THURSDAY, DECEMBER 20, 1877

THE "INFLEXIBLE"

ON July 12 last we explained the *Inflexible* case at some length to our readers on the ground that there might be seen in it the beginning of a system which not only involved the safety of the four large and costly ships then commenced or contemplated, but which "having received countenance and sanction in the highest quarters in this country, may not improbably become extended over the navies of the world." A week later we considered the Parliamentary Papers on the subject, and came to the conclusion that the *Inflexible* was not a safe ship for battle, and that the objections brought against her had been much too lightly treated. Now that the Report of the Admiralty Committee is before us we are able—notwithstanding much that appears in it—to point our readers to it as a full and complete justification of the course we and others then pursued, for that Report concludes with these words :—"We therefore desire to bring under the very serious consideration of their Lordships the necessity, before proceeding with the construction of more vessels of the type of the *Inflexible*, of thoroughly investigating whether by more beam their safety may not be largely increased without impairing their speed and efficiency." As this appeal "to the very serious consideration of their Lordship's" in arrest of the construction of other ships of the type closely follows a paragraph in which the Committee show the very great advantages of an alteration in the form and proportions of the *Inflexible's* citadel (without increase of armour), it is not conceivable that the Board of Admiralty will proceed with the other vessels of the type, and it is absolutely certain that no more ships possessing the defects of which we complained in July will be laid down. The great object which we set before ourselves, therefore, is already accomplished, and the extension of a dangerous system of design throughout our own navy, and the navies of the world, has been effectually arrested. As we know that the case of the *Ajax* and *Agamemnon* was actually before the Committee, and as their Report makes no exception of them in their appeal to the Admiralty to stop further proceedings, it is to be inferred, we presume, that the beam of these two ships will have to be increased in accordance with the Committee's views. With these results before them all those who took part in bringing about this inquiry may, we think, be congratulated on the success and value of their labours.

There only remains the *Inflexible* herself to consider in the light of the Committee's Report; and in discussing this part of the subject we must not forget that no inconsiderable portion of the report, and especially the aspect which its opening pages presents, has been greatly influenced by the form of the Admiralty reference. We take leave to say that the first of the questions put by the Admiralty to the Committee has little or nothing to do with the subject. We do not remember that even Mr. Reed, who has most strongly condemned the *Inflexible's* design ever contended that "the blowing out of *the whole* of the stores and cork by shell-fire" would occur very early in an engagement; and if he had, the elicitation of a contrary opinion from the Committee would have no serious bearing upon the subject, simply because experiment, and experiment alone, can determine the degree and rapidity of the injury to which thin iron chambers filled with cork are liable. Mere opinions, in the absence of experiments, are comparatively valueless in such a case. But what we should have thought was absolutely self-evident, even without any experiment, is that shell-fire from modern ordnance would certainly blow cork packing out of thin iron chambers at some rate or other; and yet, strange to say, this is what the Committee appear to doubt, and even to deny, for they say that in their opinion that which may "be fairly assumed to represent the greatest amount of damage the ship would be likely to suffer in any action" is the condition in which, although the unprotected ends are completely riddled and waterlogged, the cork and stores remain in place, and add to the buoyancy. It is fair to assume, then, according to the Committee, that in no naval action will the cork be blown out of place by shell-fire, and this in face of the fact that when an experiment was actually tried at Portsmouth the contrary result was experienced. It is of the utmost importance to note carefully that it is only by making the above extraordinary assumption—an assumption which we believe will not meet with the concurrence of scientific artillerists and seamen—that the Committee are able to oppose in any degree the opinions of the ship's danger which we and others expressed in the autumn. It is on this assumption that they rest their opinion that "the unprotected ends are as well able as the armoured citadel to bear the part assigned to them," and that therefore "a just balance has been maintained in the design." It unfortunately is made perfectly clear afterwards by the Committee that the "part assigned to them" is to hold the citadel and the rest of the ship upright, and it is clear that they cannot be presumed fit to perform this part if shell-fire can blow out the cork This is the weak point—we venture to think the dangerously and even fatally weak point—of the Committee's Report, and one which the common sense and observation of men will prevent them from assenting to, and consequently the *Inflexible's* safety is so ill-assured that we doubt if responsible persons will sanction the completion of such a ship.

The committee have fallen, as it appears to us, into a grave inconsistency, likewise, as regards the *Inflexible* herself. They say, as we have seen, that the unprotected ends are, as designed, well able to perform their part, and well balanced with the citadel. In subsequent passages, nevertheless, they go on to disclose and assert even more serious defects in them than any of us adverse critics of the ship have alleged, and to recommend an enormous extension of the cork chambers. What is the meaning of a scientific committee dealing with an extremely grave public question in this way? Either the unarmoured ends are well designed at present, or they are not; if they are, why alter them to the very large extent—far larger than a cursory perusal of the report might lead readers to expect, for the increase of cork chambers recommended is enormous? If they are not, why has the contrary statement been made and circulated? The truth is they have not been satisfactorily designed, as we shall presently prove from the Committee's own report. But first let us

dispose of a long argument which the Committee enters upon and pursues with the object of proving that by lengthening the citadel you would thin the armour upon it, and thus reduce its defensive power. Assuredly you would: nor can any one doubt for a single moment that it would be far better to reduce the armour a little for the purpose of making the citadel stable enough to hold the ship upright in spite of any injury to the unprotected ends, rather than to keep the present thickness, and to reduce its length sufficiently to cause the whole to capsize when the unarmoured ends only are badly damaged. We know how naval officers answer this question. But, in truth, the whole argument of the Committee on this point is beside the mark, and a mere beating of the air, for no one that we know of has urged the change which the Committee take so much pains to discuss. What we have always understood Mr. Reed to allege, and certainly what we have in NATURE maintained is, that in the *Inflexible* the citadel and unarmoured ends were neither well-formed, well-proportioned, nor well-balanced against each other, and that a ship of her type should have embraced a larger area of flotation within the citadel and a less area within the unarmoured ends. And this is precisely what the Committee themselves declare, and thus refute their own assertion that the ship is properly designed at present. Near the end of the Report they say:—

"Results which have been obtained in the course of the experiments at Torquay on the resistance of ships, show that a considerable increase of the extreme breadth of the *Inflexible*, if accompanied by a corresponding fining of the ends so as to keep the displacement unaltered, would, if anything, diminish the resistance of the intact vessel to propulsion at full speed. Supposing the ship thus increased in beam 10 feet, and the citadel shortened so as to retain the same perimeter and thickness of armour, her transverse stability would then be about doubled in the *e* and *f*[1] conditions, and in the riddled and gutted condition, would be more than it now is in condition *e* or *f*. Her longitudinal stability in the riddled and gutted condition would be reduced 10 per cent. (*t*, Appendix No. 15), but would not be diminished in condition *e*, and scarcely appreciably so in *f*. The increase of beam would also add to the area of the citadel in a horizontal plane, and thus increase the buoyancy in the riddled condition."

When the Committee, who lay down these clear and cogent proofs that the *Inflexible* is vastly inferior, in respect of stability and safety, to what she might have been, also tell us that "a just balance has been maintained in the design" of that ship, and that "a good result has been obtained," we find great difficulty in reconciling their statements, and feel strongly that if the public were to trust only to the language of the Report they might readily be led to draw extremely wrong inferences. We much fear that the gentlemen composing the Committee have thought too much of the Admiralty, with which they are all more or less connected, and too little of the public, who have been waiting for many months for their verdict. That verdict has been pronounced in a manner which, speaking on behalf of scientific men, we lament. It is inconsistent, and, so far as we can understand it, contradictory, in its several parts, and is in large part likely to beget in certain quarters a fatal confidence in a ship the defects and dangers of which the Committee evidently well understand. So uncertain and indefinite is it that it does not make it unquestionably clear even that the *Ajax* and *Agamemnon* are included in their desire to have progress arrested, for although after speaking of the *Inflexible* only they ask that no more vessels of the type may be proceeded with, and thus employ terms which cannot well be otherwise interpreted; the absence of all mention of their names nevertheless leaves room for the suggestion of a doubt on the point. It was clearly due to all concerned that their views on so weighty a matter should have been placed beyond all hesitation and question.

But those who would understand the full significance of this Report must not be deterred from perusing it carefully through, for if after reading thirteen out of its sixteen pages they were to throw it aside they would have derived from it, we say without hesitation, not only a very insufficient but a very untrue conception of the *Inflexible's* actual condition. Up to that point both a hasty and a deliberate perusal of it yields, to our minds at least, the impression that the Committee are admirers of the existing ship in almost every particular. But the disclosures which the scientific conscience of the Committee demanded and enforced commence on p. 14, and thence to the end facts of an appalling nature respecting her are gradually unfolded with so much effect that even the Committee themselves end by imploring the Admiralty not to repeat such a design! Let us briefly observe what these disclosures are.

The first relates to the inclining force which the action of the rudder exerts upon a ship of small stability. The Committee made experiments with the *Thunderer* expressly to acquire facts illustrative of the *Inflexible's* case, and the conclusion at which they arrived is thus stated:—"The *Inflexible* riddled and gutted,[1] and without water ballast, going at 7·24 knots, and turning in the circle of 1,210 feet in diameter, would require a righting lever or G Z of ·13 feet, and as the value of G Z at her maximum stability in this condition is only ·12 feet, *she would on this supposition overset.*" To soften down this alarming fact the Committee add: "It is, however, not to be expected that the ship under this condition could be driven at this speed"—a speed of 7¼ knots only round the circle, corresponding to only eleven knots in the *Thunderer* when steaming on a straight course! And this the reader will bear in mind is true of the *Inflexible*, not when her armour has been pierced by huge shells, or her bottom knocked about by rams and torpedoes, but when nothing but her exposed unarmoured ends have been badly injured. Her armour and her bottom may be perfectly intact, ay, untouched, and yet her own rudder would capsize her in steaming at a low speed. No statement ever made about the *Inflexible* by those who condemn her has gone or ever could go much beyond this. And what can be thought of the figures given? The line G Z is the lever or arm, at the ends of which the gravity and buoyancy of the ship act in opposite directions. The length required for withstanding the rudder's action under

[1] These references *e* and *f*, are to the Parliamentary Papers, and represent the ship with the ends riddled and water-logged, *e*, showing the coal as well as the cork, &c., in place, and *f* with the cork, &c., in, but not the coal.

[1] This phrase, "The *Inflexible* riddled and gutted" is (improperly) employed by the Committee when they speak of the unarmoured ends being riddled and having the cork blown out.

the given conditions was '13 of a foot (1½ inches only [!]), but even that is more than has been allowed in the design of this ship (viz. '12) in which the Committee say "a just balance" has been preserved.[1] And this inability of the ship to withstand her own rudder's action, and that at a low speed, even with virgin armour and a bottom untouched by ram or torpedo, having been asserted and urged by others, elicits no remonstrance or objection whatever from the Committee. And yet, when a little later on in their Report they have disclosed a somewhat similar degree of longitudinal instability—discovered, as they suppose by themselves, but already well-known and obvious, doubtless, to others—and have shown that the ship would not be safe at seven and nine knots speeds because of her tendency to capsize lengthwise (so to speak), and so more or less down headforemost, then the Committee see grave danger showing itself, for they say, "We consider that any large limitation in the ship's speed may expose her dangerously to the attack of ram or torpedo," and in the summary they incidentally tell us, in the mildest terms, that a blow from either would be fatal; "the small residuum of stability she would possess would not avail to render such an attack other than fatal." The only difference in the two cases is (and this is possibly the reason why the Committee lay the greater stress on this case) that it is not here necessary to suppose the cork or stores blown out, for a single shot or shell making a large wound near the stem, bulging a skin plate outwards, and completely rupturing the internal bulkheads, would so destroy the longitudinal stability of this large ironclad, costing more than half-a-million sterling, that even at seven knots' speed she would run her bows under; "and again," we are told by the Committee, "her speed is similarly limited to nine knots by wounds of a much less critical character in other parts of her sides." We have said that even this danger was doubtless foreseen by others—as it certainly was by ourselves, whether mentioned or not—before the Committee's Report appeared; but the Committee certainly have carried the subject a step forward by the experiments they have made with the model, and by their positive declaration that "on the whole the effect of sea-waves must be to aggravate, and in some circumstances greatly to aggravate," this very serious and certain source of danger. In a word, the very Committee who have in another place asserted that in the *Inflexible* the balance is fairly maintained between the armoured and unarmoured ends, have elsewhere in their Report shown that that balance is so ill maintained between the two, that with all the cork in place one or two shot and shell penetrating the unarmoured parts would so reduce her stability that she could not be steamed ahead with any reasonable speed, but would of necessity become a prey to any ram or torpedo craft that might evade or disregard her guns!

Another disclosure of the Committee is that the mere running out of the guns "would become a serious element of danger as the ship approaches the riddled and gutted condition." Here again they employ the phrase to which we have already intimated an objection in a foot-note, and speak of "the ship" approaching "the riddled and gutted condition." They mean nothing of the kind; by "the ship" they mean the exposed unarmoured ends only; and here again it is to be observed that the danger disclosed is not one contingent upon the blowing out of all the cork, &c., but arises before, when the unarmoured part only *approaches* that condition. That the danger is, again in this case as in the previous one, a very serious and practical one is shown by the Committee recommending an alteration in the gunnery arrangements, proposing that the travel of the gun on the slides should be restricted, lest by running the guns out to the full extent at present arranged for, they should capsize the ship.

We have now sketched, sufficiently for our present purpose, the substance of the Committee's Report. We may end this article as we ended that of July 19, and repeat: "The conclusions we have arrived at are, that the *Inflexible* is not a safe ship for battle in her present state, that the objections brought against her have been much too lightly treated, and that the disclosure of her condition, with the circumstances that have followed it, have excited just surprise and dissatisfaction." That surprise and dissatisfaction will be greater than ever when the Committee's Report has had time to produce its full effects, both intended and unintended.

[1] The Committee, at the bottom of page 15, give us the means of readily illustrating the amount of stability which the *Inflexible* has in the case above considered, for they tell us that 60 tons in the bottom of the ship, which herself weighs over 11,000 tons, would alter the length of G Z to '12 of a foot.

HYDROPHOBIA [1]

II.

WE do not intend to do more than allude in a cursory manner to the prophylactic treatment of hydrophobia, *i.e.* to the treatment adopted to prevent the occurrence of the disease in those who have been bitten by mad dogs. The general experience of the past sanctions, as might be expected, the practice of attempting to prevent the absorption of the poison of rabies by excising or destroying by caustics the wounds inflicted by rabid animals; of the innumerable internal remedies which have been proposed and made use of with the object of preventing the development of hydrophobia in those bitten by rabid dogs, it may be said with justice that nothing whatever is known which warrants the assertions of their advocates. This is indeed a case in which the fallacies which beset all therapeutical inquiries, especially when attempted by ignorant persons and fanatics, are specially liable to obscure the truth. Of all dogs supposed to be rabid, only an infinitesimal proportion really are so, and it is but rarely that the fact of a dog being rabid is tested by having it watched until it dies, or by the unfortunate fact that some of those bitten perish by hydrophobia; then, of all persons bitten by certainly rabid dogs, only a small proportion become affected with hydrophobia, even when no treatment is adopted, so that the value of any drug or remedial measure as a *prophylactic* could only be tested by an experience such as no one ever has had.

