THURSDAY, APRIL 12, 1877

THE ARCTIC BLUE-BOOK

THE admirably illustrated volume which has just appeared in this uninviting form, tells a tale of adventures as interesting and heroic as anything in the long record of Arctic discovery. It throws little light on the question to which public attention has been too much directed this winter—whether any of the misfortunes of the expedition were due to the officers who started the sledge p arties without adequate supplies of lime-juice. The report of the Scurvy Committee will appear in a few days. In the mean time it is clear that every pound weight on the sledges was calculated with the utmost care; that wherever anything was to be used in a fluid state an adequate corresponding supply of fuel needed to be carried; that none of the officers, judging from the experience of previous sledge expeditions, seem to have anticipated scurvy; and above all that the work of all the parties on and at the edge of the hitherto untrodden Palæocrystic sea proved so frightfully severe that if lime-juice in abundant rations had been taken the sufferings of the men would probably have been only mitigated. It is to the severity of the work, not to the absence of lime-juice, that we believe the terrible outbreaks of scurvy which crippled the sledging parties to have been really due. In Commander Markham's Journal, written on the spot a month out from the ship, he says, "The invalids are not improving, and we are inclined to believe that they are all attacked with scurvy, though *we have not been led to suppose that there is any probability of our being so afflicted and are ignorant of the symptoms.*" Swollen knees and ankles are of frequent occurrence in all Arctic sledging expeditions, and they were prepared to expect as much. Scurvy had scarcely been thought of, and the fact that it had not been thought of by officers whose lives and the lives of their men depended on their forethought, and who had studied the experience of their predecessors with anxious care, is sufficient to show that, *à priori*, there was little or no probability of its appearance. After the experience of Markham's, Aldrich's, and Beaumont's parties, no future travellers over the "Palæocrystic" will omit their lime-juice, but these officers seem all to have been unprepared for scurvy. Aldrich says in his journal, 38th day out :—

"The men are nearly all suffering a great deal with their unfortunate legs, which appear to get worse every day. This we all feel to be very disappointing, as it affects the journey, and although stiff limbs were expected, everyone thought the stiffness would wear off in time. It seems, however, inclined to hang on, and sets at defiance all the limited medical skill we possess among us, and to scorn succumbing to turpentine liniment, bandages, good 'elbow grease,' &c. With regard to bandages, I am almost afraid to apply them, for some of the limbs are not at all healthy looking; the slightest pressure of the finger leaves a dent which remains a considerable time; and although I have given the most stringent orders about lacing the foot gear on very slackly, I find the loosest moccasin string cuts an ugly, red-looking mark. One or two cannot even bear anyone to lie against them, which makes it excessively inconvenient at night, although everyone is very good tempered, and complaints are reduced to a minimum."

Lieut. Beaumont's party was accompanied by Dr. Coppinger till May 4, and Beaumont says :—

"It was at the end of this journey, May 6, that J. Hand, A.B., one of my sledge crew, told me in answer to my inquiry as to why he was walking lame, that his legs were becoming *very* stiff, he had spoken to Dr. Coppinger about them, but attributing the stiffness and soreness then to several falls that he had had, he did not think much of it, before that officer's departure; now, however, there was pain as well as stiffness, and both were increasing. I directed him to use liniment before he turned in, which he afterwards said made him better."

This was the first beginning of scurvy, but even a medical officer attached to the expedition had obviously supposed it the mere common swollen leg and ankle of ordinary Arctic sledging. Beaumont goes on :—

"On coming into camp I examined Hand's legs, and found the thighs discoloured in patches, and from his description of the stiffness and pain I suspected scurvy. *I had no reason to expect it, indeed I had never thought of it*, but the striking resemblance of the symptoms to the ones described in the voyage of the *Fox*, as being those of Lieut. Hobson, who suffered severely from scurvy, suggested it to my mind, and my suspicions were confirmed by Gray, the captain of my sledge, an ice quartermaster, who, in his whaling experience, has seen much of it. He, however, led me to believe, at the same time, that it would probably wear off, saying that many of the men in whale ships who have it lying ''twixt the flesh and the bone all the winter,' as he expressed it, wear it off by the regular exercise and work of their occupation when the spring comes; it was a good sign, he said, that it should come to the surface. Thus, from the 7th until the 10th I waited, hoping that his words might prove true. I was very reluctant to order Lieut. Rawson to return, it was like sending back half the party; it would be, I felt, a great disappointment to him to turn back then, and his advice and assistance would be a very great loss to me, but the indications of the disease and their aggravated nature became too plain to be misunderstood—sore and inflamed gums, loss of appetite, &c., all pointed too clearly to scurvy; so on May 10 it was arranged that Lieut. Rawson, with his party, should take Hand back, deciding, on his arrival at Repulse Harbour, whether to cross over to the *Alert* or go on to Polaris Bay. I at the same time called upon the remainder of my men to say honestly if they suspected themselves to be suffering from the same disease, or could detect any of its symptoms, as in that case it would be better for the party to advance reduced in numbers than to be charged with the care of sick men. I did this because two of them had complained of stiff legs after the hard work on the snow slopes, but they all declared themselves to be now perfectly well, and most anxious to go on."

So much for the scurvy question. The Blue-Book makes it manifest that neither the commanders of the sledge parties nor Captains Nares and Stephenson, nor Dr. Coppinger suspected that the sledge parties would be in danger of that terrible disease.

The most interesting part of the story is told in the daily journals kept by Markham, Aldrich, and Beaumont. No reader of these simple and modest records will doubt that "the ancient spirit is not dead" which has carried the Union Jack in triumph over every ocean, and planted it wherever honour and danger were most surely to be found.

Markham and Aldrich left the *Alert* on April 3, travelling in company to Cape Joseph Henry—latitude 82°50. From that point Markham struck straight north on April 11, with fifteen men and three sledges, weighing in all 6,079 lbs., or 405 lbs.—3⅝ cwts.—per man. They carried two ice-boats with them, the first of which, weighing 740 lbs., had to be abandoned on April 19, while the second, weighing 440 lbs., their only chance of safety if the ice should break up, had to be abandoned on May 27, while they were still seven miles or so from the nearest land. The ice on the surface of the floes was covered generally with snow some three feet deep, and the men sank in it beyond their knees. If the Palæocrystic sea had been a decently level plain covered with loose powdery snow, the work to get to the pole would have been hard enough. The party found it much such a place as South Kensington might be after an earthquake had toppled half the houses into ruin. There was seldom a floe or flat ice-surface of any extent—rarely as much as a mile in any direction—never more than a mile and three-quarters. "For the last ten or fifteen days of our outward journey," says Markham, "floes were few and far between, and it might almost be said that our road lay entirely through hummocks and deep snow-drifts." A hummock is a huge mass of ice-blocks piled up like builder's rubbish. The highest mass measured was 43 feet 2 inches, but many were observed which exceeded that height, and were estimated as between 50 and 60 feet. On the heavier floes were high hillocks apparently formed by snow drift, the accumulation probably of years, resembling diminutive snow mountains, and varying from 20 to over 50 feet in height. It was across this sort of material that the party had to drag themselves to the pole. They found that they could scarcely ever get along without "double banking." They had two sledge crews for three sledges, and they had calculated to pull the heavy sledge by the whole fifteen men, and to return for the lighter sledges which were to come up together, each dragged by its seven or eight men. Thus three miles of ground would have had to be traversed for every mile made good. In fact even the smaller sledges needed almost always the whole fifteen men, and after the larger ice-boat was abandoned on April 19th, there was little difference of weight between them. Thus each mile in advance cost five miles walking, three of them full loaded, two through the snow and without the steadying support of the drag-ropes. The back journeys were found almost as fatiguing as the others. From April 16th (Cape Joseph Henry) to May 12th, the most strenuous efforts carried them from 82.49½ to 83.20.26, *i.e.* 31 minutes northing, or about thirty-six English miles in twenty-six days, an average of 1¼ miles a-day advanced, and of seven miles walked. The advance was soon impeded by the illness of the men. It is on the 14th—eleven days from the ship, three days from the depot and last land at Cape Joseph Henry—that we first find the ominous entry, "pain in his ankle and knee, both of which exhibited slight symptoms of puffiness." On the 16th the patient had to be put on one of the sledges, so that already there were only fourteen men at the drag-ropes and 160lbs. more to drag. "On the 17th another man cannot drag but is just able to hobble after us," carrying, that is to say, his own weight, but only for half the day. On the 19th both had to be carried, and another man fell out from the drag-ropes. Although they dropped their ice-boat, 740 lbs. weight, on the 19th, on the 25th a fourth man is reported weak. A fifth man "can scarcely walk" on May 2nd, and on May 3rd all five are "utterly helpless and therefore useless." On May 4th "more of the men are complaining of stiffness and pain in their legs, which we fear are only the premonitory symptoms." Here is a glimpse of the party on May 6th :—

"The sick men are invariably the cause of great delay in starting, as they are perfectly helpless, being even unable to dress or undress without assistance. We appear to have arrived at a perfect barrier of hummocks and portions of floes, all broken and squeezed up and covered with deep snow. It is possible we may be able to penetrate these obstacles, eventually reaching larger and more level floes, on which we may be able to make more rapid progress. We ascended one large hummock, from the summit of which the prospect was anything but encouraging—nothing but one vast illimitable sea of hummocks. The height of this hummock was ascertained by means of a lead line, and was found to be from its summit to the surface of the snow at its base 43 feet 3 inches. It did not appear to be a floe-berg, but a mass of hummocks squeezed up and cemented together by several layers of snow, making it resemble one huge solid piece. The travelling has been exceedingly heavy, and with the weights on the sledges augmented, the deep snow, and a third of our band *hors de combat*, it is next to impossible to advance many feet without resorting to 'standing pulls,' or the endless 'one, two, three, haul.'"

On the 7th they had to "advance with one sledge, *unload it*, return with it empty, and then bring on the gear and invalids." On the 8th "the interior of our tents have more the appearance of hospitals than the habitations of strong working men. In addition to the cripples, four men are suffering from snow blindness." It is in this condition that they struggle through the sea of hummocks.

"The hummocks around us are of different heights and bulk, varying from small fragments of ice to huge piles over 40 feet high. Some of these larger ones are simply masses of squeezed-up ice, whilst others of great magnitude, but perhaps not quite so high, are the regular floe-bergs. Between these hummocks, and consequently along the only road that is practicable for our sledges, the snow has accumulated in drifts to a great depth, and these forming into ridges render the travelling all the more difficult. Some of the tops of these ridges are frozen hard, and it is no uncommon occurrence to step from deep snow through which we are floundering up to our waists, on to a hard frozen piece, and *vice versâ*. Occasionally these ridges are only partially frozen, sufficiently only to deceive one, which makes it exceedingly disagreeable and laborious to get through."

On May 10, "with five out of our little force totally prostrate, four others exhibiting decided symptoms of the same complaint," Commander Markham sees that it would be "folly to persist pushing on." They have been forty days out and are only provisioned for thirty more. On the 12th those left decently strong go out in the morning for their farthest north—1½ miles out from the camp, 399½ from the pole. There they sang the "Union Jack of Old England," the "Grand Palæocrystic Sledging Chorus," winding up, like loyal subjects, with "God save the Queen." When they got back to the sledge they broached a magnum of whisky sent for the purpose by a genial and henceforth famous ecclesiastical potentate, "the Dean of Dundee," smoked a single cigar apiece, presented them *ad hoc* before leaving the ship, and consumed the solitary hare they had shot on the way out. The story of their return journey is intensely interesting. On June 7, when they had still forty miles to go, and doubtless were near the end of their provisions, Lieut. Parr—whose untiring energy and admirable "road-making" made him the very perfection of companions for Commander Markham—started alone on a desperate walk to the ship for assistance. They had only *eleven good legs out of thirty-four in the party*, and "even some of these are shaky," says Markham on May 25. Fortunately two of them, both excellent, remained to Parr a fortnight later. The first death of the party happened next day, but the day after, June 9, late in the evening, relief came. Parr's wonderful walk—far more memorable than Weston's or O'Leary's—probably saved the lives of one or two men of the gallant party which has come nearest of any human being, possibly nearest of any living creature, to the solitude of the North Pole.

Space alone prevents us dwelling on the equally interesting work done by Aldrich on the northern shore of the American continent, and by Beaumont on North Greenland. The names of Markham, Aldrich, Beaumont, Parr, and Sir George Nares, have been added definitively to the long list of our Arctic heroes. Few things have been finer in seamanship than Sir George Nares' passage up Smith's Sound and Robeson Channel into the Palæocrystic sea and home again. The skill with which he devised and combined the exploring parties and prepared everything so that the utmost was accomplished which it was possible for brave men to accomplish without useless sacrifice of human life, has scarcely yet received sufficient acknowledgment either from his country or from the public.

ANTHRACEN

Anthracen; its Constitution, Properties, Manufacture, and Derivatives, including Artificial Alizarin, Anthrapurpurin, &c., with their Applications in Dyeing and Printing. By G. Auerbach. Translated and edited by William Crookes, F.R.S. (London: Longmans, Green, and Co., 1877.)

FROM the extent to which the anthracene and artificial alizarin industries have grown within the last few years, and the interest taken in them in England, it has been deemed advisable to bring forward an English edition of Auerbach's text-book on the above subject. This work has been carried out by Mr. W. Crookes, from a revised manuscript supplied by the author.

In the author's preface to this volume we are told that since the production of the first German edition four years ago, from the amount of new facts recently brought to light, it has been found necessary to make various additions, so as to render the treatise complete up to the present date. The arrangement of the earlier edition has been to a certain extent adhered to, but made rather more systematic, placing certain of the compounds in groups to admit of easy reference.

At the commencement a short acccount of anthracene is given, and reference made to the first investigations of the body, by Dumas and Laurent in 1832, and the later discoveries of Fritzsche, Anderson, Berthelot, Graebe, and Liebermann, with some remarks on the views entertained by these two latter chemists, with regard to the constitution of anthracene and its derivatives. After describing the physical properties of this body, and the different modes in which it may be formed, a full description is entered into of its manufacture on a large scale, from coal tar, according to the results obtained by E. Kopp, who has made a careful study of the preparation of anthracene from soft pitch. A description is also given of the furnace best adapted for the distillation of the pitch, and the different methods for purifying the crude anthracene by extraction with heavy naphtha, and sublimation.

In treating of the methods for the valuation of crude anthracene, the older processes in which it may be extracted by means of alcohol or carbon disulphide are mentioned, from their having to a certain extent an historical interest, but which have been superseded by the method of Luck, in which greater accuracy is obtained. This latter method depends on the conversion of anthracene into the theoretical quantity of anthraquinon when dissolved in glacial acetic acid and boiled with chromic acid. A full description is given of the hydrides of anthracene, and its chlorine and bromine derivatives. In the description of anthraquinon, before entering upon its properties and manufacture, the various methods in which it may be synthetically formed are discussed, among others, the method of Bayer and Caro, by means of which the anthraquinon derivatives may be formed from phthalic acid and phenolene; the discovery of which method has added much to a clearer conception of the nature of anthraquinon.

The latter half of the volume deals with the history and preparation of natural and artificial alizarin, and the consideration of its derivatives. In describing the different processes for the preparation of artificial alizarin, mention is made of the improvement on former methods introduced by Graebe, Liebermann, and Caro, in which they produce it from monosulphanthraquinonate of soda; the advantage claimed by these over the other methods being the direct conversion of anthracene into bisulphanthracenic acid, and its transformation into bisulphanthraquinonic acid by cheap oxidising agents.

