THURSDAY, NOVEMBER 25, 1880

SULPHURIC ACID AND ALKALI

On the Manufacture of Sulphuric Acid and Alkali. By George Lunge, Professor of Technology in the Zurich Polytechnic School, formerly Manager of the Tyne Alkali Works, South Shields. Vol. ii. (London: Van Voorst, 1880.)

THIS volume forms a fitting sequel to the first volume of Prof. Lunge's valuable work (noticed in NATURE, vol. xx. p. 263) on the alkali manufacture. The praise we bestowed upon the earlier volume may without stint be applied to this. Clearness and conciseness in style remarkable in a foreigner, accuracy and fulness in the description of both old and new processes, and admirable woodcuts of apparatus and manufacturing plant, constitute the chief merits of this by far the best treatise extant on the most important branch of chemical industry. The value of works on technical science, as well, we may also add, of the teaching of such subjects, depends not only on a sound knowledge of the scientific principles upon which the manufactures are based, but likewise upon a thorough acquaintance with technical minutiæ and the special details of construction and operation, the due observation of which is necessary for the manufacturer's success. Either one of these conditions may be fulfilled by a host of authors, but to find both fully developed, as is the case with Prof. Lunge, is rare. Manufacturers themselves, many of whom may be fully competent to the task, are, for obvious reasons, not given to make known the details of their successful manufacture. Nor is the professional chemical engineer likely to do more than describe the most common and well-known processes. Dr. Lunge enjoys the great advantage of having had manufacturing experience, if not along the whole line, at least over a very large portion of his subject; and to this he now adds that of a position in which every motive urges him to impart his knowledge unreservedly to his readers.

A criticism worth having of a book like the one under review should by good rights imply a knowledge of manufacturing detail at least comparable with that of the author. To this the present writer can lay no claim, whilst mere indiscriminate praise is a mode of treatment to which he would not subject the readers of NATURE, either for their sake or for his own. In order therefore to find out how far this work really teaches what it professes to teach, how far it is abreast of the improvements of the day, and how far it expresses a sound opinion on vexed trade questions such as "open" as against "closed" salt-cake roasters, or as Hargreaves salt-cake process as against the old Leblanc's process, the writer has called to his assistance his friend and former pupil, Mr. John H. Crossley of Widnes, in whose ability in both the theoretical and practical side of the subject he has the greatest confidence, and to whom he is indebted for an opinion on these questions.

The opening chapters of the volume are devoted to a discussion of the various methods of making salt-cake or sodium sulphate the first great step in the production of alkali from common salt. It is a fact worthy of note that although Leblanc's process had been in successful work in France from the year 1797, seventeen years elapsed before this was taken up in England at Walker-on-Tyne by Losh. This may be perhaps accounted for by the war then raging by which communication between the two countries was almost entirely cut off, but especially because of the high war duty on salt, which in 1805 amounted to no less than 30*l.* per ton, and which existed up to the year 1823! This may be regarded as the year of birth of the manufacture on a large scale, and in this year James Muspratt, whom we are glad still to be able to salute as the veteran founder of the alkali trade, erected works at Liverpool, where common salt was decomposed with sulphuric acid and the Leblanc process carried out completely. The difference in cost of production in the early part of the century and in recent years is seen by the fact that in 1814 soda crystals cost 60*l.* per ton, whilst in 1861 the price was 4*l.* 10*s.*

Dr. Lunge goes into the question of "Close" *versus* "Open" salt-cake roasters pretty fully, but deriving his practical experience from Newcastle, where open furnaces are almost exclusively employed, it is not surprising to find a leaning towards the latter form betrayed in spite of his attempt to place the matter before his readers in an impartial manner. One of his arguments in favour of open roasters is that stronger sulphate is obtained by their use; he says (p. 93) "Owing to the higher temperature of an open roaster it is much easier to calcine the salt-cake and to decompose the common salt completely. In blind furnaces this can only be obtained by employing a large area and consequently a very thin layer of material and spending a good deal of time over the calcining process. This of course is much easier with furnaces possessing two muffles to one pan."

Against this fact it may be mentioned that though the Lancashire close roasters are certainly built larger than the Newcastle open ones, a much larger charge is worked, and many works regularly turn out salt-cake testing above 97 per cent., and this too in furnaces with one muffle only, the double form he speaks of and figures on pp. 72 and 90, being certainly represented in practice by one or two isolated specimens only.

Dr. Lunge (p. 116) gives 14–15 cwt. as usual charges of salt for close roasters, but as much as 18 cwt. are frequently worked at one operation. In this same question the author hardly gives due prominence to the "Plus Pressure" system, which is now doing good work with regard to close roasters. Probably the appendix to be published with vol. iii. will deal with this.

The description (pp. 115–125) of the actual working of a salt-cake furnace is very good indeed.

Chapter IV., on Hargreaves' Process, is also excellent, and is probably the best description extant; the only fault that could be found is that the figures show a double line of cylinders separated by an arch, the idea being to allow of having a drawing-door on each side of the cylinders. This however is not by any means compensated for by the greater loss of heat by radiation. Only the earlier plants are built this way, the more modern erections having the cylinders built back-to-back so as to form one solid block.

Dr. Lunge wisely refrains from much speculation as to the future of this most ingenious process. "If we are to

pronounce finally upon the prospects of this process," he says (p. 158), "we shall not find this quite an easy task. A few years ago English alkali-makers had such a high opinion of that process that no new vitriol chambers were built, and the question was discussed whether it was more worth while to work down the existing chambers, or to defray the cost of the new plant at once. Afterwards a less sanguine opinion gained ground, and it seemed as if Hargreaves's process would again be put into the background." At the present moment the writer believes that the outlook for the Hargreaves process is more favourable than ever.

Chapters V. and XII., on the Cost of producing Sulphate and Soda-ash, can only serve to give an approximate idea of the matter. The exact cost involved is not readily imparted by manufacturers, and is moreover governed by local circumstances, such as the current price of labour, &c.

The latter half of the volume is concerned with the second stage in the manufacture of alkali known as the black ash process. In this the salt-cake is heated with limestone and coal, the resulting carbonate of soda being removed by lixiviation from the insoluble alkali makers' waste. The first part of the chapter on Hand Furnaces appears to be very complete, and the figures on the plate facing p. 386 are correct and well-drawn. This can hardly be said of that portion relating to the modern revolving furnaces, this is probably the weakest part of the book, the author having apparently had no practical experience on this point. Figures of two revolvers are given; of these Fig. 182 may be said to represent a fairly good design, though one single wide evaporating pan is considered more convenient for repairs than two narrow ones. As regards the speed of revolution (p. 411) Dr. Lunge is a little out. He says the revolver gearing must be capable of giving speeds of one revolution in four minutes to five revolutions in one minute, "usually the highest velocity does not exceed one revolution per minute." Those figures are not correct for the present style of working. Revolvers should be able to go a good deal slower, but speeds as high as seven to eight revolutions per minute should always be possible, especially when working the Pechiney-Weldon process, when the after charge has to be very well and rapidly mixed through the rest. This can hardly be obtained when the large spin-wheel on the revolver is worked from a worm-wheel as Dr. Lunge describes, a pinion-wheel should be used. The author (p. 406) says, "Leaving aside the older constructions of revolving furnaces, we shall only describe two of the most modern." The first of these has been spoken of above; the second, figured pp. 414 and 415, a revolver fired by gas, was erected at one works only in 1870, and was found to be a failure; after running a year or two it was entirely reconstructed to burn fuel. Since then the mechanical bogies and engine gearing have been completely altered, so that the figures can hardly be said to represent one of the "most modern constructions."

Regarding chimney power Dr. Lunge says (p. 412) that usually every two revolvers have a chimney 6 feet diameter and 100 feet high to themselves. This is certainly not sufficient for the most economical working; to stint a revolver of draught is a serious mistake.

On another point in the black-ash process Dr. Lunge's opinions do not tally with those of Lancashire manufacturers. A few years ago Mr. Mactear of St. Rollox proposed a plan of adding from 6 to 10 per cent. of lime to the black-ash in excess of that usually worked. This apparently simple process was believed by some likely to work wonders, and statements were made as to the actual gain of many thousands of pounds per annum in a single works by its adoption. Dr. Lunge gives more credit to this than some of our Lancashire friends seem inclined to do.

The remaining processes in the great suite of chemical changes involved in the alkali trade are as thoroughly discussed by Dr. Lunge as those which have now been noticed. Divergent views concerning many details of these may doubtless be held by various manufacturers, but all will agree in the opinion not only that this is an excellent book, but that it would be very difficult for any one to write a better one.

H. E. ROSCOE

THE FLORA OF PLYMOUTH

Flora of Plymouth: an Account of the Flowering Plants and Ferns found within Twelve Miles of the Town, with Brief Sketches of the Topography, Geology, and Climate of the Area and History of Local Botanical Investigation. By T. R. Archer Briggs, F.L.S. With Map. 8vo, pp. xxxv. and 432. (London: Van Voorst, 1880.)

THIS is a model local flora. Mr. Briggs is well known as one of the most experienced and trustworthy amongst the botanists who have made a special study of British Phanerogamia. He has established a claim upon the gratitude of his fellow-workers by acting for several years as the honorary distributor of their Exchange Club, and in this capacity has received and sent out many thousands of specimens. The present work is the result of the rambles of twenty years, and as he has restricted its limits to a radius of twelve miles from the town, the whole of the district has been within walking distance of his home, and it is probable that there is no tract in Britain of which the plants have been worked out and placed on record in such a thorough and exhaustive manner. A radius of twelve miles from Plymouth includes a great variety of soil and situation. There are the maritime plants of the seashore and the tidal reaches of the Tamar and its affluents. Inland there are in the low country besides the stream-sides, meadows, and cultivated fields, plenty of woods and deep shady lanes with high banks and thick hedgerows, and the twelve miles radius reaches to a height of 1,700 feet on Dartmoor, and includes a considerable space of open uncultivated heathy and swampy ground. The district is not rich in limestone nor in ponds, but except in the plants which affect these two kinds of station there is full scope so far as situation goes to suit all their varied requirements in habitat.

Out of the 1,680 species enumerated in the last edition of the London Catalogue 873, or considerably more than half, are found within the radius covered by this book. Out of these 728 are natives, and the other 145 more or less certainly introduced by human agency.

It is interesting to have an area so far west in the island so thoroughly worked out, and certainly one of the most instructive points in connection with the matter is to note which British plants fail to reach and become very rare within the area. Taking the species according to their types of distribution as classified by Mr. Watson in the fourth volume of his "Cybele Britannica," and adopting the more stringent scale of species-limitation which he there follows, we find that out of 1,425 British species 764 grow in the neighbourhood of Plymouth. The 120 species of Watson's highland or extreme northern type and the 49 local or doubtful species are not represented here at all. Of the eighty-one species of his Scottish type we get only 5, and out of the 37 species of his intermediate type only 3 enter into the Plymouth area. So that the boreal element of the British flora, 238 species, is represented at Plymouth only by 8 species, such plants as *Rubus saxatilis*, *Gnaphalium dioicum*, *Polypodium Phegopteris*, *Polypodium Dryopteris*, and *Lycopodium Selago* lurking in very small quantity in the recesses of Dartmoor. Of Watson's 70 Atlantic or specially western species Plymouth has 36; of Watson's 127 Germanic or specially eastern species Plymouth has only 16; of the 532 species spread almost universally through Britain Plymouth has 484. Perhaps the most noteworthy point of all is that of Watson's 409 plants of the English type of distribution, plants spread widely through England, but running out in a northern direction north of the Humber and in the Scotch Lowlands, Plymouth gets only 220, or little more than half. Amongst the absentees in widely-spread English plants, for instance, are the common Forget-me-not (*Myosotis palustris*), the Mistletoe, *Genista tinctoria*, *Veronica Anagallis*, *Glyceria aquatica*, and *Scirpus lacustris;* and amongst the great rarities the common harebell (*Campanula rotundifolia*), the cowslip, the common butter-bur, *Hieracium boreale* and *vulgatum*, and some of the common south-country weeds, like *Solanum nigrum* and *Mercurialis annua*, which round about London are exceedingly plentiful. In the critical genera of British plants Plymouth is rich in rubi and roses, very poor in willows and hieracia. Amongst the rarities of the neighbourhood are *Polycarpon tetraphyllum*, *Erynium campestre*, *Pyrus Briggsii*, a curious pear with fruit like that of a small crab-apple, *Physospermum cornubiense*, and two species of *Hypericum*, *bæticum*, and *linariifolium*, and it produces some curious hybrid epilobia and rumices.

