THURSDAY, NOVEMBER 30, 1876

FERRIER ON THE BRAIN

The Functions of the Brain. By David Ferrier, M.D., F.R.S. With numerous Illustrations. (London: Smith, Elder, and Co., 1876.)

II.

SINCE it is certain that the movement of a limb may be occasioned by an idea, an emotion, or a sensation, and also by the reflex of an external stimulation; since, moreover, it is certain that such a movement may be arrested by an idea or emotion; and since there is good ground for the hypothesis that the cerebral hemispheres are, if not the sole agents, at any rate indispensable accessories in the production of ideas and emotions, we have every right to conclude that the hemispheres play a part in the normal production of many movements; and that ideas and cerebral processes are the subjective and objective aspects of one and the same effect. But experiment proves that many, if not all, these movements can be executed in the absence of the hemispheres, therefore the hemispheres are not *indispensable* but *accessory* factors. This leads to the question, What part do they play? And this again to the questions, Are all cerebral processes or only some of them, ideas emotions, and sensations, the others being simply molecular movements which propagate their excitation to centres of muscular innervation?

I understand the Hitzig-Ferrier view to be this: One set of cerebral processes, having their centres or terminal stations in a limited region of the cortex, are sensational, emotional, and ideal; another set, having also all their centres or terminal stations in a limited region of the cortex, are motor. I am not quite sure that this representation is exact, for in both writers there is absence of explicit definition. But this much at least may be taken as exact, that they profess to have discovered limited areas of sensory and motor stimulation, and within these areas limited spots for particular sensations and particular movements. The interpretation must, of course, rest on a basis of fact, yet the facts observed may be accepted without forcing acceptance of the interpretation. There is general agreement on the facts with great want of agreement as to the conclusions. I have already suggested that the discoveries of Hitzig and Ferrier are of great importance; but only as finger-posts for anatomists seeking the pathways of stimulation, not as inductive stations for deductive inferences. All we can say at present is that electrical stimulation of certain spots is followed by certain movements; but *how* the stimulation reaches the motor nerves is as dark as before.

Thus far, therefore, the part played by the cerebral process is only recognisable as an incitation; it certainly does not effect the movements, it only incites the motor organs. And *thus far* it is on a level with *peripheral* incitations, such as the incitation of laughter by tickling the sole of the foot; or of vomiting by tickling the fauces. Laughter is a function of a complex apparatus, and this apparatus may be stimulated in very different ways from very different starting-points—an idea, a sight, or a touch. Vomiting, again, follows from a blow on the head, acidity in the stomach, a disgusting sight, a smell, or a tickling of the throat. No one considers the sole of the foot and the fauces to be centres or terminal stations for the functions of laughter and vomiting. Why, then, when we see movements of the limbs, the eyes, or the tail following stimulation of the cerebral cortex, are we to conclude these movements to have their centres in the cortex? The foot may be removed or the sole rendered insensible, yet still laughter will be stimulated by ideas, sights, &c., as before. In like manner the spot of the cerebral cortex may be removed or destroyed, yet still the limbs will move as before. Nay, not only the cortical spots, but the whole hemisphere may be removed, and still the limbs will move as before.

For anatomical purposes we make a wide distinction between grey matter and white, and a still wider distinction between central and peripheral nerve-substance. Physiologically such distinctions are, I conceive, erroneous, the whole nervous system being one. But the distinction between a centre—*i.e.*, a place [*to* which stimulations are carried, and *from* which motor impulses issue—and a peripheral region where stimulations begin or end—is both a physiological and an anatomical division usefully maintained. According to this definition of a centre, we may doubt whether the cortex of the cerebrum has any claim to be called a centre, or group of centres, *whether, in fact, it is not a peripheral region*, the processes of stimulation in it being of the same order as the processeses of stimulation in the skin, or mucous membranes, *i.e.*, simply those of peripheral incitation.

This is the paradox to which allusion was made at the close of my former article. As it would occupy too much space for development here, and as I have worked it out in a volume now at press, I merely suggest it for speculative readers, and pass on to Dr. Ferrier's book.

First as to his facts. It has been urged against his localisations that he has employed a too powerful current. On this subject I have no right to an opinion, but incline to accept his reply as satisfactory; though one must not overlook the fact remarked by Carville and Duret, that very different movements may be occasioned by the stimulation of one and the same spot according to intensity of the current—a fact analogous to what is observed in stimulations of the skin. It has been urged by Hitzig, and also by Braun (Eckhard's "Beiträge," vii., 133, 137), that in Dr. Ferrier's experiments the same movements follow stimulation of different spots, even when these spots are situated in the different regions recognised as excitable and non-excitable. The objection is not only triumphantly answered by Dr. Ferrier, but is answered by the introduction of an idea which is of great significance:—"The mere fact," he says, "that movements result from stimulation of a given part of the hemisphere does not necessarily imply that the same is a motor centre in the proper sense of the term. It will afterwards be shown that the movements which result from stimulation of the regions in question are expressive of sensation, and that the character of the movements furnishes an important index to the nature of the sensation" (p. 147; compare also p. 163).

As an answer to his critics this seems conclusive. But does it not throw a serious difficulty in the way of his hypothesis? If, as he luminously suggests elsewhere, "the sensations accompanying muscular action being repeated as often as the muscular action itself, the organic nexus between the motor and tactile centres becomes so welded, that this sensori-motor cohesion enters like a compound chemical radical as a simple factor into every association which motor centres can form with other motor centres and with sensory centres in general" (p. 268); then surely this, while it satisfactorily accounts for the motions following excitation of sensory regions, leaves unexplained the facts of other such excitations *not* being followed by motions (comp. p. 231), and raises the question whether all motions are not due to sensory excitation? On the first point let us ask why the optic thalami—said by him to be sensory centres—do not respond by motor manifestations when stimulated? He regards this "as sufficient of itself to dispose at once of the views of those who would attribute motor functions to these ganglia. The fact that lesions of the optic thalami cause paralysis of motion proves nothing regarding the real functional significance of these ganglia" (p. 239). Agreed; but then why does he not equally conclude that absence of paralysis of motion, when the corpora striata are destroyed, disproves the motor function he assigns to these ganglia, especially since direct stimulation of these ganglia does not produce motions?

Then, again, if sensorial excitations produce movements by playing upon the motor centres, why not adopt the view which regards all cerebral excitation as sensorial? The hypothesis of motor centres in the cortex would thus be resolved into the fact that particular sensations excite particular movements; and the localisation of spots on the cortex would be no more than analogous localisations on the skin—the sensation excited by tickling the sole of the foot, causing different movements from those caused by the same stimulus applied to the heel or instep.

Dr. Ferrier has explicitly declared that "there is no reason to suppose that one part of the brain is excitable and another not. The question is how the stimulation manifests itself" (p. 130). This is in accordance with what I have maintained, namely, that *neural processes* are uniform in character, the diversity of their results—sensation, motion, or secretion—depending on anatomical connections. *In itself*, a neural process is no more a sensation than it is a secretion. To determine a motor centre, therefore, we must look beyond the cortex, and detect its anatomical relations to the motor-apparatus. Do Dr. Ferrier's experiments prove that the area of the cortex, assigned by him as the motor area, has such anatomical relations to the motor apparatus, and the sensorial area such relations to the sensory organs, that we can speak of their activities as motor and sensorial *functions?* In other words, are the cortical areas to be regarded as playing the part of central functions or only of peripheral incitations?

He has argued his view with such force of fact and suggestion that I have little doubt of his carrying most readers with him; and because I dissent from his view I must occupy all my remaining space by endeavouring to weaken the effect of his argumentation. He considers that the indications naturally suggested by the observed facts of electrical stimulation are proved by the observed effects of disease and extirpation. Stimulation of particular spots is followed by definite movements; destruction of those spots is followed by paralysis of those movements. The reader is led captive by what seems irresistible logic. The evidence seems decisive. How if the evidence should be illusory? That it is illusory may be shown, I think, under three heads:—

First Head.—The Italian physiologists Lussana and Lemoigne have specially called the attention of experimenters to the fact that very many recorded contradictions result from the not distinguishing between the first and second experimental periods, namely, the effects observable soon after the operation, and the effects which are observable when the disturbance has settled down, and the organism has recovered something like its normal state ("Fisiologia dei Centri Nervosi," 1871). The first period comprises what may be called the effects of Disturbance of Function; the second period the effects of Removal of Function. The distinction, so fruitfully introduced by Dr. Hughlings Jackson, of discharging lesions and destroying lesions falls under the same conception. I will only add that neither the effects of Disturbance nor the effects of Removal are to be taken as conclusive evidence that the function disturbed or removed is the function of the organ operated on; but that whenever a function persists, or reappears, after the destruction of an organ, this is absolutely conclusive against its being the function of *that* organ.

This premised, I must suggest that Dr Ferrier's experiments cannot be considered as conclusive, because he was unable to keep the animals alive long enough to allow the effects of Disturbance to subside, so as to leave only the effects of Removal to be estimated. And this is the more to be emphasised because in some instances the animals did survive long enough to show some subsidence of the disturbance and *some reappearance of the lost functions.* Now the reappearance of a function after the destruction of an organ admits of but two interpretations —either the function was arrested as an effect of the disturbance, or its organ was destroyed, and *another* organ had vicariously taken its place. This second interpretation is much in vogue, and has received the name of the law of Substitution. The notion that a function can be driven from organ to organ, "like a sparrow driven from one branch to another," as Goltz picturesquely says, is surely raising Hypothesis to the *n*th power? Dr. Ferrier without adopting the first of the two interpretations, argues against the second with his usual force; replacing it by one which is physiologically more consistent, namely, that "there is no direct establishment of new centres in place of those which have been lost, but that those which remain may indirectly without assuming new functions make up for the loss, to some extent at least." In these cases "the path from impression to action is not as in the ordinary course of volition through the cortical motor centres to the corpus striatum, and thence downwards to the motor nuclei and motor nerves, but through the basal ganglia directly." This fails to meet the case when, for example, the function of vision on one side disappears after removal of its assigned cortical centre, and nevertheless reappears. We cannot get the ear to do the work of the eye; and if touch does indirectly make up for loss of sight, it is by a slow process of acquisition; whereas the animal recovers its lost *sight*, and that in the course of a few days.

But restricting the explanation to movements, is it not a relinquishment of the hypothesis of voluntary motor centres? Is it not an invocation of the hypothesis of peripheral incitation? Observe this, moreover: Dr. Ferrier restricts his explanation to the movements which have been automatically organised in the corpora striata. All actions not become automatic are impossible after removal of the cortical centres. "It may confidently be asserted," he says, "and perhaps it may one day be resolved by experiment, that any special tricks of movement which a dog may have learnt would be effectually paralysed by removal of the cortical centres." Well, since this was written, experiment *has* decided the point. By an ingenious method of washing away cerebral substance, Goltz has been able to greatly diminish the disastrous effects of operation, and thus preserved the animals for weeks, in the course of which he observed an *almost* complete restitution of the lost functions. One of the striking cases recorded by him (Pflüger's *Archiv*, xiii. 31) is that of a dog who had been taught to "give the paw" on command. When the surface of the left hemisphere had been washed away there was at first a complete destruction of the power to give the right paw; and the dog when urgently called upon to give it, looked wistful, and ended by stretching out the left paw. Had the dog died within six days after the operation this might have been cited as proof of the destruction of a voluntary centre; but the dog lived, and on the eighth day began to give the right paw when asked, and a month afterwards gave it as readily as before the operation.

Second Head.—Under this head we may consider the evidence adduced for the existence of definitely circumscribed areas, and definite spots within those areas. Dr. Ferrier's pages are very instructive on this point, but not, I think, competent to force his conclusion when they are confronted with Goltz's experiments, which show that the paralysis of sensation and motion cannot reasonably be assigned to the destruction of particular spots, because the paralysis is dependent solely on the *amount* of substance washed away, and not at all on the *localities*. Add to which the fact just insisted on, that the paralysis is temporary. Dr. Ferrier believes that his experiments prove the distinct localisation of motor centres. For example, he produces inflammation and suppuration in one place, and observes spasms followed by paralysis of motion in the whole of one side of the body. This is urged in proof of motion being affected without affection of sensation. On examination, however, it seems to me only to prove the effects of disturbance; and this the more decisively, because he admits than when instead of an *irritating suppuration* there is *extirpation* of the centre, the paralysis quickly disappears. "In these experiments," he adds, "the power of movement alone was destroyed, sensation remaining acute and unimpaired." This is very ambiguous. Sensation *elsewhere*—on the other side of the body—was unimpaired; but so was power of movement *there*. In the paralysed limbs there was no sensation.

Let us now turn to a sensational centre: and we will select that of Vision, because the experiments are here most striking. Destruction of the "angular gyrus" on one side causes blindness in the opposite eye. But this effect is temporary, and begins to subside the next day (p. 165). One would imagine that in presence of such observations, the fact of blindness would be attributed to Disturbance, not Removal of Function; and the recovery of vision to the subsidence of the disturbance. Dr. Ferrier interprets the recovery as due to the compensatory action of the centre in the other hemisphere (p. 169). But this is to invoke the Law of Substitution (which he has successfully refuted), and leaves unexplained why the compensatory action did not manifest itself from the first. The experiments of Goltz seem to me conclusive as to the observed blindness being merely the effect of disturbance; not only does the vision gradually return, but is proved not to depend on the compensatory action of the intact centre, because it reappears even in an animal deprived of the other eye. That is to say a dog, with only one eye, had almost the whole of one hemisphere washed away, so that on the one side it had no optical apparatus, on the other no visual cortical centre—yet it showed unmistakable evidence of being able to see. Observe the dilemma: either there is a complete decussation of the optic nerves, so that each hemisphere is the sole centre for one of the eyes; or the decussation is partial, so that each hemisphere is a centre for both eyes. In the first case destruction of the one hemisphere should produce absolute and permanent blindness in one eye—and this is disproved by experiment. In the second case destruction of one hemisphere should produce partial blindness in both eyes—and this also is disproved by experiment. Or, finally, the visual centres are *not* in the hemispheres, so that destruction of the hemispheres is not destruction of vision—and *this* is what experiment proved.

Third Head.—I must be very brief on this point—namely, that very various effects ensue on excitation of one and the same spot. If we regard the cortex as a peripheral surface of excitation there is nothing mysterious in the various effects produced by reflexes from it—as from the skin; but if we regard it as a collection of distinct sensory and motor centres, there is great difficulty in reconciling the results of observation. For example, the so-called voluntary centres for movements of the limbs and tail are found by Bochefontaine to be centres of salivary secretion. In his memoir in the *Archives de Physiologie* (1876, No. 2, p. 169), the last-named experimenter sums up the results of his observations thus—that the hypothesis of cortical voluntary centres would lead to the conclusion that the same spot was the centre for voluntary movements in a limb, and involuntary contractions of the bladder and spleen, as well as dilatation of the pupil.

My space is exhausted, and I have not been able to do more than criticise the main topic of Dr. Ferrier's book—and this not with the fulness which its importance demands. But if I have shown grounds for regarding the hypothesis of voluntary centres in the cortex as at any rate far from *proved*, and in doing so have had to adopt an antagonistic attitude throughout my review, I should not be just to him, nor to my own feelings of gratitude, if I did not, in concluding, express a high sense of the value of his work, full as it is of suggestions, and rich in facts, which no counter-facts can set aside. It will long remain a storehouse to which all students must go for material. It may be the starting-point of a new anatomy of the brain.

George Henry Lewes

OUR BOOK SHELF

British Manufacturing Industries.—Edited by G. Phillips Bevan, F.G.S. "Jewellery," by George Wallis; "Gold Working," by Rev. C. Boutell; "Watches and Clocks," by F. J. Britten; "Musical Instruments," by E. F. Rimbault, LL.D.; "Cutlery," by F. Callis. (London: Stanford, 1876.)