Anthraflavic acid, chrysammic acid, purpurin and their derivatives receive full consideration, and an appendix is attached containing some practical receipts for dyeing with purpurin and artificial alizarin.

The volume concludes with a most valuable bibliography embracing a list of the substances treated of throughout the work arranged in alphabetical order, with the names of the authors who have written on that particular branch of the subject, and with exact reference to the journals in which the researches have been published. As papers on the different subjects mentioned in the volume are scattered over many different periodicals, the completeness with which this bibliography has been arranged will prove a most valuable assistance to those who wish to consult the original memoirs.

We observe that throughout the edition Mr. Crookes has retained the German mode of writing anthracene without the final "e"; this may be unimportant, but it is not the method usually adopted in English text-books. There is a slight mistake at the top of page 157 in the use of the term "ferrous" instead of "ferric." This is doubtless a slip, but in the particular reaction described is of some importance.

We feel sure that Mr. Crookes will receive the thanks of those interested in this subject in England for the care and completeness with which he has arranged and carried out the text-book.

OUR BOOK SHELF

Half-Hours among some English Antiquities. By Llewellynn Jewitt, F.S.A., &c. (London: Hardwicke and Bogue, 1877.)

THIS ought to be an extremely useful little manual to those who desire to obtain a knowledge of the various classes of antiquities to be found in England, both prehistoric and historic. Mr. Jewitt writes with full knowledge and in a manner that cannot fail to secure the attention of the reader. He theorises very little, confining himself mainly to a statement of facts in reference to the various objects included under the name of antiquities. He speaks of barrows, stone-circles, cromlechs, flint and stone implements, bronze instruments, Roman remains of various kinds, ancient pottery, arms and armour, sepulchral slabs and brasses, coins, church bells, glass, tiles, tapestry, personal ornaments. Thus, it will be seen, Mr. Jewitt's programme is extensive and varied, and although much cannot be said in the space at his command, his little work will prove a very useful introduction to works of a more special kind. Not its least valuable features are the illustrations—upwards of 300—which accompany the text.

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.*]

Centralism in Spectroscopy

IN NATURE, vol. xv. p. 489, there are some remarkable counter-assertions by Mr. Christie to certain of my matter-of-fact statements on your p. 449, of which the most pressing for me to notice is the paragraph wherein he declares that "the Edinburgh Observatory has, for the last *four* years, possessed *three* spectroscopes which are almost precisely identical with those used with such effect by Dr. Huggins."

I beg to say that the above is not the case, and for this, amongst other reasons, viz., that though three spectroscopes are there in part, they belong solely as yet to H.M. Office of Works in London, which office, moreover, decided long since to return all of them to their maker, in lieu of *one* new spectroscope. And Mr. Christie must have known of this perfectly well when he wrote the above paragraph, for the carpenters of the department, who fetched away, about nine months ago, the one and only collimator to all those three partial spectroscopes, in order to send it back to its maker, spoke, as a matter of notoriety, of Mr. Christie himself being the adviser of H.M. Office of Works in that transaction, as well as the designer of the one new spectroscope ordered by the London office to take the place of the former three, but not received here yet.

With regard to the other new, and far more important, *Greenwich* spectroscope, of which Mr. Christie both chides me for not waiting for the full account to appear, as he now intimates, in a forthcoming number of the *Proceedings* of the Royal Society, and also challenges me to discuss its principles with him at once, I beg to say that my former remarks had reference solely to the official *codex* of last year's work at the Royal Observatory, Greenwich, as published by the Royal Astronomical Society in their last Anniversary Report, at p. 162, where all the world both may, and I suppose was intended to, see it, and where Mr. Christie's name appears no more than it did in my letter. And as in that letter (at your page 450) I ventured to assign the *next* anniversary meeting of the same society as the limit of time within which the full practical value of the said new Greenwich spectroscope will have been arrived at, I do not think we can do better than wait for that time to arrive.

15, Royal Terrace, Edinburgh, April 6 PIAZZI SMYTH

Parhelia and Paraselenæ seen on March 20, 1877, and again on March 21, 1877, at Highfield House Observatory

PERHAPS this phenomenon is the most remarkable of the many somewhat similar ones that it has been my good fortune to witness during the last forty years, the chief features being brilliancy and persistency.

Fig. 1 represents the appearance at 8 A.M.: an ordinary halo of $22\frac{1}{2}°$ radius, with an elongated mock sun at the apex. This

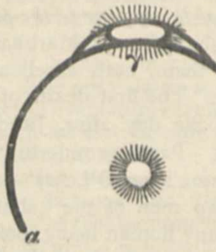


FIG. 1.—8 A.M.

lasted till 9.30 A.M., when, in addition to the halo, $\alpha\beta$, and the mock sun, γ, there was a second circle, $\delta\epsilon$, of 45° radius, also having the true sun for its centre, an inverted portion of a third circle, $\eta\theta$, of $22\frac{1}{2}°$ radius having its centre 45° above the true sun; also a portion of a fourth circle, $\iota\kappa$, of 90° radius, whose centre was 90° below the sun. The mock sun, γ, was very bright and prismatic, as also was the circle, $\alpha\beta$. The other rings were colourless.

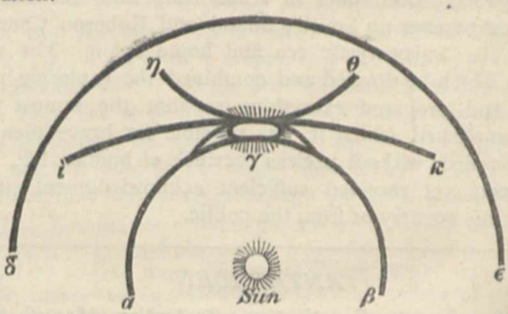


FIG. 2.—9.30 A.M.

At 9.40 A.M. the portions of circles $\eta\theta$ and $\iota\kappa$ had vanished, but a wing-like portion was now visible, and brilliant (see Fig. 3, $\lambda\omega$). This remained until 11.15 A.M., when only $\alpha\beta$ and the mock sun γ remained, lasting all the morning. At 12.57 P.M. the arc, $\iota\kappa$, again appeared, and was visible until 1.22 P.M., the halo, $\alpha\beta$, and the mock sun, γ, lasting till 5 P.M.

At 7.40 P.M. an ordinary lunar halo ($\alpha\beta$, Fig. 5), and at 8.25 P.M. a portion of a second circle, $\delta\epsilon$, of 45° radius, and of a third circle, $\iota\kappa$ (of 90° radius) and an elongated mock moon, γ,

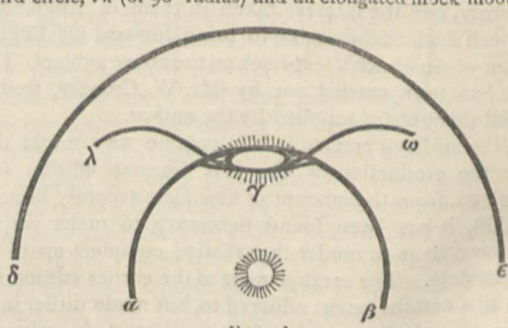


FIG. 3.

were very apparent. At 8.31 the ordinary lunar halo alone remained. At 9.10 a portion of a circle, υo, not quite 90° radius, appeared (see Fig. 6), but this did not touch the circle $\alpha\beta$, but was 10° above it. At 9.15 P.M. this also vanished, but the lunar halo remained as long as the moon was above the horizon.

On March 21, at 8 A.M., there was a solar halo and mock sun exactly like the one seen at 8 A.M., March 20 (see

Fig. 1), and this lasted all day, with the addition at 4 P.M. till 5.40 P.M. of the arc $\iota\kappa$ (being the exact copy of Fig. 4 of March 20); at 6 P.M. there was the ordinary circle $\alpha\beta$, the mock sun γ, and an inverted portion $\eta\theta$ (Fig. 2). At 6.10 only the ordinary halo remained.

From 8 P.M. till 9.40 P.M. (21st) there was a lunar halo with

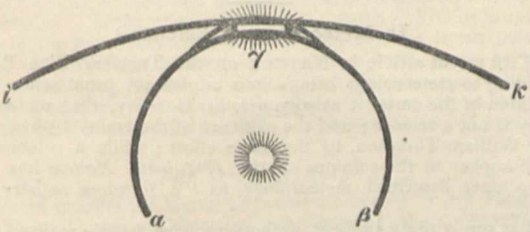


FIG. 4.

an elongated mock moon; so that a similar condition of the atmosphere prevailed for thirty-eight hours.

Whenever the circles were briliant, they were formed in a very thin haze-like cloud, through which the sun or moon (in either case) shone brightly.

The weather was cold with thick ice in the morning.

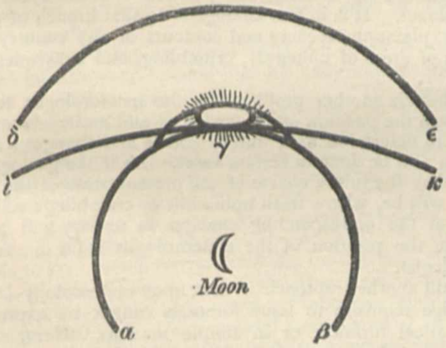


FIG. 5.

At 8 A.M. on the 20th the sky was scattered over with thin woolly cirri, indistinct in outline, and dirty-white in colour, with here and there a small prominent white portion (the sky resembling a sea with a few white wave-crests here and there). These clouds moved in a south current, but at 9 A.M. the clouds were again floating in a north-east current.

The wind on the 20th was north-east, and on the 21st north.

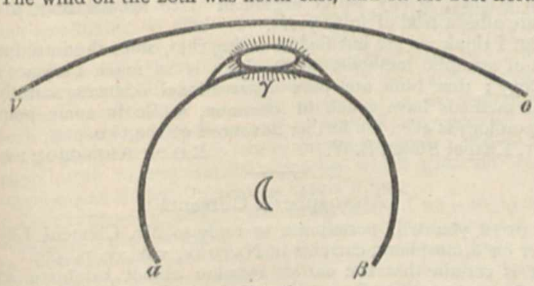


FIG. 6.

Temperature on 20th.	Dry bulb.	Wet bulb.	Temperature on 21st.	Dry bulb.	Wet bulb.
8 A.M.	32·3	31·0	8 A.M.	35·0	32·7
1 P.M.	41·9	34·0	1 P.M.	42·1	34·0
6 P.M.	37·5	34·0	6 P.M.	39·7	33·6
11 P.M.	32·8	31·7	11 P.M.	30·0	28·3

March 22, solar halo and lunar halo.
,, 23, ,, ,,
,, 27, ,, ,,
,, 28, solar halo.
,, 30, lunar halo.

E. J. LOWE

Owens College

IT seems probable that the claims of Owens College to be constituted into a university and degree-giving body for the north of England, will soon be brought before the public in a more definite shape than that of newspaper correspondence. There are one or two considerations affecting the question, which do not seem to me to have been brought forward by any of those who have entered into the discussion, and I shall esteem it a favour if you will allow me briefly to notice these.

I may premise that no word in this letter is intended to derogate from any claims that Owens College may advance on the ground of past or prospective services to education in the district of England to which its efforts must be principally, though not entirely, confined. It is almost impossible to over-rate those claims, but when they are so put forward as to imply that Owens College is the only possible centre for giving degrees in the north of England, the dwellers on the banks of the Tyne, the Wear, and the Tees, are apt to feel that too little account is made both of the necessities and of the actual educational resources of their own part of the "north of England."

In the first place, a careful study of Bradshaw makes it clear that, for the counties of Durham and Northumberland, and even the north-east portion of Yorkshire, a university examination or college course is at least as accessible in London as at Manchester; so that for these counties the benefits of Owens College, whether as college or university, are practically on a par with those of University College, London, and the degree examinations of the University of London.

Secondly, it is a fact, although apparently unknown to the majority of those who have written on this question, that there already exists in the north of England—at Durham—a university with a Royal Charter for giving degrees in all faculties, and whose conditions for giving those degrees combine in an admirable manner the modern spirit with the strictness of the old requirements.

This university was originally founded by the liberality of the Cathedral authorities, who, with a spirit worthy of imitation elsewhere, set apart certain of their own funds for the purpose of giving a liberal education in arts and theology to students who for various reasons could not avail themselves of the advantages of Oxford and Cambridge. As always happens in such cases, benefactions of scholarships and fellowships have accrued which have considerably increased the funds available for educational purposes.

Nor have these funds been restricted, as many might expect, looking at the source from which they come, to sectarian purposes. While most of the colleges at Oxford and Cambridge were still compelling Jews and Nonconformists to attend religious services to which they objected, the regulations of the University of Durham anticipated the Universities' Tests Act by granting exemption in such cases; and during the last six years more than a thousand pounds a year of the university funds has been set aside for the purposes of the Science Faculty of the university which has its local habitation in Newcastle.

I may add that the Senate and Convocation of the university have in late years adopted a most liberal view in regard to the admission of students of other colleges than those at Durham and Newcastle to the degrees which they give, and I have personally little doubt that they would consider with favour any scheme for extending the area over which their degrees are available.

The question of the desirability of multiplying the number of centres for giving degrees is a wide one, into which I have no desire to enter. My only wish is that in any consideration of the question of establishing a new centre, *all* the facts regarding its sphere of action and the centres which already exist within what is claimed as that sphere should be known.

W. STEADMAN ALDIS

College of Physical Science, Newcastle-on-Tyne, March 24

The Suspected Intra-Mercurial Planet; Occultation of Kappa Geminorum

MARCH 21 was fine here, but with frequent clouds. I had several observations of the sun from 9 o'clock to 12, Dublin mean time, and then at 12.35, 1.35, 2.0, and 3.50, after which the sun become permanently clouded for the remainder of the day. The only object remarked was a small spot with a double nucleus near the western limb followed by a few very small spots.

March 22 was finer, and I observed at 8.30, 9.12, 10.20, 10.50, 11.25, 12.38, 1.35, 1.50, 2.55, 4.25, 5.8, and 5.33. The small spots of the previous day had completely disappeared, and broad bright lacunæ occupied their place. The large spot at the last observation had so closely approached the edge that it was scarcely, if at all, visible, and the entire disc might then be said to appear free from spots of any kind. On March 23 the sun remained altogether clouded. At night, however, the sky cleared, and I had a good view of the occultation of Kappa Geminorum. The disappearance was, as usual, instantaneous, but immediately after it a delicate ray seemed to shoot out from the place of the star in a direction perpendicular to the edge of the moon, and the appearance lasted about eight seconds.

Milbrook, Tuam, March 24 J. BIRMINGHAM

α Centauri

AT the meeting of the Royal Astronomical Society December 8, 1876, Mr. Marth asked for measures of this binary.