Less uncertainty prevails in reference to the effects of treatment when hydrophobia has been developed. According to the best observers this disease has hitherto been invariably fatal. There are, it is true, a few cases—and of these two have been recorded within a comparatively recent period—in which a cure is said to have been effected, but when examined with care the gravest doubts

[1] Continued from p. 119.

of their real nature must be entertained. Here again there are fallacies to be encountered of no insignificant nature. The continued and terrible anxiety lest a bite which has been inflicted should be followed by its terrible consequences has led, in the case of persons of a susceptible and unstable nervous system—as all other long-continued worries and anxieties are liable to do—to the development of hysteria and insanity and, in the attacks of acute mania which have occurred in such individuals, many of the phenomena of hydrophobia have been imitated. An admirably-narrated case of this "hydrophobie non rabique" of French writers was lately placed on record by Mr. Hugh Norris, of South Petherton ("Hydrophobia or its Eikon—which?"—*Lancet*, September 22, 1877) :—"A gentleman underwent terrible anxiety on account of his young son having been bitten by a dog which suddenly had become ill and strange in its behaviour. The danger which threatened the boy caused the father to become intensely emotional, and prolonged sleeplessness ensued. Other worries superadded themselves to this one great gnawing anxiety, and the poor man fell ill; at first there appeared symptoms which simulated very closely the hydrophobic phenomena, but these were succeeded by a genuine attack of acute mania, which necessitated the patient's removal to an asylum, where he died on the seventh day of his illness." In this case there was no evidence that the patient was actually bitten by a dog, his statement that he had been so injured having been made, in a peculiarly suspicious manner, only after his illness had commenced, and being apparently but one of the delusions which afterwards haunted him. Had this man been certainly bitten, and had his illness been followed, as it might have been, by recovery, his case would in all probability have been quoted as one of the few instances of recovery from hydrophobia, though the impartial critic would have pointed out some anomalies which rendered the conclusion doubtful. The knowledge of the undoubted occurrence of such cases necessarily imposes great caution in the examination of alleged instances of recovery from hydrophobia.

The drug to which attention has of late been directed for the treatment of hydrophobia is curare or woorara, a substance used by the natives of South America as an arrow poison. This poison has been known since the end of the sixteenth century, when Sir Walter Raleigh made the conquest of Guinea; but attention was drawn to it in a special manner by the celebrated traveller, Waterton, who first made experiments on animals with it, which were continued by Sir Benjamin Brodie and Dr. Sibson, and were succeeded by the now celebrated researches of Claude Bernard, Kölliker, and other eminent physiologists. Curare, the active principle of which is derived from a strychniaceous plant, when directly introduced into the blood or injected under the skin, produces paralysis of all voluntary movements; this paralysis depends upon its exerting a special action upon the terminations of motor nerves in the muscles, especially in voluntary muscles. The poison leaves intact the sensory nerves of the body, and at least in the early stages of its action, the nerve-centres. An animal subjected to its influence becomes absolutely motionless, and dies rapidly of suffocation from paralysis of the respiratory muscles; if, however, respiration be kept up by artificial means, life may be prolonged for long periods, and, the poison becoming eliminated, recovery may ensue. As long, however, as the stage of complete paralysis continues, the creature is entirely unable to communicate with the external world. There is no proof that external stimuli do not affect it; that it does not feel—but the channels by which the evidence of sensibility reach us are for the time interrupted. Curare has been suggested as a remedy for many diseases of a spasmodic character, but a great obstacle to its use is presented by the danger which attends its employment. A dose which will be sufficient to arrest an abnormal spasmodic contraction of a muscle or group of muscles, will presumably cause a stoppage of respiratory movements, and the medical man, if haply he be near the patient, will find himself compelled to keep up artificial respiration—no easy task to accomplish, especially with the means which the physician, as distinguished from the physiologist, could employ. Hitherto the attempts to use curare have been few, and the results (if we except the two supposed cases of cure of hydrophobia) we think worthless. Curare is indeed a drug the employment of which must be attended with so great a risk that a very strong case should be made out in its favour before patients are exposed to it.

Knowing as we do the physiological action of curare, we may ask whether it is a drug at all likely to be serviceable in spasmodic affections generally, and specially in hydrophobia. The majority of spasmodic diseases are due to a central cause, or to a cause acting through a preternaturally excitable centre. Any drug which will cut off—as curare does—the communication between the nerve-centre and the muscle will prevent its spasm, and will of course obviate any evil results which follow directly from the spasm; but, necessarily, under these circumstances the abolition of the spasm is no index that any change has been effected in the morbid state to which it owed its existence. In hydrophobia there occur, it is true, as prominent phenomena, spasms of the muscles concerned in inspiration and in deglutition. The administration of curare in doses sufficient to stop the respiratory movements would doubtless prevent these spasms, though we must not forget that it would do so at no mean risk. But are these spasms the proximate or remote cause of death in hydrophobia? We believe not. They are spasms which, as we tried to show, are probably dependent upon a morbid state of the medulla oblongata, with which is connected a morbidly heightened reflex excitability of that portion of the nervous system. But there is no evidence that curare would exert any influence in diminishing this abnormal excitability.

From our knowledge of the physiological action of curare we should not then be inclined to believe that it could affect in a beneficial manner the progress of the essential morbid processes of hydrophobia; it could but modify some of the painful phenomena which belong to it. Actually curare has been tried in several cases, but only twice has its administration said to have been attended with success, and these two cases cannot be accepted as having been certainly cases of hydrophobia. We have seen one case of hydrophobia treated with curare without any influence having been exercised by it. There are drugs, however, which the physician is habitu-

ally employing that possess actions which would cause one to predict that they would be of special use in controlling the terrible spasms of hydrophobia, *e.g.*, bromide of potassium, chloral, and Calabar bean. These drugs all diminish in a signal manner the reflex excitability of the nerve-centres ; and the second in the list possesses in addition powerful properties as a hypnotic ; they have all been used in the treatment of hydrophobia ; and one of them—chloral—has, in sufficiently large doses, been successfully used, in so far as relief of suffering was concerned. But as for a *cure* for hydrophobia it has yet to be discovered ; and this remark applies to all zymotic diseases. The majority of these diseases, unlike hydrophobia, tend naturally towards recovery rather than death, and the physician is undoubtedly able, by judicious measures, to obviate the tendency to death. He cannot *cure* the disease, however, in the sense in which he might be said to cure it were he able to destroy the poison which is its cause ; apparently, once introduced into the system the poison must produce its effects—intense or slight—which must have a certain course, and then cease, because the poison which induced them has passed away, or because the soil which nourished the poison supplies it no longer with the conditions which it required. But the day may come, and we believe will come, when even this great result may be achieved ; when not only shall we know the conditions which attend the spread of zymotic diseases so accurately that we shall be able to prevent their spread, but when medicine may supply us the means of dealing directly with the *materies morbi* of the diseases, as, for instance, by "sterilising" the soil in which they are implanted.

Our general review of the main facts in the history of rabies and hydrophobia has naturally brought out with considerable clearness how little is known concerning it, and how much remains to be done. We hail, therefore, with satisfaction the appointment of a Committee of the British Medical Association charged with the investigation of this important and interesting subject. This Committee consists of Dr. Lauder Brunton, Mr. Callender, Mr. Ernest Hart, and Prof. Burdon-Sanderson.

Before concluding, a few words concerning the measures to be adopted for preventing the spread of hydrophobia.

From our insular position we are in a better position than any neighbouring nation for holding a check, or even "stamping out" such a disease as hydrophobia, which, as the vast mass of evidence tends to show, does not originate spontaneously, nor does it appear that it would be difficult to effect this result were the suggestions which have been made by several recent writers carried into effect. It is certain that the number of dogs kept in England is enormously in excess of any requirements, and it is equally certain that this state of matters might promptly be put a stop to. The existing dog tax need not be increased in amount, but it should be enforced in the most stringent manner, the business of collecting, prosecuting, &c., being handed over to the police authorities of each district. Each dog should have a separate number on the local register, and might be the wearer of a collar bearing its registered number. Further, the licence should bear a description of the premonitory symptoms of rabies, and owners of dogs should be cautioned instantly to report any suspicious case to the police. These regulations would, as a necessary result, lead to each dog being individually looked after and cared for, and would, we believe, in the course of very few years, lead to the disappearance of rabies.

DIEN'S "CELESTIAL ATLAS"

Atlas Céleste, comprenant toutes les Cartes de l'ancien Atlas de Ch. Dien. Rectifié, augmenté, &c., par Camille Flammarion. 3e édition. (Paris : Gautbier-Villars, 1877.)

THIS is a new and enlarged edition of Dien's "Atlas Céleste," which first appeared in 1864, with the co-operation of M. Babinet, and is brought out under the editorship of M. Camille Flammarion. That the formation of the atlas, both in its original and extended plan has involved a great amount of labour will be evident upon a very superficial examination. The first issue was said to contain upwards of 100,000 stars and nebulæ, of which 50,000 had been observed by Lalande, projected on the development of a sphere, sixty-five centimètres in diameter, their places being reduced to the year 1860, and this scale was stated to be sufficiently large to allow of the insertion without confusion of all stars to the ninth magnitude inclusive. The charts were said to contain "nearly the totality of stars in the catalogues of Lalande, Herschel I., Piazzi, Harding, Struve, Bessel, Herschel II., Groombridge, and Argelander," while for the southern heavens recourse was had to the catalogues of La Caille and Brisbane. This description of the authorities consulted is not very definite. The reference to Harding must apply to his atlas ; that to Bessel may be supposed to at least include the catalogue of equatorial stars observed by the Königsberg astronomer, which was prepared by Weisse of Cracow, and published in 1846, if not the second catalogue founded upon Bessel's observations, containing stars from 15° N. to 45° N. declination, also reduced by Weisse, which appeared in 1863. The reference to Argelander is especially indefinite ; we have the well-known catalogue of 560 stars, and the "Uranometria Nova," but previous to the year 1864, when "Dien's Atlas" was published, astronomers were also in possession of vols. 3, 4, and 5 of the "Durchmusterung," with the results of the survey of the whole northern heavens.

The programme originally prepared was a very extensive one. The new edition is stated to have received numerous corrections and considerable enlargement to bring up the work to the actual state of astronomical science, and there is sufficient evidence that an attempt has been made in this direction, but we regret to have to express the opinion, after a close examination of the "Atlas," that in its present state it does not fulfil the programme upon which it was formed. It will soon be evident, on comparing the maps with the charts issued by the Berlin Academy, or more generally with those in the original edition of Harding's Atlas, that so far from containing stars to the ninth magnitude inclusive, numerous eighths, and even stars of 6·7 magnitude, are omitted, and it is not easy to see from what cause. It might be inferred that Bessel's catalogue of equatorial stars had not been utilised, since stars of the seventh and eighth magnitude observed by Bessel and not observed by Lalande, are wanting. But in addition we soon miss stars that do occur in the "Histoire Céleste," as for

instance L. 39836, a star which Lalande considered a sixth magnitude.

Different views will be taken with regard to the proper contents of a celestial atlas, intended for general use, and it is not nerefore desirable to be too critical upon this point, but to take, we will say, two extreme uses to which an atlas of the pretensions of Dien's may be applied, first for following a small planet with the aid of a chart professing to contain stars to a less degree of brightness, and secondly, for identifying the naked-eye stars by the general maps including only these brighter stars, an elementary purpose for which an atlas may be quite as readily adapted as a globe. In the former case Dien's maps are not sufficiently filled in to allow of a planet equalling in brightness stars of Bessel's ninth magnitude being identified without some trouble and disappointment, and in the latter case we meet with a failing which is only too common with star-atlases—the outlines of constellations are so prominently drawn as seriously to interfere with, if not entirely to obliterate the naked-eye stars of the lower magnitudes, in using the "Atlas" in the open air. As a model of what an atlas should be in the latter respect, we must still refer to Argelander's "Uranometria," which, in our opinion, has yet no equal for the more elementary uses of such a work.

Among the best features in the new edition of Dien's "Atlas" are the delineation of the southern heavens, in which Brisbane's stars are laid down, the view of the distribution of double and multiple stars by M. Flammarion, the orbits of some of the principle revolving double-stars, and figures of remarkable nebulæ and clusters of stars.

OUR BOOK SHELF

Horticulture. By F. W. Burbidge. With Illustrations. (London: E. Stanford, 1877.)

THIS is one of the series of small handbooks on the British manufacturing industries, edited by Mr. G. Phillips Bevan, of which we have already noticed several volumes. A compact work on practical gardening, to serve as a guide to the amateur gardener and fruit-grower, was much wanted, and this volume to a certain extent supplies the desideratum. After a short chapter on commercial gardening, the author treats of the cultivation of fruit, and of the various descriptions of vegetables and herbs; and then of gardening in its various departments, but more from the economical than from the amateur's point of view. If the owner of a garden wants to turn his bit of land to the most profitable account, he will find Mr. Burbidge an admirable guide; but if he infers from the title of the book that he will obtain from it advice as to the treatment of his pelargoniums, fuchsias, and chrysanthemums, or the management of his hothouses, he will be disappointed. We fancy that information of this kind would commend itself to a larger number of readers than the guide-book information of the exact number of acres in each of our London parks, and the annual cost of maintaining them. The advice as to the culture of fruit and vegetables seems to us very good; but the rather poor woodcuts do not add to the value of the volume.

Mittheilungen aus dem k. zoologischen Museum zu Dresden. Herausgegeben mit Unterstützung der General-direction der königlichen Sammlungen für Kunst und Wissenschaft, von Dr. A. B. Meyer, Director des königlichen zoologischen Museums. Zweites Heft mit Tafel. (Dresden, 1877.)

IN a former volume of NATURE (vol. xiii., p. 464) we have given some account of the origin of this meritorious work, of which the second portion is now before us. Like the former half of the first volume of the contributions the present section is chiefly occupied with memoirs based upon the collections made by Dr. A. B. Meyer during his well-known expedition to New Guinea and the adjacent islands. Herr Th. Kirsch, the entomologist of the Dresden Museum, commences with two articles upon the lepidoptera and beetles collected by Dr. Meyer in New Guinea. Of the former Herr Kirsch enumerates 167 species, of which 133 belong to the diurnal section. Several novelties are described and well figured. The next article is by Dr. Meyer himself, and gives us an account of a large series of Papuan skulls which he collected on the mainland of New Guinea and in the Island of Mysore, in the Bay of Geeldink. The collection, embracing altogether 135 examples, is, we believe, by far the finest of this branch of the human family ever made, and should, we suppose, lead to some definite results upon that somewhat mysterious subject—the differentiation of the various races of mankind by their skulls. A second article by Dr. Meyer relates to the specimens of anthropoid apes in the Dresden Museum. We cannot say that the photographic plates of the stuffed specimens of these creatures are either elegant or likely to be of very great use, but it is satisfactory to have the vexed question of the identity of the celebrated "*Mafoka*" lately living in the Zoological Gardens at Dresden, and long supposed to be a gorilla, finally set at rest, as is done by von Bischoff's article on its anatomy, which follows that of Dr. Meyer. A memoir on the Hexactinellid Sponges collected by Dr. Meyer in the Philippine Seas, in the preparation of which Herr W. Marshall has given his assistance, concludes this interesting volume, of which we may say that it adds materially to the status of the Dresden Museum, and to the scientific fame of its energetic director.

LETTERS TO THE EDITOR

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

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

The Radiometer and its Lessons

I AM obliged to ask you to allow me to add a few words, by way of further explanation, to my letter printed in NATURE, vol. xvii. p. 80.

