitimate access to the honey. Hermann Müller has shown that when flowers grow in any quantity, they are so diligently worked at by the bees that only comparatively a few contain any nectar; it is therefore important for the bees to find out as quickly as possible whether a flower is worth anything or not. These holes, bitten through the corolla, enable the bees to visit the flowers more quickly, and are thus a great saving of time. He also says that, although the bee which first gnaws the hole loses time in doing so, yet the advantage of being able to get the honey from the young and as yet unvisited flowers, fully makes up for the loss of time.

In *L. sylvestris*, as in many Leguminosæ, the honey is secreted within a nectary formed by the filaments of nine of the stamens soldered together. The trough-like cavity thus formed is covered in above and converted into a tube, by the tenth stamen. But at the base, where the trough enlarges into a bulb, the stamen is not wide enough to cover it, so that it leaves a pair of holes piercing the tube one on each side. It is through these "nectar-holes," as they are called, that the bee, after passing its proboscis down the tube of the corolla, or, as in the case already mentioned, through the holes bitten at its base, gains entrance to the staminal tube, in its search for nectar.

In *L. sylvestris* the hole is gnawed through the tube of the vexillum, close to the edge of the calyx, and exactly over the left nectar-hole. (Throughout this paper I mean the right and left of an observer looking at the front of the flower.) I think the reason of this constant choice of the left side of the corolla is that the left nectar-hole is usually somewhat larger than the right. I found this to be the case in sixteen out of twenty-four specimens of the wild *L. sylvestris*, and in eleven out of sixteen in the garden variety (the Everlasting Pea). It is difficult to say how the bees have acquired this habit. Whether they have discovered the inequality in the size of the nectar-holes in sucking the flowers in the proper way, and have then utilised this knowledge in determining where to gnaw the hole; or whether they have found out the best situation by biting through the vexillum at various points, and have afterwards remembered its situation in visiting other flowers. But in either case they show a remarkable power of making use of what they have learnt by experience.

The united filaments not only form the nectary, but also a sort of casing in which the ovary is enclosed; and out of which the growing pod has to break its way as it increases in size. In *Vicia cracca* it does so by lifting up the tenth stamen, but in most *Lathyri* the filament is too stiff to allow of such a movement, and the growing pod was to squeeze its way between it and the edge of the trough formed by the nine united filaments. In doing this it enlarges and at last splits open one of the nectar-holes. In *L. sylvestris* the left nectar-hole, usually the larger of the two as I have before said, is almost always the one which is thus opened. In *L. pratensis*, on the other hand, where the nectar-holes are equal, the pod emerges indifferently to the right or left of the tenth stamen.

I am inclined to believe that the want of symmetry in the growth of the pod and the inequality in the size of the nectar-holes are in some way correlated, and that both are connected with a third asymmetrical character in the flower of this species. In most Lathyri the brush of hairs on the pistil is directed straight backwards towards the stalk of the flower. This is the case with *L. pratensis*, and also with the flower-buds of *L. sylvestris*, while very young; but, as they get older, the pistil rotates on its own axis, so that, in the adult flower, the brush is turned towards the left. I have often watched the bees sucking the flowers of the Everlasting Pea in the ordinary way, and have observed that the pistil, in consequence of being slightly bent as well as twisted on its own axis, emerges from the keel on the right side of the bee. The function of the brush is, as Mr. Farrer has shown (NATURE, vol. vi. p. 479, 1872), to sweep the pollen out of the keel, so that it may be transferred to the bees visiting the flower, and may be in this way subservient to the cross-fertilisation of the species. I believe that the twisting of the pistil helps to ensure this end, since in consequence of the brush being turned towards the left it rubs against the bee and smears it with pollen. Thus the mechanism for ensuring the cross-fertilisation of the plant is made more complete. At present the supposition that the asymmetrical character of the pistil is connected with the above described peculiarities and in the growth of the pod, is merely a conjecture.

These facts have a certain bearing on a peculiarity in the structure of the staminal tube in *Phaseolus multiflorus*, the Scarlet-runner. This flower, in common with many Leguminosæ, has a pair of nectar-holes at the base of its staminal tube; but the tenth stamen differs, as far as I know, from that of any other Leguminous plant, in possessing a little flap which projects from its upper surface just in front of the nectar-holes, and which almost completely blocks up the tube of the corolla. Mr. Farrer supposes (*loc. cit.* p. 480) that by pressing with its proboscis against this flap the bee levers up the tenth stamen, and in this way passes its trunk into the staminal tube. If this occurs at all, it must be like gnawing holes in the corolla, an illegitimate way of treating the flower, since it is impossible to believe that it should have well developed, but totally useless, nectar-holes. I believe the true function of this curious little flap to be as follows:—In many Papilionaceæ, Lathyrus for instance, the insect visiting the flower rests on a platform which is formed of the carina and the expanded alæ, but in the Scarlet-runner this platform is made up by the alæ alone, the carina being tightly coiled into a spinal close up to the entrance to the tube to the corolla. The alæ are attached, one on each side to the proximal part of the carina, so that when an insect rests on them, its weight bears on the carina, and causes the pistil which is contained in it as in a sheath to be forced out. The direction of movement of the pistil is downward and to the left, so that a bee resting on the expanded alæ and pushing in its head to the left of the coiled-up carina would come in contact with the pistil as it darted out of its sheath; but if the insect went to the right of the coil it would escape the pistil altogether. The end of the pistil is covered with hairs, and performs the same function as the brush in Lathyrus in smearing the bee with pollen. It is, therefore, of great importance for the cross-fertilisation of the plant that the bees should go to the left of the coil. As a matter of fact they all but invariably do go to the left; the very few bees that I have seen going to the right appear dissatisfied and unable to find their way into the corolla. Now to reach the nectar-holes the insect's proboscis has to pass down a tunnel formed above by the tube of the vexillum, below by the upper surface of the tenth stamen; the entrance into this tunnel is a narrow archway leaning towards the left, *i.e.* having its highest point to the left of the middle point of its base. As before stated, the flap almost blocks up the tunnel, so that to get to the nectar-holes the proboscis must pass over the top of the flap, and must therefore travel along the highest part of the tunnel, but since at the entrance arch the highest point is to the left, the bee finds it necessary to go to the left of the coiled-up carina to reach the nectar-holes in the easiest way. If this view of the function of the flap, when considered in relation with the disposition of the pistil, carina, &c., be correct, it adds another instance to the long list of mechanisms for ensuring the cross-fertilisation of flowers by means of the visits of insects. FRANCIS DARWIN

THE FRENCH MUSEUM OF PHYSICAL AND MECHANICAL SCIENCE

THE following official report of General Morin, the director of the Conservatoire des Arts-et-Métiers, Paris, to the Minister of Agriculture and Commerce, which we take from the *Journal of the Society of Arts*, furnishes some interesting details as to the present condition of this magnificent educational establishment, the like of which, dealing as it does with experimental and mechanical science, is entirely wanting in our country, although in the British Museum, the student of Natural History finds all he needs.