As the star never reaches more than 11° above my horizon, the definition must always be imperfect; but the following measures taken on the morning of February 22 appear consistent, and may be useful until better results are obtained from the southern hemisphere :—

Epoch 1877·14.	Distance.	Position.
	"	°
1	3·4	64·0
2	3·1	64·5
3	3·1	64·3
4	3·7	64·7
Mean	3·3	64·4

The component stars are of about the first and second magnitudes, and their colour is yellow. Power employed, 240; definition very bad. MAXWELL HALL

Jamaica

The Boomerang

WILL you allow me to add my experience of the use of this weapon to that furnished by your other correspondents? My experience is mainly confined to the natives about the Condamine and its affluents, where I was frequently in company with natives for about a year. They had two weapons—one large, for war, the other small, for game. I should think the weapon is seldom thrown in war, since most of their contests (such as they are)take place in scrubb or forest, where it could not be used to advantage; but I have seen a native frightfully cut in the abdomen, and was told by a native that he had been struck by a boomerang thrown by the hostile party. I have seen a few of these contests, but never saw the boomerang used in any way. The "waddys" were thrown freely, the spear seldom. The game boomerang is thrown among flights of ducks, and also parrots when congregated on the trees and gathering nectar from their flowers, and with marked effect. This I have seen several times. There are two ways of throwing the weapon, which, as I could throw it well at one time, I will endeavour to describe. It is grasped quite at the end by the right hand and raised above the head, the elbow being bent, the weapon assuming a position with its convex edge downwards on a nearly horizontal line at right angles to the intended line of flight. The arm is brought swiftly round from left to right, becoming gradually extended until it reaches a line directly in front of the face, when the weapon is delivered from the now straight arm, with the concave edge towards the line of flight. This is the method of throwing into the air. No dependence can be placed on the return of the weapon within a circle of twenty yards, though it sometimes returns dangerously near the thrower. If it meets with an obstacle it is either stopped and falls dead to the earth, or its course is changed. In either case its peculiar motion is destroyed, as must be obvious. In the other method of throwing the weapon is held in the same way, but delivered nearly on line with the hip, and made to strike the earth about ten yards in front of the thrower, pitching, I believe (though it is not easy to observe), on one of its horns. Thence it ricochets and flies straight away for perhaps seventy or eighty yards, keeping a position of about four feet from the earth, and gradually rising until it is spent. It returns very little if at all. In this way only can it be used for war, since in the other it begins to mount at once, and would soon be above the enemy's head. The weapon is made of various woods, a piece with a slight elbow being selected. It is hardened by baking. The right form is arrived at by trial, as I have seen during the process of manufacture. Those sold to Europeans are the failures. I had to pay a good price for the two I brought home, but they were excellent specimens.

The natives drive ducks. A flight is marked down on a small creek; men are then posted along the bank, others drive the birds towards them, and the boomerangs are thrown as they pass. I do not recollect having seen the weapon used for ground game. These are surrounded and killed with spears and sticks. ARTHUR NICOLS

Is Meteorology a Science?

THE recent article in NATURE on the Treasury Blue Book relating to meteorology brings into unpleasant prominence the opinion of the eminent astronomer, Sir G. Airy, that meteorology is not a science; and the evidence of the eminent physicist, Sir William Thomson, to the same effect; while a celebrated philosopher in the columns of the *Fortnightly Review* has not long since described meteorology as "a formless registry of facts."

But surely these eminent authorities have hardly realised the great change which has come over the whole aspect of meteorology since the introduction of synoptic charts?

Synoptic meteorology shows that the world is, broadly speaking, covered with shifting cyclones and anti-cyclones, which have each, subject to local, diurnal, and other variations, a characteristic weather, and physical appearance, and one great problem of meteorology is to explain the observed weather over any area, at any instant, by defining the position of these cyclones and anti-cyclones. It is in fact analogous to that branch of geology which explains the scenery and contours of any country by the position of areas of upheaval, crumbling, and subsequent denudation.

But there is another problem for the meteorologist to solve, viz., Given the position of the cyclones and anti-cyclones at any instant, to determine their future course and changes; and this can even now be done in certain cases. As if the geologist were asked what the future course of the present state of the earth's surface will be, where fresh upheavals or crumblings will occur, and what the corresponding changes in scenery will be? In this case the position of the meteorologist is far in advance of the geologist.

But still another reproach is cast upon meteorology—that the knowledge requisite to issue forecasts cannot be expressed in mathematical formulæ or in simple maxims. Here, too, the analogy of geology may show that neither formulæ nor maxims are necessary to make a science. Just as a number of skilled geologists, from long experience, agree as to the structure of a complicated piece of country, so will a number of meteorologists agree as to the probable course of any series of cyclones or anti-cyclones.

The limits of a letter do not permit me to show why mean values, or harmonic series can never much advance meteorology as a science, if any better argument were needed than their failure after a trial of forty years.

But I think we are justified in saying that, since the introduction of synoptic methods, meteorology is as much a science as geology; that both are pure observational sciences, and that their methods have much in common, while in some points meteorology is even the further advanced of the two.

21, Chapel Street, S.W. RALPH ABERCROMBY

Atmospheric Currents

I HOPE you will permit me to reply to Mr. Clement Ley's letter on atmospheric currents in NATURE, vol. xv. p. 450.

It is certain that the earth's rotation cannot originate any current, but modifies them when originated.

We are agreed as to the cause of the trade-winds. The controversy is as to the questions, Why the trade-winds do not extend to the poles? What is the cause of the counter-trades or west winds between the trade-wind regions and the poles? and what is the cause of the polar depression of the barometer?

The polar depression of the barometer is due to the centrifugal force of the vortex which is constituted by the counter-trades as they circulate round the pole from east to west. There is a depression at the centre of every vortex, as any one may see in a wash-basin.

The counter-trades are "the reaction of the trade-winds." The laws of motion make it impossible for the winds to have any effect in either accelerating or retarding the earth's rotation—supposing, what we are safe in taking as proved, that they originate exclusively as the effect of solar heat. The effect of the trade-winds alone, blowing from east to west, would be to retard the earth's rotation, but this is exactly balanced by the counter-trades of the circumpolar vortexes, blowing from west to east.

I cannot agree with Mr. Clement Ley when he says that it involves a fallacy to explain the mean winds, or great currents, on one principle, and the actual winds, or temporary currents, on another. If the great currents were much feebler in proportion to the temporary currents than they are, the mean prevalence of east winds in the tropics, and of west winds in the higher latitudes, would be discernible only as a residual fact when a number of anemometric observations made at various places were completely discussed. JOSEPH JOHN MURPHY

The Germ Theory

YOUR number for March 22 contains a review of my work on the Germ Theory of Disease, which, in some points, conveys so erroneous an impression of my exact position that I must ask you to allow me space for a few remarks regarding it.

Adverse criticism is what the author of such a book as mine expects, and, to some extent, desires.

A fair representation of his views and arguments, is what every author may insist on as a right.

It is the misleading manner in which my position is stated in your review that has induced me to pen this note.

To one or two of your statements I would refer in illustration of what I complain.

After referring to my expressed belief that contagia are living organised particles—an opinion held by many eminent physicians and men of science—your reviewer says: "If, however, the particles in sheep-pox, small-pox, and vaccine be the infecting matter, they are easily seen by the microscope, and ought, therefore, to be found in the blood, but such is not the case." This, the old and stock argument against the germ theory, is specially dealt with by me on two different occasions—at p. 25, *et seq.*, and at p. 204, *et seq.* If it was worth your reviewer's while to raise this old objection to the germ theory, it was equally worth his while to make some reference to my explanation of the fact on which it rests. This he has not done—a manifest unfairness.

A little further on the review says: "Increased elimination of urea is explained thus: *The increased consumption of liquor sanguinis by the contagium particles leads to increased formation of retrogressive albumen and of urea.* It seems by this that contagium particles have livers and kidneys."

The part which I put in italics is put in your review within inverted commas, conveying thereby the impression that it is a correct quotation from my book. It is far from being so. As given by you, it is a misquoted short passage, separated from its context, and altered to suit the purpose of your review.

The chapter on increased elimination of urea is perfectly clear and intelligible to any ordinary mind, and contains nothing which justifies your reviewer in attributing to me, as he has done, the absurd belief that contagia are possessed of livers and kidneys. Neither is he justified in using the words "eat" and "drink" to express the action of a minute organism on its environment. Such phraseology can serve only to mislead those who are ignorant of the mode in which these organisms grow; and is quite inapplicable to any nutritive process which goes on in such organisms as I have described contagia to be.

Your reviewer quotes my statement that "if we were to bleed, to purge, to give antimony to, or even simply to withhold food and water from all the cases of typhus and enteric fever which occur, there can be no doubt that we should find the mortality from these diseases greatly increased;" and remarks on this, "Dr. Maclagan is right here, for by *simply withholding food and water* there can be no doubt that he would greatly increase the mortality by starving his patients to death." Your reviewer seems to be unaware that I refer in these remarks to a mode of treating fever which at one time did prevail.

To one other point I would refer in illustration of your reviewer's inaccuracy.

He says "the heat of specific fevers is partly ascribed to the propagation of the contagium causing increased consumption of tissue. But increase of living matter causes the disappearance of heat, not its production."

Even according to this, your reviewer's own somewhat awkward statement of the matter, the increased heat is attributed by me to increased consumption of tissue, indirectly brought about by the propagation of the contagium. Nowhere do I say that increase of living matter causes production of heat; and nowhere does your reviewer attribute such a statement to me. Quite the contrary. I distinctly say that the increased production of heat results from increased disintegration of the tissues; and your reviewer distinctly attributes this saying to me. What, then, is the meaning of the latter part of the sentence just quoted? It bears but one interpretation. Your reviewer attaches to the first half of the quotation a meaning the reverse of that which it conveys. While saying that I ascribe the increased heat to increased consumption of tissue, he seems to think that he is saying something quite different, and pens his criticism accordingly. If he thus misunderstands his own statements, I need, perhaps, scarcely be surprised at his sometimes misinterpreting mine. I do object, however, to such misinterpretations and inaccuracies appearing in so influential a journal as NATURE.

Dundee T. MACLAGAN

SEXUALITY IN PLANTS[1]

THE concluding part of the tenth volume of Pringsheim's *Jahrbücher* contains three papers, one of them by Dr. Arnold Dodel, of Zurich, being of the highest importance. This paper occupies the greater part of the present Heft, and is illustrated by eight coloured plates. The title is "*Ulothrix zonata*, its Sexual and Non-Sexual Reproduction, a Contribution to the Knowledge of the Lower Limit of Sexuality in Plants." The anatomy and life-history of the Ulothrix is exhaustively treated, the whole paper being a model of careful and accurate research, as well as a valuable contribution to our knowledge of the lower plants. The paper is divided into sections, of which the following is a short summary. The results given are those obtained during fourteen months' consecutive observation of the plant. The genus Ulothrix has been divided into many species, but Dodel shows that *U. zonata* is so variable in its different stages that most of the so-called species must be reduced to one. The alternation of generations is very remarkable and divisible into four stages. During the progress of the alternation of generation three distinct forms are to be distinguished, two being *filamentous* generations, and the third a zygospore generation. The filamentous generations are invariably produced non-sexually and reproduce themselves repeatedly, forming, in fact, the plant known to systematic botanists as *Ulothrix zonata*. The third generation, the zygospore, was unknown till discovered by Dodel. In the long series of filamentous generations two distinct forms are to be distinguished. The first is produced non-sexually and is the autumn or winter generation. It develops non-sexual macrozoospores and quickly spreads the species in a given locality. The second is a sexual stage developing microzoospores. It arises from the non-sexual macrozoospore, and gives rise to the microzoospores which by conjugation form the third generation, the zygospore or zoozygospore.

The production of the microzoospore-forming generation terminates the series of filamentous generations. This stage is found in spring and summer, and by giving rise to the zygospores which by remaining in a state of rest for some months during the hot dry summer weather, reproduce the plant in the autumn. During the hot weather the filamentous generations more or less completely disappear. The zygospore generation, although a product of gamogenesis, is itself non-sexual.

Ulothrix zonata exhibits polymorphism in a remarkable degree, hence many forms looked upon as distinct species must be suppressed. This opens up a wide question in regard to other algæ, and shows how essential it is to obtain an accurate knowledge of the life-history of all forms.

The cells of Ulothrix give rise to a variable number of zoospores. A mother cell may form 1, 2, 4, 8, 16, or 32 zoospores, there being no obvious distinctions between the sexual and non-sexual zoospores. On the one side is the large macrozoospore with four cilia, and then there is every gradation down to the smallest microzoospore

[1] Jahrbücher für wissenschaftliche Botanik. Herausgegeben von Dr. N. Pringsheim. Zehnter Band, Viertes Heft. Leipzig: Engelmann, 1876.

which conjugates, and which is furnished with only two cilia. The only distinction between the macro- and microzoospores seems to be that the former have four cilia, the latter only two. When the microzoospores fail to conjugate they may develop non-sexually just like the macrozoospores. This is a fact of the highest importance. In this plant, belonging to the lowest group in which sexual reproduction occurs, the sexual and non-sexual zoospores are hardly to be distinguished, and if by any chance union of the sexual zoospores does not take place, the zoospore behaves like a macrozoospore and develops non-sexually.

After remaining in a state of rest, sometimes for nearly twelve months, the contents of the zygospore break up into zoospores, from which arise the filamentous stage of Ulothrix.

In Ulothrix the conjugating cells are generally morphologically and physiologically identical, but sometimes larger zoospores conjugate with smaller, a difference in sex being here indicated. In other cases the microzoospores which have not conjugated germinate and give rise to individuals capable of reproducing. The study of the formation and subsequent development of the zygospore shows that the product of conjugation is to be considered as a new sexually-produced generation. It is a unicellular plantlet, with a root-like process and a slowly-growing plant-body which performs the function of assimilation. It in fact represents the embryo and the sporophore of the Pteridophytes. The root-end of the plantlet is formed by the union of the germinal spots of the conjugating microzoospores, while the assimilating plant-body represents the united chlorophyll-bearing parts of the zoospore.

The Ulothrix is thus one of the Zygosporeæ, and is probably related to Hydrodictyon, but it shows certain affinities to Sphæroplea, the lowest of the Oosporeæ.

As this part concludes the tenth volume of this serial, a most useful table of contents and special index of names of plants and details treated of in all the papers in the ten volumes has been added by Herr Zopf. This enables the student at once to refer to any given plant, or even to the part of it described in the various papers.

W. R. McNab

THE ROYAL NAVAL COLLEGE, GREENWICH

On February 1, 1873, the Royal Naval College was opened at Greenwich, "for the purpose of providing for the education of naval officers of all ranks above that of midshipman in all branches of theoretical and scientific study bearing upon their profession." The first annual report on the Royal Naval College thus established has been recently presented to both Houses of Parliament.

When the College was established it was determined by the Admiralty to bring together in it all the necessary means both for the higher education of naval officers and also of others connected with the navy. During the session which terminated last year four captains, four commanders, ninety-three lieutenants, and eight navigating-lieutenants joined the college as students, but of these only one captain, thirty-three lieutenants, and three navigating-lieutenants went through the whole nine months' course, although one captain, two commanders, fifty lieutenants, and three navigating-lieutenants underwent the final examination. Besides these officers, who may all be regarded as being purely voluntary students, there was also a large number of others studying at the college, with a view to passing certain examinations, which would qualify them either for promotion or advancement or for appointment to some special branch or department of the service.

Finally, ten private students are reported as having passed through a course of instruction, nine of the number being foreign officers, a fact which testifies to the estimation in which the college is held abroad.

With regard to the subjects of study we find that, besides the course of mathematics, which is compulsory for all students, systematic courses of instruction, extending over the entire session, are given in physics, chemistry, steam, navigation, and nautical astronomy, marine surveying, permanent and field fortification, military surveying and drawing, military history, foreign languages—namely, French, German, and Spanish—and in freehand drawing. Special courses of lectures are also given on various subjects, among which the principal seem to be the Structural Arrangements of Men-of-War, International Law, Naval History, and Practical Ship-building.