THIS little volume (which is intended for popular reading) is comprised of several short essays, by different writers, upon the separate subjects indicated. Each essay contains a fairly good account of the history and general trade position of its subjects, but so far as their mechanical construction and the manufacturing operations involved therein are concerned, all are more or less disappointing. No doubt this is in great measure to be attributed to the nearly entire absence of diagrams, the essay on watches and clocks alone being illustrated, and that but scantily. Naturally some subjects suffer more than others. In jewellery, gold working, and cutlery the forms produced are familiar, the tools employed are simple, and what is the method of shaping and fitting together the various portions can easily be imagined. But with musical instruments and watches and clocks the case is different; people, *à priori*, are unacquainted with the apparatus or mechanism made use of, and a free reference to diagrams or figures becomes indispensable. In the essays upon jewellery and gold working, especially in the latter, their aspects and bearings as branches and developments of art are particularly dwelt upon. Cutlery, of course, is treated as an industry, so are watches and clocks. We are afraid the last-mentioned essay is not very carefully written, the writer, amongst other things, actually forgetting to tell us that there is any connection between the length of a pendulum and the time of its swing. And what he can be thinking of to describe Huyghens as a "French clock-maker of eminence," who "about 1650 showed great skill and ingenuity in arranging pendulums to clocks, so as to describe a cycloid," we do not know. The essay upon musical instruments (considering its not being illustrated) is much more intelligible than it might have been.

The book is neatly bound and printed, but will require considerable alteration and extension before it becomes what from its title we expected to find it.

An Introduction to the Osteology of the Mammalia. By Prof. W. H. Flower, F.R.S. 2nd Edition. (London: Macmillan and Co., 1876.)

PROF. FLOWER'S valuable "Osteology of the Mammalia" holds so high a position among scientific manuals that the appearance of a second edition requires but a passing notice from us. The author is himself so continually adding to our knowledge of the anatomy of the higher Vertebrata, at the same time keeping fully *au courant* with the investigations of both British and foreign zoologists, that there are several minor additions which he has had to make after an interval of six years, since the appearance of the volume originally. Amongst the most important of these, we notice the record of the conical form of the odontoid process of the axis vertebra in the Chevrotains (*Tragulina*), the introduction of a summary of Prof. Parker's study of the development of the skull of the pig, the account of the hyoid bones of the Ant-eater, of the large pectineal process in Phyllorhine Bats, and of the peculiarly anchylosed tarsus in the Muntjacs. In the first edition the typography and the printing of the wood-cuts was too black throughout; in the new one this defect has been entirely removed, both the type and the figures being all that can be desired. There is a new outline diagram of special interest introduced to illustrate the mutual relations of the various elements of what may be termed the typical mammalian skull. This replaces a plan drawn out for a similar purpose in which the names of the bones were distributed over a page in such a way as to indicate their relative positions. In the new diagram the employment of outlines to the bones renders the exact situation much more distinct and enables the commencing student to carry away with him a much more precise idea of the exact neighbourhood of each part of each bone than was possible from the older plan. We welcome with much pleasure this new edition of the "Osteology of the Mammalia."

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.*]

On the word "Force"

PROF. TAIT in his lecture on *Force* said that this word must be used in a certain definite sense and in this sense only. In order to claim Newton's authority for the one definite sense to which he would confine the word, he has to assume, not only that Newton translated *force* by *vis impressa*, but that he—an Englishman writing in Latin—used *vis insita*, *vis motrix*, &c., without any English equivalents. Until good evidence for these assumptions—improbable as they are on the face of them—is brought forward, Prof. Tait cannot claim the authority of Newton *in his favour*.

In the communication I made to NATURE (vol. xv. p. 8) I contended that the authority of Newton was *against* the restriction of the word to this one sense, on the assumption that the equivalent of Newton's word *vis* is *force*. To those who demur to this assumption I propose the questions: (1) Is it likely that Newton had in his mind no English equivalents for *vis insita*, *vis gravitatis*, *vis centrifuga*? (2) If force is not English for *vis*, what English word had Newton in his mind? Until some new light is thrown on these questions I maintain that Newton's authority is claimed for the restriction of *force* to the sense of *vis impressa* on, to say the least, insufficient grounds; and that the obvious interpretation of Newton's words is dead against it.

I have, I hope, in a previous communication done justice to Prof. Tait's zeal for definiteness and accuracy; and with him I feel what supreme virtues these are in a scientific man. But I contend that the wide sense of the word *force*—which I attribute to Newton—is not loose and inaccurate; it is simply general and comprehensive; each of the narrower uses, as in *vis impressa*, *vis insita*, is not more accurate but more special: these special senses are not inconsistent, though they are not identical; they are neither inconsistent with each other nor with the use of the word force in its widest sense. Some English mathematicians wish to have this valuable word all to themselves for a special technical sense; Newton claims no such monopoly, nor is it claimed by all foreign mathematicians, nor conceded by metaphysicians; nor is the claim to this monopoly likely to be conceded until a better title to it has been shown.

Cambridge, November 24 P. T. MAIN

Peripatus N. Zealandiæ

IN the November number of the *Annals and Magazine of Natural History* is a paper by Capt. Hutton on *Peripatus N. Zealandiæ*, in which the author comes to the astounding results that this species is hermaphrodite, and that its horny jaws are not foot jaws but homologous with those of annelids such as Eunice. If such were in reality the case much of my results concerning *Peripatus capensis* (*Phil. Trans. R. Soc.*, 1874, vol. clxiv. Part 2) would lose its value, and since I believe *Peripatus* to be a most important form, and a representative of the ancestral stock of all tracheates, in fact of the Protracheata of Prof. Haeckel, I hasten to write a few words in reply.

I obtained specimens of *Peripatus N. Zealandiæ* at Wellington from Mr. W. T. L. Travers, who has done so much for science in New Zealand, and who most kindly assisted me and my late colleague, R. von Willemöes-Suhm, in many ways, and who first pointed out *P. N. Zealandiæ* to Capt. Hutton also. I had further at least fifty specimens of *Peripatus* collected for me and brought to me alive. I examined these and made notes, but have been prevented by other work from publishing them hitherto. *P. N. Zealandiæ* is not hermaphrodite. I examined several males, which differ in no essential points in their structure from those of *P. capensis*. Like those of *P. capensis*, they are less numerous than the females, and Capt. Hutton has been unlucky enough not to meet with any amongst the twenty specimens examined by him. The jaws of *P. N. Zealandiæ* are further, I believe, developed just as are those of *P. capensis*. At least I saw that the earliest stages corresponded, and recognised the first pair of members of the embryo in *P. N. Zealandiæ* in the stage in which they are not yet turned inwards to become foot jaws. I have prepared a more extended answer to Capt. Hutton's paper with an account of my own observations on *P. N. Zealandiæ* for the *Ann. and Mag. of Nat. Hist.*, but as this cannot probably be published immediately, I should be much obliged if you would insert this reply in NATURE.

H. N. MOSELEY,
Naturalist to H.M.S. *Challenger*

The Age of the Rocks of Charnwood Forest

IT is no doubt to be regretted that Mr. Woodward, misled by insufficient authority, should have introduced, in his excellent work on the geology of England and Wales, still further confusion into the maltreated old rocks of Charnwood Forest, but I doubt whether their age is quite so certain as Prof. Hull seems to think. I fully agree with him that there is not a particle of evidence for their Laurentian age, and that their syenites and hornblendic granites cannot be correlated with the hornblendic gneiss of the Malverns, but I must demur to his grouping them with the Cambrian rocks of the Longmynds or of Llanberis. The authority of Prof. Sedgwick is great, but it must be remembered that the term Cambrian with him included far more than in the nomenclature of the Geological Survey, and I am not aware that he ever committed himself to the Charnwood rocks being equivalent to his *Lower* Cambrians. Except a slight lithological resemblance of some Charnwood rocks to those of Harlech and Llanberis, and a still slighter to Longmynd rocks, there is really nothing in favour of this special correlation. One point, however, there is which may give some clue to their age, which does not seem to have been much noticed hitherto, probably because the facts have been strangely overlooked in the Geological Survey description of the district. It is that beds of coarse volcanic agglomerate and ash abound among the Charnwood series. Further, the resemblance of the rocks as a whole (when not unusually metamorphosed) is very close to the "green slate and porphyry series" (or Borrowdale rocks) of the Lake District. Compared with the Welsh rocks, they are far more like those of Cader-Idris than of Llanberis. With these there is scarce any lithological resemblance, but if I mixed my Charnwood collection with those from the other two localities, especially the former, I should have great difficulty in separating many specimens. It seems then to me far more likely that this great volcanic activity in the Charnwood district should have corresponded in time with that in the Lake District or with some part of that in Wales, than that it should have happened in the age of the Harlech, Llanberis, and Longmynd groups, where we have no evidence of any volcanic disturbance. The argument may be summed up thus, as it seems to me:—The Charnwood rocks are old, so are both the competing groups; they are unfossiliferous, so are both; they are cleaved, so are both; they contain evidence of great volcanic action, so do the Borrowdale series, and not the Welsh Lower Cambrians. One point for the former. The general correspondence of their strike with that of the Borrowdale series under Ingleborough may also perhaps count for something.

T. G. BONNEY

St. John's College, Cambridge, November 25

THOUGH the discussion of the age of the rocks of Charnwood Forest is not likely in the present state of our knowledge to lead to any useful result, there are still a few points in Prof. Hull's letter on the subject which seem to call for remark. In the first place the late Prof. Jukes was by no means so strongly in favour of the Cambrian age of these rocks as Prof. Hull states. Prof. Jukes' words, in Potter's (not Porter's) "History of Charnwood Forest" are as follows:—"It is therefore uncertain whether they (the rocks of Charnwood) belong to the Devonian, Silurian, or Cambrian systems, the probability only being in favour of the latter." Secondly, the Cambrian of Sedgwick includes a great deal more than the Cambrian of the Geological Survey, and therefore there is not the perfect unanimity between these two authorities that Prof. Hull's remarks would lead us to believe. Thirdly, if lithological resemblance is to go for anything, it may be used directly against the Cambrian age of the rocks. On the western side of the forest we find sheets of crystalline rock and beds of highly altered conglomerates and breccias, which have a suggestive likeness to the lava flows and ash beds of the green slate and porphyry series of the Lake District. I don't say the resemblance proves anything, but it is worth quite as much as the similarity between the slates on the east side of the forest and the slates of Llanberis. Mr. Bonney has also called attention to the fact that the strike of the Charnwood Forest rocks is the same as that of the Volcanic Series in the Lake Country, when that group is last seen. Again, it is far from certain that the rocks of Charnwood Forest are all of the same age. I recollect seeing many years ago some sections (of which I am afraid I have kept no record) that seemed to show that some of the bosses of Dioritic rock near Markfield were older than the slates that surrounded them. If this be so, perhaps these crystalline hills may be the projecting points of a nucleus of similar rock that underlies the whole area, and which may be Laurentian in age. The rocks are not gneiss, but I know of no reason why the equivalents of the rocks of the Hebrides must be gneiss all the world over; they are, however, rich in hornblende, and so are the Hebridian rocks. With all these possibilities before us, I am afraid it will be hard to arrive at that enviable state of security which Prof. Hull seems to have been in when he penned his letter.

A. H. GREEN

I AM pleased to find in NATURE, vol. xv., p. 78, a letter from Prof. Hull, with reference to the age of our Charnwood Forest rocks. He writes against their assignment by Mr. H. B. Woodward, in his "Geology of England and Wales," to the Laurentian period (see p. 24).

But, in fact, as Prof. Hull himself points out, we also find on p. 30 a statement that part of the series may belong to the Cambrian epoch.

It would appear that as Mr. Woodward is not personally acquainted with the region, he has endeavoured to give the views of the various authors whom he knows to have written on the subject, and as these are conflicting, and based upon little personal work, it is no wonder that he has been led astray.

I do not think sufficient importance has been attached to the study of this isolated outcrop of old rocks. We can trace its continuation to the south and south-east for a considerable distance, and I would venture to suggest the possibility of a flexure or spur in this direction connecting with the old palæozoic ridge for which we have lately been fishing in the Wealden. In my "Geology of Leicestershire and Rutland," which will shortly be published, there will be found some fine photographs of the principal quarries and natural outcrops of the Charnwood rocks; and I have there given the reasons which induce me on the whole to refer the main mass of the rocks to neither Laurentian nor Cambrian, but to the *Silurian* period. The evidence is but scanty however, but a balance of probabilities at the best. As to Sedgwick's determination of the region, we must remember that much that he then called Cambrian has since been assigned to Lower Silurian.

WM. JEROME HARRISON

Town Museum, Leicester, November 24

Minimum Thermometers

SOME time ago a correspondence appeared in NATURE (vol. vi. pp. 122, 142, 221) on the subject of moisture deposited in minimum thermometers exposed on the grass. As I was at the time much annoyed with this myself I took up every hint I could get in the matter, though I must confess with indifferent success. I tried for a long time india-rubber packing, with sealing-wax, &c., of varying coats, as advised, but still moisture insinuated itself.

At last I bethought myself of a *cork* packing. I cut a piece of cork so as to fit tightly round the neck of the thermometer tube, then inserted the tube and packing into the glass case—the cork packing being about a quarter of an inch long. The exposed end of the cork I covered with two or three coats of asphalte, as used on microscopic slides. At first a slight bubbling was seen through the asphalte, but soon disappeared, and a fine uniform surface at last set in. The thermometer has now been in use for several months, and not the least trace of moisture has ever been seen within the cases, although moisture has been abundant, especially for the last three months. The process is simple enough, and I venture to send it to you, hoping that it may answer the purpose for others as it has now for some time answered mine. THOMAS FAWCETT

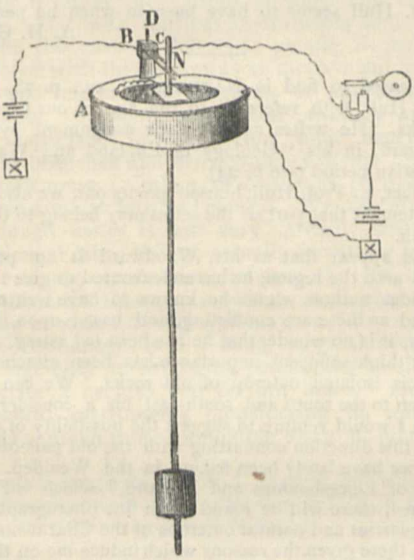
Blencowe School, Cumberland, November 6

Electric Motor Pendulum

THE following very simple apparatus may I think be of use in any laboratory or other place where at times it is necessary to have a pendulum beating seconds in order to give the time for any experiments needing it.

It consists of a Siemens' galvanoscope, A, to which is attached the pendulum; the needle N, preferably with platinum contacts, works between two platinum wires, B and C, with a small amount of play; these platinum wires are insulated from one another by being fastened into a piece of ebonite, which works on a pivot D. The needle is connected by its support to one end of the coil of the galvanoscope, the other end being to earth. To the wires, B and C, are connected the opposite poles of a small battery, the centre of the battery being to earth.

The action of the instrument is as follows:—On slightly oscillating the pendulum the needle N makes contact between B say, and the coil, the magnet being so arranged that the needle then deflects towards B, thus carrying with it the movable contact wires until the pendulum reaches its limit of oscillation, when it



falls, breaks contact with B and makes contact with C, which thus tends to pull the needle over to C, and so on; in this way the pendulum receives at each oscillation the impulse necessary to overcome the forces tending to stop it; and thus will keep oscillating as long as the battery supplies the motive power. For small arcs the beat is *not affected by variation in battery power.*

In the circuit of the battery we can introduce an electromagnet which at each contact of the pendulum on one side will make a stroke on a bell, or indeed by a detent will move by a small train of wheels the hands of a clock. If the pendulum is made to beat half seconds, then the contact being made alternately on each side, the bell stroke would beat seconds. We could of course introduce any number of arrangements of this sort at any intervals along the circuit, and so move any number of clocks at different positions in a large establishment, only one pendulum being requisite to control the whole set.

