THURSDAY, NOVEMBER 23, 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.)

I.

THIS is in many respects an important work. Full of experimental facts and theoretical suggestions, clearly and forcibly written, it is important as a contribution to our knowledge (and our ignorance) of the functions of the brain. The reader must not misunderstand my parenthesis as an epigram. That we are ignorant of brain-function is undoubted ; and this ignorance is sustained and fortified by the "false persuasion of knowledge" which prevents search in other directions. Such false persuasion of knowledge will be deepened by Dr. Ferrier's work—all the more because of its merits, *if* the conclusions maintained there are erroneous, and the conceptions which determine them are unphysiological ; and on both points I am inclined to judge affirmatively. There is something seductive in the precision of his statements and the unhesitating confidence with which only one side of a question is presented. The reader is easily led captive by a writer who has no hesitation. Add to this the many difficulties which stand in the way of controlling by experiment the experimental data, and the indisposition of most men to undertake the labour of verification, and we may foresee that physicians and psychologists will eagerly accept this work as an authoritative storehouse of material for their speculations. They will see how its "facts" harmonise with their own pet errors. They will interpret clinical observations or psychological facts by its conclusions. Already we have seen various theories invoking the Hitzig-Ferrier views ; and when nerve-cells of a larger size than usual are found in a particular region of the cortex they are straightway declared to be motor-cells, because the region is said by Hitzig and Ferrier to be motor, while the existence of these cells is adduced in confirmation of the hypothesis respecting the region !

In view of the too-probable precipitation in adopting the conclusions of this work, we cannot do better than emphasize the warning with which the author closes his Introduction :—

"We are still only on the threshold of the inquiry, and it may be questioned whether the time has even yet arrived for an attempt to explain the mechanism of the brain and its functions. To thoughtful minds the time may seem as far off as ever."

The volume opens with an elementary sketch of the structure of the brain and cord, followed by a short chapter on the reflex functions of the cord, with passing reference to Pflüger's view of its sensory functions, and to Goltz's experiments against that view. Then follows a chapter on the medulla oblongata as a respiratory and vaso-motor centre ; and one on the general relations of the mesencephalon and the cerebellum. After full, yet brief accounts of what is taught respecting the effects of removing the cerebrum, the mechanism of equilibration, the muscular sense, the function of the semicircular canals, vertigo, co-ordination of locomotion, and finally the mechanism of emotional expression, we are brought to the main topic of the book—the functions of the cerebrum and basal ganglia. Let a word, in passing, be also given to the excellent chapter in which the psychological aspect of the cerebrum is treated.

Rich as the work is in facts and suggestions, it is so deficient in the indispensable correctives of counterfacts and arguments, that the reader must be cautioned against accepting any position unless elsewhere verified. Partly because, from long occupation with his subject, Dr. Ferrier has become unable to see it in any other light than that of his own hypothesis, and therefore doffs aside all counter-facts and counter-arguments as not really significant ; partly, perhaps, because his memory has let slip what must have entered into his knowledge ; from one cause or another there is a disregard of counter-evidence, which, in a second edition, I should seriously urge him to rectify. Let me cite examples.

In arguing against the sensory functions of the spinal cord, the experiment which he urges as decisive is Goltz's well-known experiment on the insensibility of the brainless frog to pain. I formerly (NATURE, vol. ix. p. 84 pointed out the defect in logic, which concludes from the fact that under certain conditions a brainless animal is insensible to pain (equally to be said of animals with brains), therefore it is altogether without sensibility. Pain and sensation are so far from being equivalent terms that not only are the great mass of our sensations without pain, but some cannot even be exaggerated into pain. Dr. Ferrier probably did not read the article in which I answered Goltz ; but did he also overlook the article in the *Journal of Anatomy* for November, 1873, or the same article in the *Studies in the Physiological Laboratory of Cambridge*, Part 1, where Prof. Michael Foster showed by decisive experiments that the facts observed by Goltz had another interpretation ? Again, is it possible that Dr. Ferrier has never been made to hesitate in assigning the optic thalami and corpora striata respectively as the integration of sensory and motor centres, by the observations and experiments which show that sensibility sometimes persists after total destruction of the optic thalami, and that paralysis does not always follow destruction of the corpora striata ? One such observation would be decisive against these localisations. But Dr. Ferrier neither disproves the facts nor suffers them to disturb his views of the functions of these ganglia. Finally, there is an experiment by Dr. Burdon Sanderson which, as I shall presently show, cuts the very ground from under Dr. Ferrier's feet—yet this he does not even mention. He probably overlooked its significance ; at any rate he leaves his readers without the advantage of knowing that there is such a fact.

That this disregard arises from no unfairness, but simply from the onesidedness which comes from preoccupation with certain views, is evident in the way he equally disregards his own counter-evidence. A notable instance is the first assigning the occipital lobes as centres of organic sensations on the faith of observed absence of such sensations when the lobes were removed, and then citing a case of complete recovery of such sensation five days afterwards, and instead of recognising this as decisive against his hypothesis, still persisting in maintaining it.

Thus much on what may be called the "personal equation." Another and more serious source of the misleading effects of the book seems to me its following the

increasingly popular but thoroughly unphysiological conception of Localisation. Were not the current notions respecting organ and function very chaotic, and were not the indispensable artifice of analysis mistaken for more than an artifice which demanded rectification by synthesis, we should marvel to witness so many eminent investigators cheering each other on in the wild-goose chase of a function localised in a cerebral convolution. I will not, however, dwell on this point here, because it is one which would require a long discussion. It is only mentioned as a general *caveat*, and as leading up to the main question of cerebral excitation.

In 1870 Hitzig and Fritsch startled the scientific world by announcing that the universally accredited notion of the brain not being excitable was an error. The most eminent experimenters had declared that mechanical, chemical, and electrical stimuli were utterly powerless to excite the grey matter; and many a writer pointed to the paradox of the chief organ of sensation being *insensible*. We may here note another example of the common confusion of sensibility with pain; the brain was said to be "insensible" because no cutting, burning, pricking, or galvanising of it yielded evidence of pain; whether other evidence of sensibility might have been present was not asked. The utmost the experiments could prove was that the brain was not excitable by these abnormal means, though excitable by the very different normal means of peripheral stimulus. And even this conclusion Hitzig and Fritsch upset, by demonstrating that there were certain regions of the cortical substance which were excitable by electricity, as proved by the movements following such excitation; and the other "non-excitable regions" they inferred to be also excitable, though in another way, namely, by the production of sensations (*Vorstellungen*).

This was an epoch-making discovery. Experimenters in Germany, Italy, England, Switzerland, France, and America, quickly verified it, although differing among each other both as to the particular facts, and their interpretation. Among these followers the chief place must be assigned to Dr. Ferrier, both for the extent and the precision of his results; accordingly the names of Hitzig and Ferrier are usually coupled in speaking of the new hypothesis that various motor centres are located in particular spots of the cerebral cortex.

Although I have called it an epoch-making discovery, because I believe it will open a new track for the anatomical and physiological interpretation of the nervous mechanism, which will one day enable us to follow the whole pathway of stimulation, instead of—as at present—leaving us with the vague conception that "somehow" the cerebrum determines movements by setting the motor apparatus in action, I do not think that the hypothesis of motor centres in the cerebrum is tenable; nay, more, I *do not think that Hitzig and Ferrier have proved the grey substance to be excitable*. It is one thing to admit that the brain is excitable, another to admit that the excitation so effected is effected by calling into activity the special property of the grey substance. We do not consider the fauces to be the centre of vomiting, although tickling the fauces will be followed by retching. We do not consider the centre of laughter to be located in the sole of the foot, because tickling the sole causes laughter. Something more is needed; and it is precisely this something more which the Hitzig-Ferrier hypothesis has yet to find, namely, the anatomical connection of the so-called centre with the motor apparatus.

Has any proof been adduced that the electrical stimulus *first* acts on the cortex, and *then*—by the stimulation there produced—on the white substance, which in turn acts on the motor ganglia? *None that withstands criticism.* Knowing as we do that if the cortex be removed, or destroyed, the electrical stimulus nevertheless on reaching the white substance determines the same movements which had previously been determined when the stimulus was applied to the cortex, we may fairly ask: What proof is there that the current does not pass *through* the cortex (as through any other conducting medium) without exciting its activity? That it does simply pass through the cortex is probable on two grounds: (1) only the electrical current causes an excitation; mechanical and chemical stimuli have no such effects, because they cannot pass through the cortex to reach the white substance; (2) it is a well-known law that the propagation of neurility, *unlike* that of electricity, takes place only at insensible distances: if the nerve be divided, and the two cut surfaces be brought into the closest possible contact, there is still no propagation of the excitation from one surface to the other; whereas electricity passes freely *across* the cut surfaces. Now here Dr. Burdon Sanderson's decisive experiment, formerly referred to, comes, as I said, to cut the very ground from under the Hitzig-Ferrier hypothesis. "If that part of the surface of the hemisphere which comprises the active spots is severed from the deeper parts by a nearly horizontal incision made with a thin-bladed knife, and the instrument is at once withdrawn without dislocation of the severed part, and the excitation of the active spots thereupon repeated, *the result is the same as when the surface of the uninjured organ is acted upon*" (*Proceedings* of the Royal Society, No. 153). Here the interruption caused by the incision, while it must have completely prevented the propagation of *neural* excitation, did not prevent the propagation of the *electrical* current. Clearly therefore the simple passage *through* the cortex will explain all the effects of electrical stimulation. Clearly therefore some other proof is needed before we can assign the motor effects to an excitation of the cortex. The arguments of Dr. Ferrier (pp. 135–6) are all set at nought by Dr. Sanderson's experiment; and on the physiological and histological views now adopted I do not see how Dr. Sanderson's experiment can be brought into agreement with the motor centre hypothesis.

Nevertheless, although I say that the preliminary fact of excitation of the cortex is not proved by Hitzig and Ferrier, I do not myself doubt that fact, although my reasons will sound so paradoxical that I must wait for another article to give them expression.

GEORGE HENRY LEWES

(*To be continued.*)

GREEK AND LATIN PHILOLOGY

Baur's Philological Introduction to Greek and Latin for Students. Translated from the German by C. Kegan Paul and E. D. Stone. (London: King and Co., 1876.)

WITH the publication of Jacob Grimm's "German Grammar" the comparative study of language entered upon a new period of existence. Bopp and the

other great founders of Comparative Philology had been too busily engaged in laying the foundations of the science, in determining its main laws and principles, and in classifying whole groups or families of speech, to devote themselves to the minute and special investigation of single languages, and trace therein the application and action of the laws they had formulated. But a time came when the work of the pioneer was finished, and when it was necessary for special scholars to elaborate the details of the new science and to strengthen or modify its conclusions by a patient examination of individual dialects. The old-fashioned "philology" which had professed to analyse the forms of a language, as preserved in its literature, had proceeded upon a wrong method and had accordingly arrived at wrong results; its area of comparison was too narrow and limited, its procedure was capricious and at haphazard, and its doctrines were based rather upon individual taste than upon inductive reasoning. When it was discovered, however, that language is as much subject to the action of invariable laws as the bodily frame of man, that every sound in the words we utter is due to conditions which can be accurately gauged and determined, the "philology" of the last century underwent a complete change. It stands to the modern science of language in much the same relation as alchemy stands to chemistry. The general laws of language which had been obtained by a careful and far-reaching comparison of phenomena, were applied to explain and illuminate the facts presented by special languages, and these in their turn served to confirm or modify the generalisations already made.

Latin and Greek were naturally among the first to benefit by the new method of treatment. Thanks to the labours of scholars like Curtius and Corssen, the languages of ancient Greece and Rome have been placed in their true position, and probed, as it were, to their very roots. Their grammatical forms have been explained and simplified, their words have been traced back to an epoch when they were the common heritage of the Aryan race, and their phonetic characteristics have been made to yield fresh testimony to the truth that the place and nature of every consonant and vowel is the result of the working of undeviating laws. What, perhaps, is of still greater importance, is the line that has been drawn between the literary and the linguistic value of the two classical tongues. For purely philological purposes they are of less interest than many a savage jargon, the name of which is almost unknown, and certainly than those spoken languages of modern Europe whose life and growth can be watched like that of the living organism, and whose phonology can be studied at first hand. The more or less artificial dialect of a literary class stands outside the ever-moving current of living speech; in proportion as it is impressed with the individualism of particular writers, it becomes unsuitable for scientific treatment. The greater the literary perfection of a language, the less is its importance to the mere glottologist. The value of Latin and Greek, and more especially of Latin, lies rather in the literature they enshrine than in the linguistic features they present.

No language, however, can be wholly valueless or uninteresting to the student of human speech, and Latin and Greek, from the minuteness with which they have been studied, and the number and variety of monuments they have left behind them, have a special claim upon his attention when revivified and illuminated by scientific philology. The laws which have been ascertained by the observation of living utterance have been applied to explain the letter-changes of the classical tongues, and the comparison of their grammatical forms with those of the cognate languages has done much towards throwing light on the history of Aryan flexion and the vicissitudes through which it has passed. The innermost structure of the dead languages of Greece and Rome has been laid bare, and though there is much which will to the last resist analysis, the old mystery which enveloped the paradigms and "rules" of our school grammars has been dispelled for ever. Dr. Baur's attempt to convey the results of a scientific investigation of Greek and Latin in the shortest possible form is highly successful, and those who are unable to read German ought to be grateful for the translation of the work. The book is essentially a useful one, and we hope it will be extensively read in our schools and universities. As the author confines himself very strictly to the two classical languages, the teacher need have no fear of the pupil's mind being confused by a reference to less-known tongues. Indeed the book suffers from a neglect of Sanskrit, with which Dr. Baur does not seem to be acquainted. He has compiled the work, however, with German care and thoroughness, though, as is inevitable in a work of the kind, exception might be taken to some of his statements.

Thus no distinction is made between primitive Aryan *kw* and *k*, and the two sounds are accordingly confounded together in Greek and Latin words. *Quies*, for instance, can have no connection with the Greek κεῖμαι, κωμή, the English *home*, the Sanskrit *'si*, but must go back to a different root. So again it is very questionable whether the characteristic *r* of the Latin passive is really the reflexive pronoun *se*. In Old Bulgarian, it is true, we have *divlya se, diviši se*, "I admire myself," "thou admirest thyself," and in Lithuanian *dỹvyjů-s* "I admire myself;" but the Old Irish characteristic of the passive is also *r*, and in Old Irish *r* cannot be derived from an earlier *s*.

A. H. SAYCE

OUR BOOK SHELF

A Monograph of the Geometrid Moths or Phalænidæ of the United States (Tenth Report of the United States Geological Survey of the Territories). By A. S. Packard, Jun., M.D. (Washington, 1876.)

THIS is without question the most valuable contribution to the study of the Geometrid Moths which has ever come under our notice. The size of the work, the paper, and the classification of the introductory chapters are all that can be desired; as for the plates they are simply perfect, no pains having been spared to render them accurate even in the most minute details.[1]

After the Introduction, a chapter is devoted to the History of the Family from the time of Linnæus, a work demanding no little research, and which consequently must claim for the author the gratitude of all succeeding generations of lepidopterists: the only point in which we disagree with Dr. Packard is as regards the prominence which he gives to the "Tentamen" of Hübner, the value

[1] To the eye of an amateur the plates would appear overcrowded, but to the working entomologist this must be one of their greatest merits.

of which document will always be a disputed point with entomologists, inasmuch as (although twice quoted by its author in his subsequent works) it is more than doubtful whether it ever was actually published.