TYPICAL LAWS OF HEREDITY[1]

II.

First let me point out a fact which Quetelet and all writers who have followed in his paths have unaccountably overlooked, and which has an intimate bearing on our work to-night. It is that, although characteristics of plants and animals conform to the law, the reason of their doing so is as yet totally unexplained. The essence of the law is that differences should be wholly due to the collective actions of a host of independent *petty* influences in various combinations, as was represented by the teeth of the harrow, among which the pellets tumbled in various ways. Now the processes of heredity that limit the number of the children of one class such as giants, that diminish their resemblance to their fathers, and kill many of them, are not petty influences, but very important ones. Any selective tendency is ruin to the law of deviation, yet among the processes of heredity there is the large influence of natural selection. The conclusion is of the greatest importance to our problem. It is, that the processes of heredity must work harmoniously with the law of deviation, and be themselves in some sense conformable to it. Each of the processes must show this conformity separately, quite irrespectively of the rest. It is not an admissible hypothesis that any two or more of them, such as reversion and natural selection, should follow laws so exactly inverse to one another that the one should reform what the other had deformed, because characteristics, in which the relative importance of the various processes is very different, are none the less capable of conforming closely to the typical condition.

When the idea first occurred to me, it became evident that the problem might be solved by the aid of a very moderate amount of experiment. The properties of the law of deviation are not numerous and they are very peculiar. All, therefore, that was needed from experiment was suggestion. I did not want proof, because the theoretical exigencies of the problem would afford that. What I wanted was to be started in the right direction.

I will now allude to my experiments. I cast about for some time to find a population possessed of some measurable characteristic that conformed fairly well to the law, and that was suitable for investigation. I determined to take seeds and their weights, and after many preparatory inquiries, fixed upon those of sweet-peas. They were particularly well suited to my purposes; they do not cross-fertilise, which is a very exceptional condition; they are hardy, prolific, of a convenient size to handle, and their weight does not alter when the air is damp or dry. The little pea at the end of the pod, so characteristic of ordinary peas, is absent in sweet peas. I weighed seeds individually, by thousands, and treated them as a census officer would treat a large population. Then I selected with great pains several sets for planting. Each set contained seven little packets, and in each packet were ten seeds, precisely of the same weight. Number one of the packets contained giant seeds, all as nearly as might be of $+3°$ of deviation. Number seven contained very

[1] Lecture delivered at the Royal Institution, Friday evening, February 9, by Francis Galton, F.R.S. Continued from p. 495.

small seeds, all of -3° of deviation. The intermediate packets corresponded severally to the intermediate degrees $\pm 2^\circ \pm 1^\circ$ and 0°. As the seeds are too small to exhibit, I have cut out discs of paper in strict proportion to their sizes, and strips in strict proportion to their weights, and have hung below them the foliage produced by one complete set. Many friends and acquaintances each undertook the planting and culture of a complete set, so that I had simultaneous experiments going on in various parts of the United Kingdom. Two proved failures, but the final result was this: that I obtained the more or less complete produce of seven sets, that is of $7 \times 7 \times 10$, or 490 carefully weighed seeds.

It would be wholly out of place if I were to enter into the details of the experiments themselves, the numerous little difficulties and imperfections in them, or how I balanced doubtful cases, how I divided returns into groups, to see if they confirmed one another, or how I conducted any other of the well-known statistical operations. Suffice it to say that I took immense pains, which if I had understood the general conditions of the problem as clearly as I do now, I should not perhaps have cared to bestow. The results were most satisfactory. They gave me two data, which were all that I required in order to understand the simplest form of descent, and so I got at the heart of the problem at once.

Simple descent means this. The parentage must be single, as in the case of the sweet peas which were not cross-fertilised, and the rate of production and the incidence of natural selection must both be independent of the characteristic. The processes concerned in simple descent are those of Family Variability and Reversion. It is well to define these words clearly. By family variability is meant the departure of the children of the same or similarly descended families from the ideal mean type of all of them. Reversion is the tendency of that ideal mean type to depart from the parent type, "reverting" towards what may be roughly and perhaps fairly described as the average ancestral type. If family variability had been the only process in simple descent, the dispersion of the race would indefinitely increase with the number of the generations, but reversion checks this increase, and brings it to a standstill, under conditions which will now be explained.

On weighing and sorting large samples of the produce of each of the seven different classes of the peas, I found in every case the law of deviation to prevail, and in every case the value of 1° of deviation to be the same. I was certainly astonished to find the family variability of the produce of the little seeds to be equal to that of the big ones, but so it was, and I thankfully accept the fact, for if it had been otherwise I cannot imagine, from theoretical considerations, how the problem could be solved.

The next great fact was that Reversion followed the simplest possible law; for the proportion was constant between the deviation of the mean weight of the produce generally and the deviation of the parent seed, reckoning in every case from one standard point. In a typical case, that standard must be the mean of the race, otherwise the deviation would become unsymmetrical, and cease to conform to the law.

I have adjusted an apparatus (Fig. 1) to exhibit the action of these two processes. We may consider them to act not simultaneously but in succession, and it is purely a matter of convenience which of the two we suppose to act the first. I suppose first Reversion then Family Variability. That is to say, I suppose the parent first to revert and then to tend to breed his like. So there are three stages: (1) the population of parents, (2) that of reverted parents, (3) that of their offspring. In arranging the apparatus I have supposed the population to continue uniform in numbers. This is a matter of no theoretical concern, as the whole of this memoir relates to the distinguishing peculiarities of samples irrespectively of the absolute number of individuals in those samples. The apparatus consists of a row of vertical compartments, with trapdoors below them, to hold pellets which serve as representatives of a population of seeds. I will begin with showing how it expresses Reversion. In the upper stage of the apparatus the number of pellets in each compartment represents the relative number in a *population* of seeds, whose weight deviates from the average, within the limits expressed by the distances of the sides of that compartment from the middle point. The correct shape of the heap has been ensured by my having cut a slit of the proper curvature in the board that forms the back of the apparatus. As it is glazed in front I have only to pour pellets in from above until they reach the level of the slit. Such overplus as may have been poured in will run through the slit, to waste, at the back. The pellets to the right of the heap represent the heaviest seeds, those to the left the lightest. I shall shortly open the trap-door on which the few representatives of the giant seeds rest. They will run downwards through an inclined shoot, and fall into another compartment nearer the centre than before. I shall repeat the process on a second compartment in the upper stage, and successively on all the others. Every shoot converges towards one standard point in the middle vertical line; thus the present shape of the heap of pellets is more contracted in width than it was before, and is of course more humped up in the middle. We need not regard the humping up; what we have to observe is that each degree of deviation is simultaneously lessened. The effect is as though the curve of the first heap had been copied on a stretched sheet of india-rubber that was subsequently released. It is obvious from this that the process of reversion co-operates with the general law of deviation. Fig. 6 shows the principle of the process of reversion clearly.

I have now to exhibit the effects of variability among members of the same family. It will be recollected that the produce of peas of the same class deviated normally on either side of their own mean weight; that is to say, I must make the pellets which were in each of the upper compartments to deviate on either side of the compartment in which they now lie, which corresponds to that of the medium weight of their produce. I open the trap-door below one of the compartments in the second stage, the pellets run downwards through the harrow, dispersing as they run, and form a little heap in the lowest compartments, the centre of which heap lies vertically below the trap-door through which they fell. This is the contribution to the succeeding generation of all the individuals belonging to the compartment in the upper stage from which they came. They first reverted and then dispersed. I open another trap-door, and a similar process is gone through; a few extreme pellets in this case add themselves to the first formed heap. Again, I continue the process; heap adds itself to heap, and when all the pellets have fallen through, we see that the aggregate contributions bear an exact resemblance to the heap from which we originally started. A simple formula (see Appendix) expresses the conditions of equilibrium. I attended to these, when I cut out the slit in the back board of the upper compartment, by which the shape of the original heap was regulated. Thus it follows from the formula that if deviation after reversion was to deviation before reversion as 4 to 5, and if 1° of family variability was six units, then the value of 1° in the population must be ten units.

It is easy to prove that the bottom heap is strictly a curve of deviation, and that its scale tends invariably to become the same as that of the upper one. It will be recollected that I showed that every variety of curve of deviation was producible by variations in the length of the harrow, and that if the pellets were intercepted at successive stages of their descent they would form a succession of curves of increasing scales of deviation. The curve in the second stage may therefore be looked upon as one of these intercepts; all that it receives in sinking to the third stage being an additional dose of dispersion.

As regards the precise scale of deviation that characterises each population, let us trace, in imagination, the history of the descendants of a single medium-sized seed. In the first generation the differences are merely those due to family variability; in the second generation the tendency to wider dispersion is somewhat restrained by the effect of reversion; in the third, the dispersion again increases, but is more largely restrained, and the same process continues in successive generations, until the step-by-step progress of dispersion has been overtaken and exactly checked by the growing antagonism of reversion. Reversion acts precisely after the law of an elastic spring, as was well shown by the illustration of the india-rubber sheet. Its tendency to recoil increases the more it is stretched, hence equilibrium must at length ensue between reversion and family variability, and therefore the scale of deviation of the lower heap must after many generations always become identical with that of the upper one.

We have now surmounted the greatest difficulty of our problem; what remains will be shortly disposed of. This refers to sexual selection, productiveness, and natural selection. Let us henceforth suppose the heights and every other characteristic of all members of a population to be reduced to a uniform adult male standard, so that we may treat it as a single group. Suppose, for example, a female whose height was equal to the average female height + 3° of female deviation, the equivalent in terms of male stature is the average male height + 3° of male deviation. Hence the female in question must be registered not in the feet and inches of her actual height, but in those of the equivalent male stature.

On this supposition we may take the numerical mean of the stature of each couple as the equivalent of a single parent, so that a male parent plant having 1° deviation and a female parent plant having 2° of deviation, would together rank as a single fertilised plant o + $1\frac{1}{2}$°.

In order that the law of sexual selection should co-operate with the conditions of a typical population, it is necessary that selection should be *nil*, that is, that there should not be the least tendency for tall men to marry tall women rather than short ones. Each strictly typical quality taken by itself must go for nothing in sexual selection. Under these circumstances one of the best known properties of the law of deviation (technically called that of "two fallible measures") shows that the population of sums of couples would conform truly to the law, and the value of 1° would be that of the original population multiplied by $\sqrt{2}$. Consequently the population of *means of couples* would equally conform to the law, but in this case the 1° of original deviation would have to be divided by $\sqrt{2}$, the deviations of means of couples being half that of sums of couples.

The two remaining processes are productiveness and survival. Physiologically they are alike, and it is reasonable to expect the same general law to govern both. Natural selection is measured by the percentage of survival among individuals born with like characteristics. Productiveness is measured by the average number of children from all parents who have like characteristics, but it may physiologically be looked upon as the percentage of survival of a vast and unknown number of possible embryos, producible by such parents. The number being unknown creates no difficulty if they may be considered to be, on the average, the same in every class. Experiment could tell me little about either natural selection or productiveness. What I have to say is based on plain theory. I can explain this best by the process of natural selection. In each species, the height, &c., the most favoured by natural selection, is the one in which the demerits of excess or deficiency are most frequently balanced. It is therefore not unreasonable to look at nature as a marksman, her aim being subject to the same law of deviation as that which causes the shot on a target to be dispersed on either side of the point aimed at. It would not be difficult, but it would be tedious, to justify the analogy; however, it is unnecessary to do so, as I propose to base the analogy on the exigences of the typical formula, no other supposition being capable of fulfilling its requirements. Suppose for a moment that nature aims, as a marksman, at the medium class, on purpose to destroy and not to save it. Let a block of stone (Fig. 4)

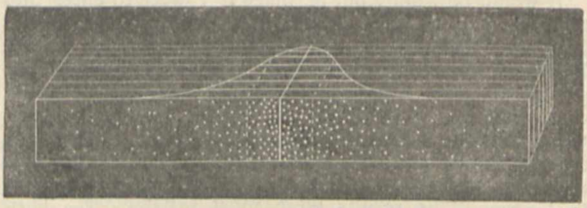


Fig. 4.

represent a rampart, and let a gun be directed at a vertical line on its side on purpose to breach it: the shots would fall with the greatest frequency in the neighbourhood of the vertical line, and their marks would diminish in frequency as the distance increased, in conformity with the law of deviation. Each shot batters away a bit of stone, and the shape of the breach would be such that its horizontal outline will be the well-known curve. This would be the action of nature were she to aim at the destruction of medium sizes. Her action as preserver of them is the exact converse, and would be represented by a cast that filled the gap and exactly replaced the material that had been battered away. The percentage of thickness of wall that had been destroyed at each degree of deviation is represented by the ordinate of the curve, therefore the percentage of survival is also an ordinate of the same curve of deviation. Its scale has a special value in each instance, subject to the general condition in every typical case, that its 0° shall correspond to the 0° of deviation of height, or whatever the characteristic may be.

In Fig. 5 the thickness of wall that has been destroyed at each degree of deviation is represented by the corresponding ordinate of the horizontal outline of the portion which remains. Similarly, in the case of an original

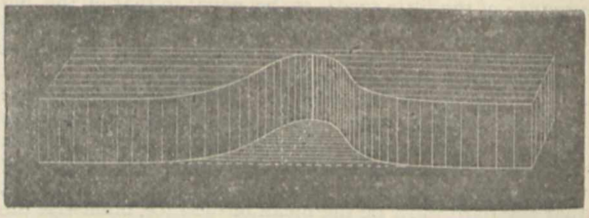


Fig. 5.

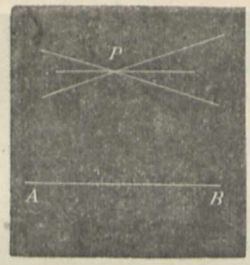
population, in which each class was equally numerous, the amount of survivors at each degree of deviation is also represented by the corresponding ordinate of this or a similar curve.

But in the original population at which we are supposing nature to aim the representatives of each class are not equally numerous, but are arranged according to the law of deviation; the middle class being most numerous, while the extreme classes are but scantily represented. The ordinate of the above-mentioned outline will in this case represent, not the *absolute number*, but the *percentage* of survivors at each degree of deviation.

(*To be continued.*)

ON THE SIMPLEST CONTINUOUS MANIFOLDNESS OF TWO DIMENSIONS AND OF FINITE EXTENT[1]

ONE of the most remarkable speculations of the present century is the speculation that the axioms of geometry may be only approximately true, and that the actual properties of space may be somewhat different from those which we are in the habit of ascribing to it. It was Lobatchewsky who first worked out the conception of a space in which some of the ordinary laws of geometry should no longer hold good. Among the axioms which lie at the foundation of the Euclidian scheme, he assumed all to be true except the one which relates to parallel straight lines. An equivalent form of this axiom, and the one now generally employed in works on geometry, is the statement that it is impossible to draw more than one straight line parallel to a given straight line through a given point outside it. In other



words, if we take a fixed straight line, A B, prolonged infinitely in both directions, and a fixed point, P, outside it; then, if a second straight line, also infinitely prolonged in both directions, be made to rotate about P, there is *only one* position in which it will not intersect A B. Now Lobatchewsky made the supposition that this axiom should be untrue, and that there should be a finite angle through which the rotating line might be turned, without ever intersecting the fixed straight line, A B. And in following out the consequences of this assumption he was never brought into collision with any of the other axioms, but was able to construct a perfectly self-consistent scheme of propositions, all of them valid as analytical conceptions, but all of them perfectly incapable of being realised in thought.