In trying to estimate the effect of the communication of heat between a solid body and contiguous gas, I have assumed that certain simplifying suppositions may be legitimately made, for the most part identical with what are very commonly adopted in discussing the pressure exerted by a gas on a solid in contact with it. That is to say, I have assumed, first, that we may resolve the velocities of the molecules of gas into three rectangular components, one perpendicular to the surface of the solid and the other two parallel to it; second, that we may conceive of the whole number of molecules as divided into three equal parts, one-third moving in the direction of each of the resolved components of the velocity respectively; third, that the mutual pressure between the solid and the gas, and any communication of heat from one to the other, may, for the purpose in hand, be attributed to *direct* impacts of molecules against the solid surface; fourth, that all the molecules endowed with a velocity perpendicular to the solid surface, and contained within a layer adjacent to this surface of a thickness not greater than the mean length of path, will strike the surface, while none of those which are outside this layer will ever reach it; fifth, that the particles which have struck the solid surface will return from it with an average velocity corresponding to the temperature of the surface, and will retain this velocity until they arrive at the farther side of the layer before-mentioned. It was on the supposition that these are legitimate assumptions that

I spoke of heat passing across a stratum of gas from one solid surface to another "*as though* there were, in contact with each solid surface, a layer of gas whose temperature is throughout the same as [it would perhaps have been better to have said "determined by"] that of the contiguous solid."

I am fully aware of the ease with which one may be led into serious mistakes by trusting too implicity to such simplifying assumptions, and also that some of the particular suppositions made above would be inadmissible in a discussion of the general problem of the conduction of heat in gases; but I do not see any fallacy in employing them for the special purpose which I had in view in my last letter, namely, to show why I think that the flow of heat across a thin stratum of gas must be facilitated by diminishing the pressure of the gas. Prof. Osborne Reynolds's argument that "if there were a layer of uniform temperature, no heat would be transmitted," does not appear to me to be applicable to the case in question. It seems conceivable, as an extreme case, that, in a very thin layer of gas, between parallel solid surfaces maintained at different temperatures, the molecular movements might take place exclusively in the direction of the perpendicular to the bounding surfaces. In such a case the particles would move from side to side of the layer of gas with a uniform velocity, though the velocity one way would be greater than the velocity the other way, and heat would be transmitted across a layer of gas having the same temperature throughout. Such a condition, whether practically realisable or not, would, if I understand him aright, be the limiting case in one direction of what Mr. Stoney has called for shortness a "Crookes's layer:" the limiting case in the other direction being the ordinary condition of a gas, where the average velocity of the molecules is independent of direction. I venture to think that, in pointing out the results which must follow from the existence of a predominating direction of molecular motion, Mr. Stoney has made a very important contribution to the kinetic theory of gases; and I do not see that his conclusions are in any way invalidated by its being shown that they are not in harmony with "the generally-accepted laws of gases," inasmuch as these laws are deduced from suppositions which expressly exclude the conditions he has investigated.

G. CAREY FOSTER

December 17

ALLOW me to say a few words on what I believe to be the correct theory of the radiometer. This theory was given to me by Prof. Osborne Reynolds during spring of 1875, and I have found it capable of explaining every experiment on the subject with which I am acquainted.

The conservation of momentum is one of the laws of nature which even molecules do not break, and that law puts some restraints on the wonderful things which the shocks of molecules can accomplish. Imagine a vessel full of gas at a certain temperature. The centre of gravity of the gas and that of the vessel are supposed to be at relative rest. Suppose now that I increase the velocity of a certain number of molecules in a given direction, the centre of gravity of the gas will move relatively to the centre of gravity of the vessel, and no number of encounters between the molecules can alter that motion until the momentum has been taken up by the vessel. If in any gas we have a passage of heat in a certain direction, we shall have a propagation of momentum owing to the fact that the molecules move more quickly in one direction than in the opposite one, and no number of encounters can alter that propagation. Where the momentum enters the gas and where it leaves it we observe certain forces. This is Prof. Reynolds's theory of the radiometer. It has been objected that an increased pressure on the cool side of the vanes of a radiometer will counterbalance the force acting on the blackened sides, when the dimensions of the vessel are large compared with the mean path of a molecule, but I do not think that such is the case. The following special case may make this point a little clearer. If the forces on the vanes are counterbalanced, the forces on the vessel must be counterbalanced as well. In the case of an ordinary radiometer the forces reduce to a couple, and I do not see how any crowding of molecules in one part of the vessel more than in another can produce a couple on the vessel. The whole problem is one of conduction of heat. All the experiments made by Mr. Crookes on cups, inclined vanes, &c., admit of the same easy explanation as the fact that when a long and a short wire are connected with the poles of a battery, the current in the shorter wire will be the strongest. In a radiometer with inclined vanes, for instance, the temperature is the same on both sides, but the gradient of temperature is much larger on one side, and hence more heat will escape on that side. The dimensions of the vessel also have to be taken into account in the same way as the length of a wire has to be taken into account when the strength of an electric current flowing through it has to be calculated. It is difficult to say exactly what takes place within very small distances from the hot surface, but it seems clear that any phenomenon, such as Prof. Carey Foster supposes to exist, must affect the passage of heat in the same way as the force on the vanes. As the careful researches of Messrs. Kundt and Warburg have shown that under great exhaustion the conduction of heat decreases and does not increase, I do not see how an increase in the force can take place.

The scientific world will judge how far Prof. Stoney has succeeded in establishing any new laws on the conduction of heat through gases. In justice, however, to Messrs. Provostaye and Dessains, whose experiments he calls to his aid, I wish to point out that their numerous experiments, with two exceptions, are in entire accordance with existing theories. At the time these experiments were made, no distinction was drawn between convection and true conduction. In order to deduce, therefore, the loss of heat due to true conduction, Prof. Stoney is obliged to subtract the effect due to convection currents. He draws, therefore, a curve representing the loss of heat due to this cause. All his conclusions must stand or fall with this curve, and I am afraid they must fall.

After Professors Clausius and Maxwell had deduced theoretically the coefficient of conductivity for gases, a series of celebrated experiments were made by Stefan, by Narr, by Plank, by Winkelmann, and last, but not least, by Kundt and Warburg. The influence of convection currents has been fully discussed in these papers and eliminated, and the conclusions arrived at by all these experimenters are fully in accordance with each other and with theory. It appears, as was expected, that when the effects of convection currents are eliminated, the coefficient of conductivity is independent of pressure until the dimensions of the vessel are comparable with the mean free path of a molecule, and that then the conductivity rapidly diminishes. It also appears that at the pressures at which Messrs. Provostaye and Dessains found that the loss of heat was independent of pressure, convection currents must have ceased to be appreciable, and therefore the great mass of their experiments is fully in agreement with later researches.

The only exception is found in the case of carbonic acid and nitrous oxide. These abnormal results were not confirmed by Messrs. Kundt and Warburg in the case of carbonic acid, the only one of the two gases which they examined. Whoever reads their account of the difficulty they had in excluding the last traces of moisture, and considers the increased conductivity which such an admixture would produce as the pressure diminishes, will have no difficulty in explaining the anomaly. At any rate I do not think Prof. Stoney will be inclined to base important conclusions on unconfirmed experiments on two gases in which we should expect the effect, owing to their density, to be particularly small. The discovery of Master Gerald Stoney, who found that a red hot wire was cooled when a tin can containing water was brought sufficiently close might, I think, have been foretold by the recognised theory. Prof. Stoney, no doubt, will find on reading over the literature on the subject, that what he calls penetration of heat, has hitherto been known under the name of conduction of heat, that it takes place at all pressures, and begins to disappear at the exact point at which he makes it appear.

The timely calculation of Mr. S. T. Preston in the August number of the *Phil. Mag.*, shows that any theory of the radiometer which makes the action depend on the comparatively large ratio of the mean free path to the dimensions of the vessel, must necessarily be wrong.

ARTHUR SCHUSTER

The Proposed Channel Islands' Zoological Station, Aquarium, and Piscicultural Institute

I AM very anxious that this project[1] should succeed, mainly because of the facilities it will afford to inland aquaria, in procuring living animals cheaper, better, more variedly, and more systematically, than at present. This, I believe, will form the most profitable part of the undertaking.

[1] Referred to in NATURE, vol. xvii. p. 102.

I hope, also, it may succeed as a sort of living museum, without any of the kind of attractions which are not biological ones, and which, indeed, are not scientific in any sense, as common and ribald music, theatricals, acrobatic and jugglery performances, and so forth. Only, no aquarium has ever permanently thriven without these things.

I greatly doubt whether anyone yet possesses the requisite knowledge to be able to rear any marine crustacean from the egg state to an adult condition, and to feed it in captivity in such manner as to be able to sell it in the open market at rates below those sold under present circumstances. Yet this is put forward, conspicuously, as one of the aims of the scheme. In Britain are eaten as human food about a dozen species of crabs, lobsters, prawns, and shrimps, and most of these have been occasionally bred in aquaria as far as the Zoea state, when they are free swimmers, and they then generally die. Rarely, some few have been brought up to a higher stage, but I know of no instance, during twenty-five years of experience, where any marine crustacean of any kind has been reared to an adult condition in an aquarium. And if such a thing could be done, I believe that to feed them in any state of captivity, with animal food, which they require in great abundance and variety, and which must be purchased, would be very expensive, indeed, far too costly I think, to be practically and commercially remunerative.

I am glad to see that in last week's NATURE, vol. xvii. p. 133, it is stated that that excellently-tasted little fresh-water lobster, *Astacus*, has been bred artificially by a Piscicultural Institution at Schwerin. If so, why should it not be similarly bred in Britain, where it is much seldomer eaten than in France and Germany. In Berlin, Hamburg, and Dresden, I have often purchased it at sixpence a dozen, while in Paris I have given as much as sixpence each for it. It is a pity, however, that the Schwerin account is not more full and explicit. It is stated that in the spring of 1876, 700 *Astacus* in egg, were placed in two round ponds, each of six feet diameter. Holes were made in these, and recently, on draining the ponds, only three or four adult crayfish were found straying about the ponds, the rest each being in a separate hole, and a large number of young ones were found, as big as bees, and very lively. What size were the crayfish at birth, and if very small, and swimming, how were they prevented from escaping from the ponds? Information is wanted as to the shape, length, breadth, direction as to angle, and distance apart of these holes, and their position in the ponds, whether in the sides or base, or both. If nearly 700 animals occupied as many holes, where were the young ones? How many young were there? If each female had only as few as 100 eggs hatched out, then 70,700 must have been the population of these two little pools. When, and in what manner, were the males introduced? We require also to be told of the material of which the ponds were constructed, and if the sides were upright, and the bottoms flat, or if rounded or basin-shaped. If water ran in and out, how much in a named time, of what quality, as to foreign substances it contained in solution and suspension, and what was its temperature at various periods of the year? In what direction and in what amount was light admitted? How much vegetation, and of what kinds, grew in the ponds? What kinds of animal food was given them, and how much and often, and was it cooked or raw? Carrots appear an odd food for crayfish Let all these things and more, be carefully ascertained, to see if they can be applied to the culture of *Homarus*, the near marine relative of *Astacus*, before much money is spent.

It would be an excellent thing for students to have a place to study at, such as is proposed to be provided for them at Jersey, and similar to the zoological station and aquarium at Naples, in the arrangement of which I had much to do. But would students be content to go only so far as Jersey? Is not the access too easy, and too cheap, as it is not easy or cheap to go so far as Naples, and to have the *name* of so going? I have often thought it odd, and evincing not at all a really zealous spirit in my own direction on the part of my fellow-naturalists, that such a thing should be, that though the Crystal Palace Aquarium has existed for seven years within less than one hour's railway ride from London, and though it contains a constantly advertised collection of living marine animals exceeded in variety and interest by none in Britain, or even Europe, yet no scientific man, except the late Edward Newman, has ever applied for permission to carry on any course of inquiry here on any subject, continuously or occasionally, connected with the habits of living creatures, in the spirit of say, Gilbert White of Selborne. Yet we offer all such advantages as table-space, good light, and the use of any animals in our tanks not having a considerable money value—lest injury be done to such specimens—absolutely free of all charges. At this moment we possess many Italian animals in our collection, as fishes, crustaceans, mollusks, zoophytes, &c., which can be seen alive nowhere else, save on the shores of the Mediterranean, and yet no professed zoologist known to me ever comes to see them, or takes the smallest interest in them. They are therefore beheld only by the general public, who only look at them for their mere prettiness, or for what untrained observers are complacently pleased to term "ugliness." No student ever asks us for more than any dead animals we may chance to have, and which we give away gratis, and these apparently afford far more pleasure than the sight of living specimens. It is not at all uncommon to meet with biologists who openly and avowedly proclaim their contempt for collections of living animals in aquaria, which they regard as being "well enough for women and children," but for *men* they say there is nothing like seeing such animals in rows of glass jars of alcohol on the shelves of a museum. My last contribution of any length to NATURE was made so long ago as October 12, 1871, when I gave a description of the Crystal Palace Aquarium, then only just opened. Since then we have had no cause to complain of the appreciation of the world of sightseers. But as regards the indifference of the scientific world, that has been and is so great that the place might never have existed. William Yarrell, the British ichthyologist of the generation just passed away, used to tell me how glad he would be to see a live John Dory (*Zeus faber*), and how much he would give to behold one swimming. But here, at Sydenham, this fish can be seen alive and in perfect health for months together, in crystalline sea-water. Yet no Yarrell ever comes to see them. Are there no Yarrells, and Whites, and Watertons, and Newmans now? or has their very spirit passed away into the region of apathy where the affectation of caring for nothing, and of being never moved to zeal in anything, in this observation of live animals, seems to be regarded as a very high accomplishment?

W. A. LLOYD

Crystal Palace Aquarium, December 15

The "Challenger" Estimates of the Volume of the Gulf Stream

IN the interesting "Voyage of the *Challenger*," just published, Sir Wyville Thomson states (vol i. p. 371) "that the Gulf Stream in its restricted sense was, early in May, 1873, at the point where we crossed it and made our observations, about sixty miles in width, 100 fathoms deep, and its rate three knots an hour." I was much surprised at reading this, as the Admiralty Report on Ocean Soundings, No. 7, p. 12, estimates it at the same point as "100 fathoms deep, and running at the rate of three miles an hour *for a width of fifteen miles*, discharging four and a half cubic miles of heated water per hour."

As no reference is made by Sir Wyville Thomson to the extraordinary discrepancy in these two estimates of the same thing at the same time—one being four times the volume of the other—and as he says he makes the statement "thus guardedly" I think, in the interest of scientific accuracy, an explanation is required.

T. MELLARD READE

Liverpool, December 8

The Fossil Peronospora as a Primordial Plant

THE concluding sentence of your notice (vol. xvii. p. 128) of my observations on a fossil fungus is so important, that I shall be glad of a word of reply. You say, "But should not this primordial plant have led a non-parasitic life? for if parasitical, then this fact points to some pre-existing plant."

Although the specimen I have figured is shown as growing within the decayed tissues of a Lepidodendron, yet it does not follow that the same fungus could not perfect itself on humus alone. Recent species of Peronospora show a tendency to grow upon the ground, as several species, including the fungus of the potato disease, will grow and produce fruit on the naked earth. A truly terrestrial species is found in the allied *Botrytis terrestris*, Persoon, and many of the Mucedines grow freely in cellars, on damp walls, or in any moist place.