The succeeding chapters are devoted to the differential characters of the family, to structural details, habits, development, secondary sexual characters, origin of the genera and species, and mimicry; then follow concise descriptions of the genera and species, with comparative and other valuable notes, descriptions of preparatory stages, &c.

It is a subject for congratulation, the importance of which none but the working lepidopterist can fully appreciate, that Dr. Packard has devoted six of the plates to the delineation of wing structure; most of the *generic* errors in Mr. Walker's lists must be attributed to his entire neglect of the characters offered by neuration; attention to this is sometimes the only means by which species, otherwise wholly similar, can be distinguished. The structure of the thorax, although of much importance, can rarely be attended to, as the destruction of the specimens is necessary before it can be detected; but in the examination of the wing-veins nothing is needed but a bottle of benzine, a brush, and a pocket lens, to reveal all that is required *without injury* to the insect.

In conclusion we heartily congratulate Dr. Packard on having produced a work in every respect worthy of himself and the Academy of which he is an officer.

A. G. B.

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.*]

Prof. Balfour Stewart on Meteorological Research

IT occurs to me to make the following remarks with reference to Prof. Balfour Stewart's proposal in NATURE, vol. xiv. p. 388.

I cannot see any objection either to the nomination of the council which is suggested, or to its constitution, provided each existing society is duly represented by a member who can, when circumstances seem to require it, attend and vote at any meeting of the Council in London. I quite agree with the Professor in thinking that the time has now come when our country should resolutely grapple with the data which have accumulated in past years and with those that are now being obtained. It is only by a thorough discussion of meteorological data that the importance of certain principles can be detected, and the necessity for altering the modes of observing can be demonstrated.

I do not see that the appointment of the proposed council should interfere prejudicially with the working of the different societies. While it is the duty of such societies to procure the facts, it seems to me essential in order to secure uniformity in instrumental observation, without which all deductions or generalisations from the data may be worse than useless, that a council of control should be appointed in order to lay down rules for regulating all observers. I would not give an arbitrary power to that council to compel every society to adopt their views, because I have a great aversion to centralisation in matters of science, for in some cases the branches may be more in the right than the head; but in the event of a society declining to comply with the rules issued by the council, that society should not receive Government aid excepting for work that is done in terms of the rules. I take it for granted, however, that the council would give due weight to the arguments which were adduced from time to time by the representatives of the different societies. I am further of opinion that the different bodies should not only be allowed but encouraged by Government aid to prosecute independently in their own way, any special subject which they may choose to take up.

I hold so very strongly the absolute necessity of uniformity in instrumental observation, that I should be disposed to recommend each society to adopt almost any change in the forms of instruments, in the kind of exposure, in the hours of observation, in the form of protecting boxes, or in any other matter which might be recommended by the proposed council, provided such changes were practicable, and were agreed to once for all by the other societies in this country, and by foreign nations.

I think it right to add that I am only stating my own individual convictions, and do not in any way profess to represent the opinions of the Council of the Society of which I am the honorary secretary, although I have no reason to suppose that they would take a different view. THOMAS STEVENSON

Edinburgh, November 18

Ocean Currents

IN the report published in NATURE (vol. xiv., p. 492) of an address given at the Glasgow meeting of the British Association, September 11, by Sir C. Wyville Thomson, and revised by the author, the following passage occurs:—

"We have come to the conclusion that this great mass of water is moving from the Southern Sea, and there seems to me to be very little doubt—although this matter will be required to be gone into carefully—that the reason why this water is moving from the Southern Sea in a body in this way, is that there is a greater amount of evaporation in the North Atlantic and over the northern hemisphere generally, than there is of precipitation, whereas it seems almost obvious that in the southern hemisphere in the huge band of barometric low pressure round the south pole, the precipitation is in excess of the evaporation."

Now I quite feel that I am guilty of very great presumption in challenging in any way the theories of so great an authority as Sir C. W. Thomson, and my only excuse for the remarks I am about to make is that there are some points that I and many other seamen would like to have cleared up before we entertain such an hypothesis.

1. Have the investigations of the *Challenger* sufficiently proved that there is no compensating or return current from the North Atlantic to the South Atlantic Ocean? Especially, is it quite certain that a stream of water from the Arctic regions does not set southerly along the West Coast of Africa, *i.e.*, south of the equator?

2. Allowing that the precipitation in the Antarctic regions is greatly in excess of that in the Arctic regions, is the precipitation in the north torrid and north temperate zones less than the precipitation in the south torrid and south temperate zones?

3. Looking to the much larger distribution of land in the northern hemisphere, is it likely that the evaporation there is in excess of the evaporation in the southern hemisphere?

4. Even supposing the evaporation in the northern hemisphere to be in excess of that in the southern hemisphere, can it be shown that this vapour is carried to the Antarctic regions for condensation, or can the excess of precipitation in the Antarctic regions be accounted for in a more probable manner?

In answer to the first question I can only say that I am not able to gather from the reports of the ocean soundings and temperatures of H.M.S. *Challenger*, published by the Admiralty,[1] that it has been at all proved that there is no compensating stream of Arctic or other water.

In answer to the second question, I have never heard it disputed, and my experience as a seaman leads me to doubt the possibility of reasonably disputing, that the rainfall in the north temperate and north torrid zones is not only not less, but that it is far in excess of the rainfall in the south torrid and south temperate zones. Maury (and no matter to what extent we may differ from his theories, we must give due weight to his data) says that the total amount of rain in the north temperate zone is half as much again as in the south temperate zone.

With reference to the third question, whether the evaporation in the northern hemisphere is in excess of that in the southern hemisphere, I think the onus of proof rests with those who start the theory, but in my present state of ignorance on this subject I must confess that it is to my mind quite inconceivable. There are, with few exceptions, no large rivers in the southern hemisphere, and surely the discharge into the sea of the large rivers in the northern hemisphere must be regarded as the return to the ocean of the excess of precipitation over evaporation in the regions which they drain.

There remains the fourth question, and before trying to answer this I should like briefly to state what I think is the general or accepted belief up to the present time with reference to atmospheric currents or circulation. The trade winds are supposed to be currents from the poles which, starting from the Polar

[1] Plate VI. Report No. 7 would appear to indicate that Arctic water does cross the equator.

regions as upper currents, descend to the surface of the globe on the equatorial side of 30° of lat. in both hemispheres, they then travel onwards towards the belt of equatorial calms, when they meet and ascend into the upper regions of the atmosphere, whence they travel back towards the poles as upper currents, until they arrive at the calm belts of Cancer and of Capricorn, on the polar sides of which they once more descend to the surface, and are then known as the westerly winds of the temperate zones. Owing to the rotatory motion of the earth, it is impossible for these westerly winds to blow direct towards the poles, but it is clear that if you surround the Polar regions with a belt of westerly winds, that no matter what the direction of the wind may be in the Polar regions, it must, if a surface wind, be supplied by this zone; and that the winds experienced in the Polar regions are winds travelling on the surface, and are drawn from this belt of warm winds, is, I think, proved by the following extract from an account of the wintering of the *Hecla*, Capt. (Sir Edward) Parry, at Melville Island, in the year 1819–20 :—

"A gale of wind, from whatever quarter it might blow, was almost invariably found to raise the thermometer several degrees, even when it came from the north, as much as 14°. An east, south-east, or east-south-east wind causes the thermometer to rise 40°."

From this extract it is evident, as might be supposed, that any current of air from this zone or belt of warm westerly winds raised the thermometer considerably, but that the wind that proceeded the more directly from the ocean and had the least land to traverse was the warmest.

Maury, without attempting to prove his case, and indeed throwing the onus of proof on those who ventured to disagree with him, considers that the south-east trade winds of the southern hemisphere become the south-west winds of the north temperate zone, and *vice versâ;* that the north-east trades of the northern hemisphere become the north-west winds of the south temperate zone. I do not say that this is not the case, but if you admit that the north-east and south-east trade winds meet in the belt of equatorial calms and there ascend, it appears to be more reasonable to suppose that their currents intermingle and that their mixed volume is then drawn off north and south as required to restore the equilibrium of the atmosphere. And there is a very strong argument against Maury's hypothesis, viz., that as the south-east trades of the southern hemisphere are stronger and extend over a greater surface than the north-east trades of the northern hemisphere, and as also the north-west winds of the southern hemisphere are stronger and more continuous than the south-west winds of the northern hemisphere, it is illogical to suppose that the stronger polar current, *i.e.*, the south-east trade, feeds the weaker equatorial current, *i.e.*, the south-west winds of the north temperate zone; it would be more reasonable to suppose the reverse to be the case.

This entire theory of atmospheric currents is antagonistic to the presumption that a larger body of vapour is carried from the northern to the southern hemisphere.

Owing to the scarcity of land, and especially of very high land in the south temperate zone, not only is the precipitation less, but the vapour-carrying winds, *i.e.*, the westerly winds, are far more constant in their direction and force than are the westerly winds of the northern hemisphere. (The proportion of westerly winds to any others in the temperate zone in the North Atlantic is two to one, while throughout the south temperate zone they are so constant as to have been christened by Maury the north-west trades.)

If you once admit that these westerly winds are equatorial currents flowing towards the poles (a fact susceptible of undoubted proof), it is easy enough to account for the low barometer in the Antarctic regions, as also the larger amount of precipitation there as compared with the precipitation in the Arctic regions.

1. Because the westerly winds being much stronger and more continuous in the southern than in the northern hemisphere, the ascension of the air in the South Polar regions must be greater than in the North Polar regions.

2. Because, owing to the westerly winds of the south temperate zone parting with less of their moisture (as previously accounted for) than the corresponding winds of the north temperate zone, and also to their being stronger and more continuous, it is evident that when they meet with Antarctic cold and their vapour is condensed, the precipitation must be greater, which also involves the giving out of a much larger amount of latent heat and the consequent greater expansion and ascension of the atmosphere in the South Polar regions.

It is, I believe, universally acknowledged that all winds must blow from a high to a low barometer, *i.e.*, from a zone of high pressure to a zone of low pressure (not directly, but in a direction modified by the earth's rotatory motion). I may therefore fairly argue that the zones of low pressure at both the equator and the poles proceed from the same causes, *i.e.*, from precipitation, and from the ascension of the atmosphere, and that the lower barometer in the South Polar regions fairly accounts for the greater strength and continuity of the westerly winds of the south temperate zone, and that without these constant inequalities of pressure we should have neither trades nor westerly winds.

I have purposely from want of space avoided speaking otherwise than generally of the effect of the land on atmospheric currents, nor is it directly pertinent to my present argument.

The hypothesis of atmospheric circulation which I have very briefly sketched is in many of its features susceptible of absolute proof, more especially in the following points, viz. —

1. That the trade-winds descend to the surface of the ocean on the equatorial sides of the calms of Cancer and of Capricorn.

2. That the trade-winds ascend in the belt of equatorial calms.

3. That currents flow from the equator in the upper regions of the atmosphere in an opposite direction to that of the trade-winds on the surface of the ocean.

4. That these upper currents, flowing from the equator, descend again to the surface of the ocean on the polar sides of the calms of Cancer and of Capricorn.

5. That these equatorial currents, subsequent to their descent on the polar sides of the calms of Cancer and of Capricorn, are known as the westerly winds of the temperate zones.

And with reference to my supposition that these westerly winds ascend in the Polar regions, one strong evidence in favour of this is, that if, as I say, the ascension of the atmosphere is greater in the South Polar than in the North Polar regions, the counter or return current towards the equator must also be greater, which is the fact.

The onus lies on the promoters of the new hypothesis either to reconcile their views with the existing theory of atmospheric circulation or to supply us with a better theory, and one which shall agree equally well with well-established facts.

October 27 DIGBY MURRAY

Definiteness and Accuracy

IN my lecture on *Force* (*anté*, Sept. 21), I take for granted that the scientific use of the word is that with which all are familiar in the expression "*the parallelogram of forces.*" Hence Newton's term for *force* is *vis impressa* (Thomson and Tait's *Nat. Phil.*, § 217); though, where there is no room for mistake, he often employs the single word *vis.*

One of the main objects of my lecture was to protest against the absurd custom of translating the word *vis* in every case by the scientific word *force.* It is not easy to get an unobjectionable single word for the purpose, for most of the available words have already a semi-scientific sense attached to them. The word *power* is very flexible in its meaning, and would have been suitable had it not been already seized by the engineers. Thus (Thomson and Tait, § 216) *vis insita* is rendered *innate power.* And, giving the word as wide an application as Newton gives to *vis*, we might render *vis viva* as *active power*, which is not far from *actual* or *kinetic energy.* But this is merely a suggestion.

In *Poggendorff's Annalen* (No. 7 of this year) Prof. Zöllner translates the scientific term, "*the perpetual motion*," by "*die beharrliche Bewegung*," and thus, to his own satisfaction at least, proves me to be ignorant alike of the proper meaning of the Latin *perpetuum mobile* and of the first law of motion!

In another journal I have lately been held up to scorn, not in the main for any real or imputed fault of mine, but because my would-be critic (Mr. R. A. Proctor) happens not to know the scientific meaning of "*absolute*" measure!!

I could give many more telling instances, great and small, but I have given enough to show how needful was my contention for definiteness and accuracy. P. G. TAIT

College, Edinburgh, November 11

On the Internal Fluidity of the Earth

THE question of the solidification of the crust of the earth from the fluid interior nucleus, as referred to by Mr. Mathieu Williams, in NATURE, vol. xv. p. 5, is one which has been long since fully discussed in my papers in the *Philosophical Transactions*, the *Atlantis*, vol. i., and in a paper of which an abstract appears in the Report of the British Association for 1856. As far as I am aware, no person has controverted my conclusions as to the process of solidification of the earth. The results are, in the main, somewhat similar to those so admirably illustrated and enforced by Mr. Robert Mallet, and also such as Mr. Williams upholds in this journal.

In articles 6 and 7 of my "Researches in Terrestrial Physics," Part I, this subject is discussed as a problem of fluid equilibrium, and the conclusion is there deduced that the fluid interior mass of the earth must consist of spheroidal strata of equal density, the density of each stratum increasing from the surface to the centre of the nucleus. The mode in which this arrangement of the fluid matter would favour the formation of a solid crust is pointed out. In Part 2 Section III., the probable law of density of these fluid strata is discussed. In Section IV. the shape of these strata is investigated, and also that of the inner surface of the shell or crust. It is shown independently of the law of density that the least ellipticity of this inner surface of the crust cannot be less than the ellipticity of its outer surface. A similar result was soon afterwards enunciated by Plana in a paper in the *Astronomische Nachrichten*. In the same section the theory of a solid nucleus in the earth originally proposed by Poisson, is examined and shown to be incompatible with physical laws.

Owing to the pointed manner in which Sir William Thomson invited discussion in a previous number of NATURE, I ventured to controvert his views as to the rigidity of the earth in a paper inserted at p. 288, vol. v. of this journal. Never at any time have I had even a doubt as to the untenable character of Sir William Thomson's views regarding the solidity of the earth.

In again reiterating this opinion in NATURE, vol. ix. p. 103, a reference to my paper was given, in which vol. vii. p. 288, is misprinted for vol. v. p. 288.

In his address at Glasgow Sir William Thomson, while maintaining his opinion as to the earth's solidity, appears to have seen the weakness of some of his former arguments by calling on his hearers (NATURE, vol. xiv. p. 428) to erase whole paragraphs of his paper on the Rigidity of the Earth, in the *Philosophical Transactions*. At the same passage of his address he refers to a hint from Prof. Newcomb, that viscosity might suffice to render precession and nutation, the same as if the earth were rigid. "This," he says, "I would not for a moment admit, any more than when it was *first* put forward by Delaunay." The *Comptes Rendus* of the Academy of Sciences of Paris for March 6, 1871, contains a paper in which my priority on this point is clearly established. In NATURE, vol. iii. p. 420, the following statement occurs:—"Paris Academy of Sciences, March 13.—M. Delaunay read a declaration stating that he acknowledged that Mr. Hennessy had used the same arguments as himself against Mr. Hopkins' theory relative to the fluidity of the interior parts of the earth."

