THURSDAY, MARCH 22, 1883

PATHOLOGICAL ANATOMY

A Text-Book of Pathological Anatomy and Pathogenesis. By Ernst Ziegler. Translated and Edited for English Students by Donald MacAlister, M.A., M.B., St. John's College, Cambridge. 8vo. (London: Macmillan and Co., 1883.)

FOR some years the student of medicine has felt the want of an English manual of modern Pathological Anatomy. He has been compelled either to trust entirely to his teacher, or to consult works and memoirs little adapted for beginners. This felt want the English edition of Ziegler's Pathology, when completed, will in great part meet. The author believing "that the learner gains a readier grasp of his subject when it is first presented to him as a uniform and coherent system of doctrine," has, by avoiding "much matter of controversy," succeeded in making a clear and concise statement of each subject treated. In this the author has been well seconded by the editor, who, by carefully revising and amending the original, by adding numerous references to English and French memoirs, and by otherwise with characteristic ability adapting the work for English readers, has greatly enhanced its value.

Although the authors have kept the student chiefly in view in preparing this manual, a glance at the small print and the numerous references given, will at once prove that those desirous of gaining an exhaustive knowledge of the subject, and those engaged in special investigations, have not been neglected. It seems to us that this is by far the best plan for a text-book. It is to be regretted that students at the present day read so little. In many instances they content themselves with "learning" in order afterwards to retail what they purchase from their teachers; or what is worse, when they are unfortunate enough not to have their teacher as one of their examiners, they "get up" an endless number of often useless facts, derived from all possible sources, before presenting themselves for examination. This waste of time and energy in great part results from the want of good text-books. The books available are generally too large, they are often quite beyond the grasp of the beginner, and at the same time not a little out of date. In order to be able to utilise fully the opportunities now offered for gaining a practical knowledge of pathology, and other allied subjects, lectures are not enough; there must be something to fall back upon, by means of which the impressions received from the teacher may be tested, something that will form a foundation on which an intelligent knowledge of the subject may be built. We believe that the work before us will serve this purpose, and that it will be equally useful to the teacher by enabling him to take for granted that the fundamental facts of his science can be again and again referred to as the student requires, and by providing short, concise statements which he can modify at will, and to which he can add much that is of historical interest, or that is too recent for any manual, however complete, to contain.

The volume now published deals with General Pathological Anatomy. It is divided into seven sections. Those on Malformations, Inflammation, Tumours, and Bacteria deserve especial mention. In treating these subjects the authors have been careful to avail themselves of all the recent investigations, not only in Pathology, but also in Embryology and other branches of Biology, and by making free use of small print and giving abundant references, they have succeeded in drawing up a more complete account than exists in any other English manual.

In a very suggestive introductory chapter some of the special terms used by pathologists are defined, and the functions of pathological anatomy indicated. In the section on the Formative Disturbances of Nutrition the researches of Strasburger and Flemming on the changes in cells and nuclei during subdivision are considered, and a diagram showing indirect cell-division is introduced. In speaking of cell-multiplication it is pointed out that the proposition, "The stronger the external stimulus the greater the proliferation," cannot be accepted; that "one can at most admit that very slight stimuli, sufficient merely to excite the cell without injuring it, may perhaps call into play its power of multiplication; but nothing has been experimentally established concerning the nature, the action, or the mode of application of such stimuli"; further, that "when the nutritive and formative activities of a cell are morbidly increased, the effect is due to augmentation of the physiological stimuli or diminution of the physiological resistances to growth, or the direct influence of external stimuli"; the factors probably favouring proliferation being (1) an increased capacity in the cell to assimilate nutriment, (2) an increased supply of nourishment, (3) the removal of the normal checks to growth. In the same chapter there is an account of the origin of epithelium, fibrous and adipose tissue, and of new blood-vessels; and, in the chapter immediately following, an account of the origin of pus-corpuscles and of the mode in which tissues are regenerated.

Tubercle and other allied diseases, such as lupus, leprosy, and glanders, are spoken of as "Infective Granulomata." A tubercle is defined from a histological point of view as "a non-vascular cellular nodule which does not grow beyond a certain size, and at a certain stage of its development becomes caseous"; but it is afterwards pointed out that when Koch's recent investigations are taken into consideration it must be spoken of as "a cellular nodule containing within it the specific tuberculous virus, the bacillus tuberculosis."

Among these infective granulomata we have the new disease known as "actinomycosis," which is associated with the presence of the peculiar fungus *Actinomyces*. In this disease the infection probably starts from the mouth, and results in the formation of granulations and fibrous tissue and in suppuration.

The classification of tumours has long been a puzzle to pathologists. Later writers have more and more recognised their relation to the embryonic layers, and now we have, we believe for the first time in an English text-book, a purely embryological arrangement, tumours being divided by the authors into: (1) those derived from the mesoblast—the connective-tissue tumours; (2) those containing elements derived from epithelial cells—the epithelial tumours. This classification, which commends itself by its simplicity, is likely to be generally adopted.

The consideration of the different kinds of tumours is followed by a chapter on their ætiology, in which Cohnheim's embryonic hypothesis is discussed at some length, and the objections to its general acceptance pointed out. In answer to the question, How does the tumour assume properties distinct from those of its surroundings? there is as a reply, "We believe that the phenomenon is ultimately due to some change affecting individual elements of a tissue whereby they are rendered dissimilar to their neighbours." The change is manifested especially in this —that the normal checks to the indefinite growth of the proliferous cells are inoperative or inadequate, either because the formative and productive energy is increased, or because the restraining influence of the surrounding structures is diminished, or from both causes together.

The last section of the present volume is devoted to Parasites. On comparing the German account of animal parasites with the English, we note very considerable additions and improvements. The chapter contains a sufficiently complete account of the structure and life-history of the ordinary parasites for all practical purposes. The chapter on Bacteria is extremely valuable. The editor has been careful to incorporate in the text all the important recent discoveries, and references are given to all the memoirs that the student or investigator is likely to require to consult. We thus have in a connected form the results of numerous inquiries into the nature of the organisms which for some time have been claiming not a little of the attention of biologists and physicians. In describing the bacteria, reference is made to the influence of temperature and of the surrounding medium on their growth and development, also to the influence they exercise on the nutrient liquid, and to their presence without and within the living body.

In reference to the existence of bacteria within the body we read:—

"Bacteria are perpetually entering the body with the food we eat and the air we breathe. They must, therefore, be at times found in the tissues, especially in places where access is direct. The fact that they are not easy to demonstrate is readily explained. It must be only a small number that are able to multiply in the tissues they have penetrated; the majority must quickly perish." Bacteria are described as pathogenous and non-pathogenous, the latter being harmless unless the normal secretions undergo some alteration, or the bacteria develop to an unusual extent. Under such conditions, inflammation may be set up, or the whole system may be influenced by the absorption of the soluble products of decomposition, some of which are extremely poisonous, and capable according to Hiller, of altering or even destroying the tissues exposed to them. "The pathogenous bacteria have the power of settling, not merely in the ingesta and secretions, or in dead tissue, but also in living tissue. This happens chiefly in the mucous membranes and in the lungs. The uninjured skin is protected against invasion by the horny epidermis."

"Many of the bacteria can settle in perfectly healthy mucous membranes. In the case of others we must imagine that they do not find a proper soil for their development, unless the mucous membrane is injured or altered. Of course injury or alteration of this kind may seem to make the outer skin or any other accessible tissue the starting-point of a bacterial invasion (wound-infection). All that is necessary is that a bacterium should reach a spot that affords the conditions of its development. If this occurs, it multiplies and forms colonies or swarms. These may, according to the species of the fungus and the nature of its soil, remain in aggregation, forming heaps or masses, or may spread through the tissues. Such a settlement is never without effect on the affected tissues. The bacteria may force their way into the substance of the constituent elements, and especially into the tissue-cells, which are sometimes found to be crammed with bacteria."

All that is necessary is that a bacterium should reach a spot that affords the conditions for its development, *i.e.* "the temperature of the body must be such as favours its development; it must be able to abstract fit nutriment from the tissues in which it settles; it must nowhere encounter substances which check or injure it." When in the tissues, the increase of the bacteria may be arrested by the aggregation of living cells resulting from the inflammation they set up, assisted by the regenerative action of the fixed tissue-cells. If this does not happen, they spread into the surrounding tissues, usually reaching the lymphatics and blood-vessels, some to perish, others rapidly to multiply.

The bacteria are supposed to lead to disease by withdrawing nourishment, setting up chemical changes—partly by their direct action on the nutrient material, and partly by the action of the unorganised ferments they form; and finally, as a result of these changes, by producing poisonous matters. In doing this they enter into conflict with the tissue-cells, influencing their nutritive activity, changing them or even leading to their destruction. Whether it is a change in the fermentive action of the cells, or a disturbance of the functions of the central nervous system which leads to fever, has not been determined. Neither is it known whether the unsusceptible condition of the tissues which usually follows when the bacteria have been eliminated, results from "a modification in the chemical constitution of the tissues, or to a change in the vital activity of the cells."

In referring to the relation of bacteria to infective diseases it is stated "that among the infective diseases there are certainly some which are due to the invasion of a microphyte, and that it is highly probable the others have a like origin." This chapter further gives a short account of the various diseases which have been described as resulting from the influence of bacteria, and concludes by discussing the burning question of the present moment —the mutability of bacterial species. It is well known that Naegeli, Buchner, and others believe "that both the morphological and the physiological characters of the bacteria are mutable"; that "a given bacillus does not invariably produce bacilli of the same structure, and does not always pass through the same developmental stages." "A bacterium which, under given conditions, gives rise to a definite kind of fermentation, may lose this property when cultivated under different conditions." Koch and others believe that bacteria do not alter in their properties, and that "even when the nutrient medium is altered from time to time no recognisable differences are produced."

The authors point out that "at present we are unable to draw any certain conclusion regarding the relation of non-pathogenous to pathogenous bacteria. Clinical experience would indicate that the activity of the infective virus may vary within certain limits. And we must apparently admit that the infective bacteria have not always possessed their noxious qualities, but have acquired them somehow in the course of ages. But this is not enough to convince us that harmless bacteria can acquire infective properties rapidly. . . . We may therefore provisionally conclude that the transformation of innocuous into noxious bacteria can occur but rarely, and under special conditions."

Recent work both in this country and on the Continent seems to go against the mutability theory, and in all probability it will soon be made clear that Buchner's experiments are capable of another interpretation from that hitherto adopted.

Enough has been said to indicate that the English edition of Ziegler's Pathology will not only prove of immense help to the student, but that it will also be invaluable to the practitioner. It is to be hoped that the second part, on Special Pathological Anatomy, will soon appear, and that it will equally commend itself to English readers.

The numerous woodcuts with which the work is illustrated are beautifully distinct, the type and paper are everything that could be desired, and so successful has the editor been that there is no evidence of the greater part of the work being a translation.

ENSILAGE

Ensilage in America. By James E. Thorold Rogers, M.P. (London: W. Swan Sonnenschein and Co., 1883.)

PROFESSOR ROGERS has contributed a most interesting little book on Ensilage in America. He has no doubt been serviceable to his country in drawing public attention to a subject of importance; but like most persons who focus their eyes upon a single point, he has lost the due proportion in which it stands to its background, foreground, and surroundings. Perhaps this may be forgiven as a common fault, or it may be the secret of strength, in all propagandists. Be this as it may, it is a marked feature in the volume before us. Ensilage is to be the temporal salvation of the farmer. The Professor appears to have been carried away on the full tide of American enthusiasm, buoyed up by a certain youthful airiness scarcely consistent with the gravity of an Oxford Don. He has forgotten the salt, and those who read his book (and we trust they may be numbered by thousands) must add it for themselves.

Ensilage is the preservation of green fodder in its natural succulent condition in pits or *Silos.* These pits must be airtight and watertight, and the fodder must be so well trampled into them and weighted on the top as to arrest fermentation. The theory of the process is that, in the case of fodder so treated, heat is generated and fermentation commences. The small amount of oxygen held in the interstitial air is speedily absorbed, and its place taken by carbonic acid gas. Just as a lighted candle extinguishes itself in a bath of choke-damp of its own making when burnt in a closed vessel: so the fermentation and its accompanying heat are arrested in the mass of closely packed fodder which is in fact immersed in a bath of carbonic acid, and thus securely protected from ordinary atmospheric action. Well preserved ensilage comes out of the pit almost as green and fresh as when it was first put into it, and has acquired a pleasant vinous smell and slightly acid flavour, which has given it its name of sourhay in Germany, Austria, and Hungary. The process is at once simple and effective, but is no doubt expensive when carried out upon the scale which a successful experiment demands. Thus the larger the pit the more assured the success, as all the conditions are more perfectly attained. At p. 22 we read: "M. Havemeyer's silos were four—two fifty-nine feet long and fourteen feet wide; and two thirty-five feet long and twelve feet wide, each pair being twenty-five feet deep. They are under the same roof as the feeding barn, where there is standing-room for ninety-eight cows." The pits are bricked and cemented, or built with concrete walls, and they may be carried up higher than the level of the ground, or may be built entirely from the surface. When the ground is naturally dry and of a clayey or close texture the silo need not be lined. It is recommended that a drain should if possible be carried from the bottom of the silo to take off superfluous water. Simple as these directions undoubtedly are, they point to a heavy initial expenditure, only to be recommended after very mature consideration. On the other hand silos of smaller size, as, for example, $22' \times 9' \times 15'$ deep and other dimensions, are also mentioned. Still the fact remains that in small silos there is more waste and greater uncertainty. Also that for practical purposes a small silo would be of little value. The process of storing the fodder is very easy to understand. It is, in the case of green maize, cut up with a powerful chaff-cutter, trampled into the pit by men or horses, and when the space is filled it is covered with boards and weighted down with boxes of stone or earth to a pressure of about 100 lbs. per square foot. The fodder settles down under pressure, and is found after several months to be perfectly palatable and fresh.

Such is the process which Prof. Rogers now lays before the British public with the strongest possible recommendations. Not only so, but with threatenings or at least warnings also, for we are told that "if the New Englanders and New Yorkers succeed in extending their ensilage system, they will strive to find a foreign market for their increased produce." This process, it is urged, is entirely to revolutionise agriculture. It is to be a new point of departure, a "new dispensation." "Is there not a bonanza (a mining term for peculiarly rich ore) in the farms with this new enterprise? Will it not give the farmer such profits, with less labour, as will enable him to be more independent? Is it not going to create new interests with our sons, when they can find a more profitable employment, with less hard labour than can be found in any business in our cities?" It is to double the population of "our New England cities," and indeed appears to be a veritable *El Dorado* for farmers.

In thus introducing ensilage to the attention of his countrymen, Prof. Rogers is scarcely cautious in the manner in which he discounts the value of scientific and especially of chemical opinion upon this subject. "Ensilage is to be the food of the future for pigs and poultry as well as for horses and bullocks." But it is only grass after all, and we can hardly believe that it can be superior to the herbage from which it was made. Pigs and poultry will graze in pastures it is true, but the digestive systems of these animals demand more concentrated foods. There is an evident tendency to "forget the rock from which it was hewn," if we may apply such words to the process of ensilage. It is green fodder *preserved* until winter. Well! if preserved until winter it cannot be eaten in summer. If eaten in summer it surely would also have been realised.

It may be better than hay, but we cannot expect from ensilage such superlative results above what might reasonably be expected from hay. Such high-flown anticipations as are embodied in Prof. Rogers's book are usually doomed to disappointment, and the process of ensilage will probably take its place in American and English agriculture as it has already taken its place in the agriculture of the Continent of Europe, among other improvements of the nineteenth century, but without overtopping any of them.

Prof. Rogers does not appear to have informed himself as to the state of our knowledge in England upon this topic. The process was fully described by the present writer for the Royal Agricultural Society in 1874. He also drew attention to it in two letters to the *Times* in 1875, when it evoked considerable discussion, under the title of "Potted Hay." The process was also both described and illustrated by drawings in the *Agricultural Gazette* at the same period. Since then it has been repeatedly tried, but in all cases without marked success. We are ready to allow that this want of success has been due to the experiments having been conducted upon a small scale and probably with too much regard to economy of outlay. The process is too generally successful in many countries to be capable of being challenged. So late, however, as 1875 Prof. Tormay of Pesth wrote to us that practical men were greatly divided as to its value. No doubt the making of sourhay deserves further trial, and there is as little doubt that it will be largely experimented upon during the coming summer. It must, however, be remembered that it may be as profitable to eat the herbage when growing, as to preserve it *in any form* for the winter. Also that our turnips, swedes, and mangels give us a means of producing meat in these countries which is not possessed by American agriculturists. Turnips and hay are probably a better combination of succulent and dry food for winter feeding than turnips and ensilage would be. In this matter we prefer to suspend judgment for a while upon the uses of ensilage to the British farmer. At the same time we cordially agree that it is likely to be particularly useful in the Eastern States of America, where the soil and climate are unfavourable to the growth of roots and favourable to the growth of maize. On this point we have abundant testimony in Prof. Rogers's book. A special attraction towards ensilage is that it can be carried out without delay in any weather, and that it saves the anxiety of haymaking.

Those who have tried it in this country complain that it is very difficult to keep the pit good when it has been once opened. Still the process is worthy of more extended trial, and if carried out without too much fear of the initial expense and risk of failure, may be shown to be of service to English agriculturists.

JOHN WRIGHTSON

OUR BOOK SHELF

Another Book of Scraps, principally relating to Natural History. With thirty-six Lithographic Illustrations from Pen and Ink Sketches of Wild Birds. By Charles Murray Adamson. (Newcastle-on-Tyne: Reid, 1882.)

MR. ADAMSON has been so much amused by the preparation of his first "Book of Scraps" that he has prepared another, and invites our opinion upon it. We cannot say that we have derived much information from looking through his letterpress, although we perfectly agree with him that the study of natural history, which he advocates, "opens out a wide field for the profitable employment of the mind." But the thirty-six illustrations which form the main portion of the book certainly show that the author has studied the forms and habits of wild birds to some purpose, although in an artistic point of view, perhaps, it would not be difficult to criticise the surroundings amongst which he places them. The drawings are a little rough, as Mr. Adamson himself confesses, but no naturalist can turn them over without recognising at once the species which are intended to be portrayed. We have seen pictures in the Royal Academy of which the same remark could not truthfully be made. Mr. Adamson is evidently most at home on the sea-shore. His sea-birds are best. With his woodcocks and partridges we are not so well satisfied. But "Another Book of Scraps" will make a nice addition to a drawing-room table.