The area is divided into five districts, founded on river-drainage, two of which are in Cornwall and three in Devonshire; and under these the special localities of the species are carefully traced out, the abundance in which each occurs being particularised, and the claims of each to be regarded as wild being in all doubtful cases carefully investigated.

As stated in the title, the book includes a map and short sketches of the climatology and geology of the district, and of the progress of botanical investigation within its bounds from the days of Lobel and Parkinson down to the present day. We can recommend it with confidence to all our readers who are interested in geographical botany as one of the most complete, conscientious, and interesting works of its kind that have ever appeared.

OUR BOOK SHELF

Peruvian Antiquities: The Necropolis of Ancon in Peru. A Series of Illustrations of the Civilisation and Industry of the Empire of the Incas. Being the Results of Excavations made on the Spot. By W. Reiss and A. Stübel. (London: Asher and Co., 1881.)

A FIRST instalment now lies before us of this magnificent undertaking, which, if fully realised, bids fair to rival in scientific interest and typographical splendour Lord Kingsborough's great work on Mexican Antiquities. Reserving a full notice for a later stage of the project, it will suffice here briefly to indicate its main features, and direct attention to its paramount importance for antiquarian and ethnological studies. The authors, who have lately returned from South America laden with archæological treasures of all kinds, have been encouraged by the munificence of the directors of the Berlin Royal Museum to place the results of many years' diligent research at the disposal of the public. Under the general heading of "Peruvian Antiquities" the publishers, Messrs. Asher and Co., of Berlin and London, propose to issue simultaneously in English and German a series of folio volumes illustrating the whole field of the ancient Quichua-Aymara culture, such as it existed at the time of the Spanish invasion. The publication will spread over a number of years, each volume appearing in separate parts varying in number according to the nature of the subject. Each part will contain a number of chromolithographic engravings with corresponding pages of explanatory text. These illustrations, which of course are the great feature of the work, will be produced in the most finished style of modern typographic art, and will consist of perfect facsimiles either in natural or reduced size of every conceivable object associated with the ancient civilisation of the Incas. The series begins with a volume devoted entirely to the "Necropolis of Ancon," now an obscure watering place and fishing village on the Peruvian coast, a little north of Lima, but in pre-Spanish times evidently the centre of a thickly-peopled district that had long been occupied by a settled population. The "finds" made in the mummy graves of this burial-place are of extraordinary archæological interest, illustrating in the most vivid manner every aspect of the social and domestic life of the ancient Peruvians. The volume is to be completed during the course of the ensuing two years in ten uniform parts, as above described, and to judge from Part I., which has just appeared, it is likely to prove of the utmost value to the antiquary and ethnologist. But our remarks on all details must be postponed till this volume is completed. The English text has been entrusted to Mr. A. H. Keane, whose special knowledge of the subject must ensure accuracy in the descriptive and explanatory part of the work.

Exposé Historique concernant le Cours des Machines, dans l'Enseignement de l'École Polytechnique. 23 pp. (Paris: Gauthier-Villars, 1880.)

THE council for the improvement of the course of study at the Polytechnic School has for some time had under consideration a revision of the *Programme d'Instruction* of the two years' course, and at different times, for instance in 1865, steps have been taken with a view to their improvement, but, according to this pamphlet, different circumstances, especially in 1870, have deferred the realisation of such schemes. Upon such a wide subject our author does not venture, but he confines himself merely to that part which relates to the *Cours de Machines*. We are indebted for this very interesting and full historical sketch of the matter from the very foundation of the school to the veteran geometer, M. Chasles.

LETTERS TO THE EDITOR

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

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

Fertilisation of Yucca

IN NATURE, vol. xxii. pp. 606, 607, appears a letter to which my attention has only to-day been called—signed E. L. Layard—on the subject of the fertilisation of yuccas successfully introduced and cultivated in New Caledonia.

The writer shows himself to be under some misapprehension as to the generic characters and appearance of the insect which is generally credited with the fertilisation of these plants in their native country. The moth of the genus *Pronuba*, to which he refers, is not a "large moth having yellow under-wings." Although a common species belonging to the *Noctuidæ*, standing in our British lists under the genus *Triphæna* (Ochs), but included in Dr. Standinger's European Catalogue in the genus *Agrotis* (Ochs), is distinguished by the specific, not generic, name *pronuba* (Lin.), as well as by the characteristic appearance to which your correspondent evidently alludes.

The genus *Pronuba* (Riley) was founded for the reception of *Pronuba yuccasella* (Riley) (see *Proceedings* Acad. Sci. Missouri, ii. pp. 55, 333; *Report* Nox. Ins. Missouri, v. 151, vi. 131; *Canadian Entomologist*, iv. 182; Hayden's *Bulletin* of the U.S. Geological and Geographical Survey, iii. 121–141, &c.), which has also been described by Prof. Zeller in the *Verhandlungen der zoologisch-botanischen Gesellschaft in Wien*, 1873, vol. xxiii. pp. 232, 233, under the name *Tegeticula alba*.

This small white moth, of which some varieties have a few black dots on the fore-wings, belongs to the Lepidopterous group *Tineina* (Stn.), possibly to the family *Hyponomeutidæ*. Prof. Riley finds that the female, which has the basal joints of the maxillary palpi developed into a long curved tentacle furnished with spines, uses these appendages to collect and convey the pollen of the yucca to the tube of the stigma, which it could not otherwise reach; the eggs are then deposited, and the larva feeds upon the fruit, subsequently hibernating and becoming a pupa on the earth. It would be most interesting to ascertain whether *Pronuba yuccasella* (Riley) has been introduced with the yucca into New Caledonia, or whether any other insect, either indigenous or not indigenous to North America, has been found to take its place in carrying on the work of fertilisation. Prof. Riley considers the fact that yuccas introduced into the more northern portions of America have failed to produce seed may be attributed to the absence of *Pronuba*.

If Mr. Layard will direct his attention to this point he can scarcely fail to supply some valuable and instructive evidence bearing upon the subject. WALSINGHAM

Eaton House, Eaton Square, November 13

Skin Furrows of the Hand

ALLOW me to contribute the information in my possession in furtherance of the interesting study undertaken by your Japan correspondent (vol. xxii. p. 605).

I have been taking sign-manuals by means of finger-marks for now more than twenty years, and have introduced them for practical purposes in several ways in India with marked benefit.

The object has been to make all attempts at personation, or at repudiation of signatures, quite hopeless wherever this method is available.

(1) First I used it for pensioners whose vitality has been a distracting problem to Government in all countries. When I found all room for suspicion effectually removed here, I tried it on a larger scale in the several (2) registration offices under me, and here I had the satisfaction of seeing every official and legal agent connected with these offices confess that the use of these signatures lifted off the ugly cloud of suspiciousness which always hangs over such offices in India. It put a summary and absolute stop to the very idea of either personation or repudiation from the moment half a dozen men had made their marks and compared them together. (3) I next introduced them into the jail, where they were not unneeded. On commitment to jail each prisoner had to sign with his finger. Any official visitor to the jail after that could instantly satisfy himself of the identity of the man whom the jailor produced by requiring him to make a signature on the spot and comparing it with that which the books showed.

The ease with which the signature is taken and the hopelessness of either personation or repudiation are so great that I sincerely believe that the adoption of the practice in places and professions where such kinds of fraud are rife is a substantial benefit to morality.

I may add that by comparison of the signatures of persons now living with their signatures made twenty years ago, I have proved that that much time at least makes no such material change as to affect the utility of the plan.

For instance, if it were the practice on enlisting in the army to take (say) three signatures—one to stay with the regiment, one to go to the Horse Guards, and one to the police at Scotland Yard—I believe a very appreciable diminution of desertions could be brought about by the mere fact that identification was become simply a matter of reference to the records.

And supposing that there existed such a thing as a finger-mark of Roger Tichborne, the whole Orton imposture would have been exposed to the full satisfaction of the jury in a single sitting by requiring Orton to make his own mark for comparison.

The difference between the general character of the rugæ of Hindoos and of Europeans is as apparent as that between male and female signatures, but my inspection of several thousands has not led me to think that it will ever be practically safe to say of any single person's signature that it is a woman's, or a Hindoo's, or not a male European's. The conclusions of your correspondent seem, however, to indicate greater possibilities of certainty. In single families I find myself the widest varieties.

15, St. Giles, Oxford, November 13 W. J. HERSCHEL

P.S.—It would be particularly interesting to hear whether the Chinese have really used finger-marks *in this way*. Finger-dips (mere blots) are common in the East, as "marks."

The Aurora of the 3rd Instant

MR. E. DOWLEN has kindly communicated to me some particulars of the above as seen by him at Southport.

He first noticed the aurora at 6h. 50m. (it had however been visible before that time) as a greenish white glow on the north horizon. This gradually rose until 7h. 45m., when the top of the arch was estimated at two-thirds of the way up between the horizon and the Great Bear. It then gradually died out from the ends of the arch, and at 8h. 30m. had disappeared. During the time it was watched the following changes took place:—

From 7h. to 7h. 15m. it faded away from the eastern end until 7h. 30m., when nearly half the arch was gone. The western end then seemed to gather itself up somewhat, and to get brighter. After this the ends again lengthened out until 7h. 45m., when the whole began to fade away. At 7h. 25m. a narrow-arched band of black cloud concentric with the auroral arch was formed. It seemed to start from the ends, and meet over the middle point. At first this lay close upon the aurora. It then rose quickly, passed through the Great Bear, and vanished. It took about ten minutes to form, rise, and disappear.

Mr. Dowlen saw no streamers, but faint ones might have been present and escaped notice owing to adjacent gas-lamps. The aurora was at no time bright, and Mr. Dowlen doubts whether any beyond the green line would have been seen in the spectroscope.

The cloud formation detailed seems to me of considerable interest. J. RAND CAPRON

Guildown, November 19

Temperature of the Breath

THERE is no doubt that Dr. Roberts has discovered the true explanation of the phenomena that puzzled me and a good many others to whom I showed them. I have repeated Dr. Roberts's method of heating the enveloping material so as to expel all moisture from it, cooling it down to the temperature of the room and then breathing through it. In every case where I did so the thermometer showed a rise to 112° and upwards at the end of a minute; at the end of two minutes the index was pushed into the small bulb at the top, showing a temperature of about 116°. It is evident, therefore, that the high temperature observed is not the actual temperature of the breath, but is

caused by the caloric evolved by the transition of the aqueous vapour of the breath into the liquid or solid form.

Before seeing Dr. Roberts's explanation I referred the matter to the greatest living authority on heat, and he, after carefully repeating my experiments, was of opinion that the heat was produced by the compression of the air when forced through the material. Had he known of Dr. Roberts's simple but ingenious variation of the experiment there is no doubt he would have accepted Dr. Roberts's explanation. R. E. DUDGEON

November 18

THE following experiment may serve to supplement the observations of Dr. Roberts as to the cause of the high reading of a thermometer wrapped in a handkerchief and placed in the mouth. An ordinary non-registering thermometer was wrapped in about twelve folds of a dry linen handkerchief placed in the mouth, and the following readings taken at intervals of one minute :—Inspiration was effected through the nostrils, expiration through the handkerchief. The thermometer was in the mouth from the beginning to the end of the experiment. Temperature under the tongue before commencing, 37°·0 C. The reading of the thermometer wrapped as above described, one minute after introduction into the mouth, was 43°·0. At the end of the second minute, 44°·1, 3rd 42°·9, 4th 41°·2, 5th 39°·6, 6th 38°·2, 7th 37°·1, 8th 36°·9, 9th 36°·9, &c. After the experiment the temperature under the tongue was 37°·6. Capillarity is probably the chief cause of the rapid condensation of water, and the consequent liberation of heat in the dry fabric.