"The total number of persons who attended the lectures of the fourteen professors amounted in 1872 to 135,443, at 559 lectures, or in the proportion of 241 to each lecture. The smallest number of lessons given by any one professor was 40, from the opening in the commencement of November, until the last days of April. The total number of persons attending is smaller than in preceding years, which is explained by the decrease of the floating population of Paris. This year, as in all others, the decrease commenced when the days got longer, and work kept the people in the workshop.

"I would here limit this report if I did not think it necessary to add a few words upon the means of instruction which the Conservatoire offers to the public and the working-classes of all ranks.

"This establishment, as is known, owes its origin to the illustrious Vaucanson, inspector of factories, who, after having made at the Hotel du Montagne, Rue de Charonne, a collection of machines, instruments and tools, for the instruction of workmen, presented it to the Government, on the sole condition that its original purpose should be maintained. Louis XVI. accepted the gift by an act of council, and the illustrious Vandermonde, member of the Academy of Sciences, was named administrator and conservator of this first industrial museum. Later, by the decrees of the 15th and 18th of August, 1793, the Convention created a temporary commission of arts, to put a stop to the dispersion of objects of art, science, and industry. This commission succeeded in collecting a large number in a depot formed at the Hotel d'Aiquillon, Rue de l'Université. The value of these collections soon after determined the Convention, upon the report of Gregory, to make a decree, the 19 Vendémaire, year 3, that there should be formed in Paris, under the name of Conservatoire des Arts-et-Métiers, a public collection of machines, models, tools, drawings, descriptions, and books of all kinds of arts and science, the use of which should be explained by three lecturers attached to the establishment.

"It may be well to mention that the title of 'demonstrateur' or lecturer, often corresponded to that of professor, and that the professors of the Jardin des Plantes remained long after they had commenced giving regular courses. However that may be, the organisation of the Conservatoire, which was checked by several circumstances, was again mooted by Alquier at the Council of the Ancients, on the 27th Nivose, year 7, which urged the great advantage of such an institution to workmen, by saying that it is of more use showing them articles than merely speaking of them. It was not, however, until the 12th Germinal, year 7, that the buildings of the priory of St. Nicholas of the Fields were put into the possession of the members of the Conservatoire, who were then composed of Le Roy, Conti, Molard, and Benvelot, designer. The names of these savants, and that of Montgolfier, who soon after replaced Le Roy, did not allow of any comparison being made between the functions of these lecturers and those who are differently named now-a-days.

"At length, in the year 8, all the models and machines belonging to the State were definitively removed to this building, and formed collections destined solely for the instruction at sight. The functions implied by the title of lecturer were never exercised, and this will easily be believed when it is said that the numerous visitors who are attracted by the rich collections sometimes amount to 200,000, which makes all verbal explanation on the spot impossible. But that which is not possible to do for the public has been for a long time afforded by the Conservatoire to persons who are really desirous of information. A complete and methodical catalogue has been made out and published, and to it are added, from time to time, all new acquisitions; this has already passed through four editions. The galleries have been systematically classified, a guide has been placed in each, who, if he cannot give any practical explanation, can at least show where such and such a model is to be found, each of which is ticketed and numbered, both in the catalogue and in the inventory. Should an engineer or a workman wish to examine separately a machine or machinery, a study card for the necessary time is given to him. Or should any more complete information or explanation be required, either the curator of the collections, the under-director, or the director, is always ready to furnish them, their office being freely open to all.

"The staff in charge of the collections consists of the conservator, an assistant conservator, and of fourteen chosen guardians, who, for the most part, are picked from old non-commissioned officers or soldiers. The wish to give explanations by these, even with the aid of written details for the 9,000 models or articles which are there, would lead to great errors and confusion by a zealous but a badly instructed staff. In asking that popular conferences, such as are held at the Polytechnic Institution of London, should be introduced here, account has not been taken of the great difficulties which stand in the way, and greatly exaggerated ideas exist as to their value.

"It is not by common and vulgar explanations that the principles of Science can be spread amongst our workmen, and the facts and experience which are so necessary; their minds and intelligence are developed enough, so no fear need be had to speak to them on difficult scientific questions, if it is done with wisdom.

"All the professors who have followed this mode of teaching have often been convinced, on meeting some of their old hearers in workshops, that what may be termed the knowledge of truth and scientific principle has more deeply entered into their minds than into that of scholars of more celebrated schools. Hence it was not without reason that, in 1819, a decree of the king, brought about by the respected Dean, M. le Baron Charles Dupin, added to the instructions at sight given by the collections, that of oral instruction in the amphitheatres, by professors chosen from among the ranks of science. The number of chairs, at first only three, is now fourteen, and the half of the professors are members of the Institute, who diffuse and popularise science, the progress of which they promote by their labours. This instruction, unique of its kind in Europe, only takes place during winter; it is free to all without any condition for admission or any examination, and the number of persons who have frequented it during the last few years amounts to from 150,000 to 180,000.

"To the honour of workmen it must be said, that a more attentive audience can nowhere be found; never does the slightest disorder arise, and I am happy to say that during the unhappy events which have taken place in France, the Conservatoire was always respected, and underwent no disturbance or invasion.

"But if we think the part of casual lecturers in the galleries useless, and if we are convinced that the real duty of the Conservatoire des-Arts-et-Métiers consists in the classification, maintenance. and increase of its collections, and in the teaching of the applied sciences, which it gives on such a large scale, we also believe that the Government should attach great importance to that teaching, which, during twenty years, we have developed under the name of technical education, and which has produced such good results in several of our great industrial centres.

"Your department pursues the realisation of this wish, and we hope it will be able, with the aid of the resources placed at its disposal by the National Assembly, to develop more and more this practical instruction, which, beginning at the primary school, gradually enables men, according to their intelligence and love of study, to rise from the lowest to the highest grades of society."