I am willing to believe that Sir William Thomson had neither seen the *Comptes Rendus* nor the paragraph in NATURE just quoted, but it is to be regretted that a presidential address should contain an erroneous statement on a point of recent scientific history, especially when the error could be avoided by a glance at the most widely known scientific publications.

All through the portion of his address which refers to the earth's structure Sir William Thomson assumes that the views of Mr. Hopkins are established and admitted. A reference to some of the past volumes of this journal alone shows the inadmissibility of such an assumption. At pp. 45 and 182 of vol. iv. and elsewhere Mr. Hopkins' views are distinctly controverted on mechanical, physical, and geological grounds.

It appears that in the discussion on my paper in the Academy of Sciences of Paris, in which some of the most eminent mathematicians and geologists of France took part, not one of them adopted Mr. Hopkins' "Discovery of the earth's solidity." As far as I am aware, this "discovery" is not adopted anywhere on the continent of Europe. I have studied with as much care and attention as I could give to them, the mathematical and physical researches of Mr. Hopkins and Sir William Thomson relative to this subject, and for reasons already partly unfolded in this journal at vol. v. p. 288 and vol. iv. p. 182, I continue to firmly adhere to the almost diametrically opposite conclusions long since enunciated in the publications referred to at the outset of this communication.

HENRY HENNESSY

Royal College of Science, Dublin

The Age of the Rocks of Charnwood Forest

I SEE that in Mr. Woodward's "Geology of England and Wales," recently published, the rocks of Charnwood Forest, in Leicestershire, are (with some hesitation) referred to the "Laurentian" series, Prof. Ansted and Dr. Holl being quoted as authorities. The reviewer in the recent number of the *Saturday Review* adopts the same opinion; at the same time it is proper to add that Mr. Woodward states in another place (p. 31) that the Charnwood Forest rocks "may be of Cambrian age," so that the reader is left to take his choice.

For my part I confess to being at a loss to understand on what grounds these old rocks can be referred to any other than the Cambrian period. The evidence in any case is small, but what there is points to this conclusion.

In the first place it ought to be remembered that the age of these rocks was first indicated by Prof. Sedgwick, whose opinion on such a question should not be disregarded unless on very substantial grounds. Sedgwick's opinion of their age was founded almost entirely on lithological grounds, and no one was better qualified to recognise the representatives of the Welsh Cambrians, though rising up isolated amongst much newer formations in the heart of England. Prof. Jukes, in his description of the geology of Charnwood Forest, appended to Porter's "History," adopted the same view, in which the officers of the Geological Survey, including the present Director-General, who made a personal examination of the forest rocks, concurred (see "Geology of the Leicestershire Coal Field," Mem. Geol. Survey, 1860). I am not aware that they have changed their views owing to what has been since written on the subject.

As regards the determination of the age of these rocks, if it is impossible to prove them to be of Cambrian age, there are very good grounds for concluding they are not of "Laurentian" age, assuming that term to refer to the fundamental gneiss of the north-west Highlands and Isles of Scotland. These rocks consist, according to the description of Murchison, of coarsely crystalline gneiss, full of granite veins. They are everywhere intensely metamorphosed. Now, this is far from being the case with the Charnwood Forest rocks. Generally they are no more metamorphosed than are the Cambrian beds of the Longmynd, or of Llanberis. True "gneiss" is very exceptional, and metamorphic action is quite local, and is chiefly confined to one district. Any argument, therefore, drawn from lithological resemblance to the rocks of the typical district entirely fails; and I cannot admit that the occurrence of rocks (syenite, &c.) resembling those of the Malvern Hills, is of any force in this question, as it is very far from having been proved that the Malvern Rocks are of Lanrentian age.

As regards evidence founded on organic remains, it is of the most meagre kind, but whatever the obscure markings on the slates of Charnwood may really be, they are certainly not those of *eozoon*. From whatever side, therefore, the question is viewed, there appears to be no good ground for departing from the view regarding the age of these rocks originally adopted by Sedgwick.

EDWARD HULL

Geological Survey Office, Dublin, November 3

Mind and Matter

MR. SPALDING in his critique on Maudsley's "Physiology of Mind" (NATURE, vol. xiv. p. 541), while admitting that "the dependence of consciousness on nervous organisation, seemed," by the science of nerve physiology, "to be fairly established," stated that the difficulty of conceiving *how* consciousness stood related to the material organism, was a difficulty which had not yet been overcome.

Might not this problem be solved somewhat thus:—It is as easy to predicate subjectivity (or susceptibility to consciousness) of one entity called matter, as of another entity called soul or spirit. It is no more difficult to conceive of matter being subjective than of spirit being subjective.

Again, energy accompanies matter in all its forms, and yet

how it is related to the entity called matter, is no less mysterious than how subjectivity may be a property of matter. Energy moreover may be divided, Why may not subjectivity? Energy exists in isolated as well as in grouped and combined forms, Why may it not be so with subjectivity? Energy exists in a potential form when opposing forces neutralise each other—that condition of matter we call rest. May not subjectivity exist in a potential form when opposite kinds of subjective states tend to establish themselves in the same material mass—so constituting that condition of matter which we call unconsciousness? Energy is only kinetic or active, that is, only shows itself in the form best realised by us when one force has of all the others the ascendency, or is the expression of their united tendencies. May it not be so of subjectivity that it only develops into its active and best recognised form when one kind of subjective state has the ascendency, or is the expression of the united subjective states? Thus, as energy potential is rest, so subjectivity potential is unconsciousness. As kinetic energy is motion, so active subjectivity is consciousness.

In this way while all matter is subjective or susceptible of consciousness, this subjectivity seems to exist in the potential form only in all but organisms possessed of a nervous system. In the active nerve fibre the subjectivity of matter appears alone to be active or conscious, while the complex organisation of the nervous systems of the higher animals alone permits of matter rising to the powers of mind by harmoniously combining many subjective states so as to build them up into perception, understanding, memory, imagination, reason, invention, and judgment.

WM. S. DUNCAN

Stafford, November 9

Meteor

I OBSERVED from a high point overlooking the Weald, on the night of November 6, about the time Mr. Nostro mentions, a large meteor fall from a point a little below the zenith in the northern sky. It burst twice, emitting bluish sparks in doing so, once shortly before it disappeared, and the second time on its disappearing. Could it have been the same meteor seen from different positions?

I could not be positive to a point or two as to its exact magnetic bearing, but I do not think I am far wrong in saying it fell almost due north from where I observed it.

CECIL H. SP. PERCEVAL

Pulborough, November 18

THE PRESENT STATE OF MATHEMATICAL SCIENCE

AT the meeting of the Mathematical Society on November 9, Prof. H. J. S. Smith gave an address on this subject, in which he excluded all reference to applied mathematics. "I shall regard it," he said, "as a fortunate circumstance if my successor when he, in his turn, is looking round for a subject for his own presidential address, should be attracted by a domain on which I must myself decline to enter, but of which he, better perhaps than anyone among us, is fitted to give us a clear and comprehensive view." He professed to offer only fragmentary remarks, "hoping that even such fragmentary remarks may not be without their use if they serve to remind us of the vastness of our science, and yet of its unity; of its unceasing development, rapid at the present time, promising to be still more rapid in the immediate future, and yet deriving strength and vitality from roots which strike far back into the past, so that the organic continuity of its gigantic growth has been preserved throughout. In every science there is a time and place for general contemplations, as well as time for minute investigations. And it is a rule of sound philosophy that neither of these shall be neglected in its proper season ('itaque alternandæ sunt istæ contemplationes,' says Lord Bacon, 'et vicissim sumendæ ut intellectus reddatur simul penetrans et capax')."

Touching upon a charge brought against the *Proceedings* of the Society that its memoirs "have shown and still continue to show a certain partiality in favour of one or two great branches of mathematical science to the comparative neglect and possible disparagement of others," it might be rejoined "with great plausibility that ours is not a blamable partiality but a well-grounded preference. So great (we might contend) have been the triumphs achieved in recent times by that combination of the newer algebra with the direct contemplation of space which constitutes the modern geometry—so large has been the portion of these triumphs, which is due to the genius of a few great English mathematicians—so vast and so inviting has been the field thus thrown open to research, that we do well to press along towards a country which has, we might say, been 'prospected' for us, and in which we know beforehand we cannot fail to find something that will repay our trouble, rather than adventure ourselves into regions where, soon after the first step, we should have no beaten tracks to guide us to the lucky spots, and in which (at the best) the daily earnings of the treasure-seeker are but small, and do not always make a great show, even after long years of work. Such regions, however, there are in the realm of pure mathematics, and it cannot be for the interest of science that they should be altogether neglected by the rising generation of English mathematicians. I propose, therefore, in the first instance, to direct your attention to some few of these comparatively neglected spots."

The foremost place is assigned, by Prof. Smith, to the Theory of Numbers. "Of all branches of mathematical knowledge this is the most remote from all practical application, and yet, perhaps more than any other, it has kindled an extraordinary enthusiasm in the minds of the greatest mathematicians. We have the examples of Fermat, of Euler, of Lagrange, Legendre, of Gauss, Cauchy, Jacobi, Lejeune Dirichlet, Eisenstein, without mentioning the names of others who have passed away, and of some who are still living. But, somehow, the practical genius of the English mathematician has in general given a different direction to his pursuits; and it would sometimes seem as if we measured the importance of the subject by what we find of it in our best treatises of algebra, or as if we accepted the denunciations of Auguste Comte, and regarded the votaries of the higher arithmetic as reprobate of positive science, as moving in a vicious circle of metaphysical ideas, and as guilty of a great crime against humanity in the pursuit of knowledge beyond the limits of the useful. . . . I would rather ask you to listen to what is recorded of the great master of this branch of science."

Gauss (we are told by his biographer) held mathematics to be the queen of the sciences, and arithmetic to be the queen of mathematics—"She sometimes condescends to render services to astronomy and other natural sciences (so spoke the great astronomer and physicist); but under all circumstances the first place is her due." A citation was also made from Jacobi's Life of Göpel: "Many of those who have natural vocation for pure mathematical contemplation find themselves in the first instance attracted by the higher parts of the theory of numbers."

Three great departments of arithmetic were instanced: The arithmetical theory of homogeneous forms (or quantics)—"It is a memorable fact that some of the greatest conceptions of modern algebra had their origin in connection with arithmetic, and not with geometry or even with the theory of equations." In the "Disquisitiones Arithmeticæ" are given for the first time the characteristic properties of an invariant and a contravariant (for ternary quadratic forms). "But the progress of modern algebra and of modern geometry has far outstripped the progress of arithmetic; and one great problem which arithmeticians have now before them is to endeavour to turn to account for their own science the great results which have been obtained in the sister sciences. How

difficult this problem may prove is, perhaps, best attested by the little advance that has been made towards its solution." As an example, the researches of Cayley, Bachmann, and Hermite on the algebraical problem of the automorphies of a quadratic form, containing any number of indeterminates, were alluded to. Omitting many other points which were brought out, we can only mention the second department of arithmetic, the theory of congruences. In connection with this division, Prof. Smith also dwelt in detail upon the subject of complex numbers. "The last part of arithmetical theory to which I would wish to direct the attention of some of the younger mathematicians of this country is the determination of the mean values, or the asymptotic values of arithmetical functions. This is a field of inquiry which presents enormous difficulties of its own; it is certainly one in which the investigator will not find himself incommoded or crowded out by the number of his fellow-workers. 'Nemo est fere mathematicorum,' said Euler, in the last century; 'qui non magnam temporis partem inutiliter consumpserit in investigatione numerorum primorum;' but I do not think that (as a rule) the mathematicians of the present day have any reason to reproach themselves on this score." The speaker then pointed out what had been done in this direction since the days of Euler. "I do not know that the great achievements of such men as Tchebychef and Riemann can fairly be cited to encourage other and less highly gifted inquirers, but at least they may serve to show two things—first, that nature has fixed no impenetrable barrier to the further advancement of mathematical science in this direction; and secondly, that the boundary of our present knowledge lies so near us that at any rate the inquirer has no very long journey to take before he finds himself in the unknown land. It is this peculiarity, perhaps, which gives such perpetual freshness to the higher arithmetic. It is one of the oldest branches—perhaps the very oldest branch—of human knowledge, but yet its tritest truths lie close to some of its most abstruse secrets. I do not know that any more striking example of this could be furnished than by the theorem of M. Tchebychef. To understand his demonstration requires only such algebra and arithmetic as are at the command of many a schoolboy; and the method itself might have been invented by a schoolboy with the genius of Pascal or of M. Tchebychef."

Passing on to other branches of analysis, Jacobi's method of approximation ("a natural extension of the theory of continued fractions"), Lejeune Dirichlet's researches on complex units and his great generalisation of the theory of the Pellian equation, Liouville's treatment of irrational quantities, Lambert's proofs that neither π nor π^2 nor ϵ are rational with M. Hermite's extensions, who, though he has proved that ϵ is a transcendental irrational, declines entering on a similar investigation for the number π, but leaves this to others, adding, "Nul ne sera plus heureux que moi de leur succès, mais, croyez m'en, il ne laissera pas que de leur en coûter quelques efforts"—all came in for a notice.

Another class of questions mentioned were those which relate to the transcendental or algebraic character of developments in the form of infinite series, products, or continued fractions. The theorem of Eisenstein and M. Hermite's recent investigation of it, lately communicated to the Society, "are amply sufficient to awaken the expectation of great future discoveries in this almost unexplored field of inquiry."

Amongst important objects for mathematicians to set before them were named the advancement of the integral calculus ("confessedly all important in the applications of mathematics to physics"). In this connection the theory of differential equations and of singular solutions came in for a detailed notice, as also did the subject of elliptic functions.

Towards the close of the address, Prof. Smith said: "I am convinced that nothing so hinders the progress of mathematical science in England as the want of advanced treatises on mathematical subjects. We yield the palm to no European nation for the number and excellence of our text-books of the second grade; I mean of such text-books as are intended to guide the student as far as the requirements of our University examinations in honours are concerned. But we want works suitable for the requirements of the student when his examinations are over—works which will carry him to the frontiers of knowledge in certain directions, which will direct him to the problems which he ought to select as the objects of his own researches, and which will free his mind from the narrow views which he is apt to contract while getting up work with a view to passing an examination, or, a little later in his life, in preparing others for examination. Can we doubt that much of the preference for geometrical and algebraical speculation which we notice among our younger mathematicians is due to the admirable works of Dr. Salmon; and can we also doubt that if other parts of mathematical science had been equally fortunate in finding an expositor, we should observe a wider interest in, and a juster appreciation of, the progress which has been achieved?

There are, of course, other works besides those of Prof. Cayley and Dr. Salmon to which I might refer; there is, for example, the work of Boole, on Differential Equations; and there are the great historical treatises of Mr. Todhunter so suggestive of research, and so full of its spirit; we have also a recent work by the same author on the functions of Laplace, Lamé, and Bessel. But the field is not nearly covered . . . There are at least three treatises which we sadly need, one on definite integrals, one on the theory of functions in the sense in which that phrase is understood by the school of Cauchy and of Riemann, and one (though he should be a bold man who would undertake the task) on the hyperelliptic and Abelian integrals.

Geometry, and some other subjects, were hardly more than mentioned.