Many of the results he arrived at were very curious; such as, for instance, that the three angles of a triangle would not be together equal to two right angles, but would be together less than two right angles by a quantity proportional to the area of the triangle. If we were to increase the sides of such a triangle, keeping them always in the same proportion, the angles would become continually smaller and smaller, until at last the three sides would cease to form a triangle, because they would never meet at all.

There are many other assumptions, at variance with the axioms of Euclid, which may be made respecting distance-relations, and which yield self-consistent schemes of propositions differing widely from the propositions of geometry. We see, therefore, that geometry is only a particular branch of a more general science, and that the conception of space is a particular variety of a wider and more general conception. This wider conception, of which time and space are particular varieties, it has been proposed to denote by the term *manifoldness*. Whenever a general notion is susceptible of a variety of specialisations, the aggregate of all such specialisations is called a manifoldness. Thus space is the aggregate of all *points*, and each point is a specialisation of the general notion of *position*. In the same way time is the aggregate of all *instants*, and each instant is a specialisation of the general notion of *position in time*. Space and time are, in fact, of all manifoldnesses, the ones with which we are by far the most frequently concerned.

Now there is an important feature in which these two manifoldnesses agree. They are both of them of such a nature that no limit can be conceived to their divisibility. However near together two points in space may be, we can always conceive the existence of intermediate points. And the same thing holds in regard to time. Mathematicians express this fact by saying that space and time are *continuous* manifoldnesses. But there is another feature, equally important with the foregoing, in regard to which space and time are strikingly contrasted. If we wish to travel away from any particular instant in time, there are only two directions in which we can set out. We must either ascend or descend the stream. But from a point in space we can set out in an infinite number of directions. This difference is expressed by saying that time is a manifoldness of *one dimension*, and that space is a manifoldness of *more than one dimension*. An aggregate of points in which we could only travel backwards or forwards would be, *not* solid space, but a *line*. A line, therefore, is a manifoldness of one dimension. A *surface*, again, may be regarded as an aggregate of lines; and it is an aggregate of such a nature, that if we wish to travel away from a particular line, there are only two directions in which we can set out. It is therefore a line-aggregate of one dimension. Considered as a point-aggregate it has two dimensions, and accordingly it is a manifoldness of two dimensions. In the same way it will be seen that solid space is a manifoldness of three dimensions.

I have endeavoured by these remarks to explain what is meant when we speak of a continuous manifoldness of two dimensions. It is the object of this paper to communicate some results I have arrived at respecting the properties of the simplest of such manifoldnesses which has a finite extent. The existence of the particular manifoldness I shall endeavour to describe has been referred to in a remarkable lecture by Prof. Clifford on "The Postulates of the Science of Space," but, so far as I am aware, its properties have not hitherto been worked out in detail.

The simplest of all doubly-extended continuous manifoldnesses is the *plane*. But it is not a manifoldness of finite extent. It reaches to infinity in every direction, and its area is greater than any assignable area. It is therefore not the manifoldness of which we are in search. Now the circumstance in which the plane differs from those doubly-extended manifoldnesses which are next to it in order of simplicity, is the possibility that figures constructed in it may be magnified or diminished to any extent without alteration of shape; in other words, that figures which can be constructed in it at all can be constructed to any scale. That this property is not possessed by curved surfaces, may be seen by considering the case of a spherical triangle. If the sides of a triangle constructed on a given sphere be all of them increased or diminished in the same proportion, the shape of the triangle will not remain the same. Now it has been found by Prof. Riemann that this property of the plane is equivalent to the following two axioms:—(1) That two geodesic lines which diverge from a point will never intersect again, or, as Euclid puts it, that two straight lines cannot inclose a space; and (2) that two geodesic lines which do not intersect will make equal angles with every other geodesic line. The second is precisely equivalent to Euclid's twelfth axiom. Deny the first of these axioms, and you have a manifoldness of uniform positive curvature; deny the second, and you have one of uniform negative curvature. The plane lies midway between the two, and its curvature is zero at every point.

Let us consider, then, the case of a doubly-extended manifoldness, of which the curvature is uniform and positive. The first of the before-mentioned two axioms

[1] Read before the London Mathematical Society, December 14, 1876.

is no longer true. Geodesic lines diverging from a point do not continue to diverge for ever. They meet again and inclose a space. The first question which presents itself is with reference to the situation of the point towards which they ultimately converge. In the case of a spherical surface they will converge towards a point which is separated from the starting-point by half the length of a geodesic line. And this is the only case we are able to conceive. The surface of a sphere is the only doubly extended manifoldness of uniform positive curvature which geometry recognises, and it is the only one which we can figure to ourselves in thought. It is not, however, the *simplest* of such manifoldnesses. To obtain the simplest case we must suppose that the point towards which two geodesic lines converge is separated from their starting-point, not by *half*, but by the *entire* length of a geodesic line; or, what amounts to the same thing, that it *coincides* with the starting-point. It is true that we are utterly unable to figure to ourselves a surface in which two geodesic lines shall have only one point of intersection, and shall yet inclose a space. But we are perfectly at liberty to reason about such a surface, because there is nothing self-contradictory in the definition of it, and because, therefore, the analytical conception of it is perfectly valid. It is the simplest continuous manifoldness of two dimensions, and of finite extent, and those few properties of it which I have worked out appear to me to be very beautiful. In order to make my observations more intelligible, I shall for the future speak of it as a surface, and its geodesic lines I shall speak of as straight lines. I have the highest authority for using this nomenclature, and though it will impart to my theorems a very paradoxical sound, it is calculated, I think, to give a juster idea of their meaning, than if I were to use the more accurate, but less familiar terms.

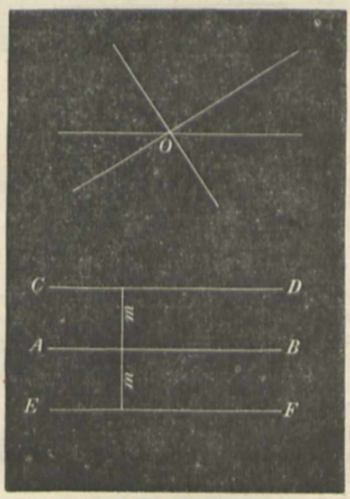
Assuming, then, as the fundamental properties of our surface, that every straight line is of finite extent (in other words, that a point moving along it will arrive at the position from which it started after travelling a finite distance), and that two straight lines cannot have two points in common, the first corollary I propose to establish is that all straight lines in the surface are of equal extent.

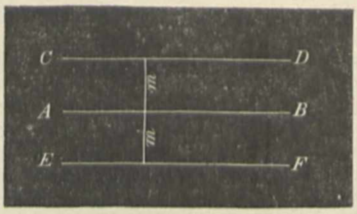
Let A, B, be two straight lines in the given surface. If possible, let A be greater than B. From A cut off a portion equal to B. Let P, Q, be the extreme points of this segment, and let R be any point in B. Apply the line A to the line B in such a manner that the point P falls on the point R, then, since in a surface of uniform curvature equal lengths of geodesic lines may be made to coincide, the segment PQ will coincide with the entire straight line B. Hence Q will fall upon R. But P coincides with R, and P and Q do not coincide with one another, since PQ is less than the entire straight line A; therefore Q cannot coincide with R. Hence A cannot be greater than B.

The straight lines here spoken of are, of course, not *terminated* straight lines. What the proposition asserts is that the *entire* length of all straight lines in the given surface is the same. The corresponding proposition in spherical geometry is that all great circles of a given sphere are equal.

There are a great many other analogies between the imaginary surface here treated of and the surface of a sphere. Its straight lines, though they are like the straight lines of a plane in the circumstance that any two of them have only one point of intersection, are in many other respects analogous to great circles. In any of its straight lines, for instance, each point has a corresponding point which is *opposite* to it, and farther from it than any other point in the line. For if by setting out from a point and travelling a finite distance in a particular direction we get back to the starting-point, there must be a point half way on our journey which is farther from the starting-point than any other point in the line, and which may very appropriately be called its opposite point. It is an obvious corollary that the distance between any two points will be the same as the distance between their opposite points.

Let us now consider the case of a number of straight lines radiating from a centre. In each of them there will be a point which is opposite to that centre. And it will be a separate point for every separate straight line. For no two straight lines can have two points in common, and since these radiating lines have a common centre of radiation, they can have no other point in common. Hence, if we suppose one of these lines to rotate about the centre, the point opposite to the centre will describe a continuous line, and one which finally returns into itself. It is the locus of all points in the surface opposite to the centre of radiation. What now is the character of this locus? In the first place it is a line which is of the same shape all along, and of which all equal segments therefore can be made to coincide. For any two positions of





the rotating line which contain a given angle may be placed upon any other two positions which contain an equal angle. Then, since the length of all straight lines in the surface is the same, the opposite points will coincide, and by parity of reasoning all intermediate points of the locus. But, in the second place, the locus is also of the same shape on both sides. For each point in it may be approached from the centre of radiation in two different ways, and it is at the same distance from that centre, whether it be approached in the one way or the other. Any particular segment, in fact, of the locus has its extreme points joined to the centre of radiation by lines which are of equal length, and which include an equal angle—lines, therefore, which may be made to coincide. Since this is the case for any segment whatever, and for every subdivision of a segment, all the points of a segment will still remain on it if the segment be turned round and applied to itself. Hence the locus is of the same shape, whether viewed from the one side or from the other. But since it is also of the same shape all along, it satisfies Leibnitz's definition of a straight line, and it is, in fact, a geodesic line of the surface.

Hence we have this second proposition—that all points in the surface opposite to a given point lie in a straight line.

From the method of its construction, this straight line is farther from the given point than any other line in the surface. Travelling from the given point as a centre, in whatever direction we might set out, we should, after completing half our journey, arrive at this farthest straight line, we should cross it at right angles, and we should then keep getting nearer and nearer to our starting-point, until we finally reached it from the opposite side.

Each separate point in the surface, moreover, has a separate farthest line. For if any two points be taken, the points opposite to them on the straight line which joins them will be distinct. Hence their farthest lines will cut this joining line in two separate points. They must, therefore, be two *separate* lines, for the same straight line cannot cut another straight line in two separate points. In a similar manner it may be shown that each straight line in the surface has a separate farthest point. Hence there exists a reciprocal relation between the points and straight lines of the surface, a relation which we may express by saying that every point in the surface has a *polar*, and that every straight line in the surface has a *pole*. It is then easy to show that when a point is made to move along a straight line its polar will turn about a point, and that when a straight line is made to turn about a point, its pole will move along a straight line.

It is interesting to compare these propositions with the corresponding ones in spherical geometry. There, too, each point has a farthest geodesic line; that is to say, a geodesic line which is farther from it than any other geodesic line on the sphere. But each geodesic line has *two* farthest points or poles, instead of having only one. Hence there is not that perfect reciprocity of relationship between points and geodesic lines which exists in the surface we have been examining; and this is one of the many ways in which the sphere shows itself to be inferior to that surface in simplicity.

The most astounding fact I have elicited in connection with this surface is one which comes out in the theory of the circle. Defining a circle as the locus of points equidistant from a given point, we shall find that it assumes a very extraordinary shape when its radius is at all nearly equal to half the entire length of a straight line. For let us again figure to ourselves a number of straight lines radiating from a point. Let l be the total length of each straight line. Then the supposition we have to make is that the radius of our circle shall be nearly equal to $\frac{l}{2}$. Let us suppose it equal to $\frac{l}{2} - m$, where m is small as compared with l. Each of the radiating lines will cut the circle in two points, and each of these points will be at a distance from O equal to $\frac{l}{2} - m$ or $\frac{l}{2} + m$, according as the distance is measured in the one direction or the other. And their distance from each other will be equal to $2m$, that is to say, it will be comparatively small. But each point on the polar of O will be at a distance from O equal to $\frac{l}{2}$. Hence each point on the circle will be at a distance from this polar equal to m. Moreover, every point at a distance of m from the polar will be a point on the circle, because it will be at a distance of $\frac{l}{2} - m$ from O. But the locus of points at a distance of m from the straight line, A B, will consist of two branches, C D and E F, one on either side of A B, and at the same distance from it along their whole length. It is true that these branches form in reality a single continuous line. A point travelling along from C to D, and further in the same direction, would ultimately appear at E, travel along to F, and then, after a further journey, reappear at the point C. But this does not alter the fact that when a small portion only of this line is contemplated, it presents the appearance of two straight lines, each of them parallel to, and equidistant from, A B.

In the limiting case, where the radius becomes equal to $\frac{l}{2}$, C D and E F both of them coincide with A B. The circle merges into a straight line, and becomes, in fact, the polar of its own centre. It is not, indeed, quite accurate to say that it merges into a straight line, for it reduces itself rather to two coincident straight lines, and its equation in co-ordinate geometry would be one of the second degree.

In regard to the surface here treated of, it is easy to see that, as with the sphere, the smaller the portion of it we bring under our consideration, the more nearly its properties approach to those of the plane. Indeed, if we consider an area that is very small as compared with the total area of the surface, its properties will not differ sensibly from those of the plane. And on this ground it has been argued that the universe may in reality be of finite extent, and that each of its geodesic lines may return into itself, provided only that its total magnitude be very great as compared with any magnitude which we can bring under our observation.

In conclusion, I cannot do better than quote the passage in which Prof. Clifford explains what must be the constitution of space if this hypothesis should be true. "In this case," he says, "the universe, as known, is again a valid conception, for the extent of space is a finite number of cubic miles. And this comes about in a curious way. If you were to start in any direction whatever and move in that direction in a perfect straight line according to the definition of Leibnitz, after travelling a most prodigious distance, to which the parallactic unit—200,000 times the diameter of the earth's orbit—would be only a few steps you would arrive at—this place. Only, if you had started upwards, you would appear from below. Now one of two things would be true. Either when you had got half way on your journey you came to a place that is opposite to this, and which you must have gone through, whatever direction you started in, or else all paths you could have taken diverge entirely from each other till they meet again at this place. In the former case every two straight lines in a plane meet in two points, in the latter they meet only in one. Upon this supposition of a positive curvature the whole of geometry is far more complete and interesting; the principle of duality, instead of half breaking down over metric relations, applies to all propositions without exception. In fact I do not mind confessing that I personally have often found relief from the dreary infinities of homaloidal space in the consoling hope that, after all, this other may be the true state of things."