LETTERS TO THE EDITOR

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

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

Incubation of the Ostrich

I HAVE received the following letter from Mr. J. E. Harting, and with his permission send it to you for publication. I do so partly in justice to Mr. Harting himself—the letter having been originally written to the *Spectator* in vindication of his own accuracy, and having been rejected by the editor—and partly because I think it desirable that the point in natural history which it discusses should be definitely cleared from the erroneous views which, as I shall presently show, are still prevalent with regard to it.

ANIMAL INTELLIGENCE

To the Editor of the Spectator

SIR,—I have just read in your issue of February 3 a letter from Mr. G. J. Romanes to which a long editorial note is appended, and which raises an interesting question relating to the incubation of the ostrich. As my name is mentioned as having written something on the subject, perhaps you will allow me to offer a few remarks.

Briefly stated, the point under discussion is this: Mr. Romanes, in his recently-published work on "Animal Intelligence," has observed that in the case of the ostrich *the task of incubation is shared by both the sexes.*

In reviewing this work your critic alleges that "female ostriches take *no* part in the duty of incubation"—that is, they do not assist the male.

Whereupon Mr. Romanes cites his authorities for the statement made by him, and refers amongst other sources to my book on ostriches, published in 1876, wherein (at p. 41) I remark that "the males are polygamous, each associating with three or four hens, all of which lay their eggs in one large nest scooped out in the sand, and relieve each other by turns at incubation. Le Vaillant purposely watched an ostrich's nest, and during the day saw four hens sit successively on the same eggs, a male bird coming late in the evening to take his turn at incubation." A little further on, I added: "Incubation lasts six weeks, the cock-bird taking his turn at sitting like the hens."

Your reviewer, still sceptical, replies: "The passage in Mr. Harting's book is based on the statement of Le Vaillant, whose observations, except when confirmed by later experience, are justly discredited by the best-informed naturalists of the present day, as he was notoriously so often unworthy of belief."

Permit me to point out that in making the statement above quoted, I by no means relied *solely* on Le Vaillant. I had before me the evidence of several modern observers on the subject, whose publications are referred to in my "List of Works quoted," at the commencement of my volume. At p. 189 I have alluded to the experiments made at San Donato, near Florence, in 1859 and 1860, by Prince Demidoff, who says that "the female ostrich began to sit as soon as the first egg was laid, and sat for three hours daily, leaving the male for the rest of the time."

At p. 196, quoting a report forwarded in 1873 by a resident of experience in South Africa to the Council of the Zoological and Acclimatisation Society of Victoria, who were then contemplating the introduction of the ostrich into that colony, I find this distinct statement: "The process of hatching is performed by the male and female sitting alternately, one keeping a vigilant look-out as sentry, as well as procuring food."

Again, in a Report by Dr. W. G. Atherstone of Grahamstown, based on observations made by himself and friends on different ostrich farms in the neigbourhood of Grahamstown, and quoted by me *in extenso*, the following passage occurs on p. 202 of my book:—"They sit alternately, the male at night grazing and guarding the females. During the daytime, the time of the male bird going on the nest vari s during the period of incubation, as also does the time between the female leaving the nest and the male taking her place, the exposure and cooling being probably regulated by the temperature of the incubation fever at different stages."

In addition to the evidence of these observers I had before me the testimony of Mr. F. Denny of Grahamstown, which is too long to be quoted here, but which will be found embodied in an interesting note published in the *Zoologist* for 1874 (p. 3916); so that I felt perfectly justified in asserting in effect, as Mr. Romanes has done, that *the task of incubation with the ostrich is shared by both the sexes.* It would be easy to adduce further evidence on the subject if necessary, but I will not occupy space further than to observe that if your reviewer will turn to p. 107 of Douglass's "Ostrich Taming in South Africa," published by Messrs. Cassell and Co. in 1881, he will see a full-page illustration thus lettered, "*Hen bird sitting.* From a photograph taken at Heatherton Towers."

Admirers of Le Vaillant will be glad to learn that in this case at least his assertions (to quote your reviewer) "have been confirmed by later experience," and are therefore not to be discredited.—I am, Sir, your obedient servant,

22, Regent's Park Road, N.W. J. E. HARTING

After such a battery of evidence it seems almost needless to adduce more; but as the point is an interesting one to ornithologists, I shall briefly add some corroborative proof from other sources.

In the *Spectator*, besides referring to the above, I gave a reference to two articles published by Mr. E. B. Biggar on the ostrich-farms of the Cape Colony, and also to the recently published work by Mr. Nicols; from each of these sources I shall now quote brief passages. Mr. Biggar writes as follows:—

"Some will sit throughout with the most solicitous maternal instinct; . . . others manifest such anxiety, that when the hen has been a little late in taking her morning turn upon the nest, he has gone out, and, hunting her up, has kicked her to the nest in the most unmanly manner. Some are very affectionate over their young, others the reverse; thus do individuals differ even among ostriches. As a rule the cock bird forms the nest, sits the longest, and takes the burden of the work of hatching and rearing. Contrary to what has been currently understood, and what is still stated even in recent colonial accounts, the cock bird sits at night, not the hen. In this peculiarity the hand of Providence may be seen, for the worst enemies of the nest appear at night, and the cock, being stronger and braver, is better able to resist them; moreover, the feathers of the cock being black, night sitting would not expose him to that exhaustion from the sun's rays which would ensue if he sat during the day; while at the same time the grey feathers of the female are less conspicuous while she sits during the day."—*Field*, August 21, 1880.

And again, "After turning the eggs over one by one with her beak, she will sit perhaps for hours with her head stretched flat and snake-like on the ground, and her body as motionless as a mound of earth. Occasionally, on hot days, she may be seen with her body lifted slightly out of the nest to admit a current of air over the eggs; and sometimes she will even leave the nest for two or three hours, till instinct tells her that the lowering temperature requires her return" (*Century*, January, 1883).

Mr. Nicol's work, entitled "Zoological Notes," repeatedly states that the hen bird assists the cock in the process of incubation, and on my writing to him to ask whether he had witnessed the fact, he answers that although he has not done so himself, a well-educated friend "who had passed some time in visiting ostrich-farms in South Africa" had done so; and, in answer to his express inquiry on the subject, wrote, "that the female took part in the task, though not nearly to so great an extent as the male," adding that he was surprised to hear there should be any question concerning a fact so well known to the ostrich farmers.

Lastly, having recently been to Florence, I took the opportunity of calling upon the superintendent and proprietor of the Zoological Gardens there, and obtained all the particulars of the case alluded to by Mr. Harting in the above letter as having occurred at San Donato. I found that two broods of young had been raised in successive years by the same pair of ostriches, and that on both occasions the female assisted the male to incubate the eggs: "que le male et la femelle couvent alternativement," in the words of the published report ("Guide du R. Jardin Zoologique de Florence," p. 81, 1868). Here, however, as in all the previously-mentioned cases, the fact which I stated in "Animal Intelligence" was apparent, viz. that the cock bird undertook the whole duty of sitting during the night.

Now when all this evidence is taken together it appears to me impossible to doubt that the female ostrich assists the male in the process of incubation. Yet from the fact of this evidence not having been clearly focused, an old error on the subject still appears to be prevalent. This error arose some twenty years ago from the observations of M. Noel Suchet (? or Suquet) on a pair of ostriches kept in confinement. Thus, in 1863, Dr. Sclater wrote:—"We now know with certainty from the observations of M. Noel Suchet, Director of the Zoological Gardens at Marseilles, that the normal habits of the ostrich (as regards incubation) do not differ materially from those of its allies of the same family" (*Proc. Zool. Soc.*, 1863, p. 233); and Mr. Darwin, following the judgment formed by Dr. Sclater, wrote in the "Descent of Man" (p. 479) that the male bird "undertakes the whole duty of incubation." Again, my reviewer in the *Spectator*—who, although curiously weak in his logic, appears to be strong in his ornithology—pins his faith entirely to this single observation of M. Suchet. Lastly, Prof. Newton in his article on "Birds" in the "Encyclopædia Britannica" (p. 771), relying, I presume, on the same observation, writes:—"A band of female ostriches scrape holes in the desert sand, and therein promiscuously dropping their eggs, cover them with earth, and leave the task of incubation to the male, who discharges the duty thus imposed upon him by night only, and trusts by day to the sun's rays for keeping up the needful fostering warmth."

Thus it appears that the influence of M. Suchet's observations has been very disproportionate to its merits, and has misled some of our principal ornithologists concerning the normal habits of ostriches.[1] Possibly Prof. Newton, with his extensive knowledge of the literature of such matters, and writing since the appearance of most of the counter-evidence which I have given, is cognisant of some other observations on which he rests his statement. But, if so, it becomes desirable that he should supply his references, as otherwise his statement appears to rest, as my reviewer in the *Spectator* would say, "simply on the survival of the old belief." GEORGE J. ROMANES

March 12

Difficult Cases of Mimicry

I HAVE received from Mr. Thos. Blakiston, of Tokio, Japan, a communication to the *Japan Mail* by himself and Prof. Alexander,

[1] I may observe that Mr. R. B. Sharpe, writing in "Cassell's Natural History" (vol. iv. p. 228), has not been thus misled, for he says distinctly that the cock and hen "relieve each other by turns."

commenting on my article in NATURE, vol. xxvi. p. 86, and pointing out some errors as to the estimated advantage derived by the mimicking butterflies. On referring to my article, I find that I have, by an oversight, misstated the mathematical solution of the problem as given by Dr. Fritz Müller and confirmed by Mr. Meldola, and have thus given rise to some confusion to persons who have not the original article in the *Proceedings of the Entomological Society* to refer to. Your readers will remember that the question at issue was the advantage gained by a distasteful, and therefore protected, species of butterfly, which resembled another distasteful species, owing to a certain number being annually destroyed by young insectivorous birds in gaining experience of their distastefulness. Dr. Müller says: "If both species are equally common, then both will derive the same benefit from their resemblance—each will save half the number of victims which it has to furnish to the inexperience of its foes. But if one species is commoner than the other, then the benefit is unequally divided, and the *proportional advantage* for each of the two species which arises from their resemblance is *as the square* of their relative numbers." This is undoubtedly correct, but in my article I stated it in other words, and incorrectly, thus: "If two species, both equally distasteful, resemble each other, then the number of individuals sacrificed is divided between them in the proportion of the square of their respective numbers; so that if one species (*a*) is twice as numerous as another (*b*), then (*b*) will lose only one-fourth as many individuals as it would do if it were quite unlike (*a*); and if it is only one-tenth as numerous, then it will benefit in the proportion of 100 to 1."

This statement is shown by Messrs. Blakiston and Alexander to be untrue; but as some of your readers may not quite see how, if so, Dr. Müller's statement can be correct, it will be well to give some illustrative cases. Using small and easy figures, let us first suppose one species to be twice as numerous as the other, *a* having 2000 and *b* 1000 individuals, while the number required to be sacrified to the birds is 30. Then, if *b* were unlike *a* it would lose 30 out of 1000, but when they become so like each other as to be mistaken, they would lose only 30 between them, *a* losing 20, and *b* 10. Thus *b* would be 20 better off than before, and *a* only 10 better off; but the 20 gained by *b* is a gain on 1000, equal to a gain of 40 on 2000, or four times as much *in proportion* as the gain of *a*. In another case let us suppose *c* to consist of 10,000 individuals, *d* of 1000 only, and the number required to be sacrificed in order to teach the young birds to be 110 for each species. Then, when both became alike, they would lose 110 between them, *c* losing 100, *d* only 10. Thus *c* will gain only 10 on its total of 10,000, while *d* will gain 100 on its total of 1000, equal to 1000 on 10,000, or 100 times as much *proportional gain* as *c*. Thus, while the gain in actual numbers is inversely proportional to the numbers of the two species, the *proportional* gain of each is inversely as the *square* of the two numbers.

I am, however, not quite sure that this way of estimating the *proportionate* gain has any bearing on the problem. When the numbers are very unequal, the species having the smaller number of individuals will presumably be less flourishing, and perhaps on the road to extinction. By coming to be mistaken for a flourishing species it will gain an amount of advantage which may long preserve it as a species; but the advantage will be measured solely by the fraction of *its own numbers* saved from destruction, not by the proportion this saving bears to that of the other species. I am inclined to think, therefore, that the benefit derived by a species resembling another more numerous in individuals is really in inverse proportion to their respective numbers, and that the proportion of the squares adduced by Dr. Müller, although it undoubtedly exists, has no bearing on the difficulty to be explained.

ALFRED R. WALLACE

MR. A. R. WALLACE has been so good as to forward me the extract from the *Japan Mail* above referred to, together with his reply. The article in question bears the title, "Protection by Mimicry—a Problem in Mathematical Zoology." The authors, while admitting the broad principles involved in Dr. Fritz Müller's theory, fail to see why the advantage derived by the mimicking species, in cases where the latter is less numerous than the model, should be as the square of the relative numbers. They admit that "the ingenious explanation seems perfectly satisfactory," but the proportional benefit appeared to them exaggerated. Mr. Wallace has now, I think, cleared up the misunderstanding with reference to this part of the question, but it may be of use in assisting towards the further discussion of the problem if I here give the simple algebraical treatment adopted in the original paper.

Let a_1 and a_2 be the numbers of two distasteful species of butterflies in some definite district during one summer, and let n be the number of individuals of a distinct species which are destroyed in the course of a summer before its distastefulness is generally known. If both species are totally dissimilar, then each loses n individuals. If, however, they are undistinguishably similar, then the first loses $\frac{a_1 n}{a_1 + a_2}$ and the second loses $\frac{a_2 n}{a_1 + a_2}$. The absolute gain by the resemblance is therefore for the first species, $n - \frac{a_1 n}{a_1 + a_2} = \frac{a_2 n}{a_1 + a_2}$; and in a similar manner for the second species, $\frac{a_1 n}{a_1 + a_2}$. This absolute gain, compared with the total numbers of the species, gives for the first (A_1), $\frac{a_2 n}{a_1(a_1 + a_2)}$, and for the second ($A_2$), $\frac{a_1 n}{a_2(a_1 + a_2)}$. We thus have the proportion, $A_1 : A_2 = a_2^2 : a_1^2$.

With reference to Mr. Wallace's concluding paragraph, I may point out that the advantage of the mimic is "measured solely by the fraction of *its own members* saved from destruction." Thus, taking his last example, the species *c* saves only 1/1000 of its whole number, and *d* saves 1/10 of its whole number by the resemblance to *c*. The fact that these numbers stand to one another in the ratio of $1 : 10^2$, whilst $c : d = 10 : 1$, is a mathematical necessity from which I do not see how we can escape. As the numerical disproportion betwen the species increases, the advantage derived by the more abundant insect is practically a vanishing quantity; whilst, on the other hand, if the two species are equal in numbers, it is obvious that they both derive the same advantage, each losing only half the number that it would if there was no resemblance between them.

It must not be forgotten in considering the question of mimicry between two nauseous species that the foregoing calculations apply only to the case where the resemblance is perfect, *i.e.* so exact that the insects are absolutely undistinguishable by their foes. The initial steps may be hastened in these cases by the near blood-relationship of the species, and it is a remarkable circumstance that large numbers of species belonging to different distasteful genera have a close similarity of wing-pattern, although the distinctness of the genera has never been called in question. But the genera concerned, although distinct, are very closely related, and this is quite in accordance with the views here advocated.

The general question as to the persecution of distasteful butterflies by young inexperienced birds, &c., is certainly one on which much work remains to be done, and very great service could be rendered if naturalists residing in the tropics would undertake some systematic experiments in this direction. My friend, Mr. W. L. Distant, the author of the "Rhopalocera Malayana," has already given reasons in these columns (vol. xxvi. p. 105) for disbelieving in any such want of experience, and I have discussed this phase of the question with him elsewhere (*Ann. and Mag. Nat. Hist.*, December, 1882).

R. MELDOLA

On the Value of the "Neoarctic" as One of the Primary Zoological Regions

IN the *Proceedings of the Academy of Natural Sciences of Philadelphia* (December, 1882) Prof. Angelo Heilprin has an article under the above title, in which he seeks to show that the Neoarctic and Palæarctic should form one region, for which he proposes the somewhat awkward name "Triarctic Region," or the region of the three northern continents. The reasons for this proposal are, that in the chief vertebrate classes the proportion of peculiar forms is less in both the Neoarctic and Palæarctic than in any of the other regions; while, if these two regions are combined, they will, together, have an amount of peculiarity greater than some of the tropical regions.

This may be quite true without leading to the conclusion argued for. The best division of the earth into zoological regions is a question not to be settled by looking at it from one point of view alone; and Prof. Heilprin entirely omits two considerations—peculiarity due to the absence of widespread groups, and geographical individuality. The absence of the families of hedgehogs, swine, and dormice, and of the genera *Meles*, *Equus*, *Bos*, *Gazella*, *Mus*, *Cricetus*, *Meriones*, *Dipus*, and *Hystrix*, among mammals; and of the important families of flycatchers and starlings, the extreme rarity of larks, the scarcity of warblers, and the absence of such widespread genera as *Acrocephalus*, *Hypolais*, *Ruticilla*, *Saxicola*, *Accentor*, *Garrulus*, *Fringilla*, *Emberiza*, *Motacilla*, *Yunx*, *Cuculus*, *Caprimulgus*, *Perdix*, *Coturnix*, and all the true pheasants, among birds, many of which are groups which may almost be said to characterise the Old World as compared with the New, must surely be allowed to have great weight in determining this question.

The geographical individuality of the two regions is of no less importance, and if we once quit these well-marked and most natural primary divisions we shall, I believe, open up questions as regards the remaining regions which it will not be easy to set at rest. There runs through Prof. Heilprin's paper a tacit assumption that there should be an equivalence, if not an absolute equality, in the zoological characteristics and peculiarities of all the regions. But even after these two are united, there will remain discrepancies of almost equal amount among the rest, since in some groups the Neotropical, in others the Australian, far exceed all other regions in their speciality. The temperate and cold parts of the globe are necessarily less marked by highly peculiar groups than the tropical areas, because they have been recently subjected to great extremes of climate, and have thus not been able to preserve so many ancient and specialised forms as the more uniformly warm areas. But, taking this fact into account, it seems to me that the individuality of the Neoarctic and Palæarctic regions is very well marked, and much greater than could have been anticipated; and I do not think that naturalists in general will be induced to give them up by any such arguments as are here brought forward.