In connection with the above I may mention a schoolboy's trick, viz., gripping the arm of a schoolfellow with the teeth and breathing forcibly through his coat-sleeve. The sensation of heat thus produced is much greater than when the breath is allowed to impinge on the bare skin.

In conclusion, I must freely confess that Dr. Dudgeon completely upset my objection, as to compression of the bulb having anything to do with the high reading, by the experiments quoted in his last letter. F. J. M. P.

Coral Reefs and Islands

IN my letter on "Coral Reefs and Islands," published in NATURE, vol. xxii. p. 558, I have just noticed an important slip in writing which demands correction.

In the third paragraph and ninth line, for *metres* read *miles*, so that the passage shall read thus: "On the Florida coast we have barriers with channels 10–40 *miles* wide."

More accurately, the space between the southern coast of Florida and the line of Keys (old barrier reef) gradually widens from a few miles in its eastern to more than 40 miles in its western part. The channel between the line of Keys and the present reef is 6–7 miles wide and about 150 miles long.

Berkeley, California, November 2 JOSEPH LECONTE

Vox Angelica

I HAVE received a letter from Mr. Samuel Ray of Stoke Newington with reference to my remarks on the Vox Angelica stop on an Estey American organ. Mr. Ray informs me that Gordon's supplementary tuning-valve is used for the desired effects. The *rationale* of the method is, that by partly closing the mute the reeds are flattened, just as one reed is when the key is partially depressed. Mr. Ray also says, that by pulling out the stop a little way and making the reeds beat the latter are liable to be drawn out of tune; but this was the original method, but is now improved upon. A separate mute is placed on the top of the tubes, so that the wind strikes one of the sets of reeds vertically, whereby undue strain is avoided.

GEORGE RAYLEIGH VICARS

Woodville House, Rugby, November 18

Fascination (?)

PROBABLY none of your readers have thought it worth while to make any comment on the letters on this subject which have recently appeared, because it would seem needless to discuss the *origin* of "fascination" by means of the eye of a snake (or whatever may be the stimulus to the alleged condition) while all the evidence we can obtain from these reptiles in confinement proves that the condition does not exist. It devolves upon those who might object to observations on reptiles in a glass case as untrustworthy, to show us why—all their other actions being normal—the prisoners should not exhibit the same habit in respect to this "fascination," as they are alleged to practise when free. It is rather late in the year now; but if Mr. L. P. Gratacap will take the first opportunity of seeing snakes feed, and if any of your readers will pay a visit to the Zoological Gardens, both he and they will, I think, come to the conclusion that, beyond the expression of a little surprise (on the part of ducks and pigeons chiefly) which soon wears off at the sight of an *unfamiliar* object, both the birds and animals regard the snakes with marked unconcern. I have seen a guinea-pig, after finding no place of exit from the cage, quietly settle itself down in the midst of the coils of an Australian constrictor, shut its eyes and go to sleep. Ten minutes afterwards the snake had moved, and the guinea-pig was washing its face with its paws. Not once, but a dozen times, a rabbit has nibbled the nose of a River Jack viper (*V. rhinoceros*) in a pretty, inquiring way, heedless of the strong blows the reptile would administer with its snout to the impertinent investigator of that queer-looking object. For fully ten minutes one day a rabbit sat gazing at the poised and threatening head of a puff adder, now and then reaching forward to smell the reptile's nose, and anon sitting on its hind legs to wash its ears, and again returning to the "fascinating" object of its inquiries. If during that time the rabbit had fallen into the state of trance, it was so soon released from that condition as to be able to attend to its own comfort and busy itself about its toilet. The birds show no more recognition than the other animals of the dangerous position in which they are placed. We see them hopping about on the snakes and pecking lustily at their scales; sitting on the branches, preening their feathers and behaving themselves just as though no such dreadful (or pleasing?) sensation as "fascination" was possible!

I saw once a sparrow perched upon the body of a snake twisted round a branch, and preening itself. By-and-by a constrictor crept up slowly, touched the bird with its nose, and then threw the crushing folds around it. The deliberate approach of the snake and the unconscious attitude of the sparrow, concerned about its private affairs, would have staggered any ordinary believer in "fascination." I have closely watched the behaviour of snakes *intent* on feeding. It may be a sudden rush, when the victim has no time to see its enemy, or the gradual, lazy advance of the reptile; in either case the doomed victim betrays no suspicion of danger—at least so far as I have been able to ascertain after passing some hundreds of hours contemplating the snakes in the unequalled representative collection of the Zoological Society.

The expression in Mr. Gratacap's letter, "glittering" eyes, applied to the orbits of a snake, which are veiled by the "antocular" membrane, and capable of very slight movement, may remind us of Virgil's "Suffecti sanguine et igni," and help to confirm the "basilisk" (not a snake, by-the-by) superstition, but can only serve to perpetuate a myth. Whatever may be the value of Mr. Foot's opinion, I would ask, "Who has ever seen a snake 'raise its tail' after the manner of the cats?"

Charles Darwin has much to say on this subject to any one who chooses to consult the "Origin of Species." He does not see any advantage in the cat's "waving" tail or the noise of the "rattle" of *Crotalus*, for no predatory animal would derive any benefit from a signal of warning to its prey. The snake certainly never "waves" its tail when intent on mischief.

ARTHUR NICOLS

Soaring of Birds

REFERRING to NATURE, vol. xxiii. p. 10, may I suggest the following?—The question seems to be: "How can birds, having attained a certain elevation, thence rise without further muscular effort?" If I am not in error in what follows, they can theoretically do so if they *start* with a difference between their horizontal velocity and that of the wind, and *end* with a less difference; *e.g.*, if they start at rest with respect to the earth, and end by drifting with the wind entirely.

Take this last case, and consider the earth as plane, and the wind as horizontal, and having a velocity $= v$ with respect to the [earth and] bird. Finally we suppose the bird gains a horizontal momentum $= mv$. Then, by conservation of horizontal momentum, the only force acting being vertical, the air must lose an equal horizontal momentum.

Now we know that in all cases of bodies colliding and ultimately acquiring the same velocity, while we have conservation

of momentum we have loss of visible kinetic energy, except when the coefficient of restitution = 1. This kinetic energy is transformed into the vibrational kinetic energy of sound and heat in general.

But cannot we have it partly transformed into potential energy by "soaring" against gravity? On this supposition we have the two laws, conservation of momentum where no forces act, and conservation of energy, holding. But we have visible kinetic energy lost and *partly transformed into potential energy with respect to the earth*, partly (as usual) into vibrational kinetic energy of sound and heat. [The *sound* is evident in the "singing" of the wings.]

It seems to me that the swooping referred to by your correspondent is only a matter of convenience to the bird, and does not really affect the mechanical question; and that the comparison to a kite (which is held by a string) is not very satisfactory. But from my own observation of sea-gulls I do not think one can say that all the manœuvres and turns of the bird in the air are performed without real muscular effort, though certainly without *flaps* of the wing; and if there be muscular effort there can be work done—against gravity in this case.

The above is only a suggestion. I wish to induce some more mathematical reader to write a clear answer on this interesting question. W. LARDEN

Cheltenham, November 8

The Photophone

ON reading the description published in NATURE, vol. xxiii. p. 15, of Prof. Graham Bell's wonderful discovery, the transmission of speech by light, I notice that in "the photophone" the varying of the intensity of the beam of light thrown on the receiving instrument is accomplished by the simple and ingenious means of allowing the sound-waves to beat on the back of a thin plane mirror. It seems to me, however, that this arrangement is not complete, and is open to some objection. As the plane mirror will, if provision be not made against it, become convex and concave alternately, it must, unless the vibrations be confined within very narrow limits, give in one vibration *two* periods of maximum and minimum illumination at the receiver, and therefore the received sounds, apparently, should be (assuming the periods between each maximum and minimum illumination to be of the same duration, which could never exactly occur) an octave higher than those transmitted. This I think follows from the fact that the rays from the mirror would be dispersed not only when convex, but also when concave, *after* they had passed the focus. If, therefore, the vibrations of the mirror are sufficiently great to bring its focus between the mirror and the receiving instrument, there would be a second point of minimum illumination. If however the mirror were made slightly convex, or were constrained by a spring or otherwise, this defect would be cured.

Curiously enough, *theoretically* "the photophone" is the more effective the greater the distance between the transmitter and the receiver, as the degree of variation of the intensity of light falling on the selenium will be, when perfectly adjusted, greater as the distance increases, and it is on this element that the intensity of the sound depends. A. R. MOLISON

Ffynone Club, Swansea, November 15

[Our correspondent is obviously right in supposing that with a beam of light focussed accurately upon the selenium receiver a *single* complete vibration of the transmitting disk would produce *two* periods of maximum and minimum illumination. This would not however be the case if the lenses were not set originally to exact focus, for then a displacement of the disk in one direction would scatter the rays more, while a displacement in the other would concentrate them more. In practice, we believe, exact focussing is never obtained or even attempted.—ED.]

Salts of Zinc

IN Roscoe and Schorlemmer, vol. ii. p. 264, it states: "The salts of zinc do *not* impart to the non-luminous flame any tint;" and on p. 258, "the metal burns with a bright *white* flame."

What then is the green colour imparted to the Bunsen flame by zinc sulphate due to? Also the green flame obtained by heating metallic zinc on charcoal before the blowpipe? S.

THE green tint referred to by "S." (*supra*) as imparted by zinc sulphate to the Bunsen flame is only observed whilst the water of crystallisation contained in the salt is being given off; the dry salt which remains imparts no colour to the flame. It therefore appears probable that the green colouration of the flame is caused by very finely divided particles of the salt being carried off into the upper part of the flame by the escaping water of crystallisation. These particles then become so intensely heated as to emit the peculiar greenish light and very likely suffer previous reduction by the carbon of the flame. Other zinc salts, especially the acetate, impart to the flame, when first heated, a greenish-blue tint resembling that observed when metallic zinc is burnt in the air, this being doubtlessly due to a partial reduction of the acetate. The characteristic zinc lines (6362 and 6099 in the red, and 4928, 4924, and 4911 in the blue) are not seen in the case of the salts or when the metal is burnt. A more correct description of the combustion of zinc than that referred to would be: "the metal burns with a bluish-white flame."

Chemical Laboratory, Owens College W. BOTT

THE WORKS OF CARL VON NÄGELI

THE beginning of the forties in the present century marks an important epoch in the history of botany. The "Naturphilosophie" which had for many years so banefully influenced the development of the science, was being routed by the energetic attacks of Schleiden. Botanists were becoming alive to the fact that if their study was to have a place as a science by the side of physics and of chemistry, it must be pursued by the inductive method; that speculation must give way to research, and, above all, that development must be studied before any conclusions could be drawn from the investigation of mature forms. The early discoveries of von Mohl, and the demonstration of the cellular structure of the tissues by Schleiden, were among the first fruits of this awakening. To this period belongs also Nägeli's first contribution to science—a paper on the Development of the Pollen (1842). The first sentence in the introduction shows how thoroughly Nägeli was imbued with the same spirit which possessed Schleiden. He says:—"The right knowledge of an object includes an acquaintance with its mature form and a study of its development: the one is dependent upon the other, and the one without the other is insufficient to afford a complete conception of the object." The actual observations detailed in the paper appear from the drawings to have been accurate, and they were an important addition to the knowledge of the subject; but their interpretation was so far influenced by Schleiden's theory of cell-formation, which was then prevalent, that the process of the development of the pollen grains is described as being one of free cell-formation.