F. W. FRANKLAND

HYDROGRAPHY OF WEST CENTRAL AFRICA

MR. STANLEY'S second letter in last Thursday's *Telegraph* contains important information on the district between Tanganyika and the Albert and Victoria Nyanza—information complementary to that given in his former letters, which we embodied in a map, vol. xiv. p. 374. He has, in fact, discovered another "source" of the Nile, and one evidently of great length and volume—the Kagera—which he has gallantly named the Alexandra Nile. This river issues from a large lake, Akanyaru or Alexandra Nyanza, in two branches and flows north, uniting under 1° S. lat., and flowing east to the Victoria Nyanza. Mr. Stanley was only able to see the Alexandra Nyanza from a distance, but it is evidently of considerable size, and receives a river at its west end, the Upper Alexandra Nile, which probably comes from a considerable distance. Mr. Stanley believes that the Alexandra Nyanza has a marshy connection with Kivu Lake on the south, from which issues the Rusizi, an affluent of the Tanganyika. If then these various connections are ultimately verified, the problem of African hydrography becomes more complicated than ever. The Rusizi will connect the Nile system with Tanganyika, and very shortly, at least, Mr. Stanley believes, the Lukuga will carry the water of the latter to the west—to the Congo, say some. Meantime Mr. Stanley is probably at or has already left Nyangwe. After deciding this question of the connection of Albert and Tanganyika Lakes from that side, he will probably devote himself to the task of tracing down the Lualaba, which, according to Cameron, should bring him into early communication with Dr. Nachtigal, who is to trace up the Congo.

It may not be uninstructive to point out what is the present state of the problem which these two explorers have set themselves to solve. Our principal scientific authorities on the Congo are still Capt. Tuckey and Prof. Smith, who in 1816 ascended about 200 miles up the river, and who have left us a record yet deserving of study. They left England at a time when the outlet of Mungo Park's Niger was a subject of speculation, and amongst the theories then started, the Congo, as an outlet, held a high place. The same notions of the magnitude of this river obtained then, and Capt. Tuckey and his civilian scientific staff started with the idea that they would be able to navigate it for hundreds of miles. They had, however, only been in the river some four or five days when Prof. Smith makes this entry in his diary :—"The channel is very narrow and the current never more than three knots . . . everthing yet seems to indicate that the descriptions of the great breadth of the river, the length of its course, &c., have been exaggerated." Again, twelve days afterwards, when they had got considerably further up the river, he writes, "The whole appearance of the river, its numerous sandbanks, low shore, inconsiderable current, narrow channel, seem but little to justify its extravagant fame. Its sources cannot be further inland than those of the Senegal and Gambia." Capt. Tuckey, who ties himself very rigidly to a statement of facts, ventures to say that at Fathomless Point the true mouth of the river "is not three miles in breadth ; and allowing the mean depth to be forty fathoms and the mean velocity of the stream four and-a-half miles an hour, it will be evident that the calculated volume of water carried to the sea has been greatly exaggerated." The mean velocity of the current higher up the river than the true mouth appears to be about two miles, and Tuckey remarks that they found no difficulty in rowing the gigs to the foot of Casan Yellala *against the current*.

These falls or rapids (Yellala) deserve some notice. They extend continuously for about twenty miles along the river, and are very much like the rapids on the Somerset Nile between Foweira and Magungo, where Col. Gordon reports a fall of 700 feet in a space of ten or fifteen miles. On August 14, 1816, Prof. Smith says, "We discovered the celebrated fall of Yellala, at a distance of about a mile and a half. But how much were we disappointed in our expectations on seeing a pond of water only with a small fall of a few hundred yards." They had been led to expect a second Niagara, and instead of that, found a rapid having a perpendicular fall of thirty feet in a slope of 300 yards formed of a descending bed of mica slate. The width of the river is very various, sometimes expanding to half a mile. It is compared by Tuckey to Loch Tay and by Smith to the Drammen, in Norway, at the bridge. Sometimes it contracts to 100 yards ; in one place it is reduced to fifty yards in breadth, but at this point the stream rushes through at the rate of eight miles an hour. The rapid and considerable rise of the water during the rainy season is largely accounted for by the fact that "the hills do not absorb any of the water that falls, the whole of which is carried direct to the river by gullies and ravines, with which the hills are furrowed all over." These hills are composed entirely of slate, with masses of quartz and syenite, and their extreme barrenness forms one of the most striking features of the country.

It would appear from Capt. Tuckey's and Prof. Smith's reports that the farthest point they reached on the river was at least 1,000 feet above the sea, and as this point is about 800 miles in a direct line from Nyangwe, which Cameron has fixed at 1,400 feet, the connection between the Congo and Lualaba on the question of level alone seems very doubtful.

The Casai and Kwango are doubtless the chief affluents of the Congo ; it may have tributaries from the north and north-west behind the coast ranges, but these will be of secondary importance. As soon as we get east of the Congo water-parting, we begin to descend to the great valley of the Lualaba, Livingstone's "central line of drainage." This river occupies the centre of a saucer-like depression, one lip being probably the Congo water-parting, the other the Bambarre, or perhaps Kabogo Mountains to the west of Tanganyika. The fall of this depression is from south to north ; commencing at the Katanga copper mines of the Pombeiros, it runs to Lake Kassali 1,750 feet, to Nyangwe 1,400 feet ; thence to the "Unvisited Lake" of Livingstone, the "Great Lake" of Poncet, or the "Sankorra" of Cameron, probably also the "Liba" of the Benin slaves. and so on by the Shari to Lake Chad, 830 feet.

From these statements, then, it will be seen that the solution of the hydrographical problem of Western Central Africa is difficult to arrive at on the data we at present possess, and that to advocate any special theory may be rash. The Congo theory is a fascinating one, but the levels seem against it. However, with two such men as Nachtigal and Stanley in the field, the solution of this problem, as of others almost equally interesting, will soon be discovered.

THE LONDON INDUSTRIAL UNIVERSITY

WE give below a series of extracts from an admirable letter addressed by Major Donnelly, the chief of the scientific staff of the Science and Art Department, to Sir Sydney Waterlow, with reference to the proposed Industrial University to be established by the City Guilds in London :—

London, March 14, 1877

DEAR SIR SYDNEY WATERLOW,—In reply to your request, I am happy to place at your service such suggestions, with regard to the proposed "City Guilds' Industrial University," as my experience in connection with the Science and Art Department enables me to offer. . . .

Under anything like a broad view of the subject it would be difficult to say what branch of learning should be omitted in an Industrial University. But if we confine ourselves to what is practicable with the probable means immediately at command, and if we look to commence by supplying that of which there is the greatest want, we shall, I think, have no hesitation—considering the relative facilities for obtaining instruction in the different branches of knowledge—in deciding that science as now understood, and particularly Applied Science, has the first call on our attention.

. . . The Industrial University might be *commenced* by establishing professorships with the necessary laboratories, tutorial staff, &c., in the following branches of Science and Art :—

Mathematics (Pure and Applied and Practical Geometry).
Chemistry.
Physics (Heat, Light, Magnetism, and Electricity).
Mechanics (Practical Mechanics, Machinery, and Machine Drawing).
Engineering and Building Construction ; and in

Applied Art (Modelling, Designing, Enamelling, Repoussé work, Wood Carving, &c.).

As the teaching would be specially directed to the industrial applications of science it is needless to say that considerable subdivision would be required in the subjects named. . . .

. . . It is of great importance that the professors should be not only teachers, but investigators, constantly endeavouring to enlarge the field of accurate knowledge, and scientific procedure, in our industries. To appreciate how much may be effected in this way we have only to consider the millions saved to France by Pasteur's researches on the disease of the silkworm, or the knowledge obtained by his inquiry on fermentation.

. . . The time of the professors may be much economised by making it no part of their duty to commence their courses with the elements of general science. It is quite unnecessary that they should do so. This teaching may be obtained at other places, with which the Industrial University would be only needlessly interfering if it gave elementary instruction. It should, on the contrary, be its object to supplement and specialise the knowledge obtainable in ordinary science classes from which the students should be drawn; and they should be expected to have acquired sufficient general knowledge of science before entering the classes and laboratories of the University to be able to follow its courses with advantage.

A leading feature of the University should be evening courses —not merely popular lectures—for the use of those whose circumstances in life have rendered it necessary that they should commence the practical work of life early. By circumstances in life I do not refer solely to poverty. There are many occupations that it is advisable, if not necessary, to enter upon early. For instance, it is of the utmost importance for a mechanical engineer to be a good practical workman. To do this he must join the workshop when young. And the lad who enters when he is thirteen has an advantage which might not be expected. Mr. Phythian, the Master of the Oldham School of Science and Art established by the Messrs. Platt, informs me that to the lads who come into the workshop at this age the evening intellectual work is no effort; it is a relaxation and recreation. To the apprentices who enter at eighteen it is almost an impossibility. They are so exhausted by their day's labour that they cannot pay attention.

It is agreed on all hands that if the teaching of science is to be of any use it must be essentially practical—that is to say, the teaching of the laboratory. And no pains should be spared to make the laboratories perfect and readily available. By them the University may supply a great want.

It is perhaps necessary to guard against the idea that the University is to teach any trade or business. There could be no greater mistake than for it to attempt to do so. The purely technical knowledge of a trade must be learnt by practising it. The teaching of a public institution can with advantage only extend as far as the special application of various branches of abstract science to the different arts. It is no doubt difficult to define how far the teaching of applied science may go without trenching on the workshop. But in practice the limits are readily found. This difficulty will be still less felt in an institution drawing its pupils from among those actually engaged in trade, who will know what they can acquire in the University, and what they can better learn directly in business. The programme of examinations in technology by the Society of Arts will give many suggestions on this subject.

I have no doubt that the Society of Arts would be willing to transfer the whole or a part of their system of examinations in technology to such a body as the City Guilds, who, with far larger funds at their disposal, may give it a development which the Society of Arts can never obtain for it. By employing local agencies and taking advantage of the machinery of the Science and Art Department, these examinations are held throughout the country. And by availing itself of this and similar organisations, the Central University might be brought *en rapport* with every part of England, Scotland, and Ireland.

. . . Through the action of the Government, stimulating local effort, the country is being rapidly covered by a network of Science and Art schools and classes, where the working classes—whose interests and advances the City Guilds are, I understand, especially anxious to promote by the Industrial University—have opportunities of obtaining that elementary instruction in Science and in Art which must be the basis of any sound technical education. There are now 1,750 separate schools or classes in the country in connection with and receiving aid through the Science and Art Department.

It is therefore unnecessary to consider the question of the creation of any organisation for giving instruction in elementary general science or art. What are wanted are a stimulus to increase the number of students, the development of systematic courses of instruction onwards and upwards from the elementary school, and means to enable poor, but clever and industrious, youths to pursue such courses. The award of small scholarships or bursaries in competition which would support the holders while carrying forward their studies in a higher school—the retention of the bursary being contingent on a definite course of instruction being pursued satisfactorily—is therefore, I believe, the most effective means the Guilds can adopt to aid technical instruction.

. . . It is very necessary to bear clearly in mind in what directions the University must look for its pupils. Broadly speaking, these will I believe be :—

1. The holders of Bursaries and Scholarships.
2. Young men whose means enable them to carry on their education beyond the school age, and who can attend an institution in London more conveniently than elsewhere.
3. A limited number of students of the same class who are attracted by the goodness of the instruction and its appropriateness to their future pursuits. I say a limited number, because, however good the instruction, it will take years to divert the class of students from the channels which time has consecrated.
4. Evening students—men who are engaged during the day.

It would be useless to expect many students from classes 2 and 3 at first. . . .

The real point seems to me to make a beginning. Get a good site—by a good site I mean a site in an accessible position, sufficiently large to allow of expansion as the University grows—build chemical and physical laboratories, and lecture rooms, and some mathematical class rooms, on a portion of the site. If these are well managed, and are in a prominent position, such as that suggested on the Embankment, where they cannot but be seen—it is difficult to make anything known in London—surely there must be many rich men in the city besides the city companies who will seize the opportunity, by adding to the endowment or the buildings, of perpetuating their memories as munificent patrons of what will eventually be a credit to the country.

. . . It always seems to be forgotten that the population of London is as large as that of Scotland; and that if its provision for instruction were tenfold what it is, it would not be proportionately larger than that of the Canton of Zurich.

Any plan you commence upon must be much modified as the institution is expanded and developed. To succeed, the University must be built up by slow degrees and adapted, with the experience you gain from day to day, to meet the wants and circumstances of the time. That it will be a success, and a great success, if taken heartily in hand by the City of London, there can be no doubt.

Believe me,
Yours very faithfully,
J. F. D. DONNELLY

OUR ASTRONOMICAL COLUMN

THE OPPOSITION OF MARS IN 1892.—Early in August, 1892, the planet Mars will come into opposition at a distance sensibly the same as in September of the present year, when it is proposed to make a serious attempt to determine the solar parallax by observations of this planet, a method which has not hitherto been applied under such advantageous circumstances as are now possible, but which is calculated to furnish the sun's distance from the earth with a degree of precision comparable with that to be attained by the observation of a transit of Venus, and with far less trouble and expense. It will not perhaps be without interest at the present moment, when the attention of astronomers is particularly directed to the efficient observation of Mars near the opposition in September next, if we present an ephemeris for the opposition of 1892, the only one of the present century yet to come, which can be to all intents and purposes as favourable as that of 1877. The ephemeris is founded upon the tables of M. Leverrier, which have been applied with sufficient accuracy for the object in view. The positions are for mean noon at Paris.

1892	App. R.A. of Mars. h. m. s.	App. Decl. of Mars. ° ′ ″	Log. distance from Earth.
July 23	21 18 47·4	— 22 25 24	9·50247
,, 25	— 17 5·9	22 38 27	9·58837
,, 27	— 15 16·0	22 51 22	9·58489
,, 29	— 13 19·0	23 4 0	9·58201
,, 31	— 11 16·4	23 16 13	9·57975
Aug. 2	— 9 9·3	23 27 51	9·57812
,, 4	— 6 59·4	23 38 47	9·57714
,, 6	— 4 48·1	23 48 55	9·57678
,, 8	— 2 36·8	23 58 7	9·57705
,, 10	21 0 27·1	24 6 18	9·57796
,, 12	20 58 20·4	24 13 23	9·57951
,, 14	— 56 18·0	24 19 18	9·58166
,, 16	20 54 21·4	— 24 23 59	9·58443

The opposition will take place on August 4, and Mars will be in perigee on August 6 at a distance of 0·3774. The distance in perigee in the present year will be 0·3767.

THE COMET 1873 II. (TEMPEL, JULY 3).—This very interesting comet of short period will return to perihelion in 1878. The elements which rest upon the widest extent of observation are those of Mr. W. E. Plummer; in his orbit the period of revolution is 1850·25 days, or 5·066 years, and the perihelion passage in 1873 having taken place June 25·38, G.M.T., the comet, neglecting the effect of perturbations which in the present revolution is not likely to be material, will be again due in perihelion about 1878, July 19·5. Probably geocentric places derived from Mr. Plummer's orbit, with this date for perihelion passage, will give a sufficient idea of the circumstances of the next appearance, and a few positions so derived are accordingly subjoined:—

At 12h.	R.A.	N.P.D.	Distance from earth.
June 29	322·5	97·4	0·437
July 9	328·7	100·7	0·400
,, 19	334·7	104·9	0·377
,, 29	340·0	109·8	0·369
Aug. 8	344·3	114·6	0·375
,, 18	347·4	118·8	0·397

The comet, therefore, appears under conditions nearly as favourable as possible for observations, the least distance of its orbit from that of the earth being 0·33, at a greater radius-vector. In aphelion the comet is distant from the sun 4·555, and its distance from the orbit of the planet Jupiter at this point (which is that of nearest approach) is 0·736. Four days after perihelion passage the comet approaches the orbit of Mars within 0·05, all these distances being expressed in parts of the earth's mean distance from the sun.

There does not appear to have been any observation of this comet previous to 1873, notwithstanding its short period. It could neither have been the object seen on one morning only in October 1846 by Hind, nor that observed by Goldschmidt on May 16, 1855, which was at first mistaken for the short-period comet of De Vico (1844 I).