P. HIGGS

PROF. YOUNG ON THE SOLAR SPECTRUM

THE paper of Prof. C. A. Young read at the last meeting of the American Association for the Advancement of Science describing his recent measures of the displacement of the D and other absorption lines at the receding and approaching limbs of the sun, has a double interest.

By careful measures to which all the necessary corrections have been rigorously applied, obtained by using a diffraction grating in combination with a prism, Prof. Young deduces from observation of the D lines a value of $1{\cdot}42 \pm 0{\cdot}035$ miles per second for the surface velocity of rotation at the sun's equator Direct observation of the motion of spots gives $1{\cdot}25$ miles per second, and the author thinks that the difference of these two values being so many times larger than the probable error of the spectroscopic method, the result of which agrees so well with Vogel's result, indicates that a portion of the displacement observed is produced by the difference in the angular velocity of rotation of the solar atmosphere which causes the absorption lines and the underlying luminous surface, and the sign of the difference would indicate that the atmosphere is swept forward with the greater velocity of the two.

This conclusion is itself one of great interest, but for many persons the fact that it is based on the acceptance of Doppler's theory will be a source of satisfaction as indicating that the recent disputes as to its soundness are beginning to be considered settled and in its favour, as at any rate a near approximation to the truth.

One of its first assailants, on mathematical grounds, was Prof. Petzvall. But, as was pointed out by Mach in a "Contribution to Doppler's Theory," published at Prague, in 1874, his main argument fell beside the mark, while the only one which touched it went to prove that for comparatively small velocities of translation in the source of sound or light, compared to the velocity of wave transmission, Doppler's theory was a correct approximation.

More recently Van der Willigen's mathematical objections have been apparently fairly disposed of by Mr. Christie, while the discrepancy that Father Secchi has lately pointed out between the measures of displacement of spectral lines in the case of certain stars as observed by Mr. Huggins on the one hand and at Greenwich on the other, does not really affect Doppler's theory at all, but only the degree of certainty with which it can be applied to the determination of stellar motion. But the facts are not as Father Secchi represents them. He points out, in a list of thirteen stars, that the displacement in the case of some five stars as observed by Huggins is in the opposite direction to that observed at Greenwich. But the Greenwich observations that he takes are some early tentative observations. We have taken the trouble to refer to the most recent Greenwich measures, and find that of the five disagreements insisted on only one holds.

INDIAN GEOLOGY

THERE seems to be a very pretty quarrel just now—and one urged with the usual absence of acrimony in scientific controversies—as to the age or ages of an important group of rocks in Her Majesty's Indian empire.

For years it has been known that while a large mass of the rocks forming the Peninsula of India are unfossiliferous, there is also in that country an extensive series of beds the predominant, and frequently the only, fossils of which are vegetable remains. These beds were often spoken of as the Plant-beds of India. Among the flora certain forms which used to be called *Palæosamiæ*, now *Ptilophyllum*, were pretty generally distributed, while the genera *Schizoneura* and *Glossopteris* were found in lower portions of the series.

On the evidence of the first-named fossils and several others, a Jurassic age was assigned to the containing beds, while the identity of the *Glossopteris* with Australian forms involved these Indian beds in the dispute as to whether the coal-rocks of that country were likewise Jurassic or really carboniferous.

One portion of the Indian plant beds contained a limited terrestrial fauna which on high authority (Huxley) was considered probably Triassic. And another portion of this plant series (in Kutch) was found associated with a marine fauna ranging from the age of the Bath Oolite up to the Tithonian of Continental geologists.

On the whole question, the Triassic fossils, though their suggestiveness was admitted, were not considered sufficient to have much influence, and to the upper part of the plant-beds a Jurassic age was assigned, while the lower portion, apparently on the evidence of the *Glossopteris*, was thought to be Palæozoic, an opinion, however, which does not appear to have been universally received.

In this state, or one very similar, Dr. Feistmantel found the subject upon commencing his examination of the Indian fossil plants, aided by the separation of the series into several groups on stratigraphical grounds by the officers of the Geological Survey.

His examination has led to results so far as yet published, which will be found in two papers among the *Records* of the Geological Survey (vol. ix., parts 2, 3).

From these it appears he considers the Kutch flora to represent a period in the Jurassic epoch somewhat earlier than that indicated by the associated marine fauna, so far as this has been examined, which is one of the points in debate, and that five of the upper divisions of the Indian plant beds (or Gondwana series) are Jurassic, while he contends that the remaining three of its lower groups are neither Jurassic nor Carboniferous, but Triassic.

In a paper immediately following Dr. Feistmantel's second contribution to the *Records* of the Indian Survey, Mr. W. T. Blanford discusses the whole question, and arrives at the conclusion that the evidence connecting the lower Indian plant beds with the Australian carboniferous rocks is about equal to that for their being of the same age as the Trias of Europe—pointing out that the land faunas and floras of Palæozoic and Mesozoic times differed from each other in different parts of the globe, at least as much as they do at present, and far more than the fauna of the sea. Also that the evidence founded upon fossil plants of the age of rocks in distant regions must be received with great caution, being certainly in same cases opposed to that furnished by the [contemporaneous] marine fauna.

In a coming volume of the "Palæontologia Indica," Dr. Feistmantel will no doubt state his views and their reasons as fully as possible; meantime, enough has been said to show the interesting, yet rather difficult, nature of the subject, for more detail regarding which the papers just mentioned may be referred to.

THE RIVER CLYDE

THE profession of Civil Engineering, as defined by Telford, which definition is incorporated into the Charter of the Institution of Civil Engineers, is "the art of directing the great sources of power in Nature for the use and convenience of man," and there are few more striking examples of what science may do for commerce, or of what man may accomplish by working hand-in-hand with Nature than is the proud position of the River Clyde at the present day, as compared to what it was one hundred years ago, or even as late as the year 1840.

To many of those who attended the recent meeting of the British Association, and who have fresh in their memory the geography of the City of Glasgow, with which must indissolubly be connected the princely hospitality of its inhabitants, it may be interesting to know that the noble river which has made Glasgow the mighty city that it is, from whose shores some of the largest ironclads of our fleet have been launched is a water highway, almost as much the work of man as is the Suez Canal itself.

One hundred years ago the Clyde was little more than a picturesque mountain-stream, so shallow, that at a place called Dumbuck Ford, twelve miles below Glasgow Bridge, passengers could traverse it on foot. Now, vessels drawing twenty-three feet of water can ride safely in the heart of the city at low water, and the largest ocean-going steamers can come up the river at all times of the tide.

This wonderful change has been brought about by a succession of engineering operations, in all of which Nature has been coaxed, by artificial means, into doing the largest share of the work; and the operations of man, great as they have been, have been directed solely to assist that work, and to remove obstacles which stood in its way. The names of the engineers under whose directions these improvements have been made, alone show that the highest scientific skill has been brought to bear upon the development of the water-way to the City of Glasgow; for, within the last 100 years, among the engineers who have been employed by the Clyde trustees, either to carry out improvements, or to prepare reports in connection therewith, will be found the names of Smeaton, Golborne, Watt, Sir John Rennie, Telford, Stevenson, Walker, Scott Russell, and Bateman; but the largest engineering operations have been left for Mr. Deas, the present engineer of the Clyde navigation to carry out, for since the year 1872 greater progress has been made than during any previous equal period. In that time no less than 1,505 lineal yards of quayage have been added, slip and graving-docks have been constructed, large cranes erected, and very considerable progress towards completion has been made in the celebrated Stobcross Docks of which we shall speak further on.

The exceptional construction of these docks, necessitated by the local peculiarities of the geological strata, formed the subject of a valuable paper read before the British Association by Mr. James Deas, C.E., under whose directions they are being constructed, and who has recently published a most interesting work upon the Clyde,[1] illustrated with maps, sections, and tidal diagrams, and from which many of the data contained in this article have been derived, and to which we would refer those of our readers who wish for further investigation.

It is just a hundred and eight years ago since John Golborne, of Chester, visited the Clyde and made his first report, in which he pointed out that the shores of the river "in most places being much softer than the bottom, the current has operated there, because it could not penetrate the bed of the river, and has by those means gained in breadth what is wanting in depth;" and, he added, "I shall proceed on these principles of assisting Nature when she cannot do her own work, by removing the stones and hard gravel from the bottom of the river where it is shallow, and by contracting the channel where it is worn too wide."

Golborne, carrying these principles into practice, erected a number of rubble jetties so as to contract the channel, giving to the stream greater rapidity, and consequently greater scouring power, and by a system of dredging in the deeper shallows, and horse-ploughing in those which were exposed at low water, he loosened the hard crust forming the bed of the river, exposing to the action of the current the softer material below, which was speedily scoured away, and in less than eight years the depth of water at Dumbuck Ford was increased from 2 feet to 14 feet. Golborne was followed by Rennie, who, in 1799, recommended the shortening of some of the jetties, the lengthening of others, and the construction of new ones, so as to direct the channel in its proper course with the least obstruction to the water; and to insure this he recommended building rubble walls from point to point of the jetties, so as to avoid the formation of shoals between them. These suggestions were carried out by the Clyde

[1] "The River Clyde; an Historical Description of the Rise and Progress of the Harbour of Glasgow." By James Deas, M.Inst.C.E. (Glasgow: James Maclehose, 1876.

trustees, and upwards of 200 jetties were thrown out between Glasgow Bridge and Bowling, a distance of eleven miles. During the next forty years improvements continued to be carried on, and the names of Telford and Rennie appear again in connection with them. In 1836 a report of Mr. Walker, the engineer to the navigation at that time, showed that there was a depth of water at the Broomielaw, just below Glasgow Bridge, of 8 feet at low water, and he adds that "the river which by artificial means was to be rendered capable of taking craft of thirty to forty tons to Glasgow, has, by what Golborne called 'assisting Nature,' been rendered capable of floating vessels nearly ten times the burthen." But improvement did not stop here. Since that date the Harbour of Glasgow has been widened by 240 feet, and vessels of 3,000 tons burthen can float where at that time stood one of the largest cotton mills in the city.

Some very curious phenomena connected with the tides have resulted from the alterations of the tidal channel produced by these engineering operations. The level of low water in Glasgow Harbour has been getting lower and lower, until it is now no less than 8 feet lower than it was in 1758, and during the last fourteen years there has been a depression of level of over a foot. This has been accompanied during the same period by a corresponding rise in the level of high water at ordinary spring tides. These phenomena are no doubt due to the greater facility with which the tidal wave can pass up and down the river than formerly, its shallow, broken, irregular, and tortuous channel having been straightened and deepened, and obstructions offering resistance to its flow having been removed. The increase of the rapidity of the flow is as remarkable as the increase of the volume of water. In the year 1807 the time of high water was three hours later at Glasgow than at Greenock, thirty years after there was a difference of 1 hour 23 minutes, and at the present time that difference has been reduced to 1 hour and 5 minutes. At Greenock the tide flows for about 6¼ hours and ebbs for about 6 hours, whereas at Glasgow it flows for 5¾ hours and ebbs for 6¾ hours.

In the improvements of the Clyde the one principle followed by all the engineers has been the increasing of the volume of the tidal wave and the prolonging of its flow into the upper reaches of the river. Very little work has been done by the natural fresh-water stream, although that is estimated at an average of 48,000 cubic feet per minute, which represents in round numbers over 300 million gallons for every twenty-four hours. This fact demonstrates very forcibly that it is to the tidal ebb and flow that we must look for the conservation of the channels of tidal rivers, rather than to the action of the land-water, which cannot be depended upon for constancy, and its tendency is more often to deposit than to scour.

To keep the channel of the Clyde in order, constant dredging all the year round has to be maintained, and under the able administration of the engineer to the navigation this has been brought to a high state of perfection, both in amount of work done and in its very small cost, averaging as it does from about one shilling per cubic yard for gravel to 2½*d.* per cubic yard for sand, and these costs are inclusive of repairs.

Although the Trustees already possess the largest steam-dredging fleet in the world, they have lately given to Messrs. Rait and Lindsay, of Glasgow, whose firm has a world-wide reputation for the construction of such plant, an order for four new steam hopper barges, thus bringing the number up to eighteen. These vessels are designed by Mr. Deas, and will measure 150 feet long, 26 feet wide, and 12 feet deep, and each capable of carrying 500 tons of dredged material. They will be fitted with compound high and low pressure engines, which will also be constructed by Messrs. Rait and Lindsay.

Dredging is employed for widening the river as well as for deepening the bed. When a bank has to be cut away the dredger is worked close to it so as to undermine it, and by this means much cost of excavation is saved.

For the removal of boulders, some of which weigh over six tons, diving bells are employed. Last year one bell lifted no less than 656 tons of boulders from the bed of the river. These bells are also employed for removing the *débris* resulting from sub-aqueous blasting operations which are continually going on, both dynamite and gunpowder being employed for the removal of Whinstone or trap rock. The charges are contained in tin canisters, which are inserted in holes of 3 inches diameter drilled in the rock, which are afterwards sealed up with Portland cement. They are fired in groups by a voltaic battery on the deck of the diving-bell barge, and the shattered rock is removed by the bells.

To give an idea of the benefits which engineering operations of this kind can confer upon the community, it is interesting to notice that whereas the reports of Smeaton, Telford, and Rennie, showed that the river was navigable only for barges to Glasgow, at the present time the registered export and import tonnage of Glasgow amounts to 2½ million tons, or equal to half the tonnage of London or of Liverpool. Population statistics point to the same result. In 1831 the population of Glasgow numbered 202,000, in 1861 it had risen to 395,000, and it is estimated at the present time at 535,000.

The great increase of the shipping trading into the Port of Glasgow has had to be met by the extension of quays and by the construction of docks. The first of these, Kingston Dock, was opened in 1867, giving about 5½ acres of water-space, but the Trustees are now constructing docks at Stobcross which will have an area of over thirty-three acres, and capable of accommodating 1,000,000 tons of shipping. A graving dock 560 feet long and 72 feet wide, with a depth of water of 22 feet, has also lately been opened.

The Stobcross Docks possess an especial scientific interest from the fact that the quay walls are supported on groups of concrete cylinders, a system of sub-aqueous foundation adopted here for the first time by the Clyde Trustees at the recommendation of Mr. J. F. Bateman and Mr. James Deas, and the results have proved so eminently successful that this system is likely to be universally employed for dock foundations in sandy or gravelly soils. During the execution of this work the variety of the geological strata was particularly interesting, ranging as it did from boulder clay of the most tenacious character to the finest and sharpest of sand, much of which was used for the manufacture of glass.

The concrete cylinders are arranged in groups of three together, and are built up of rings formed in movable wooden moulds; they are 27 feet 6 inches in height, made up of eleven rings each, and rest upon iron shoes. When a group of three cylinders is built up to its height, diggers specially designed for the purpose are set to work excavating the sand and gravel from within the cylinders; as this comes away the whole structure disappears into the ground, being helped in its descent by the addition of about 300 tons of cast-iron weights placed on the top. The average rate of sinking is about 1 foot per hour, but as much as 5 feet per hour has occasionally been attained. When the group has been sunk it is cleaned out by the diggers to the level of the shoe, each cylinder is then filled with Portland cement concrete, and upon this foundation the quay wall is built.

Want of space will not allow us to describe the hydraulic swing bridge which will cross the entrance from the river, nor to do more than mention the powerful hydraulic cranes built upon a similar foundation to that of the quays. For these and many other particulars of great interest in connection with the River Clyde we must refer our readers to Mr. Deas's book and to his paper "On the Construction of the Stobcross Docks," read before Section G of the British Association.