NOTES

WE have with much regret to record the death of Mr. Edward Blyth, on December 27 last, in his sixty-fourth year. Of Mr. Blyth it may be said that he was a Zoologist in the truest sense of the word, and his practical knowledge of the birds and mammals of India and the surrounding countries was probably greater than that of any living naturalist. Up till 1840 he devoted himself to the study of the ornithology of the British Isles, and in that year appeared an English translation of Cuvier's "Regne Animale," in which the mammals, birds, and reptiles were edited by him; many of his own notes suggesting modifications in the then existing systems of classification, have been subsequently fully substantiated and adopted. For twenty-two years after this date Mr. Blyth held the post of Curator to the Calcutta Museum of the Asiatic Society of Bengal, during which time, and in conjunction with Dr. Jerdon, he did more than anyone to advance the study of Natural History in India, and to improve the value of the collection he controlled. After a short visit to Burmah, during which he did much good to zoological work, he returned to England in 1863, since which time he has contributed many valuable papers to ornithological and other journals, and under the very appropriate signature "Zoophilus," a large number of excellent articles to the *Field*. With an unparalleled memory Mr. Blyth combined exceptional powers of observation and a genuine enthusiasm for natural history, which is but rarely seen; these made his impromptu observations and opinions of more than ordinary value, and no one was more willing than himself freely to give all information at his command, towards the assistance of any fellow-worker, or the elucidation of any difficulty in his favourite subject.

DR. FRANCIS C. WEBB, editor of the *Medical Times* and *Gazette*, died suddenly on the morning of December 24 last, at the age of 47 years.

AT a preliminary meeting of the Varley Testimonial Committee, held on November 20, it was resolved to recommend that a Memoir of the late Cornelius Varley, illustrated with a Photographic Portrait, should be prepared and issued under the superintendence of the Committee, and that a copy be presented to his family, in token of the high estimation in which he was held; and further, that some Memorial be erected to his memory at the place of his interment.

TELEGRAMS from Naples of the 3rd and 4th inst., state that Prof. Palmieri announces a severe eruption of Vesuvius to be imminent. A rumbling noise is audible from the mountain, and although fire has not been seen in the interior of the craters, the density of the smoke indicates the proximity of fiery matter.

MR. MANLEY HOPKINS, Consul-General at Hawaii, having written to the *Times* that he had discovered in the Samoan Islands a living specimen of the Dodo, believed to have been extinct a century ago, Prof. Owen wrote to the same paper that the bird referred to is the dodlet. "The extinct dodo of the Island of Mauritius was about six times bulkier. Coloured figures of both birds—that of the dodo, copied from paintings by the Dutch artists, who saw the living bird in the time of their Stadtholder Maurice, that of the dodlet from the bird living in the Zoological Gardens about ten years ago, with the skeletons of both didus and didunculus are given in my work on the Dodo (quarto)."

A VERY suggestive anatomical point has been made out by Sir Victor Brooke, respecting the tarsus in certain of the *Cervidæ*. He finds that in the species of the genus *Cervulus* (the Muntjacs), the tarsus, instead of consisting of a naviculo-cuboid bone, together with two separate cuneiform bones, has the outer of the two cuneiform masses anchylosed to the naviculo-cuboid mass, to form a single bone, leaving the minute internal cuneiform free. In a very young specimen of *Cervulus muntjac* the cuboid was free, and the naviculare anchylosed to the outer cuneiform bones, showing that the tendency to blend in this direction is greater than that of the naviculare and the cuboid to combine. This same peculiarity is also found in the Pudu Deer of South America.

THE question as to the limit of capability of the microscope is investigated by Prof. Abbe, of Jena, in a recent number of Max. Schultze's *Archiv*; and he is led by a series of physical deductions to the remarkable result, that this limit is already as good as reached by our best microscopes, and that all hope of a deeper penetration into the material constitution of things, than such microscopes now afford, must be dismissed. Experiment and theory agree in showing how the changes wrought by diffraction of light passing through fine structures, whose elements are so small and near each other as to call forth this phenomenon, are such as to prevent the object being imaged *more geometrico*. Thus it may happen, on the one hand, that different structures give the same microscopical image, and, on the other, that like structures give different images. Consequently, while objects of the kind (systems of fine lines and the like) may appear ever so distinct and well marked in the microscope, we are not entitled to regard such appearances as of morphological significance, but merely as physical phenomena, from which nothing further can certainly be inferred than the presence of such structural conditions as are capable of producing the diffraction effects obtained. The remark has notable applications to many of the microscopical researches on markings of diatoms, and on striated muscular fibre. And it affects not merely the morphological relations of the objects, but the deductions, made from microscopical observation, as to properties (such as differences of transparence, colours, polarisation, &c.). The author lays down the following principle as basis for determination of a limit:—By no microscope can parts be distinguished (or the marks (*Merkmale*) of a really present structure perceived), if they are so near to each other that the first bundle of light rays produced by diffraction can no longer enter the objective simultaneously with the undiffracted cone of light. Prof. Abbe has also recently described a new illuminating apparatus for the microscope, formed of a condensing system of two unachromatic lenses, which are fixed in the stage of the microscope, and transmit the rays from the mirror below; the purpose being that the object (immediately above the upper lens) may be illuminated by light from a great many different directions.

WE have received from the Science and Art Department "Extracts from the Reports of the Professional Examiners in Science on the Examinations of May 1873." The reports are most thorough and painstaking analyses of the results of the examinations, and give one the conviction that there is little chance for candidates who are not masters of the subjects they profess. On the whole the examiners report unfavourably on the great bulk of the candidates, and it would be for the good of future candidates, especially such as expect to be able to pass by cramming, that these reports should be brought under their notice.

AN observation of particular interest has been made by Mr. C. S. Tomes (*Quarterly Journal of Microscopical Science*). In studying the development of the teeth of the Armadillo (*Tatusia peba*), he finds, contrary to what would have been expected, that in their earliest stages, the first indications of their differentiation are manifested by the formation of an "enamel organ" as in those of higher mammals; whereas in the teeth themselves there is no enamel present, as is well known. Another peculiarity is that behind each primitive tooth a second smaller sac is seen, which corresponds in all its relations with the germ of the permanent tooth in other mammalia. Consequently, *Tatusia peba* at least, amongst the *Dasypodidæ*, is not monophyodont, as has been previously stated by Rapp, Gervais, and Flower, from which it may be inferred that the Edentata as an order, must have descended from a truly diphyodont type, and have become subsequently specialised.

THE U.S. Exploring Expedition under Lieut.-Col. Wheeler has now brought its work nearly to a close, having surveyed about 100,000 miles of territory in Southern Colorado, Southern Utah, Eastern Arizona, and Western New Mexico. The entire geological formation of all this vast country has been carefully studied, and from this study and the survey, accurate maps will be drawn this winter. Over 1,000 species of plants have been collected in Southern Colorado, and over 500 in Arizona and New Mexico. Some plants, supposed to have medical properties, or such as might prove of technical use, have been taken along for investigation. For example, the muskal, which is used by the Indians as a principal article of food, is tasteless when raw, but upon cooking, by being embedded in hot coals, turns sweet and is like the best honey. Attention was also paid to the geographical distribution of plants, and many interesting points elucidated. With regard to the fauna, more than 800 bird skins have been collected and stuffed by one of the naturalists, many of them very rare and beautiful. Some very rare reptiles have also been obtained, among which the gilamonster forms a peculiar feature. This animal, which is repulsive in appearance to many, and generally believed to be exceedingly poisonous, is quite harmless and very interesting. Some new species of rattlesnakes have been found. Very few butterflies have been seen, but bugs and beetles were collected in great quantities. The waters of the different streams were searched for the finny tribe. Skulls of bears and mountain lions and other specimens of the animal creation are included in the list of collections. With regard to the Indians, many interesting facts have been collected; among others the vocabularies of seven languages—the Apache, Navajoe, Tehua, Gohun (Tonta Apaches), Waltoa (Jemes), Isletta, and Moquis.