> "Verum hæc ipse equidem spatiis exclusus iniquis
> Prætereo, atque aliis post memoranda relinquo."

"In these days, when so much is said of original research, and of the advancement of scientific knowledge, I feel that it is the business of our Society to see that, so far as our own country is concerned, mathematical science should still be in the vanguard of progress. I should not wish to use words which may seem to reach too far, but I often find the conviction forced upon me that the increase of mathematical knowledge is a necessary condition for the advancement of science, and, if so, a no less necessary condition for the improvement of mankind. I should tremble for the intellectual strength of any nation of men whose education was not based on a solid foundation of mathematical learning, and whose scientific conceptions, or in other words, whose notions of the world and of the things in it, were not braced and girt together with a strong framework of mathematical reasoning. It is something to know what proof is, and what it is not; and where can this be better learned than in a science which has never had to take one footstep backward, and which is the same at all times and in all places. . . . I shall be more than satisfied if anything that may have fallen from me may induce any one of us to think more highly than he has hitherto done of the first and greatest of the sciences, and more hopefully of the part which he himself may bear in its advancement."

The address, delivered in the author's effective style, was frequently applauded by an appreciative group of members. On the proposal of Prof. Cayley it was resolved (with the author's consent) that the address should be printed in the *Proceedings*.

THE AUSTRIAN ARCTIC EXPEDITION[1]

IN addition to the points referred to in our article of last week, there are several others touched on in Lieut. Payer's work, which, in view of some of the results of our own expedition, it may not be unprofitable to dwell upon. Indeed a comparison between the observations and deductions of so keen and accomplished an observer as Payer and those of Capt. Nares's party, when these have been fully published, might, we think, lead to a distinct advance of our knowledge of the Arctic basin. And here we may be allowed to say that when so experienced and cautious an Arctic explorer as Payer expresses a decided conviction, as we understand he has done, that Capt. Nares acted in the only way possible under the circumstances, and no expedition could have been better conducted, surely it is a strong proof that our expedition was essentially successful.

The translator in his Preliminary Notice refers to the ice-experiences of the Austrian expedition as compared with those of the English expedition, and finds in many points a striking similarity between them. We have already referred to the tedious journey of the *Tegetthoff* party over the piled-up ice after they abandoned the ship, when they were able to make only nine miles in two months, suggesting inevitably the now well-known and ever-memorable experiences of Capt. Markham and his party. To all appearance this retreat of the Austrians was over a part of the same field which held the *Tegetthoff* in its grip, and which those on board saw in the very process of changing from a level floe to mountains of ice, as Payer calls them. It seems to be inferred by some that the ice of such enormous thickness met with by Markham was the result of the freezing of layer on layer through a long succession of years, since the last glacial epoch as it has been put—only of course a violent figure of speech. This notion we believe to be a delusion.

"The thickness which ice acquires in the course of a winter," Payer says, in his instructive chapter on "The Frozen Ocean," "when its formation is not disturbed, is about eight feet. In the Gulf of Boothia, Sir John Ross found the greatest thickness about the end of May; it was then 10 feet on the sea and 11 feet on the lakes. In his winter harbour in Melville Island Parry met with ice 7 or 7½ feet thick; and Wrangel gives the thickness of a floe on the Siberian coast, which had been formed in the course of a winter, at 9½ feet. According to the observations of Hayes the ice measured 9 feet 2 inches in thickness in Port Foulke. He estimates it, however, by implication, far higher in Smith's Sound: 'I have never seen,' he says, 'an ice-table formed by direct freezing which exceeded the depth of eighteen feet.' The rate at which ice is formed decreases as the thickness of the floe increases, and it ceases to be formed as soon as the floe becomes a non-conductor of the temperature of the air by the increase of its mass, or when the driving of the ice-tables one over the other, or the enormous and constantly accumulating covering of snow places limits to the penetration of the cold. While therefore the thickness which ice in free formation attains is comparatively small, fields of ice from 30 to 40 feet high are met with in the Arctic Seas; but these are the result of the forcing of ice-tables one over the other by pressure, and are designated by the name of 'old ice,' which differs from young ice by its greater density, and has a still greater affinity with the ice of the glacier when it exhibits coloured veins."

It seems evident, then, that the palæocrystic ice, like the ice in which the *Tegetthoff* was beset, is not the result of direct freezing of layer on layer, but to a great extent the result of pressure, by which a wide field may be broken up, and the pieces so piled over each other as to form impassable mountains and valleys. How this is accomplished may be learned from the impressive description of Lieut. Payer:—

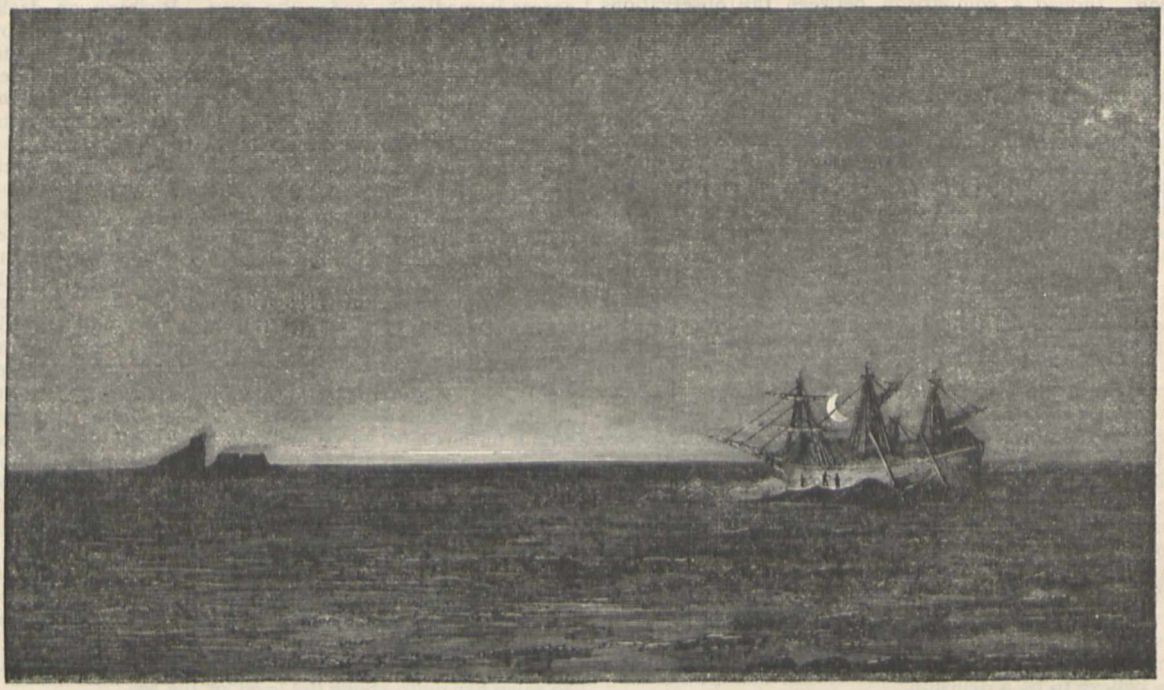
"A dreadful day was the 13th of October—a Sunday; it was decisive of the fate of the expedition. . . . In the morning of that day, as we sat at breakfast, our floe burst across immediately under the ship. Rushing on deck, we discovered that we were surrounded and squeezed by the ice; the after part of the ship was already nipped and pressed, and the rudder, which was the first to encounter its assault, shook and groaned; but as its great weight did not admit of its being shipped, we were content to lash it firmly. We next sprang on the ice, the tossing tremulous motion of which literally filled the air with noises as of shrieks and howls, and we quickly got on board all the materials which were lying on the floe, and bound the fissures of the ice hastily together by ice-anchors and cables, filling them up with snow, in the hope that frost would complete our work, though we felt that a single heave might shatter our labours. But, just as in the risings of a people, the wave of revolt spreads on every side, so now the ice uprose against us. Mountains threateningly reared themselves from out the level fields of ice and the low groan which issued from its depths grew into a deep rumbling sound, and at last rose into a furious howl as of myriads of voices. Noise and confusion reigned supreme, and step by step destruction drew nigh in the crashing together of the fields of ice. Our floe was now crushed, and its blocks piled up into mountains, drove hither and thither. Here they towered fathoms high above the ship, and forced the protecting timbers of massive oak, as if in mockery of their purpose, against the hull of the vessel; there masses of ice fell down as into an abyss under the ship, to be engulfed in the rushing waters, so that the quantity of ice beneath the ship was continually increased, and at last it began to raise her quite above the level of the sea."

It can easily be imagined that were ice which had been subjected to such a process to get jammed permanently into any position, it would become a formidable barrier to all passage over or beyond it. But the question arises—Does such mountainous ice never break up? Are these areas in the Arctic basin eternally covered with such ice, or is there a perpetual movement going on all over the Polar region? That the palæocrystic ice is not a fixture in the position in which our expedition found it we endeavoured to show in a previous article; if the observations of Hall and his party are to be trusted, and we believe they are perfectly reliable, the southern latitude of the formidable barrier must change considerably. That there is an open Polar Sea we do not think there is the least ground for believing. So far as we have seen, its only serious advocate is Dr. Hayes, one of its surviving "discoverers," and it is not to be at all wondered at that he should cling fondly to his pet theory. It is to be regretted that he did not wait for Capt. Nares's report, ere he rushed to an attack of the conduct and results of the English expedition; he might then have spoken more coolly and courteously. At Cape Fligely Lieut. Payer came upon a large stretch of open water which one less well-informed and with less of a scientific training might at once have eagerly taken for the border of an "open Polar Sea." Not so Lieut. Payer, who has no faith in such a dream; he took his open water for what it undoubtedly was seen to be on careful inspection, a polynia, or water-hole. Here is his opinion on the question. After referring to the experiences of previous explorers he says:—

"Those propitious ice-years amount, therefore, to nothing more than a greater recession of the outer ice-barrier—trifling when compared with the mighty whole—or to an increased navigability of certain coast waters, or to a local loosening of the inner polar ice-net. In reality the whole Arctic Sea, with its countless ice-fields and floes, and its

[1] "New Lands within the Arctic Circle. Narrative of the Discoveries of the Austrian Ship *Tegetthoff* in the Years 1872–1874." By Julius Payer, one of the Commanders of the Expedition. Maps and numerous Illustrations. Two vols. (London: Macmillan and Co., 1876.) Continued from p 65.

web of fine interlacing water-ways, is nothing but a net constantly in motion from local, terrestrial, or cosmical causes. All the changes and phenomena of this mighty network lead us to infer the existence of frozen seas up to the Pole itself; and according to my own experience gained in three expeditions I consider *that the states of the ice between* 82° *and* 90° N.L. *will not essentially differ from those which have been observed south of latitude* 82°; *I incline rather to the belief that they will be found worse instead of better.*"



Noon on December 21, 1873.

This is almost prophetic of the results obtained by the English expedition, and is one more proof of the accuracy of their observations. Still that there are one or more bodies of open water in the polar basin, bodies which are never permanently frozen over seems evident from even the comparatively little information we have.

Icebergs at the Base of the Middendorf Glacier.

The ice seems to be in almost constant motion within the basin except in the immediate vicinity of coasts, and in order that this may happen there must be open spaces somewhere. The southern edge of the Novaya Zemlya ice varied in the years 1871-2-4 by about 300 miles, and Hall saw only water and easily penetrable ice where our sailors were baffled by the impenetrable ancient ice. This simply shows that there is a constant shifting of the southern ice border, but that its position and that of whatever open water exists within the basin itself will ever be so favourable as to enable a ship to navigate to the Pole is to us quite incredible. That the polar ice, like all other phenomena, is subject to some laws in its movements, we must believe; what these are we as yet know not, but that

The View from Cape Tyrol. Collinson Fiord—Wiener Neustadt Island

they have some connection with the sun-spot period, is most likely. These and other points can only be satisfactorily settled by an international ring of Arctic observatories.

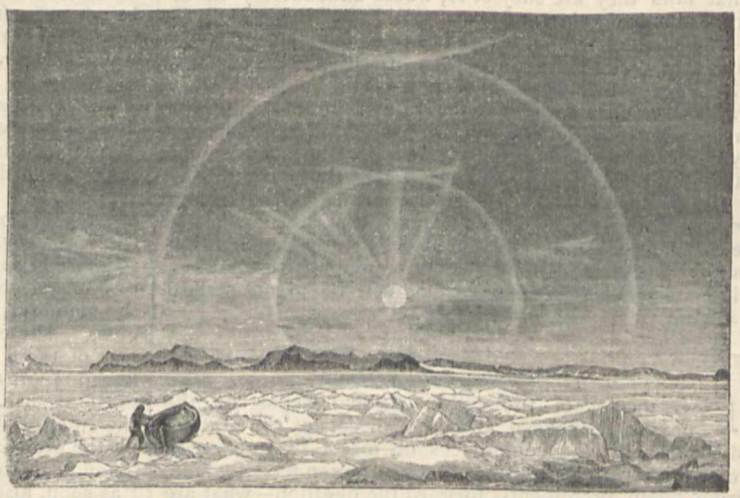
As to the future of the polar question, Payer believes that the days of large expeditions are past, and that until we are able to devise some aërial method of reaching or crossing the polar area, we ought to content ourselves

with completing our knowledge of what has been discovered, and carrying on observations on the plan proposed by Lieut. Weyprecht. Still that there will be attempts to penetrate farther northwards we think is very likely; and should any other nation surpass the latitude attained by Markham's party, or even find the secret of the pole itself, the English nation will not grudge it the honour. Ballooning has, since the return of our expedition, been frequently advocated as a means of polar exploration, and it may be interesting to mention that more than twenty years ago Parry used balloons as a means of scattering messages while his ship was frozen up.

The meteorological observations of the Austrian expedition are likely to be of the greatest value when fully published. Some data are given in the appendix, and a few extracts in the text from Admiral von Wüllerstorf-Urbair's analysis of them.

The observations made use of by Admiral v. Wüllerstorf-Urbair are those of the winds, including both direction and force, and such of the astronomical observations as served to fix the positions of the ship while it drifted in the ice from Novaya Zemlya to Franz Josef Land. The results were published in two charts in Petermann's *Mittheilungen* in 1875, which show the positions of the ship from August 24, 1872, to November 1, 1873, and the mean direction of the wind at the same times. They are deeply interesting from the light they seem to cast on the air and sea currents over this portion of the Arctic Sea. Speaking generally, during the first half of the course, or from October to the beginning of February, westerly and southerly winds prevailed and, during the latter half easterly and northerly winds; these winds being, it may be remarked, in accordance with the mean distribution of atmospheric pressure for the different regions and months respectively. The ice-drift followed approximately a similar course, subject apparently, however, to deflections which may be supposed to be due to the coasts of Novaya Zemlya and Franz-Josef Land, and to powerful ice-drifts from the Kara Sea and from the sea to eastward of Franz-Josef Land. From the investigation so far as carried out, it is concluded that in the sea lying between Novaya Zemlya and Franz-Josef Land the existence of a sea-current is probable, the prevailing winds being also in accordance with this supposition, and that a great expanse of sea to the north and north-east of Novaya Zemlya is also probable.