RESEARCHES ON THE RADIOMETER

By Prof. Paul Volpicelli.

1. ALL radiometers do not possess the same sensibility necessary for every experiment.

2. The most sensitive of the two which are in the physical museum of the Roman University shows that the freezing mixture of chloride of sodium and snow, applied to the upper hemisphere of the small globe, produces a rotation of the mill in the same direction in which it is produced by heat radiation, *i.e.*, with the white face of the small discs in advance.

3. If to this lowering of temperature be added a radiation of heat, the rotation of the apparatus is accelerated at the same time.

4. If the freezing mixture referred to be placed on the lower hemisphere of the same small globe, the apparatus will rotate with the absorbing, *i.e.*, the black faces in advance, and consequently in the direction contrary to that of the preceding experiment, *i.e.*, to the direction produced, if to the same lower hemisphere, radiant heat be applied.

5. If during the rotation produced by the application of the freezing mixture to the lower hemisphere of the small globe we cause radiant heat to strike the same globe, the apparatus will be brought to a stop; and as soon as the source of heat is withdrawn, the rotation will immediately commence.

6. If the small globe is plunged entirely in a heated liquid, or even in a freezing mixture, the apparatus will remain at rest.

7. It should be noted that the freezing mixture applied to the upper hemisphere of the small globe, produces a rotation in the direction opposite to that produced by the same mixture when applied to the lower hemisphere.

8. It has been stated that the radiometer in darkness remains at rest; but it should be remarked that if even in darkness it is affected by dark radiant heat, the apparatus will assume a rotatory movement; yet the instrument may remain at rest even when placed in a dark space.

9. The luminous rays of the full moon focussed by means of a lens, do not cause rotation of the instrument.

10. If the radiation of the flame of a Locatelli lamp is caused to traverse several plates of perfectly transparent glass, it will be seen by the number of turns of the instrument, that the law of De la Roche is verified regarding the absorption of radiant heat through these plates, however many they may be. I have been able by this means to diminish the radiant heat to such an extent as to cause the rotation of the radiometer to cease, although the light of the same radiation was increased by means of a lens.

11. The same radiation, that, viz., produced by Locatelli's lamp, by traversing a saturated but perfectly transparent solution of alum, before reaching the radiometer, did not set it in motion, although the radiant light was but little diminished; and the same is the case when the light is increased.

12. It would appear at present that the rotation of the radiometer depends on radiant heat and not on the luminous rays.

13. It appears also that the mechanical cause of the rotation of the radiometer consists in the motion of the molecules of very rarefied gas in the small globe, which is in accordance with the opinion of modern thermodynamics.

THE SIPHON RECORDER AND AUTOMATIC CURB SENDER

FOR some time after the introduction of submarine telegraphy Sir William Thomson's mirror galvanometer was the only instrument delicate enough to receive the signals transmitted through a long cable. The spot of light reflected from the mirror moves over the scale and indicates every change of current in the cable. The clerks by degrees learn to interpret the motions of the spot of light, and are able to read the signals sent. The signals, however, must be read at the instant of arrival, and the clerk has no way of correcting what he receives

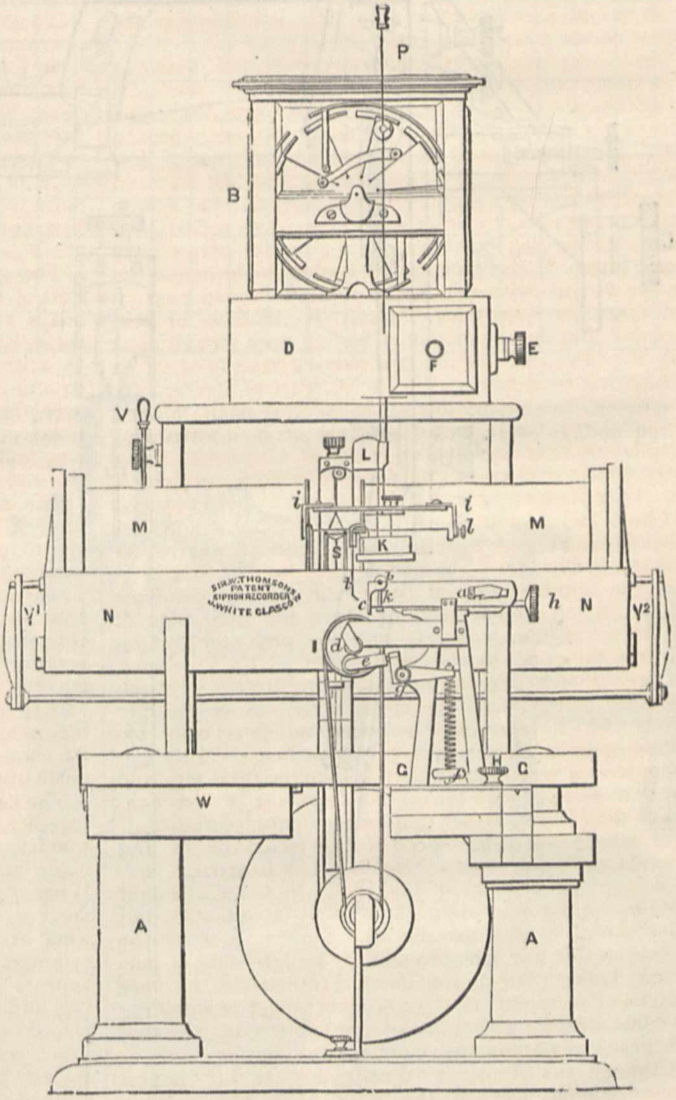


FIG. 1.

except by having the signals repeated from the distant end.

The Siphon Recorder was devised, more recently by Sir William Thomson, for the purpose of receiving and recording the signals transmitted through a submarine cable; though it may also be used for receiving signals sent along a land line. It actually draws on paper the curves corresponding to the changes of current which pass through the line. Thus a permanent record is made of every signal that is sent, and not only can the clerk be sure that he reads the signals correctly, but also any mistakes in transmission can be traced to the station and person where they occur.

The Recorder consists of a powerful electro-magnet, between the poles of which a coil of fine insulated wire is delicately suspended, so as to be able to move round a vertical axis. The current from the cable is made to pass through this coil of wire. When a current passes through a coil suspended between the poles of a magnet, the coil tends to take up a position with its plane at right angles to the line joining the poles. There are two weights suspended from the bottom of the coil, which, when no current is passing, keep the plane of the coil in the line joining the poles of the magnet. When a positive current is passing, the coil tends to turn round a vertical axis in one direction, and when a negative current is passing, it tends to turn round in the opposite direction.

The coil is connected by means of silk fibres with a very fine glass siphon, suspended so that one end dips into a metal box containing ink, and the other end hangs down nearly touching a paper ribbon. The motion of the coil is thus transmitted to the glass siphon. The metal box containing the ink is insulated and is electrified by means of an electrostatic induction machine while the paper is connected with the earth. The ink being electrified, is drawn from the point of the siphon and spurted out in small drops on the paper. When no current is passing they form a straight line on the paper as it is drawn past the end of the siphon; but when a current passes through the coil, it being deflected, draws the siphon to one side, and the line on the paper is no longer straight, but indicates the deflections of the coil. The well-known Morse alphabet is used with the recorder. A deflection of the siphon-point to one side corresponds to a dot, and one to the opposite side to a dash.

Fig. 1 shows a front view of the Recorder. B is the electrostatic induction machine, called the mouse-mill, which is driven by an electro-magnet inside the box D. The mouse-mill serves two purposes. It generates electricity, which is communicated to the box K, containing the ink, by means of the rod P, and it draws the paper along, past the siphon point. M M are the electro-magnets and S is the coil of wire suspended between them; *t* is the siphon, one end of which dips into the ink-box K, and the other end is almost touching the paper *c*. The paper passes under the spring *a* which keeps it stretched, over the roller *b*, then vertically down, past the siphon point, to the driving drum *d*. The battery used for the electro-magnet M M and for the mouse-mill is of the form of Daniell's battery known as the *Tray* battery. It is fully described in NATURE, vol. vi. p. 32.

Fig. 2 is an enlarged view of the signal coil and siphon. Fig. 3 shows a front view of the suspension of the coil. S is the coil suspended by a silk fibre passing round the pulley *r*. From the coil hang two weights, which can slide up and down the guides *z*. The coil surrounds a stationary piece of soft iron placed there for the purpose of increasing the intensity of the magnetic field. A silk fibre *v* connects one corner of the coil with one end of a vertical lever U. The other end of the lever is connected to the siphon *t* by means of another silk fibre, and the motion of the coil is thus communicated to the siphon.

In consequence of the electrostatic capacity of submarine cables, a retardation occurs in the transmission of signals, so that when a current enters at one end of a cable, a certain time elapses before any effect can be detected by the most delicate instrument at the other end. Fig. 4, Curve I., represents the strength of the current, received at the remote end of a cable, as it gradually increases, when the end operated upon is connected to one pole of a battery and kept permanently so. The vertical ordinates represent the strength of the current. The horizontal ordinates represent intervals of time reckoned from the first application of the battery in terms of an arbitrary unit, *a*. This unit of time, *a*, may be defined as follows:—Suppose a cable electrified so that

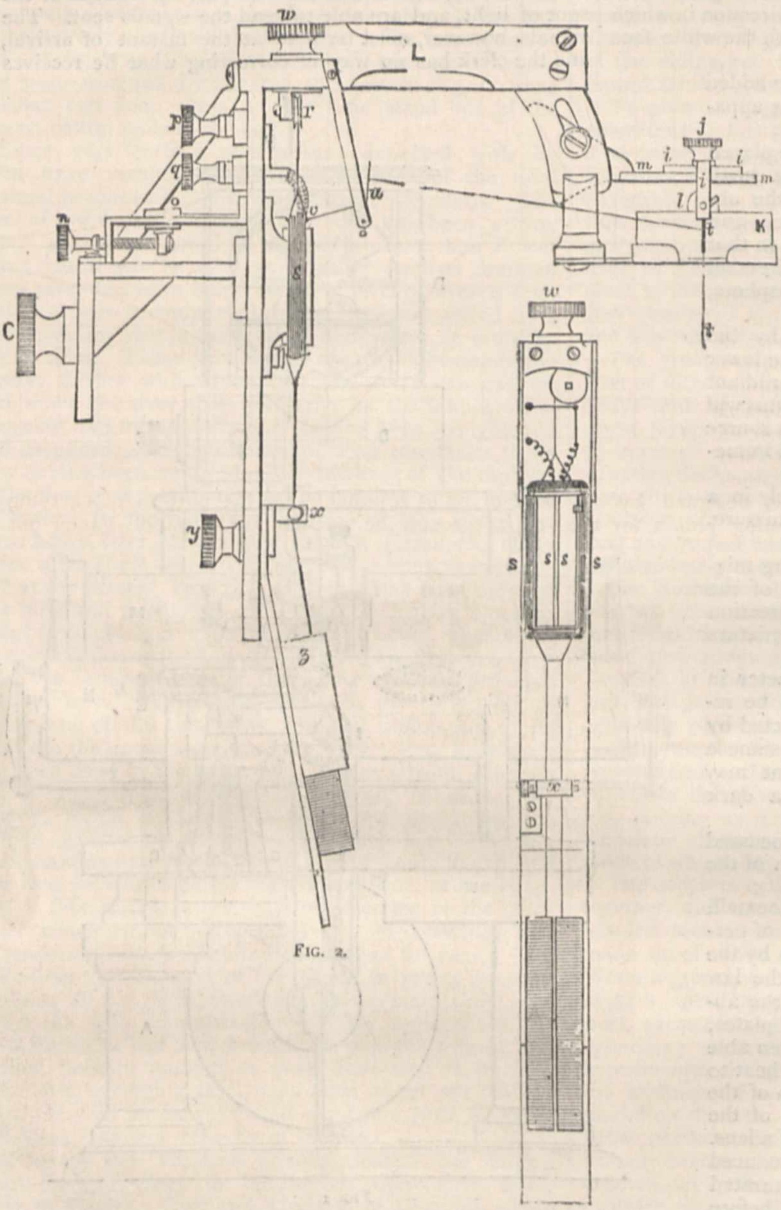


FIG. 2.

FIG. 3.

the electrification along the cable may be represented by a harmonic curve, with single maximum in the middle and zero at each end. Now let both ends be connected to earth. Then the time that the harmonic electrification takes to subside to three-fourths of its initial value is denoted by *a*. Curve I. coincides so nearly with the line, O X, at first, as to indicate that there is no sensible current until the interval of time corresponding to *a* has elapsed; although, strictly speaking, the commencement of effect at the remote end is instantaneous. After the interval, *a*, the current rapidly increases in strength. When an interval of time equal to 5*a* has elapsed from the first application of the battery, the current will be about half its ultimate strength. After 10*a*, the current will have attained to nearly its full strength, that the further increase will be scarcely sensible. Theoretically the full strength is not reached until an infinite time has elapsed.

Fig. 4, Curve II., shows the effect at the remote end of applying a battery during a time equal to 4*a*, and then putting the cable to earth. It will be observed that a current gradually diminishing in strength continues to flow out of the cable for a considerable time after the battery has been disconnected. It is this after-effect which interferes so seriously with the working of submarine cables.

The Automatic Curb Sender was designed by Sir William Thomson and Prof. Fleeming Jenkin for the purpose of diminishing the effects of the retardation due to the electrostatic capacity of submarine cables. This was accomplished by making each signal consist of two currents, the second being of opposite name to the first and of a shorter duration. The latter, or curbing current, hastens the neutralisation of the cable after the first current has passed, and tends to do away with the effecis of the first current. For example, let one end of the cable be connected to one pole of a battery for an interval of time equal to 4*a*, and then let it be connected to the opposite pole for a time 3*a*. The effect at the remote end of this latter current, if it had occurred alone, would be represented by Curve III. of Fig. 4. The joint effect of the two currents is represented by Curve IV., whose ordinates are the algebraic sum of the ordinates of Curves II. and III. The quick return of Curve IV. to the zero line OX, as compared with Curve II., shows the advantage of curb sending.

The curve traced out by the point of the siphon represents the curve of arrival, and the effect of curb sending is to give a sharp outline to the signals and to bring the point of the siphon back or nearly back to the zero line between each signal. It is necessary for the success of curb sending that the spaces for the signal and curb currents should be perfectly correct, and this can only be effected by means of automatic mechanism.