WE notice with considerable satisfaction from the statistics published in the *Publishers' Circular* that the number of scientific works issued during the past year in England bears a very large proportion to other classes and to the whole number of works published. The number of new books and new editions published during 1873, including 242 importations from America, is 4,991. Of these, 588 are classed under the head "Arts, Sciences, and Historical Works," by which, we presume, is meant Science theoretical and applied and the history of Science, as there are other heads under which history and the fine arts more appropriately come. This number, 588, is inferior only to that of works of fiction, and theological and religious works, the former numbering 831, and the latter, second in the list for the first time, 770. Were we to class "Voyages, Travels, and Geographical Research," 283 volumes, among scientific works, the number would be 871, exceeding even that of works of fiction, not to mention theology. The number of *new* books in Arts, Science, and Geography is 593.

DR. E. REGEL, Director of the Botanical Gardens, St. Petersburg, has published a work on the species of vines met with in North America, Northern China, and Japan, in which he discusses the long-controverted question of the origin of the vine. According to him, the cultivated vine, which forms our vineyards and produces our wines, is not a distinct and separate botanic species; it is a hybrid of two species, belonging equally to the genus Vine, viz. *V. labrusca* L., and *V. vulpina* L. The former of these two species is met with in a wild state in Northern America, in Japan, in Mantchuria, and in the Himalayas. Its leaves have their inferior face covered abundantly with a cotton-like down. The second species, which grows naturally in the same countries, has upon the inferior face of its leaves only small hairs, short and very stiff upon the nerves. The first of these two species has furnished the two most remarkable varieties of American vines, the *Catawba*, much cultivated for the production of wine, and the *Isabella*, the grape of which, sought after for the table, has a perfumed flavour and peculiar odour, agreeable to some, but disagreeable to others.

ON Thursday evening last, by invitation of the Committee of the Post Office Library, a large company assembled in the galleries of the new Post Office buildings, St. Martin's-le-Grand, in commemoration of the reopening of the library. In the south-west gallery there was arranged a museum of early telegraphic instruments and appliances, the latest improvements in the science of telegraphy being illustrated by the mode of transmitting news to, and receiving messages simultaneously from, nineteen of the larger towns of the kingdom. The new process of despatching messages simultaneously in opposite directions through a single wire by the instrumentality of Mr. Stearn's invention was worked throughout the evening, communication having been effected for the purpose with Southampton. In the central gallery there were wires working in direct communication with Australia, India, Teheran, America, St. Petersburg, Paris, and Berlin, the process being rivalled in interest by the action of the pneumatic tubes which connect the Central Telegraph Station with the principal offices for the collection and delivery of messages in the metropolis. There was also in this gallery a working model of the travelling post-office, with the apparatus for the receipt and delivery of the mails while the train is in motion. In the course of the evening the Postmaster-General briefly addressed the company, sketching in outline the history of the English postal service. The Post Office Library was founded in 1859 for the benefit of the clerks and other officers of the Post Office. It was started by subscription among the employées, but has received large donations of books from authors, publishers, and the public. The library contains at present 2,500 volumes, of which we are glad to hear a fair proportion consists of popular scientific works, which it is hoped will be shortly increased.

MESSRS. LOVELL, REEVE, AND CO., have in the press a volume on St. Helena, comprising a physical, historical, and topographical description of the island, with its geology, fauna, flora, and meteorology. The author is Mr. J. C. Melliss, C.E., F.G.S., F.L.S., late Commissioner of Crown Lands, Surveyor and Engineer of the Colony.

THE *Scotsman* reports that a piece of gold-bearing quartz has been found in the island of Bute.

BEE-KEEPING has become a vocation or avocation of so much importance in America that there actually exists a "North American Bee-keepers' Society," which, like more important associations, meets yearly in one of the towns of the States. This year the society met at Louisville and continued its sittings for several days. Among the papers read was one by General D. L. Adair against the practice, common among apiarians, of clipping the wings of the queen, the paper showing a very considerable acquaintance with the structure of the bee.

THE *Times* takes the following from an American paper, and asks "Why not in London?":—"In Pittsburg, Pennsylvania, an electric clock has been established to move the hands of seventy different clocks, scattered all over the city. The motive clock is powerful, and has a pendulum composed of hollow coils of copper wire. These swing to and fro over the poles of horseshoe magnets, and every time they pass from one pole to the opposite a current of electricity is called up inductively in the coils, flows up the wire, and then to the seventy dials, giving a current of an opposite nature at each swing. Behind each dial is an astatic permanent magnet, suspended on a pivot, and surrounded by a coil of wire, and it rotates under the electric influence from the wires. A small weight may be used to each dial if the hands are heavy, and the pivoted magnet may merely regulate the time. Of course every clock will be exactly alike, and will run with very little attention. To prevent the pendulum of the motive clock from moving too fast by the increase in the length of vibration of the pendulum, a magnetic bridling apparatus is attached."

A LETTER appears in the *Times* of the 30th instant, from a correspondent with the "Livingstone East Coast Expedition," dated Mdaburu, Ugogo, Central Africa, July 15, and is principally occupied with a description of the many annoyances to which the expedition was subjected.

THE finest kitchen garden in France is that of Versailles, which belongs to the State, and brings in a yearly revenue, taking good and bad years together, of about 20,000f. The Assembly has determined to apply this valuable property to the formation of a model market garden and school of horticulture. The details of the institution are not yet arranged, but it is presumed that it will be self-supporting, and that it will render valuable assistance in the development of horticultural science in France.

THE additions to the Zoological Society's Gardens during the past week include two Violaceous Plantain-cutters (*Musophaga violacea*) from W. Africa, purchased; two Senegal Touracous (*Corythaix persa*) from W. Africa, presented by Mr. Hawkins; two Chinese Storks (*Ciconia Boycei*) from China, presented by Mr. R. B. Boyce of Shanghai; a Grivet Monkey (*Cercopithecus lalandii*) from W. Africa, presented by Mrs. Couteam; a Coati (*Nasua nasica*) from S. America; three Derbian Screamers (*Chauna derbiana*) from Columbia; a Chinese Water Deer (*Hydropotes inermis*); a Common Otter (*Lutra vulgaris*), British, deposited; two Black-tailed Hawfinches (*Coccothaustes melanurus*) from China, purchased.