WORTHINGTON G. SMITH

15, Mildmay Grove, N.

THE "CHALLENGER" IN THE ATLANTIC[1]

THE *Challenger* left Portsmouth on December 21, 1872, and on the evening of May 24, 1876, she dropped her anchor at Spithead after an eventful voyage, which lasted three and a half years. Shortly after her arrival we gave a sketch of her cruise over the Atlantic and Pacific Oceans. The two volumes just published consist chiefly of an abstract of the less technical portions of the journal kept by Sir Wyville Thomson during the first year of the *Challenger's* voyage, and during the early part of the fourth year's voyage, when she was on her way home. During both these periods the *Challenger* was in the Atlantic, so that we now obtain the record of her survey of this great ocean in a very complete form, and are led to look forward to several additional volumes, in which the account of her cruise in the Pacific Ocean and amongst its fair islands will appear. A great deal of credit must be given to the author of these two splendidly illustrated volumes for his so speedily publishing them. A large portion of one of them was actually passed through the press while the *Challenger* was at sea, and the preparation of the second volume had to be carried on amid the cares not only of professional duties, but also of getting the immense collections made into order, and of making arrangements for the thorough working out of the scientific results of the voyage. May we express the hope that his energy will enable him speedily to complete the popular narrative of this cruise thus so auspiciously begun. The strictly scientific records of the *Challenger* voyage cannot be published for some time; the working out of old forms, the describing and illustrating of new ones, takes time; such work, to be done well, must necessarily be done slowly, and hence we all the more urge on Sir Wyville Thomson to let us have, as soon as can be, the completion of the popular narrative of the general results of his four years' work. This preliminary account is indeed not solely a popular one, for we find in these two volumes a mass of exact scientific details that will make them always works of reference to the scientific student; and while some few of the wondrous new species of animals and plants are but incidentally introduced to us, their descriptions are often so well written, and their forms are so exquisitely portrayed, as to leave us for the time somewhat independent of their more exact scientific diagnosis.

In our previous sketch of the voyage of the *Challenger* we dwelt somewhat in detail on the work accomplished by her during the first six months of 1873. About the middle of June in that year she left the Bermudas for the Açores and Madeira, establishing twenty-five stations on her way, some of these showing ocean depths of 2,800 fathoms. A few pleasant days were spent (July 1873) at Ponta Delgada, the capital of San Miguel and the chief town of the Açores. On acccount of the presence of an epidemic of small-pox no delay was made at Madeira, but the vessel's course was struck for the Canaries and Cape de Verde Islands, keeping somewhat parallel to the Coast of Africa until nearly opposite Cape Palmas, when they turned westward and shaped their course to Saint Paul's Rocks. These solitary rocks are nearly under the equator, midway between the coasts of Africa and of South America. They were visited in 1832 by the *Beagle*, and are noticed in Darwin's charming "Voyage of a Naturalist." Merchant-vessels usually give them a wide berth. They seem to have struck the travellers by their small dimensions; it being rather under a quarter of a mile from the one end of the group to the other, they form quite little specks of rocks out in mid-ocean. Landing on these rocks was no easy matter. A loop of eight or ten ply of whale-line was passed round one of the rocks; to this a hawser was run from the ship lying about seventy yards out, with her bows in 104 fathom water; the hawser was made fast to the whale-line, and the ship thus moored to the rocks. Having landed on the rocks a line was laid across the mouth of the cove, which made the landing easier for the next parties.

Only two species of birds were found on the rocks, the "booby" (*Sula fusca*) and the "noddy" (*Sterna stolida*), both being widely distributed birds on tropical islands and shores. They were here in enormous numbers, were quite tame, even allowing themselves to be taken up with the hand. The breeding season was over. No land plants were found, not even a lichen. The terns used a green alga to line their nests; all the crannies of the rock were crowded with an amphibious crab (*Grapsus strigosus*), which was much more wary than the birds, though "wherever a morsel of food came within their reach there was instantly a struggle for it among the foremost of them, and they ambled away with their prize wonderfully quickly: their singular sidelong gait and a look of human smartness about them had a kind of weirdness from its being exhibited through a set of organs totally different in aspect from those to which one usually looks for manifestations of intelligence."

Leaving these desolate rocks on August 29, the island of Fernando Noronha was in sight on September 1, rising like most of the ocean islands, abruptly from deep water, the depth of the ocean within six miles of the island being more than 1,000 fathoms. This island presents a most remarkable appearance; the land is generally not very high, but there is an irregular cliff which rises to a height of about 100 feet from the sea, succeeded by undulating land and conical hills, usually covered with luxuriant vegetation. The Peak is an extraordinary-looking mountain, formed of a column of rock which starts up to a height of 600 feet from a more or less level plateau of rock, itself some 400 feet above the sea. There is a village and a citadel, the place being a penal settlement belonging to Brazil. There were at the time on the island nearly 1,400 convicts and a garrison of 200 soldiers. The convicts enjoyed a considerable amount of liberty, each of them occupying a hut, and being allowed to cultivate a little piece of garden ground, though their time and labour from six in the morning until four in the evening belonged to the Government. Sir Wyville Thomson and his assistants were extremely anxious to investigate thoroughly the flora and fauna of this island, but unfortunately the military commandant set his face against this, and the land work had to be abandoned.

"The coast scenery was here and there very beautiful, little sandy bays with a steep cultivated slope above them, or a dense tangle of trees absolutely imbedded in one sheet of matted climbers, separated by bold headlands of basalt or trap stuff. Besides the tropic birds, there were to be seen beautiful little terns, snowy white, which usually flew in pairs a foot or two apart, one following all the motions of the other, like a pair of paper butterflies obedient to the fan of a Japanese juggler. They could be seen flying over the land, and often alighting upon the trees. The noddy was very common, and the booby was in considerable numbers. High upon the cliffs the nests of the frigate bird (*Tachypetes aquila*) could be seen, and from time to time these splendid birds moved in slow and graceful circles overhead." No wonder that the author adds, "We lay for some time below the cliffs admiring the wonderful wealth of animal and vegetable life ere we returned slowly to the ship."

On September 14, as they neared the coast of Brazil, a shower of butterflies fell on the ship, fluttering in multitudes over it; and over the sea as far as the eye

[1] "The Voyage of the *Challenger*. The Atlantic: a Preliminary Account of the General Results of the Exploring Voyage of H.M.S. *Challenger* during the Year 1873 and the Early Part of the Year 1876." By Sir C. Wyville Thomson, Knt., LL.D., F.R.SS. L. and E., &c., Regius Professor of Natural History in the University of Edinburgh, and Director of the Civilian Scientific Staff of the *Challenger* Exploring Expedition. Two volumes. Published by Authority of the Lords Commissioners of the Admiralty. (London: Macmillan and Co., 1877.)

could reach they quivered in the air. Looking up into the sky where they were thickest, they were seen to be close together and had much the appearance and peculiar motion of large flakes of snow. Amidst such a downpour the entrance to Bahia was seen. It is very beautiful; the coast is not elevated; it is neither mountainous nor hilly, but rises from the sea-shore in even terraces, broken here and there by ravines and wooden knolls, every space gloriously clothed with vegetation, and the sky-line broken by long lines of palm trees—from the sea it reminded one of Lisbon, but its splendid luxuriance of vegetation gives it a character of its own.

The scientific work of the *Challenger* was to be on the ocean, and Sir W. Thomson properly discouraged his staff from expending too much of their time or energies on investigating the natural history of the few spots of North or South America that they from time to time landed on. We therefore in these volumes meet with very few references to the glimpses that they got of this continent, but some time had to be spent at Bahia, and we cannot avoid giving the following interesting extract which describes a visit made by Sir W. Thomson to Santo Amaro.

"Mr. Wilson was obliged to be next day at Sto. Amaro, a little town about thirty miles distant, across one of the ridges on another river where he had a line of steamers plying, and he asked us to ride there with him; so we went back to his house and dined, and spent the evening at his window inhaling the soft flower-perfumed air and gazing at the stars twinkling in their crystal dome of the deepest blue, and their travesties in a galaxy of fire-flies glittering and dancing over the flowers in the garden beneath us. It was late when we tossed ourselves down to take a short sleep, for two o'clock was the hour fixed to be in the

FIG. 1.—The *Challenger* at St. Paul's Rocks.

saddle in the morning. We rode out of the town in the starlight, Mr. Wilson, Capt. Maclear, and myself, with a native guide on a fast mule. We were now obliged to trust entirely to the instinct of our horses, for if a path were visible in the daylight there was certainly none in the dark, and we scrambled for a couple of hours right up the side of the ridge. When we reached the top we came out upon flat open ground with a little cultivation, bounded in front of us by the dark line of dense forest. The night was almost absolutely silent, only now and then a peculiar shrill cry of some night-bird reached us from the woods. As we got into the skirt of the forest the morning broke, but the *réveil* in a Brazilian forest is wonderfully different from the slow creeping on of the dawn of a summer morning at home, to the music of the thrushes answering one another's full rich notes from neighbouring thorn-trees. Suddenly a yellow light spreads upwards in the east, the stars quickly fade, and the dark fringes of the forest and the tall palms show out black against the yellow sky, and almost before one has time to observe the change the sun has risen straight and fierce, and the whole landscape is bathed in the full light of day. But the morning is for yet another hour cool and fresh, and the scene is indescribably beautiful. The woods, so absolutely silent and still before, break at once into noise and movement. Flocks of toucans flutter and scream on the tops of the highest forest trees hopelessly out of shot, the ear is pierced by the strange wild screeches of a little band of macaws which fly past you like the wrapped-up ghosts of the birds on some gaudy old brocade. There is no warbling, no song, only harsh noises, abrupt calls which those who haunt the forest soon learn to translate by two or three familiar words in Portuguese or English. Now and then a set of cries more varied and dissonant than

usual tell us that a troop of monkeys are passing across from tree to tree among the higher branches; and lower sounds to which one's attention is called by the guide indicate to his practised ear the neighbourhood of a sloth, or some other of the few mammals which inhabit the forests of Brazil. And the insects are now all awake, and add their various notes to swell the general din. A butterfly of the gorgeous genus *Morpho* comes fluttering along the path like a loosely-folded sheet of intensely blue tinsel, flashing brilliant reflections in the sun; great dark blue shining bees fly past with a loud hum; tree-bugs of a splendid metallic lustre, and in the most extraordinary harlequin colouring of scarlet and blue and yellow, cluster round a branch so thickly as to weigh it down, and make their presence perceptible yards off by their peculiar and sometimes not unpleasant odour; but how weak it is to say that that exquisitite little being, whirring and fluttering in the air over that branch of *Bignonia* bells, and sucking the nectar from them with its long curved bill, has a head of ruby, and a throat of emerald, and wings of sapphire—as if any triumph of the jeweller's art could ever vie in brilliancy with that sparkling epitome of life and light.

"It was broad day when we passed into the dense forest through which the greater part of the way now lay. The path which had been cut through the vegetation was just wide enough for use to ride in Indian file, and with some care to prevent our horses from bruising our legs against the tree-trunks, and we could not leave the path for a single foot on either side, the scrub was so thick, what with fallen tree-trunks, covered with epiphytes of all descriptions, and cycads, and arums, and great thorny spikes of *Bromelia*, and a dense undergrowth, principally of melastomads, many of them richly covered with blue and

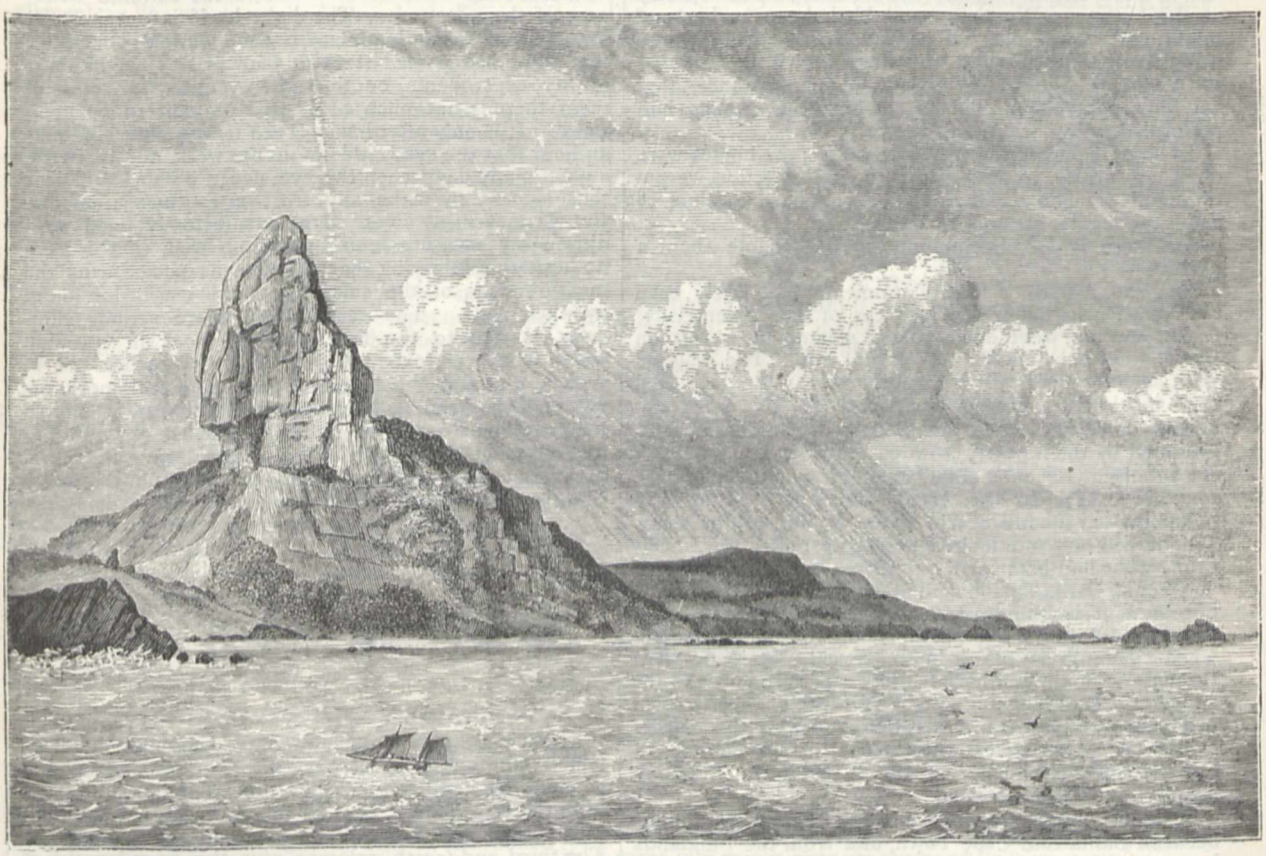

FIG. 2.—Fernando Noronha.

purple flowers. Above the undergrowth the tall forest trees ran up straight and branchless for thirty or forty feet, and when they began to branch, a second tier of vegetation spread over our heads, almost shutting out the sky. Great climbing *Monsteras* and other arals; and epiphytic bromeliads; and orchids, some of them distilling from their long trusses of lovely flowers a fragrance which was almost overpowering; and mazes of *Tillandsia* hanging down like tangled hanks of grey twine. Every available space between the trees was occupied by lianas twining together or running up singly, in size varying from a whipcord to a foot in diameter. These lianas were our chief danger, for they hung down in long loops from the trees and lay upon the ground, and were apt to entangle us and catch the horses' feet as we rode on. As time wore on it got very close and hot, and the forest relapsed into silence, most of the creatures retiring for their noonday siesta. The false roof of epiphytes and parasites kept off the glare of the sun, and it was only at intervals that a sheaf of vertical beams struck through a rift in the green canopy, and afforded us a passing glimpse of the tops of the forest trees, uniting in a delicate open tracery far above us.