Fig. 5 is an engraving from a photograph of the automatic sender. The message to be sent is punched on a strip of paper in right and left holes, which correspond to the dot and dash of the Morse alphabet, and a central

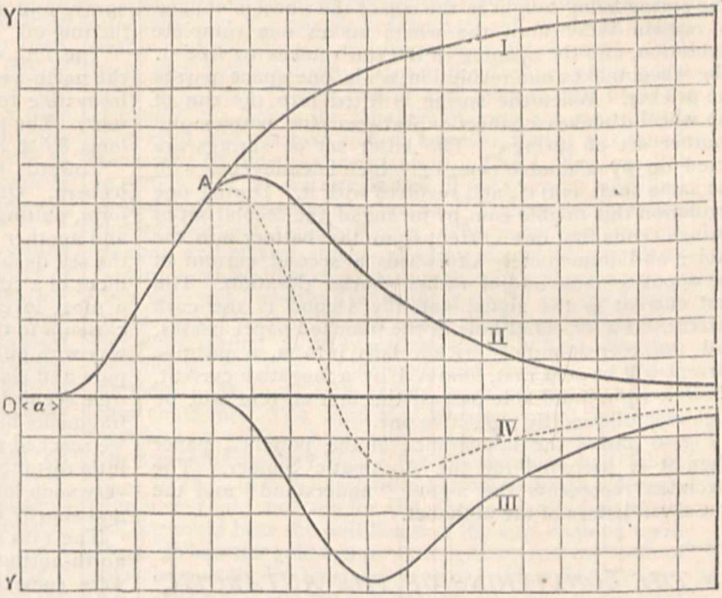


FIG. 4.

row of holes is punched for the purpose of carrying the paper through the machine. The paper is then put into

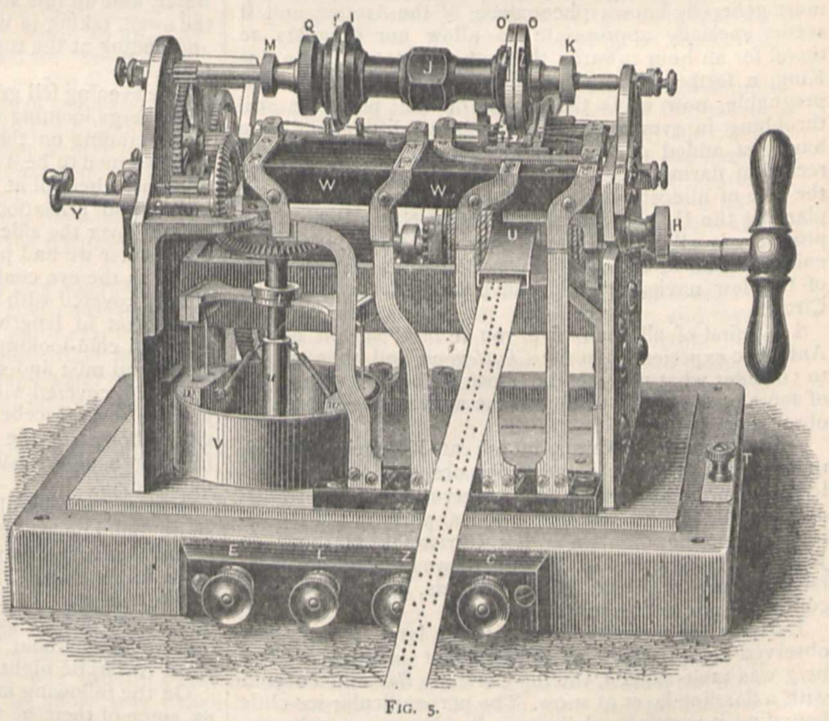


FIG. 5.

the machine and drawn along at a uniform speed by means of clock-work. The paper passes through the

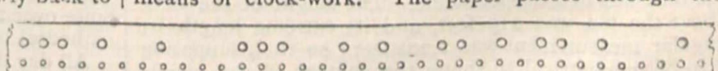


FIG 6.

tube U and underneath two prickers placed so as to correspond to the right and left holes in the paper. When a right or left hole passes, the corresponding pricker falls into the hole, and in doing so lifts a spring through the opening *l* or *l'* into the rim of the revolving wheel O O'. The spring being caught in the rim of the wheel is obliged to remain there until the wheel makes one complete revolution, and the opening in the rim returns to free it. The wheel makes one revolution while one space passes the pricker. When the spring is lifted into the rim of the wheel it makes connection between the battery and another set of springs. The latter set of springs are acted on by a double cam, I I', which is connected with the same shaft as O O', and revolves with it. During one revolution this double cam by means of the second set of springs sends first one current from the battery into the cable, and immediately afterwards a second current of the opposite name and of rather shorter duration. The first current is the signal and the second is the curb current. If a left-hand hole in the punched paper passes, and the corresponding pricker falls into it, a positive current will be sent first, followed by a negative current, but if a right-hand hole passes, the first current will be negative, followed by a positive one.

Fig. 6 shows the appearance of the punched paper when it is prepared for the Automatic Sender. The specimen represents the signal "understand" and the first seven letters of the alphabet.

ON THE CONDITIONS OF THE ANTARCTIC [1]

MY principal object in this evening's lecture is to direct your attention to some of the peculiarities in the physical conditions of the Antarctic regions, and to put you in a better position to contrast these with the more generally known phenomena of the Arctic; and it seems specially appropriate to allow our thoughts to travel for an hour towards that other fortress of the Ice King, a fortress apparently even more hopelessly impregnable, now while the pulse of the nation is still throbbing in sympathy with the brave little band who have just added another chapter to a long and terrible record of daring and self-sacrifice, and have succeeded in the face of almost unparalleled hardships in once more planting the Union-Jack nearest to the North Pole. The propriety is all the greater seeing that Capt. Nares, the gallant leader of the northern explorers, is also the last of the few navigators who have crossed the Antarctic Circle.

I will first of all then give you a brief sketch of our Antarctic experiences in the *Challenger*, and then go on to consider what may be the most probable explanation of some of the most striking of the appearances which we observed.

After spending about a month at Kerguelen Island, making meteorological and other observations, and selecting a suitable spot for the observation of the transit of Venus by the English astronomical party in the following season, the *Challenger* left Christmas Harbour on January 31, 1874, and on February 6 we reached the desolate little group of the Heard Islands, and on the 7th continued our course southwards.

Early on the morning of the 11th a large iceberg was observed bearing south-south-east about six miles off. The berg was table-shaped, the top perfectly flat and covered with a dazzling layer of snow. The perpendicular ice-cliffs bounding it were of a delicate pale blue, apparently perfectly clear, with some caves and slight recesses, where the blue was of a deeper shade. The height of the berg above the sea was 219 feet, and its extreme length by angular measurement was 2,202 feet; so that, supposing it to be symmetrical in shape, the contour of the visible portion being continued downwards, its depth below the water may probably have been about 1,500 to 1,800 feet. In the afternoon Lord George Campbell observed during his watch a large piece come off the side, dashing up the spray, and we afterwards saw a quantity of fragments floating off.

The 12th was misty, with a breeze force = 3–4 from the north-west by west. Many icebergs came in sight from time to time and quickly became obscured in the mist. The position of the ship at noon was lat. 62° 36′ S., long. 80° 4′ E.

Towards evening we passed close to a very beautiful iceberg. One part of it was rounded and irregular in form, putting us in mind of the outline of the Sphinx, and another portion, separated from the first by a fissure, the sea dashing through between them, was like a fragment of a colossal cornice. As the sun sank the ice took a most lovely pink or mauve tint, and when we came close up to the berg it showed out veined in a wonderful way with lines of deep cobalt blue. The ice was perfectly pure and clear. The bergs which we were passing at this time seemed to be breaking up very rapidly; some large fragments had been detached from this one shortly before we reached it, for a quantity of *débris* was floating at a little distance. The pieces washing about in the water very soon lose their edges and angles, and get rounded, and shortly disappear.

The 13th was a fine day, with a light wind from the north-north-east and occasional snow showers. There were some large tabular icebergs along the southern horizon. In the afternoon we passed close to a beautiful berg, very irregular in form, all the curves and shadows of a most splendid blue. The lower portion of the side of the iceberg next us formed a long steep slope into the water, and up this slope the surf ran with every heave of the swell, taking in its course the glorious blue of the ice and ending at the top of the glacis in a line of glittering foam.

The evening fell grey and slightly misty, with a number of icebergs looming through the mist. One or two of us were standing on the bridge about midnight looking at what seemed to be a low bank of white fog coming down upon us, when all at once a universal grating and rasping sound and sensation seemed to pervade the ship, and looking over the side we found that instead of sailing in open water we had passed into the edge of the pack, and as far as the eye could reach to the eastward the sea was closely covered with blocks of ice of all sizes up to six or seven feet in length among which the ship ground her way. A cold-looking moon struggled faintly through the cloud and mist and showed the pack vaguely for a mile or so ahead, covered with a light fog through which we could just see several icebergs looming right ahead of us and on either bow, and the masses of ice becoming larger and forming a closer pack as we passed inwards from the outer edge.

It was a wonderful and in a certain sense a beautiful sight, but one which would certainly require for its full enjoyment very fine weather such as we had, or a specially strengthened ship.

The necessary orders were given, and we veered round and slowly passed out of the pack and into open water; and we hung about beyond the line of wash-ice for the short Antarctic night.

On the following morning there were icebergs all round us, some of them of very fine forms. One which we saw all day on the port quarter was gable-shaped with a glorious blue Gothic arch in the centre, and a separate spire over 200 feet high. It was like a gorgeous floating cathedral built of sapphire set in frosted silver.

All day the pack could be seen from the deck stretching away to the east and south as far as the eye could reach, a mass of rugged glittering blocks one piled on the top of another. The ice-blink, a beautiful and characteristic phenomenon, was very marked above the pack—a clear

[1] The substance of a lecture by Sir C. Wyville Thomson, F.R.S., delivered in the City Hall, Glasgow, on November 23, under the arrangements of the Glasgow Science Lecture Association.

band of white reflection rising some 12° above the horizon, and frequently bounded above by a dark-rolled cloud.

The 15th was clear and calm, with a light wind from the south-east. There were innumerable icebergs in all directions, some with their blue cliffs entirely visible from the bridge, and the blue waves lapping about their base, and springing up into fissures and recesses in jets of dazzling foam; some only rising above the horizon and slowly developing their varied outlines, and for a time deluding us into the idea that they were low—sloping gently from the water, and that it might be possible to land upon them. All the very large bergs, and some of them were one or two miles in length, were table-topped, evidently retaining their original position.

About 10 o'clock in the evening our attention was called by the officer of the watch to a very beautiful effect of light. There had been a fine crimson sunset, and now a dark curtain of cloud had sunk almost to the water's edge, leaving between it and the sea a long open line of the most vivid flame-colour, broken here and there by an iceberg, which, according to its position, took a rosy glow from the sky, or merely interrupted it with its cold grey outline.

During the forenoon of February 16 we passed on under sail through a splendid double chain of icebergs, most of them table-topped, and showing little evidence of change of form; and all day, on the southern horizon, berg after berg rose solemnly out of the water, at first a white line only, the blue bounding-cliff growing in height as we ran southwards. Shortly after noon we crossed the Antarctic circle, and a little later we reached our most southern point, lat. 66° 40′ S.; long. 78° 22′ E. exactly fourteen hundred miles from the South Pole.

As the season was advancing, and as there was no special object in our going further south—a proceeding which would have been attended with great risk to an unprotected ship, since, while the temperature of the surface-water ranged between − 1°·67 and − 2°·0 C. (29° and 28°·4 F.), very close to the freezing-point of sea-water, the temperature of the air fell to − 4°·44 C. (24 F.), and once or twice the water began to show that sludgy appearance which we know sets so rapidly, converting in a few hours an open pack into a doubtfully penetrable barrier—Capt. Nares decided upon following the edge of the pack to the north-eastward, towards the position of Wilkes' "Termination Land."

From our most southern point the sea was tolerably clear of ice for at least twenty miles in a south-westerly direction. The whole of the horizon to the south-east was closed by a chain of very uniform and symmetrical flat-topped bergs, all about 200 feet high above the water, one upwards of three miles in length, and several between one and two miles.

During the next week we were making our way slowly to the north-east, along the edge of the pack, sometimes dipping into it a little way, or crossing outlying loose patches.

The pieces of ice on which we were bumping every now and then were 10 to 20 feet in length, rising from 1 to 2 feet out of the water. Most of them were covered with a smooth layer of lately fallen snow, which had apparently not even got splashed with the water which was lapping round the blocks, it was so pure and white.

When the ship struck a block, the ice was usually driven aside unbroken; but the crust of snow was shattered and fell into the water. At the line where the water broke against the ice-blocks, they were all more or less honeycombed and worn-looking, and along this line many of them were of a dirty-yellow colour, probably from the washing of diatoms and crustaceans into the spongy ice. The temperature of the air averaged about − 4°·7 C. (23°·5 F.) in the shade; and that of the surface of the sea − 2°·78 (27° F.); every overhanging ledge of an iceberg was fringed with delicate new icicles, and the "gummy" look of the surface, threatening the formation of young ice, was very evident. The sea was usually a splendid deep blue.

The weather changed during the night of the 23rd, and at daylight on the morning of the 24th the wind was rising fast with a cloudy sky and frequent snow-showers. We were very anxious to get a haul of the dredge in this position, and Capt. Nares had it put over in the hope of getting it up before the weather became too boisterous. The wind and sea rose so fast, however, that it was found necessary to shorten the operation. The dredge was got in safely, but, as we anticipated, it was empty, and had probably never reached the bottom. During the forenoon the weather got rapidly worse. The snow became continuous, and was so thick—blinding clouds of singularly beautiful wheel-like crystals, which stung the face as if they were red hot—that we could scarcely see the length of the ship. We tried to get under the lee of an iceberg; but while reefing an eddy caught the ship and dragged her towards the berg, which she fouled, carrying away her jibboom. At three P.M. things were nearly as bad as they could be. The wind was blowing from the south-east by east, with a hurricane force, in the squalls; the sea was running very high; the temperature had fallen to − 6°·11 C. (21° F.); we were surrounded with icebergs, which we could not see for the sheets of blinding snow, but we could hear the dull boom of the surf dashing upon them. When the gale was at its height we saw the loom of an iceberg on the lee-bow, and we were drifting directly upon it. As there was no time to steam ahead, Capt. Nares went full-speed astern with the four boilers, and set the reefed main topsail aback, and under this sail the ship fortunately gathered stern-way, keeping broadside to the wind, and we drifted past the berg. Towards evening the wind fell a little, and we moved about all night between two bergs, whose position we knew, keeping as much as possible under their lee till daylight.

On the morning of the 25th this storm, which was one of the most trying and critical episodes in the whole voyage, was entirely over, and the air was calm and clear. We pushed a couple of miles into the pack to the north-east. We were now about fifteen miles from the position of Termination Land on the chart sent by Lieut. Wilkes to Capt. Ross. The sky was clear to the southward and eastward, the direction of the supposed land, but there was nothing which could be taken even for an "appearance of land." A sounding taken close to the edge of the pack had given a depth of 1,300 fathoms, and there was no trace of land *débris* on any of the icebergs. We were forced to conclude that Lieut. Wilkes had been in error, and that there was no land in this position. We now ran on steadily in a north-easterly direction towards Cape Otway, and on March 4 we passed a low irregular iceberg, the last we saw during our southern cruise of 1874. We sighted Cape Otway on March 16, and on the 17th we anchored off Sandridge Pier, in Hobson's Bay.

In these high southern latitudes, at all events at the point where we crossed the Antarctic circle, it seems that originally all the icebergs are tabular, the surface perfectly level and parallel with the surface of the sea, a cliff on an average 200 feet high bounding the berg. The top is covered with a layer of the whitest snow; now and then a small flock of petrels take up their quarters upon it, and trample and soil some few square yards, but after their departure one of the frequent snow showers restores it in a few minutes to its virgin whiteness. The upper part of the cliff is of a pale blue, which gradually deepens towards the base. When looked at closely, the face of the cliff is seen to be traversed by a delicate ruling of faint blue lines, the lines more distant above, and becoming gradually closer. The distance between the well-marked lines near the top of the berg may be a foot, or even more; while near the surface of the water it is not more than 2 or 3 inches, and the spaces between the blue lines have lost their dead whiteness and have become hyaline or bluish. The blue lines are very unequal in their strength and in their depth of colouring; sometimes a group of very dark lines gives a marked character to a part of a berg. Between the stronger blue lines near the top of the cliff a system of closer bands may be observed, marking the division of the ice by still finer planes of lamination, but these are blended and lost in the narrower spaces towards the water-line. The blue lines are the sections of sheets of clear ice; the

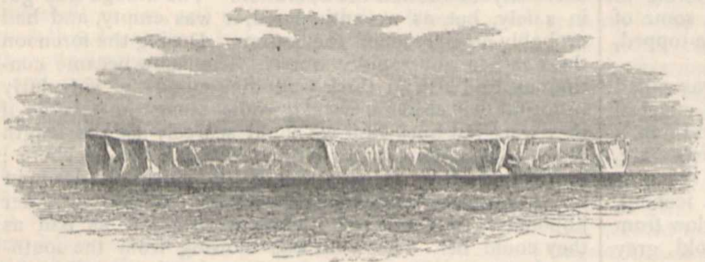


FIG. 1.—February 11, 1874. Lat. 60° 52′ S., Long. 80° 20′ E.

white intervening bands are the sections of layers of ice where the particles are not in such close contact—ice probably containing some air.