ALFRED R. WALLACE

A Remarkable Phenomenon.—Natural Snowballs

I TAKE the liberty of inclosing a copy of an account of natural snowballs which I furnished to the *Courant* newspaper in this place. It may be well to state that the distance from Long Island Sound to Massachusetts is some seventy miles, and that the Connecticut Valley Railroad is about fifty miles long, and runs close to the bank of the Connecticut River for some forty miles; the rolls of snow on the frozen river are said to have been very large and handsome.

SAMUEL HART

Trinity College, Hartford, Conn., U.S.A., February 22

On Tuesday evening a light but damp snow fell upon the crust that had formed over the snow of Sunday's storm; and the south wind, which arose at a later hour, produced an unusual phenomenon. Wednesday morning the college campus, the park, and vacant lots everywhere hereabouts were seen to be strewn with natural snowballs, some of them resembling spheres with diameters of from one to nine inches or more, and others looking very much like rolls of light cotton batting, having a cylindrical shape, but in nearly every case with a conical depression at each end reaching nearly or quite to the middle. It was easy to see how the balls had been formed, as it is easy to see how boys roll up the snow for their forts. The wind had in each case started a small pellet of the moist snow, and it had rolled along until it grew so large that the wind could move it no further. The ball not only increased in diameter as it rolled, but also grew gradually in length as a little more of the snow stuck to it on each side, and thus the snow was formed into the peculiar shape described—that of a cylinder with a hollow at each end, as if a long isosceles triangle were rolled up, beginning at its vertex. The largest of the cylinders measured on the college campus had a diameter of twelve inches and a length of eighteen inches, while others in the fields in the neighbourhood seemed much larger. The path of the balls could in many cases be readily traced for a distance of twenty-five or thirty feet. The snow, it should be added, was not at all closely packed, but lay together very lightly and yielded to a slight touch, so that it was impossible to move a ball without breaking it.

Observers in other parts of the city report that some balls were seen of the size of a barrel which left tracks behind them for more than sixty feet. From East Hartford it is reported that they studded the fields thickly, especially in places where the wind had a long range, and were of every size to that of a half bushel or larger. Similar balls were seen yesterday morning in many places from the Sound north to Massachusetts. All along the line of the Valley Railroad they appeared on every rod of ground, and at some places they had left tracks showing that the wind had blown them in every direction, even in some cases up hill.

This interesting phenomenon, though quite unusual, has been noticed before in different places in this country and elsewhere, the most striking instance on record being one which was observed in New Jersey in 1808; this was in the daytime, when the whole process could be watched. On this occasion some of the masses of snow which were rolled up by the wind attained a diameter of three feet. They appear to have been seen, however, over an area of only some four hundred acres, whereas the snowballs yesterday were spread thickly over many square miles.

[We have received a communication on the same subject from Prof. Brocklesby of Hartford.—ED.]

The Late Transit of Venus

I AM told that, in referring to the observations on the late transit of Venus which were made from a station on our college grounds by the astronomers of the German Imperial Commission, you speak of them as using the photographic process. This is not correct; besides contact observations they restricted themselves to the use of the heliometer. The first and the second contacts were not seen by reason of clouds; but four half sets and six full sets of heliometric measurements were made—128 in all. The third and the fourth contacts were observed by the German astronomers and by myself.

SAMUEL HART

Trinity College, Hartford, Conn., U.S.A., February 22

Rankine's "Rules and Tables"

I DO not know upon what authority your reviewer of Rankine's "Rules and Tables" bases his dictum that the r in the rule for the extension or compression of a spiral spring should be to the second power instead of to the third power. Prof. Rankine's view was that it should be r^3. I would refer your reviewer to vol. xviii. of the *Transactions of the Institution of Engineers and Shipbuilders in Scotland*, where he will find, amongst other results of an experimental committee's investigations upon the important question of the loading of safety-valves by such springs, that the *third* power of the radius or diameter of the spring is also used.

W. J. MILLAR

Glasgow, March 10

[The formula given by Mr. Millar is, the writer of the notice informs us, perfectly correct, and the error is his.—ED.]

Meteors

ABOUT five minutes past seven this evening I saw the most beautiful "*shooting star*" I have ever witnessed. It was moving from east to west directly over this town, and disappeared at an apparent distance of ten or twelve miles, after traversing an arc of about 75° as I saw it. It was visible whilst one might count ten or twelve at the usual rate of speaking. In its course it not only left a most unusually long train of light behind, but whole pieces kept *dropping*. What appeared is thus best described. These pieces followed the original for a space, leaving perceptible lines of light. Probably ten or a dozen such pieces were broken off during the time I was looking. Some idea of it may be gathered from the fact that for a time I thought it was a rocket. The light was remarkably white, the brilliance much above that of Venus at any time, and its rate of motion slow. The most remarkable feature, however, was the continuous breaking away of pieces, which left in turn visible trains of light.

THOMAS MASHEDER

The Grammar School, Ashby-de-la-Zouch, March 17

IN NATURE, vol. xxvii. p. 434, reading somewhat hastily, I took the brilliant meteor there mentioned to be one I myself saw. Reading more carefully, however, in last week's issue, I see that both day and hour and direction differ. On March 4, about 8.45 p.m., a very large and bright meteor passed at a low altitude from south to north. It was of a greater apparent size than Venus, quite as bright, but with a greener light. The motion was slow, no train; it only became incandescent during a short part of its transit, and passing behind the roofs of some houses was immediately lost to sight. HENRY CECIL

Bregner, Bournemouth, March 20

P.S.—If a line be drawn north and south, the meteor became visible at a point due east, which direction I was facing.

THE BRITISH CIRCUMPOLAR EXPEDITION[1]

THE journey to Fort Rae, though long, was full of interest and variety. Our party, consisting of myself, two sergeants, and an artificer, of the Royal Artillery, left Winnipeg on June 9 by steamer for Fort Carlton, on the Saskatchewan, *viâ* Lake Winnipeg. We were detained a day in that lake by ice, but reached the mouth of the Saskatchewan on the 13th, where we were delayed four days trans-shipping cargo to the river steamer, which lay three miles off at the upper end of the rapids; a tedious voyage of eight days took us to Carlton, a stockaded port on the south bank of the river. For the first three days the country seemed one immense swamp, with numerous shallow lakes; then the banks gradually grow higher, till at "the Forks" (the confluence of the north and south branches of the Saskatchewan) they are about 150 feet above the river. Here the soil seems very rich and fertile, and about the new settlement of Prince Albert, a day higher up, the country is quite English in appearance—undulating, covered with rich grass, with woods here and there—a far more attractive-looking country than the flat, treeless prairie near Winnipeg.

From Carlton, after a day or two spent in hiring transport carts, we started on the 30th with a train of ten carts, containing our provisions and baggage. The country was very pretty, well wooded and watered, with duck, snipe, and prairie chicken in abundance; it was at times difficult to believe one was not in an English park. But the most vivid imagination cannot picture the swarms of mosquitoes that at times attacked us: they came against our faces like flakes in a heavy snowstorm, and though we found our veils and gloves a good protection whilst travelling, yet, when mealtimes came, veils had to be laid aside, and the wretched insects seized the opportunity of taking their meal too.

On the third day of our journey, on reaching the crest of some rising ground, an extended view opened before us, ridge behind ridge, a sombre sea of pinewood stretching away in the distance. It was the great sub-Arctic forest which extends northwards to the barren grounds at the Arctic circle and east and west to the Atlantic and the Pacific. On entering the woods the mosquitoes were not quite so bad, but our unfortunate animals became the prey of an enormous horsefly, which settled on them in thousands, biting them till they were streaming with blood. Fortunately they only came out during the heat of the day, and we were sometimes obliged to make a halt and light fires so that the animals might stand in the smoke, which they were very willing to do; indeed they often put a newly-lighted fire out by rolling in it.

The road through the woods was very bad, and breakdowns were numerous, but at last on July 9 we reached Green Lake, which we left by boat on the 11th for Ile à la Crosse. Our conveyance was now one of the Hudson Bay Company's inland boats, with a crew of eight Indians. As we had the stream with us, we were able to drift all night, only landing when we required to cook; so we reached Ile à la Crosse early on the 14th.

We left it the same evening with a crew of eight Chipewyans, the best crew we ever had. I think they must have pulled sixty miles on one day, the day after we left the fort. On the evening of that day we had an aurora shortly after sunset, which is unusually early in the evening for one. This one appeared to be remarkably close, from its rapid motion and from its being between us and a cirro-cumulus cloud. It was accompanied by a distinct swishing noise like the sound of a sharp squall in a ship's rigging, or the noise a whip makes in passing through the air. I have not heard it since, though there have been plenty of auroras, but from what I have been told by those who have passed their lives in the country, I am of opinion that this sound is occasionally, though rarely, heard, and that it would be heard oftener were it not that the aurora is generally at too great a height.

Two more days brought us to Portage la Loche, a track of some fourteen miles across the watershed dividing the basin of the Arctic Ocean from that of Hudson's Bay. It is fairly level till the last mile, when the edge of the valley of the Clearwater River is reached, some 600 feet above the stream. From this point the view of the valley is very fine, and it strikes one the more from the monotonous nature of the scenery hitherto. The river flows between two ranges of hills, from 800 to 1000 feet in height, and here and there in rapids between limestone cliffs. The first "portage" (where the boats have to be hauled some distance overland) is particularly picturesque, but the whole valley abounds in bits that would delight an artist's eye.

On July 28 we reached the Athabasca River, a fine stream, half a mile or more in width, and the strong current, aided by a fair wind, took us to the lake in a couple of days. There are several springs of naphtha and one of sulphur on the banks.

On crossing Lake Athabasca to Fort Chipewyan, there is a complete change in the character of the country. On the south side the banks are nearly level with the water, all reeds and mud; on the north side is a savage wilderness of Laurentian rock. From a hill at the back of the fort is an extensive view of this strange and desolate country. To the west the lake stretches away to the horizon; on the other side is a mixture of lake, island, and river, and to the north the land, a wilderness of rock in low rounded hills, with a few stunted pines in the valleys, all pretty enough, but so lonely looking.

We were detained a fortnight at Fort Chipewyan till the arrival of the Mackenzie River boats. The heat was at times extreme—as much as 90° in the house.

The Slave River, or Mackenzie, as it really is, is a magnificent river, especially after its junction with the Peace River, which is at least as big as the Athabasca. The united stream is often a mile in width. About half way to Slave Lake are the rapids, where the scenery is very fine. There are four portages, over three of which the boats had to be hauled, so it was two days' work getting through them. We had a sharp frost on the morning of the 19th, the buckets, &c., that were left with water in them had a quarter of an inch of ice on them in the morning.

On the next evening, while running down the rapid to the last portage, the "Portage des Noyés," after sunset, a bright parhelion made its appearance, some 10° or 12° above the horizon. It was of a bright red colour, and threw a brilliant reflection in the water, remaining visible for about twenty minutes, when it changed into a crimson column, that gradually died away.

On August 22 we reached Fort Resolution—a wretched-looking place on a flat muddy coast—and the same evening we left for Fort Rae. At sunset the pilot of the boat insisted on stopping for the night at a small rocky island at the mouth of the Slave River. I thought it a pity to stop as we had a fair wind, but the natives of the country have a great dread of lakes, and certainly Great Slave Lake is a stormy place. At midnight a heavy swell suddenly arose, and our boat was stove in and sunk in a very few minutes. It was a pretty wet job to land all the baggage and stores, which of course were all saturated

[1] Letter from Capt. Dawson, R.A., in command of the Expedition See p. 243.

with water; but fortunately the instruments all escaped unhurt, and nothing was lost but a pair of boots and a couple of hats, and all our salt and most of our sugar, which the water dissolved.

For the next two days we were employed repairing the boat, it blowing a gale and raining hard the whole time, so that we could dry nothing; and when at last we started, almost constant head-winds and frequent gales made our journey a slow one. Fortunately our course lay among islands, so that we enjoyed a certain degree of shelter from the wind, and harbours of refuge were always at hand in case of necessity. These islands are all of rock and well wooded, but destitute at this season of the year of game, which was unfortunate for us, as our provisions were getting short, and our crew were reduced to a pound of flour per diem, with a little tea and sugar. There were not even fish to be caught, though they are usually abundant, but I suppose the rough weather had driven them into the deep water. At last we shot some seagulls, and we were all glad enough to eat them.

At length, on the 30th, we reached Fort Rae, which lies in lat. 62° 38′ N. and long. 115° 25′ W., half way up a long gulf that runs for about 100 miles in a north-west direction from the mouth of the Yellow Knife River. The fort is situated at the foot of a rocky hill that rises some 200 feet above the lake, which is about four miles wide at this point. The Indians who resort here for trade hunt for the most part in the "barren lands" near the Coppermine River, whence they bring quantities of skins and beef from the musk-ox, which seems to be very abundant. Deer too are very plentiful, and in the winter they migrate in great herds from the barren lands to the country between the arm of the lake on which Fort Rae lies and the Mackenzie. Sometimes these herds pass quite close to the fort, and take two or three days in passing. Their numbers must be very great; a single band has been known to kill over 15,000 in an ordinary season.

This year the deer have passed at some distance, but the Indians are now bringing in fresh meat daily.

These Indians are of the "Dog-rib" tribe—T'akfwelottiné, they call themselves—a quiet, inoffensive race, like all the wood-Indians. They are almost all Roman Catholics, the missionaries of that religion being very numerous in the country, and they are certainly very devoted and hard-working. There are also Protestant missionaries, but they do not appear to have made any converts.

The Dog-ribs are a branch of the Chipewyan family which occupies all that portion of the continent between the Rocky Mountains and Hudson's Bay to the north of the parallel of 55°. They are unprepossessing in appearance, and their language is almost unpronounceable by a European. Their alphabet, if they had one, would contain no less than seventy-one letters, that being the number of distinct sounds. I believe the language is allied to the ancient Mexican—at any rate the Navajo is the nearest to it of existing languages—and the combinations of letters that one sees in Mexican names (*tl*, for instance) are common in this language. The Dog-ribs have the remarkable peculiarity of a national habit of stammering, which is most marked in those who seldom come in to the fort. They treat their women with more kindness than is usual among the American Indians.

Fort Rae is one of the windiest and cloudiest places I have ever seen, but I am told this is an exceptional year. It is certainly a very late autumn; the lake was not frozen till November 1, and it is only within the last day or two that the cold weather has really set in. Last night the thermometer was at −34°.

My space is at an end, but by the next mail I hope to give you an account of our winter here.

Fort Rae, December 1

ON THE NATURE OF INHIBITION, AND THE ACTION OF DRUGS UPON IT[1]

IV.

A CONDITION very nearly similar to that caused by atropia is produced by morphia. When this substance is given to a frog, its effects are exactly similar to those produced by the successive removal of the different parts of the nervous system from above downwards. Goltz has shown that when the cerebral lobes are removed from the frog it loses the power of voluntary motion and sits still; when the optic lobes are removed it will spring when stimulated, but loses the power of directing its movements. When the cerebellum is removed it loses the power of springing at all; and when the spinal cord is destroyed reflex action is abolished.

Now these are exactly the effects produced by morphia, the frog poisoned by it first losing voluntary motion, next the power of directing its movements, next the power of springing at all, and lastly reflex action. But after reflex action is destroyed by morphia and the frog is apparently dead, a very remarkable condition appears, the general flaccidity passes away and is succeeded by a stage of excitement, a slight touch causing violent convulsions just as if the animal had been poisoned by strychnia.[2]

The action of morphia here appears to be clearly that of destroying the function of the nerve centres from above downwards, causing paralysis first of the cerebral lobes, next of the optic lobes, next of the cerebellum, and next of the cord. But it seems probable that the paralysis of the cord first observed is only apparent and not real, and in order to explain it on the ordinary hypothesis we must assume that during it the inhibitory centres in the cord are intensely excited so as to prevent any motor action, that afterwards they become completely paralysed, and thus we get convulsions occurring from slight stimuli.

On the hypothesis of interference, the phenomena produced both by atropia and by morphia can be more simply explained. These drugs, acting on the nervous structures, gradually lessen the functional activity both of cells and of fibres; the impulses are retarded, and thus the length of nervous connection between the cells of the spinal cord, which is calculated to keep them in proper relation in the normal animal, just suffices at a certain stage to throw the impulses half a wave-length behind the other, and thus to cause complete inhibition and apparent paralysis.

As the action of the drug goes on, the retardation becomes still greater, and then the impulses are thrown very nearly, but not quite, a whole wave-length behind the other, and thus they coincide for a short time, but gradually again interfere, and therefore we get on the application of a stimulus, a tonic convulsion followed by several clonic ones, and then by a period of rest. This explanation is further borne out by the fact observed by Fraser, that the convulsions caused by atropia occurred more readily during winter, when the temperature of the laboratory is low and the cold would tend to aid the action of the drug in retarding the transmission of impulses.[3]

The effect of strychnia in causing tetanus is very remarkable; a very small dose of it administered to a frog first renders the animal most sensitive to reflex impulses, so that slight impressions which would normally have no effect, produce reflex action. As the poisoning proceeds, a slight stimulus no longer produces a reflex action limited to a few muscles, but causes a general convulsion throughout all the body, all muscles being apparently put equally on the stretch. In man the form assumed by the body is that of a bow, the head and the heels being bent backwards, the hands clenched, and the arms tightly drawn to the body.

[1] Continued from p. 468.

[2] Marshall Hall, Memoirs on the Nervous System, p. vii. (London, 1837). Witkowski. Archiv für exper Path. und Pharm., Band vii. p. 247.

[3] Transactions of the Royal Society of Edinburgh, vol. xxv. p. 467.

My friend Dr. Ferrier has shown that this position is due to the different strengths of the various muscles in the body. All being contracted to their utmost, the stronger overpower the weaker, and thus the powerful extensors of the back, and muscles of the thighs keep the body arched backwards and the legs rigid, while the adductors and flexors of the arms and fingers clench the fist and bend the arms, and draw them close to the body.[1] The convulsions are not continuous, but are clonic; a violent convulsion coming on and lasting for a while, and then being succeeded by an interval of rest, to which after a little while another convulsion succeeds. The animal generally dies either of asphyxia during a convulsion, or of stoppage of the heart during the interval.

When the animal is left to itself the convulsions—at least in frogs—appear to me to follow a certain rhythm, the intervals remaining for some little time of nearly the same extent.

A slight external stimulus, however, applied during the interval—or at least during a certain part of it—will bring on the convulsion. But this is not the case during the whole interval. Immediately after each convulsion has ceased I have observed a period in which stimulation applied to the surface appears to have no effect whatever.