We look forward with much interest to the publication of the detailed account of the meteorological work of this expedition for the elucidation of several questions, such as the remarkable changes in the course of the ice-drift in the end of 1872 and beginning of 1873, viewed in connection with the weather of Northern Europe at the time. Thus, at Archangel the barometer rose on November 4 to 30·476 inches, fell on the 8th to 29·118 inches, and on the following day the temperature rose to 33°·8; immediately after this the wind shifted from S.W. to N.E., temperature fell to − 13°·0 on the 11th, and the barometer rose to 30·717 inches on the 13th. The great deviation in the course of the ice-drift, which extended from November 9, 1872, to February 2, 1873, began with this rise of the barometer, shift of the wind, and fall of the temperature at Archangel. Again, during January, 1873, the mean height of the barometer was 30·027 inches at Archangel; 29·826 inches at Kem, on the west coast of the White Sea; 29·838 inches at Vardö and 29·770 inches at Alten, both in the extreme north of Norway; 29·229 inches at Thorshavn, Farö; and 29·131 inches at Stykkisholm, Iceland. In connection with the remarkably disturbed state of the atmosphere in this arctic and sub-arctic region, as indicated by these figures, it is to be remarked that it was just during this time that the most remarkable deviation from the general course of the ice-drift took place. On January 2 the course suddenly changed from a north-north-westerly direction to a direction almost due east, which was steadily maintained untill February 2, when it again suddenly changed



Parhelia on the Coast of Novaya Zemlya.

Ice Pressure in the Polar Night.

to a north-north-westerly direction, the ship having drifted between these dates from 66° 50′ to 73° 20′ long. E.

No more powerful argument, we think, could be adduced than these facts for the establishment of a series of Arctic observatories; the influence of the changing Arctic conditions on the climate of Europe is unmistakable, and a knowledge of what these conditions are, and what laws they are subject to, would undoubtedly be of great practical value.

The chapter on the aurora is very interesting; it contains the valuable observations by Lieut. Weyprecht, which we published in NATURE, vol. xi., p. 368. Contrary to the experience of our own expedition and others in high latitudes on the American side, the auroras seen by the *Tegetthoff* were remarkably brilliant. No sound of any kind was observed to accompany the phenomenon.

Lieut. Payer's work, though professing to be only a popular narrative of the expedition, contains, it will be seen, much of great scientific interest, and we repeat that in the discussion of the results of our own expedition, his observations and conclusions will be found of real value.

OUR INSECT FOES

THE receipt of the eighth Annual Report on the noxious, beneficial, and other insects of the State of Missouri, and the conferences on insect destruction in connection with the Paris Insect Exhibition recently held, bring again prominently forward the question—what are we to do to cope with our insect foes? Mr. Riley, the State entomologist for Missouri, in his report, gives account of five noxious insects—the Colorado Potato-beetle (*Doryphora 10-lineata*, Say), the Canker Worms (*Paleacrita vernata*) and (*Anisopteryx pometaria*), the genus Paleacrita being a new one; the Army Worm (*Leucania unipuncta*, Han.), the Rocky Mountain Locust (*Caloptenus spretus*, Tho.), and the Grape Phylloxera. In each case an account is given of the estimated amount of damage done, and the proposed methods for attacking the enemy, as well as the life history, so far as is known, of the insect itself. While the damage by Colorado Beetle during 1875 was less than usual, owing to the excessive wet drowning the broods, and the Army Worm did comparatively little damage, the devastation caused by the locust was unusually heavy. Mr. Riley gives separately the accounts of different counties of the State.

One or two quotations will serve to indicate the gravity of the question, what is the remedy to be adopted? For example, in the account of Jackson County—"All kinds of growing crops disappeared before the black dead line of their advance. . . . With all the crops of wheat, rye, oats, flax, clover, corn, gardens, and pastures consumed in defiance of every human effort to stay the general devastation, the fields being as bare as the public roads, the outlook was gloomy beyond description. Many gave up in despair and left the county." So great was the destitution that relief meetings were held, the story of suffering being that many were reduced to a scanty supply of bread. Take again Buchanan County (written June 7): "The crops are all destroyed now, together with meadows and pastures." Again, Bates County: "all our crops and pastures eaten off until they are as bare as in mid-winter." St. Clair County: "The terrible sights of the cruel war are now being outdone by the cruellest of sights—starvation." And so on with a large proportion of the counties. Some counties were so fortunate as to escape with small damage. The total loss to the State for the year is set down at $15,000,000. A day of supplication to Almighty God, with fasting, was ordered on June 3 by the Governor. Mr. Riley, however, repudiates the idea that this calamity was a divine visitation, and quotes from a speech he made in the previous May, in which he said, "When I suggested last winter that a law should be passed offering a bounty for the eggs, the idea was ridiculed, but the people see now how wise such a course would have been. A few thousand dollars appropriated by the legislature for the purpose would have been the means of averting the present injury" (p. 93). The accounts given from some States describe the air as thick with locusts on the wing, so that darkness as of twilight was produced. We fortunately in England do not suffer from the locust, but we may learn a lesson as to what is the course considered necessary for coping with insect ravages. Nothing short of an Act of Congress to enforce the action to be taken seems to be regarded as of any real use. Although districts have previously suffered to the verge of starvation, we find Mr. Riley saying (p. 132), "It is very evident that if anything can be done at all in averting this evil, it must be done by national means. The advantage of having the matter properly investigated by the national government has been repeatedly urged by many prominent persons in the west best competent to judge." Societies have recently passed resolutions, the resolutions have led to a memorial, and the memorial to the introduction of two bills into Congress. The one proposed the appointment of a commission of three by the Commissioner of Agriculture, who are to report on the best means of preventing incursions of the locusts. The other proposed that the Secretary of the Interior shall appoint a board of three entomologists on the nomination of the National Academy of Sciences. They were to report on noxious insects generally, and "as soon, also, as the information gathered shall enable them, the commissioners shall compile practical instructions for the suppression of the different insects referred to." The amendments to both these bills were finally adopted in this form:—"That it shall be the duty of the Commissioner of Agriculture to investigate and gather information relative to those insects, &c. . . . and to make public from time to time such information and practical instructions for the suppression of the different insects." This, Mr. Riley remarks, is what people outside the senate were in the habit of supposing to be his duty. The chief practical suggestion Mr. Riley has to make is that State aid should be given for bounties of so many cents a bushel for the young insects while hatching. It will be some time, however, before we shall know what the Commissioner of Agriculture proposes to have done. Dr. Leconte, in his address before the American Association for the Advancement of Science at Detroit last year, urged the need for a law to compel farmers to destroy insects on their lands at a particular time.

Let us now turn to what has been done in France. We have already in a note, a few weeks ago, mentioned the way in which it is sought, through the elementary schools, to spread a knowledge of practical entomology. It remains now to refer to the attempts at legislation. As far back as 1732 a law was passed ordering farmers and landowners to destroy the caterpillars on their lands under a penalty of fifty livres. This 1732 law was renewed by prescriptions in 1777 and 1787. During the revolution, fines were abolished and rewards for destruction were substituted. It was found this plan was of no practical use. In 1796 the law known as that of 26 Ventose, an. iv. was passed. It enacted that the destruction of caterpilliars should be effected by the owners or tenants of land, and that if they neglected to do it the adjoints were to have it down and recover the expense from the negligent owner or tenant. The public lands were to be done at public cost, and the *Commissaires du Directoire Exécutif* were to visit the districts to see that all been carried out. The penalty fixed was not less than three nor more than ten days labour, in addition to repaying the expenses incurred by the employment of workmen. This law, made in 1796, is still the law for France, though practically it is not put in force. The local officials were found to hesitate in the performance

of the duties laid down for them, those even who were disposed to carry them out saw how useless it was for one district to be cleared while adjoining districts remained as practically breeding grounds for the pests. That a workable law is wanted has been shown by the attempts in 1839, 1849, 1851, and 1872, to introduce a bill that shall repeal the old law and substitute one that could be worked. The first two attempts fell through in consequence of dissolutions, the *coup d'état* interfered with that of 1851, but the proposal of 1872 was considered in March, 1873, passed a first reading December, 1874, a second on January 5, 1875; while the amendments were under discussion, however, M. Ducuing, the proposer, died. Last May a *projet du loi*, based on M. Ducuing's proposal, and somewhat modified in accordance with the discussion on the amendments, was introduced by MM. de la Sicotière, Grivart, and the Comte de Bouillé. They propose the law to apply to all harmful insects, the duty of destruction being imposed on the landowners and tenants. The time of the year to be selected for the destruction is to be made known by the prefect who will have scientific advice; the maires and commissionaires are to see that the law is carried out; in cases of neglect they are to have the work done, and recover the cost from those who should have done it. Special provisions are made for public lands, lands bordering on roads and railways. The fines are to range from 10 to 25 francs for a first offence. The articles that refer to the protection of birds that eat insects are not applicable to the wants of England as we already have legislation on that subject. But for years the want of some definite action to cope with "our insect foes" has been over and over again the subject of articles, speeches, and letters to the public press. The experience of France and America is that farmers must be compelled to look to their own interests. The department of practical entomology under the direction of the Committee of Council on Education is designed to give information regarding England's insect pests, but the question remains—How is the knowledge to be applied for the practical good of the country?

In connection with this important subject, the following extract from a Memorandum of the Canadian Minister of Agriculture, in reference to a despatch of the Secretary of State for the Colonies, on the subject of the Colorado Beetle is interesting:—

"The remedies which necessity has taught on this side of the Atlantic are such as to require for their application the joint effort of the community at large, kept alive to its interests and duties by the authorities, and men of devotedness to the common welfare. These remedies are (1) Searching for and crushing every potato-beetle wherever found; (2) Frequent visits to the potato-fields, and searching for the eggs deposited on the under-side of the leaves of the potato-vine; and (3) Watching for the presence of the larvæ on the buds and on the leaves of the plant, in order to destroy them by means of Paris green, the only substance yet discovered to be effectually operative on a large scale for the destruction of the insect in its larva state. By these means, and by these means only, the invaded American States, and the Western part of Canada, have been able to secure potato-crops in a measure commensurate with the care and energy bestowed, and by similar means only can the invasion be retarded and lessened in its effects. No measure has been taken in Canada, for reasons given, to prevent the falling or creeping of individual insects on board ships loading in Dominion seaports. There is, however, almost a certainty that the environs of Montreal will be invaded next year, and with that prospect in view, general orders may be given to public officers and *employés* of the ports to look for and destroy any beetles which might be observed on the wharves, on sheds, on packages of goods to be embarked, or on board ships. A general appeal might also be made to all persons having to deal with the shipping for assistance in the execution of such preventive measures. The undersigned respectfully recommends the adoption of such precautions, beyond which he does not see that there is anything within the power of the Canadian Government to do."

CARL JELINEK

DR. CARL JELINEK was born at Brünn, in Moravia, on October 23, 1822. He entered the University of Vienna in 1839 as a student of law, but soon thereafter his attention was turned more exclusively to the mathematical and physical sciences. In 1843 he assisted in the work of the Vienna Observatory, and in 1847 was appointed assistant in the observatory of Prague. It was while assisting in the work of the Vienna Observatory, then under the direction of Kreil, that his interest in exact observations in the fields of meteorology and magnetism was awakened—an interest deep and strong, which soon merged in a life-sacrifice to the furtherance of these sciences.

It is not necessary here to dwell upon his connection with the events of 1848 further than to say that the high moral qualities for which he was in after life so remarkable were even then conspicuous, and that the knowledge he then acquired of men and affairs was an invaluable training for the successful discharge of the duties of the public offices he afterwards filled.

His first important contribution to science was a paper published in 1850 on the construction of self-registering meteorological instruments, an important department of practical science to which he continued to make contributions down to the last. Indeed the last published number of the *Journal* of the Austrian Meteorological Society opens with the last of a series of articles by him on this subject. He was appointed in 1852 Professor of the higher mathematics in the Polytechnic School at Prague. Eleven years afterwards, or in 1863, he returned to Vienna as successor to Kreil, the first director of the Central Institute for Meteorology and Magnetism at Vienna. In this new sphere his remarkable powers of administration and organisation had full scope. The influence of this calm, eager, untiring, and clear-sighted worker and administrator was immediately felt. A new spirit was infused into the machinery of the institute, its resources were increased, connections were formed on all sides with the similar institutes and societies of other countries, and its annual publications were enlarged and improved; and in the course of time not the least important change was effected by the erection of new buildings for the Meteorological Institute on the Hohe Warte—an open commanding position on the outskirts of Vienna—thoroughly equipped with all the instruments required for meteorological and magnetical observation of the most improved construction, and placed in positions which indicate a clear perception of the problems to be investigated and the methods by which the observational data for their solution might be obtained.

On June 14, 1864, he was elected a corresponding, and on August 3, 1866, a full member of the Imperial Academy of Sciences at Vienna. In 1864 he became a member of the Unterrichtsrath, and from 1870 to 1873 acted as secretary of the High Schools for technology and the schools for industry and commerce, and brought to bear on the discharge of these duties the matured results of science, a strong will, and an eagerness and activity that never flagged, which were productive of the best results to the interests of the department he served. His public services were recognised when he retired from the education department in 1873, by having conferred on him the title and rank of Hofrath and the distinction of Knight of the order of the Iron Crown.

It is, however, in meteorology that he appears as an

original investigator endowed with an extraordinary capacity for work. In these respects meteorology is perhaps the sternest of the sciences in exacting from those of its votaries who make any permanent contribution to its literature not only keen scientific insight, but also courage to encounter for years, if need be, the constant drudgery of calculations before the end sought can be attained.

That Dr. Jelinek published scarcely any important work on meteorology from 1850 to 1865 need excite no surprise if we keep in view the great and important works on Austrian meteorology which appeared in quick succession from his pen between the years 1865 and 1870. The papers here referred to, about ten in number, several being really voluminous productions, are all of them rich in well-digested tabular matter, which, in relation to the subjects discussed, is of the most satisfying character. The paper on the five-day mean temperatures at Austrian stations from 1848 to 1863 contains 130 closely-printed quarto pages of tables. Among the subjects discussed in these papers are the mean annual and monthly distribution of atmospheric pressure and thunderstorms, and the annual, monthly, five-day, daily, and hourly distribution of temperature over the Austrian Empire; the temperature of Vienna for the ninety years from 1775 to 1864 thus supplying data calculated to throw light on not a few cosmical questions; the cold weather which occurs in May, and the storms of November and December, 1866. An important result of this work is that over the whole of Austria a closely approximate statement can be given as to how far the temperature as observed at any hour of any day of the year is above or below the average. In addition to the above, he wrote his admirable and well-known "Anleitung zu Meteorologischer Beobachtungen," which has already in the present year reached its third edition, and in 1866 established and edited, jointly with Dr. Hann, the *Journal* of the Austrian Meteorological Society, which is published fortnightly, and which, from its liberal and catholic spirit, and the position in science it has attained, stands alone among meteorological publications. In 1865 he succeeded in introducing telegraphic weather reports in Austria. Dr. Jelinek was also Secretary of the Meteorological Society of Austria, and the important services he rendered in connection with the Meteorological Congresses at Leipsig and Vienna are well known.

Thus the Austrian Meteorological Institute, under Dr. Jelinek's management, has not merely made observations and published results, but it has also discharged the functions of a discussing body of a high order. The domain of meteorology in which Dr. Jelinek takes the highest position is that which is concerned with the discussion of averages, taking the term in its widest significance. It is here where his scientific insight appears to the best advantage. We may refer in illustration to the judicious use he makes of the method of differentiation in the discussion of such problems as the normal atmospheric pressure in Austria during the months of the year. He does not commit the mistake of taking different terms of years for different places, according as observations at each place were available, but by the application of the method of differentiation he practically takes the same terms of months and years for all places. In all his writings there is evinced the greatest care to avoid giving expression to any view or speculation unless he had taken the trouble of collecting together all available information that lay in his power bearing on the point in question.