The stratification in all the icebergs which we saw, was, I believe, originally horizontal and conformable, or very nearly so. In very many of them the strata had become inclined at various angles, or vertical, or reversed; in many they were traversed by faults, or twisted or contorted or displaced, but I never saw a single instance of deviation from the horizontal and symmetrical stratification which could in any way be referred to original structure; which could not, in fact, be at once accounted for by changes which we had an opportunity of observing taking place in the icebergs.

I think there can be no reasonable doubt from their shape, and from their remarkable uniformity of character,

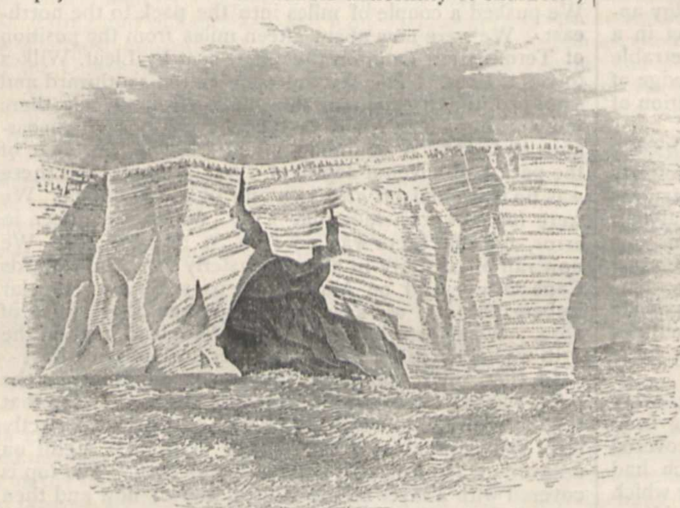


FIG. 2.—February 23, 1874. Lat. 64° 15′ S., Long. 93° 24′ E.

that these great table-topped icebergs which we saw all around us, and closing in our southern and eastern horizon at the southernmost point of our voyage, and breaking down and melting a little further to the north, are prismatic blocks riven from the edge of the great Antarctic ice-sheet, portions of whose vertical cliff were seen by Ross in January, 1841, and in February, 1842, in lat. 78° 4′ and lat. 77° 49′ S. to the southward of New Zealand, and by Lieut.-Commandant Ringgold in the U.S. ship *Porpoise*, on February 1 and 2, 1840, in long. 130° 36′ E. lat. 65° 49′ S. There is unfortunately great difficulty in determining when the wall of ice to which the term "ice-barrier" was restricted by Capt. Ross was seen by Lieut. Wilkes or any of his party, since Lieut. Wilkes applies the term indiscriminately to the solid ice-walls and to the masses of moving pack by which his progress was from time to time interrupted. The wall is satisfactorily described at one or two points only.

I believe that the stratification of those portions of the icebergs which were visible to us is due entirely to successive accumulations of snow upon a nearly level surface. The spaces between the transverse blue lines on the bergs may possibly represent approximately the snow accumulation of successive seasons. The direct radiant heat of the sun is very great in these latitudes, and during summer the immediate surface of the snow is frequently melted in the middle of the day, the water percolating down among the snow beneath and freezing again at night, or when it has trickled down into the shade. This process repeated every clear day for the two or three months of summer might well convert a very considerable belt of snow into ice more or less compact. That the process does go on we had ample evidence in the icicles fringing the snow which was lying upon flat pieces of the pack, which dropped rapidly in the sun even when the thermometer in the shade was several degrees below the freezing point.

The finer laminations may probably indicate the more feeble results of the same process after successive snow-falls. As I have already said there was not, so far as we could see, in any iceberg the slightest trace of structure stamped upon the ice in passing down a valley, or during its progress over *roches montonnées* or any other form of uneven land; the only structure except the parallel stratification which we ever observed which could be regarded as bearing upon the mode of original formation of the ice-mass was an occasional local thinning out of some of the layers and thickening of others, just such an appearance as might be expected to result from the occasional drifting of large beds of snow before they have time to become consolidated.

We certainly never saw any trace of gravel or stones or any foreign matter necessarily derived from land on an iceberg; several showed vertical or irregular fissures filled with discoloured ice or snow, but when looked at closely the discoloration proved usually to be very slight, and the effect at a distance was chiefly due to the foreign material filling the fissure reflecting light less perfectly than the general surface of the berg. In one or two distant bergs there seemed to be thick horizontal beds of ice deeply coloured brown or bottle-green, but this was also, I believe, chiefly an effect of light.

In the pack, which is made up of fragments of all sizes of berg-ice mixed with masses of salt-water ice, the berg-ice is almost always either white with pale-blue streaks, blue with a white opalescence, or rarely deep blue, or still more rarely black from absolute transparency; it is seldom soiled in any way. It is so occasionally; on the 10th we passed, not far from our turning-point, a piece of berg-ice with a small flock of penguins upon it. The birds had evidently been there for some time for the snow on the surface of the ice was trampled into a dirty brownish mud; another fall of snow would have converted this layer into a discoloured vein in the block.

C. WYVILLE THOMSON

(*To be continued.*)

OUR ASTRONOMICAL COLUMN

CHANGE OF COLOUR IN A STAR.—Dr. Klein of Cologne first directed attention to a periodical change of colour in the star α Ursæ Majoris, from yellow to an intense fiery red, and estimated the duration of the period to be about five weeks. In a communication to the *Astronomische Nachrichten*, he gives observations by Herr Weber from August 22, 1876, to October 24, which support his inference. Thus on September 5 and October 10 the star was considered "stark feuerroth" and "feuerroth, tief," while on August 22 and October 24, it was noted "weissgelb" and "gelb, schwach blaulich" respectively: from September 5 to October 10 is a period of thirty-five days, confirming the earlier estimation by Dr. Klein. Herr Weber observed with a Steinheil achromatic of $2\frac{3}{8}$ inches aperture, and $3\frac{1}{2}$ feet focal length with a power of 90. Probably since the suspicion of a regular change of colour was made known, the star has engaged the attention of other amateurs, who may be able to add something to the evidence pro or con.

NEW DOUBLE STARS.—Mr. Ormond Stone, Director of the Observatory, Cincinnati, has circulated a list of fifty double stars, varying in distance from 0"·8 to 8"·0, which are assumed to be new, and which have been recently found by Mr. Howe with the eleven-inch refractor; the whole are included between 8° and 40° south declination. Positions are given for 1880, with estimated angles and distances, and the magnitudes of the components. Since the number of rapidly-revolving double-stars is probably much greater than at present known, it appears very desirable that micrometrical measures of objects newly discovered should be at once placed upon record, in place of merely estimated angles, which form no satisfactory starting-points for the calculation of orbits.

THE BINARY STAR η CASSIOPEÆ.—Dr. Gruber, of Buda-Pesth, has investigated elements of this binary, from normal position-angles formed with the aid of Duner's orbit. His figures agree as nearly as can be expected in such a case with those obtained by the careful calculation of Doberck. With the value of annual parallax obtained by O. Struve, viz., 0"·154, we find—

	Gruber.		Doberck.
Mass of the system	4·632	...	5·256
Semi-axis major	56·097	...	63·831

The sun's mass is taken for unity, and the semi-axis major is expressed in mean distances of the earth from the sun. Dr. Gruber's period is $195\frac{1}{4}$ years.

In the only two cases which we are at present able to compare with that of η Cassiopeæ we have for α Centauri, mass of system = 2·2 sun-masses, while for 70 Ophiuchi the similar value is 3·1.

THE MASS OF NEPTUNE.—It is understood that M. Leverrier, from his final researches on the motion of Uranus, obtains a sensibly larger value for the mass of Neptune than has been assigned by Prof. Newcomb, and one approximating to that which was inferred many years since from Mr. Lassell's direct measures of the distances of the satellite.

THE NAUTICAL ALMANAC FOR 1880 has been published during the last week. The ephemeris of the planet Saturn, which since the appearance of the almanac in its improved form has hitherto been founded upon Bouvard's Tables, is computed from heliocentric plans communicated by M. Leverrier in advance of the publication of his new tables in vol. xii. of the *Annales* of the Observatory of Paris; the number of standard stars has been increased from 149 to 197, and in extending Damoiseau's Tables of Jupiter's satellites, certain corrections supplied by Prof. Adams have been introduced.

The impression of the *Nautical Almanac* now considerably exceeds 20,000 copies.

METEOROLOGICAL NOTES

ACCELERATED TRANSMISSION OF WEATHER MAPS.—The *New York Herald* of November 7 publishes a map of the weather of that morning, exhibiting the lines of atmospheric pressures and of the temperatures over the United States. The meteorological charting which was finished at the Central Office in Washington at 10 A.M. was immediately transmitted from Washington in *fac-simile* by telegraph to Philadelphia, where it was received at 10.30 A.M. It was shortly thereafter published in the supplement of the *New York Herald* of the same day, being the first occasion on which such telegraphic charting had appeared in any newspaper. The fact of telegraphing and printing such charts solves one of the greatest difficulties of exchanges of Weather Reports. It may now be regarded as only a question of time when the more important newspapers of our British large towns will be in a position to present their readers every morning with a chart of the weather as existing only two or three hours before going to press; and indeed it will not be till this result be effected that the practical utility of weather warnings will be properly developed, owing to our close proximity to the Atlantic and the rate at which our weather-changes pass to the eastward.

GREAT STORM OF WIND AT SYDNEY.—Mr. Russell, the Government astronomer at Sydney, reports that during a heavy storm of wind which occurred in that part of Australia on Sunday, September 10, the wind, in a gust lasting one or two minutes, attained the extraordinary rate of velocity of 153 miles per hour, as ascertained by Robinson's cup anemometer; and that during the twelve minutes, from 12.18 to 12.30 A.M. $22\frac{1}{2}$ miles of wind passed the Observatory, being at the rate of 112 miles per hour. This extraordinary recorded velocity may be regarded as a new contribution to meteorological observation, and we look with much interest to the description which will doubtless be given of the method by which it was determined. It scarcely admits of a doubt that the maximum velocity or force of the wind that occurs in great storms is frequently much understated.

THE TEMPERATURE OF THE NORTHERN PART OF THE ATLANTIC.—An important contribution to the physics of the North Atlantic appears in the November number of *Petermann's Geogr. Mittheilungen*, in a paper by Prof. Mohn on the temperature of the sea beween Norway, Scotland, Iceland, and Spitzbergen The material employed in the discussion consists of the observations collected by the Norwegian Meteorological Institute from the lighthouses on the coast of Norway and from Norwegian ships, and the observations published by the Scottish Meteorological Society from their stations in Scotland, Farö, and Iceland—the observing stations, exclusive of the ships, numbering twenty-two. At places where observations only for two or three years are available, they are reduced to the longer period of the nearest station by the process of differentiation, with the result that virtually the averages are all good and fairly comparable with each other. The results are represented on seven charts, well executed in colours, showing by six distinct shades, as well as by isothermal lines, the distribution of temperature over this portion of the Atlantic for each set of two months and for the year, and the changes in the positions of the same temperatures from season to season. The outstanding feature of the charts is a strong-marked warm thermal axis, taking a north-easterly direction about 150 miles to westward of Scotland and Norway, extending even beyond the North Cape. Along this line of warm water temperatures are considerably higher than elsewhere in the same latitude. On the July-August chart, however, the warm axis approaches much nearer to the coast of Norway, and extends only from off the Naze to about lat. 66°. From June to September the North Sea is coldest on the Scottish coast and warmest in the Skagerak, but during the rest of the year this is reversed.

In other words, during the hottest months the influence of the heated land is powerfully felt, but during the other two-thirds of the year the peculiar distribution of the temperature is determined by the strong south-westerly winds and current of the Atlantic. This influence of the Atlantic on the temperature of Western Europe is enormous, the thermic anomaly for January being estimated by Prof. Mohn to be 10°·8 in the interior of Norway, 25°·2 in Scotland, 32°·4 in the north-west of Iceland, while in Lofoten it amounts to 41°·4. In May–June and in a slight degree in July–August, a large extent of cold water appears as if thrust out from the Arctic Sea west of Jan Mayen to the south-eastwards as far as Farö, deflecting and crowding together the whole of the isothermals over this region in a most remarkable manner. This is a point which well deserves the most careful investigation, not merely from its evident importance to the fisheries of this part of the Atlantic, but also from its meteorological significance, it being in May and June that atmospheric pressure reaches its annual maximum, northerly and easterly winds their greatest predominance, the weather becomes brightest and clearest, and the rainfall sinks to its annual minimum over the extreme north-west of Europe. An instructive comparison is also made between the temperature of the sea and that of the air, and a valuable discussion is added of the observations made at different depths of the sea between Iceland and Norway; but for these and other interesting points we must refer to the paper itself.

BIOLOGICAL NOTES

NEW FRESH-WATER RHIZOPODS.—For many years the small group known as Actinophrys, first accurately described by Ehrenberg, remained somewhat isolated, distinguished among unicellular forms by its nearly constant spherical form, and the persistence of its straight radiating processes or "pseudopodia." But in recent years a whole series of organisms has been described, and has attained sufficient prominence to constitute, with Actinophrys, an order denominated Heliozoa. Many interesting papers have appeared in the *Archiv für mikroskopische Anatomie* on these organisms, contributed by E. Hertwig, Lesser, Cienkowski, Greeff, and others; and in our own country Mr. W. Archer has been the most active and successful student of the Heliozoa, his papers having been principally published in the *Quarterly Journal of Microscopical Science.* From these one derives an idea of these animalcules as being the most complex of the free forms which possess pseudopodia, and are at the same time unicellular; they are neither multicellular and differentiated like the Radiolaria, nor organised like the ciliated Infusoria. While a few forms, as Actinophrys, are quite devoid of skeleton, most of them possess certain hard parts, consisting, it may be, of a single solid and globular piece, but in other cases of very minute rod-shaped spiculos, sometimes siliceous, sometimes easily soluble. These are either disposed so as to invest the main body of the organism more or less closely, or radially, projecting as spines. When food is taken, these minute hard parts can be pushed aside to allow access to the central body-mass; and the same occurs when indigestible material is thrust out. Locomotion is usually very slow; most Heliozoans move by balancing themselves on the tips of their pseudopodia, and thus very gradually rolling onwards. Multiplication of the organisms is effected by division, either simple, or occurring after encystation. Some forms are remarkable for containing a great abundance of chlorophyll granules.

RE-ARRANGEMENT OF THE ORDERS OF ENDOGENS.—At a recent meeting of the Linnean Society of London, Mr. Bentham proposed an entirely new arrangement of the orders of Endogens, which he believes to be more in accordance with their genetic affinities and the essential points of their structure, than any at present in use. He proposes to classify Endogens under four series, viz., 1. EPIGYNÆ; flowers with a double usually petaloid perianth; ovary usually inferior syncarpous. 2. CORONARIEÆ; flowers with a double usually petaloid perianth; ovary superior, almost always syncarpous. 3. NUDIFLORÆ; flowers usually achlamydeous, or with a dry perianth; ovary mostly apocarpous; and 4. GLUMALES; perianth replaced by membranous scales (pales or lodicules); ovary always uniovular. The orders are arranged thus in the four series:—in the first, Hydrocharideæ, Scitamineæ (including Musaceæ, Cannaceæ, &c.), Orchideæ, Burmanniaceæ, Irideæ, Amaryllideæ (including Hæmodoraceæ), Taccaceæ, Dioscorideæ, and Bromeliaceæ (?); to the second, Roxburghiaceæ, Liliaceæ (including Melanthaceæ, Smilaceæ, &c.), Pontederiaceæ, Philydraceæ, Xyrideæ, Commelynaceæ, Junceæ, and Palmæ; to the third, Pandaneæ, Aroideæ, Typhaceæ, Lemnaceæ, Naiades (including Juncagineæ), and Alismaceæ (?); and to the fourth, Eriocauleæ, Centrolepideæ, Restiaceæ, Cyperaceæ, and Gramineæ.