It is rather extraordinary also, that although touching the surface produces convulsions, irritation of the skin by acid does not do so.[2]

The cause of those convulsions was located in the spinal cord by Magendie in an elaborate series of experiments.

Other observers have tried to discover whether any change in the peripheral nerves also took part in causing convulsion; but from further experiments it appears that the irritability of the sensory nerves is not increased.[3]

According to Rosenthal, strychnia does not affect the rate at which impulses are transmitted in peripheral nerves; according to him, however, it lessens the time required for reflex actions. Wundt came to the conclusion that the reflex time was on the contrary increased.

In trying to explain the phenomenon of strychnia tetanus on the hypothesis of interference, one would have been inclined by Rosenthal's experiments to say that strychnia quickened the transmission of impulses along those fibres in the spinal cord which connect the different cells together.

The impulses which normally, by travelling further round fell behind the simple motor ones by half a wave-length, and thus inhibited them, would now fall only a small fraction of a wave-length behind, and we should have stimulation instead of inhibition.

Wundt's results, on the other hand, would lead to the same result by supposing that the inhibitory wave was retarded so as to fall a whole wave-length behind the motor one. On the assumption, however, that the fibres which pass transversely across from sensory to motor cells, and those that pass upwards and downwards in the cord connecting the cells of successive strata in it, are equally affected, we do not get a satisfactory explanation of the rhythmical nature of the convulsions. By supposing, however, that these are not equally affected, but that the resistance in one—let us say, that in the longitudinal fibres—is more increased than in the transverse fibres we shall get the impulses at one time thrown completely upon each other causing intense convulsion, at another half a wave-length behind, causing complete relaxation, which is exactly what we find.

This view is to some extent borne out by the different effect produced by a constant current upon these convulsions, according as it is passed transversely or longitudinally through the spinal cord. Ranke found that when passed transversely, it has no effect, but when passed longitudinally in either direction, it completely arrests the strychnia convulsions, and also the normal reflexes which are produced by tactile stimuli.

[1] Brain, vol. iv. p. 313.
[2] Eckhard, Hermann's Handb. d. Physiol. Band ii. Th. 2. p. 43.
[3] Bernstein quoted by Eckhard, *op. cit.* p. 40. Walton, Ludwig's Arbeiten, 1882.

Ranke's observations have been repeated by others with varying result, and this variation may, I think, be explained by the effect of temperature.

Near the beginning of this paper I mentioned that the touchstone of the truth or falsehood of the hypothesis of inhibition by interference was to be found in the results of quickening or slowing the rate of transmission of stimuli.

Heat and cold are the two agents regarding whose action in this respect we have the most trustworthy experimental data. In peripheral nerves, heat up to a certain point quickens the transmission of stimuli, and cold retards it. In the spinal cord warmth increases the excitability, and at a temperature of 29 to 30 may of itself cause tetanus.[1] Cold also beyond a certain temperature increases the reflex excitability.

The effect of warmth and cold upon strychnia tetanus is what we would expect on the hypothesis of interference. With small doses of strychnia warmth abolishes the convulsions, while cold increases them. When large doses are given, on the contrary, warmth increases the convulsions, and cold abolishes them.[2]

We may explain this result on the hypothesis of interference in the following manner:—

If a small dose of strychnia retard the transmission of nervous impulses so that the inhibitory wave is allowed to fall rather more than half a wave-length, but not a whole wave-length, behind the stimulant wave, we should have a certain amount of stimulation instead of inhibition. Slight warmth, by quickening the transmission of impulses, should counteract this effect, and should remove the effect of the strychnia. Cold, on the other hand, by causing still further retardation, should increase the effect. With a large dose of strychnia, the transmission of the inhibitory wave being still further retarded, the warmth would be sufficient to make the two waves coincide, while the cold would throw back the inhibitory wave a whole wave-length, and thus again abolish the convulsions.

The effect of temperature on the poisonous action of guanidine is also very extraordinary, and is very hard to explain by the ordinary hypotheses, although the phenomena seem quite natural when we look at them as cases of interference due to alterations in the rapidity with which the stimuli are transmitted along nervous structures. Guanidine produces, in frogs poisoned by it, fibrillary twitchings of the muscles, which are well marked at medium temperatures, but are abolished by extremes of heat and cold. Thus Luchsinger has found that, when four frogs are poisoned by this substance, and one is placed in ice-water, another in water at 18° C., a third in water at 25° C., and a fourth in water at 32° C., the fibrillary twitchings soon disappear from the frog at 0° C., and only return when its temperature is raised to about 18° C. In the frog at 18° C. convulsions occur, which are still greater in the one at 25° C. In the frog at 32° C., on the other hand, no trace of convulsions is to be seen; the animal appears perfectly well, and five times the dose of the poison, which at ordinary temperatures would convulse it, may be given to it without doing it any harm, so long as it remains in the warmth,[3] although when it is cooled down the effect of the poison at once appears.

Another cause of tetanus that is difficult to understand on the ordinary hypothesis of inhibitory centres is the similar effect of absence of oxygen and excess of oxygen. When an animal is confined in a closed chamber, without oxygen it dies of convulsions; when oxygen is gradually

[1] Cayrade, Recherches critiques et exper. sur les Mouvements Reflexes, p. 48.
[2] Kunde and Virchow quoted by Eckhard, *op. cit.* p. 44; Foster, Journal of Anatomy and Physiology, November 1873. p. 45.
[3] Luchsinger, Physiologische Studien, Leipzig, 1882, p. 44.

introduced before the convulsions become too marked, it recovers. But when the pressure of oxygen is gradually raised above the normal, the animal again dies of convulsions. This is evidently not the effect of mere increase in atmospheric pressure, but the effect of the oxygen on the animal, inasmuch as 25 atmospheres of common air are required to produce the oxygen convulsions, while 3 atmospheres of pure oxygen are sufficient. This effect is readily explained on the hypothesis of interference by supposing that the absence of oxygen retards the transmission of impulses in the nerve-centres; so that we get those which ought ordinarily to inhibit one another, coinciding and causing convulsions. Increased supply of oxygen gradually quickens the transmission of impulses until the waves first reach the normal relation, and then the normal rate being exceeded, the impulses once more nearly coincide, and convulsions are produced a second time.

In discussing the action of the nervous system we have hitherto taken into account only that of the nerve fibrils, and left out of the question the nerve cells. We have assumed that the waves arrived in the reservoir (in our diagram) from a distance, and were simply transmitted along channels, but in the nervous system we have to take into account the origination of the waves in the nerve cells themselves, as well as their propagation along the nerve channels.

There is a great difference between the function of the nerve cell and of the nerve fibre analogous to that which exists between the cell and the wire in a galvanic battery. The particular form of energy which we met with in both cases originates in the cell and is transmitted along the fibre or the wire. In both cases also the energy appears to originate from chemical changes going on in the cell. Material waste of some sort goes on in both, and in both the products of this waste if allowed to accumulate will by and by arrest the action.

We find an indication of the difference between the amount of chemical change which goes on in the nerve cell and in the nerve fibre in the amount of blood supplied to each respectively. The nerve cells are abundantly supplied with blood, and the nerve fibres very sparingly so. The free supply of blood secures to the nerve cells both the supply of fresh material and ready removal of waste products.

Perhaps the best illustration that we can find in physics of the processes which take place in the nervous and muscular systems is however afforded by singing flames in which the sounds and movements are produced by very numerous small explosions: for both in the nervous and muscular systems the tissue change appears to go on as a series of small explosions. The material which yields nervous and muscular energy undergoes oxidation, but the oxygen concerned in the process is not derived directly from the external air. Substances which yield oxygen are contained within the tissues themselves just as nitre is contained along with oxidisable substances in a charge of gunpowder.

In this paper also we have spoken of waves of nervous interference as if they were simple, but it is much more probable that they are very complex, resembling much more the beats of sound produced by two singing flames which are not in unison, than simple waves of water.

The number of nervous discharges which issue from the motor cells of the spinal cord during tetanus and set the muscles in action is, according to Dr. Burdon Sanderson, about 16 per second, but in all probability each of these impulses consists of a large number of small vibrations. In rhythmical actions, such as that of the respiration, we have probably at the very least three rhythms, 1st, exceedingly rapid vibrations in the nervous cells; 2nd, slower vibrations or beats from 16 or 18 per second, which issue from them and excite the muscles to action; and 3rd, a still slower rhythm, of 16 per minute, probably due to interference between groups of cells, which leads to inspiratory movements alternating with rest or with active exspiration. The consideration of these complicated phenomena would, however, at present lead us too far, and they as well as the subject of nervous interference in the heart and rhythmic contraction of muscles, must be reserved for another time.

In this paper I must be content with the attempt to show that inhibition and stimulation in the nervous system are not dependent on special inhibitory or stimulating centres, but are merely relative conditions depending on the length of path along which the stimulus has to travel and the rate of its transmission. The test of the truth or falsehood of this hypothesis is to be found in the effect of alteration in the rapidity of nervous transmission upon inhibitory phenomena. The application of this test appears, so far as our present data go, to support this hypothesis. T. LAUDER BRUNTON

BEN NEVIS OBSERVATORY

IN NATURE, vol. xxvii. p. 39, I gave a brief notice that on November 1—owing to stress of weather forbidding the regular daily ascents of Ben Nevis—I was obliged to discontinue the daily work of the meteorological observing system on the summit and slopes of the mountain. This was in simultaneous connection with my system of observations near the sea-level at Achintore, Fort William. As in the previous summer, I had the honour to organise and carry on the work under the auspices of the Scottish Meteorological Society. The experience gained in 1881, when I first commenced observing on the Ben, enabled me to draw up and submit to the Society a more elaborate plan of mountain observation for the summer and autumn of 1882; and as I have been fortunate enough to carry it through for five months without any hitch, and as I am not aware that anything of the kind had, previous to my first undertaking, been attempted, I am naturally anxious that NATURE should have a more complete account of my last year's operations. My plan was to have *fixed* stations at different altitudes between the main observatories at the base and on the summit of the mountain, so placed in fact that I could observe *regularly* at half-hourly intervals during the daily ascent and descent of the Ben; to extend the number of summit observations to five sets; and to have in every case simultaneous observations taken at the sea-level station—my grand base of operations. All this was with a view to localising disturbances existing in the stratum of atmosphere between the sea-level and the top of Ben Nevis, to furthering meteorological research generally, and so ultimately to gain forecasting material. I arrived at Fort William from Edinburgh on May 25, and at once proceeded to give effect to my plans. During the next few days I was engaged mainly in erecting Stevenson's thermometer screens, and laying out the sea-level station; in establishing a new "midway" observatory at the lake, erecting screen, and building there a granite cairn for a barometer; and in reopening the temporary observatory on the summit of the mountain. It was only by dint of great exertion and a gang of men that I got all in order on the top of the Ben on May 31. I had no occasion, however, to alter the arrangements of the previous summer; and the heavy work of reopening chiefly consisted in digging out from the vast accumulations of snow the barometer cairn, hut, and thermometer cage which here, as a safeguard, incloses Stevenson's screen. The snow, in fact, was nearly four feet deep, and it was necessary to cut out wide areas around the instruments. I also erected another screen to contain Negretti and Zambra's self-registering clock-hygrometer, most kindly placed at my disposal by that eminent firm for the purpose of obtaining 9 p.m. values. I had also to fix a new roof of ship's canvas to the rude shanty that affords some little shelter from the piercing cold and storms. The barometer, a fine Fortin, had been left in its cairn built up during the past winter; and great labour was expended before the north side of the cairn was reopened, the stones being so hard frozen that a crowbar had to be employed. The instrument was found in good condition.

Passing over all other details of arranging the stations and fixing instruments, I may say that I had all in order and commenced work on June 1. I now give a list of the stations, with positions, hours, and elements of observation.[1] The distances in the text are given in right lines from the sea-level station. Fig. 1 at once shows the bearings, and distances by the actual track followed. Fig. 2 is a longitudinal section giving total actual distances.

ACHINTORE, FORT WILLIAM, BASE OR SEA-LEVEL OBSERVATORY.—*Position*: About 28 feet above sea, on a level sward, perfectly open on all sides, running parallel and immediately adjacent to Loch Linnhe; soil, gravelly loam.

Hours.—5, 5.30, 6.15, 7, 7.30, 7.55, 8.30, 9, 9.30, 10, 10.30, 11, and 11.30 a.m.; and noon, 0.30, 1, 1.45, 2.30, 3, 6, and 9 p.m.

Elements.—Atmospheric pressure by mercurial barometer, temperature of air and evaporation (dry and wet bulbs), direction and force of wind, kind and amount of

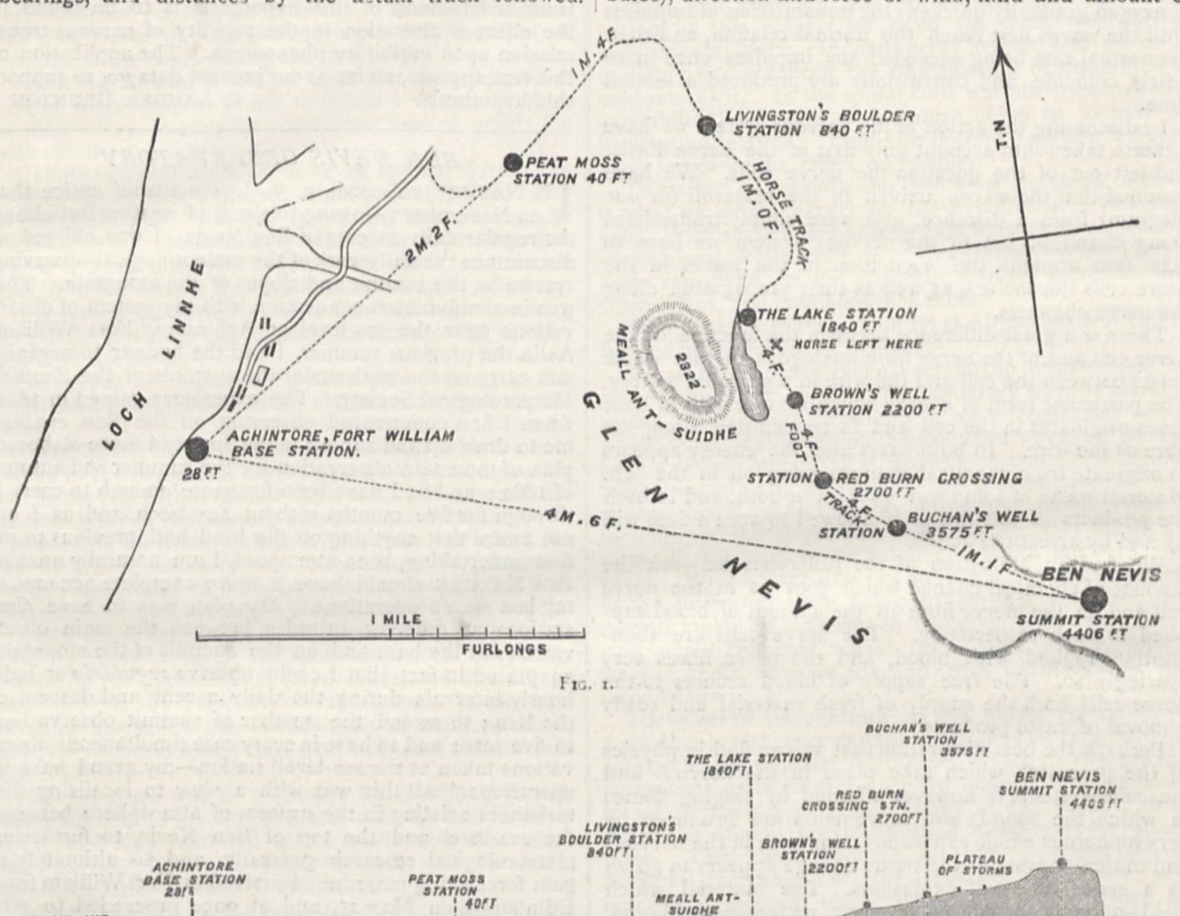


FIG. 1.

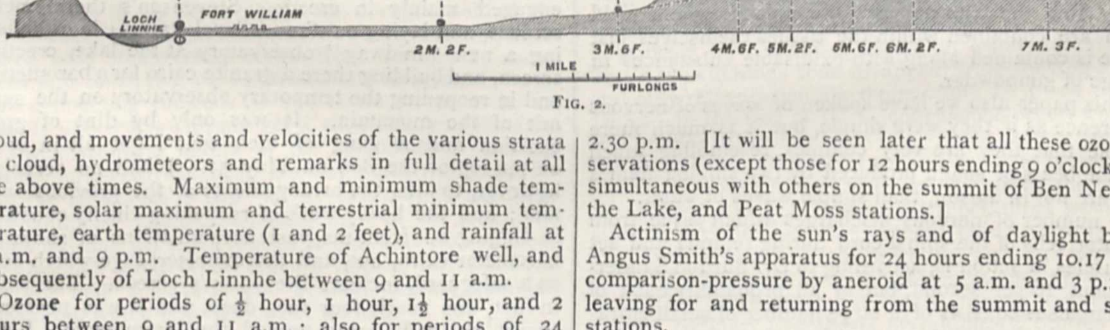


FIG. 2.

cloud, and movements and velocities of the various strata of cloud, hydrometeors and remarks in full detail at all the above times. Maximum and minimum shade temperature, solar maximum and terrestrial minimum temperature, earth temperature (1 and 2 feet), and rainfall at 9 a.m. and 9 p.m. Temperature of Achintore well, and subsequently of Loch Linnhe between 9 and 11 a.m.

Ozone for periods of ½ hour, 1 hour, 1½ hour, and 2 hours between 9 and 11 a.m.; also for periods of 24 and 12 hours, ending 9 a.m. and 9 p.m. Ozone also for the following periods of exposure:—6 hours ending 1 p.m., and 18 hours ending 7 a.m., and subsequently in addition for 15 hours ending 5.30 a.m., and 9 hours ending 2.30 p.m. [It will be seen later that all these ozone observations (except those for 12 hours ending 9 o'clock) were simultaneous with others on the summit of Ben Nevis, at the Lake, and Peat Moss stations.]

Actinism of the sun's rays and of daylight by Dr. Angus Smith's apparatus for 24 hours ending 10.17 a.m.; comparison-pressure by aneroid at 5 a.m. and 3 p.m. on leaving for and returning from the summit and slopes' stations.