In the year 1844 appeared the first number of the *Zeitschrift für wissenschaftliche Botanik*, edited—probably on account of the sympathy existing between them—by Schleiden and Nägeli. This short-lived periodical (1844 to 1846) was practically an organ for the publication of Nägeli's researches and for the expression of his views, for it does not contain a single contribution from Schleiden's pen. The first number opens with an article—a sort of confession of scientific faith—"On the Present Aims of Natural History, and especially of Botany," in which he gives an account of the actual state of botanical knowledge, and strongly urges the necessity of empirical study in order that the generalisations of the science might be in the future, not baseless speculations, but inductions resting upon a firm foundation of ascertained fact. The *Zeitschrift* further contains an important paper "On the Nuclei of Cells and the Formation and Growth of Cells," in which the process of free cell-formation, which Schleiden had asserted to be universal, is shown to be only one of the processes by which a multiplication of cells is effected; these processes are clearly defined and classified. This is followed by a number of researches on the morphology of the lower cryptogams, which are of interest inasmuch as they open up new lines of approach to the study of the complicated morphology of more highly

organised plants. Nägeli showed, for instance, that since in a unicellular alga (*Caulerpa*) a morphological differentiation of root, stem, and leaf is indicated, morphological is not dependent upon histological differentiation. He discovered also that in the organs of certain cryptogams growth is effected by the repeated segmentation of a single apical cell, and that this segmentation may take place always in one plane only (*Delesseria*), or in two or three planes (stem of *Echinomitrium*, *Phascum*, *Iungermannia*, leaves of mosses).

In the following year (1847) he published his work on "The Classification of the Algæ" ("Die Neueren Algensysteme"), which is of great value, partly on account of the acute criticisms on the various proposed classifications of this group of plants which it contains, but more particularly on account of the number of new facts concerning their structure and life-history which are contributed. The descriptions of *Valonia*, *Udotea*, and *Acetabularia* may be especially mentioned: it is shown that they have essentially the same structure as *Caulerpa*. The same praise may be awarded to another work, "The Genera of Unicellular Algæ," which appeared two years later.

The next publication of importance was the first number of the *Pflanzenphysiologische Untersuchungen*, and the most interesting of the papers which it contains is the one on the Primordial Utricle. Attention is directed to its presence in all living cells, to its influence upon the osmosis of substances in solution into or out of the cell, and to its activity in forming the cell-wall; in short, it is clearly shown to be the living portion of the cell. The second number did not appear until 1858, although the MS. was ready in 1855, the delay being due principally to Nägeli's removal from Freiburg to Zürich, and then again from Zürich to Munich on his acceptance of the Professorship of Botany in that University. Although it must have been vexatious, still the delay enabled Nägeli to extend his researches in various directions and thus contributed materially to make the great work on starch-granules one of the most complete monographs which was ever written on any subject. This second number is entirely taken up by this work, which gives an account of these bodies, including their structure, development, chemical composition, and physical properties, as well as their distribution in plants. It is a monument of patient, accurate investigation, devoted to a subject which appears, at first sight, to be of limited interest, but which ultimately suggested one of the most remarkable generalisations of modern times, namely, what is known as Nägeli's theory of the structure of organised bodies. The primary fact upon which this theory is based is the property which starch-granules have of swelling-up—that is, of absorbing a certain amount of fluid with a consequent increase of bulk—when treated with certain reagents (dilute acids and alkalies), and of diminishing in size in consequence of a loss of water when treated with other reagents (alcohol). From these phenomena he inferred that the starch-granule consists of solid particles, which are impenetrable by water, but which are capable of taking up a certain amount of water between them, and that the amount of this water may vary according to circumstances. When the granule is absolutely dry, these solid particles—to which he gave the name of *molecules*—apparently come into perfect contact, for the granule does not lose its transparency, which would be the case if air were included in its substance.

It may be remarked here parenthetically that the word *molecule* used by Nägeli to designate these solid particles has not the same sense as it has when it is used in chemistry; one of these molecules is probably an aggregation of chemical molecules. In order to avoid any possible confusion on this score Nägeli has substituted the word *micella* for molecule in his more recent works. The forces by which these micellæ, with their surrounding watery areas, are held together are, firstly, the attraction existing between the micellæ; and secondly, the attraction which exists between each micella and the water which surrounds it; the latter of these attractions must necessarily be greater than the former, but whereas the former varies inversely as the square of the distance, the latter must vary inversely as some higher power. Thus, if A represent the attraction between two micellæ, B the attraction between a micella and the water, and D the distance between two micellæ, the limit of swelling-up or imbibition will be reached when $\frac{B}{D^{2+x}} = \frac{A}{D^2}$. As to the form of the micellæ, it is evident that they are not spherical or oval, for in that case the starch-granules would necessarily contain air when dry, and further, the denser parts of them would have to contain at least 26 per cent. of water, whereas, as a matter of fact, they only contain 14 per cent. They must be therefore more or less polyhedral, but they are not equiaxial since the swelling-up does not take place equally in all directions.

By this theory it was found possible to explain satisfactorily certain difficult points of structure, such, for instance, as the stratification of starch-granules and the striation and stratification of cell-walls. All these depend upon the alternation, in one or more planes, of dense and less dense layers. The proportion of solid to fluid is greater in the dense than in the less dense layers, or, in the terms of Nägeli's theory, the relative size of the micellæ to the watery areas surrounding them is greater in the layers of greater density. Further, this theory affords a satisfactory explanation of the mode of growth of a cell-wall. It is easy to understand that when the limit of extensibility is nearly reached—that is, when the micellæ of the membrane are separated as far as possible—new micellæ can be deposited in the interstices, the extended condition of the membrane being thus rendered permanent. This mode of growth is commonly known as *growth by intussusception*.

This is the stage to which the development of the theory is brought in this work. In the year 1862 Nägeli published a paper in the *Proceedings* of the Bavarian Academy on the "Application of Polarised Light to the Study of the Structure of Plants," which advanced it very considerably. He found, in the first place, that organised structures, such as starch-granules or cell-walls, are doubly refractive, and that this property is not affected by causing them to increase or diminish in size in consequence either of the absorption or removal of water, or by mechanical stretching or pressure. From this he concluded that the double refraction is not a property of the organised structure as a whole, but that it belongs to each individual micella: hence these micellæ must be crystalline. Again, from the interference colours which these objects present when examined with polarised light, he ascertained that the crystalline micellæ have three axes of elasticity, that they must be bi-axial crystals; and further, by comparing the effect produced by the passage of polarised light through glass under various degrees of pressure, he arrived at the conclusion that the micellæ are so arranged in the membrane of which they form part that one of their axes of elasticity is perpendicular to the surface, whereas the other two axes lie in the plane of the membrane. In a subsequent paper contained in the same periodical, he shows that the crystals of proteid substance, which occur in various seeds and tubers, have the same molecular constitution as starch-granules and cell-walls. By close and acute reasoning from carefully observed facts, Nägeli has therefore succeeded in establishing this theory of the molecular constitution of organised bodies, a theory which satisfactorily explains many of the peculiarities of structure and properties which they present. There can be little doubt that it is justifiable to extend this theory to the explanation of the intimate structure of protoplasm; in fact, in his later publications Nägeli has asserted as much, and in

this he is supported by such authorities as Sachs and Strasburger; but it is impossible to say anything at present as to the form and arrangement of the micellæ of protoplasm beyond this, that they do not so act upon polarised light as to suggest that they are crystalline. Full details on this subject, as well as a vast amount of other information, is given in the treatise on the Microscope (second edition, 1877), which Nägeli wrote together with Schwendener; fortunately an English edition of this important work may soon be expected to appear.

In tracing the development of Nägeli's theory, it has been necessary to depart from the chronological order of his works. In the years between 1858 and 1868 he published his *Beiträge zur wissenschaftlichen Botanik*, which include several important works, for the most part anatomical. In the first number there is an elaborate paper on "The Arrangement of the Fibro-vascular Bundles and the Mode of Growth in the Stem and Root of Vascular Plants," which is important as containing a purely morphological classification of the different forms of tissue of which these organs consist. This is followed by a detailed account, in the fourth number, of the mode of growth in thickness and of the arrangement of the fibro-vascular bundles in the stem among the Sapindaceæ, and this number also contains Nägeli's well-known investigation into the mode of development and growth of roots, in which Leitgeb was associated with him. This publication has a further interest connected with it, in that Schwendener's first papers on what is now known as his Lichen-theory appeared in it.

During this period Nägeli frequently contributed papers (the *Botanische Mittheilungen*) on a variety of subjects of botanical interest to the *Proceedings* of the Bavarian Academy, an activity which continues up to the present time. Allusion has already been made to some of these, and it would be worth while, did space permit, to give an account of most of them. Among the more important the following may be mentioned:—"On the Sieve-Tubes of Cucurbita," "On the Proteid Crystalloids of the Brazil-nut," "On the Development of Varieties," "A Theory of Hybridisation." Of late years Nägeli has turned his attention more especially to the study of the chemical composition and vital processes of the lower Fungi, such as Yeast and Bacteria. Among the interesting results obtained is the discovery, in yeast-cells, of a ferment (invertin) which converts cane- into grape-sugar, and of peptones. But the real importance of these researches only became apparent on the publication of two larger works, viz.: "The Lower Fungi in their Relation to Infectious Disease" (1877), and "A Theory of Fermentation" (1879). It is of course impossible to give here anything like a satisfactory account of the contents of these two books. The first treats fully of the important part played by Bacteria in infection and contagion, showing, in fact, that these organisms are the causes and carriers of the various forms of disease. In the second, after an exhaustive account of the process of alcoholic fermentation has been given, a new theory of it is propounded, based, not upon chemical principles, like that of Liebig, but upon the principles of molecular physics. Fermentation is defined as being "the communication of the oscillations of the molecules, groups of atoms, and atoms of the substances composing the living protoplasm to the molecules of the fermentible substance, in consequence of which the equilibrium of the molecules of that substance is disturbed, and decomposition is the result." It is also pointed out that, in the case of yeast, the sugar is to some extent decomposed within the cells, but for the most part outside them.

Though this account of his works is but little more than an enumeration of them, yet it will suffice to show how important are Nägeli's contributions to botanical science in the departments of morphology, anatomy, and physiology, not merely as additions to the accumulated store of facts, but as new generalisations from those facts, and as opening up fields for future research.

SYDNEY H. VINES

PROF. TAIT ON THE FORMULA OF EVOLUTION[1]

ANOTHER point to which I ought thus early to direct your attention is the necessity for perfect definiteness of language in all truly scientific work. Want of definiteness may arise from habitual laziness, but it much more commonly indicates a desire to appear to know where knowledge is not. Avoid absolutely all so-called scientific writings in which (as Clerk-Maxwell said) the attempt is made to "give largeness of meaning" to a word by using it sometimes in one sense and sometimes in another. It is true that we may thus economise in our language, and avoid the necessity for introducing new and hard terms. But it would be a most expensive and pernicious economy. It is only a blockhead who could object to the use of a new term for a new idea.

Our only source of information in physical science is the evidence of our senses. To interpret truly this evidence, which is always imperfect and often wholly misleading, is one of the tasks set before Reason. It is only by the aid of reason that we can distinguish between what is physically objective, and what is merely subjective. Outside us there is no such thing as noise or brightness:—these no more exist in the aerial and ethereal motions, which are their objective cause, than does pain in the projectile which experience has taught us to avoid. You will find many prominent ideas, relics of a less enlightened age, from which Natural Philosophy has not yet wholly shaken itself free, which owed their existence solely to the confusion of the subjective with the objective.

With observation and experiment as our sole sources of information we have no right, in physical science, to introduce *à priori* reasoning. We may (unprofitably of course) speculate on what things might have been, but we must not dogmatise on what they ought to have been; we must simply try to discover what they are.

For aught that we can tell, the properties of matter, and physical laws in general, might have been other than we find them to be. How can any one of us tell whether his conscious self might not have been associated in life with the body of an Eskimo or of a New Zealander, instead of with what he (no doubt) considers its much preferable tenement? Speculations of such a kind must always be wholly unproductive and unprofitable, but for all that we cannot but allow that they are not intrinsically absurd.