"For some hours our brave little horses struggled on, sometimes cantering a little where the path was pretty clear, and more usually picking their way carefully, and sometimes with all their care floundering into the mud-holes, imperfectly bridged over with trunks of trees.

"As we had made our ascent at first, all this time we had been riding nearly on a level on the plateau between the two river valleys. Suddenly the wood opened, and we rode up to the edge of a long irregular cliff bounding

the valley of Sto. Amaro. The path ran right up to the edge and seemed to come to an end but for a kind of irregular crack full of loose stones which went zigzagging down to the bottom at an angle of about 70°, and we could see the path down below winding away in the distance towards the main road to Sto. Amaro. We looked over this cliff and told Mr. Wilson firmly that we would *not* go down the side of that wall on horseback. He laughed, and said that the horses would take us down well enough and that he had seen it done, but that it was perhaps a little too much ; so we all dismounted, and put the horses' bridles round the backs of the saddles and led them to the top of the crack and whipped them up as they do performing horses in a circus. They looked over with a little apparent uneasiness, but I suspect they had made that precarious descent before, and they soon began to pick their way cautiously down one after the other, and in a few minutes we saw them waiting for us quietly at the bottom. We then scrambled down as best we might, and it was not till we had reached the bottom, using freely all the natural advantages which the *Primates* have over the *Solidunguli* under such circumstances, that we fully appreciated the feat which our horses had performed.

"The next part of the road was a trial ; the horses were often up nearly to the girths in stiff clay, but we got through it somehow, and reached Sto. Amaro in time to catch the regular steamer to Bahia."

And here is an uncommonly good anecdote about a parrot :—

"At Sto. Amaro a line of tramways had lately been laid down also under the auspices of our enterprising friend, and we went down to the steamboat wharfs on one of the trucks on a kind of trial trip. The waggon went smoothly and well, but when a new system is started there is always a risk of accidents. As the truck ran quickly down the incline the swarthy young barbarians, attracted by the novelty, crowded round it, and suddenly the agonised cries of a child, followed by low moanings, rang out from under the wheels, and a jerk of the drag pulled the car up and nearly threw us out of our seats. We jumped out and looked nervously under the wheels to see what had happened, but there was no child there. The young barbarians looked at us vaguely and curiously, but not as if anything tragical had occurred, and we were just getting into the car again, feeling a little bewildered, when a great green parrot in a cage close beside us went through no doubt another of his best performances in the shape of a loud mocking laugh. A wave of relief passed over the party, but we were rather late, and the drivers expressed to the parrot their sense of his conduct, I fear strongly, but in terms which, being in Brazilian *patois*, I did not understand."

In another notice we will tell of the *Challenger's* doings between Bahia and Cape Town, and from the Falklands home, and we will also more particularly allude to the general results of the scientific work she has so successfully accomplished.

(To be continued.)

ON THE PRESENCE OF OXYGEN IN THE SUN

I HAVE spent the greater part of last winter and the beginning of this in an investigation of the spectra of oxygen. My experiments will be published, I hope, in another place ; but there are one or two points of more immediate interest, and, I venture to think, of some importance, which I trust you will allow me to discuss in your columns.

Prof. Draper has lately announced the important discovery that the lines of oxygen are found to be present in the sun. These lines, however, are bright, and not dark, as the Fraunhofer lines. I had found that at a certain temperature, lower than that at which oxygen shows its well-known lines, it gives another spectrum, and it occurred to me, when I heard of Prof. Draper's discovery, that if the temperature of the sun, at some point intermediate between the photosphere and the reversing layer was the same as that at which the spectrum of oxygen changes, the fact that the *known* spectrum of oxygen appears bright would be fully explained. The spectrum of lower temperature, which, for reasons to be given, I shall call the compound line spectrum of oxygen, ought in that case to be found reversed in the solar spectrum, like the remainder of the Fraunhofer lines.

I have consequently devoted all my time during three weeks to the exact measurement of these four lines, and I do not think that the evidence which I am about to give will be considered to fall far short of an absolute proof that the spectrum is really reversed in the sun.

Two difficulties have put themselves into the way of exact measurement. The first is due to the extreme weakness of the spectrum. The light itself is not stronger than that of a non-luminous Bunsen burner ; and after that light has passed through four prisms, as in most of my experiments, or through seven, as in some of them, there is not much of a spectrum left to be measured. It is only after having been in the dark for half-an-hour that the eye is able to do the work, and there are a good many days when the eye never obtains sufficient sensitiveness to make any trustworthy measurements. But whenever my eyes were in sufficiently good condition, my measurements agreed so well, that I have no hesitation in saying that they are as accurate as the measurements of the solar lines which will be found by their side. The second and more serious difficulty is due to the fact that the lines in question widen to a great extent with increased pressure and in such a way that the brightest part, and still more, the centre of the band, is displaced towards the red. I have not been able to get the lines perfectly sharp, and the measurement of the centre of the band will give, therefore, too high a value of the wave-length. The following table contains the numbers which I have obtained :—

Oxygen.	Width.	Solar Lines.	
		A.	S.
α 6156·86	±0·3	6156·70	6156·69
β 5435·55	±0·3	5435·44	5435·56
γ 5329·41	±0·6	5329·3	5329·10
δ 4367·62		4367 58	

The first column contains the wave-length of the compound line spectrum of oxygen. The second column contains the number which has to be added or subtracted from the wave-length, in order to get the edge of the lines, as it is their centres which are given in the first column. The third and fourth columns give the wave-lengths of the corresponding solar lines as observed by Angström (A) or myself (S.). The greatest difference is found in the line γ, but even this difference only amounts to the twentieth part of the distance between the sodium lines, and it would require a spectroscope of very good dispersive power and definition to separate two lines which would be that distance apart from each other. Nevertheless the amount in question is greater than the possible errors of observation, and I believe the difference to be due to the fact mentioned above, that the lines widen unequally. It will be seen from the table that the solar line would fall within the oxygen line, but about one-third of the distance between its most refrangible and least refrangible edge. At a higher pressure the brightest part of the band lies about 5331. None of the other lines widen nearly as much, and δ is always perfectly sharp. Angström gives it as an iron line, but according to Kirchhoff, the solar line is composed of two lines, and separated by a distance of about 0·1.

The average distance between the solar lines in the green, which have not yet been identified, is about 4·4, or more than fourteen times the difference between the centre of the oxygen line and the corresponding solar line. The average distance between the non-identified lines near O a is 4·9, or about twenty-nine times the corresponding difference. In judging, however, of the value of the evidence, I should like the reader to leave the line δ out of account. Although the agreement seems perfect, I have not the same confidence in the correctness of the wave-length as I have with the other lines. The line β is weaker than the others, and the error of observation may be a little larger than with a and γ, which will, I think, be found correct to the decimal place.

Let me point out in a few words the importance of the results obtained. The compound line spectrum of oxygen can only exist under a limited range of physical conditions. It is broken up at a higher temperature into the elementary line spectrum, and at a lower temperature it tumbles together into a continuous spectrum. During its existence its lines may be subject to variations owing to pressure. The spectrum of oxygen is therefore pre-eminently fitted to be at once the pressure gauge and thermometer of the sun. We cannot at the present moment give the exact temperature of the points at which the changes take place; but we can say with certainty why it is that the line spectra of many metalloids are not found reversed in the sun, for the temperature which gives these line spectra is higher than that which gives the compound line spectrum of oxygen, and therefore higher than that of the reversing layer of the sun. Consequently we must look for their band spectra and not for their line spectra. The same may be true for the spectra of some of the heavier elements like gold, silver, and platinum, which have not yet been discovered in the sun. The continuous spectrum of the base of the corona is most likely the continuous spectrum of the cooler oxygen.

As the science of spectroscopy advances we shall be able to determine the physical conditions which exist on the surface of the sun with as great a degree of certainty and a much smaller degree of discomfort than if we were placed there ourselves. I hope that this communication will prove to be a step in that direction. All my experiments were made in the Cavendish Laboratory.

ARTHUR SCHUSTER

St. John's College, Cambridge, November 30

OUR ASTRONOMICAL COLUMN

JUPITER'S SATELLITES.—Amongst the recorded phenomena connected with the motions of the satellites of Jupiter are several notices of observed occultations of one satellite by another, and of small stars by one or other of the satellites. The following cases may be mentioned:—On the night of November 1, 1693, Christoph Arnold, of Sommerfeld, near Leipsic, observed an occultation of the second satellite by the third at 10h. 47m. apparent time. On October 30, 1822, Luthmer, of Hanover, witnessed an occultation of the fourth satellite by the third at 6h. 55m. mean time.

Flaugergues, writing to Baron de Zach, from Viviers, on November 18, 1821, says: "I begin with an observation, very useless, no doubt, but extremely rare, for I have not found a similar one in the collections of astronomical observations which I have examined; *i.e.*, the occultation of a very small star by the third satellite of Jupiter." He proceeds to mention that on August 14, 1821, he repaired to his observatory very early to observe an eclipse of this satellite, and having looked at Jupiter with the telescope, he remarked a very small star near the third satellite. The satellite approached this star, and at 1h. 47m. sidereal time, it appeared to touch it, and at 1h. 56m. 52s. it was not possible to distinguish the star—it had disappeared. The satellite became fainter and disappeared in its turn at 1h. 59m. 10s. sidereal time, on August 13, or 16h. 30m. 8·5s. mean time at Viviers. The sky was perfectly clear, and Flaugergues considered his observations very exact. He adds that he continued to observe for a long time after the immersion of the satellite, hoping to see the star reappear, but he could not again distinguish it; the twilight had much increased, and small stars in the neighbourhood of Jupiter were soon effaced.

There is a similar observation by Mr. G. W. Hough, at Cincinnati Observatory, communicated in a letter to Dr. Brunnow, when Director of the Observatory at Ann Arbor, Michigan, and published in his "Astronomical Notices" Mr. Hough states that on March 28, 1860, he witnessed the end of an expected occultation of a star 9·5 mag., by Jupiter, and the occultation of the same star by the first satellite. When first seen it was distant from the limb of the planet about one diameter of the satellite, or one second of arc, so that the real separation had taken place about six minutes before (or about 8h. 9m. sidereal time), though he was not able to see it. At 10h. 27m. sidereal time the star was occulted by the first satellite and remained invisible eight minutes. Mr. Hough further says that the star is found in the "Redhill Catalogue," an obvious oversight; it would appear to be No. 1630 of Zone + 22° in the *Durchmusterung*, a star of 9·3m. the approximate place of which for 1855 was in R.A. 7h. 8m. 5s., N.P.D. 67° 3′·3.

DONATI'S COMET OF 1858.—This comet which attained so great a celebrity in the autumn of 1858, makes a very close approximation to the orbit of Venus near the descending node, and it may be reasonably inferred that the actual form of its path round the sun may be due to a very near approach of the two bodies at some distant epoch. The discussion of the totality of observations was undertaken some years since by Dr. von Asten, who has published his results in a dissertation entitled "Determinatio orbitæ grandis cometæ anni 1858, e cunctis observationibus." The comet was discovered by Donati on June 2, and was observed until the beginning of March, 1859, at the Cape of Good Hope and at Santiago de Chile; consequently the observations extended over a very wide arc of the orbit, and there have been very few cases where careful discussion could be expected to lead to more reliable results. The period of revolution deduced by Dr. von Asten is 1,880 years, and there is a high probability that this does not differ materially from the true one, applying to the time of the comet's appearance. Prof. Hill, of Washington, also by a complete investigation, obtained a somewhat longer period, but the general character of the orbit remains the same. Employing Dr. von Asten's elements, it will be found that in heliocentric longitude 343°·7, the distance of the comet from the orbit of Venus, is only 0·0047 of the mean distance of the earth from the sun. In 1858 the two bodies came into pretty near proximity, their mutual distance on October 17 being 0·088. It has been mentioned above that the point of closest approach of the orbits of the planet and comet is situated near the descending node; the opposite node falls in the region of the minor planets.

THE OBSERVATORY OF LYONS.—The *Bulletin Hebdomadaire* of the French Scientific Association reports that M. André is actively employed in the establishment of this new astronomical institution and is energetically supported by the Government. M. Raphël Bischoffsheim, the munificent donor of the meridian circle, lately mounted at the Observatory of Paris, has also intimated his intention to present the Lyons Observatory with its fundamental instrument, a meridian-circle of dimensions but slightly inferior to those of the circle, for which the Observatory at Paris is indebted to him. It will also be constructed by Eichens. The Paris meridian-circle is intended to replace the instruments of Gambey, which are now placed in one of the saloons of the institution with other instruments which have seen their day. M. Wolf

remarks that the scientific zeal and liberality of M. Bischoffsheim "inaugurates in France a path long followed in England by wealthy amateurs of astronomy."

THE METEORITE OF JUNE 14, 1877.—M. Gruey has calculated the orbit of this meteorite with the assistance of the Observatory of the Puy-de-Dôme, and accounts obtained through the press of Clermont, where he observed it at 8h. 55m. P.M. local time. Observations made at Bordeaux and at Angoulême were combined with those at Clermont. He obtained for the velocity of the meteor relatively to the sun 93 kilometres in a second, in the direction – heliocentric longitude 15° 17′, latitude – 17° 3′, and neglecting the insignificant effect of the earth's attraction upon a velocity so great, and the unknown effect of atmospheric resistance, he found for the heliocentric motion of the meteor the following elements of a hyperbolic orbit. Eccentricity, 7·079, semi-axis, 0·137. Ascending node, 83° 49′, inclination, 18° 14′, perihelion from node, 286° 50′, longitude at appearance, 263° 49′; the meteor approaching its perihelion was thus distant 23° from it.

This adds another case to several previous ones in which hyperbolic orbits have been obtained for meteorites by Petit, Galle, Tissot, &c.

PROF. NEWCOMB.—At the meeting of the Royal Society on Thursday last, the distinguished mathematical astronomer, Prof. Simon Newcomb, of Washington, was elected one of its foreign members. There was previously on the list only a single American, viz., Prof. Asa Gray. Prof. Newcomb's important contributions to astronomical science will be admitted to have richly entitled him to an acknowledgment at the hands of our leading society.