In addition to the comet in question, Tempel is also the discoverer of comet 1866 I, associated with the great November meteor-shower, and comet 1867 II, which was re-observed in 1873, after its orbit had undergone considerable change from a near encounter with Jupiter about the preceding aphelion passage.

NEW COMET.—Prof. Winnecke, the director of the Imperial Observatory at Strasburg, announces his discovery of "a fine bright comet, with nucleus and trace of a tail," early on the morning of April 6. The following position depends upon observations with an annular micrometer on a 3½ feet telescope, the comet being inconveniently situated for the larger instrument.

April 5 at 15h. 53m. 39s. mean time at Strasburg, Right Ascension 22h. 7m. 49·44s., Declination + 14° 54′ 15·4″. The diurnal motion in R.A. is rather less than 1m., and that in Decl. about 1¼°, both increasing.

The dearth of comets which had prevailed since December 1874, appears to have terminated, and we must soon hear of the re-discovery of the one which bears the name of D'Arrest, and has been so elaborately calculated by M. Leveau.

[Since the above was in type the following elements, calculated by Herr Hartwig, have been received from Prof. Winnecke:—Perihelion passage, April 18·1741, Berlin time, longitude of perihelion, 251° 59′ 57″; ascending node, 317° 51′ 18″; inclination, 56° 42′ 42″; logarithm of perihelion distance, 9·96767, motion retrograde. By these elements the comet at midnight on April 25, in R.A. 22h. 39m. and N.P.D. 42°·7, will have twice the theoretical intensity of light that it had on the date of discovery.]

CHEMICAL NOTES

THE NEW METALS ILMENIUM AND NEPTUNIUM.—About thirty years ago R. Hermann announced the discovery of a new metal, ilmenium, accompanying tantalum and niobium in various minerals, and closely allied to them in its general characters. Several years later he relinquished his claims to the discovery, in consequence of researches by Marignac in the same field leading to entirely different results. Later investigations have, however, strengthened his belief in the existence of ilmenium, and in the February number of Kolbe's *Journal für praktische Chemie* he not only brings forward results tending to establish the individual character of ilmenium, but describes a new metal, *neptunium*, belonging to the same group, and occurring in tantalite from Haddam, Connecticut. As the quantities obtained are small, the characteristic reactions limited, and as the spectral properties cannot be made use of, chemists will naturally reserve their opinion till confirmatory observations have been made by some other well-known investigator. The following are the essential results obtained by Hermann. The mineral was found to consist of equal portions of columbite ($ROMe_2O_3$) and ferroilmenite ($RO2MeO_2$). By fusion with potassium bisulphate the hydrates of the metallic oxides were separated out in the following proportions:—

Ta_2O_3	32·39
Nb_4O_7	36·79
Il_4O_7	24·52
Np_4O_7	6·30
	100·00

The hydrates can be changed into double fluorides, and from the greater solubility of potassium-neptunium fluoride, it may be obtained free from tantalum and ilmenium salts but retaining a small quantity of the niobium salt; these, however, on being changed into niobate and neptunate of sodium may be separated on account of the greater solubility of the latter. By fusion of the neptunate of sodium with potassium bisulphate and treatment with water, the hydrate of neptunic acid was obtained in a pure condition. Neptunium may be distinguished from niobium and ilmenium by its having, along with tantalum, the property of forming an amorphous insoluble precipitate on the addition of caustic soda to the boiling solution of the fluoride; the other two form crystalline and easily soluble compounds. The very soluble character of neptunium-potassium fluoride as compared with the corresponding tantalum salt serves to distinguish it from that metal. The reactions with phosphorus salts in the inner part of the bunsen flame are the following:—tantalic acid, colourless; niobic acid, blue; ilmenic acid, brown; neptunic acid, wine yellow. Addition of tincture of galls to solutions of the sodium salts gives characteristically-coloured precipitates. The atomic weight of neptunium, determined from the double salt $4KFl + Np_2Fl_7.2H_2O$, was found to be 118. Hermann has also obtained ilmenium in the form of a black powder by heating potassium-ilmenium fluoride with potassium chloride and potassium.

ABSORPTION OF HYDROGEN BY ORGANIC SUBSTANCES UNDER THE INFLUENCE OF THE SILENT DISCHARGE.—M. Berthelot has recently found that under the effect of the discharge, benzene absorbs about two atoms of hydrogen, yielding a polymeride of C_6H_8, a resinous substance with an irritating smell. On heating, benzene first distils over; then a liquid, soluble in strong nitric and sulphuric acid, finally leaving carbon containing a little hydrogen. Oil of turpentine absorbs about 2·5 atoms of hydrogen, yielding resinous products. Pure carbon does not combine with hydrogen under the influence of the discharge, and a mixture of hydrogen with acetylene behaves much in the same way as pure acetylene. A mixture of hydrogen and carbon monoxide yields the solid body observed by Brodie and Thénard, $5CO + 3H_2 = CO_2 + C_4H_6O_3$, a trace of acetylene being formed.

PHOSPHORUS PENTAFLUORIDE.—Professor Thorpe has lately described this body (Liebig, *Ann.* clxxxii.), which he prepares by the gradual addition of phosphorus pentachloride to arsenic trifluoride. Phosphorus pentafluoride is a colourless gas, with a pungent and extremely irritating odour; it reacts upon water, forming phosphoric and hydrofluoric acids. The density with regard to hydrogen was found to be 63·23 (theory requiring 63); under the pressure of twelve atmospheres at 7° it exhibits no marked deviation from Boyle's law; it does not seem to be affected by the passage through it of electric sparks either when pure or when mixed with hydrogen or oxygen. With dry ammonia it forms the compound $2PF_5 5(NH_3)$.

MOLECULAR VOLUMES OF SULPHATES AND SELENATES.—An account of investigations on this subject has lately been published by Otto Petterson (*Deut. chem. Ges. Ber.*, ix. 1559), in which he finds that, in the series of sulphates and selenates of potassium, ammonium, rhubidium, and cæsium the molecular volume of the compound is regularly increased by 6·6 when the group SO_4 is exchanged for the group SeO_4; also, that the substitution of a molecule of ammonium, rubidium, or cæsium for a molecule of potassium produces an increase in volume of 9, 8, and 23 respectively in the selenates as well as in the sulphates. He has also examined the double sulphates and selenates of cobalt, nickel, and copper with potassium, in which results are found tending to confirm the hypothesis that in double salts the components are unaltered; this is more marked in the case of the selenates, in which the volumes of the double salts are equal to the sums of the volumes of their components. The author disagrees with Favre and Valson in their conclusions that double salts cannot exist in solution, and are formed at the moment of crystallisation; he believes on the contrary that as no contraction takes place on crystallisation these salts may be held to exist in the same condition in solution as after crystallisation; the double salt of thallium is, however, an exception. In the case of the alums also when obtained in an anhydrous condition the volume of the salt exactly equals the volumes of its components.

CONTRIBUTIONS TO THE THEORY OF LUMINOUS FLAMES.—A continuation of experiments on the above subject is given by K. Heumann (*Liebig's Ann.*, clxxxiii.), in which he finds that carbonaceous matter will give luminous or non-luminous flames, according as the temperature of the flame is high or low; diluting the gaseous combustible with indifferent gases also requires a higher temperature to cause a separation of the carbon, and thus produce luminosity. Reduction of temperature in a flame prevents either partially or entirely the formation of carbon, consequently the author thinks that the deposition of carbon on cold surfaces in a flame is not the consequence of cooling, as a deposition may be formed on red-hot surfaces, but burns away in contact with air. In burners of different materials, those of iron were found to prevent the luminosity of the lower part of the flame to a greater extent than those of steatite, also when the burner is heated, a greater amount of light is produced, the consumption of the combustible remaining the same. Herr Heumann thinks that by heating the burner the luminosity is increased, and extends to a greater extent over the lower part of the flame.

NOTES

WE are informed that H.M. Government has just been pleased to sanction the necessary expenditure to replace the important deep-soil thermometers of the Royal Observatory, Edinburgh, which were so cruelly broken by a madman last September. The estimate has been prepared by Messrs. Adie and Son, Princes Street, Edinburgh, and is understood to include everything that can conduce to scientific accuracy.

PROF. J. DEWAR, F.R.S.E., Jacksonian Professor of Natural Experimental Philosophy in the University of Cambridge, has been elected Fullerian Professor of Chemistry to the Royal Institution in the room of Dr. Gladstone, resigned.

DR. COLAN, the senior medical officer of the recent Arctic Expedition, has been promoted to be Deputy Inspector-General of Hospitals.

DR. W. B. CARPENTER, C.B., commenced, on Monday evening, at the School of Mines, Jermyn Street, a free course of lectures on geology, which he is delivering as Swiney Professor.

AT the meeting of the French Geographical Society on April 4 it was announced that the great gold medal of the Society had been awarded to Commander Cameron in recognition of his services in the cause of geographical science.

THE estimate for "Education, science, and art in Great Britain amounted in 1853–4 to 578,000*l.*; this year the estimate was 3,546,000*l.*" "In 1835 the Government paid for public education a sum of 26,750*l.*, but in 1875–6 the amount had increased to 3,972,008*l.*"

AMONG the fifty-seven candidates for admission into the Royal Society are two clergymen of the Church of England, one Wesleyan minister, one peer, one foreign baron, one baronet, eleven M.D.s, &c.

THE late Mr. J. C. Tufnell has bequeathed to University College, Gower Street, 5,000*l.* to be used in establishing two scholarships, one in general chemistry and the other in analytical and practical chemistry.

THE Rev. E. Ledger, Gresham Professor of Astronomy, will deliver a course of Lectures on the Telescope, in the theatre of Gresham College, on the evenings of April 17, 18, 19, 20. The electric light will be used to illustrate the lectures.

LIEBIG is to have another monument. A few weeks ago we noted the "inauguration" of one at Darmstadt. Subscriptions are now being collected for the purpose of raising a statue to him in Munich. About 7,000*l.* has already been contributed.

MR. H. W. S. WORSLEY-BENISON, F.L.S., has been appointed Lecturer on Botany at Westminster Hospital.

THE services of Mr. W. Saville Kent, F.L.S., F.Z.S., have been engaged temporarily to superintend and place in thorough order the "Fish House" at the Zoological Society's Gardens, Regent's Park. A considerable number of marine fish and other specimens of interest have been imported to the tanks during the past week.

TWO views have been offered as to the mode of action of the gas in the radiometer. One attributes the motion to reaction of gas particles getting heated on the vanes, then dancing off; the other to air currents which are directed towards the plate in consequence of heated air rising from it. M. Neesen has endeavoured (*Pogg. Ann.*) to decide between these views. If the second view is correct, he argued, the wall of the vessel, by becoming also heated, must also acquire influence through rise of heated air from it as from the vanes. If the rotation be merely a phenomenon of reaction there is no reason to suppose such an influence of the fixed wall. Now by giving the radiometer an eccentric position within the glass vessel such an influence of the walls should be readily recognised. He describes a number of experiments made in this way, and which he regards as supporting the second view.

IN an article contributed to *Poggendorff's Annalen*, M. Zollner is led to take the following positions in reference to the radiometer. The explanation of radiometric motions based on the principles of the mechanical theory of gases, makes suppositions about the relation of the mean lengths of path of the gas molecules to the dimensions of the vessel which are not realised in fact. This explanation further leaves out of consideration, without sufficient ground, the simultaneous existence of mercury vapours whose molecules have a more than seven times greater mass and a much smaller mean length of path than the molecules of the gas acting according to the mechanical theory of gases. Hence we are not warranted in regarding the radiometric motions discovered by Crookes as an empirical confirmation of the mechanical theory of gases.

BERLIN dealers in delicacies have recently received from the south, and especially from Upper Italy, immense quantities of edible birds which have been captured there in their flight northwards. Unfortunately there were not only snipe, fieldfare, and larks, or so-called "delicacies" among the birds sent, but also singing birds, that are never eaten in Germany, such as goldfinch, thrush, and nightingales. The animals were caught on their migratory flight by means of nets, or surprised during the night and indiscriminately killed. A new indication of the importance of an international law for bird protection!

AN exhibition of objects relative to pre-historic archæology will be opened shortly in Moscow, and promises to be very interesting.

AN elaborate volume just published by the Federal Statistical Bureau of Switzerland, gives the number of scientific societies in the country in 1875 as 46, with 54,955 members. The societies for educational purposes numbered 816, with 54,424 members.

THE municipal authorities of Berne have set aside the sum of 24,000*l*. for the foundation of a Museum of Natural History in that city.

ASSOCIATIONS and Committees are being formed in most of the large towns of the Netherlands with the object of "fitting out a suitable vessel for Nova Zembla and other stations of interest in the Arctic regions." The avowed aim of the expedition is not the discovery of the Pole, but the erection of some unpretentious granite monuments to the memory of the glorious discoveries of the earlier Dutch navigators. About the end of the seventeenth century, "in the name and on behalf of the honourable Council of the renowned City of Amsterdam," Willem Barends set out on his third voyage, which ended in the explorer's wintering on Nova Zembla, whence he never returned. It is, above all, the memory of Barends which the Dutch are about to honour. The costs of the expedition are to be defrayed by voluntary subscriptions, and the vessel will, in all probability, be commanded by a Dutch lieutenant who has taken part in three Arctic expeditions under the British flag.

THE Committee of the German African Society has issued an appeal for help towards the establishment of a series of permanent stations in Africa, so as gradually to narrow the area of the unknown country, to serve as centres of culture, and to be depots for information and for trade with the natives. The effort would be in sympathy with that of the International Congress at Brussels, and the appeal is made specially to Germany to maintain the exceptionally high place she has taken in the scientific discovery of Central Africa.

GEOGRAPHICAL students will be glad to learn that an index to Petermann's *Mittheilungen aus Justus Perthes's geographischer Anstalt über Wichtige neue Erforschungen auf dem Gesammtgebiete der Geographie* has been published for the period between and including the years 1865 to 1874. The value of this is greatly increased by the publication therewith of index maps, which show at a single glance those parts of the world of which maps have been published in the *Mittheilungen* during the period in question, with references to the places where they have appeared.

GEN. UCHATIUS bases his invention of the steel bronze, or more correctly, hard bronze, cannons, now introduced into the Austrian service, on the observation, that all metals (with exception of lead and zinc) have their elasticity increased, when they have undergone a continuous weighting above their first limit of elasticity. In the first February number of *Dingler's polytechnisches Journal*, M. Uchatius gives, in reply to the objections of some technologists, the results of further experiments, which appear to show that even homogeneous bronze is capable of a great increase of its elasticity through simple stretching, without condensation. It is only a stretching of the metals above their limit of elasticity, whereby the molecules, brought to a state of flow, glide over each other, and assume a wholly new position more favourable to resistance, that causes the increase of elasticity. A simple condensation produces merely an increase of the absolute solidity and diminution of the tenacity, but no real increase of elasticity. The limit of elasticity may be raised nearly to the breaking consistence, so that in many cases it is six and seven times the original. Mere stretching for a short time is of little use; the tension must act a considerable time. It is also well to apply a gradually increasing weight.