PEAT MOSS.—*Position*: About 40 feet above sea; 2m. 2f.; perfectly open; near the middle of the extensive moss at the foot of *Meall an t-Suidhe*; peaty, swampy soil, with hummocks around.

Hours.—5.30 to 6 a.m. (this was the only hour in the entire system that varied, and extra simultaneous read-

[1] Cloud movements and velocities were not, however, noted absolutely *every* time.

ings were taken at Achintore whenever this was the case), and 2.30 p.m. From August 1 also at 9, 9.30, 10, 10.30, and 11 a.m.

Elements.—Temperature of air and evaporation (dry and wet bulbs), wind and force; kind of cloud, amount and velocity; hydrometeors and remarks in full detail as before at all the above times. Pressure by aneroid, 5.30 to 6 a.m., and at 2.30 p.m. Rainfall at 9 a.m. Ozone for 15 hours, ending about 5.30 a.m., and for about 9 hours, ending 2.30 p.m.; also for periods of $\frac{1}{2}$ hour, 1 hour, $1\frac{1}{2}$ hour, and 2 hours between 9 and 11 a.m. (simultaneously with the summit and base stations). Temperature of adjacent water-hole subsequently about 5.30 a.m. and 2.30 p.m.

PEAT MOSS CROSSING.—A minor station about 70 feet above sea, 3m. of., situated at the burn *Allt Coire an Lochain.*

Hours and Elements.—About 5.50 a.m. and 2.17 p.m.: pressure by aneroid, and temperature of burn.

LIVINGSTON'S BOULDER.[1]—*Position:* 840 feet above sea; 3m. 1f.; close to the burn *Allt Coire an Lochain*, on a level swampy patch; ground around undulating, with large boulders of coarse-grained granite lying adjacent.

Hours.—6.15 a.m. and 1.45 p.m.

Elements.—Pressure by aneroid, temperature of air and evaporation (dry and wet bulbs), temperature of burn; wind and force; kind of cloud, amount and velocity; hydrometeors and remarks in full detail as before each time.

THE LAKE.—*Position:* A plateau-valley 1840 feet above the sea, 3 miles, on swampy ground, fairly level, by the north-east shore of the tarn, *Lochan Meall an t-Suidhe:*

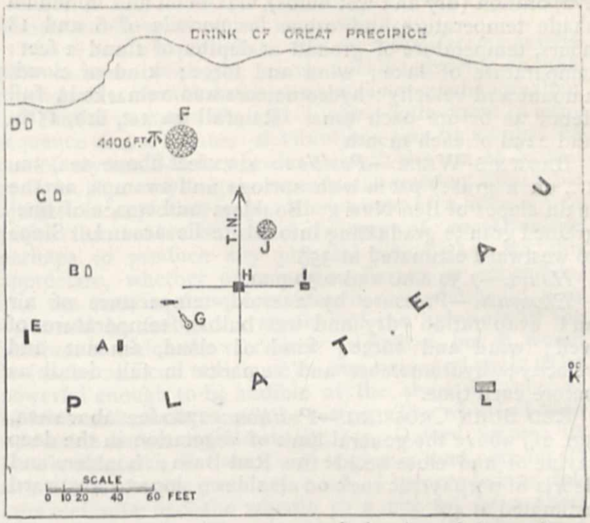
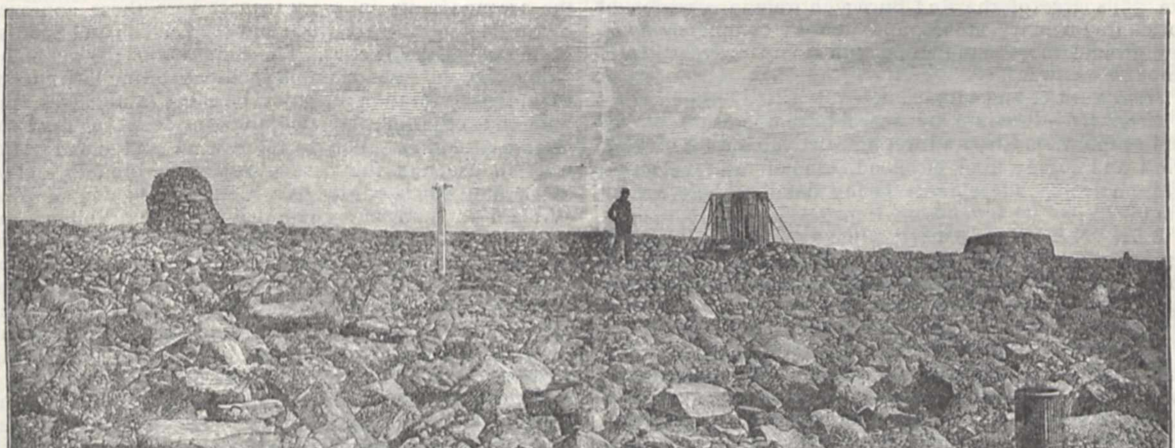


FIG. 3.—A, B, C, D, raingauges; E, notice board; F, Ordnance Survey cairn; G, solar and terrestrial radiation thermometers; H, Stevenson's thermometer cage and ozone tests; I, self-registering hygrometer; J, barometer cairn; K, earth thermometers; L, hut. The apparatus for measuring the actinism of light is near the edge of the precipice N.E. from the hut.



Barometer Cairn. Solar Radiation Thermometer. Thermometer Cage. Hut. Standard Rain-gauge.

FIG. 4.

granite blocks and hummocks of bog, moss, and dwarf-heather to eastward. Main slopes of Ben Nevis on east-south-east side, and *Meall an t-Suidhe* on west side.

Hours.—7 a.m. and 1 p.m.

Elements.—Pressure by mercurial barometer, com-

[1] The altitude of this station and those of the following intermediate stations, except the Lake, were determined by mean results of aneroid readings, and must be accepted accordingly.

parison-pressure by aneroid, temperature of air and evaporation (dry and wet bulbs), maximum and minimum shade temperature, and ozone for periods of 6 and 18 hours, temperature of ground at depths of 1 and 2 feet; temperature of lake; wind and force; kind of cloud, amount and velocity; hydrometeors and remarks in full detail as before each time. Rainfall on 1st, 8th, 15th, and 22nd of each month.

BROWN'S WELL.—*Position:* 2200 feet above sea, 3m. 1f., on a grassy patch with springs and swamps, on the main slopes of Ben Nevis. Boulders and stones of fine-grained granite graduating into felsite lie around. Slope to westward estimated at 35°.

Hours.—7.30 a.m. and 0.30 p.m.

Elements.—Pressure by aneroid, temperature of air and evaporation (dry and wet bulbs), temperature of well; wind and force; kind of cloud, amount and velocity; hydrometeors and remarks in full detail as before each time.

RED BURN CROSSING.—*Position:* 2700 feet above sea, 3m. 2f., above the general limit of vegetation in the deep ravine of and close beside the Red Burn; boulders and debris of porphyritic rock on all sides; slope to westward estimated at 40°.

Hours.—7.55 a.m. and noon.

Elements.—Pressure by aneroid, temperature of air and evaporation (dry and wet bulbs), temperature of burn; wind and force; kind of cloud, amount and velocity; hydrometeors and remarks in full detail as before each time.

BUCHAN'S WELL.—*Position:* 3575 feet above sea, 3m. 5f., source of the Red Burn: entirely in a region of rocks, fragmentary stones, and debris; completely open, and ground more undulating, with comparatively gentle slope to westward.

Hours.—8.30 and 11.30 a.m.

Elements.—Pressure by aneroid, temperature of air and evaporation (dry and wet bulbs), temperature of well; wind and force; kind of cloud, amount and velocity; hydrometeors and remarks in full detail as before each time.

BEN NEVIS, SUMMIT OBSERVATORY.—*Position:* 4406 feet above sea, 4m. 6f., in the centre of a rough rocky plateau, covered with felstone lavas and volcanic agglomerates (see Figs. 3 and 4).

Hours.—9, 9.30, 10, 10.30, and 11 a.m.

Elements.—Pressure by mercurial barometer, comparison-pressure by aneroid, temperature of air and evaporation (dry and wet bulbs), wind and force; kind of cloud, amount, and velocities of strata; hydrometeors and remarks in fullest detail as at the sea-level and intermediate stations at all the above times.

Maximum and minimum shade temperature, solar maximum and terrestrial minimum temperature, and rainfall by four gauges at 9 a.m.

Temperature of Wragge's Well and of ground at depths of 1 and 2 feet between 9 and 11 a.m.

Ozone for periods of ½ hour, 1 hour, 1½ hour, and 2 hours between 9 and 11 a.m., also by two differently exposed tests for 24 hours ending 9 a.m.

Actinism of the sun's rays and of daylight by Dr. Angus Smith's apparatus for 24 hours ending 10.17 a.m.

Hygrometric conditions prevailing about 9 o'clock the previous night by self-registering dry and wet bulbs, were noted at 10.50 a.m.

A moment's consideration, then, will show that the observations at the sea-level station were in every case simultaneous with those at the summit and intermediate stations, and that the hours at the latter were so arranged as to "mean" to the 10 a.m. readings at the base and summit of the mountain, and also at the Peat Moss.

Rainband by Browning's spectroscope was observed at various altitudes, and its indications proved of considerable value. Full notes were taken of the cloud limits, and of any important changes observed between the stations.

Of course my first business was to get the main observations—pressure, temperature, hygrometric conditions, wind, cloud, &c.—into full swing by June 1; and as I felt my way and got my hours and distances well under command I added to my work. Thus the ozone observing-system and the three extra rain-gauges on the summit were added on June 15, and the delicate operations for measuring the actinism of light on July 9. The additional gauges were established to discover if and to what extent the rainfall varies in connection with the wind at different points of the plateau from the centre to the edge of the great precipice.

During June, Stevenson's screens were in use only at the sea-level, lake, and summit; and hence at the other places the dry and wet bulbs had to be swung and the latter moistened afresh from adjacent water at each swinging. But aching wrists and sore fingers soon made me determine to have louvred screens at all the stations, and by July 1 they were in their places and dry and wet bulbs supplied by Hicks and Negretti and Zambra fixed permanently in each. So above all was accuracy the better insured, and the whole system went like clockwork. I left Achintore before 5.30 a.m., and returned about 3 p.m., and the rate of ascending and descending was so regulated as to insure punctuality usually within a few seconds—often to the second—at the various stations.

The new screens were a trifle smaller than the others. I need hardly add that the instruments at all stations were the best observing-standards procurable, and that the arrangements in every respect were those approved by the Meteorological Office and the English and Scottish Meteorological Societies. The condition of the wet bulbs, fixing of ozone tests, clamping self-registering instruments to prevent vibration in gales, levelling rain-gauges, and numerous other matters of important detail required the closest attention. The Beaufort wind and cloud scales were in use, and the ozone tests were Moffat's. Two assistants, educated by Mr. Colin Livingston of Fort William—a sufficient guarantee for their ability—and trained by myself, helped in the work; and relieved me in the ascent of the mountain three times a week, and on these occasions I took the sea-level station. One of the greatest difficulties I had to contend with in the Ben Nevis routine was as to the pony on which I rode to and from the Lake, where it was left to graze and await my descent. Occasionally the stable-boy overslept, and I had to make up for lost time,—no easy matter, as the wretched track leads over deep ruts and treacherous swamps, and the poor brute had a trying time of it. Still more frequently the person to whom it belonged gave me rotten saddlery in spite of all remonstrance; and on commencing the ascent the girth would break, and I had to turn the animal adrift and plod on to the Lake my fastest. This was decidedly hard, inasmuch as I was obliged to climb afoot some 2500 feet from the tarn in less than two hours by a circuitous route and over rough rock stopping to observe at the other intermediate stations. Again, the pony often wandered in his hobbles or having broken the tethering rope had made off to the moss; so also on the homeward journey I had sometimes to leave him and run my hardest over ruts and through swamps, by a short cut, to get my readings at the next station. Other trying parts of the work consisted in the journeys between Buchan's Well and the top in the allotted time, in having the two hours' exposure on the summit in bad weather, and in becoming chilled after profuse perspiration. The rude hut, with its walls full of holes of all shapes and angles through which the wind whistles and the snow-drifts drive, afforded but a poor shelter from the drenching rain and cold, and it was impossible to keep anything dry. My hands often became so numbed and swollen, and my paper so saturated that I had the utmost difficulty in handling keys, setting instruments and entering my observations. Usually so laden was the air with moisture and so very dense and lasting was the cloud-fog that, even when no rain had actually fallen, all the fixings and instruments were dripping; and although, of course, I made a point of wiping the dry bulb, it almost immediately became wet again. Occasionally I timed the interval between wiping and fresh condensation on the bulb, and have found it wet again within *thirty seconds*.

After November 1, then, I had to discontinue the work. The hut had become choked with snow, and the carrying on of the undertaking satisfactorily impossible. I was, however, satisfied; and very pleased that I had secured five months' observations without the break of a single day.

It is not my part to refer in this paper to any results. Such I must in duty leave to be discussed and made known by the Scottish Meteorological Society. But, from what I myself know of the meteorology of Ben Nevis from the experience of two summers and autumns, I do most strongly urge the establishment of a permanent observatory on the summit, firmly believing that most important and unexpected results will accrue to meteorology from continuous observations there. Such, in connection with others at the sea-level, would in my opinion enable the energetic staff at the Meteorological Office, under Mr. Robert H. Scott's able direction, to forecast storms with far greater certainty.

I cannot conclude this account without expressing my best acknowledgments to Dr. Angus Smith for placing at my disposal his apparatus for measuring the actinism of light, which I consider an immense acquisition to a meteorological observatory; to Mr. John Browning for his rainband spectroscope; to Messrs. Negretti and Zambra for their clock-hygrometer; and finally to the Scottish Meteorological Society for the kind encouragement and liberal assistance they have given me.

CLEMENT LINDLEY WRAGGE

HYDROGEN WHISTLES

IT may be recollected by some of the readers of NATURE that a few years ago[1] I contrived a whistle for testing the upper limits of the power of hearing very shrill notes by different men and animals. When properly made, it easily suffices to do this, in the case of men and most animals, but it cannot, neither can any other instrument hitherto devised, emit such notes as it is conceivable that insects may hear. The problem whether any insects can hear notes whose numbers of vibrations per second is manyfold greater than those of the notes audible to men has not yet been fairly put to the test of experiment. I wish to show that this can now be done.

The whistles of which I speak have their lower ends closed with a piston that admits of being inserted more or less deeply, and thus of varying the depth of the whistle and consequently its note; but as a whistle will not give its proper note unless its depth be greater than its width, say, $1\frac{1}{2}$ times as much, and as the depth of a whistle that gives, say, 24,000 vibrations per second is is only 0·14 inch, it follows that their bores must be very small, and that a limit of minuteness is soon reached.

Having had occasion lately to reconsider the subject, it occurred to me that I could greatly increase the shrillness of any whistle by blowing a gas through it that was lighter than common air.

The number of vibrations per second caused by whistles is inversely proportional to the specific gravity of the gas that is blown through them; therefore by the use of hydrogen, which is thirteen times lighter than air, the number of vibrations per second produced by a given whistle would be increased thirteenfold.

I have made experiments with most satisfactory results with common coal gas, whose specific gravity, though much greater than that of hydrogen, is not much more than half that of common air, and I have little doubt in consequence that a number of vibrations may be excited by one of my small-bore whistles through the use of hydrogen gas, that very largely exceeds the number attainable hitherto in any other way. They would of course fail to excite the sense of sound in any of ourselves, or perhaps to produce any physical effect that we can appreciate, whether on sensitive flames or otherwise, and the note to those creatures, if any, who could hear it, would be feebler on account of the lightness of the medium in which the vibrations originated, but it would be (so far as I can anticipate) a true note, and ought to be powerful enough to be audible at the short distances at which small creatures may be tested. The whistle I used was made for me by Hawksley, 357, Oxford Street; its bore is 0·04 inch diameter, and it gives a loud note for its size. After some prefatory trials, I proceeded as follows:—I attached the whistle to a gas jet by a short indiarubber tube. Then, without turning on the gas, I retested my range of hearing by setting the piston at various lengths and giving sharp squeezes to the tube as it lay in the hollow of my hand. The effect of each squeeze was to force a little air through the whistle, and to cause it to emit a sharp "cheep." When I relaxed the grasp, air was sucked in through the whistle, and the tube became again filled with air, ready for another squeeze.

My range of hearing proved to be such that when the depth of the whistle was 0·13 inch, I could hear no musical note at all—only a puff; at 0·14 inch I could just perceive a very faint musical note enveloped, as it were, in much puff; even at 0·20 some little puff remained, but before 0·25 the note had become purely musical. This having been established, I kept the whistle set at 0·25 and turned the gas on, giving it abundance of time to expel all air from the tube. Then, turning the stopcock to shut the indiarubber tube from behind, I gave a sharp squeeze as previously, but the whistle, instead of emitting a pure note, gave to me just the same barely perceptible sound that it did when it was set at 0·14. I relaxed my grasp and instantly retightened it, and then the whistle emitted a pure note. A little common air had regurgitated into the whistle when my grasp was relaxed, and it was the reissue of this that gave the note. I repeated the experiment several times with the same result. With a depth of 0·24 I could hear no note at all when using the gas. Then I pulled out the piston to 0·35, and the gas gave a clear musical note; on the second squeeze the note was considerably deepened. The specific gravity of the gas from the jet, as calculated from these data, would be to that of the air at the time, as 14 to 25, or as 0·56 to 1. This happens to be the specific gravity of carburetted hydrogen, but that of common street gas is heavier. Perhaps my measurements were not quite accurate; probably the note given by the gas being really fainter (though not perceptibly so) than that given by air somewhat falsified the judgment. A very slight difference in the data would raise the 0·56 to 0·60 or more.

By the use of hydrogen the little whistle when set at 0·14 inches would produce 312,000 vibrations per second. I know by experiment on others that it will give a true musical note when made much shorter than this, and I see no cause to doubt that it will sound truly at half the above length, and therefore be capable of emitting twice the above enormous number of vibrations per second.