CHEMICAL NOTES

MINERAL OIL IN A LAVA OF MOUNT ETNA.—In the basaltic zone which reaches from the foot of Mount Etna in a south-south-easterly direction, near the village of Paterno there is a prehistoric doleritic lava containing olivine, which surrounds the clay deposits of a mud volcano and which has been examined by Sig. Orazio Silvestri. Under the microscope the lava shows an augitic principal mass with a quantity of olivine and many white transparent crystals of labradorite. The lava contains numerous round or irregular cavities which are coated with arragonite and which are filled with mineral oil. This oil, of which there is about 1 per cent. by weight in the whole mass, was taken from one of the cavities at 24° C. At about 17° C. it begins to solidify and is of a yellowish green tint by transmitted light, while by reflected light it is opalescent and light green. Chemical analysis of the liquid proved it to contain :—

Liquid hydrocarbons (boiling point 79°·28) ...	17·97 per cent.
Hydrocarbons solidifying under 0° (b. p. 280°–400°)	31·95 ,, ,,
Paraffine, melting point 52°–57°	42·79 ,, ,,
Asphalt (leaving 12 per cent. of ashes) ...	2·90 ,, ,,
Sulphur	4·32 ,, ,,
	99·93

FORMATION OF CERTAIN BODIES AT TEMPERATURES ABOVE THAT OF THEIR DECOMPOSITION.—MM. Troost and Hautefeuille have lately demonstrated that under certain circumstances it is incorrect to suppose that bodies undergoing decomposition or rather dissociation at a low temperature may not exist as definite compounds at higher temperatures. Their arguments are founded on the decomposition of silicon sesquichloride (Si_2Cl_6) at 800°, which may be represented as—

$$2Si_2Cl_6 = 3SiCl_4 + Si,$$

if, however, the reaction be carried on at a temperature above 1200° the following change takes place—

$$3SiCl_4 + Si \quad 2Si_2Cl_6.$$

If the tube in which this reaction takes place be cooled suddenly the sesquichloride is found, but if cooled slowly it undergoes gradual decomposition. They also find that although ozone is converted into oxygen at 250°, if a silver tube inclosed in a porcelain tube be kept at about 1300° a deposit of dioxide of silver is produced due to the formation of ozone. They state that the ozone can be recognised by the usual tests if the gas be rapidly drawn off and quickly cooled. They have also examined certain similar phenomena in the production of oxide of silver at 1400°.

IODATES OF COPALT AND NICKEL.—Prof. F. W. Clarke describes these salts, which were prepared by dissolving the carbonates in aqueous iodic acid, and allowing the solution to evaporate spontaneously when salts of the composition $CoI_2O_6.6H_2O$ and $NiI_2O_6.6H_2O$ crystallise out. If the solution of the carbonate of cobalt in iodic acid is evaporated rapidly, then the iodate of Rammelsberg, containing $1\frac{1}{2}$ molecules of water may be obtained, but not otherwise. The cobalt iodate loses four molecules of water at 100°, but the remaining two molecules cannot be driven off without partial decomposition of the salt. The specific gravities of the two salts are almost identical, the cobalt iodate at 21° being 3·6893, the nickel iodate at 22° being 3·6954. No numbers of the solubilities of the two salts are given by Prof. Clarke, but these, when obtained, will be of some interest.

ORIGIN AND FORMATION OF BORACIC ACID.—M. Dieulafait (*Comp. Rend.* lxxxv. 605) finds that under certain conditions by spectrum analysis $\frac{25}{1000000}$ grammes of boron, and by the colour imparted to a hydrogen flame $\frac{1}{1000000}$ grammes may be detected. He considers boracic acid to be a normal constituent of sea-water and salt marshes lying above beds of carnallite. M. Dieulafait finds that this acid may be recognised in a drop of sea-water weighing about 0·0378 grammes, and that the minimum quantity found in the Mediterranean is two decigrammes per cubic metre of water. He arrives by geological reasoning at conclusions differing from those of Dumas and others with regard to the origin of this body in the lagoons of Tuscany, and thinks that the source of boracic acid in this district may be found in a relatively modern formation.

NEW MODES OF FORMING ETHYLEN OXIDE.—In the *Comptes Rendus*, lxxxv. 624, Mr. H. Greene mentions the results of experiments on the action of certain metallic oxides on the bromide, iodide, and chloriodide of ethylene. Oxide of silver has a rapid action on ethylen iodide at a temperature of 150°, forming ethylen oxide; its action on ethylen bromide produces the same result but requires a higher temperature. Ethylen bromide and chloriodide both act upon sodium oxide at 180°, the latter of the ethylene compounds being the one found most advantageous by the author in preparing ethylen oxide. He has also studied the action of these substances on the oxides of the diatomic metals barium and lead. These oxides do not give ethylen oxide when heated with bromide or chloriodide of ethylene. These experiments show, on the one hand, the analogy between the silver and sodium oxides confirmed by the isomorphism of their anhydrous sulphates and chlorides, and on the other their difference from the group of diatomic oxides.

THE ACTION OF CERTAIN ANTISEPTIC VAPOURS ON THE RIPENING OF FRUITS.—MM. Lechartier and Bellamy give an account in the *Comptes Rendus*, lxxxiv. 1,035, of some experiments they made on the fermentation of apples when inclosed in vapours such as carbolic acid, camphor, and potassium cyanide. From their results it appears that no fermenting action took place in the apples surrounded by vapour of carbolic or hydrocyanic acids, and a slight action only in the one surrounded by camphor vapour. The camphor vapour, in fact, diminishes without entirely destroying the vitality of the cells. In this journal, also, there is an account of

experiments performed in the same direction by M. Gayon. He incloses the apples in vapours of chloroform, ether, and carbon disulphide, and his results agree with those of the first observers. The chloroform and ether act in the same manner as the carbolic and hydrocyanic acids; the carbon disulphide in the same way as camphor, permitting partial fermentation only.

A PROBLEM IN CHEMICAL AFFINITY.—In his work on "Gasometric Methods," Prof. Bunsen details an interesting series of experiments on the phenomena accompanying the explosion of hydrogen and carbon monoxide with a volume of oxygen insufficient for its complete combustion. From the results he deduces the conclusion that the ratio between the products of combustion ($H_2O : CO_2$) can always be expressed by *small whole* numbers (1 : 2, 1 : 3, 1 : 4, &c., and that it alters suddenly from one figure to the next by gradually increasing the amount of hydrogen. Deeming the nine experiments upon which the conclusion was based as insufficient for the establishment of a general principle, Prof. Horstmann, of Heidelberg, describes in the *Verhandlungen des heidelb. naturf. med. Vereins*, an extensive series of observations designed to test the truth of the law. Among his results the following facts are of interest. In exploding CO with gradually increasing quantities of $H_2 + O$, while the ratio between H and CO increased from 0·25 : 1 to 2·33 : 1, the ratio between H_2O and CO_2 gradually increased from 0·8 : 1 to 4·5 : 1, with no evidence of a predilection for rational numbers. Experiments on a mixture of CO and H, with gradually increasing amounts of O, led to the same results, showing no such regularity in the division of O between the two combustible gases as Bunsen's law would indicate. When aqueous vapour is present in the mixture less H and more CO unites with O, while the presence of CO_2 reverses the case. By gradually increasing the amount of O in the explosive mixture, it was noticed that the ratio between the resultant H_2O and CO_2 increased until it attained a maximum, when 35 per cent. of the combustible gases were oxidised, and then sank regularly to the ratio denoting complete combustion. The oxygen appears to be divided among the two gases according to the following law:—The ratio between the resultant H_2O and CO_2 is equal to the ratio between the residual H and CO multiplied by a co-efficient of affinity which is independent of the ratio between the combustible gases but dependent on the relative quantity of O present. This co-efficient of affinity varied between 4 and 6·4, showing that always more H relatively than CO is consumed, and hence that the affinity of O to H is greater than that to CO.

HALOGEN DERIVATIVES OF AMINES.—An attempt has frequently been made by chemists to replace the hydrogen in the hydrocarbon group present in amines, by Cl, Br, or I. These efforts have hitherto resulted simply in the substitution of the basic H atoms of the amine by halogens—as $C_2H_5.NCl_2$—or in complete decomposition. A. Michael (*Berl. Ber.*, x., 1644) has devised a method for accomplishing this end, which consists in first replacing these basic H atoms by acid residues, and then exposing to the action of a halogen ethyl-phthalimide,

$$C_6H_4(CO)_2N.C_2H_5,$$

yields in this way with Br a tribromo-ethyl-phthalimide.

DOUBLE SALTS WITH CYANIDE OF GOLD.—C. G. Lindbom publishes in the *Univ. Arsskrift* of Lund an exhaustive account of these compounds, which may be regarded as salts of the two acids, HCy.CyAu and $HCy.CyAu.Cy_2 + 1\frac{1}{2}$aq., neither of which, however, can be obtained pure for analysis on account of their tendency to decompose. Most of the auro salts unite directly with a molecule of the halogens; for example aurocyanide of sodium, $NaCy_2Au$, forms bromo-auricyanide of sodium, $NaCy_2AuBr_2 + 2$aq. Aurocyanide of ammonium, $AmCy_2Au$, is decomposed at 100°.

THE FOURTH NITROBENZOIC ACID.—Prof. F. Fittica has discovered lately a new nitro-benzoic acid, making the fourth of the isomeric acids, which has been contested by other chemists, especially as it fails altogether to harmonise with the theories at present accepted in regard to the structure of benzene derivatives. In the October session of the Deutsche chemische Gesellschaft, he strengthens his position by announcing the discovery of a fourth nitro-benzaldehyde, obtained by the action of H_2SO_4 on benzaldehyde and ethylic nitrate, which on oxidation is changed into the new nitrobenzoic acid, $C_6H_4.NO_2.COOH$.

INFLUENCE OF ISOMERISM ON THE FORMATION OF ETHERS BETWEEN ACIDS AND ALCOHOLS.—In the September session of the Russian Chemical Society, Prof. H. N. Menschutkin presented an elaborate paper on this subject based on observations of the formation of acetic ethers. The process consisted in inclosing molecular weights of an alcohol and acetic acid in glass tubes, immersing it in a glycerine bath at 154° for a certain time, and then rapidly cooling it, and titrating the unaffected acetic acid with baryta water. The results show that in regard to the rapidity and limits of etherification, the primary alcohols are sharply divided from the secondary, and the latter from the tertiary; and the saturated alcohols from the non-saturated. A regular decrease in the rapidity coincides with an increase in the molecular weight of the alcohol. As in many other series of experiments, methylic alcohol shows considerable deviations from the laws governing its higher homologues. In the case of non-saturated alcohols the rapidity is less than that of the corresponding primary alcohols, but greater than that of the corresponding secondary alcohols.

PHOSPHIDES OF TIN.—Since the introduction of phosphorus bronze, the compounds of phosphorus and the metals are receiving more attention. S. Natanson and G. Vortmenn describe (*Berl., Ber.*, x. 1459), several methods of preparing phosphides of tin, viz., throwing P on molten tin, melting a mixture of vitreous phosphoric acid, charcoal, and tin, and passing phosphorus vapours over molten tin in a hydrogen stream. These processes all yield a crystalline silvery white compound, containing from 1½ to 3 per cent. of P, and leaving on treatment with HKO a residue of pure SnP.

CHEMICAL ACTION OF LIGHT.—In a late number of the *Annales de Chimie et Physique*, M. Chastaing advances, in connection with a variety of observations on this topic, the theory that the chemical action of the various coloured rays on inorganic substances is dependent on refrangibility, blue and violet acting as reducing agents, red and yellow causing oxidation. Prof. H. W. Vogel attacks this opinion vigorously in the last session of the German Chemical Society, claiming that the nature of the substance causes the action to be one of reduction or oxidation. The union of H and Cl, which takes place so rapidly in violet light, is regarded as purely analogous to oxidation, and he alludes to Timiriazeff's late experiments, showing that the reduction of CO_2 by plants, proceeds more rapidly in red light than in green.

NOTES

AT the meeting of the Royal Society, on Thursday last, the *Times* states, the following were elected foreign members:—Marcellin Berthelot, of Paris; Joseph Decaisne, of Paris; Emil Dubois Reymond, of Berlin; Adolph Wilhelm Hermann Kolbe, of Leipsic; Rudolph Leuckart, of Leipsic; Simon Newcomb, of Washington; and Pafnutij Tschebytschow, of St. Petersburg. By this election the foreign list of the society is made up to its full complement of fifty members.

MR. ALEXANDER AGASSIZ, it is understood, proposes to spend the winter in the prosecution of scientific research in the Florida

seas, and will carry a line of dredgings and trawlings from Key West to Yucatan. Bearing in mind the very great success that has been experienced by the use of steel wire in taking soundings, he proposes to try the experiment of a steel rope $1\frac{1}{8}$ inches in diameter in the work of dredging and trawling. This, he thinks, will reduce the friction to such an extent as to greatly diminish the time and power necessary in making a cast of the dredge.

THE Emperor of Russia has conferred the order of St. Anne on Mr. Carl Bock, F.G.S.

THE *Monthly Microscopical Journal* expires with the number just issued for the last two months. It was edited from the commencement by Dr. Henry Lawson—who, after a long period of failing health, died on October 4 last—and has been in existence for nine years. Many valuable papers are contained in it, by distinguished authors, including the *Proceedings* of the Royal Microscopical Society, which will in future be published independently.

WITH reference to the brilliant meteor of December 6, we learn from Capt. Tupman that it will take him some time to determine the most probable path from the immense number of observations, good, bad, and indifferent, sent to him. Meantime he thinks that Prof. Herschel's preliminary calculation, not yet published, that it began fifty-three miles over Wigan, and burst thirty-three miles over a point half way between Great Orme's Head and Douglas in Man, with radiant 78° + 6° (γ Orionis), agrees better with the observations than any other path. We hope to publish Capt. Tupman's conclusions when his calculations are completed.

THE subject of Prof. Tyndall's six Christmas lectures to juveniles is to be Heat, Visible and Invisible. They commence on Thursday week.

MM. FEIL and FREMY, at last week's meeting of the Paris Academy of Sciences, read a paper describing a new process for the manufacture of rubies and other precious stones. The sensation created by these wonderful experiments has been so general that the Association of Jewellers have written to some of the papers stating that it was impossible for human art to compete against nature, that mysterious maker having at her disposal an indefinite number of centuries, which is not the case with any human worker. M. Daubrée, the Director of the School of Mines, has expressed the wish to open, in the public museum of that magnificent establishment, a gallery for the exhibition of minerals produced artificially. M. Feil has already produced in his glass foundry, and by the same process as rubies, an immense number of stones which can be compared with the most admirable crystalline productions of nature. Some of them are so inexpensive that they may be used for ordinary decorative purposes.

AN extraordinary but happily unsuccessful attempt was recently made upon the life of Mr. Russell, the Government Astronomer at Sydney, New South Wales. On September 8 a lad of about nineteen years of age left a box at the observatory for Mr. Russell, who, under the impression that it contained instruments of some kind, proceeded to open it. He found the lid a sliding one, similar to those adapted to ordinary instrument cases, and he had not drawn it far when he discovered that the affair partook more of the character of an infernal machine than anything else. The movement of the lid became rather stiff, and upon inspecting it and the box a little more closely he discovered at one end of the latter several grains of powder. The box was then taken into the open air, where it was investigated with special care. The lid was released, and there were found in the box at least 4½ lbs. of blasting-powder. In it were no less than sixteen matches, stuck with their sulphurous points in dangerous proximity to a sheet of sand-paper fastened to the under-side of the lid, the design being evidently to cause an explosion by the friction of the sand-paper against the matches; and there can be little doubt that this would have been effected had not great care been exercised in handling the affair. Besides the matches and powder, dangerous enough in themselves, a ginger-beer bottle, filled with gunpowder, and evidently intended to act as a shell, was found in the box; Mr. Russell has expressed his belief that altogether there was a sufficient quantity of explosive material present in the box not only to destroy life, but to blow the building down. One of the workmen at the observatory was arrested on suspicion.