*ON THE SPECTRA OF COMETS**

THE spectrum-analytic method of examining the light from comets has only been applied hitherto to comets of weak light; yet the observations are fitted to extend considerably our knowledge of these objects. The spectra of all the comets that have been examined have consisted of a few bright lines or bands of light, and a very faint continuous spectrum. The chief part of the comet's light appears, accordingly, to be proper to it, and is probably from glowing gas, while the remaining portion is reflected sunlight.

Among the brightest comets which have appeared since the introduction of spectrum analysis are those of Brorsen (I. 1868) and Winnecke (II. 1868). The spectrum of the former consisted of three bright bands, whose position Huggins sought to determine with great accuracy; but he found no coincidence with the spectral lines of any terrestrial substance. The spectrum of Winnecke's comet, also examined by Huggins, was somewhat different, but similarly consisted of three bright bands (in addition to the continuous spectrum always present), which were sharply defined on the side nearest to the red end of the spectrum, but diffuse on the other. A comparison of the comet's spectrum with that of olefiant gas showed striking similarity between them; and Huggins was able to establish, with some certainty, a coincidence of the three bright bands. The expressed opinion that the material of this comet might be hydrocarbon found general acceptance; and the inference has been extended to other comets, so that it has been taken as demonstrated, that the comets are formed of hydrocarbons. (Dr. Zenker in *Astr. Nachr.* Nos. 1890 to 1893.)

I will now give a summary of all the observations known to me of cometary spectra, from which it will be seen how far the conclusion in question is warranted.

1. The first comet examined by spectrum-analysis is the Comet I. 1864. Donati found its spectrum to consist of three bright bands, which (if one may judge from the figure in *Astr. Nachr.* No. 1488) do *not* coincide with those of the hydrocarbon spectrum.

2. Huggins and Secchi observed Tempel's Comet I. 1866, and got from it a weak continuous spectrum, in which Secchi saw *three* bright lines, Huggins only *one* The line seen by both was the brightest, and situated in the middle between *b* and *F* of the solar spectrum; accordingly *no* coincidence with the hydrocarbon spectrum.

3. In the spectrum of Comet II. 1867, the continuous spectrum was relatively so strong that Huggins found it difficult to detect bright lines. "Once or twice," he says, "I suspected the presence of two or three bright lines, but of this observation I was not certain. The prismatic observation of this faint object, though imperfect, appears to show that this small comet is probably similar in physical structure to Comet I., 1866." In this case, again, probably no hydrocarbon.

4. Brorsen's Comet I. 1868, was observed by Huggins and Secchi. Both observed three zones of light; the middle one being brightest, and lying in the green; while its brightest part was somewhat less refrangible than the brightest line of the air spectrum (wave-length = 500·3 mill. millim.). From this observation, and the determination of the position of the other two faint bands, it appears that the comet spectrum was *neither* similar to that of *nitrogen*, nor to the *hydrocarbon spectrum*.

5. Winnecke's Comet II. 1868, was also observed by Huggins and Secchi. The measurements and direct comparisons of Huggins gave an agreement of the cometary spectrum with that of carbon in olefiant gas. From Secchi's measurements it appears, that the sharply defined side of the middle band (towards the red end), nearly coincided with the line-group *b* of the solar spectrum; at which part also the beginning of the middle band in the spectrum of hydrocarbons is situated.

6. Comet I. 1870 was observed by Wolf and Rayet; the spectrum consisted of three bright bands, whose position, however, was not accurately determined.

7. Comet I. 1871 was observed by Huggins and myself. Huggins found *three* bands, I only *two*. The measurements of the bands observed in common agree well; the spectrum appears to be identical with that of Brorsen's comet.

8. Comet III. 1871 (Encke) was observed by Huggins three days, by Young four, and by myself six; it showed, as usual, a spectrum of three bands. Huggins thought this agreed with

* Abstract of paper in Poggendorff's Annalen, by H. Vogel.

the hydrocarbon spectrum; while Young and I observed *no* such coincidence.

9. Comet IV. 1871 (Tuttle), examined only by me, gave a spectrum of three bands. Accurate measurement of their position showed *no* coincidence with the hydrocarbon spectrum.

Of these nine comets, there is only one (I. 1870) for which we have no observations as to the position of the bright bands. Of the remaining eight, the spectra of five (1, 2, 4, 7 and 9) have shown *no* agreement with the hydrocarbon spectrum. As regards the Comet II. 1867 the supposition is offered that its spectrum was similar to the spectrum named; as to Encke's Comet III. 1871, it remains uncertain in which class it is to be reckoned (Huggins' observations being at variance with those of Young and myself). There remains only the Comet II. 1868, for which Huggins' and Secchi's observations assert a probability of coincidence of the lines in its spectrum with those in the spectra of volatile hydrocarbons.

It thus appears a somewhat questionable view, that the comets consist of such matter; and we should, I think, content ourselves with the deduction, that a portion of the light emitted by the comet is its own light, and very probably from glowing gas. Perhaps a brighter comet may enable us to find out their nature more exactly, yet it seems to me extremely difficult to determine the nature of the glowing gas of the comet through a comparison of spectra from the electric spark in Geissler tubes; since there must be, in the comet, circumstances of pressure and temperature, which it is impossible for us to imitate, and through which, it is known, the spectrum undergoes great modifications.

Dr. Zenker has further asserted (*Astr. Nach. loc. cit.*) that "in the spectrum of Brorsen's comet, Huggins has recognised the bright line of nitrogen." This statement is incorrect; the observation having been, that the bright band situated in the green of the spectrum, had *nearly* the same position as the brightest line of the nebulæ, which, it is known, coincides with the double line of nitrogen. The band in the comet spectrum is a little displaced towards the red end; and this displacement could not be due to the motion of the comet, for, as Huggins pointed out, the latter was moving towards the earth, and the line would have been displaced towards the *violet*. At an earlier date, Huggins, observing the Comet I. 1866, gave out the opinion that the material forming it might be nitrogen; the spectrum appeared to consist of only *one* band of light, which nearly coincided with the brightest nitrogen line. But Secchi disproved this view, having observed three bands, and the weaker bands showing no coincidence with those of the nitrogen spectrum. The accurate measurements afterwards made by Huggins with the bright Brorsen comet, are of interest specially because they put it beyond doubt, that there is no connection between the spectrum of nitrogen and that of the comet.