Some years ago a critic of Mr. Herbert Spencer's Philosophy happened to quote from a book of mine the remark I have just made (that the properties of matter might have been other than we find them to be). Mr. Spencer's observation on this point is highly instructive. Had he not been a severely grave philosopher I should have taken it for a joke. He said, "Does this express an experimentally ascertained truth? If so, I invite Prof. Tait to describe the experiments."[2] Mr. Spencer has quite recently published a species of analytical inquiry[3] into my "mental peculiarities," "idiosyncrasies of thought," "habits of mind," "mental traits," and what not. From his illustrative quotations it appears that some or all of these are manifested wherever there are differences between myself and my critic in the points of view from which we regard the elements of science. Hence they are not properly personal questions at all, but

[1] Part of an Introductory Lecture delivered October 26, 1880.

[2] In my letter (NATURE, vol. ix. p. 402) will be found an illustrative anecdote, which Mr. Spencer declares to be "*not to the point.*" A great scientific man, to whom I showed the correspondence, remarked that Mr. Spencer must be the only man in England who could not see the perfect appositeness of the anecdote.

[3] *Appendix to First Principles, dealing with Criticisms.* (Williams and Norgate, 1880.)

questions specially fitted for discussion here and now. I may, therefore, commence by inquiring what species of "mental peculiarity" my critic himself exhibited when he seriously asked me whether I had proved *by experiment* that a thing might have been what it is not!!

The title of Mr. Spencer's pamphlet informs us that it deals with *Criticisms;* and I am the first of the subjects brought up in it for vivisection, albeit I have been guilty (on Mr. Spencer's own showing) only of "*tacitly*" expressing an opinion! Surely my vivisector exhibits here also some kind of "mental peculiarity." Does a man become a critic because he quotes, with commendation if you like, a clever piece of analysis or exposition published by another?

In NATURE for July 17, 1879, I reviewed Sir E. Beckett's able little book, "Origin of the Laws of Nature," and as an illustration of that author's method I said:—

"He follows out in fact, in his own way, the hint given by a great mathematician (Kirkman) who made the following exquisite translation of a well-known definition:—

"'Evolution is a change from an indefinite, incoherent, homogeneity to a definite, coherent, heterogeneity, through continuous differentiations and integrations.'

"[Translation into plain English]—'Evolution is a change from a nohowish, untalkaboutable, all-alikeness, to a somehowish and in-general-talkaboutable not-all-alikeness, by continuous somethingelsifications and sticktogetherations.'"

Later in my article occurs the following paragraph, which also is quoted by Mr. Spencer:—

"When the purposely vague statements of the materialists and agnostics are thus stripped of the tinsel of high-flown and unintelligible language, the eyes of the thoughtless who have accepted them on authority (!) are at last opened, and they are ready to exclaim with Titania

"Methinks 'I was enamour'd of an ass.'"

The translation is from Kirkman's remarkable work, "Philosophy without Assumptions," which at that date I had just read with pleasure and profit. Humiliating as the confession may appear, I there saw Mr. Spencer's "Formula" for the first time, and I did not notice the title given to it. Hence, in quoting it from Kirkman, I very naturally called it by its proper name, a "Definition." For this I have incurred the sore displeasure and grave censure of the inventor of the definition. It seems I should have called him the *discoverer of the formula!* Now this is no petty quibble on words. It involves, as you will see immediately, an excessively important scientific distinction, to which your attention cannot be too early directed.

Mr. Spencer complains that an American critic (whose estimate is "tacitly" agreed in by Mr. Matthew Arnold) says of the "Formula of Evolution":—"This may be all true, but it seems at best rather the blank form for a universe than anything corresponding to the actual world about us." On which I remark, with Mr. Kirkman, "Most just, and most merciful!" But mark what Mr. Spencer says:—

"On which the comment may be that one who had studied celestial mechanics as much as the reviewer has studied the general course of transformations, might similarly have remarked that the formula—'bodies attract one another directly as their masses and inversely as the squares of their distances,' was at best but a blank form for solar systems and sidereal clusters."

We now see why Mr. Spencer calls his form of words a *Formula*, and why he is indignant at its being called a *Definition.* He puts his Formula of Evolution along-side of the Law of Gravitation! Yet I think you will very easily see that it is a definition, and nothing more. By the help of the Law of Gravitation (not very accurately quoted by Mr. Spencer) astronomers are enabled to predict the positions of known celestial bodies four years beforehand, in the *Nautical Almanac*, with an amount of exactness practically depending merely upon the accuracy of the observations which are constantly being made:—and, with the same limitation, the prediction could be made for 1900 A.D., or 2000 A.D., if necessary. If now Mr. Spencer's form of words be a formula, in the sense in which he uses the term as applied to the Law of Gravitation, it ought to enable us to predict, say four years before-hand, the history of Europe, with at least its main political and social changes! For Mr. Spencer says that his "formula" expresses "all orders of changes in their general course,—astronomic, geologic, biologic, psychologic, sociologic"; and therefore "could not possibly be framed in any other than words of the highest abstractness."

Added, November 11, 1880.

Mr. Kirkman has lately "discovered a formula" more general than that of Evolution, the "Formula of Universal Change." Here it is:—

"Change is a perichoretical synechy of pamparallagmatic and porroteroporeumatical differentiations and integrations."

Even to this all-embracing formula, with Mr. Spencer's leave, I would apply the humbler but fitter term "definition."

Of Mr. Spencer's farther remarks there are but three which are directed specially against myself. (Mr. Kirkman is quite able to fight his own battles.) He finds evidence of "idiosyncrasies" and what not, in the fact that, after proclaiming that nothing could be known about the physical world except by observation and experiment, I yet took part in writing the "Unseen Universe"; in which arguments as to the Unseen are based upon supposed analogies with the seen. He says:—"clearly, the relation between the seen and the unseen universes cannot be the subject of any observation or experiment; since, by the definition of it, one term of the relation is absent." I do not know exactly what "mental peculiarity" Mr. Spencer exhibits in this statement. But it is a curious one. Am not I, the thinker, a part of the Unseen; no object of sense to myself or to others; and is not that term of relationship between the seen and the Unseen always present? But besides this, Mr. Spencer mistakes the object of the book in question. The theory there developed was not put forward as probable, its purpose was attained when it was shown to be conceivable and not inconsistent with any part of our present knowledge.

Mr. Spencer's second fault-finding is *àpropos* of a Review of *Thomson and Tait's Nat. Phil.* (NATURE, July 3, 1879) by Clerk-Maxwell. Maxwell, knowing of course perfectly well that the authors were literally quoting Newton, and that they had expressly said so, jocularly remarked "Is it a fact that 'matter' has any power, either innate or acquired, of resisting external influences?" Mr. Spencer says:—"And to Prof. Clerk-Maxwell's question thus put, the answer of one not having a like mental peculiarity with Prof. Tait, must surely be—No." Mr. Spencer, not being aware that the passage is Newton's, and not recognising Maxwell's joke, thinks that Maxwell is at variance with the authors of the book!

Finally, Mr. Spencer attacks me for inconsistency &c. in my lecture on Force (NATURE, September 21, 1876). I do not know how often I may have to answer the perfectly groundless charge of having, in that Lecture, given two incompatible definitions of the same term. At any rate, as the subject is much more important than my estimates of Mr. Spencer's accuracy or than his estimates of my "mental peculiarities," I may try to give him clear ideas about it, and to show him that there is no inconsistency on the side of the mathematicians, however the idea of force may have been muddled by the metaphysicians. For that purpose I shall avoid all reference to "differentiations" and "integrations"; either as they

are known to the mathematicians, or as they occur in Mr. Spencer's "Formula." Of course a single line would suffice, if the differential calculus were employed.

Take the very simplest case, a stone of mass M, and weight W, let fall. After it has fallen through a height h, and has thus acquired a velocity v, the Conservation of Energy gives the relation

$$M\frac{v^2}{2} = Wh.$$

Here both sides express *real things*; $M\frac{v^2}{2}$ is the kinetic energy acquired; Wh the work expended in producing it.

But if we choose to divide both sides of the equation by $\frac{v}{2}$ (the average velocity during the fall) we have (by a perfectly legitimate operation)

$$Mv = Wt,$$

where t is the time of falling. This is read:—*the momentum acquired is the product of the force into the time during which it has acted.* Here, although the equation is strictly correct, it is an equation between purely artificial or non-physical quantities, each as unreal as is the product of a quart into an acre. It is often mathematically convenient, but that is all. The introduction of these artificial quantities is, at least largely, due to the strong (but wholly misleading) testimony of the "muscular" sense.

Each of these modes of expressing the same truth, of course gives its own mode of measuring (and therefore of defining) force.

The second form of the equation gives

$$W = \frac{Mv}{t}.$$

Here, therefore, force appears as the time-rate at which momentum changes; or, if we please, as the time-rate at which momentum is produced by the force. In using this latter phrase we adopt the convenient, and perfectly unmisleading, anthropomorphism of the mathematicians. This is the gist of a part of Newton's second Law.

The first form of the equation gives

$$W = \frac{M\frac{v^2}{2}}{h},$$

so that the same force now appears as the space-rate at which kinetic energy changes; or, if we please, as the space-rate at which energy is produced by the force.

Here are some of Mr. Spencer's comments:—"force is that which changes the state of a body; force is a rate, and a rate is a relation (as between time and distance, interest and capital); therefore a relation changes the state of a body."

The contradiction which Mr. Spencer detects here, and over which he waxes eloquent and defiant, exists in his own mind only. The anthropomorphism which has misled him is but a convenient and harmless relic of the old erroneous interpretations of the impressions of sense.

P. G. TAIT

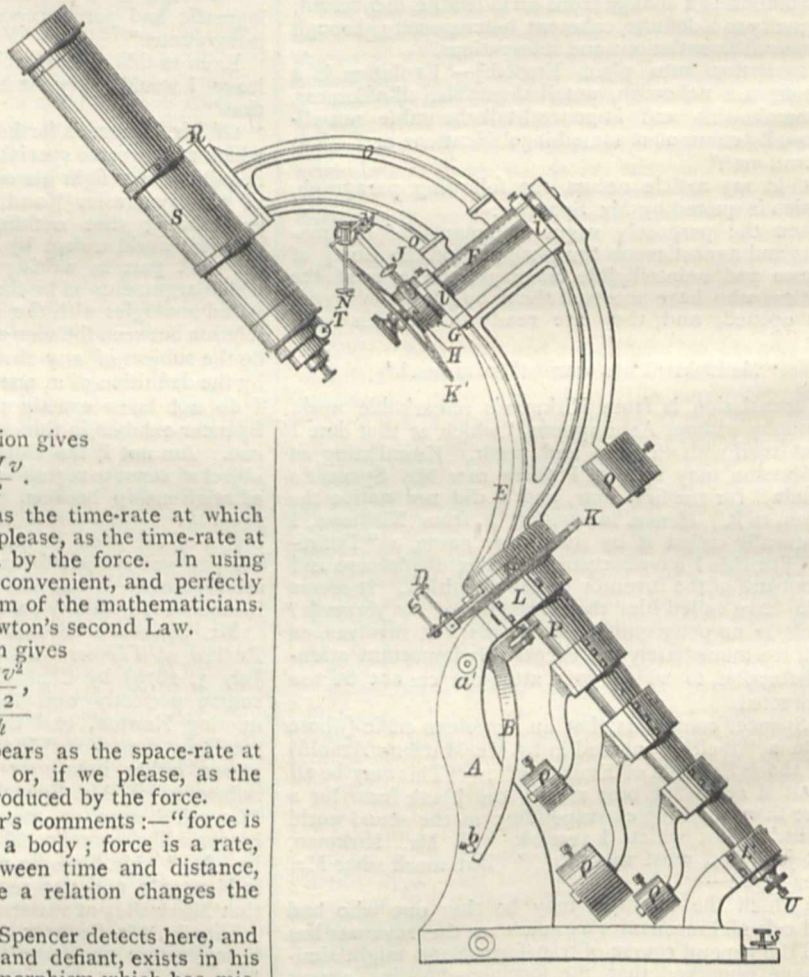
COMET-FINDERS

IT is only lately that the meteorites, or many of them which we see of a night making bright streaks in the heavens, have been shown to belong to definite streams having definite orbits and periods, and with the increase of our knowledge of these orbits the number of comets identified as travelling in the same orbits as meteor-streams has likewise advanced.