THE first number is announced to appear on January 3 of a new weekly *Revue Internationale des Sciences*, under the editorship of Dr. De Lanessan, Professor of Natural History in the Medical Faculty of Paris. The publisher is Doin, of the Place de l'Odéon, Paris. Among the *collaborateurs* are several well-known names in France and Germany, England being represented by Mr. Francis Darwin.

THE expected change has taken place in the French Ministry, M. Faye has resumed his place as one of the Inspectors of Public Instruction, and Member of the Bureau des Longitudes. M. Bardoux, one of the most able members of the republican party, has been appointed Minister of Public Instruction. M. Bardoux is the President of the General Council of Puy de Dôme, who constructed, at the expense of the department, the observatory built on the top of the mountain of the same name.

M. BARDOUX is preparing a bill granting to the rectors of the several French academies (there is one in each of the eighty-two departments) the right to appoint the teachers in the public schools. Up to the present time these nominations were made by the prefects and too often the choice was influenced by political considerations.

THE enlarged council of the Paris Observatory held last Saturday a very interesting meeting. M. Faye has not resumed his seat as councillor. Several reclamations were read against the resolutions which had been adopted in the previous sitting. One of them was on behalf of the Bureau des Longitudes, asking to be allowed to have a voice in the presentation of the Director of the Observatory, as well as the Council and the Academy of Sciences. From the foundation of the Bureau des Longitudes up to 1854, when M. Leverrier was appointed director for life by Napoleon III., the Bureau des Longitudes had the control of the observatory. Each year the Bureau appointed one of its members to superintend the observations, and the custom was to reappoint the same member up to his death. Arago thus held his office by yearly tenure for more than a quarter of a century. The discussion of meteorological matters was begun, and the meeting adjourned till to-day. No formal proposition will be made to sever the International Bureau from the Observatory, the aim of certain members being confined to the establishment of a Central Board for Meteorology, which will give its advice on the organisation of the International Bureau, the Montsouris Central Observatory, the Puy de Dôme, the Pic du Midi, and any other establishment which may be founded for meteorological purposes.

WE are happy to state that the rumour widely spread in Paris of the death of Drouyn de Lhuys is unfounded, the learned gentleman having recovered, against almost all hopes. He will very likely resume his place in the several scientific societies which he had resigned.

M. MILNE EDWARDS has been appointed president of the French Scientific Association, which was founded by M. Leverrier thirteen years ago. Under the direction of M. Leverrier the association spent not less than 250,000 francs for scientific purposes, and has accumulated a sum of about 400,000

francs. The association is supported solely by voluntary contributions, and meets yearly at Paris. Many improvements are contemplated by the new president.

A CHIMPANZEE, about 2½ years old, has been recently placed on view at the Westminster Aquarium by Mr. Farini. It is very gentle in disposition, and is undergoing an education in the usages of civilised life at the hands of its keeper, Mr. Zack Coup. For some time it has been in one of the private houses at the Zoological Gardens, and there it caught cold. On its removal to the room at the Aquarium, where a temperature of about 70° is maintained, it improved, but the fog of yesterday (Wednesday) seemed to oppress it very much. It is suffering both in head and lungs, though it still struggles very successfully to be cheerful and entertaining. It is curious that Pongo's lungs were found all sound, though the few chimpanzees that have been exhibited in Europe have succumbed to lung disease. With the chimpanzee are also a very fine cynocephalus, a "sacred" monkey from India, and a number of monkeys less rare. There are close by a python, a boa, and two anacondas, and in order to insure that they hall not be hidden in rugs when visitors want to see them, they are at intervals exhibited by an Abyssinian girl, who goes through the ceremony of an incantation each time.

IN his introductory address at the opening meeting of the Royal Society of Edinburgh Sir Alexander Grant stated that the society was an emanation from the University of Edinburgh, from which it sprang on the suggestion of Principal Robertson in the latter part of 1782. Thus, in the same year that the University would celebrate its tercentenary the society would be able, perhaps conjointly, to celebrate its 100th birthday. In one essential particular it differed from the Royal Society of London. From the first the promotion of literature as well as science was the object of the Royal Society of Edinburgh. But it had been observed that the literary element in their proceedings had been gradually dwindling away. Sir Alexander had inquired as to the number of papers not connected with philosophical science which had been contributed during the last fifteen years, and it appeared to be considerably less than forty, or little more than two per annum. In the last fifteen years, out of about 370 ordinary Fellows of the Society, only about twenty had come forward to contribute papers other than philosophical or mathematical. The Council of the Society have awarded the Macdougall-Brisbane prize, consisting of a gold medal and 15*l.* 14*s.* 7*d.*, to Mr. Alex. Buchan, for his paper on "The Diurnal Oscillations of the Barometer." Prof. Balfour reported that the membership of the Society at present was 427, consisting of 373 ordinary and fifty-four honorary or non-resident Fellows.

ADVICES from the Howgate Arctic Expedition have been received up to the date of October 2, at which time the vessel had reached Niantitik Harbour, in Cumberland Gulf. The passage of forty-three days from New London had been very stormy, but, as far as reported, without any disaster. Mr. L. Kumlien, the naturalist of the party, had gathered some specimens, but did not find the promise of suitable collecting ground in the immediate vicinity very good. He hoped, however, to change his quarters to a better location.

WE learn from the *Isvestia* of the Russian Geographical Society, that at the beginning of September M. Prshevalsky had already started for Tibet. He is accompanied by an aid, M. Ecklon, four cossacks, and two soldiers. Having arrived at the conclusion that it will be impossible to reach Tibet by way of Lake Lob-Nor, he will try the route through Guchen and Hami, and thence to Zaidam and the upper parts of the Blue River. He expects to be at Lassa next year, about May or June, and if he succeeds, he will remain in Tibet for a year.

AFTER having penetrated last year for 160 miles up the Amu Daria, the Russian steamer *Samarkand* has penetrated this year as far as the fortified town Chardjui. A complete survey of the river was made, and considerable botanical and zoological collections were brought back by the officers on board.

RUSSIAN newspapers announce that the *Morning Dawn* reached St. Petersburg on December 1, having left the mouth of the Yenissei on August 21. This ship, or rather boat, 56 feet long, 14 feet wide, and drawing only 6 feet water, was built for the transport of wares up the Yenissei from Kureïka. It was never intended to go to sea, and "never," Prof. Nordenskjöld says, "so wretched a boat dared to enter the waters of the Arctic Ocean." Nevertheless Capt. Swanenberg, with a crew of four men, safely crossed on board this boat the Kara Sea, and reached the Russian capital after a hundred days' cruise. With a compass almost useless because of the deviation occasioned by the iron on board, and struggling with ice, he reached, on August 30, the Kara strait, where he experienced a heavy gale. On September 11 he was at Vardö. Thence, after a fortnight's stay, the *Morning Dawn* went in tow of a Norwegian steamer to Christiania, and further, in the same manner, to Göteborg, which was reached on November 3, and to Helsingfors, and finally to St. Petersburg The reception the gallant crew met with in the Norwegian towns was everywhere the most enthusiastic.

AT the last meeting, December 7, of the Russian Geographical Society, Col. Tillo read a report on the magnetical measurements made by M. Smirnoff in Russia. These measurements, accomplished with the utmost accuracy, embrace no less than 548 places, the declinations and inclinations having been measured at 287 places, and the former alone at 261. At the same meeting the Society resolved to enter into relations with different governments in reference to the establishment of polar meteorological stations, and to submit an elaborate scheme in connection with that subject to the next International Meteorological Congress.

GERMANY is still waging war against the illegal use of the doctor's title. A "Dr." Harmuth in Berlin who received his diploma from Philadelphia, was lately sentenced to pay 300 marks for using the prefix publicly.

M. POLYAKOFF, who was sent by the St. Petersburg Academy of Sciences for the exploration of the mammoth remains in the Government of Tomsk, has now returned to St. Petersburg after having made a journey in the Western Altai, the Kirghiz Steppe, and in the Seven Rivers' Province, where he visited the lakes Alakul and Balkash. He brings back very rich collections of animals and plants, and the results of his varied observations will appear in the *Memoirs* of the Academy.

THE scheme for telegraphing without wires, the *New York Tribune* states, by means of aërial currents of electricity, has been revived by Prof. Loomis. He has met with success in using kites for this purpose, a copper wire being substituted for the usual kite string. Signals were transmitted thus between kites ten miles apart. His new experiments are made in the mountainous regions of West Virginia, between lofty peaks. Continuous aërial currents are found at these altitudes, which will serve the purposes of the telegraph, except when rarely interrupted by violent disturbances of the atmosphere. A scheme is now on foot to test the merits of aërial telegraphy in the Alps.

THE *Journal of Forestry*, which started in the month of May last, in the interest of forest conservancy and management generally, maintains the reputation which the early numbers indicated. In recent numbers, Prof. Boulger, F.L.S., has contributed some papers, which are being continued, on "Cultural First Principles," in which he considers (1) "climate," (2) the "nature of the soil," and (3) "theoretical considerations (as to the treatment of the soil." In this latter portion of the subject drainage forms of course no inconsiderable part. Sloping plantations, it is shown, will seldom require artificial drainage,

for such a situation on high soils is quite suited for conifers, and if on heavy soils sufficient for oak and other hard-wooded trees. Mr. Boulger points out, what ought to be apparent to all, that the growth of rushes, the wood rush (*Luzula*), the bog asphodel (*Narthecium*), a yellow star-like plant, the tufted hair-grass (*Aira cæspitosa*), or of mosses on the surface, are sure indications of the absolute necessity of drainage. Though these are facts with which a practical forester is well acquainted, it is nevertheless necessary to impress them upon the minds of all interested in forest produce. Some useful hints as to the preservation of timber for fencing, or in damp underground situations, are given in the number of the *Journal* for the current month.

R. PICTET describes some interesting experiments (*Arch. Sc. Phys.*, lix.) made for the purpose of determining the conditions under which transparent or non-transparent ice is formed. It was ascertained that water frozen in a vessel dipped in a cold glycerine solution formed perfectly transparent ice as long as the temperature ranged between 0° and − 1·5°. If the solution was cooled below − 3°, the ice was whitish and of a less specific gravity, these properties being intensified with the lowering of the temperature. No difference in the melting-point or amount of warmth required for melting was observable among the various varieties of ice. The opacity of ice results from an irregular arrangement of the ice-crystals, as well as from the presence of small bubbles of air—less than ½ mm. in diameter—which are mechanically inclosed. They can be removed by slowly conducting through freezing water large bubbles of air which carry with them the small bubbles.

AT a public meeting held in the Cheltenham Masonic Hall on December 15, Sir F. Abbott in the chair, it was resolved to institute a "Cheltenham Philosophic Society," which should hold its meetings during the winter months. Upwards of fifty gentlemen signified their desire to become members, and a committee was formed to draw up rules to be submitted at a future date to a general meeting for their sanction.

DR. JOHN RAE asks us to state that in his paper on Eskimo skulls read at the Anthropological Institute on May 8 last, and published in the newly-issued number of the *Journal* of the Institute, he by mistake called the Western Eskimo Brachycephalic, whereas they are Mesocephalic.

IN the letter on the meteor last week, p. 124, "8 (± 2) χ Lyræ," should be 8 (± 2) times Lyræ.

THE additions to the Zoological Society's Gardens during the past week include a pair of Musk Deer (*Moschus moschiferus*), from Central Asia, presented by Sir Richard Pollock; a pair of Axis Deer (*Cervus axis*), from India, presented by Dr. Carl Siemens; a Diana Monkey (*Cercopithecus diana*), from West Africa, presented by Mr. P. Spink; a Bonnet Monkey (*Macacus radiatus*), from India, presented by Mr. T. H. Evans; a Patas Monkey (*Cercopithecus ruber*), from West Africa, a Red-backed Saki (*Pithecia chiropotes*), a Red-faced Spider Monkey (*Ateles paniscus*), two Kinkajous (*Cercoleptes caudivolvulus*), a Coati (*Nasua nasica*), an Azara's Fox (*Canis azaræ*), a Black Vulture (*Cathartes atratus*), from South America, deposited two Schlegel's Doves (*Chalcopelia puella*), from West Africa, two Lesser Razor-billed Curassows (*Mitua tormentosa*), from South America; two Waxwings (*Ampelis garrulus*), four Bullfinches (*Pyrrhula rubicilla*), European, purchased; four Common Waxbills (*Estrelda cinerea*), two Cinereous Waxbills (*Estrelda cærulescens*), six Orange-cheeked Waxbills (*Estrelda melpoda*), two African Silver Bills (*Munia cantans*) seven Yellow-rumped Seed Eaters (*Crithagra chrysopyga*), from West Africa, received in exchange; a Chinchilla (*Chinchilla lanigera*), born in the Gardens.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The Syndicate, appointed in May, 1875, to consider the requirements of the university in different departments of study, have just issued their fourth report on the subject. After stating that in their opinion the inter-collegiate system should be further stimulated and supplemented by the institution of university readerships, and that by a more complete organisation the requisite provision for teaching and the encouragement of research might be to a considerable extent made, they nevertheless are of opinion that certain subjects of great importance are not now represented in the professoriate, the absence of which from such representation constitutes a serious defect in the Cambridge system, and they recommend that professorships amongst other subjects should be created as soon as the resources of the university permit, in comparative philology, mental philosophy and logic, physiology, English language and literature.

The Syndicate, with regard to existing professorships, suggest that the Professorship of Mineralogy should be for the future a Professorship of Mineralogy and Mineral Chemistry.

There are other subjects of scarcely less importance of which there ought always to be recognised teachers in the University, although the Syndicate are not prepared to say that the teacher ought always to have the status of a professor. Such are analytical chemistry, morphological and physiological botany (as distinguished from systematic botany), comparative anatomy (as distinguished from zoology), pathological anatomy.

In other subjects, again, it is desirable that the University, without establishing permanent offices, should have the means of appointing professors or other teachers from time to time when there is the opportunity of securing the services of a specially competent person. Such are the theory and history of education, as also some special departments of natural science.

The following is the Natural Science Tripos' list for this year:—Class I.—(2) Ds. Bower, Trinity; (4) Cullen, Christ's; (1) Fenton, Christ's; (1) Greaves, Christ's; (2 and 3) Hill, Downing; (1) Ohm, Emmanuel; (3) Sedgwick, Trinity. Class II.—Ds. Harrison, Christ's; Holthouse, Trinity; Houghton, St. John's; Murton, St. John's; North, Sidney; Taylor, E. F. Vinter, Sidney. Class III.—Ds. Allen, St. John's; Buckmaster, Downing; Foster, Trinity; Wallis, St. John's; Weldon, Caius. The numbers indicate the subjects for knowledge of which the candidates are placed in the first class as follows:—1. Physics, chemistry, and mineralogy. 2. Botany. 3. Zoology and comparative anatomy, human anatomy, and physiology. 4. Geology.

EDINBURGH.—The second meeting of the session of the Edinburgh University Chemical Society, was held on the 12th instant, John Gibson, Ph.D., F.R.S.E., vice-president, in the chair. J. S. Thomson communicated a paper on solutions of litmus, in which he explained the preservation of the colour of such solutions on exposure to the air by the action of the air preventing fermentation. He also read a paper on the Determination of Melting Points.