Again, Dr. Zenker arrives at the conclusion that there must be water-vapour in the comets; since they have, according to Schmidt, a yellowish-red colour, and the sun's rays, when they pass through a considerable thickness of aqueous vapour, are coloured thus. But apart from the consideration that sunlight has a yellowish-red colour on passing through other vapours, as well as aqueous, I would remark, that we must take the proper light of the comet, which appears from spectral analytic observations, to be generally more intense than the reflected light, as determining its colour. According to the observations made, we should expect that the comet is, on the whole, of greenish or greenish-blue colour, since all the spectra consist, as we have seen, of two or three bands of light, of which one is in the yellow, the second and brightest in the green, and the weakest in the beginning of the blue. Of the (generally very faint) continuous spectrum, only the brightest part—yellow, green, and commencement of blue—is visible. The entire image, therefore, even where the weak continuous spectrum appears, will seem of greenish colour. Colour-data have been furnished by other observers besides Schmidt; and the head of the Comet 1811, *e.g.* had, according to Herschel, a greenish or bluish-green colour; the nucleus was slightly red. The colour of Halley's comet, at its return in 1825, was a bluish-green (Struve). Winnecke says of the comet of 1862, "The colour of the neck appears to me yellowish; the coma has bluish light."

With regard, lastly, to Dr. Zenker's proposition that "every gas belonging to the solar system, as soon as it is visible on the dark ground of the heavens, must appear with the same lines of the spectrum, as, according to its nature, it absorbs out of the sunlight," I may be permitted to remark that I am not quite convinced of this; there is not yet furnished a satisfactory experimental basis for the assertion. But to seek to explain the line spectrum of a nebula thus, and by saying that the nebula is shone upon by a fixed star in its "near neighbourhood," is doubtful, inasmuch as it is a very rare case that bright stars are situated in such nearness to nebulæ (especially the planetary, which best show the gas spectrum), that one can suppose a physical connection between them and the nebulæ.

I have been prompted to the foregoing remarks by the observation that in recent speculations on the constitution of the universe, the value of perceptions of sense, on which these speculations rest, has been greatly over-estimated. The principles on which the edifice of an hypothesis is raised must, above all, be secure, and observations not sufficiently confirmed, or even denoted as uncertain by those who have made them, should preliminarily be disregarded, if it is desired that the hypothesis have a stimulating and furthering influence on the progress of scientific research.

SCIENTIFIC SERIALS

Justus Liebig's Annalen der Chemie. Band 169, Heft 3.—This number of the Annalen contains the following papers:—On the decomposition of nitric acid by heat, by L. Cairus. This paper, upwards of seventy pages in length, deals exhaustively with the subject. Very numerous tables of the results of various conditions of temperature, &c., are given, and the paper is illustrated with two plates.—On the chlorides of molybdenum, by Dr. L. P. Liechti and B. Kempe.—Chlorides of the formulæ $MoCl_2$, $MoCl_3$, $MoCl_4$, and $MoCl_5$ are described. The authors point out the parallelism shown by these bodies to the Tungsten chlorides, where, however, Tungsten wants the corresponding trichloride, while molybdenum wants the hexachloride. In both these series the colours of the salts become darker as the chlorine increases in quantity.—On the atomic weight of molybdenum, by L. Meyer. The author from sixteen results deduces the atomic weight 95·86 for molybdenum, chlorine being taken as 35·37 and silver 107·66. This agrees very well with the result obtained by Dumas 96, and by Debray 95·94. The author also points out the following relations in three groups of elements:—

V	51·2	Cr	52·4	Cu	63·3
Plus	43		43·2		44·4
Nb	94	Mo	95·6	Ag	107·7
Plus	88		88·4		88·5
Ta	182	W	184·0	Au	196·2

On chromic dioxide, by E. Hintz. The author describes the preparation, &c., of this body.—The number concludes with a paper on sulpho-ortho-toluidinic acid, by F. Gerver, and one on the specific heat of zirconium silicon, and boron, by W. G. Mixter and E. S. Dand.

THE new number of the *Quarterly Journal of Microscopical Science* contains many papers of interest. Prof. Allman commences by giving an account of Kleinenberg's researches on the anatomy and development of Hydra, in which, while he has confirmed many of the statements of former observers, he has shown the incorrectness of others, and has discovered several important points in its anatomy, specially in connection with the structure of the ectodermic layer, and the subject of development.—Prof. Martin Duncan records some observations on the method of development in *Fucus vesiculosus*, in which, after suggesting that they obtain their nutrition in part at least, from the organic matter always present in sea-water, he describes the growth of the terminal cells of two sets of finger-shaped processes; showing that by in-growths from the lateral walls, membranous septa are formed at the apices of the processes, an active mass of protoplasm occupying the extreme end.—Following this is a translation, with a plate illustrating it, of George O. Sars' paper on the anatomy of that aberrant form *Rhabdopleura mirabilis* (M. Sars), so peculiar in combining a creeping stem in which is an axial cord; lateral cells in which the somites are free, except that a contractile cord binds each to the axial cord; a pair of tentacular arms; a differentiated alimentary canal, and a foot-like process between the alimentary orifices. Mr. E. R. Lankester, in a separate paper, very clearly shows, with the aid of some excellent diagrams, that this animal is a true molluscan form, intermediate between the Polyzoa and Mollusca, and not in reality related to the Hydrozoa as imagined by M. M. Sars.—Mr. Tomes' observations on the development of the teeth of the Armadillo are referred to in our Notes.—A translation follows of the researches of Ph. van Tieghern and G. Le Monnier, on the *Mucorini*, condensed from their memoirs in the

Annales des Sciences Naturelles. It will well repay the study of microscopists.—Rev. E. O'Meare continues his researches on the Diatomaceæ, describing the *Achnantheæ*, *Gomphonemeæ*, *Amphipleureæ* and their allies.—Dr. Bowditch, of Harvard University, gives a new method of injecting the Lymph Spaces in fasciæ, by stretching fascia over the neck of a bottle; and injecting in several places a turpentine solution of alcannine with the point of the syringe partially perforating the fascia; allowing the whole to dry, during which process the fluid penetrates the finest lymph spaces.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, Dec. 18, 1873.—"On the Nervous System of *Actinia*," Part I., by Prof. P. Martin Duncan, F.R.S.

After noticing the investigations of previous anatomists in the histology of the chromatophores, the work of Schneider and Rötteken on these supposed organs of special sense is examined and criticised.

Agreeing with Rötteken in his description, some further information is given respecting the nature of the bacillary layer and the minute anatomy of the elongated cells called "cones" by that author. The position and nature of the pigment-cells is pointed out, and the peculiarities of the tissues they environ also. It is shown that the large refractile cells, which, according to Rötteken, are situated between the bacilli and the cones, are not invariably in that position, but that bacilli, cones, and cells are often found separate. They are parts of the ectothelium, and when conjoined enable light to affect the nervous system more readily than when they are separate. Further information is given respecting the fusiform nerve-cells and small fibres noticed by Rötteken in the tissue beneath the cones, and the discovery of united ganglion-like cells, and a diffused plexiform arrangement of nerve is asserted. The probability of a continuous plexus round the *Actinia* and beneath each chromatophore is suggested, and the nature of the physiology of the structures in relation to light is explained.