Mr. Hawksley is making for me an apparatus with small gas bag for hydrogen pure or diluted, valves, and an indiarubber ball to squeeze, to enable hydrogen to be used with the whistle when desired. The whistle is

[1] "South Kensington Conferences, in connection with Loan Exhibition of Scientific Apparatus, 1876," p. 61.

fixed to the end of a small indiarubber tube in order to be laid near the insect whose notice it may be desired to attract. FRANCIS GALTON

PRELIMINARY NOTE ON THE BACILLUS OF TUBERCULOSIS (KOCH)

I. THE absorption and consequent retention of certain stains by this bacillus does not appear to be effected by the hydrates of potassium, sodium, and ammonium and by aniline alone. Sodic phosphate, potassic acetate, vegetable alkaloids, &c., appear to exert a similar action. Further experiments are in progress. I have some very good preparations which were rapidly stained with a *very faintly* coloured stain containing sodic phosphate (sod. phos. cryst. B. P.).

II. The sections of tissue shown (by the kind arrangement of Mr. Blaker) at the Brighton meeting of the British Medical Association, in which the bacilli were very distinct, were stained, &c., then floated on to the glass slides, *dried over concentrated sulphuric acid* (or fused $CaCl_2$), and mounted in balsam. Hitherto my attempts to fix the colour of the bacilli, by means of a mordant, in such a way that it might remain unaffected by alcohol, and by oil of cloves, have not proved successful.

III. Treatment with a solution of potassic acetate will probably prove well adapted to free preparations from those last traces of nitric acid which so often cause their ultimate destruction.

From (II.) I should omit a very beautiful and remarkable preparation showing the spores of this bacillus in the lymphatics of the lung. This slide was prepared by Dr. Barron, of University College, Liverpool, and for his kindness in lending it to me and for much invaluable advice I am very grateful.

To Mr. Blaker, M.R.C.S., of Brighton, and to Mr. Black, M.R.C.S., of the Sussex County Hospital, I am under many obligations for their kindly interest and assistance. J. W. CLARK

THE SHAPES OF LEAVES[1]

III.—*Origin of Types*

THE two most general and distinctive types of foliage among angiosperms are those characteristic of monocotyledons and dicotyledons respectively. They owe their principal traits of shape and venation to the manner in which these two great fundamental classes have been separately evolved from lower ancestors.

Mr. Herbert Spencer has shown that there are two chief ways in which a central axis or caulome may conceivably be developed from an integrated series of primitive stalkless creeping fronds. The *first* way is by the in-rolling or folding of the fronds so as to form a complete tube, often with adnate edges, as represented in the accompanying diagram (Fig. 20), modified by Mr. Spencer's kind permission from the "Principles of Biology." For details of the explanation, the reader must be referred to that work (vol. ii. part iv. chap. iii.); it must suffice here to note that as in such case each frond must envelop the younger fronds within it, the process is there shown to eventuate in an endogenous stem and a monocotyledonous seed—two characteristics found as a matter of fact constantly to accompany one another in actual nature. The *second* way is by the thickening and hardening of a fixed series of midribs, as shown in the next diagram (Fig. 21), also modified after Mr. Spencer; and this method must necessarily result in an exogenous stem and a dicotyledonous seed. The diagrams in Figs. 22 and 23, which represent according to Mr. Spencer (slightly altered) the development of the monocotyledonous and dicotyledonous seedling respectively, will help further to illustrate the primitive characteristics of the two types.

The monocotyledonous type of foliage is for the most part extremely uniform and consistent, in temperate climates at least, for in the tropics the presence of large arborescent forms, such as palms and screw-pines, as well as of gigantic lilies, amaryllids, and grasses, such as the bananas, yuccas, agaves, and bamboos, gives a very distinctive aspect to the *ensemble* of the class. Being in principle a more or less in-rolled and folded frond, every part of which equally aids in forming the caulome or stem, the monocotyledonous leaf tends as a rule to show little distinction between blade and leaf-stalk, lamina and petiole. For the same reason, the free end also tends to assume a lanceolate or linear shape, while the lower part usually becomes more or less tubular or sheathing in arrangement. Again, for two reasons, it generally has a parallel venation. In the first place, since the leaves or terminal expansions are mere prolongations or tips to the stem-forming portion, it will follow that the vascular tissues will tend to run on continuously over every part, instead of radiating from a centre which must in such a case be purely artificial. In the second place it is clear that parallel venation is the most convenient type for long narrow leaves, as is plainly shown even among dicotyledons by such foliage as that of the plantains, descended from netted-veined ancestors, but with chief ribs now parallel. Still better are both these principles illustrated in those cases among dicotyledons where the lamina is suppressed altogether, and the flattened petiole assumes foliar functions, as in *Oxalis bupleurifolia* and *Acacia melanoxylon* (Fig. 24). These phyllodes thus resembling in their mode of development the monocotyledonous type, and continuous throughout with the caulome-portion of the primitive leaf, exhibit both in shape and venation the chief monocotyledonous characteristics. A typical monocotyledon in shape and venation is represented in Fig. 25.

The dicotyledonous type, though far more varied, is equally due in its shape and venation to the original characteristics implied by its origin. Only the midrib instead of the whole leaf being here concerned in the production of the stem, there is a far greater tendency to distinctness between petiole and lamina, and a marked preference for the netted venation. The foliar expansion is not here a mere tip; it becomes a more separate and decided element in the entire leaf. And as the petiole joins the lamina at a distinct and noticeable point, there is a natural tendency for the vascular bundles to diverge there, making the venation palmate or radiating, so as to distribute it equally to all parts of the expanded surface. Fig. 26 shows the resulting characteristic form of dicotyledonous leaf. Its variations of pinnate or other venation will be considered a little later on.

Among monocotyledons, the central type is perhaps best found in the mainly tuberous or bulbous orders, such as the orchids, lilies, and amaryllids. These orders, having rich reservoirs of food laid by underground, send up relatively thick and sturdy leaves; but their shape is decided by the ancestral type, and by their strict subordination to the central axis. Hence they are usually long, narrow, and rather fleshy. Familiar examples are the tulips, hyacinths, snowdrops, daffodils, crocuses, &c. Those which have small bulbs, or none, or grow much among grass, like Sisyrinchium, are nearly or quite linear; those which raise their heads higher into the open, like Listera, are often quite ovate. Exotic forms (bromelias, yuccas, agaves) frequently have the points sharp and piercing, as a protection against herbivores. In the grasses there is generally no large reservoir of food, and their leaves accordingly show the central type in a stringy drawn-up condition. So also in sedges, woodrushes, and many others. But where the general monocotyledonous habit has been more lost, and something

[1] Continued from p. 466.

like the dicotyledonous habit acquired, the leaves become more like those of the opposite class. Thus the Arums, with their very unlilylike mode of growth, and their long petioles rising high into the open air, have usually a very distinct broad lamina, and have the veins accordingly branched or netted, almost as in dicotyledons. Very much the same type recurs under similar circumstances in *Sagittaria sagittifolia* (Fig. 27). Still more markedly dicotyledonous-looking are the leaves of certain very aberrant Amaryllids, such as *Tamus* and the other Dioscorideæ, which have taken to climbing, and have therefore acquired broader leaves with netted veins between the

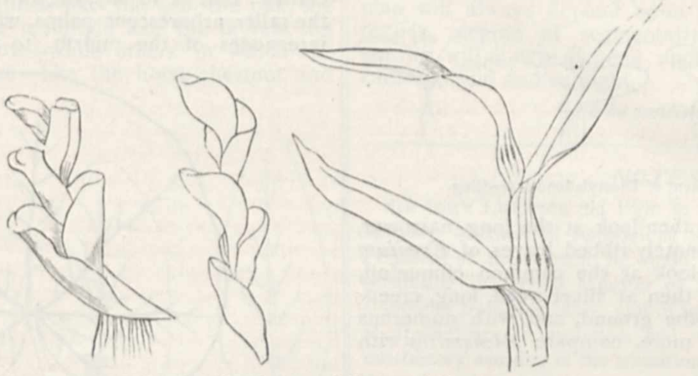


FIG. 20.—Developmen of Monocotyledonous stem.

ribs. Compare with these the like result in *Smilax;* and then look at both side by side with such dicotyledons as *Convolvulus.* The influence of the ancestral type is here seen in the arrangement of the main ribs; the influence of environment is shown both in the approximation of general shape, and in the netting of the minor veins. Once more, the ovate type of Listera leads on readily enough to the whorled leaves of *Paris* and *Trillium,* where the venation has become similarly netted. A bushy type, like *Ruscus,* develops broad leaf-like peduncles, which closely simulate the true leaves of dicotyledonous bushes with like habit, such as box or privet.

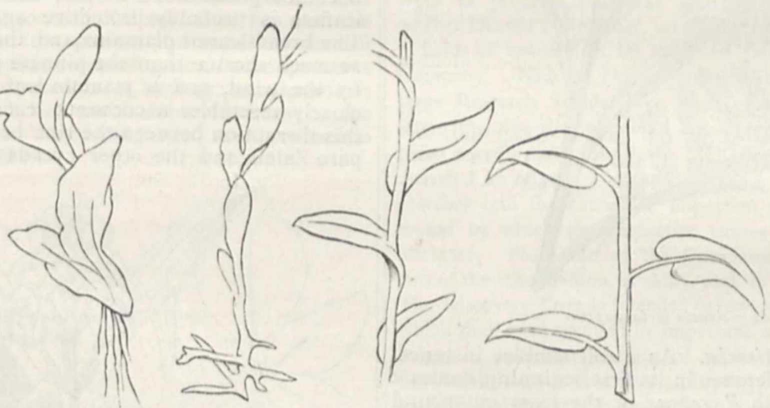


FIG. 21.—Development of Dicotyledonous stem.

But the widest departure of all from the central monocotyledonous type is found in leaves like those of the tropical arborescent forms—the palms, screw-pines, &c. Most of these have long pinnate foliage, whose origin may best be considered when we come to examine the

FIG. 22.—Development of Monocotyledonous seedling.

chief dicotyledonous types; meanwhile such forms as the cocoanut or the date-palm may be advantageously compared, as to conditions and general shape, with the tree-ferns in one direction, and the cycads in another. The bananas cast much analogous light upon the origin of these tropical pinnate forms. Where the plant is less arborescent, as in *Chamærops,* the leaf assumes rather a fan-shaped than a pinnate development.

Among dicotyledons it may be fairly assumed that the earliest form of leaf was simple, ovate, and nearly ribless, or with faint digitate venation. This is shown both by the nature of the earliest leaves in most seedlings, and the constant recurrence to such a type wherever circumstances are favourable for its reproduction. Hence, as a whole, digitate venation seems the commonest in most humble dicotyledons; and the only problem is how pinnate venation came to be substituted for it in certain cases. The answer seems to be that wherever circumstances have caused leaves to lengthen faster than they broadened, and so to assume a lanceolate rather than an ovate shape, the tendency has been for the main ribs to be given off, not from the same point, but a little in front of one another. If the technical botanists will pardon such a phrase, the internodes of the midrib, usually suppressed, seem here to have been fully developed. Figs. 28, 29, and 30 show the stages by which such a change may be brought about. Figs. 31, 32, and 33 exhibit a slightly different form of the same tendency.

That this is the real origin of pinnate venation seems pretty clear on a comparison of a good many otherwise closely related forms. Look for example first at the rounded, almost orbicular leaf of *Geranium molle* and its allies,

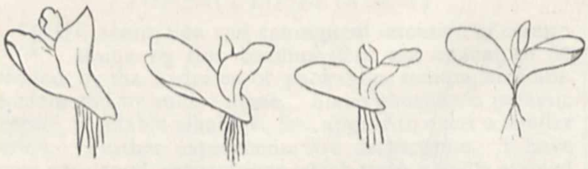


FIG. 23.—Development of Dicotyledonous seedling.

with palmate ribs; and then look at the long, narrower, doubly pinnate, and pinnately-ribbed leaves of *Erodium cicutarium*. Or again, look at the common cinquefoil, erect and palmate; and then at silver-weed, long, creeping, closely pressed to the ground, and with numerous pinnate leaflets. Once more, compare *Alchemilla* with

FIG. 24.—*Acacia melanoxylon*.

Poterium and *Sanguisorba*. As a still simpler instance, where we get the difference in its first beginning, contrast *Ranunculus acris* with *R. repens*, or the least compound leaves of the blackberry bramble with its own most compound foliage. As a rule the most pinnate groups, such

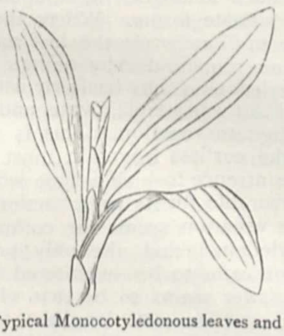


FIG. 25.—Typical Monocotyledonous leaves and venation.

as the lesser crucifers, the peaflowers, &c., have very long leaves.

This suggested origin of pinnate venation in dicotyledons becomes even more probable when we look at the pinnate members of other classes. Among monocotyledons the long-leaved arums, though their venation is fundamentally parallel in type, have yet acquired a branching and practically pinnate set of ribs. The plantains and bananas, with very long and broad foliage, carry the same tendency yet further; for their leaves are pinnately ribbed from a stout midrib. The lower shrubby or bushy palms, like Chamærops, have fan-shaped leaves, with veins diverging in rough parallelism from a common centre; that is to say, they are in fact palmate; but in the taller arborescent palms, with their long leaves, the internodes of the midrib (to use the same convenient

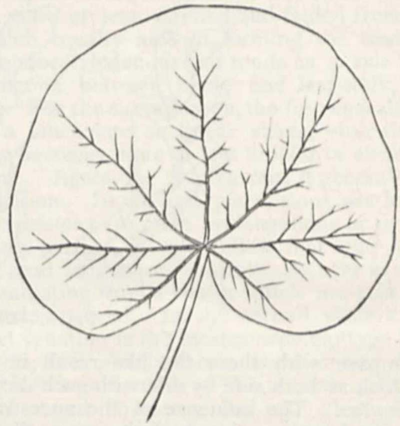


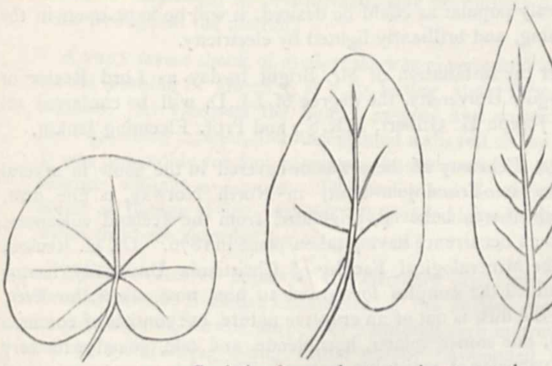
FIG. 26.—Typical Dicotyledonous leaf and venation.

phrase once more) are fully developed, so that the leaf becomes pinnatifid. In this case the subdivision into leaflets is probably protective against tropical storms. The broad-leaved plantains and the Chamærops, though so much shorter than the pinnate palms, are often torn by the wind, and a plantain leaf so torn into ribbons closely resembles a cocoanut leaf: in the taller palms this disruption between the ribs becomes normal. Compare Zamia and the other cycads among gymnosperms.

FIG. 27.—*Sagittaria sagittifolia*.

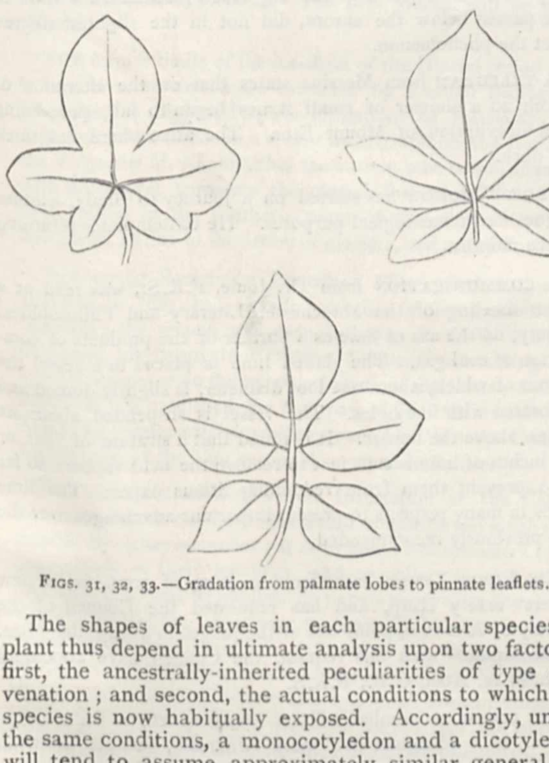
Once more, the ferns are a class with long lanceolate fronds as a rule, and their venation is almost always pinnate; the only ferns that vary much from the central type being some like the Maidenhairs, which are tufty and rather ovate in general form, and have so modified their venation as closely to approach the herb Roberts and other hedgerow plants in the outer effect. We may fairly conclude, therefore, that pinnate venation is best adapted to very long leaves, both because of the support it gives to the cellular mass and because of the easy manner in which it distributes sap to every part alike.

It seems also probable that pinnate ribs are especially adapted to forest trees. Most of these indeed have their leaves rather long in outline—like the ash, the oak, the chestnut, the walnut, the mountain ash, the laurels, the hornbeam, and the willow—while others in which the primary ribs are palmate—like the horse-chestnut and



FIGS. 28, 29, 30.—Gradation from palmate to pinnate venation.

the plane—have their secondary ribs pinnate and their lobes or leaflets very long, so that the total effect is in the end pretty much the same. But even when the leaf is rather shortened in general outline, as in the elm, the beech, the alder, and the poplar, the venation is still pinnate. Doubtless this form of ground-plan protects the leaves of these exposed trees best against the wind; and where the leaflets are much subdivided, as in the acacias, the subdivision may be regarded as a protection against severe storms.



FIGS. 31, 32, 33.—Gradation from palmate lobes to pinnate leaflets.

The shapes of leaves in each particular species of plant thus depend in ultimate analysis upon two factors: first, the ancestrally-inherited peculiarities of type and venation; and second, the actual conditions to which the species is now habitually exposed. Accordingly, under the same conditions, a monocotyledon and a dicotyledon will tend to assume approximately similar general external forms; but their underlying ancestral peculiarities may generally be perceived through the mere analogical resemblance produced by an identical environment. By the interaction of the two factors we must endeavour to explain every particular form of leaf. To do this throughout the whole vegetable kingdom would be of course an endless task, but to do it in a few selected groups is both a practicable and a useful botanical study. The ground-plan will always depend upon the ancestral type; the outline, degree of segmentation, and minuteness of cutting, will always depend upon the average supply of carbonic acid and sunlight. GRANT ALLEN

(*To be continued.*)

NOTES

SIR JOHN LUBBOCK did right to ask the Prime Minister on Monday, whether, in remodelling the department of the Lord President of the Council, he would consider the desirability of separating the actual Minister of Education in the House of Commons from that office, and of transferring to him the power of appointing the inspectors and other officers on whom the satisfactory working of the education of the country so greatly depends. As might have been expected, Mr. Gladstone held out no hope of any change being made for a long time; that, however, is no reason why the efforts of the friends of science and education in this direction should cease.