Now that the intimate relation between comets and meteorites has been settled, greater interest attaches to the discovery of these casual visitors, many of which have passed in our neighbourhood unobserved. This is shown by the increased number of comets seen, now that it is part of the business of several observatories to keep up a systematic search.

To do this properly, a telescope of large field of view is required, and a constant sweeping of the heavens must be kept up, and to do this with an ordinary equatorial is extremely tedious, owing to the continual change of the position of the body required.

To go back to early days of comet-finding, we call to mind the first instrument specially constructed for the purpose, so far as we are aware. It is a telescope of Galilean construction, with an object-glass of 2½ inches



diameter, and having a total length of 5 inches. This was made by Dollond during the first few years of this century for Dr. Kitchener. Since that time astronomical instruments have grown apace, and we have now before us Dr. Carl's "Repertorium für Experimental-Physik" containing a description of the new comet-finder constructed by Herr Schneider for the Observatory at Vienna.

The telescope of this instrument has an object-glass of

6 inches aperture and 4½ feet focal length, and the mounting is a striking change from what we are usually accustomed to see. The great point to be attained by it is to carry the telescope equatorially and allow it to move on a declination axis in such a manner that the eye-end remains stationary while sweeping the heavens. It will be seen from the plate which, by the kindness of Dr. Carl, we are able to reproduce, that the declination axis is carried above the polar axis somewhat in the usual way, but that the telescope, instead of being carried by its middle at the end of the declination axis, is carried by a frame, O, so that the eyepiece is in the prolongation of that axis, and also in the prolongation of the polar axis, so that it remains stationary, while the object-glass sweeps in all directions. The handles D and N, within easy reach of the observer, enable him to give the requisite motion to the telescope without the change of position necessary with an ordinary instrument. The telescope is balanced on the declination axis F by the counter-weight Q, and the excess of weight on one side of the polar axis is balanced by the counter-weights Q Q Q.

Herr Schneider proposes to mount telescopes of much greater size, say 30 or 40 feet long, in the same manner.

NOTES

M. MILNE-EDWARDS having completed the publication of his great work on "Physiologie Comparée," a subscription has been opened by M. Dumas, the Perpetual Secretary of the Academy of Sciences, for the purpose of presenting the veteran zoologist with a gold medal. Subscriptions are to be sent to M. Maindron at the Secretariat of the Academy of Sciences, or to M. Victor Masson, publisher, Boulevard St. Germain, Paris. M. Milne-Edwards's great work is composed of fourteen large octavo volumes—the first four of which are out of print—of 500 pages each; the publication began in 1857, and has been accomplished by twenty-three years of continual work. It includes all the lectures which have been delivered by M. Milne-Edwards at the Museum of Natural History during that lengthened period, and could not have been accomplished if the author had not had the advantage of the immense scientific resources accumulated in that establishment during the last two centuries for the study of nature.

A VIENNA correspondent sends us the following data regarding the Agram earthquake:—The damp ejected matter of the mud-volcano at Resnica near Agram was found to contain no elementary sulphur nor sulphuretted hydrogen, but it contained sulphur metals decomposable by acids, and earthy carbonates, along with organic substances of a humus nature. The chief constituent of it is fine sand with water, and it comes from no great depth. The mud-volcano at Sevete, near Agram, also ejects (2 m. high) chiefly a clayey-sandy mud, which may be thrown up by movements of the ground water. The Gratz geologist, Peters (writing in the *Tagespost*), characterises the Agram earthquake of November 9 to 14 as one of the most normal which could be observed in that region. The movement kept exactly the direction of south-south-west, and was thus precisely at right angles to the chief direction of the Eastern Alps. The entire breadth of the territory affected appears to be indicated by the towns of Klagenfurt (Carinthia) and Szegedin (Hungary). Since the formation of the Alps, and so through a long series of geological periods, all subterranean movements in this region of Central Europe have been in this one direction (as Süss first showed). For some months past movements have been perceived to be in progress in various localities. That Agram should be affected as it has been is explained by an inspection of the geological map. Not very far north from that town rises a remarkable block of greenstone surrounded by chlorite schist, limestone and other layers. A not very broad band of recent Tertiary deposits separates the low ground from that mountain block, which thus forms a comparatively fixed point in the system. Every movement coming from south-south-west propagated by these strata must impinge horizontally on the greenstone block, and cause a greater or less curvature of the strata, which manifests itself most where the lower ground remains free from Tertiary deposits. Unfortunately for Agram the strongest movement was directed precisely against that mountain block, and so upon the town before it. The whole phenomenon has nothing to do with volcanic processes. The repetition of the shocks is easily explained by the reaction from curvature of the strata not occurring all at once. In opposition to Peters, the astronomer and meteorologist, Rudolph Falb of Gratz, holds the Agram earthquake to be volcanic, and connected with the strong attraction of subterranean lava by the moon. They seem to have continued at more or less frequent intervals during the past week.

IN several parts of the Tyrol (Hall, Thaur, Rum, Innsbruck) an earthquake-shock was experienced on the 14th inst. about 9.15 a.m., and on the same day there was a considerable shock (lasting 20 sec.) in Bavaria, at Partenkirchen and Mittenwald about 8 p.m. Dr. Franz Woehner has been delegated by the Vienna Academy to Croatia to report on the phenomena.

A CYCLONE accompanied by earthquake shocks is reported to have occurred at Sitka in Alaska on October 25, causing much devastation.

JUST after the death of its founder, Dr. Broca, the well-known *Revue d'Anthropologie* entered on its tenth year. His successor in the direction of the *Revue*, Dr. Topinard, issues a prospectus intimating that it will be continued with renewed energy on the lines laid down by its founder. The *Revue* embraces all the varied departments of anthropology, and its editor has the collaboration of the most eminent workers in the varied departments in France. Broca left a great number of anthropological papers in various stages of completeness, and these are to be published in successive numbers of the *Revue*, which deserves every encouragement.

THE laboratory of M. Lacazes Duthiers at the Sorbonne has been opened this year again for experiments in zoology. In the summer it will be transferred to the coast station in Britanny.

THE Paris Museum of Natural History being situated in a somewhat out-of-way place, is rather deserted by the students, and great efforts are made to render the course of lectures which are delivered there unusually attractive. M. Fremy, Lecturer on Chemistry, will speak on the great discoveries in chemistry made almost simultaneously in Paris and in London about a century ago, and will perform all the original experiments, some of them with the very instruments which were used by the discoverers.

A VERY interesting acquisition has just been made by the botanical department of the British Museum. In 1783–4 John Millar made a series of water-colour drawings for the Earl of Bute, showing the "leaves, stalks, and ramifications of plants, for the purpose of ascertaining their several species." They are bound in five volumes, with an elaborately flourished title-page, and fill 928 octavo pages. The museum has purchased the drawings.

THE seismograph on Mount Vesuvius is said to indicate great subterranean dynamism. Streams of lava continue to flow down the north-west side of the cone, and are increasing both in volume and number. "The Vesuvian eruption," the *Times* correspondent states, "has entered on a phase of greatly increased activity. The news reached us on Saturday, but, as it appeared only in those papers which are directly interested in the Funicular

Railway, it was looked upon as an exaggeration to attract Sunday excursionists. It is now, however, confirmed that the lava is flowing over the side towards Naples, and, after having destroyed the outwork built to protect the upper station of the railway, is running rapidly in a vivid streak of fire parallel to the line, but at a distance which does not thus far imperil its safety. The spectacle is described as magnificent, and crowds were out watching the course of the lava and speculating on the fate of the Funicular Railway."

An International Congress of Electricians accompanied by an International Exposition of Electricity will be held in Paris during the autumn of 1881. This Exposition is to be opened (under the patronage of the Government, though at the pecuniary risk of private parties) on August 1, and to continue until November 15 following. The Congress of Electricians will meet on November 15 in the rooms of the Palace of the Trocadéro. Opportunity will be given for exhaustive research in all the various branches. The Exposition will remain open each evening until eleven o'clock to afford opportunities of testing the practicability of the different systems of electric lighting. The Congress is to assemble under the presidency of the Minister of Posts and Telegraphs, and the vice presidency of three French and three foreign delegates.

On February 10, 1879, a few gentlemen interested in the study of man met in the Smithsonian Institution to devise a method of mutual improvement. The effort resulted in the formation of the Anthropological Society of Washington, with Major Powell for president, and Dr. Reynolds and Prof. O. T. Mason as recording and corresponding secretaries. Twenty-four papers have been read, which, if one might judge from their titles, are most interesting and valuable contributions. We learn from the *American Naturalist* that it is not yet decided whether a journal will be published, inasmuch as the Smithsonian Institution and the Bureau of Ethnology "afford ample opportunities of preserving all papers of permanent value." Without doubting this fact, we still hope that this young and vigorous society may not only have its own publication, but also that a long career of activity may ensue to provide the material for filling the pages of the same.

Herr V. Bergsö, in a recent work, "Fra Mark og Skov," has given some interesting data in regard to the habits of the Tarentula, *Lycosa tarentula*, Latr., whose nests he has traced and examined on the Roman Campagna. He found that the nest, which was well rounded and smooth, was approached by a tunnel which, after running about a foot straight down below the surface of the ground, made a sudden short turn before it finally descended for about another foot into the spider's abode. The entrance to the tunnel is concealed by an arched covering made by the interlacing of grasses and leaves. The eggs are inclosed in a spun bag, and the young appear in the autumn, when they immediately seat themselves on the body of the mother where they remain till about April, neither parent nor offspring seeking food during their hybernation. As many as 291 individuals were on one occasion removed in February from the body of an emaciated tarentula. The superstitious error of assuming that the bite of the animal induces an irresistible desire of dancing is due to the fact, that dancing having been originally employed as a remedy against the poison, which is believed to be eliminated by profuse perspiration, the action of the poison was confounded with the means of its eradication.

Exotic butterflies have long, from their beauty, engaged the enthusiastic attention of wealthy collectors, some of whom, as notably the late Mr. Hewiston, have also enriched entomological literature with works containing coloured figures of their favourite insects. M. C. Oberthür of Rennes, who, with his brother René, is the possessor of a very extensive entomological museum, in which is contained the late Dr. Boisduval's collection of Lepidoptera, has just published his Quatrième livraison of a work, "Études d'Entomologie," which has more or less regularly appeared during the last few years. The present part is devoted to the "Papilionidæ" of his collection, and six coloured plates illustrate the species and varieties which he considers it necessary to describe.

Much interest has been excited in Norway by the recent appearance of a colony of beavers on the Voldifjord, a branch of the Frierfjord, which is at a considerable distance from the beaver-station still remaining at Omli on Nedenæs.

Under the title *Iagttagelser over Nordlys anstillede i Norge, Sverige og Danmark*, bearbeidede af Sophus Tromholt (Christiania), we have the results yielded by 839 observations of the aurora borealis, at 132 Scandinavian stations, on 154 nights, between September 1878 and April 1879 on which the northern light was visible. These observations are arranged under four heads in accordance with (1) longitude and latitude of stations; (2) time of year and age of moon; (3) colour, form, and altitude of streamers; (4) sound. Herr Tromholt considers that it may be accepted as certain, that the aurora is a local phenomenon, circumscribed by narrow limits, and manifested at inconsiderable distances from the earth's surface; that the light is generally white, and less often red or green, but that in latitudes higher than Bergen it not unfrequently presents spectral colours; and that the accompaniment of sound is an indisputable fact in relation to the auroral phenomenon. We learn from *Naturen* that Herr Tromholt has resumed his observations of the aurora borealis, to which he has devoted his attention for many years. It is his intention to make a catalogue of every recorded manifestation of the northern light in Norway; and for this purpose he requests the co-operation of other observers, and will be grateful for reference to any foreign sources of information, such as ships' logs, journals, weather tables, almanacs, &c., which might yield materials towards the better elucidation of this phenomenon.