TECHNICAL UNIVERSITY.—It may be remembered that a Committee of the City Companies has been for some time at work elaborating a scheme for founding a technical university in London. The last step taken by the Committee was to procure reports and suggestions from six specially nominated referees, viz., Prof. Huxley, Col. Donnelly (of the Science and Art Department), Capt. Douglas Galton, Mr. H. T. Wood (Assistant-Secretary of the Society of Arts), and Mr. Bartley (of the Science and Art Department). After having decided on these names, the Committee adjourned to allow time for the preparation of the reports. On Thursday last week they met again, and the reports were laid before them. After some discussion the further consideration of the subject was adjourned till January 17.

TAUNTON COLLEGE SCHOOL.—The Science Scholarship at Keble College, Oxford, is awarded to Mr. R. G. Durrant, of the Taunton College School. This is the fourth brilliant success that the school has gained in the last five months, and it is probably the last. The able science master, Mr. Shenstone, leaves at Christmas to take a Science Mastership at the revived Exeter Grammar School. He will not be replaced at Taunton, and the science teaching, which, after years of toil against obstacles, is just beginning to bear fruit, will become a thing of the past.

AUSTRIA.—The Austrian Government has for a number of years been accustomed to bestow liberal grants to the more promising students in the universities, under the condition that the recipients shall make use of them to undertake a course of study in the German universities. The results of this plan seem to be satisfactory, for we notice that this winter an unusually large number of students in all branches have been sent to the various universities in Germany.

A BERLIN POLYTECHNIC.—Berlin, with all her numerous educational establishments, has lacked hitherto a polytechnic such as is to be found in most of the German industrial centres at the present day. This want will soon be repaired, a commission having completed the plans for an extensive institution which will embrace nearly every branch of technical education. The plans for the necessary buildings have already been prepared, and as there is but little doubt that the Prussian Chamber of Deputies will grant the 9,300,000 marks required, the work of erection will commence next spring. On account of the extensive character of the proposed edifices, five years will be required for completion.

SOCIETIES AND ACADEMIES

LONDON

Mathematical Society, December 13.—C. W. Merrifield, F.R.S., vice-president, in the chair.—The Rev. W. Ellis was elected a member.—Mr. S. Roberts read a paper on normals, which contained theorems depending on the invariants and covariants of the quartic equation representing a pencil of four normals to a conic, and drew attention to the remarkable cubic locus of the points of possible concurrence of these normals at the vertices of a given inscribed triangle.—Dr. Hirst and Mr. J. J. Walker spoke on the subject. Prof. Cayley, F.R.S., read a paper on "the geometrical representation of imaginary quantities and the real (m, n) correspondence of two planes."

Linnean Society, November 15.—Dr. Gwyn Jeffreys, F.R.S., vice-president, in the chair.—Mr. J. Jenner Weir exhibited a case of Alpine butterflies, interesting for their similarity to, though not specifically identical with, those obtained by the naturalists of the Polar Expedition.—Three papers on the Arctic fauna followed. I. Report on the Insecta including Arachnida, collected by Capt. Feilden and Mr. Hart during the recent Arctic expedition, by R. McLachlan. It seems there were obtained of Hymenoptera 5, Coleoptera 1, Lepidoptera 13, Diptera 15, Hemiptera 1, Mallophaga 7, Collembola 3, Araneida 6, and of Acarida 6 species, namely, a total of 57 species. Bearing in mind these are from localities between 78° and 83° N. lat., that among them are thirty-five specimens of gaily-coloured butterflies and two species of humble-bees, and it becomes evident the insect-fauna of this so-called "land of desolation" is, after all, not so meagre as anticipated. The paucity of beetles and abundance of butterflies are each striking features. From variations in certain well-known species obtained, Mr. McLachlan suspects they represent a local fauna, and he regards the latter as having affinity to the Lapland fauna, inclining to think both are but lingering remnants of a once former and extensive circumpolar fauna.—II. Preliminary notice on the surface fauna of the Arctic Seas as observed in the recent Arctic expedition, by Dr. Edw. L. Moss (late surgeon, H.M.S. *Alert*). The author observes that the seas north of the Greenland settlements are subject to such varying conditions at different seasons of the year that their surface-fauna cannot be supposed to be very constant. Nevertheless, judging from what fell under his observation during the voyage, he divides the watery area into three zoological regions: (*a*) A district in the latitude of Melville Bay, temporarily monopolised by infusoria, *Peridinea*: (*b*) a north-water region inhabited by Pteropods, Tunicates, and Hydrozoa; and (*c*) a sub-glacial region comparatively lifeless, so far as sea-surface implies.—III. On the annelids of the British North Polar Expedition (1875-76), by Dr. W. C. McIntosh. This collection, dredged between 70° and 82° N., was not so rich in numbers or species as that procured by the storeship *Valorous* in Davis Straits, but some eight species were got which were not among the latter's collection. None are new, but notwithstanding they help to render clear some points in the geographical distribution of the marine worms, so far as the circumpolar area is concerned.—Dr. H. Trimen exhibited specimens of the Olibanum, or Frankincense tree (*Boswellia carterii*, Birdw.), gathered by Mr. J. Collins from the trees planted at Aden. Dr. Trimen, in making some remarks on the variability of the foliage of the species of *Boswellia*, expressed the opinion that *B. Bhau-Dajiania*, Birdw., was not specifically separable from *B. Carterii*. *B. Frereana*, which yields the fragrant resin called "Luban Metyi," and which Hanbury considered to be the African "Elemi," is much chewed by Orientals, but rarely imported into England. It is found in the Somali land, where Hildebrandt recently collected it.—The following gentlemen were elected Fellows of the Society: Mr. W. S. Lawson, Mr. W. Joshua, and the Rev. M. A. Mactherd.

Geological Society, December 5.—Prof. P. Martin Duncan, F.R.S., president, in the chair.—Dr. Isaac Bayley Balfour, David Burns, Samuel Cooke, Henry Drummond, Sandford Fleming, Rev. John Hodgson, William Etheldred Jennings, Henry Merryweather, Robert Robinson, Martin Stewart, George Eastlake Thoms, Robert F. Tomes, and Irwine J. Whitty, were elected Fellows of the Society.—A portrait of Mr. J. Evans, D.C.L., F.R.S., V.P.G.S., was presented by the President.—The following communications were read:—On the building-up of the White Sinter Terraces of Roto-Màhànà, New Zealand, by the Rev. Richard Abbay, M.A., F.G.S.—Additional notes on the Dimetian and Pebidian Rocks of Pembrokeshire, by Henry Hicks, F.G.S. The additional facts communicated by the author show that at a distance of about ten miles to the east of the Dimetian axis of St. David's there is another ridge of these rocks, which also runs nearly parallel with it. This is also flanked by Pebidian and Cambrian rocks, and made up of rocks like those in the St. David's axis. The Dimetian formation, so far as it is at present known, consists chiefly of the following rocks:—1. Quartz porphyries, containing frequently perfect quartz crystals (double pyramids), subangular masses of quartz, and crystals of felspar in a felspathic matrix. 2. Fine-grained greyish quartz-rocks, very compact, and interstratified with the above. 3. Ashy-looking shales of a dull green colour, sometimes highly indurated, but usually showing lines of lamination. Microscopically these show basaltic characters, and are probably greatly altered interbedded basaltic lavas. 4. Compact granitic-looking rocks. 5. Quartziferous breccias. 6. A series of compact quartzites and crystalline schists, interstratified by green and purple altered basaltic lavas, with a slaty and schistose foliation, and by some dolomitic bands. Of the *Pebidian* formation new areas were added, and the portions described in the author's previous paper were further extended, and details as to the chief mineralogical characters added. At the base of the series resting unconformably on the Dimetian is seen an agglomerate composed of large angular masses of a spherulitic felstone, pieces of quartz and quartzites, indurated shales, crystalline schists, &c., cemented together by a sea-green matrix of felstone. These are followed by conglomerates of the same materials, which are again succeeded by indurated shales, often highly porcellanitic in character, with a conchoidal fracture. These are followed by a thick series of silvery white and purplish shales and green slates, alternating with fine and rough ashes, often conglomeritic, hornstone breccias, felstone lavas, &c. The series, as exhibited at St. David's, has a thickness of over 8,000 feet; and as it is everywhere, so far as yet seen, overlapped unconformably by the Cambrians, it may probably be of much greater thickness. It evidently consists very largely of volcanic materials, at first derived from subaërial, but afterwards from submarine, volcanoes. These materials, however, were also undoubtedly considerably aided by sediments of a detrital origin. The whole series shows that the sediments have undergone considerable changes, but yet not sufficient to obliterate the original characters, and the lines of lamination and bedding are usually very distinct. That they were altered nearly into their present state before the Cambrian sediments were deposited upon them, is clear from the fact that the pebbles of the Cambrian conglomerates which rest immediately on any portion of the series are almost invariably made up of masses of the rocks below, cemented by gritty materials on an unaltered matrix, and from which the pebbles may be easily removed. The great conglomerates at the base of the Cambrians, everywhere in Wales, indicate that there were beach- and shallow-water conditions over those areas at the time, and that the sea was then encroaching on an uneven land, becoming gradually depressed to receive the subsequent Cambrian sediment.—On some pre-Cambrian (Dimetian and Pebidian) rocks in Caernarvonshire, by Henry Hicks, F.G.S. In this paper the author gave an account of the special examination of the great ribs of so-called intrusive felspathic and quartz porphyries which are found associated with the Cambrian rocks in Caernarvonshire, made by him in company with Prof. Hughes,

Mr. Hudleston, and Mr. Homfray last summer.—On the pre-Cambrian rocks of Bangor, by Prof. T. McKenny Hughes, F.G.S. The author described a series of slates, agglomerates, and porphyritic rocks which, near Bangor, are seen to pass under the Cambrian and seem to rest conformably upon the quartz felsites and granitoid rocks of Caernarvon. He considered that in the main the Bangor beds were the equivalents of the Pebidian of Dr. Hicks, while the Caernarvon beds nearly represented his Dimetian. But he thought there was as yet no proof of an unconformity between these formations.—An appendix by Prof. Bonney, on the microscopical examination of the rocks referred to, accompanied this paper.

Royal Microscopical Society, December 5.—Mr. H. C. Sorby, president, in the chair.—The president announced that in consequence of the death of Dr. Lawson it had become necessary to reconsider the subject of publication, and the Council had, after careful attention to the matter, decided in future to publish their own proceedings.—A paper by Herr Zeiss on Abbé's apertometer was read by Mr. Ingpen, who exhibited the apparatus to the meeting and further explained its construction and method of application by means of black-board diagrams. Mr. Ingpen also described the method of measuring angular apertures last adopted by Mr. F. H. Wenham.—A paper by Mr. F. A. Bedwell on *Cephalosyphon* was read by Mr. Slack, who afterwards explained the structure of this rotifer, and pointed out the special features to which attention was drawn by the author of the paper.—Another paper by the same author on a new method of examining *Actinia mesembryanthemum*, was read by Mr. Chas. Stewart; it was illustrated by drawings, some of which were enlarged upon the black-board.

Institution of Civil Engineers, December 11.—Mr. George Robert Stephenson, president, in the chair.—A description of Cofferdams used at Dublin, Birkenhead, and Hull, by Mr. William James Doherty, Assoc. Inst. C.E., was read.

GÖTTINGEN

Royal Academy of Sciences, August 24.—The division of a language into several different languages, by M. Benfery.—On the earthquake of Iquique on May 9, 1877, and the tidal movements thereby produced in the ocean, by M. Geinitz.

November 7.—Report on the Physical Institute (department of experimental physics), from 1871 to 1877, by M. Riecke.

November 14.—D instead of N, by M. Benfery.—Contributions to physiography of rock-forming minerals, by M. Lang.

November 21.—Antiquities in the south-west of Switzerland, and in Turin, by M. Wieseler.—On the secondary intestine of the echinoideæ, by M. Ludwig.—Obituary notices of M. Hartmann and M. Marx.

PARIS

Academy of Sciences, December 10.—M. Peligot in the chair:—The following papers were read:—On some applications of elliptical functions (continued), by M. Hermite.—On invariants, by Prof. Sylvester.—On the arrangements which, in the system of a navigation sluice with single oscillation, conduce to the maximum of production and the minimum of expense of construction, by M. De Caligny.—On the development of eggs of the phylloxera of the oak, and the phylloxera of the vine, by M. Boiteau.—M. Volpicelli sent a note tending to prove, by means of potential, that induced electricity of the first species has no tension.—Application of Leyden jars of large surface for distributing, at various points, the effect of the current from a single source of electricity, with strengthening of the effect, by M. Jablochkoff. Connecting one surface of such an apparatus (called in this case an *exciter*) with one of the conductors of a machine which gives alternate currents, an alternating current is got by the other surface of the exciter and the second conductor (or the earth), more powerful than the current given directly by the machine. If a series of exciters with surfaces of nearly 500 square metres be thus connected with a machine which gives a spark equivalent to that of six or eight Bunsen elements, a voltaic arc of 15 to 20 mm. is obtained, and carbons of 5 mm. diameter are reddened to an extent of 6 to 10 mm. from their extremity. Such effects are utilised in electric lighting.—On the law of absorption of radiations through bodies, and its employment in quantitative spectrum analysis (continued), by M. Govi. He shows how *surfaces of chromatic absorption* may be obtained by means of the analysing photometer, measuring the various simple radiations which take part in a complex radiation.—On some properties of chloride of calcium, by M. Ditte. He deals with the calorific phenomena accompanying the reaction of water with this chloride. If the latter be anhydrous, a heating is observed, and fresh additions of water cause successive heatings; but if the chloride be hydrated, its mixture with water produces at first a considerable cooling followed by heating if some more of the solvent be added.—Application of palladium wire to determination of the hydrocarbons mixed in a small proportion with air, by M. Coquillion. It is necessary to operate with a cherry-red, near white-red. The results agreed with theory.—On the development of the functions of M. Weierstrass according to the increasing powers of the variable, by M. André.—On the lesions of the nervous system in diphtheritic paralysis, by M. Dejerine. There is an atrophy of the anterior roots, which follows destruction of the cells of the anterior horns of the spinal cord, by a process similar to that of myelitis.—Orography, by M. Schrader. The author presented a geographical map of Mont Perdu, made with his orograph, which consists of a circular paper-covered plate with central vertical axis carrying a sleeve which can turn round freely. On the top of the sleeve is a telescope, the movements of whose frame in the vertical direction are communicated to a pencil, and transformed by gearing into to and fro movements. If the telescope describes a circle round the horizon, the style describes a corresponding circle on the plate; if the telescope goes up or down, the trace produced is further from or nearer to the central axis. A spirit level being fixed to the telescope, the circle made when it is even, gives a means of estimating the heights and depressions.—On the folding of the lacustrian strata of Auvergne in Central Limagne, and its consequences, by M. Olivier.—Influence of soil and forests on climate; temperatures of air layers over woods; consequences as regards vegetation; effects of currents arising from differences of temperature under wood and beyond wood, by M. Fautrat. The frigorific action of the forest is very manifest in the hot season. Under pines in September the temperature is lowered 1·60°. Pure sand raises the temperature of a place. Leafy woods, during vegetation, produce a slight lowering of temperature in the atmosphere above. Above pines, in the daytime, there is always a rise of temperature, from the solar heat being retained by the vapours enveloping the tree-tops. From the differences of temperature within and without woods, a current arises in the wood from below upwards, and round the woods course lateral currents from the wood to the plain.—On the disinfecting properties of cellulosic substances carbonised by concentrated sulphuric acid, by M. Garcin.

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