THE Grocers' Company have issued a scheme for the encouragement of original research in sanitary science. It consists of two forms of endowment: the one, meant as maintenance for work in progress, in fields of research to be chosen by the worker himself; the other, meant as reward for actual discovery in fields of research to be specified from time to time by the Company. With the former intention the Company establishes three Research Scholarships, each of 250*l.* a year; with the latter intention they appoint a Discovery Prize of 1000*l.*, to be given once in every four years. The Research Scholarships are intended as stipends for persons engaged in making exact researches into the causes of important diseases, and into the means by which the respective causes may be prevented or obviated. The Court of the Company propose to appoint to two of the scholarships in May, and to a third in May, 1884. The Discovery Prize is intended to reward original investigations, which shall have resulted in important additions to exact knowledge, in particular sections of sanitary subject-matter. The Court will, once in four years, propose some subject for investigation; and the first subject will be announced in May.

THE Annual Report of the City and Guilds of London Institute, taken in conjunction with the Annual Meeting held last week, shows that technical education has taken firm root and is making rapid progress in this country. Though hardly yet so universal as on the Continent, there is every reason to believe that it soon will be, and Lord Selborne, who presided at the Annual Meeting, was justified in congratulating the Institute on its success. As the *Times*, in a sensible article on the Annual Meeting, says: "Lord Selborne did not dwell at length upon the general aspects of technical education. He assumed, and he had good reason to assume, that the need for a systematic development of it is proved beyond question, and is almost universally accepted. No observer now doubts that if the English artisan is to hold his ground in the struggle for existence, he must be kept up to the mark by proper teaching; and no one who has at heart the moral well-being of the working classes doubts the enormous importance of giving them an insight into principles and processes which will raise their work as much as possible out of the mere mechanical groove."

THE following are the arrangements for the lectures after Easter at the Royal Institution:—Prof. J. G. McKendrick, ten lectures on physiological discovery; Dr. Waldstein, four lectures on the art of Pheidias; Prof. Tyndall, three lectures on Count Rumford, originator of the Royal Institution; Mr. R. J. Poole, three lectures on recent discoveries in (1) Egypt, (2) Chaldæa and Assyria, (3) Cyprus and Asia Minor; Mr. A. Geikie, six lectures on geographical evolution; and Prof. C. E. Turner, four lectures on historical sketches of Russian social life. The discourses on the Friday evenings will be as follows:—April 6, Dr. Archibald Geikie, F.R.S., The Cañons of the Far West; April 13, Dr. Waldstein, The influence of athletic games on Greek art; April 20, Prof. Bayley Balfour, The island of Socotra and its recent revelations; April 27, Dr. C. William Siemens, F.R.S., Some of the questions involved in solar physics; May 4, Robert H. Scott, F.R.S., Weather knowledge in 1883; May 11, Prof. Huxley, V.P.R.S., Oysters and the oyster question; May 18, Prof. C. E. Turner, of the University of St. Petersburg, Kustarnoe proiezvodstro: or, the peculiar system of domestic industry in the villages of Russia; May 25, Prof. Flower, F.R.S., Whales, present and past, and their probable origin; June 1, Frederick Pollock, LL.D., The sword: its forms and its history; June 8, Prof. Dewar, F.R.S.

IN reference to the course of ten lectures on physiological discovery, to be given at the Royal Institution on Tuesdays, beginning April 3, by Prof. J. G. McKendrick, we may say that the object of the course will be to trace the progress of physiological research from about the beginning of the sixteenth century to recent times, and more especially along those lines which have led to great results. Admitting that the deepest foundation of physiological science is a knowledge of structure, both of organ and of tissue, it will be the aim to show how physiology has gradually attempted to solve some of its problems by the methods of physics and of chemistry, and has thus become a branch of experimental science. The method followed will be to describe briefly the lives of the great discoverers, to indicate the influence of contemporary science on their ideas and opinions, and to show how their labours have brought us to our present position. As far as possible, the fundamental experiments of discoverers will be shown or illustrated, and these will be compared with present methods.

BARON NORDENSKJÖLD, having inspected the Royal Mail Steamer *Sophia*, which the Government have asked the Swedish Parliament to lend for his expedition to Greenland, finds that the vessel is not large enough to carry the quantities of coals and provisions required, although very suitable in other respects. He has therefore decided that a vessel shall be despatched from Denmark with these requisites, and depots established in convenient places on the coast. The *Sophia* will be overhauled and fitted for her voyage in Gothenburg, and as her commander Capt. Nilsson, of that city, has been selected.

THE position of the Lena Meteorological Station is 73° 22′ 30″ N. lat. and 126° 34′ 55″ E. long. The house erected there for the observers is reported to be quite comfortable, and the health of the expedition is satisfactory.

THE group of fishing Chukches, which Baron Nordenskjöld has prepared from materials collected in the *Vega* expedition for the coming Fisheries Exhibition, is now on view in Stockholm.

WITH the completion of the buildings in which the varied collection of the great International Fisheries Exhibition is to be housed, the preparatory work of the executive committee is drawing to an end. Not much remains to be done to the buildings which now almost cover the southern half of the Horticultural Society's gardens, and the nature and distribution of the exhibits may now be approximately given. Before handing over to the care of representatives of the colonies and of foreign Powers the places allotted to their countries, the committee on Friday invited members of both Houses of Parliament and their friends to see the buildings. To add to the interest of the aquaria, Lord Walsingham has offered to let off a lake, of about seventy-two acres in extent, on his estate at Merton, in Norfolk, and to send all the fish worth forwarding alive, and besides pike, perch, tench, and other coarse fish, he promises 1000 specimens of the celebrated golden tench. Additional value will be given to the natural history department by the exhibition in a building near the new Natural History Museum of the fine collection of fish preserved in spirit now to be brought from Bloomsbury. In order to make tne exhibition as truly popular as could be desired, it will be kept open in the evening, and brilliantly lighted by electricity.

AT the installation of Mr. Bright to-day as Lord Rector of Glasgow University, the degree of LL.D. will be conferred on Dr. Joseph H. Gilbert, F.R.S., and Prof. Fleeming Jenkin.

ON February 26 there was discovered in the snow in several places in Trondhjem Amt, in North Norway, a fine dust, which, it was believed, originated from the Iceland volcanoes, such an occurrence having taken place in 1876. Dr. H. Reusch, of the Mineralogical Faculty of Christiania University, having examined the samples forwarded to him, now states, however, that the dust is not of an eruptive nature, but consists of common sand, fine stones, quartz, hornblende, and talc, mixed with very fine particles of vegetable matter. The phenomenon is nevertheless very remarkable, as the dust must have been carried a very long distance, the whole of the country having for months been covered with deep snow. The dust fell over a district of several degrees. The wind blew strongly from north-northwest.

ON the night of the 4th inst. there was observed in Gestrikeland, in Sweden, a display of aurora borealis, the extent, vividness, and magnitude of which, it is reported, has not been observed in that country for years. An interesting feature of the phenomenon was that the big clouds, which from time to time passed below the aurora, did not in the slightest degree affect the phenomenon.

A TELEGRAM from Messina states that on the afternoon of March 20 a shower of small stones began to fall, proceeding from an eruption of Mount Etna. The atmosphere was thick and dark.

PROF. VIRCHOW has started on a journey to Sicily, whither he goes for archæological purposes. He contemplates returning in two months.

A COMMUNICATION from Dr. Joule, F.R.S., was read at a recent meeting of the Manchester Literary and Philosophical Society, on the use of lime as a purifier of the products of combustion of coal gas. The slaked lime is placed in a vessel the bottom of which, about one foot diameter, is slightly domed and perforated with fine holes. The vessel is suspended about six inches above the burner. It is found that a stratum of four or five inches of lime is sufficient to remove the acid vapours so far as to prevent them from reddening litmus paper. The lime seems in many respects to present important advantages over the zinc previously recommended.

MR. ELLIS LEVER has offered a prize of 500*l*. for a new miners' safety lamp, and has requested the Council of the Society of Arts to appoint one of the judges to award the prize. In accordance with this request, the Council have appointed Prof. F. A. Abel, C.B., F.R.S.

AN enormous aërolite fell on February 16, a little before 3 p.m., in a ploughed field near Alfianello, between Cremona and Brescia, sinking more than one metre in the ground, and producing a rumbling noise, heard twenty kilometres off, and a reeling of the nearest houses as by an earthquake. Unhappily the ignorant country people, when the first fright passed, with mattocks and sticks smashed it and took away the pieces, so that Prof. Calderoni, who directly ran up from Cremona, could obtain only some little fragments for chemical analysis and for scientific cabinets.

A SCHEME is proposed for introducing electric lighting into the Canton of Vaud. The motive force would be derived from turbines of 5000 horse-power at Vallorbes, and the water supply being constant and abundant, it is believed that gas, which is very costly in Switzerland, may be entirely dispensed with throughout the district.

A VERY severe shock of earthquake was experienced in Cyprus on the morning of March 5, at 7.30, lasting about fifty or sixty seconds. At Limassol the houses swayed and rocked in the most appalling manner, and uncemented walls fell to the ground. It was impossible for foot passengers in the streets to keep their balance without assistance. The mules and horses staggered about as though in fits. It was altogether the severest shock which has been recorded for many years.

WE have received copies of the circulars just issued by the Local Scientific Societies Committee of the British Association to 324 societies, for the purpose of obtaining such information as will be useful in suggesting further action. Appended is a list of about 120 local societies which publish Proceedings.

THE Easter excursion of the Geologists' Association will be to Hythe, Romney Marsh, Sandgate, and Folkestone (March 26 and 27). On April 7 the Association will visit Westcombe Park, Greenwich; on April 14 the College of Surgeons; and on April 21 Berkhampstead and Boxmoor.

WE understand that a new weekly journal, devoted to the popular exposition of sanitary matters and to the education of the people in the laws of health, will be shortly issued by Messrs. Wyman and Sons, London. The new journal will be entitled *Health*.

THE former limits of the ice-sheet of the Glacial period appear to be still more and more extended by Russian geologists, in proportion as the post-Pliocene formations of Russia are better explored. We notice in a recent monograph on the Geology of the Volga, by M. Krotoff, that the author, who is well acquainted with this region, considers the glacial formations described by Prof. Miller in the southern parts of the province of Nijni-Novgorod, as due to the action of glaciers, and not of floating ice.

THE young Society for Caucasian History and Archæology, founded in 1881, has already published a first fascicule of its *Bulletin*; the second will soon follow. Prof. V. Miller has published his linguistic "Osetian Studies," containing in the appendix a paper on the religious beliefs of the Osets; and Prof. Patkanoff has published the first part of his "Materials for an Armenian Dictionary," as well as a pamphlet "On the Cuneiform Inscriptions of the Van system discovered in Russia."

THE Administration of Public Instruction of the Caucasus has conceived an excellent idea which cannot be too much recommended for other countries; it is to invite schoolmasters to write descriptions of their localities, and to collect local traditions, folk-lore, &c., and to publish the papers received in the shape of a special collection. It is easy to conceive, indeed, the amount of knowledge which might be gathered in this way, and the attraction which is thrown by a scientific pursuit into the wearisome life of a schoolmaster, who is lost in a small town or village, far from intellectual centres. When he knows that his work will not be lost, and when he is supplied from an intellectual centre with the scientific works he needs, he surely will find interest in his pursuit. This of course applies more to Russia than England. The two first parts of the collection thus started on the Caucasus wholly confirm these previsions; as is seen from an analysis of them published in the *Izvestia*, they contain, indeed, much valuable information. The descriptions of Erivan, Gori, Wakhichevan with its district, and of Chernolyesskoye village are spoken of as very useful work. Two papers, on the formation of Lake Paleostome, and a summary of all places where the Caucasus is mentioned by the ancients, are very elaborate; whilst a series of smaller papers and notes contains a variety of ethnographical sketches, folk-lore, and traditions.

LAMPART AND CO. of Augsburg are issuing in parts a third revised edition of Hellwald's "Kulturgeschichte in Ihrer Näturlichen Entwicklung bis zur Gegenwart." Trübner and Co. are the London publishers. The work will be completed in twenty parts.

AT the last meeting of the Meteorological Society of France, M. Moureaux, physicist to the Bureau Central, read a paper showing that the regimen of the rains south of the Central Plateau was independent of the meteorological conditions on the oceanic side. This communication is considered as an argument in favour of granting to the Bureau Météorologique of Algiers the privilege of being in direct communication with the other offices, and issuing warnings for the northern side of the Mediterranean.

THE additions to the Zoological Society's Gardens during the past week include a Common Seal (*Phoca vitulina*), British Seas, presented by Mr. William Whiteley; a Common Squirrel (*Sciurus vulgaris*), British, presented by Mrs. Campbell; two Prairie Grouse (*Tetrao cupido*) from North America, presented by Mr. Henry Nash; six Common Trout (*Salmo fario*), British fresh waters, presented by Mr. S. Wilson; two Common Seals (*Phoca vitulina*), British Seas, eight Prairie Grouse (*Tetrao cupido*) from North America, deposited; three Common Sheldrakes (*Tadorna vulpanser*), three Common Pintails (*Dafila acuta*), four Shovellers (*Spatula clypeata*), European, four Chilian Pintails (*Dafila spinicauda*) from Antarctic America, two Bahama Ducks (*Dafila bahamensis*) from South America, two Chiloe Wigeons ((*Mareca chiloensis*) from Chili, nine Summer Ducks (*Aix sponsa*) from North America, six Mandarin Ducks (*Aix galericulata*) from China, purchased; an Axis Deer (*Cervus axis* ♂), two Black Swans (*Cygnus atratus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

THE OBSERVATORY AT MELBOURNE.—The seventeenth annual Report of the Board of Visitors of this establishment, together with the Report of the Government astronomer, Mr. Ellery, for the year ending June 30, 1882, has just been received. The meridian work with the transit-circle was for the most part limited to observations of standard stars, for the ordinary purposes of an observatory and the determination of places of stars used for positions of comets. The 8-inch equatorial had been arranged for the observation of the small planets *Victoria* and *Sappho*, during the last autumn, according to a programme agreed upon with several European and American, and other southern observatories, with the view to another determination of the solar parallax. The large reflector was employed on celestial photography, for sketching a number of Sir John Herschel's smaller nebulæ, for drawings of comet 1881, IV., &c. The nebula about η Argus was examined on three evenings, and was found to agree very closely with the drawing made in 1875. The majority of the smaller nebulæ were found to accord well with Herschel's descriptions. Nos. 57 and 1423, however, were much fainter than Herschel indicated, and Nos. 1655 and 2181 differed considerably from his description. The positions of these nebulæ for 1883·0 with Herschel's notes are as follows:—

		R.A. h. m. s.	N.P.D. ° ′
No.	57	0 21 43	147 37·7
,,	1423	6 25 6	121 12·3
,,	1655	8 16 27	125 50·9
,,	2181	10 37 27	125 45·0

No. 57.—Pretty bright, small, round, much brighter in the middle.

,, 1423.—Pretty bright; considerably large, round, very little gradually brighter in the middle; 4′.

,, 1655.—A double star $= h.$ 4023 in a pretty small nebula, among some seventy stars.

,, 2181.—Pretty faint, small, much extended in 0° ±; very suddenly, very much brighter in the middle; the first of three.

The photo-heliograph was used on every fine day possible, and 217 pictures were obtained in the year.

The necessary funds have been voted for a new transit-circle more in accordance with the modern requirements of astronomy, and its construction has been intrusted to Messrs. Troughton and Simms. Mr. Christie, the Astronomer-Royal, was invited to modify the specification sent to England, if he found reason to do so.

The Supposed Variable μ Doradûs—A Spurious Star.—Dr. B. A. Gould has made a very unexpected discovery, from which it appears that μ Doradûs of our catalogues, long supposed to be a variable star, was never observed by Lacaille in the position he assigns it in the Catalogue of the *Cœlum Australe Stelliferum*, and further, that by similar error, five other stars observed by Lacaille on the same day, which are found in the reduced catalogue published by the British Association, have no existence in the positions given. The case is a curious one, and as the *Cœlum Australe* of Lacaille is now a scarce work, we may be excused for transcribing the observations in question as they stand. They were made in Zone XI., 1751, December 16, *in parte inferiore* of Lacaille's rhomboid; the numerals are our own:—

	mag.	h. m. s.		mag.	h. m. s.
No. 1	6	4 17 38	No. 6	6·7	4 46 27
		24 2			53 34
,, 2	7	4 39 51	,, 7	7	4 51 5
		47 9			54 33
,, 3	7	4 25 23	,, 8	5	4 59 22
		31 0			5 8 28
,, 4	6·7	4 30 16	,, 9	7	6 5 53
		32 38			9 8
,, 5	7	4 41 33			
		43 41			

Lacaille appears to have entered correctly the times of beginning and ending of describing the chord of his rhomboid for Nos. 1 and 2, but instead of 4h. 25m. 23s. for the third star, the time was really 5h. 25m. 23s., and this error of 1h. runs on up to No. 8 inclusive; No. 9 is correct. This will be readily seen by inspecting the above times. The star entered in the Catalogue as μ Doradûs is No. 8, called 5m. in the observations but 6m. in the Catalogue, which gives its place for 1750·0, R.A. 76° 11′ 17″, Decl. −62° 7′ 4″. The place given by the B.A. reductions is R.A. 5h. 4m. 44·3s., N.P.D. 152° 6′ 57″, which is correctly deduced from the transits as printed. With the correction of +1h. to the times, the position for 1750 becomes R.A. 6h. 4m. 44·2s., N.P.D. 152° 6′ 49″, and the star "μ Doradûs" is seen to be identical with Brisbane 1172 = B.A.C. 2000 = Stone 2836, in Pictor. The other spurious stars introduced in the Catalogue by the error which Dr. Gould has brought to light are Nos. 1542, 1554, 1633, 1680, and 1706. The following identifications of the stars really observed may be useful:—

Spurious stars of the reduced Catalogue.				Stars really observed by Lacaille.		
No. 1542	Reticulum	7m.	=	Stone 2497,	Dorado	7·6m.
,, 1554	,,	6·7	=	,, 2532,	,,	6
,, 1633	Dorado	7	=	,, 2630,	,,	6·7
,, 1680	,,	6·7	=	,, 2707,	,,	6.7
,, 1706	,,	7	=	Brisb. 1109,	Taylor V. 516	
,, 1766	(μ Doradûs)	5	=	Stone 2836,	Pictor	5m.