In a letter addressed to Mr. Cust by Prof. F. W. Newman, and just published in the *Journal* of the Royal Asiatic Society, on the Libyan languages, the writer remarks that St. Augustine in his own day attested that one language prevailed in Roman Africa, and that it was quite natural to suppose the same to be the case now, when a large and striking similarity was found in the leading nouns and verbs. The changes however induced in 1500 years have broken up the original unity, and Prof. Newman states that we are now forced to admit at least four languages, each differing from the other more than German from Dutch, or Portuguese from Spanish.

The annual course of five lectures in connection with the Brown Institution will be delivered by Dr. W. S. Greenfield, Professor-Superintendent, in the theatre of the University of London, Burlington Gardens, W., on December 13, 15, 17, 20, and 22, at 5.30 p.m. Subject: Further Investigations on Anthrax and Allied Diseases in Man and Animals. Microscopic specimens will be exhibited on December 22 from 4.30 p.m.

We learn from *Psyche* that Miss Emily A. Smith, a well-known entomologist of Peoria, Illinois, has gone to Leipzig, where, if the university authorities will allow it, she will pursue a general course of zoological work in the new laboratory of Prof. Leuckart. This lady was recently elected a member of the Entomological Society of London.

Capt. H. King, R.N., writes with reference to the instances of fascination mentioned at p. 56, vol. xxiii., that having heard that the American ostrich might be enticed within gunshot by a person lying upon his back and kicking his legs and arms in the

air, he tried this with perfect success in Uruguay; he supposes that curiosity was the motive. A large coral upon the copper of a man-of-war, Capt. King states, is not unprecedented; he remembers in 1839 seeing one of the size and shape of a large cauliflower, taken from the bottom of a vessel of the Indian navy, in the Persian Gulf, by a pearl-diver.

PROF. GRAHAM BELL has promised to read a paper before the Society of Arts upon his "Photophone" at the ordinary meeting on Wednesday, December 1. As considerable interest is likely to attach to this paper it is announced that only members of the Society can be admitted, and that they will be required to provide themselves with special tickets issued for the occasion.

WE referred in the "Physical Notes" of our issue of November 11 to a paper read before the American Association at Boston by Prof. Young, which combated certain phenomena in thermo-electricity which were alleged to have been observed by Herr Exner. We have since received from Mr. T. Brown of Belfast a letter in which, on behalf of Prof. Franz Exner of Vienna, he expressly disavows any such discoveries as those which Prof. Young has set himself to refute. We readily accord to our courteous correspondent the opportunity for this disavowal, since any reflections cast even inadvertently upon the accuracy of Prof. Franz Exner's work might unfairly prejudice readers against the general reliability of the researches which he has published in another department of science, and which our readers are aware are just now exciting considerable attention.

IN NATURE, vol. xxii. p. 616, it was stated, on the authority of the Japanese papers, that Prof. Atkinson had, "during a sojourn in the Mitake Mountains of the Province of Koshu, discovered another valuable deposit of coal." We are now informed that although Mr. Atkinson visited the Mitake Mountains last summer, he can lay no claim to so important a discovery.

THE Hon. Sir Ashley Eden, K.C.S.I., has appointed Babu Ambika Churn Sen, M.A., and Synd Sakhawat Hossein, B.A., a native of Behar, to the two scholarships of 200*l.* a year each, recently created by the Bengal Government to be held at the Royal Agricultural College, Cirencester.

THE Procureur-General of Paris having sent an explanatory note stating that he did not mean to attack the character of the medical advisers of the public prosecutor, but merely to give vent to his peculiar views, these gentlemen have withdrawn their resignations and resumed their work.

THE Cutlers' Company have arranged for a course of lectures being delivered, or papers read, at the hall of the company during the ensuing winter season. The course will consist of four lectures or papers upon subjects intimately connected with the materials used in the manufacture of cutlery, the lectures to take place on the following dates:—Wednesday, December 1, 1880; Wednesday, January 5, 1881; Wednesday, February 2, 1881; Wednesday, March 2, 1881. Sir Henry Bessemer, C.E., F.R.S., has promised to commence the course, and will, on December 1, read a paper "On the Manufacture and Uses of Steel, with special reference to its employment for Edge Tools." The admission will be entirely free, but by ticket, which may be obtained on application to the hon. secretary, addressed to the Cutlers' Hall.

IT is announced that the electric cable manufacturing firm, Berthoud Borel and Co. of Cortaillod, in Neuchâtel, have made a highly important discovery in practical telegraphy. After a long and expensive series of experiments they have succeeded in devising a method of laying cables whereby the inductions of the electric current from one wire to another, although the wires are in juxtaposition, is prevented. This discovery, it is asserted, removes the last obstacle in the way of the widest possible extension of facilities for telephonic communication.

OUR ASTRONOMICAL COLUMN

THE THIRD COMET OF 1869.—This comet, the orbit of which has so close a resemblance to that of the comet discovered by Mr. Swift on October 11, was detected at Marseilles by M. Tempel on November 27, 1869, in the constellation Pegasus, and appears to have been last observed on December 31 at Leipsic and Kremsmunster, the hope of seeing it after the next period of moonlight not having been verified. On November 29 Dr. Vogel, observing at Leipsic, described it as a very faint large object elongated in the direction of the declination circle: in the comet-seeker its diameter was about 6′. On December 7 it was still very faint, large, and elongated in the direction 300°, the central condensation very slight. On the following night its diameter was 5′; it had "a peculiar milky appearance" and hardly any central condensation, so that observations were attended with difficulty. On the 21st it was seen only with much exertion of the eye, but on the 31st, though the comet was very faint, Prof. Bruhns considered his separate comparisons certain to about ten seconds of arc. At Kremsmunster Prof. Strasser found it "extraordinarily faint" during its entire visibility, and in consequence of wanting central condensation, very difficult to observe, and hence considered that his positions would not possess the ordinary degree of accuracy. The elements of the orbit were calculated by Tiele, Oppolzer, Schulhof, and Bruhns, the parabolic orbit published by the latter in No. 1788 of the *Astronomische Nachrichten* being founded upon nearly the whole extent of observation; he remarks with respect to it:—"Eine angestellte Vergleichung hiesigen Beobachtungen scheint aber doch auf eine Abweichung der Bahn von der Parabel hinzudeuten . . ." We are not aware that any further examination of this point was made. If the period of revolution be really something less than eleven years, the circumstance of the comet having escaped observation prior to 1869 will not nevertheless occasion surprise, considering that both in 1869 and 1880 it has approached near the earth and has yet been very faint and diffused, so that when the perihelion passage has occurred at other seasons of the year it might be beyond reach of the telescope. It will be most essential for the theory of the comet's motion that observations should be continued as long as possible at the present appearance, that if it prove to be one of short period its next return to perihelion may be closely predicted: the computation of the planetary perturbations during the period 1869-80 will of course be a necessary process with this object in view.

THE STAR LALANDE 1013-4.—Mr. G. Knott has examined this star, to which we lately drew attention, as being credited with the very discordant magnitudes 5, 7·7, and 10. He writes from Cuckfield on November 21: "I looked the star up on November 8 and again on November 19, and found it on each occasion 7·9 mag., and sensibly equal to B.D. + 51°, No. 131, which forms a convenient comparison star. This estimate, it will be seen, agrees nearly with that in the *Durchmusterung*; Harding marks the star 6m.

CHEMICAL NOTES

IN the last number of the *Berichte* of the German Chemical Society Herr v. Lippmann describes experiments which show that a solution of pure cane sugar, when charged with carbon dioxide, is slowly converted into inverted sugar. If the carbon dioxide be pumped into the sugar solution under pressure, the rate of inversion is considerably increased: at 100° the inversion takes place rapidly.

IN the *Annales Chim. et Phys.* the results of M. Raoul's experiments on the freezing points of alcoholic liquids are detailed. An aqueous solution of alcohol containing 1·6 per cent. by volume freezes at − 0·5; a solution containing 47·9 per cent. freezes at − 32°. The freezing point of solutions containing from 24 to 51 gram alcohol per 100 grm. water is decreased by 0°·528 for each gram of alcohol: when more than 51 grm. alcohol are present to 100 grm. water no regular decrease in the freezing point was observable. The freezing points of various wines are given in the paper referred to.

IN *Comptes rend.* M. Kessler announces that he has prepared a crystallised hydrate of hydrofluosilicic acid, viz., $H_2SiF_6 . 2H_2O$. The hydrate is a hard, colourless, very deliquescent solid, which fumes strongly in air, and melts at about 19°.

In the same journal M. de la Source describes his experiments on the dialysis of ferric oxide dissolved in a solution of ferric chloride. "Fer Bravais" of medicine consists of $30Fe_2O_3 . Fe_2Cl_6$; after three months' dialysis of a dilute solution of this substance the greater part of the chlorine had passed into the dyalysate, the proportion of ferric oxide to chloride was then $116Fe_2O_3 . Fe_2Cl_6$, and the chlorine yet continued to pass through the dialyser. The author thinks that ferric hydrate is, *per se*, under certain conditions soluble in water.

Herr A. Herzen describes in *Bied. Centralblatt* some experiments on acetous fermentation. In each of three flasks was placed 100 c.c. pure water: to the first flask 10 per cent. pure alcohol and a drop from the surface of a fermenting wine full of *Mycoderma aceti* were added; to the second flask were added 5 per cent. of pure acetic acid and a drop of the fermenting wine; and to the third flask were added 5 per cent. acetic acid, 5 per cent. of a saturated solution of boric acid, and a drop of the fermenting wine. After eight days at 25° no *Mycoderma* appeared in the first flask, much appeared in the second, and a little in the third. Hence the author concludes that *Mycoderma aceti* lives at the expense of acetic acid already formed in wine, and that it does not cause the transformation of alcohol into acetic acid, but that it is rather a consequence of this chemical change; further, that boric acid retards the development of *Mycoderma*, but does not prevent it in presence of already-formed acetic acid.

In *Dingler's Polytech-Journal* a paper appears by Drs. Lunge and Schäppi, on bleaching-powder. The results confirm the now generally accepted formula first proposed by Odling, viz., CaOClCl.

It was shown some time ago by H. T. Brown that alcoholic fermentation proceeds more slowly under diminished than under ordinary pressure. According to Boussingault (*Compt. rend.*), however, sugar is rapidly transformed into alcohol by the action of yeast, if the carbon dioxide and alcohol, as these are produced, be rapidly removed from the fermenting liquid. Addition of alcohol soon stops fermentation under ordinary circumstances; Boussingault shows that if the vessel containing the fermenting liquid be connected with an air-pump which is worked energetically, fermentation proceeds rapidly even when a considerable amount of alcohol has been added to the liquid.

In connection with the recent liquefaction of ozone by Hautefeuille and Chappuis, the following numbers, from a paper by the same authors in *Compt. rend.*, are of interest, as showing the exact influence of temperature and pressure on the ozonising of oxygen. Diminution of pressure does not tend to increase the amount of ozone produced, but decreased temperature exerts a marked action in increasing the amount of oxygen transformed into ozone:—

Tension of oxygen.	Tension of ozone.				Proportion of ozone by weight.			
	−23°.	0°.	20°.	100°.	−23°.	0°.	20°.	100°.
760	108·70	82·84	53·96	—	0·214	0·149	0·106	—
380	51·68	38·76	31·54	1·48	0·204	0·152	0·125	0·0117
300	40·20	30·60	22·20	—	0·201	0·1525	0·112	—
225	24·80	22·95	15·52	0·088	0·191	0·153	0·104	0·0118
180	22·30	16·38	10·52	—	0·181	0·137	0·089	—

A. Ditte describes in *Compt. rend.* a number of new fluorine compounds of uranium; the most important are $UF_6 . 8HF$ and UO_2F_2, produced by the action of hydrofluoric acid on the oxide U_3O_8; when the former compound is heated in a closed platinum dish it melts, gives off hydrofluoric acid and small quantities of the oxyfluoride UOF_4, which compound is produced in larger quantity by heating the above-mentioned oxyfluoride, UO_2F_2, in a closed vessel. The hexfluoride UF_6 is produced by heating the double salt $UF_6 . 8HF$ in an open crucible. Various double salts are also described, the general formula being $UO_2F_2 . 4MF$, where M may be K, Na, Li, Rb, or Tl.