THE SENSATION OF SOUND.—At a recent meeting of the Vienna Academy a paper was communicated by Dr. Isidor Hein "On the Relations between Perceptions of Touch and of Hearing." His conclusions are these:—1. The sound produced by striking a solid body is always accompanied by a sensation of touch, which, like the sound, differs according to the nature of the body. If the sound is different in different parts of a body, there goes along with the variation of the sound, a variation in the touch-sensation; and if the surface be divided into several sections according to differences in sound, a congruent division may be made on the basis of touch. 2. On bringing a struck body towards a reflecting wall, the sound and touch-perceptions show similar variations. 3. To the touch-perception in question correspond vibratory motions of the exterior body, produced even with the weakest striking, whereas sound only begins to be perceived with impacts of a certain intensity. 4. The sense of touch is capable of perceiving vibrations and comparing the differences of these. It brings hereby to consciousness, a special quality of touch-sensation, which is to be distinguished from sensation of pressure. 5. This distinguishing power of the organ of touch, not sufficiently appreciated hitherto, offers practical medicine a peculiar mode of investigation, which greatly enlarges the doctrine of the physical symptoms of the human organism, and for which the author suggests the (German) name of "Erschutterungs-palpation."

THE ABSORPTION OF ORGANIC MATTER BY PLANTS.—In a communication from Prof. Calderon, of the Institute of Las Palmas, Canary Isles, he contests the ordinary view that the nitrogen of the tissues of plants is derived entirely from the nitrates and ammoniacal salts absorbed through the roots. He does not, however, adopt the old theory that the source is the free nitrogen of the atmosphere, but rather the nitrogenous organic matter which is always floating in the air. The nutrition of plants he divides into three classes: *necrophagous*, the absorption of dead organic matter in various stages of decomposition; *plasmophagous*, the assimilation of living organic matter without elimination, or distinction of any kind between useful and useless substances, such as the nutrition of parasites; and *biophagous*, the absorption of living organisms, such as that known in the case of insectivorous plants. A further illustration of the latter kind of nutrition is, according to Prof. Calderon, furnished by all plants provided with viscid hairs or a glutinous excretion, the object of which is the detention and destruction of small insects. To prove the importance of the nitrogenous substances floating in the air to the life of plants, he deprived air of all organic matter in the mode described by Prof. Tyndall, and subjected lichens to the access only of this filtered air and of distilled water, when he found all their physiological functions to be suddenly suspended.

NOTES

THE Kew Committee have appointed Mr. G. N. Whipple, B.Sc., Superintendent of the Observatory. Mr. Whipple's connection with the establishment is of nearly nineteen years standing, he having entered it in January, 1858.

DR. PETERMANN has sent us a map in which he has embodied the discoveries of the English Arctic Expedition. He appears still to maintain his well-known hypothesis of the continuation of the north coast of Greenland towards and beyond the North Pole. He draws the line of the visible horizon of the English Expedition at about fifty nautical miles north of Grant Land, its furthest north point being the 84th deg. of lat., and remarks that north of this line may be either land or sea. Of course no one can contradict this. He has, however, restored President's Land in rather a remarkable position, identifying it apparently with Beaumont's Cape Britannia, to the north of Beaumont's furthest point on the north coast of Greenland. The map will be found a very useful one in studying the route of the expedition, especially as compared with that of the *Polaris*.

CAPT. NARES and the other Arctic officers have been entertained by the Greenwich cadets and by the Royal Naval Club at Portsmouth. Capt. Nares stated that when the whole story of the efforts of the expedition is published, it will be seen that better work could not have been done.

THE constituents of fodder-remains of rhinoceroses and mammoths in Northern Siberia have been examined by several observers, and the conclusion arrived at that these animals lived in the places where their frozen bodies have been found, on plants which are still to be met with in Northern Siberia. New ground for this opinion has been furnished by M. Schmalhausen to the St. Petersburg Academy, who has examined microscopically the constituents of a mass of dark-brown matter extracted from hollows in the teeth of a rhinoceros in the Irkutski Museum. That this was truly the remains of fodder of the animal seemed clear from the appearance and the macerated character of the vegetable substance, of which only the woody and cuticular parts showed a more or less distinct structure. The greater portion of the piece consisted of leaf-remains, with here and there a fragment of stem. For the most part the stem and leaf-fragments were those of monocotyledonous plants, probably of Gramineæ; there were also, in less quantity, leaf-fragments of dicotyledonous plants. Besides leaf-shreds of Coniferæ, there were woody pieces which indicated the existence of Picea (Obovata ?), Abies (Sibiria ?), Larix (Sibirica ?), Gnetaceæ, Betulaceæ, and Salicineæ. If it is scarcely possible to determine certainly the species of a plant merely from some of the wood received in the state indicated, and from the structure of the leaf epidermis, it yet seems unquestionable that these remains must be referred to northern plants and to such as are still partly found in the high north.

MR. COXWELL writing to the *Daily News* in reference to Arctic ballooning, maintains that the ordinary practice of ballooning would be quite unsuitable for the conditions found in the Arctic regions.

M. MORITZ, director of the Tiflis Observatory, makes an interesting communication on the results of an examination of the magnetic instruments there last July by Prof. Smirnoff, of Kasan, who has been engaged during the last six years travelling through to determine the elements of terrestrial magnetism. The instruments employed by Prof. Smirnoff, which cost 300 roubles, were all verified at Kew, and are furnished with microscopes and all the other accessories of the English system. The compass has three needles (No. 4, No. 3, and No. 16), four inches in length, and the mean of the readings of the three needles is the true inclination. At Tiflis there are two compasses long in use, constructed according to the old method of Gambey, which are now generally looked upon as inferior instruments. They cost respectively only 165 and 40 roubles. The results of a very careful comparison are these:—Assuming the mean of the indications of the three Kew needles as the true inclination the errors of the instruments were, Kew No. 4, $-0'.25$; No. 3, $+2'.45$; and No. 16, $-2'.22$; the error of the large Gambey being $+0'.30$, and of the small Gambey $-0'.09$. M. Moritz remarks that the microscope and detached circle on which the readings are made, and which add so greatly to the price of the English compass, appear to add nothing to the precision with which the inclination is determined, facilities for minute readings of the compass being made counting but little, it being the form of the pivots of the physical axis of the needle which stamps its character on the instrument.

"ON Some Insect Deformities," by Dr. Hermann A. Hagen, is the title of No. 9, vol. ii., of the *Memoirs* of the Museum of Comparative Zoology, Harvard College, Cambridge, U.S.

WE have received from Herr Schmidt, publisher, Zürich, Dr. Hermann Berge's "Beiträge zur Entwicklungsgeschichte von *Bryophyllum* Calycinum," and Dr. Gustav Schoch's "Die Schweizerischen Orthopteren."

THE following German publications have been sent us by Messrs. Williams and Norgate:—"Ueber die Zugstrassen der Vögel," by J. A. Palmén, of Helsingfors; "Grundzüge der Mikrophotographie," by Max Hauer; "Sammlung wissenschaftlicher Vorträge von Prof. Wilhelm Förster;" "Die Darwin'schen Theorien und ihre Stellung zur Philosophie, Religion, und Moral," by Rudolf Schmid.

THE Goldsmiths' Company, whose donation to the Chemical Society we noted last week, have voted 500*l.* in aid of the fund for extending Edinburgh University buildings. It is stated that this Company spends annually 6,000*l.* for educational purposes alone. We wish the other City Companies would follow such a good example.

THE following were elected office-bearers of the Royal Society of Edinburgh on Monday last:—President, Sir William Thomson; Vice-Presidents, Rev. Dr. Lindsay Alexander, Bishop Cotterill, Sir Alexander Grant, Prof. Kelland, Lord Neaves, and David Stevenson; General Secretary, Prof. Balfour; Secretaries to Ordinary Meetings, Professors Tait and Turner; Treasurer, David Smith; Curator of Library and Museum, Prof. Maclagan; Members of Council, Prof. Crum Brown, Dr. James Bryce, Alexander Buchan, Dr. Matthews Duncan, Dr. A. Fleming, Dr. T. Harvey, D. Milne Home, Prof. McKendrick, Dr. C. Morehead, Sir C. Wyville Thomson, Dr. R. H. Traquair, and Dr. Robert Wyld.

THE next meeting of the French Association for the Advancement of Science will take place, as we have already intimated, at Havre in the end of August, 1877, under the presidency of Dr. Broca. A local committee has been already formed at Havre under the presidency of M. Mazurier, the mayor. At the first meeting M. Gariel, the secretary of the committee, suggested many useful and interesting experiments to be made in the Seine or the sea. Special efforts will be made to procure a large attendance of English *savants* and members of the British Association. It is reported that a special deputation will be sent to the Dublin meeting of the British Association. The place of meeting for 1878 has not yet been determined upon, but it will probably be Versailles or Paris.

THE *Times* of Monday contains a journal kept by the Rev. Mr. Lawes during a voyage from Port Moresby to China Straits, New Guinea. It contains a good many new facts of interest.

At Hood's Bay they sailed up a considerable river, and near the coast found a large village, regularly laid out in streets and squares, scrupulously clean, with gardens and carefully cultivated flowers. Canoes of large size and excellent make they saw being hewed out with stone hatchets. At a lagoon at Cape Rodney a regular lake village was found, the lagoon leading up to a considerable river. Near Table Point a large canoe "manned" by at least twenty-one women, came alongside; it is reported at Port Moresby, and all along the coast, that there is a village of women somewhere near Amazon Bay.

AT the last meeting of the French Geographical Society, the president, M. Malte Brun, intimated that the Council General of the Seine had voted a sum of 2,000 francs to assist M. Largeau in his exploration of the Sahara. M. Largeau left on November 19 for Algiers, whence he will proceed to Constantine, Tuggurth, and Central Sahara. The subscription on his behalf is proceeding. The Geographical Council of Lyons has voted a small subsidy of 12*l.*, and the Municipal Council of Lyons will send him, within a very few days, a considerable donation. M. Melidin, a rich landed gentleman, has offered to the Society to maintain during one year any traveller approved by the Society. The offer was accepted with thanks. On December 20 the general secretary will deliver a lecture on "The Progress of Geography during 1876," and on the following day the annual banquet will take place at the Grand Hotel.

AT the Geographical Society on Monday night papers were read on the results of Col. Gordon's expedition in Central Africa, from General Stone, the Rev. E. J. Davis, and Signor Gessi· Sir Rutherford Alcock announced the death of an African missionary, Mr. Redman, who, by his explorations, had materially helped subsequent travellers, and who first suggested that there was a great system of lakes in Central Africa.

A GREAT extension of the medical department of the University of Heidelberg has been in progress for some time. Large additions to the Medical School and the Hospital attached to it have been made, and when completed, as it will be shortly, this institution will be one of the most complete in Europe. Every provision has been made for scientific investigation in connection with the healing art in all its departments.

THE *Archiv für Anthropologie* (ix. 173) contains a paper by Herr L. Lindenschmidt, in which he pronounces his conviction that the drawings upon the fossil bones found in the Thayingen Cave are spurious and the result of intentional deception. These drawings, which represent a bear, a fox, and a stag, were generally admired in scientific circles in 1874 (when they were found) as being amongst the most perfect specimens of the kind; they also led to the supposition of the highly-civilised state of the ancient cave inhabitants. Herr Lindenschmidt produces evidence to the effect that precisely the same drawings are contained in a little work by Leutemann, "Die Thiergärten und Menagerien mit Ihren Insassen," which was published in 1868, *i.e.* six years before the discovery of the cave near Thayingen. As the work in question had a very wide circulation in Germany the inference drawn is obvious.

A NOTICE in the *Ostsee Zeitung* accounts for the frequent deficiencies in the aroma of foreign cigars by announcing that from Guben whole waggon-loads of dried cherry-leaves are weekly exported for the manufacture of "tobacco."

A SUCCESSFUL *soirée* of the Manchester Scientific Students' Association was held last Friday, when Prof. Williamson gave an address on Insectivorous Plants.

A VERY fine new university building has been erected at Kiel, one marked peculiarity of which is that it has no "carcer," or prison, which hitherto, it seems, has been an invariable appendage to German universities.

THERE met in Berlin, a few days ago, a German Government Commission whose business it is to look after the moors and marshes of Germany. They resolved to establish an experimental station at Bremen, to be opened on April 1 next year, drew up a plan for obtaining statistics and a topography of moors, and made arrangements for the complete canalisation of the moors in the Duchy of Bremen. The labours of the Commission will include the whole of Germany.

THE directors of the Swedish Government railways have turned their special attention to the frequent occurrence of colour-blindness amongst their engine-drivers and other officials. Prof. Holmgren has lately examined the whole staff of the Upsala-Gefle Railway, and amongst the 266 persons examined has found no less than eighteen who suffered from this defect, and who therefore were utterly useless and unfit for railway service. This investigation proves that cases of colour-blindness are far more frequent than is generally supposed, and that our railway companies would do well to follow the example of the Swedish State railways.

THE *Kölnische Zeitung* of the 14th inst. declares that the recent sudden cold was a perfectly abnormal meteorological phenomenon, and all the more so since it did not only visit a part but nearly the whole of Europe. Reports of heavy snowstorms have come from the whole of North Germany, Austria, Servia, and Roumania, and the temperature had fallen below freezing in all these countries as well as in Western France, Italy as far south as Rome, and the whole of European Turkey. In Hungary the cold reached 13° C. Violent gales were raging in the Black Sea, the Adriatic, and the Baltic. In another article, dated from the Teutoburg forest, the paper reports that the whole of that district is snowed up, and a valuable crop of vegetables, beetroot, &c., has been destroyed by the cold.

A MONS. MÉNIER, of Bordeaux, has invented a new contrivance for the steering of balloons. The mechanism is placed behind the car, and by a clever arrangement of network acts upon a belt which encircles the body of the balloon, extending about four or five degrees above and below a horizontal plane through its centre—its equator, so to say. The rudder is plane, and can be used as a sail. The balloons are said to move obliquely upwards and downwards and also sideways, according to the position of the rudder. The sideway motion is very likely facilitated by changing the position of ballast. One circumstance, which may be of special practical use, is that a balloon provided with this new apparatus, when falling to the ground, can be made to touch the earth's surface very obliquely and thus avoid any sudden shock, and at the same time facilitate a safe anchoring.

HERR EDWARD TREWENDT, publisher, Breslau, has issued the prospectus of a New Encyclopædia of the Natural Sciences, which will include all departments of science. The first part will appear on January 1, 1878.

MORE than 18,000 francs have been already collected for the erection of Arago's statue at Perpignan.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus radiatus*) from India, presented by Mr. E. F. Mathews; a Slender-billed Cockatoo (*Licmetis tenuirostris*) from South Australia, presented by Mr. Stevens; a Gannet (*Sula bassana*), European, presented by Mr. R. H. W. Leach; a Peregrine Falcon (*Falco peregrinus*), captured at sea, presented by Mr. A. Whyte; a Grivet Monkey (*Cercopithecus griseo-viridis*) from Nubia, an Indian Leopard (*Felis pardus*) from India, a Hooded Crane (*Grus monachus*) from Japan, a Globose Curassow (*Crax globicera*) from Central America, deposited; an Indian Muntjac (*Cervulus muntjac*), a Japanese Deer (*Cervus sika*), born in the Gardens.