The nature of the minute construction of the muscular fibres and their attached fibrous tissue in the base of *Actinia* is noticed; and the nervous system in that region is asserted to consist of a plexus beneath the endothelium, in which are fusiform cells and fibres like sympathetic nerve-fibrils. Moreover, between the muscular layers there is a continuation of this plexus, whose ultimate fibrils pass obliquely over the muscular fibres, and either dip between or are lost on them.

The other parts of the *Actinia* are under the examination of the author, but their details are not sufficiently advanced for publication. The nervous system, so far as it is examined, consists of isolated fusiform cells with small ends (Rötteken), and of fusiform and spherical cells which communicate with each other and with a diffused plexus. The plexus at the base is areolar, and its ultimate fibres are swollen here and there, the whole being of a pale grey colour.

Anthropological Institute, Dec. 30, 1873.—Prof. Busk, F.R.S., president, in the chair.—The following paper was read:—"Ethnological Data from the Annals of the Elder Han," Part I. Translated by A. Wylie, of Shanghai, with an introduction by H. H. Howorth. The Imperial Chinese Annals of the various Dynasties which are as yet almost untouched are distinguished by the extreme accuracy of their details, and in them is to be found a minute account of the intercourse of China with its neighbours, reaching back in contemporary annals to at least the second century B.C. The series of Chinese annals begins properly with those of the Han dynasty which reigned from about 202 B.C. to about 220 A.D. That was the golden prime of Chinese history, when the empire reached its furthest limits, when Buddhism was introduced and when a great literature flourished. During the dynasty of Cheou, the old imperial unity had been invaded by the creation of various feudatories who became almost independent. It was the aim of the immediate predecessors of the Han dynasty to destroy those feudatories and to restore the unity of the empire; and to effect that purpose all the ancient books and histories were ordered to be burnt. The annals, in the present communication, contain an account of the numerous conquests from the date of the Elder Han and embrace the history and migrations of a large portion of the peoples of Central and Eastern Asia. Mr. H. H. Howorth communicated the twelfth and concluding paper on the Westerly Drifting of Nomades: the Huns.

EDINBURGH

Royal Society, Jan. 5.—Prof. Sir William Thomson, president, in the chair.—The following communications were read:—A New Method of Determining the Material and Thermal Diffusivities of Fluids, by Sir William Thomson.—Continuants: A New Special Class of Determinants, by Thomas Muir, M.A.—Remarks upon the Foot-Prints of the Dinornis on the Sand Rock of Poverty Bay, New Zealand, and upon its recent Extinction, by T. H. Cockburn Hood.

DUBLIN

Royal Irish Academy, Nov. 29, 1873.—Prof. Jellett, president, in the chair.—Samuel Ferguson, LL.D., read a paper on "The completion of the biliteral key to the values of the Letters in the South British Ogham Alphabet."—The president read a paper on "The question of Chemical Equilibrium," the determination of the law, according to which an acid divides itself between two bases which are present in the same solution, has been long known to be one of the obscure questions of chemistry, it is generally admitted by chemists that there is a division, and that the relative masses of the two bases exercise an important influence upon the law which governs it, but the law itself remains unknown, and the object of Prof. Jellett's paper was to give at least a partial, possibly a final, solution to the problem, treating the question as one of chemical equilibrium, and defining these terms as follows:—Two or more substances may be said to be in chemical equilibrium if they can be brought into chemical presence of each other, without the formation of any new compound or change in the amount of any of the substances which are thus brought together. If an acid be added to a mixture of two bases, four substances will be present, *i.e.* two salts and two portions of bases remaining uncombined, these four are in chemical equilibrium—the question is why—and the author showed that there can be but one equation of equilibrium, inasmuch as the quantities of the four substances which are present in the solution, are functions of three independent variables, namely:—the original quantities of each base (2) and the original quantity of acid (1) denoting by b_1 b_2 the quantities of free base, and by s_1 s_2 the quantities of each salt respectively, the equation of equilibrium is necessarily of the form $U = F(b_1, s_1, b_2, s_2) = 0$, and the object of the author's investigations was to determine the form of the function F. The bases selected for experiment were quinia and brucia. In quinia the rotatory power of any of its salts exceeds the rotatory power of the base. In quinia the reverse is the case, and as the result of careful and long continued experiments, it was proved that equilibrium is not troubled by dilution, for a disturbance could not arise without altering the rotation—there was no alteration, and the equilibrium, therefore, depended only on the ratios of the four substances, hence:—

$$U = F\left(\frac{1_1}{s_1}, \frac{b_2}{s_2}, \frac{b_1}{b_2}\right)$$

By a second series of experiments it was proved—putting r_1 = rotatory power of brucia, ρ_1 = rotatory power of hydrochlorate of quinia, r_2 = rotatory power of brucia, ρ_2 = rotatory power of hydrochlorate of brucia, r = actual rotation for acidulated mixture, a = total amount of acid corresponding to the unit of bulk of solution, x = amount which combines with the quinia, it is easily seen that

$$r = \left(\frac{\beta_1 x}{a}\rho_1 + (b) - \frac{\beta_1 x}{a}\right) r_1 + \frac{\beta_2 (a - x)}{a}\rho_2 + \left(b_2 - \frac{\beta_2 (a - x)}{a}\right) r_2$$

where β_1, β_2, a are the atomic weights of the two bases and the acid respectively, and b_1 b_2 are the quantities of each base contained in the bulk of the solution. Solving this equation for x, we have

$$x = A a + B(r - b_1 r_1 - b_2 r_2), \text{ where}$$

$$A = \frac{\beta_2 (r_2 - \rho_2)}{\beta_1 (\rho_1 - r_1) + \beta_2 (r_2 - \rho_2)}$$

$$B = \frac{a}{\beta_1 (\rho_1 - r_1) + \beta_2 (r_2 - \rho_2)}$$

If r_0 be the actual rotation caused by the unacidulated mixture, it is evident that $r_0 = b_1 r_1 + b_2 r_2$. The foregoing may therefore be written