He died, after a lingering illness, on October 19, being thus prematurely cut off at the comparatively early age of fifty-four—a man of singularly noble and spotless character, ever on the alert, if we may use the expression, to discover and recognise real work wherever it appeared, and ever ready to offer his help to workers in science, even though he could do so only at the expense of much personal trouble and fatigue. His beneficence was characteristic of the man, being absolutely without ostentation, and his kindly acts were performed as if his left hand knew not what his right was doing.

OUR ASTRONOMICAL COLUMN

THE DISTANCES OF THE STARS.—We shall here endeavour to present at one view the most reliable results of investigations relating to stellar parallax up to the present time. In making the selection parallaxes less than a tenth of a second of arc are omitted except in the case of the pole-star, for which independent researches have given values closely approximating to this amount. In estimating the "light-years," we adopt Struve's determination of the time occupied by light in traversing the mean distance of the earth from the sun, viz., 8m. 17·78s. (According to Leverrier's last value for the solar parallax, and Clarke's diameter of the earth's equator, this would assign for the velocity of light, 185,360 miles per second, at which rate of travelling it would arrive at the planet Neptune in 4h. 10m., or the breadth of the planetary spaces as at present known would be traversed in less than 8½ hours.) By "light-years" is of course to be understood the interval which light would require to pass from the star to the earth at the distances respectively assigned.

The authorities are, for α Centauri, Henderson's value as corrected by Peters, and that of Moesta, the mean; for 61 Cygni, Auwer's mean of his own result and that of Otto Struve; Lalande 21185, Winnecke; β Centauri, Sir Thomas Maclear; μ Cassiopeæ, Otto Struve; Groombridge 34, Auwers; Capella, Otto Struve; Lalande 21258, Krüger; Oeltzen 17415, Krüger; σ Draconis, Brünnow; Sirius, Gylden from Maclear's observations at the Cape of Good Hope; α Lyræ, Brünnow's mean; 70 Ophiuchi, Krüger; η Cassiopeæ, Otto Struve; Procyon, Auwers; Groombridge 1830, a mean of results of Brünnow, Schlüter, Wichmann, and Otto Struve; and for Polaris, Peters.

Name of Star and Magnitude.	Annual Parallax.	Distance in Solar Distances.	Light-years.
	″		
α Centauri (1 and 4) ...	0·928 ...	222,300 ...	3·5
61 Cygni (5½ and 6) ...	0·553 ...	373,300 ...	5·9
Lalande 21185 (7½) ...	0·501 ...	411,700 ...	6·5
β Centauri (1)	0·470 ...	439,100 ...	6·9
μ Cassiopeæ (5½)	0·342 ...	603,100 ...	9·5
Groombridge 34 (8½) ...	0·307 ...	671,900 ...	10·6
Capella (1)	0·305 ...	676,300 ...	10·7
Lalande 21258 (8½) ...	0·271 ...	761,400 ...	12·0
Oeltzen 17415 (8½) ...	0·247 ...	835,100 ...	13·2
σ Draconis (5)	0 246 ...	838,500 ...	13·2
Sirius (1)	0·193 ...	1,069,000 ...	16·9
α Lyræ (1)	0·180 ...	1,146,000 ...	18·0
70 Ophiuchi (4½)	0·162 ...	1,273,000 ...	20·1
η Cassiopeæ (4½ and 7)	0·154 ...	1,339,000 ...	21·1
Procyon (1)	0·123 ...	1,677,000 ...	26·5
Groombridge 1830 (6½)	0·118 ...	1,748,000 ...	27·6
Polaris (2)	0·091 ...	2,267,000 ...	35·7

In the third column is given the distance of the star from the earth, in mean distances of the earth from the sun, as is usual; it will be seen how greatly the alteration, even of a single unit in the last decimal place of the annual parallax in the preceding column, affects these numbers.

So far as our present knowledge extends, light, travelling at upwards of 185,000 miles per second requires 3½ years to pass from the nearest fixed stars to the earth, and it does not reach us from our well-known northern polar star in less than thirty-five years.

THE TOTAL SOLAR ECLIPSES OF 1239, JUNE 3, AND 1241, OCTOBER 6.—Prof. Celoria has published an important memoir on these eclipses, in the *Transactions* of the Royal Institute of Sciences at Milan, vol. xiii. He refers to a note in NATURE, vol. xii., p. 167, in which, when remarking on his first compu-

tation of the eclipse of 1239, it was suggested that this phenomenon might deserve further examination in connection with the eclipse of 1241, which had been already calculated by Hansen. The present memoir contains a very careful and complete discussion of the two eclipses, employing Leverrier's tables for the sun, and Hansen's lunar tables, except that the last values for the terms involving the square of the time, given by Hansen in "Darlegung der theoretischen Berechnung der in den Mondtafeln angewandten Störungen," Part 2, are substituted for the values adopted in the tables. The position of the belt of totality in the eclipse of 1241, in its passage across Germany, is very well defined by the statements of contemporary writers, taken chiefly from the great work of Pertz, "Monumenta Germaniæ Historica;" Prof. Schiaparelli had been similarly successful in laying down the actual track of totality across Italy in the eclipse of 1239, from the Records in Muratori's collection of Italian writers. In both cases totality is assumed to have taken place, when there is distinct mention of stars having appeared, which is about the only criterion that has value at these distant times. We shall probably revert to the subject of Prof. Celorai's able memoir.

NOTES

MANY geologists who have visited the Philadelphia Exhibition and seen the geological collections there have been impressed with the importance of having as nearly complete a collection as possible on exhibition, of geological specimens, maps, and sections, in accordance with a previously arranged plan. The International Exhibition to be held at Paris in 1878 will furnish such an occasion, and it is proposed to invite to that end governmental geological surveys, learned societies and private individuals throughoutt the world, to send to Paris such collections as will make the geological department of that exhibition as complete as possible. In order to take advantage of the collections which may thus be brought together, it is moreover proposed to convoke an International Geological Congress, to be held at Paris at some time during the Exhibition of 1878, and to make that Congress an occasion for considering many disputed problems in geology. In accordance with this plan it is proposed that the Geological department of the International Exhibition of 1878 shall embrace :—1. Collections of crystalline rocks, both crystalline schists and massive or eruptive rocks, including the so-called contact-formations and the results of the local alteration of uncrystalline sediments by eruptive masses. 2. Collections illustrating the fauna and the flora of the Palæozoic and more recent periods. 3. Collections of geological maps, and also of sections and models, especially such as serve to illustrate the laws of mountain structure. In pursuance of the above plan the American Association for the Advancement of Science during its annual meeting at Buffalo, appointed a Committee to carry out this scheme, to which were added the names of Prof. Huxley, Dr. Otto Torell, and Dr. E. H. von Baumhauer. Prof. James Hall was elected chairman, and Dr. T. Sterry Hunt, secretary. It was then resolved to prepare a circular to be printed in English, French, and German, and distributed to geologists throughout the world, asking their co-operation in this great work of an International Geological Exhibition and an International Geological Congress to be held at Paris in 1878; the precise date of the Congress to be subsequently fixed. All those interested in this project are invited to communicate with any one of the following members of the Committee :—Prof. T. H. Huxley, London, England; Dr. Otto Torell, Stockholm, Sweden; Dr. E. H. von Baumhauer, Harlem, Holland; Dr. F. Sterry Hunt, Boston, Mass., U.S.A.

AT a recent meeting of the Literary and Philosophical Society of Manchester, Prof. Osborne Reynolds, in justly animadverting on the large type sensation headings in which some newspapers announced what, in their perversity or ignorance, they called the "failure" of the Arctic expedition, showed that in truth the expedition had been one of the finest achievements ever accomplished. Looked at boldly, it comes to this. Since Hudson's time, more than 200 years ago, Arctic navigators had succeeded in penetrating about sixty or seventy miles of the 540 to be passed before the Pole could be reached. Whereas Capt. Nares has, in one year, carried the British flag some sixty miles nearer, so that nearly one half, and this by far the most difficult half, of the entire results of all expeditions since Hudson's time has been accomplished by the last. And this is not all. Capt. Nares seems to have pursued the journey to its end, at least by that route; and in coming back can say that he did not leave a single uncertainty behind him. So far, therefore, from having been a failure, this has been the most successful expedition ever sent out.

IT is expected that the French Government will ask our Admiralty to establish an Arctic department in the Exhibition of 1878, in which all the relics of English Arctic exploration will be collected and exhibited, as well as all the Parliamentary papers and publications relating to the subject.

M. CHEVREUL was entertained at dinner the other day at the Café Corazza, in the Palais Royal, by eighty *savants* in celebration of the fiftieth anniversary of his professorship and membership of the Academy of Sciences. M. Chevreul, now the oldest member of the Academy of Sciences, is ninety years old, and enjoys perfect health and mental vigour. The most notable instances of academical longevity have been Fontanelle, one of the perpetual secretaries, who died in 1742, aged close on 100 years; M. Biot, who lived ninety-two years, and preserved to the end of his days his mental powers; M. Mathieu, who died March 5, 1875, was also a nonogenarian, and the *Annuaire du Bureau des Longitudes* for 1875 was edited by him. He had succeeded in 1817 Messier, an astronomer, who was an Academician during more than forty years, so that the same seat had only two occupants in a whole century.

A SERIES of lectures is now being given by eminent men of science, explanatory of the instruments in the Loan Collection of Scientific Apparatus at South Kensington. The lectures are free, and working men are invited to attend. The lectures at present arranged for are as follows :—Saturday, November 25, Prof. W. Leith Adams, F.R.S., on "Extinct Animals," as represented by magic lantern slides and specimens in the loan collection. Saturday, December 2, J. S. Gardner on "The Collection of Fossil Leaves." Saturday, December 9, J. Norman Lockyer, F.R.S., on "The Spectroscopes in the Collection." Saturday, December 16, Prof. Huxley, F.R.S., on "The Systematic Teaching of Biology." The lectures will be delivered in the Lecture Theatre of the South Kensington Museum at eight o'clock P.M.

PROF. HUGHES read a paper before the Cambridge Philosophical Society last Monday, in which he criticised the evidence offered to support the view that man existed on the earth during or before the glacial period. He first reviewed several of the older cases which had been put forward, and tried to show that the evidence was always incomplete, or that its trustworthy character disappeared on closer examination. Coming to the two more recent and important instances of human remains or implements being found beneath glacial beds or in beds older than the glacial, Prof. Hughes gave his opinions from personal inspection and acquaintance with the localities. The human fibula found under glacial till in Victoria Cave, Settle, with *Elephas antiquus*, *Rhinoceros leptorhinus*, &c., had been regarded as decisive. Mr. Tiddeman (NATURE, vol. xiv. p.

506) says, "The Settle till is undoubtedly of the age of the ice-sheet." Prof. Hughes said that although the boulder clay at the mouth of the cave had got rather underneath the brow of the hill, yet from intimate knowledge of the physical nature and conditions of the district, which he had himself mapped, he saw no impossibility in the idea of the boulder clay having tumbled from the cliff above during the process of wearing back. Very often the upper limestone was so dissolved as to form pockets into which the boulder clay was let down, and then when an escarpment was disintegrated, he could quite conceive how such a pocket was thrown obliquely against the mouth of the cave in post-glacial times. This had ponded back the water that came into the cave, and necessarily produced a stratified deposit, in which the remains in question were found. With regard to the evidence brought forward by Mr. Skertchley, of the occurrence of palæolithic implements in brick earth beneath the chalky boulder-clay at Thetford, near Brandon, he had visited the locality, and his opinion was that in each case there was a missing link in the proof that the clay beneath which the implements were found was identical with clay at no great distance which was indubitable boulder-clay. In fact, there were many indications of the opposite. Local conditions in denudation, solution of chalk, formation of the valleys, &c., were abundantly present to mask the true state of things. The proof in this case was certainly not cogent; and it must be cogent to be accepted.

We are glad to be able to confirm the report which appeared in our columns a short time ago that the Goldsmiths' Company had promised to contribute 1,000*l.* to the Chemical Society's Research Fund. This sum has now been handed over to the society and raises the amount of the fund already received to 3,050*l.*, a sum which we hope will be still further largely increased.

In a communication to the St. Petersburg Society of Naturalists, Prof. Fr. Schmidt sketches the Tertiary formations on the northern shores of the Pacific as follows:—The formation consists of two stages. The Lower is a continental Miocene deposit with coal-seams and numerous plants, the complete description of which will soon appear by Dr. Oswald Heer. This deposit has a very wide extent, having been found in the middle parts of the Amoor basin, on the Sakhalin, in Kamtchatka, Alaska, and on Vancouver Island; and nearly the same rich flora which it contains can be traced as far as the Mackenzie River, Greenland, and Spitzbergen. An immense continent between North-eastern Asia and North-western America must thus have existed at this epoch, and its flora shows the prevalence of a far warmer climate than now, probably like that which the middle parts of the United States now enjoy. The Upper Tertiary stage is a marine Pliocene deposit with numerous remains of molluscs, and it was observed on the Sakhalin Island (but wanting at the same time on the closely-adjacent Siberian continent), in Kamtchatka, on the Aleutian Islands, in Oregon, U.S., and in California. Notwithstanding their varied lithological characteristics, these deposits contain a remarkably uniform fauna. The number of species already described by Prof. Schmidt, on the basis of large collections made during the last thirty years, is eighty, out of which eighteen have no living representatives, six inhabit only the Polar Sea and the Northern Atlantic, and the remaining fifty-six still inhabit the Northern Pacific. Out of the eighteen extinct forms six were already found in the Tertiary of Oregon and California, and one of them (*Nucula ermani*, Girard) will probably prove to be the same as the *N. cobboldiæ*, Sow., of the English Crag. Generally, during the Pliocene epoch, the faunas of the northern parts of the Pacific and the Atlantic were far more alike than now, and it must be supposed that the connection between both oceans through the Polar Sea was far closer than now, a supposition supported also by the close likeness of some forms inhabiting the Pacific and the Atlantic shores of Northern America. Their close likeness, which appears so strange when we learn that they do not now inhabit the Polar Sea, is perfectly explained when we find them in a fossil state in the Pliocene deposits of the far north, as was the case with the *Pholas crispata* and the *Pectunculus pilosus*, which were found fossil, the former on the Northern Dwina and Jenissei, and the second on the Kadiak Island. The fossil fauna of the Arctic regions thus explains the present distribution of forms. Prof. Schmidt expresses the wish that the Pliocene deposits of these regions were thoroughly explored as soon as possible.

At the meeting of October 21 of the Geological Section of the St. Petersburg Society of Naturalists, Prof. Friedrich Schmidt made an interesting communication on the Post-Glacial Period in Esthonia. Starting from the supposition—which he supports in common with Swedish and Finnish geologists—that Esthonia was covered during the Glacial period with an ice-sheet which concealed it with Scandinavia, Finland, Northern Russia, reaching probably the southern slope of the Waldai plateau, Prof. Schmidt proved that after the melting of the ice the country was covered with numerous immense laxes. The land was then submerged by the sea, but only to a small extent, as notwithstanding many years' careful researches, formations with marine fossils have not been found in Esthonia further than 30 kilometres distant from the Gulf of Finland, nor on levels higher than 60 feet above the sea. They are Post-Glacial, containing a fauna which, with very few exceptions, inhabits now the Baltic. After the submergence the land rose to its present height, but this elevation was probably accomplished during pre-historic times. At least, M. Schmidt states, contrary to the assertions of MM. Baer, Hofmann, and others as to the present rising of all the islands of the Gulf of Finland, there was not in Esthonia evidences of the rising of the land during the last four centuries which could be accepted as unmistakable. It may be remarked that the conclusions of Prof. Schmidt as to the small submergence of Esthonia, however contradictory of current opinions are also supported by the circumstance that marine formations were not found in Eastern Sweden above a level of 100–120 feet; and that in Finland the traces of marine clays (with *Cardium edule* and *Tellina balthica*) totally disappear at a level higher than 62 feet. These negative evidences have some weight, both countries having been well explored along some parts of their coasts.