Brisbane observed a star close upon Lacaille's erroneous position of his μ Doradûs, and according to his general custom gave it Lacaille's magnitude. Moesta (*Astron. Nach.*, No. 1545) stated that he had observed this star at Santiago de Chile from February, 1860, to January, 1865, and had found it 8½ or 9 of Argelander's scale; he therefore considered it to be variable, and thought the period of variation would prove to be of long duration.

The Comet of 1812.—MM. Schulhof and Bossert's sweeping ephemerides for this comet are continued in No. 2489 of the *Astronomische Nachrichten*.

INSECTS VISITING FLOWERS

The interest arising out of the writings of Darwin, Lubbock, and Hermann Müller relative to the part played by insects in their oft-recurring visits to flowers has of late years attracted much attention. The subject, in fact, has created a taste for observation, and an incentive has been given to watch the frequency of visits of various species to certain flowers, and especially to the insects' choice of colours of flower. While the mere registering of visits may seem a comparatively simple one, the reason why insects should show a preference to alight upon flowers of a certain colour, or choose certain species of plants, is a much more complicated problem than at first sight it would appear. Sir John Lubbock has shown by experiment that blue is the bees' favourite colour; H. Müller avers that in the Alps bees are attracted to the yellow rather than the white flowers. However this may be, certain it is that a much larger number of observations are yet needed before a positive law can be deduced. Two papers read at the last meeting of the Linnean Society (March 1): one by Mr. Alf. W. Bennett, "On the Constancy of Insects in their Visits to Flowers," and the other by Mr. R. M. Christy, "On the Methodic Habits of Insects when Visiting Flowers"—point out that a strict watch and ward is being kept on the movements of the busy bee and its kindred. Mr. Bennett states that butterflies show but little constancy in their visits, citing only a few instances to the contrary; but according to him, to some extent they seem to have a choice of colour. The Diptera exhibit greater constancy, though by no means absolute. The Apidæ, especially the hive-bee, manifest still greater constancy. From these data he infers that the ratio of increase is in proportion to the part performed by the insects in their carrying pollen from flower to flower. As respects preference for particular colours, in a series of observations Mr. Bennett has noted among the Lepidoptera that 70 visits were made to red or pink flowers, 5 to blue, 15 to yellow, and 5 to white; the Diptera paid 9 visits to red or pink, 8 to yellow, and 20 to white; Hymenoptera alighted 303 times on red and pink flowers, 126 on blue, 11 on yellow, and 17 on white flowers. Mr. Christy records in detail the movements of 76 insects, chiefly bees, when engaged in visiting 2400 flowers. He tabulates the same, and concludes therefrom that insects, notably the bees, decidedly and with intent confine their successive visits to the same species of flower. According to him, also, butterflies generally wander aimlessly in their flight: yet some species, including the Fritillaries, are fairly methodical in their habit. He believes that it is not by colour alone that insects are guided from one flower to another of the same species, and he suggests that the sense of smell may be brought into play. Bees, he avers, have but poor sight for long distances, but see well at short distances. Of 55 humble-bees watched, 26 visited blue flowers: of these 12 were methodic in their visits, 9 only irregularly so, and 5 not at all; 13 visited white flowers, whereof 5 were methodic and 8 the reverse; 11 visited yellow flowers, of which 5 were methodic and 6 not; 28 visited red flowers, 7 appearing methodic, 9 nearly so, while 12 were the contrary.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

Cambridge.—In the last local examination 17 per cent. of those Juniors who sent up papers in Trigonometry obtained no marks, although some questions were of the very simplest nature. Among the Seniors Hydrostatics produced unsatisfactory answers. Many candidates had no ideas worth the name about pressure at a point, density, specific gravity, and weight. This is due partly to corresponding imperfections in some current text-books, and partly to the habit of teaching Hydrostatics apart from general physics or practical applications. The answers in Statics were the least satisfactory; yet according to the examiner there are few subjects in which good teaching tells more quickly than in elementary mechanics. Thus many who passed did very good papers.

Junior Chemistry obtained a favourable report; but the Seniors displayed lack of reasoning power, with great readiness to reproduce cut-and-dried statements from books. In Heat the results of the examination were encouraging and satisfactory. In Experimental Physics generally there was great lack of practical acquaintance with the subjects. Only one candidate did really well among the Seniors. In Botany the answers were weak throughout, showing great lack of teaching by real objects handled by the students. Zoology appears to have been too much studied by Juniors from older and worthless text-books. The Seniors did better, but spread themselves over too wide a field of work. The knowledge of Physical Geography was better than that of Geology, but neither was good.

The report recently made by Mr. R. D. Roberts of his visits to the centres where local lectures have been established, and on the present state of the local lectures' scheme, contains many most interesting facts regarding the high appreciation with which the intelligent working classes regard the lectures, and the difficulties which the cost of the lectures occasions. Of the results of a course of electricity at Newcastle, the examiner says that the work done in answer to a long and difficult paper of questions was fully equal to that attained in a scientific University course. The greatest difficulty that occurs is not lack of demand for or interest in education, but the provision of funds to meet the expenses. If a solution of this could be found, the scheme would be taken up largely in towns where it is now out of the question. The people who are eager for knowledge and travel long distances to obtain it, in all kinds of weather over the roughest roads, are just those who, if they must pay for the lectures, must have less bread for their families. This is certainly the case with the Northumberland and Durham miners. Whether the State will in some way assist in providing the knowledge and teaching which are so eagerly desired, must be again made a practical and urgent question.

The following are the lectures in Chemistry, Physics, and Mineralogy for the Easter Term (el. signifies elementary, ad. advanced):—

Elementary Course of Chemistry, by a Demonstrator; General Course, continued, Mr. Main, St. John's College; Non-metals, continued, and Organic Chemistry, el., Mr. Pattison Muir; General Principles, continued, and Organic Chemistry, ad., Mr. Muir, Caius College; Organic Chemistry, el., Mr. Scott (Prof. Dewar's assistant); Demonstrations in Gas Analysis, Mr. Scott; Sound, Lord Rayleigh; Heat, Mr. Trotter, Trinity College; Physics, el., Mr. Glazebrook, Trinity College; Physics, el., Mr. Shaw, Emmanuel College; Physics, ad., papers, Mr. Glazebrook and Mr. Shaw; Chemistry and Physics, el., papers, Mr. Pattison Muir and Mr. Shaw.

Practical Chemistry, University, St. John's, Caius, and Sidney College Laboratories.

Practical Physics, Cavendish Laboratory; Demonstrations in Light and Acoustics; and in Optics and Electricity, el.

Mineralogy, Course by Prof. Lewis, and Demonstrations for both parts of the Natural Sciences Tripos.

The following arrangements have been made by Prof. Hughes for lectures during the Easter term:—Local Stratigraphy, Prof. Hughes; Geology (General Course, continued), by Dr. R. D. Roberts, Clare College; Petrology, Mr. Harker, St. John's College; Palæontology, Mr. T. Roberts, St. John's College. Dr. R. D. Roberts will continue to set papers and superintend the course of reading of students in the Museum.

The Strickland Curatorship being about to become vacant, Mr. Salvin having completed his valuable catalogue, a new code of regulations for the Curatorship has been drawn up. The Strickland Curator is to be appointed by Mrs. Strickland, the foundress, during her lifetime; then by Mrs. Catherine Strickland in case she shall survive the foundress; and, after her decease, by the Superintendent of the Cambridge Museums of Zoology and Comparative Anatomy. In addition to caring for the Strickland Collection, the Curator is to take charge of any University ornithological collections, to catalogue them, to assist scientific visitors in studying the ornithological collections, and to aid and promote the progress of ornithological science.

UNIVERSITY COLLEGE, LONDON.—Twenty lectures on Quantitative Analysis will be delivered by Richard T. Plimpton, Ph.D., on Mondays and Fridays at 3 o'clock, during the third term. The first lecture will be given on April 13.

PROF. STOKES, Lucasian Professor of Mathematics in the University of Cambridge, has been appointed to deliver the first course of lectures on Natural Science under the auspices of the Burnett Literary Fund, Aberdeen.

THE Earl of Zetland has given 500*l*. to the Edinburgh Association for the University education of woman to found a bursary for the benefit of its students. This bursary will be known as the Earl of Zetland's Bursary.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 8.—"Note on the Order of Reversibility of the Lithium Lines," by Professors Liveing and Dewar.

In their communications on the reversal of the lines of metallic vapours, the authors have several times noticed (*Proc. Roy. Soc.* vol. xxviii. pp. 357, 369, 473) the reversal of the lithium lines, and concluded that the blue line is more easily reversed than the orange line. This, however, does not appear to be really the case. When much lithium is introduced into the arc, a second blue line is developed close to but slightly more refrangible than the well-known blue line. This second blue line produces with the other the appearance of a reversal, which deceived the authors until they became aware of the existence of the second line. The blue line (wave-length 4604) is really reversed without difficulty when sufficient lithium is present, but under these circumstances the orange line is also reversed. The latter line is also the one which first (of the two) shows reversal, and also the one which is more persistently reversed. Hence they place the lines in order of reversibility as follows: red, orange, blue, green, violet.

Mathematical Society, March 8.—Prof. Henrici, F.R.S., president (and subsequently Sir J. Cockle, F.R.S., vice-president), in the chair.—Mr. Alfred Lodge, Fereday Fellow of St. John's College, Oxford, was elected a Member, and Major Allan Cunningham and Mr. H. T. Gerrans were admitted into the Society.—Prof. Henrici feelingly announced, in a few well-chosen sentences, the loss the Society had sustained since its last meeting by the death of Prof. Henry Smith, one of its most distinguished ornaments, and who had been a Member almost from its commencement in 1865. The loss to mathematics in this country was almost irreparable, and it would be hard to find anywhere a fitting successor to him as an exponent of the higher geometry. It had been said that there were not half a dozen mathematicians in Europe who could breathe on the mathematical heights to which he was accustomed; it was further true that few were so fitted as he for introducing others to those heights. His charm of manner and power of fixing the attention of his hearers were wondrous, and were as strikingly exhibited at the December meeting of the Society (the last meeting at which he was present) as on any previous occasion. What Clifford once said when reading a paper by Hesse might be said with equal truth of Henry Smith's papers: "This is reading poetry." [Perhaps this Society will miss him more than any other; he was always willing, if possible, to respond to the Secretary's request for a paper, and he was a true imitator of the Jewish king, for he never gave us of that which cost him nothing. "Everything that he did was as perfect as he could make it." In a letter now before us the writer says truly: "Of all who 'knew' him, none knew or saw *him himself* as we did at the Mathematical Society." "Very pleasant" was he to us, and his death has left a void in our ranks which time will hardly fill.] —Mr. J. W. L. Glaisher made a communication on the calculation of the hyperbolic logarithm of π.—Mr. Tucker read (in its entirety) a paper by Prof. Cayley entitled "On Monge's 'Mémoire sur la Théorie des Déblais et de Remblais.'"—Mr. J. Hammond made a few critical remarks on a recent paper by Prof. Sylvester in the *American Journal of Mathematics*.

Zoological Society, March 6.—Osbert Salvin, F.R.S., vice-president, in the chair.—The Secretary exhibited, on behalf of the Rev. F. O. Morris, the drawing of a bird shot in Hampshire in November, 1882, which it was suggested represented a Tinamou of some species that had escaped from captivity.—Mr. J. E. Ady exhibited some microscopical preparations of bone, in one case showing the growth of blood-vessels into cartilage previous to ossification, and in another case presenting a hard section in which the lacunæ and canaliculi were extremely well shown.—Dr. Hans Gadow read a paper on the laryngeal muscles of birds, and pointed out first that the muscles of the syrinx are developed from the sterno-hyoid muscles; and, secondly, that the cutaneous muscles are derived from superficial layers of the common muscular stratum. Thirdly, the author considered the connection between muscle and nerve-supply, illustrating his remarks by diagrams.—A communication was read from the Rev. H. S. Gorham, F.Z.S., containing the descriptions of some new species of Coleoptera belonging to the family Erotylidæ. Twenty-nine new species of this family were described, of which ten were from the Philippine Islands, three were from the Andaman Islands, two from Assam, two from the Malay district, six from Africa, and six from Peru. The species treated of belonged chiefly to the subfamilies *Encaustini* and *Dacnini*, the author reserving the remaining subfamilies for a future communication.—Dr. Gwyn Jeffreys read the sixth part of his communications on the Mollusca procured during the *Lightning* and *Porcupine* Expeditions. This included an account of the specimens of the groups of *Scissurella*, *Trochus*, *Turbo*, and part of *Littorina*, referable altogether to seventy species. Four genera and twenty species were for the first time described as new.—A communication was read from Mr. H. O. Forbes, F.Z.S., describing a species of scarlet *Myzomela* obtained in the Island of Boeroe, one of the Ceram group.—Mr. G. A. Boulenger read a paper on the Geckos of New Caledonia. The object of the author in preparing this paper was that it might serve as a guide to the identification of the Geckotidæ of New Caledonia, and at the same time bring the synonymy into order. To this end the author had compared the typical specimens in the Museums of Brest, Lisbon, Paris, and Brussels with those in the British Museum, and had given short descriptions of every species taken from typical or well-authenticated specimens. The number of species of Geckotidæ actually known from New Caledonia was fourteen: of these two were recorded for the first time, one being new to science.

Geological Society, February 21.—J. W. Hulke, F.R.S., president, in the chair.—Rev. John Birks, Capt. James Scott Black, John Bradford, Thomas Alexis Dash, Henry Lewis, and Thomas Morris were elected Fellows of the Society.—The following communications were read:—On the relation of the so-called "Northampton Sand" of North Oxfordshire to the Clypeus-Grit, by Edwin A. Walford, F.G.S.—Results of observations in 1882 on the positions of boulders relatively to the underlying and surrounding ground in North Wales and North-West Yorkshire; with remarks on the evidence they furnish of the recency of the close of the Glacial period, by D. Mackintosh, F.G.S. The author entered into a consideration of the time which has elapsed since the close of the Glacial period, and stated the main results of his observations as follows:—1. That the average *vertical extent* of the denudation of limestone rocks around boulders has not been more than six inches. 2. That the average *rate* of the denudation has not been less than one inch in a thousand years. 3. That a period of not more than six thousand years has elapsed since the boulders were left in their present positions by land ice, floating-ice, or both.—Notes on the Corals and Bryozoans of the Wenlock Shales (Mr. Maw's washings), by G. R. Vine. Communicated by Prof. P. Martin Duncan, F.R.S.

Entomological Society, March 7.—Mr. J. W. Dunning, M.A., F.L.S., president, in the chair.—Three new members were elected.—Exhibitions: A specimen of *Polistes hebræus*, Fabr., an East Indian wasp, captured alive in one of the London docks, by Mr. R. McLachlan; Two British Ichneumons, and an orthopterous insect (*Copiophora cornuta*, De Geer) from Central America, by Mr. T. R. Billups; A preparation showing the structure of the thorax in a large beetle (*Chalcolepidius porcatus*, Linn.), by Dr. D. Sharp.—Paper read: "Further additions to Mr. Marshall's Catalogue of British *Ichneumonidæ*," by Mr. J. B. Bridgman.

Physical Society, March 10.—Prof. G. C. Foster in the chair.—New Member, Major W. S. Boileau.—Mr. Shellford Bidwell read a paper on a new method of measuring resistances with constant currents. It consists in placing a resistance-box in the arm of the bridge which afterwards has to contain the unknown resistance. A balance is effected by unplugging resistance in this box. The unknown resistance is then inserted in the same arm, and the balance restored by plugging resistance out of the box. The amount plugged out equals the unknown resistance.—Prof. F. Guthrie made a communication on liquid slabs. Films of liquid, spread out like a flattened drop on a solid surface, are found by the author to have a thickness which is a physical constant for the same liquid, provided the area is very great in proportion to the thickness. Sodium amalgam inserted in a mercury slab causes it to spread out further. Prof. Guthrie also finds that an electric current increases the diffusion of sodium amalgam through mercury in the direction of the current.—Mr. Baily suggested that, as the diffusion produces a current, an opposing current might be found to stop the diffusion. Mr. Stanley said the largest water-drop he had measured was one-fifth of an inch in diameter.

EDINBURGH

Mathematical Society, March 12.—Mr. J. S. Mackay, M.A., F.R.S.E., president, in the chair.—Prof. Chrystal, in his address on "Present Fields of Mathematical Research," remarked at the outset that the times seemed peculiarly suitable for the foundation of such a society in Scotland where, as in England and America, the tide of mathematical research had certainly begun to flow. The direct effect of the work of the society would be to keep alive the interest of its members in mathematics, and especially, by division of labour, to benefit the teacher whose daily tasks leave him somewhat unfitted to undertake in moments of leisure the reading necessary to keep him abreast of the time. Further, such benefits would surely extend their influence to the improvement of secondary education in Scotland. The lines along which members might advantageously work were then indicated in a suggestive sketch of the history in modern times of geometry and algebra. Descartes' system of analytical geometry was the first great step, though for long it remained simply a series of solutions of special problems. The discovery and development of the calculus no doubt kept analytical geometry for a time in the background; but there is every reason to believe that great progress in developing geometrical methods was effected by Pascal, Desargues, Newton, and Maclaurin. With them originated the idea of projection, which was systematised into a powerful geometrical method by Monge and his disciples, Poncelet, Chasles, Brianchon, and others. Monge also, however, established the analytical side of geometry, as well as the synthetic, upon an independent basis; his work has been ably supplemented by Dupin and others, and more especially by Plücker. The treatment by the latter geometer of the singularities of higher plane curves, his introduction of the abridged notation, and his invention of the system of line geometry, have been developed each into an extensive branch of mathematics. At the same time algebra has been differentiating itself into well-marked parts. The theories of forms, of equations, of substitutions, and of determinants have been greatly developed by Abel, Jacobi, Galois, Cayley, Sylvester, Jordan, Clifford, and others. The address concluded with a reference to the theory of transcendents and the closely-related properties of the complex variable. A hearty vote of thanks was accorded to Prof. Chrystal on the motion of Prof. Blyth of Glasgow, seconded by Mr. Muir of Glasgow.

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