$$x = A a + B(r - r_0)$$

By a third series of experiments it was seen that if a solution of quinia is acidulated so that the quantity of uncombined base bears to the acid the same ratio as in the foregoing mixture between the uncombined quinia and the quantity x, and a solution of brucia is prepared so as to preserve the ratio of the uncombined brucia to $a - x$. Then the ratios in these of b_1 to s_1 and of b_2 to s_2 are the same as in the case of equilibrium, the rotation caused by these fluids being r_1 and r_2. Let them be mixed in the proportions $m : n$, and the rotation caused by the mixture is $\frac{m r_1 + n r_2}{m + n}$, and whatever be the ratio of $m : n$, there being no rupture of equilibrium, it is evident that if the ratios $b_1 : s_1$ and $b_2 : s_2$ have the values proper for equilibrium, the latter will be preserved, however the ratio $b_1 : b_2$ may vary. Hence, in mathematical language, $U = F\left(\frac{b_1}{s_2}, \frac{b_2}{s_2}\right)$ By a fourth series of experiments a mixture of solution of quinia and brucia was made, in which these bases have to each other the same ratio as the uncombined bases in the second series of experiments. A second mixture is made of the same solutions in which the bases have the same ratio as the combined bases in the second series of experiments. Sufficient acid is added to the latter mixture to convert the bases into salts. Here the ratios $b_1 : b_2$ and $s_1 : s_2$ have the values for equilibrium. If these now be added to each other in the proportion of $m : n$, the rotation caused by the mixture is—

$$\frac{m r_1 + n r_2}{m + n}$$

$r_1 r_2$ being the rotation caused by each of the added fluids separately, it is inferred as before that $U = \int\left(\frac{b_1}{b_2}, \frac{s_1}{s_2}\right)$, but if U satisfy both these conditions it is easily shown mathematically that $U = \int\left(\frac{b_1}{s_1} \div \frac{b_2}{s_2}\right)$, hence it is evident that the required equation of equilibrium is $\frac{b_1}{s_1} \div \frac{b_2}{s_2} = \text{constant}$. The author showed the bearing of the law upon the theory that chemical combination is not statical but dynamical, observing that this theory is quite in accordance with the results obtained by him. (This valuable memoir will appear in full in the Transactions of the Royal Irish Academy.)

PHILADELPHIA

Academy of Natural Sciences, Aug. 19, 1873.—Dr. Ruschenberger, president, in the chair. "The Composition of Trautwinite." The author gave a few additional details concerning this new mineral, which was described in the Proceedings of the Academy for January 1.

Sept. 9.—Mr. Gentry communicated a notice of a great swarm of ephemerids which passed through the town of Lewisburg, on the Susquehanna River, on the afternoon of August 22. The swarm was estimated to be about a mile in length by nearly a half mile in width, and was so dense as even to obscure passers-by on the opposite side of the street.

Sept. 15.—The following papers were presented for publication:—"On a new American species of Glyptocephalus," by Theo. Gill; "Description of fifty-two species of Unionidæ," by Isaac Lea. The last-named paper was, on report of the committee, ordered to be published in the Journal of the Academy.

BOSTON, U.S.

Society of Natural History, Nov. 5, 1873.—Mr. F. W. Putnam read a paper on *Myxine*, a low genus of fishes, known to fishermen as *hags*, giving an account of its anatomy, which was illustrated by a series of specimens exhibited. The several species described by various authors must be reduced to one, having a wide geographical distribution, being found on both sides of the Northern Atlantic, and also on the southern coast of South America. Mr. Putnam showed that the variations in the number of lingual teeth, which are from eight to eleven in each row in specimens from the North Atlantic and from the Straits of Magellan, could not be considered as of specific importance. The different varieties of this species he considered as follows:—*Var. septentrionalis*, the short and thick form, from the North Atlantic, *var. limosa*, the long and slender variety, also from the North Atlantic; while the southern variety may be called *australis*, the name under which Jennyns described it as a true species.—Dr. Thomas Dwight read a paper on the "Structure and Action of Striated Muscular Fibre." His studies had been made on the muscles of the legs of the small water beetle *Gyrinus*. Their covering is quite transparent, and after the leg has been cut off and put into a drop of water under a covering glass, the contractions can often be observed for over an hour. He found that the fibre, at rest, consisted of narrow granular transverse stripes, with broad light-coloured bands between them. Close to the black stripe there was a glaring white reflection, but midway between two stripes the fibre was gray. When the fibre contracted the black bands came nearer together, and their granular structure became more obscure; the gray band disappeared, so that there was merely an alternation of black and white stripes. The ends of the white stripes bulged out during contraction. As the wave of contraction moved along, it was easy to see that there was no interchange of position between the black and the light substances, and no homogeneous transition stage, as is maintained by Merkel. When one part of the fibre is in contraction, the part from which the wave is running is put upon the stretch; the black bands are divided into two rows of granules, and there is less distinction between the white and gray substances.

PARIS

Academy of Sciences, Dec. 29, 1873.—M. de Quatrefages, president, in the chair.—The following papers were read:—On the formation of equations of the condition which results from the observations of the Transit of Venus on December 8, 1874. —A new answer to M. Pasteur, by M. Trecul. This was a general review of M. Pasteur's views as to the origin of yeast. M. Pasteur briefly replied.—A theoretical essay on the equilibrium and elasticity of pulverulent masses and on the thrust of non-cohesive earth, by M. J. Boussinesq.—On the isomerism of albumenoids, by M. Béchamp. The author gave many details with regard to various albumenoids; he had discovered three in cow's milk. M. Dumas confirmed the latter result, which he had himself attained by different means.—Action of water on sheet lead, by H. Marais.—Note on hybernating *Phylloxera* and on their agility and artificial restoration, by M. Max. Cornu.—Observations on a note of M. Menabrea relating to Lagrange's series, by M. Genocchi.—Researches on arsenious hydride, by M. Engel. The author has been repeating Wiederhold's researches on the supposed As_2H; he, however, did not obtain the substance in question.—Note on the action of iodine on uric acid, by M. F. Wurtz. The author found that when these bodies were allowed to act in the presence of water, alloxan and hydriodic acid were formed, and probably also urea with other bodies.—Synthesis of oxalyl urea (parabanic acid), by M. E. Grimaux.—On a new arrangement of the sulphate of copper battery, by M. Trouvé.—Observations on the existence of certain relations between the colouring and geographical distribution of birds, by M. A. Milne-Edwards.—On fossil remains of Batrachia, Lacertia, and Ophidia found in the phosphate of lime deposits at Aveyron, by M. Filhol.—On the development of the *phragmostracum* of the *Cephalopoda* and the zoological connection of the *Ammonites* with the *Spirulæ*, by M. Munier-Chalmas.—On waterspouts and cyclones, by M. E. Mouchez.—On the effects of Indian hemp, by M. A. Naquet.—During the meeting, elections were held for the posts of correspondent of the astronomical section, vacant by the deaths of Encke and Admiral Smyth, to which Messrs. Lockyer and Roche were elected.

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