At the meeting of the Literary and Philosophical Society of Manchester, on October 17, Mr. Baxendell drew attention to the paper "On the Protection of Buildings from Lightning," read by Prof. J. Clerk Maxwell at the late meeting of the British Association at Glasgow, and stated that the system of protection recommended by the professor, and which he appears to have regarded as new, was suggested, and its adoption strongly advocated, nearly forty years ago by the late Mr. Sturgeon, whose many valuable contributions to electrical and magnetical science seem to have been strangely overlooked by recent investigators and writers. The paper in which the system was first described was read before the London Electrical Society on March 7, 1838, and an abstract of it was published in the second volume of the "Annals of Electricity." There is, however, one important difference between the two systems. Mr. Sturgeon considered it necessary that the copper sheathing or covering of a protected room or powder magazine should be well connected with the ground; but Prof. Maxwell is reported to have stated that "there would be no need of any earth connection. They might even place a layer of asphalte between the copper floor and the ground, so as to insulate the building." It is obvious, however, Mr. Baxendell states, that if the magazine were struck by lightning, a disruptive discharge through the layer of asphalte would in all probability take place, which might rupture the copper sheath-

ing, and thus ignite the contents of the magazine; but by the adoption of Mr. Sturgeon's plan an accident of this kind could not occur.

HER Majesty has commanded that instructions be given to the Master of the Mint to prepare a die and cast a sufficient number of medals commemorative of the Arctic Expedition. These are to be distributed amongst the officers and crews of the *Alert*, the *Discovery*, and the *Pandora*. The Lord Mayor, Sir Thomas White, is to give a dinner to the crews of the *Alert* and *Discovery* at the Mansion House, on the evening of December 5. The Lord Mayor intends to invite the officers of the expedition to a banquet a few days later, probably December 8. The inhabitants of Portsmouth intend to entertain the crews of the *Alert*, *Discovery*, and *Pandora* at a dinner on the 30th inst.

MR. R. J. FRISWELL has resigned the honorary secretaryship of the Photographic Society.

THE Ethnographical Museum of Berlin has lately received several valuable additions, viz., the objects acquired by Dr. Lenz during the German-African Expedition. They represent tribes from the West Coast of Africa hitherto little known, and consist of various wooden weapons, domestic implements and musical instruments. Very shortly the excellent collections of the celebrated African traveller, Dr. Nachtigal, are also to be exhibited at the museum. Some of these come from tribes which Dr. Nachtigal was the first European to see. Much inconvenience is felt at the museum through want of space, and particularly so with reference to some American curiosities sent for exhibition by Prof Bastian.

ST. JOHN'S COLLEGE, Cambridge, offers for competition an exhibition of 50*l.* per annum, for proficiency in Natural Science. The examination will commence on Saturday, April 7, and will be in chemistry, including practical work in the laboratory; Physics, viz., Electricity, Heat, and Light; and in Physiology Candidates will also have the option of being examined in Geology, Comparative Anatomy, and Botany, provided they give notice of the subjects in which they desire to be examined four weeks prior to the examination. No candidate will be examined in more than three of these six subjects, whereof one at least must be chosen from the former group. It is the wish of the master and seniors that excellence in some one department should be specially regarded by the candidates. Names should be sent to one of the tutors at least fourteen days before the examination.

A CHAIR of Physical Astronomy has been created in the Faculty of Sciences at Lyons.

HOW rapidly an interest in prehistoric researches is spreading among the educated classes in Germany is seen in the progress of the Anthropological Society of Munich. It was constituted in 1870 as a branch of the German Anthropological Society, founded only three weeks before it. It began with thirty-five members and now numbers 300.

THE present efforts of the German Anthropological Society are directed towards the preliminary preparations for an Anthropological and Primitive History of Germany; by means of a territorial division of labour, extremely important data have already been obtained. Contributions on particular points are appearing in the *Correspondenzblatt* of the German Anthropological Society, the *Archiv für Anthropologie*, and the Berlin *Zeitschrift für Ethnographie*. Meantime, at the Anthropological Meeting held in Munich last year, a plan was formed by a number of Anthropologists, in reference to the astonishing richness of the prehistoric discoveries in Bavaria, for the advancement of German Anthropology, by the establishment of an organ for the publication of the complete results which have been obtained in reference to Bavaria. The accomplishment of this scheme has now commenced with the publication of a part of the *Beiträge zur Anthropologie und Urgeschichte Bayerns*, an organ of the Munich Society of Anthropology. This part contains a monograph on the "Lake Dwellings in the Würm See," by S. v. Schab, who has thoroughly explained this prehistoric dwelling-place, and collected and arranged the rich finds. The most important of the latter are shown in seventeen lithographic plates, partly coloured, with maps, plans, &c.

THE *Daily News* Alexandria correspondent sends to that paper, November 21, some particulars of the life and work of an African explorer who has been quietly doing good service for many years. This is Signor Piaggia, who went to Tunis first in 1851 as a gardener, and there and in Alexandria saved money for years, with which he went up to Khartoum. From then until now he has made several important journeys; to the country of the "Kieks," the territory of the Niam-Niams, where he became a great favourite, and stayed more than two years, the Bogos territory on the borders of Abyssinia with the Marquis Antinori, into Cerada, south-east of Lake Tzana, which he explored minutely. Latterly, after accompanying Gessi for some distance, he has been in the districts of Mrooli and Mtesa, and made a thorough exploration of the Lake Capechii. From all his journeys he has brought back large collections of objects of all kinds, which, it is believed, are now for sale.

THE *Moniteur de l'Algérie* states that from November 8 a violent sirocco, or wind from the desert, was felt in Algeria for several days. This accounts for the unusual elevation of temperature in Algeria.

THE exhibition of fossils, paintings, plants, and other objects illustrative of the mountain limestone in the Manchester Aquarium, was closed on Monday, having been open to the public a little more than a week. The exhibition has been in every way successful.

FURTHER details received regarding the cyclone of October 31 prove it to have been one of the most terrible calamities on record. Estimates based on official returns from each police section put the loss of life in the districts of Backergunge, Noakholly, and Chittagong at not less than 215,000. Three large islands—Dakhin Shahabazpore, Hattiah, and Sundeep—and numerous small islands were entirely submerged by the storm wave, and also the mainland for some five or six miles inland. These islands are all situated in or near the estuary of the Meghna, a river formed by the confluence of the Ganges and Brahmapootra rivers. Up to 11 P.M. on the night of the catastrophe there were no signs of danger, but before midnight the storm wave swept over the islands to a depth in places of 20 feet, surprising the people in their beds. The country is perfectly flat, and, therefore, trees were the only secure range. Almost every one perished who failed in reaching trees. A strange fact about the disaster is that in Dakhin Shahabazpore and Hattiah most of the damage was done by the storm wave from the north sweeping down the Meghna. Several theories, the *Times* Calcutta correspondent states, have been started to account for this. One is that the cyclone, forming in the bay, struck the shore first near Chittagong, and went north for some distance, and then turned southward again. Another is that the wind blew back the waters of the Meghna, which rebounded with terrific force when the pressure relaxed. A third supposition is that there were two parallel storms with a centre of calm between them. The first or third theory seems most probable, as in Sundeep and Chittagong the destruction came from the south.

MR. HENRY MEIGGS, the well-known American railroad contractor and engineer, whose efforts in connection with the establishment of railroads in Peru are so well known and appreciated, has recently furnished the means to M. Bur, a

French ethnologist, for carrying on some explorations in the vicinity of Tiahuanuco, in Bolivia, a region which abounds in objects of archæological interest. Mr. Meiggs has arranged that a full series of these objects shall be presented to the U.S. National Museum.

A VALUABLE entomological collection has been presented to the Oxford University Museum by Mrs. Tylden, the relict of the late Rev. W. Tylden, formerly of Balliol College. The collection numbers 23,518 specimens, arranged in cabinets.

PROF. PALMIERI, Director of the Observatory on Mount Vesuvius, has been made an Italian Senator.

A DESPATCH received at Rome from Aden, November 19, states that the Italian African Expedition has arrived in Shoa.

THE Government Resident at Somerset has telegraphed to the Colonial Secretary that Messrs. D'Albertis, Hargrave, and party have returned safely from their expedition to the Fly River. They ascended the country a distance of 350 miles above the spot reached by the expedition party of last year. They were unable to communicate in any way with the natives who were very numerous and hostile.

A SIXTH edition of Prof. Page's well-known "Advanced Text-Book of Geology" has been published. The work has been enlarged "to embrace whatever is new and important in the science, to afford space for additional illustration, and to combine, as far as possible, the principles with the deductions of geology."

WE have on our table the following books :—Preliminary "Report on the Forests of Pegu," by Sulpice Kurz (Calcutta). "The Aquarium," J. E. Taylor (Hardwicke and Bogue). "Spiritualism and Animal Magnetism," Dr. Zerffi (Hardwicke and Bogue). "The Theory of Colour," Dr. W. von Bezold (Trübner). "The Art of Retouching," Burrows and Colton (Marion). "Science in Sport made Philosophy in Earnest," by Robert Routledge (George Routledge and Sons). The ninth edition of "Kirke's Physiology," edited by Morrant Baker (John Murray). "Between the Danube and the Black Sea," H. C. Barkley, C.E. (John Murray). The fourth edition of Wanklyn's "Water Analysis" (Trübner). "Demonstrations of Microscopic Analysis," Harley and Brown (Longmans). "Mushrooms and Toadstools," Worthington G. Smith (Hardwicke and Bogue). "Geological Observations," Charles Darwin, F.R.S. (Smith, Elder, and Co.). "Lessons in Electricity," John Tyndall, F.R.S. (Longmans). "Our Birds of Prey," The Raptores of Canada, H. G. Vennor (Sampson Low and Co.).

THE additions to the Zoological Society's Gardens during the past week include two Prussian Carp (*Carassius vulgaris*), European, presented by Lord Arthur Russell, F.Z.S. ; a Bubaline Antelope (*Alcelaphus bubalinus*), an Addax Antelope (*Addax naso-maculatus*) from North Africa, a Buff-breasted Partridge (*Ptilopachys ventralis*) from West Africa, four Brazilian Cormorants (*Phalacrocorax brasiliensis*) from Brazil, purchased ; a Macaque Monkey (*Macacus cynomolgus*) from India, a Chilian Sea Eagle (*Geranoëtus aguia*) from South America, deposited ; a Hairy-rumped Agouti (*Dasyprocta prymnolopha*), born in the Gardens.

SCIENTIFIC SERIALS

Schriften der physikalisch-oekonomischen Gesellschaft zu Königsberg (1875, 1 and 2).—These parts, amongst a number of smaller papers and notes, contain the following more important treatises: —On the determinations of temperature in the soil at different depths at the station at Königsberg, by Prof. E. Dorn.—Observations on the genera of *Nematidæ*, by C. G. A. Brischke and Prof. Dr. G. Zaddach. This paper occupies more than half of Part 1.—On the temperature in the interior of animal bodies, by Dr. Adamkiewicz.—On a new species of Algæ, *Merismopedium Reitenbachii*, Casp., by Prof. Caspary.—On the different forms of the stigmatic disc of *Nuphar luteum*, Sm., by the same.—On the latest investigations made by M. Lassaulx on earthquakes, by O. Tischler.—On the so-called "Moosbrüche," specially on the "Zehlaubruch," near Tapiau, by Herr Stiemer.—On the courses of rivers," &c., in the North German plains during the Diluvial period, by Prof. Berendt.—On one of Euler's geometrical problems, by Dr. Saalschütz.—On the courses of rivers in the province of Prussia, by Herr Stiemer.—On the Colorado beetle, by Dr. Schiefferdecker.—On reflectors, by Dr. Berthold. —On Gore's rotating ball, by Herr Momber.—On the artificial production of colours from the white of eggs, by Dr. Adamkiewicz.—On Phylloxera, by Dr. Benecke.—On the oscillations of *terra firma*, by Dr. A. Jentzsch.—New list of Prussian beetles (fourth paper), by Dr. Lentz. The author makes the total of different species to the number of 3,216!—On old Prussian "Kjökken Möddings" at the coast of the "Frische Haff," by Prof. Berendt.—On the conception of value in the different theories of the same, by Adolf Samter.—On the power of accommodation amongst plants and insects, by Dr. G. Czwalina.—On an erratic block-limestone found near Tilsit, by Dr. Friederici.—On salicylic acid, by Prof. Samuel.—Archæological researches on the "Kurische Nehrung," by O. Tischler.—On an alleged proof of the early existence of man in Europe, by Dr. Jentzsch.—On an unusually large fungus, *Agaricus suffructicosus*, by Prof. Caspary.—The Appendix contains the report sent by the Society to the Provincial Landtag on its geognostical researches in the province of Prussia.—The parts further contain a memoir of the late Prof. Argelander, of Bonn, by Dr. Luther.

SOCIETIES AND ACADEMIES

LONDON

Mathematical Society, November 9.—Prof. H. J. S. Smith, F.R.S., president, in the chair.—Mr. J. W. L. Glaisher communicated a note on certain identical differential relations.—Mr. Tucker read parts of papers by Mr. Spottiswoode on curves having four-point contact with a triply-infinite pencil of curves, and by Mr. E. B. Elliott on some classes of multiple definite integrals.—In a paper published in the *Mathematische Annalen* (vol. iii. p. 459) Brill has investigated the case of curves having three-point contact with a doubly infinite pencil of curves ; and in the same journal (vol. x. p. 221) H. Krey, of Kiel, has applied a method, similar to that of Brill, to the next step in the problem proposed in Mr. Spottiswoode's paper. He does not, however, appear to have succeeded in completely eliminating the differentials which occur in the process ; and in that respect his solution is incomplete. Some formulæ used in Mr. Spottiswoode's paper on the contact of curves and surfaces, and in particular in that on the sextatic points of a plain curve (*Phil. Trans.*, 1865, p. 657), prove to be directly applicable to the question. An application of them to Brill's problem will be found in a paper in the *Comptes Rendus* (1876).

Astronomical Society, November 10.—Mr. Huggins, president, in the chair.—The Astronomer-Royal gave a short account of the proceedings of the Royal Observatory during the recess, describing the lunar and physical observations which had been assiduously prosecuted and the state of the calculations for his new lunar theory.—A paper by Prof. Langley, of the Allegany Observatory, Pennsylvania, on the measurements of the direct effects of sun-spots on terrestrial climates was read. Prof. Langley has made experiments to determine the difference in the amount of heat radiated from the centre of a sun-spot and from an equal area of penumbra and photosphere. Combining these results with the amount of the sun-spot area given as existing during a period of maximum of sun-spot frequency in the tables of Messrs. De la Rue, Stewart, and Lœwy, he calculated that the mean terrestrial temperature due to solar radiation at a period of sun-spot minimum would be something between three-tenths and one-twentieth of 1° C. greater than at a period of sun-spot maximum. The Astronomer-Royal pointed out that the observations of underground temperature made at the observatories at Paris, Edinburgh, and Greenwich showed differences in the mean annual temperature of the surface soil which amounted to as much as 6° F. An examination of the temperatures at different depths showed that the differences of surface temperature had their cause in something external to the earth, but he had not found that the differences of mean surface temperature coincided with the variations in the amount of the English serial crop as given by the Board of Trade returns or with the periods of sun-spots maxima. Mr. De la Rue said that it did not follow that the

amount of solar radiation would necessarily vary inversely as the sun-spot area, for at a period of maximum sun-spot area it was possible that the radiation from the photosphere might be increased to such an extent as wholly to counteract the difference caused by the decrease in the apparent area of the photosphere. He further remarked that the numbers given in his papers in conjunction with Messrs. Balfour Stewart and Lœwy must not now be relied upon, as some serious errors had been discovered which he was now endeavouring to put straight by a re-investigation of the whole subject.—Mr. Penrose read a paper entitled "An Endeavour to simplify the Method of making the Correction for the Spheroidal Figure of the Earth in Lunar Observations, and particularly with Reference to its Effect upon the Lunar Distance."—Mr. Christie described some observations which he had made with a polarising photometer upon the relative brightness of different parts of the disc of Venus. He had found that when the disc of Venus was gibbous, the last part of the disc to disappear, as its brightness was decreased by rotating his photometer, was a sausage-shaped patch, the convex edge of which was found to be distinctly within the limb of the planet. He thought that his observations supported Mr. Brett's theory as to specula reflection from the surface of Venus.