A SINGULAR fact with reference to the production of heat is described by M. Olivier (*Comptes Rendus*). A square bar of steel 15 mm. in width and 70 to 80 ctm. long is seized with the two hands, placed, one at one end, the other in the middle of the bar. The other end is pressed against an emery grind-stone rotating rapidly. In a few minutes the rubbed end is considerably heated. The band at the middle has no sensation of heat, but that at the extremity is strongly heated, so that it has to be taken from the bar. Thus, in certain cases, heat appears not to be propagated in metals from one part to that next it.

IN the year 1824 M. Wöhler made the observation that palladium, whether in the form of sponge or of polished sheet, has the property of becoming sooty in a spirit flame, and gradually coated with a thick layer of carbon. A piece of spongy palladium will thus be enlarged to several times its original volume. The same phenomenon occurs if the metal is made to glow in a coal-gas flame. If the deposited carbon be burnt, there remains always a fine skeleton of palladium, which is then found to be penetrated with the carbon and quite brittle. By more recen experiments M. Wöhler convinced himself that the phenomenon is not due to a special affinity of palladium for carbon. He is rather of opinion that the strong absorption power of this metal for hydrogen is the reason why ethylene gas and the gases of the spirit flame, which themselves are not absorbed by palladium, are decomposed under the influence of this metal, as the experiments show, with separation of carbon.

LED by speculative considerations regarding the formation of the earth, M. Sacher, of Salzburg, made experiments a short time ago on the solidification of balls of spermaceti floating freely in a liquid, and he has more recently experimented on the propagation of heat in unequally dense liquids. In a beaker glass, 16 ctm. high, he put five layers of alcohol varying in density from 0·98 to 0·82, and each 3 ctm. thickness. Three thermometers were placed with their bulbs in the first, third, and fifth layers respectively, and the vessel was slowly heated from below. In another similar vessel containing liquid all of the same density, the three thermometers showed nearly the same temperature;

but in the stratified liquid marked differences appeared between the layers on readings being taken every five minutes. Thus in ten minutes the readings were 31° (below), 18·5° (middle), 18° (top); after twenty minutes, 44°, 19·5°, 18°; after fifty-five minutes, 77°, 40°, 21°. The numbers prove, then, that in liquids of decreasing density heat is distributed very slowly from below upwards. Experiments in cooling led to a similar result.

PROF. QUINCKE, of Heidelberg, has long experimented as to whether gases can penetrate through the pores of glass. A pressure of forty to one hundred and twenty atmospheres is found to be incapable of forcing a perceptible quantity of carbonic acid or hydrogen gas through a glass wall 1·5 mm. thickness, during a period of seventeen years. No loss of weight was perceptible. M. Quincke, however, will not draw the inference that the molecules of hydrogen and carbonic acid have larger dimensions than the molecules or pores of the glass. The distance at which the molecular forces of the glass act on the gas-particles is of course greater than the dimensions of the molecules themselves. The pore walls of the glass may get coated with an "absorbed" gas layer, which itself becomes immovable through the nearness of the solid substance, and hinders the passage of gas particles from the interior of the glass tube into the outer air. Perhaps, too, there may be dropable liquid in the pores of the glass, preventing outflow of the gas. A similar objection applies (according to M. Quincke) to M. Traube's method of determining the size of the molecules of a substance from the possibility of passing through a so-called "precipitate-membrane."

A DROUGHT in excess of any that have occurred during the last fourteen years, as regards long continuance and severity, has prevailed for some months in Victoria and parts of Australia adjoining. It terminated about February 12, and from that date to the 22nd of the same month, when the mail left, heavy thunderstorms and rainfall had prevailed, and cooler weather set in. The reports from Deniliquin and other places in the interior state that not a blade of grass was to be seen on the plains, and cattle were dying in thousands.

THE Russian Naval Department proposes to send a ship this summer to the mouths of the Obi and Jenissei to make a thorough maritime survey of both gulfs.

THE *Western Review of Science and Industry* is the title of a new monthly devoted to various departments of science, and published in Kansas City, Mo.

IN view of the promising future of the African continent M. Bernardin, of Ghent, has done a good service by publishing a *brochure* (compiled from the works of various travellers), on the commercial products of Central Africa. An excellent map of Petermann's, showing the standpoint reached by exploration up to September, 1876, is included in the pamphlet.

THE death is announced of Prof. P. Panceri, the eminent Italian anatomist. He died suddenly whilst lecturing in the University at Naples.

THE additions to the Zoological Society's Gardens during the past week include a Common Wolf (*Canis lupus*) European, presented by Mr. J. A. Parlet; a Ceylon Fish Owl (*Ketupa ceylonensis*) from Ceylon, presented by Capt. B. B. Turner; a Vulpine Phalanger (*Phalangista vulpina*) from Australia, presented by Mr. W. Bazeley; two Sykes's Hemipodes (*Turnix sykesii*), a Rain Quail (*Coturnix coromandelica*), an Asiatic Quail (*Perdicula asiatica*) from India, three Chinese Quails (*Coturnix chinensis*) from China, presented by Mrs. Wood Mason; an Entellus Monkey (*Semnopithecus entellus*) from India, received in exchange; a Collared Fruit Bat (*Cynonycteris collaris*) born in the Gardens.

SOCIETIES AND ACADEMIES

LONDON

Chemical Society, April 5.—Prof. Odling, F.R.S., in the chair.—A lecture on the discrimination of crystals by their optical characters was delivered by Prof. N. S. Maskelyne, F.R.S. After a few general remarks on the use, to the chemist, of the methods employed by crystallographers, the lecturer proceeded to consider the methods of determining the symmetry of crystals by their optical characters. The origin and meaning of various terms used in crystallography were explained and illustrated by models, &c.; the lecturer then threw on the screen, by means of a polarising apparatus and the electric light, the beautiful coloured effects produced by crystals of cerussite, barytes, borax, &c., the effect of heat in altering the position of the optical axes of a crystal of gypsum being especially beautiful. In conclusion, the lecturer pointed out the ready means, which the examination of the optical characters of a crystal under the polarising microscope often afforded to the chemist, of acquiring a great deal of information in a very short time, and expressed a belief, that if chemists would work up suitable groups of crystals for examination by the crystallographer, very important knowledge as to the functions of various groups of molecules in a crystal would be gained.

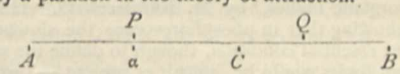
Anthropological Institute, March 27.—Col. A. Lane Fox, F.R.S., V.P., in the chair.—Capt. W. Samuells, of the Bengal Staff Corps, was elected a member.—An account of some Kitchen Middens near Ventnor, by Mr. Hodder M. Westropp was read by the director. A corn-crusher, of Scandinavian appearance, was found in one of them, and in another higher up in the cliff, there was discovered a small cinerary urn of unusual shape encircled with a pattern of coralline sea-weed.—Messrs. W. Power and E. Laws communicated a short paper on a Kitchen Midden near Tenby; Dr. Crockley Clapham a paper on the brain-weights of the Chinese and Pelew Islanders; and Mr. James Shaw some notes on right-handedness and improved instinct in animals during the human period. Dr. Clapham found that the weight of the brain both of the Chinese and the Islanders was above the average, but they presented certain peculiarities in their convolutions. The skulls of the Pelew Islanders were markedly dolichocephalic. The size of the brain of the Chinese and the Islanders was in no wise an index of the intelligence possessed by them.

MANCHESTER

Literary and Philosophical Society, February 6.—E. W. Binney, F.R.S., president, in the chair.—Notice of the Junior Literary and Philosophical Society of Manchester, 1806-1807, by W. E. A. Axon, M.R.S.L.—On compound combinations, by Prof. Cayley, F.R.S., &c.—On ternary differential equations, by Robert Rawson.—On the powerful oxidising action of animal charcoal upon organic matters as shown by the analysis of the drainage from a large heap of a mixture of night-soil and animal charcoal, by William Thomson, F.R.S. Edin.—A plea for the word "Anglo-Saxon," by Rooke Pennington, LL.B., F.G.S.

CAMBRIDGE

Philosophical Society, March 12.—Prof. Cayley, vice-president, in the chair.—Prof. Clerk Maxwell communicated to the Society a paradox in the theory of attraction.



Let the line AB be divided in any given point C, and let

$$PC^{-1} - AC^{-1} = CQ^{-1} - CB^{-1}$$

be the condition of correspondence of two points P and Q, in the segments AC, CB respectively, then if P and Q vary simultaneously, still remaining correspondent,

$$PC^{-2}d(PC) = CQ^{-2}d(CQ),$$

or the corresponding elements are to each other as the squares of their distances from C. If we now suppose that AB is a material line of uniform density, the law of attraction being the inverse square of the distance, the attractions of corresponding elements on the point C will be equal and opposite. But every element of AC has a corresponding element in CB, and hence we might conclude that the attraction of AC on C is equal and opposite to that of CB on C, which is evidently not the case, unless $AC = CB$. The paradox is explained by considering that all that we have proved is that the attraction of AP on C is equal and opposite to that of QB on C, and this however near to C the corresponding points P and Q are taken, and this is strictly true. But however near to C the corresponding points are taken, the attractions of PC and of CQ are both infinite, but differ by a constant quantity, namely the attraction of $A\alpha$ on C, where αC is taken equal to CB.—Prof. Clerk Maxwell also made a communication on double and triple integration by summation.—Mr. J. W. L. Glaisher gave a preliminary account of the results of an enumeration of the primes in Burckhardt's tables (1 to 3,000,000).

PARIS

Academy of Sciences, April 2.—M. Peligot in the chair.—The following papers were read:—Isoperimetric triangles having one side of constant length and satisfying three other conditions, by M. Chasles.—On a theorem relative to expansion of vapours without external work (continued), by M. Hirn.—Report on a new work of M. Bertin, following his note on rolling. This work gives an account of M. Bertin's double oscillograph, which records each instant the inclinations of the ship in the direction of the rolling and the inclination of the part of the wave which carries the ship; also observations with it from the war-ship *Crocodile*. Though the indications are only approximate, they are thought of considerable interest. Admiral Paris called to mind an instrument devised by his son in 1867, for tracing waves.—Experimental researches on natural sulphides, by M. Meunier. It is a general fact that natural sulphides brought into the presence of suitable metallic solutions cause reduction in the free state of the dissolved metal. The experimental facts given seem to have a bearing on the *mineralogical associations* so frequent in metalliferous veins. If a vein of galena receive the infiltrations of sea-water (which always contains silver), all the silver will be held and concentrated by the sulphide. Now native silver exists in a certain number of galenas, and we may suppose it has been thus introduced.—New nebulæ discovered and observed at the Observatory of Marseilles, by M. Stephan. Thirty in number.—On the approximation of a class of transcendants which comprise, as a particular case, hyperelliptic integrals, by M. Laguerre.—On the paraboloid of eight straight lines, by M. Mannheim.—On the theory of frigorific machines, by M. Terquem. Even under the best conditions, frigorific air-machines cannot successfully compete with machines having volatile liquid, (1) because of the large size necessary; (2) the passive resistances due to this, and the use of two pumping bodies; and (3) the want of adaptability to produce different degrees of refrigeration. Their advantages are the production of lower temperatures, simplicity, and the use of a safe and cheap agent.—Researches on the metallic reflection of obscure and polarised calorific rays, by M. Mouton.—On the sulphide of manganese, by MM. De Clermont and Guiot. They produce the green sulphide in new cases, and by reactions in which its formation was said never to have been observed. Thus M. Muck says it is impossible to transform manganous carbonate into green sulphide; but the authors effect this by heating in free air, with ebullition, carbonate of manganese, precipitated with some sulphydrate of ammonia. They find the rose sulphide dried at 105° contains 9 per cent. of water (green sulphide at 105° is anhydrous). The rose sulphide is much more soluble in chlorhydrate of ammonia. These sulphides are thought isomeric modifications of one and the same body, more or less hydrated.—Reply to remarks of M. Chevreul concerning the phosphorescence of organic bodies, by M. Radziszewski. He adduces some facts showing that in phosphorescence the slowness of the reaction is an essential condition, though to define the maximum and minimum limits would be difficult at present.—Two cases of aneurism of the bend of the elbow treated successfully with the antiseptic ligature of catgut, by M. Boeckel.—On some abnormal fecundations in star fish, by M. Fol.—On the distribution of carbonic acid of the blood between the red corpuscles and the serum, by M. Fredericq. It is generally said that all or nearly all the carbonic acid held in the blood is in the serum (or plasma) in the state of combination or solution. Examining venous horse-blood, the author found the red corpuscles capable of absorbing a considerable quantity of CO_2, though always less than that taken by an equal volume of serum (about a half less). Passing a current of CO_2 through the blood, the excess seemed to be distributed equally between the corpuscles and the serum. While blood can be almost directly deprived of its gases by vacuum and heat; it is quite otherwise with serum, which, after such treatment, will give a fresh liberation of CO_2, when treated with phosphoric acid newly boiled. This invalidates some of MM. Mathieu and Urbain's results.—On the *rôle* of stomates and cuticular respiration, by M. Barthélemy. He thinks M. Merget's recent experiments overlook the most important factor in the case, viz., the living being submitted to experiment, the leaves having been detached from the plant and submitted to various vapours.—Observations of globular lightning formed and bursting without sound above a layer of clouds, by M. Blanc. The apparent diameter of the balls at 18 k. distance was 1°; they were reddish or yellow, but always white on bursting; they went horizontally, and looked like immense soap bubbles.

VIENNA

Imperial Academy of Sciences, March 1.—The following among other papers were read:—Main outlines of a theory of the sense of temperature, by M. Herzog.—Researches on the Tunicata of the Adriatic and the Mediterranean, by M. Heller. The freely-moving Salpæ and Salpæ-like Ascidians, which are numerous in the Mediterranean, are almost wholly wanting in the Adriatic.—On normal hexylic alcohol and normal œnanthylic acid, by M. Janecck.—Researches on the extension of the tonic vascular nerve-centres in the spinal cord of the dog, by M. Stricker.—The development of the antheridium of *Anthoceros*, by M. Waldner.—On Ranvier's representation of bone-structure, with remarks on the use of a Nicol in microscopic researches, by M. Ebner.—On metanitro- and metamido-benzacetylic acid; on the action of animal charcoal on salts; on solution of sulphur in acetic acid; and on demonstration of fuchsine in wine, by Dr. Liebermann. Fuchsine solutions give very characteristic absorption bands, in the spectrum, between yellow and green. Fuchsine may be detected even with a dilution of 1 : 500,000.—Note on molecular transformations, by M. v. Sonstorff. Iodine crystals kept eight years in a glass vessel were found to grow by volatilisation and subsequent condensation. Amorphous phosphorus passed, in part, into the crystalline state.—On the origin of the zodiacal light, by M. Noë.—Behaviour of calcium-phosphate towards sugar solutions, by M. Krasan.—On new Rudista from the Bohemian chalk formation, by M. Teller.—On the Sarmatian deposits between the Danube and the Timok, by M. Toula.—Researches on the etiology of Pelorian flower-forms, by M. Peyritsch.—On a new method of determining the internal resistance of galvanic batteries, by M. Fleischl. The two like poles of two equal elements (of the kind to be measured) are connected, and the resistance of this currentless combinaation is then compared with a known resistance.—On the geological character of the Isthmus of Suez; the pliocene formations of Zante and Corfu, the nature of the Sarmatian formation and its analogies in the present and in earlier geological epochs, by M. Fuchs. The fauna of the Red Sea and Mediterranean are very different, but they appear to have been so also before the isthmus arose.

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