SCIENTIFIC SERIALS

Morphologisches Jahrbuch, vol. ii. Part 2.—On the structure of the toes of Batrachia, &c., by Dr. F. Leydig (32 pages).—On the valvular apparatus in the conus arteriosus of Selachians and Ganoids, by Dr. Stöhr.—Contribution to the anatomy and histology of Asteroids and Ophiuroids, by Wichard Lange (46 pages, 3 plates).

REICHERT and Du Bois Reymond's *Archiv*, September, 1876.—Studies on animal heat, Part 3 (conclusion), by Dr. A. Adamkiewicz.—Contribution to the theory of the growth of bone, by Dr. L. Lotze.—On the negative variation of the muscular current during contraction, Part 3 (conclusion), by Du Bois Reymond.

Archiv für mikroskopische Anatomie, vol. xiii. Part 1, July, 1876.—Rhizopod studies, No. vi., by F. E. Schulze.—On the anatomy and histology of the Coccidæ, by E. L. Mark.—Studies of the development of Gastropods, by N. Bobretzky (75 pages), with 6 plates illustrating the development of *Nassa mutabilis*, Natica, and Fusus.—On the hypothesis of discharge and the motor end plates, by W. Krause.

Bulletin de l'Académie Impériale des Sciences de St. Pétersbourg, t. xxii., No. 2.—This part contains the following papers of interest:—Diagnoses plantarum novarum Japoniae et Mandshuriae, decas xx. (continued from last vol.), by C. J. Maximovicz.—On the plants of the bear period found in the deposits of the Ogour river, a tributary to the Jenissei, in Siberia, by J. Schmalhausen.—Preliminary communication by the same, on a microscopical examination of the food-remains of Siberian fossil rhinoceros, viz., *Rhinoceros antiquitatis seu tichorinus*.—On the supposed satellite of Procyon (α Canis min.), by O. Struve. The other contents are only of archæological and philological interest.

SOCIETIES AND ACADEMIES

LONDON

Linnean Society, November 16.—Prof. Allman, president, in the chair.—Messrs. J. C. Oman, R. H. Peck, and D. G. Rutherford were duly elected fellows.—Mr. H. N. Moseley, of H.M.S. *Challenger*, read a paper on the flora of Marion Island. This island possesses considerable interest from its isolation and being within the Antarctic drift. It is about 1,000 miles from the African Continent, 450 from the Crozets, 1,200 from the desolate Kerguelen Island, above 2,000 from Tristan D'Acunha, and 4,500 from the Falklands, to which, nevertheless, its flora appears related. It is of volcanic origin and snowclad. The rocks at half-tide are covered with *Darvilea utilis*, above high tide *Tillæa moschata* is found in abundance, and beyond the beach a swampy peaty soil covers the rocks, where there is a thick growth of herbage; this is principally composed of species of *Acæna*, *Azorella*, and *Festuca*, the first of these three being the most abundant plant on the island, though the latter grass is by no means scarce. The cabbage-like plant *Pringlea antiscorbutica* is less profuse than at Kerguelen's Land. Some of the ranunculus group are met with at water pools near the sea; four kinds of ferns were obtained, *Lomaria Alpina* being the most numerous. Lichens are scarce, but mosses in plenty form yellow patches, which stand out conspicuously midst the green vegetation, which rises to an altitude of probably 2,000 feet. From the occurrence of *Pringlea* on Marion Island, the Crozets, and Kerguelen Island, and the existence of fossil tree-trunks on the two latter, the author surmises an ancient land connection between them.—A memoir on the birds collected by Prof. Steere (U.S. Michigan) in the Philippine Archipelago was read by Mr. R. Bowdler Sharpe, and copious coloured drawings of the new and rare forms exhibited and commented on. Although it is but lately that Lord Tweeddale's (President Zool. Soc.) remarkable monograph on the Philippine birds was published, with immense additions to the avifauna, yet Steere's collection has yielded over sixty hitherto unknown species. Many novelties may therefore still be expected as further exploration proceeds. The recorded species of birds from the Philippines at the present now amount to 285.—A letter containing observations on the American Grasshopper (*Caloptenus femur-rubrum*), with remarks on the same by Mr. Frederick Smith, was noticed.—Mr. Moseley exhibited some insular floral collections in illustration of his paper and of the various parts touched at by the *Challenger*. He also called attention to a series of volumes and pamphlets, &c., on natural history obtained by him in Japan.

Zoological Society, November 21.—Prof. Flowers, F.R.S., vice-president, in the chair. The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of October.—Mr. Sclater exhibited and made remarks on the skin of a young rhinoceros (*R. sondaicus*), belonging to Mr. W. Jamrach, which had been captured in the Sunderbunds, near Calcutta, in May last.—The Secretary exhibited on behalf of Mr. Andrew Anderson, a coloured drawing of a specimen of *Emys hamiltoni*, lately captured at Futtehgurh (Ganges). The occurrence of this *Emys*, chiefly confined to Lower Bengal, so far west as Futtehgurh, was considered as of much interest.—A letter was read from Count T. Salvadori, containing remarks on some of the birds mentioned by Signor D'Albertis, as seen by him during his first excursion up the Fly River.—A communication was read from Mr. G. B. Sowerby, jun., containing descriptions of six new species of shells, from the collections of the Marchioness Paulucci and Dr. Prevost.—Mr. Edward R. Alston read a paper containing the descriptions of two new species of *Hesperomys* from Central America, which he proposed to call respectively *Hesperomys teguina* and *H. couesi*.—A paper was read by Prof. Garrod, F.R.S., on the Chinese Deer, named *Lophotragus michianus*, by Mr. Swinhoe, in which he showed that the species so called was identical with *Elaphodus cephalophus* (A. Milne Edwards), obtained by Père David in Moupin. The close affinity between the genera *Elaphodus* and *Cervulus* was demonstrated, the latter differing little more than in the possession of frontal cutaneous glands not found in the former.—Mr. Arthur G. Butler read a paper containing descriptions of new species of Lepidoptera, from New Guinea, with a notice of a new genus.—A communication was read from Dr. J. S. Bowerbank, being the eighth of his series of "Contributions to a General History of the Spongiadæ."

Meteorological Society, November 15.—Mr. H. S. Eaton, M.A., president, in the chair.—Messrs. R. A. Allison, John Evans, F.R.S., Dr. W. Marcet, F.R.S., Rev. T. G. P. Pope, and Mr. G. Washington were elected fellows.—The following papers were read:—Results of meteorological observations made at Rossinière, Canton Vaud, Switzerland, during 1874 and 1875, by Mr. William Marriott. Rossinière is situated in a valley running north-east and south-west, about three quarters of a mile broad, the mountains on the north being 3,000 to 4,000 feet above the valley of the Sarine, and those on the south, 1,000 to 3,000 feet. The valley is shut in at either end by a gorge, that on the east being about one mile, and that on the west about two miles distant. The observations were all taken by Col. M. F. Ward, F.R.A.S. The mean temperature, as deduced from the mean of the maximum and minimum readings, was 43°·4 for 1874, and 43°·5 for 1875. The monthly means ranged from 20°·0 for December, 1874, and 20°·5 for December, 1875, to 64°·9 for July, 1874. The highest temperature in 1874 was 89° on July 3, and 1875, 85° on August 18; the lowest in 1874 was − 4° on December 24, and in 1875 − 7° on January 1. Owing to the situation of Rossinière, the prevailing winds are those from north-east and south-west. In the winter months the air is for the most part calm, and it is owing to this absence of wind that the intense cold is not so severely felt as it would otherwise be. The total rainfall for 1874 was 54·282 inches, and for 1875, 55·870 inches. The months of greatest rainfall are July and November, and those of the least February and March. Thunderstorms occur frequently from May to August, as many as five being sometimes recorded in one day. The number of thunderstorms observed in 1874 was forty-five, and in 1875, forty-three. No thunder was heard or lightning seen in the months of December to March.—The climate of Fiji, by Mr. R. C. Holmes. This paper contains the results of meteorological observations taken at Delanasau, Bay of Islands, north coast of the province of Bua, Fiji, during the five years ending 1875. The average annual mean temperature is 79°·1. The highest temperature recorded was 97°·7 on January 12, 1871, and the lowest 58°·5 on August 20, 1875, the extreme range in the five years being 39°·2. The average annual rainfall is 124·15 inches, and the number of rainy days 170. The greatest fall in 24 hours was 14·95 inches, which occurred on March 19, 1871. After describing somewhat fully the chief characteristics of the months and seasons, hurricanes and storms, earthquakes, waterspouts, &c., the author concludes with the question, "Is the climate of Fiji a healthy one?" In reply he says that, considered as a tropical country, an affirmative answer may be given without hesitation. Those fatal diseases so common in tropical countries, fevers of various kinds, cholera, and liver complaints, are almost unknown. This is owing partly to the geographical position of the group, lying in the region of the trade winds, so that it enjoys almost perpetual breezes, calms being rare, and the islands so small that the sea-breeze from all directions can penetrate into every corner.—Notes on some remarkable errors in thermometers recorded at Sydney Observatory, 1876, by Mr. H. C. Russell, F.R.A.S. For upwards of five years the same hygrometer has been in use at the Observatory; the dry bulb is small, only 0·3 inches in diameter, and the instrument up to February 26 had always given very satisfactory readings, tested by those of a standard which hangs only 3 inches from it; the difference in the readings was usually 0°·2 to 0°·3. On that day the maximum shade temperature rose to 96°·4 about noon; at 3 P.M. the dry-bulb and standard read 83°·7, and at 9 P.M., 68°·9 and 69°·0. Next morning they read 69°·6 and 69°·8; as this was Sunday, they were not read again until 9 A.M. on the 28th, when the dry bulb read 87°·3, and the standard 64°·9, showing a difference of 22°·4. It was at once thought that the glass had cracked and let in the air, but as no crack could be seen after careful examination, it was determined to continue the readings. The author had always found before that if a thermometer cracks in the bulb, the mercury rises till the tube is full, and he expected it would be so in this case, though he could see no crack. The result, however, was that the difference steadily decreased, at first at the rate of 1° each day, and in thirty-five days the difference had fallen to less than 0°·5, or almost to its normal condition. Between April 7 and 17, it rose again, then fell; on May 3, and again on the 7th, sudden rises took place, since then the difference has been diminishing, except a slight rise on May 21 and 22. When very closely examined with the microscope, a very small piece of coloured glass is to be seen in the bulb, as if lead had been reduced by the blowpipe, and on one side of the bulb a mark is visible, as if there was a minute quantity of water between the mercury and the glass at one spot.

PARIS

Academy of Sciences, November 20.—Vice-Admiral Paris in the chair.—The following papers were read:—Meridian observations of small planets made at the Greenwich and Paris observatories during the third trimestre of 1876, communicated by M. Leverrier.—Tables of the planet Uranus, based on comparison of theory with observations, by M. Leverrier.—On the physical and chemical properties of ruthenium, by MM. H. Sainte-Claire Deville and Debray.—New researches on the chemical phenomena produced by electricity of tension, by M. Berthelot. He studies the relations of the reactions to sign and tension of the electricity.—On the composition of some phosphites, by M. Wurtz. Phosphite of calcium, neutral and acid phosphites of barium.—On the quantity of rainfall at Rome during fifty years, 1825 to 1874, by P. Secchi. The yearly maximum is in the end of October and beginning of November. No relation was perceptible between the quantity of rain and the sun-spots. The great rain-periods at Rome coincided with floods of the lake of Fucino, 100 kilos. distant.—Organisation of a new meteorological observatory on Monte-Cavo; meteorological observations near Rome, by P. Secchi.—On the modifications of elæomargaric acid produced by light and heat, by M. Cloëz.—On the phenomenon of the black drop, by M. André.—On a series of experiments with regard to the flow of water made at the reservoir of Furens, by M. Græff.—On exchange of gases in the tympanic cavity; physiological considerations and therapeutical applications, by M. Lœwenberg. In cases of obstruction of the Eustachian tube, causing deafness, the volume of gas in the tympanic cavity is diminished, and the membrane presses inwards. The author says this diminution must be due, not to absorption, but to exchange by diffusion, and he suggests as remedies—(1) the insufflation of air that has been inspired and expired four or five times; (2) the insufflation of hydrogen.—New observations on the cure of typhoid fever by parasiticide phenicated medication (phenic acid and phenate of ammonia, in draughts, and subcutaneous injection in large doses), by M. Declat.—The theory of systems of land-elevation, *à propos* of the system of Mont Seny, by M. Vezian.—Researches on the structure and the vitality of eggs of phylloxera, by M. Balbiani.—Remarks on M. Bouilland's observations regarding the effects produced by sulpho-carbonates, by M. Mouillefert.—Experiments on treatment of phylloxerised vines by phenic acid and alkaline phenates, by M. Rommier.—On the practical conditions of employment of insecticides to oppose phylloxera, by M. Delachanal.—Results obtained from the use of sulpho-carbonates in the vines of the Puy-de-Dôme, by M. Aubergier.—On the employment of pyrites in treatment of vines affected with oïdium, by M. François.—On the gaseous movement in the radiometer, by M. Salet.—Experiments on the immersed radiometer, by M. de Fonvielle.—Absorbent powers of bodies for heat, by M. Aymonnet. The atomic absorbent power appears to be constant (1) for all simple bodies dissolved in the same medium; (2) for all simple bodies forming part of compounds of like chemical composition.—On a process of determination of alkaline sulphates, by M. Jean.—On the causes of error involved in the application of the law of mixtures of vapours, in the determination of their density, by MM. Troost and Hautefeuille.—On the saccharine matter contained in the petals of flowers, by M. Boussingault. This is considerable, amounting in some eighteen cases examined to an average of 4·88 per cent. in the undried flower.—On a process of testing wines for fuchsine, by M. Fordos.—On the investigation of rosolic acid in presence of fuchsine, by MM. Guyot and Bidaux.—New researches on the action of non-arsenical fuchsine introduced into the stomach and the blood, by MM. Feltz and Ritter.—On the action of iron in anæmia, by M. Hayem. It causes the globules to become charged with a larger amount of colouring matter, and this, not merely in the curable anæmic, but even in cachexia; where, the organism being exhausted, the production of red globules is almost entirely stopped.—Experiments on the pneumogastric and on nerves supposed to be inhibitory, by M. Onimus.—Researches on the urea of the blood, by M. Bernard.—On the power possessed by certain Acarians, with or without a mouth, of living without food during whole phases of their existence, and even their whole life, by M. Megnin.—On crystals of oxydulated iron presenting a singular deformation, by M. Friedel.—On the figures which are formed in superposed liquids, when a movement of rotation is imparted to them, by M. Bouquet de la Grye. These show some analogies to sun-spots. M. Faye added a few comments.—On some peculiarities of lightning, by M. Renon. Beaded lightning; purple or violet flashes; lightning in a near cloud without thunder; sound of thunder heard at 40 kilom. distance.—Observations of falling stars during the nights of November 12, 13, and 14, 1876, at Clermont Ferrand, by M. Gruey.—On the present state of volcanic phenomena of Carvassèra, by M. de Cigalla.

VIENNA

Imperial Academy of Sciences, October 12, 26.—The following among other papers were read:—The nerves of hair-covered skin, by M. Arnstein.—The germination of plant spores in its relation to light, by M. Leitgeb.—On spontaneous variations of blood-pressure, by M. Mayer.—On the action of chloroform and ether on breathing and circulation, by M. Knoll.—On some remarkable phenomena in Geissler tubes, by MM. Reitlinger and Urbanitzky.

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