Linnean Society, November 2.—Prof. Allman, president, in the chair.—In exhibiting a live specimen of the Norwegian Lemming, the survivor of seven at starting, Mr. Duppa Crotch called attention to charts he had made showing the nature of the ground traversed in two instances in which he himself had witnessed the westerly migration of this singular little rodent.—Mr. G. Bentham, vice-president, read a paper on the distribution of the Monocotyledonous order into primary groups, more especially in reference to the Australian flora, with notes on some points of terminology.—Dr. Francis Day drew attention to examinations he had made on some Irish sticklebacks (*Gasterostei*). These had led him to doubt the conclusions arrived by M. Sauvage (*Nouv. Archiv. d. Mus.*, 1874), as to the propriety of dividing the family into subgenera and some seventeen species. Dr. Day has noticed such abnormal variations in the presence and absence of ventral fin and spines in specimens of the three-spined and ten-spined sticklebacks as cause him to believe these appendages to be a very imperfect diagnostic and specific character. Nay more, as certain other Acanthopterygians have been generically divided by such features, it is questionable whether further observations may lead to considerable necessary revision of the families. He is of opinion, moreover, that the spinal armature of at least the *Gasterostei* has an increment in the ratio of their proximity and access to a maritime habitat.—Mr. H. W. Bates communicated a paper by Mr. D. Sharp on the respiratory function of the Carnivorous Water Beetles (*Dytiscidæ*). Experiments made by the author on numerous species show that there are wide differences in the length of time they spend submerged and on the surface for breathing exposure. For example, the *Pelobius Hermanni* remains under water in a ratio of 375 to 1 of air exposure; whereas *Dytiscus marginalis*, a more highly developed form, has a corresponding ratio of about 12 to 1. Most specimens of the group are more active by night than by day. The *P. Hermanni* and *Hydrovatus clypealis* he regards as much less developed, and adapted for moving through the water than our indigenous water-beetles; and therefore, along with the American Amphizoa, appear to him to represent the most rudimentary and primitive of existing forms of the Dytiscidæ.—Prof. Dickie gave a supplemental notice of Marine Algæ obtained in the *Challenger* Expedition. Of some fifty species one only is new.—A description of *Thaumantis pseudaliris* and *Amesia pexifascia*, two new Lepidopterous forms from Malacca, by Mr. A. Butler, was taken as read.—The same author also had a communication on the genus *Euptychia*, a revision, with the addition of twelve new species being made; a case of these butterflies was exhibited in illustration of his paper.—A second communication, by Mr. D. Sharp, referred to new species of beetles (*Scarabæidæ*) from Central America; these had been captured by Mr. Belt, chiefly in the neighbourhood of Chontales.—Mr. A. Peckover exhibited and made a few remarks on two skins of the young of the Madagascar insectivore, *Hemicentetes nigriceps*, Günth., and on a series of insects from the same island, collected by Mr. A. Kingdon, near Antananarivo.—Mr. E. D. Crespigny showed a specimen of the Umbelliferous plant, *Tordylium maximum*, L., obtained near Tilbury Fort, a locality where it had disappeared for a considerable length of time.

Chemical Society, November 16.—Prof. Abel, F.R.S., president, in the chair.—A paper on barwood, by the late Prof. Anderson, was read by the Secretary, describing the method of preparing *baphuin* from it, and also some of the educts obtained by the action of various reagents.—The second communication was on the alkaloids of the aconites, Part I. on the crystallisable alkaloids contained in *Aconitum napellus*, by Dr. C. R. A. Wright. The author finds that the alkaloids from *A. ferox*, which he calls *pseudanicotine*, $C_{33}H_{49}NO_{11}$, differs both in properties and in composition from *aconitine*, $C_{33}H_{43}NO_{12}$, the crystalline alkaloid of *A. napella*. In one instance, however, he obtained from the root of the latter a perfectly distinct bitter crystalline alkaloid, *picroconinute*, possessing scarcely any toxic power; whether this is an alteration product of aconitine or not remains at present undetermined.—Mr. G. S. Johnson then read a paper on potassium triiodide, a crystalline compound obtained on saturating a saturated solution of potassic iodide with iodine, and slowly evaporating the solution over sulphuric acid. It forms prismatic or tabular crystals having an appearance very similar to that of iodine.—The last communication was by Mr. T. S. D. Humpidge, on the coal-gas of the metropolis. He has carefully analysed and determined the illuminating power of different samples, and comes to the conclusion that the gas at present supplied is but little if any better than it was twenty-five years ago, the actual increase in illuminating power being due to the use of improved burners.

Physical Society, Nov. 4.—Prof. G. C. Foster, president, in the chair.—The following candidates were elected members of the society:—Warren de La Rue, D.C.L., F.R.S., and W. H. Preece.—Dr. Guthrie read two letters which he had received from Dr. Forel, in continuation of a communication which he made to the Society on May 27 last, in reference to the "Seiches" or periodic oscillations which take place in the Swiss lakes, and on which he has recently made an elaborate series of observations. Since his communication he has found in a pamphlet by Dr. J. R. Mérian, published in 1828, a formula which is strictly applicable to the phenomena under consideration. If t be the duration of half an oscillation, h the depth of the lake, and l its length, $t = \sqrt{\frac{\pi l}{g}} \left\{ \frac{e^{\frac{\pi h}{l}} + e^{-\frac{\pi h}{l}}}{e^{\frac{\pi h}{l}} - e^{-\frac{\pi h}{l}}} \right\}^{\frac{1}{2}}$. Considering that probably this formula will be applicable to lakes of irregular depth if h be the mean depth, he has applied it to several lakes, and the following are some of his results. In the case of transverse seiches on Lake Leman, the formula gives 216 metres as a mean depth, and 334 metres is the greatest known depth. With a longitudinal oscillation, the mean depth is found to be 130 metres. In the case of Lake Wallenstadt, the formula having shown the mean depth to be somewhat greater than the generally accepted greatest depth, Prof. Forel took a number of fresh soundings, and found a great basin of comparatively even bottom and of such a depth as to render probable the mean depth given by the formula.—Mr. O. J. Lodge suggested that the formula would be rendered more simple by using the hyperbolic function. It would then become $t = \pi \sqrt{\frac{l}{g}} \operatorname{Coth} \frac{1}{2} \frac{\pi h}{l}$. Mr. Lodge also indicated the curve which this equation represents.—Dr. Stone exhibited some diffraction gratings on glass and metal, ruled for him by Mr. W. Clark, of Windsor Terrace, Lower Norwood. The majority of them were close spirals about 1,000 to the inch, which, when held between the eye and a distant lime-light, exhibited circular spectra of great brilliancy. The slight difference between the spiral and true circles appeared to exercise no appreciable effect on the result. The metal gratings were of linear form, 1,000 lines to the inch, intended for use by reflection in a spectroscope. The spectra thus obtained were of much greater brilliancy than those ordinarily obtained by refraction, and presented obvious advantages for examining the ultra-violet rays. He explained the mechanical difficulties which had been surmounted in their manufacture together with the manner in which the diamond cutters are prepared. The metals hitherto employed, namely, cast-steel and German silver, are objectionable, and Dr. Stone proposes, on the suggestion of Prof. McLeod, to employ speculum metal, and will report the result of the experiments more fully at a subsequent meeting.—Dr. Guthrie then briefly described some experiments which he has made to determine the effect of a crystalloid on a colloid when in the presence of water. Mr. Graham, in his classical researches, made numerous experiments with a salt on one side of

a colloid membrane and water on the other, and Dr. Guthrie thought it might be well to determine what action, if any, takes place when a salt is added to a solution of a colloid such as size. Two or three lumps of rock salt were added to a jelly of size, and the whole hermetically sealed in a glass tube. The colloid parted with its water readily, a saturated solution of the salt was obtained, and the size became perfectly white and opaque, having undergone a structural change. Experiments were also made employing a more hygrometric salt, such as chloride of calcium.—Mr. W. C. Roberts pointed out that a jelly containing 5 per cent. of silicic acid readily parts with water to sulphuric acid, and dries into a hard glass like hydrate of silica. He asked whether this might be considered as analogous to the action of salt on size, or whether the strong affinity between the acid and water removed it to another class of action. Dr. Guthrie thought it might be possible to establish the existence of a point at which the jelly did not give up its water to the hygrometric substance. He also pointed out the analogy between a jelly and a mass of small bags filled with liquid.

Entomological Society, November 1.—Prof. Westwood, president, in the chair.—Mr. F. Smith exhibited some remarkable specimens of thorns from Natal and Brazil, which had been taken possession of by certain species of *Cryptoceridæ* for the construction of their nests. Some of the thorns were as much as 3 inches in length.—Prof. Westwood mentioned an instance of the hairs of a larva of *Lasiocampa rubi* having caused considerable irritation of the skin, and that the irritation was complained of by his correspondent for a week afterwards.—The Professor exhibited a singular Coleopterous larva from Zanzibar, of a flattened, ovate form and a steel-blue colour, with two points at the extremity of the body, and with long, clavate antennæ. The head bore some resemblance to that of the dipterous genus *Diopsis*. He also exhibited a specimen of the butterfly, *Hesperia sylvanus*, received from the Rev. Mr. Higgins, of Liverpool, having the pollinaria, apparently of an Orchid, attached to the base of the tongue. Also an Orchid bulb purchased by Mr. Hewitson with a collection of roots from Ecuador, which was found to contain nine living specimens of cockroaches, comprising six different species, viz., *Blatta orientalis*, *Americana*, *cinerea*, *Maderæ*, and two others unknown to him, some being of considerable size.—Mr. Dunning read a "Note on *Acentropus*," in which he remarked on Heer Ritsema's Second Supplement to his Historical Review of the genus, published in the *Transactions* of the Entomological Society of the Netherlands, in which that author tried to prove that two distinct species existed, of which one (*A. niveus*, Oliv. = *A. Garnonsii*, Curt.) has a female with rudimentary wings, and the other (*A. latipennis*, Möschl. = *Zancle Hansoni*, Ste.), has a female with normally developed wings; whereas, Mr. Dunning argued that the facts, as stated by Heer Ritsema, did not in any way prove the duality, but were quite consistent with the unity of the species.

Institution of Civil Engineers, November 14.—Mr. George Robert Stephenson, president, in the chair.—The paper read was on the Japan lights, by Mr. R. H. Brunton.

PARIS

Academy of Sciences, November 13.—Vice-Admiral Paris in the chair.—The following papers were read:—Theorems relative to systems of three segments making a constant length, by M. Chasles.—Note on the recent progress of phylloxera in the departments of the two Charentes, by M. Bouilland.—Continuation of observations of eclipses of Jupiter's satellites at the Observatory of Toulouse, by M. Tisserand. For eclipses of the first satellite little seemed to be gained by using larger instruments; for those of the second and third the difference was greater.—M. Milne-Edwards presented the first part of tome xii. of his work on comparative physiology and anatomy of men and animals. It treats of audition and sight.—Report on a memoir of M. Fouqué, "Mineralogical and geological researches on the lavas and dykes of Thera" (island of Santorin group). These lavas contain two, and often three triclinic felspars (some say volcanic rocks never contain more than one); albite predominates among the small crystals; labradorite or anorthite among the large. These anorthite lavas (hitherto thought exceptional) form forty-one of the dykes of Thera. M. Fouqué shows, from experiments, that a lava fused and suddenly cooled is quite as crystalline as when it has solidified slowly; crystals are formed before ejection from the ground. Contrary to M. Tschermak, who would eliminate from the catalogue of mineralogical species all triclinic felspars except albite and anorthite, M. Fouqué shows reason for retaining oligoclase and labradorite. Tridymite, a variety of crystallised silicon, is found in the lavas in form of thin hexagonal imbricated plates; M. Fouqué regards it as a posterior formation to the other elements, and as having arisen at a high temperature under the influence of imprisoned droplets of water, when the surrounding rock was liquid or viscous. The report speaks highly of the value of this memoir.—Researches on the brachistochrome of a heavy body, with regard to passive resistances, by M. Haton de la Goupillière.—On the characteristics of systems of conics and surfaces of the second order, by M. Halphen.—M. François recommended, against phylloxera, the vitriolic water from the mines of pyrites of Sainbel.—Observations relative to the general theory of trombes, by M. Virlet d'Aoust. He describes some dust whirlwinds observed on the Mexican plateaux.—Determination, by the method of analytic correspondence, of the envelope-surface of a surface whose equation contains n parametres connected together only by $n-2$ relations, by M. Saltel.—Influence of temperature on magnetisation, by M. Gaugain. The value of the temporary variation varies considerably from one bar to another. To determine the influence of temperature this should be kept invariable throughout an experiment; the author describes how he accomplished this. With a bar susceptible of considerable temporary variation, the magnetism developed at 300° is weaker than at ordinary temperature, but in the opposite case it is stronger.—On the hydrates of sulphate of copper, by M. Magnier de la Source.—On margaric chloride and its derivatives, by M. Villiers.—Researches on quercite, by M. Prunier. He considers quercite to form a transition between the fatty series and the aromatic series.—On angelic acid, by M. Demarçay. He verifies his former experimental results against some contradiction of them by M. Fittig in the Berlin Chemical Society.—Physiological experiments on the functions of the nervous system of Echinida, by M. Fredericq. The cords described as the nervous system are the means by which harmony of movements is established. Facts seem also to favour the existence of a nervous plexus situated in the thickness of the external tegument.—On the mobile state of *Podophrya fixa*, by M. Maupas. This, he says, hardly merits its name; it is more mobile and vagabond than known Acinetinians, and is an intermediate type between suctorial infusoria and ciliated infusoria, properly so called. He describes in detail the changes which take place in it during its mobile period.—On the existence of asparagine in sweet almonds, by M. Portes.—On the influence of leaves and floral branches on the nature and quantity of sugar contained in the scape of agave, by M. Balland. Both leaves and flowers have an incontestable *rôle* in the formation of sugar.—On a meteoric iron very rich in nickel, found in the province of Santa Catharina (Brazil), by MM. Guignet and Ozorio de Almeida; iron 64 per cent., nickel 36. It appears to belong to the terrestrial rocks. M. Daubrée remarked that a careful examination of all that region was very desirable.—Chemical composition of the water of the Bay of Rio de Janeiro, by MM. Guignet and Teller. It contains considerable quantities of silica and alumina (9·5 and 7·5 gr. respectively, per cubic metre). This is from decomposition of the gneiss and granite rocks under friction of the water.

CONTENTS

ERRATUM.—P. 57, col. i. line 12 from bottom, for "Tides" read "Fish."