Cleve has made a redetermination of the atomic weight of the very rare metal erbium (*Compt. rend.*). Assuming the formula of the oxide to be Er_2O_3, the atomic weight of the metal is 166. Pure erbia, Er_2O_3, is a beautiful rose-coloured earth, slowly soluble in acids, having a specific gravity of 8·64, and forming salts characterised by a deep-red colour; several of these salts are described by Cleve.

The same author has succeeded in separating nearly pure *thulium*; this metal and its salts are colourless, but solutions of the salts show two absorption bands, one strongly marked in the red, and one broad band in the blue. The atomic weight of thulium is 129·6 or 170·7, according as the metal is regarded as di- or tri-valent.

PHYSICAL NOTES

It is stated that amongst the recent discoveries of Prof. Bell in connection with the photophone research is the interesting fact that melted sulphur conducts electrically like selenium, but only at temperatures below that at which it thickens and becomes dark and viscid.

The *Comptes rendus* for November 2 informs us that Prof. Graham Bell and M. Janssen have attempted to *hear* with the photophone the sounds believed to accompany the rapid commotions taking place in the solar photosphere. The experiments were made at the Observatory of Meudon, a selenium cylinder being placed in different parts of an image of the sun some two feet in diameter. No very conclusive results were obtained, but M. Janssen has further suggested that a sort of concentrated effect might be obtained by passing a number of successive photographs of a sun-spot across a beam of light, the variations of the intensity of the beam producing sounds when they fall upon the sensitive "photophonic pile" of selenium. Some experiments in furtherance of this suggestion are now proceeding.

Having undertaken a series of researches upon the rapidity of evaporation of liquids, in dependence from the cohesion of molecules on their surfaces, M. Sreznevsky has measured how this rapidity varies with the variations of the height of the meniscus. He has established that, the diameter of the meniscus remaining invariable, the rapidity of evaporation increases as the height of the meniscus diminishes, that is, as its radius increases. There is however an anomaly as to this last law for distilled water: when the evaporation is measured in a meniscus the height of which is greater than the radius of its basis, the rapidity of evaporation increases throughout, however the radius of the meniscus begins by diminishing, and increases only after having passed through a minimum, but this minimum does not have a corresponding minimum in the rapidity of evaporation.

At the recent meeting of the Helvetic Society of Natural Sciences M. Forel described a *thermal bar* which is developed in winter parallel to the shore of a lake of fresh water, and which separates the pelagic from the littoral region. The water of the former region remains long, and in some lakes always, at a temperature above 4° C.; in the littoral region, if the winter be cold, the temperature descends between 4° and zero; and between the two there is a band of water at 4°, descending to the bottom—a kind of mountain with crest parallel to the shore and a talus on either side.

M. Dufour described at the same meeting an apparatus for indicating the variations of chemical intensity of the sunlight. It has some likeness to Draper's tithonometer; the principle is, opposing the variable action of light on a mixture of chlorine and hydrogen, with an electric current (of variable intensity, and measurable each instant), which by its passage causes decomposition of a quantity of hydrochloric acid equal to that produced by action of the light on the mixture of chlorine and hydrogen. The apparatus is like a Rumford differential thermometer; in one bulb is some hydrochloric acid solution, with carbon electrodes, in the other some sulphuric acid. The light acts on the former. One mode of measurement is to note the time taken in displacement of the sulphuric acid column a certain distance along the connecting tube. Then bring back the column to its original position by passing the current.

M. Pictet has lately made experiments (*Arch. de Sci.*) as to the dissolving power of gases and vapours on one another. Various solutions of alcohol and water were successively put into one of two glass balloons connected by a tube; pressure was diminished with an air-pump, so that the space became filled with vapours from the mixture. By closing the tapered point of the second balloon with the blowpipe, the apparatus allowed of distillation being effected with small differences of temperature. Plunging successively the balloon that held the solution in water at from 0° to 80°, and the other in water only 1° or a fraction of a degree below that of the liquid, M. Pictet got condensed products, the quality of which indicated what "affinity of solution" existed between water and alcohol. The following conclusions were arrived at: The weight of condensed liquid is proportional, in unit time, to

the *difference of temperature* between the liquid in ebullition and the condensed liquid. The weight of liquid condensed in unit time is independent of the interior pressure or of the mean temperature during distillation. Analysis shows that the gases have no power of solution on one another. M. Pictet was thus led to an industrial process for rectification of spirits.

GEOGRAPHICAL NOTES

AT the meeting of the Geographical Society on Monday, Sir Bartle Frere read what may best be described as a suggestive paper on Temperate South Africa as a route to the Central Equatorial Region. After defining the temperate region as the vast tract of country extending to Cape Frio on the Atlantic coast and to the mouth of the River Tugela on the opposite side of the continent, and giving a brief account of its geography, &c., Sir Bartle addressed himself chiefly to the task of pointing out how it could be made available as a base of operations in exploring the country north of the Zambesi, and suggesting agencies which might be turned to account for the extension of geographical knowledge. These agencies are the traders and hunters, who have a wide acquaintance with many regions otherwise unknown, and missionaries of various denominations. The latter have no less than eighty-four fixed stations beyond the colonial boundaries, manned by 812 Europeans, many of whom are highly-cultivated and intelligent men, and have great opportunities for acquiring geographical information. Sir Bartle Frere also hoped that the Council of the Society might see their way to urging the Government to undertake a proper survey of the coast-line, as well as of the interior of the five colonies.

AT the meeting of the Berlin Geographical Society on November 6 the safe arrival of Dr. Lenz at Timbuctoo (by a route not before taken by any European) was announced. Two of his followers were lost in the desert, and two had gone back. Dr. Stecker (who lately went to Massowah with Herr Rohlfs) will, according to circumstances, either push through the Galla regions or to the East coast, or to the Great Lakes. Major v. Mechow reached a town on the Quanza, in the territory of the Hollo, about 200 km. from Malange on July 19, after great difficulties, especially in carriage of the boat. The natives were friendly throughout. A little above the place reached are the two last falls of the Quanza, between which is the mouth of the Cambo. The Major seems to have been the first white to visit these waterfalls. He was going to Löpung with a view to determine the course of the Quanza. Dr. Pogge and Lieut. Wissmann were also travelling in that region the same month, intending to reach Mussumba, the residence of the Muata Jambo; Dr. Pogge's object is to establish stations in the interior. Lieut. Wissmann will make journeys for topographical and collecting purposes. The Italian traveller, Dr. Matteucci, is seeking to reach Bornu from South Dar-For, going round Wadai and Bagirmi. *Inter alia* the Society resolved to memorialise the German Government to take part in the international project of systematic Polar investigation.

AT the sitting of November 19 of the Société de Géographie of Paris M. Zweifel received the palm of Officer of the Academy as a reward for the discovery of the sources of the Niger, in company with M. Marius Moustier. The laureate declining to speak himself, an address was delivered on behalf of him and his companions by Dr. Harmand, the well-known explorer of Cochin China. It appears that MM. Zweifel and Moustier saw a granite rock from which the powerful stream takes its rise; but they were not admitted to the site, owing to the high priest of Tembi Saleh, who inhabits an island situated on a small lake formed by the stream at a very few miles from its source. So something more remains to be done to complete the work begun by Laing, Reade, and Blyden.

SIR ALLEN YOUNG leaves England next month in his yacht, and will visit, among other places, the Canary Islands, a portion of the West Coast of Africa, and St. Helena, extending his voyage as far as the Cape, where he will make preparations and inquiries for a projected expedition of discovery to be undertaken by him to the Antarctic regions. It will be remembered that the *Erebus* and *Terror*, commanded by Sir J. Ross and Capt. Crozier, penetrated in 1841 to 78° 4′ S., a latitude which has never been reached before or since.

THE November number of *Petermann's Mittheilungen* has a long paper by Spiridion Gopčevič, containing his ethnographical studies in Upper Albania. A very fine map embodies the important results of Severzov's exploration of the Pamir in 1878, with accompanying text, followed by an account of Lieut.-Col. Pjevzov's journey through Mongolia in 1878–9, to Kuku-Choto and Kalgan. A summary is given of the Arctic work of 1880, followed by the usual monthly notes.

THE first *Bulletin* of the recently-formed International Geographical Institute at Berne consists of a programme of the projected Italian Antarctic Expedition under Lieut. Bone, which is to leave Genoa in March 1881. A sketch is given of what has been previously done in this region, showing that the field is practically virgin so far as scientific work is concerned. The programme of the Italian expedition is very comprehensive, and the ultimate object is to pave the way for the establishment of an Antarctic observing station.

NO. 3 of vol. iii. of the *Deutsche geographische Blätter*, the organ of the Bremen Geographical Society, contains the continuation of the unfortunate Dr. Rutenberg's journal in Madagascar, and the lecture given at the Danzig meeting of the German Association by Dr. Neumayer on "Polar Expeditions or Polar Research?" To the latter able lecture we referred last week, the point insisted on being that while the two are perfectly congruous, the former should be subjected to the latter, which must be carried out on the system of Polar observatories advocated by Weyprecht, and to which nearly every civilised nation adheres except England.

THE new number of the Marseilles Geographical Society's *Bulletin* contains a very voluminous account by Messrs. Zweifel and Moustier, of their expedition to the sources of the Niger. This memoir is illustrated by a map showing their route, and supplemented by an appendix containing information as to the natural resources of the country traversed, the races of the interior, &c.

THE last part of *Le Globe* contains a paper (with map) on the Island of Cyprus, by M. Paul Chaix, and some account of recent researches in the Pamir, furnished by M. Veniukoff.

IN the current number of *Les Missions Catholiques*, M. Armbruster has commenced a series of papers on Corea, drawn from information furnished from time to time by the Romish missionaries, the only Europeans who have ever had any opportunity of acquiring a real knowledge of the interior.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—The preliminary examination in the Natural Science School begins on Monday next, November 29. The Final Honour School begins on Monday, December 6.

The Brakenbury Scholarship in Natural Science at Balliol College has been awarded to Mr. William Stroud, from Owens College, for proficiency in physics and chemistry. *Proxime accessit* Mr. A. D. Hall, from Manchester Grammar School. Mr. J. J. Hart, Manchester Grammar School, and Mr. J. E. Marsh of Balliol, are honourably mentioned.

ON A METHOD OF DETERMINING THE CRITICAL TEMPERATURE FOR ANY LIQUID AND ITS VAPOUR WITHOUT MECHANISM[1]

A PIECE of straight glass tube—60 centimetres is a convenient length—is to be filled with the substance in a state of the greatest purity possible. It is to contain such a quantity of the substance that, at ordinary atmospheric temperatures, about 3 or 4 centimetres of the tube are occupied by steam of the substance, and the remainder liquid. Fix the tube in an upright position, with convenient appliances for warming the upper 10 centimetres of the length to the critical temperature, or to whatever higher or lower temperature may be desired; and for warming a length of 40 centimetres from the bottom to some lower temperature, and varying its temperature conveniently at pleasure.

Commence by warming the upper part until the surface of separation of liquid and steam sinks below 5 centimetres from the top. Then warm the lowest part until the surface rises

[1] By Sir William Thomson, British Association, Swansea, Section A. Tuesday, August 31.

again to a convenient position. Operate thus, keeping the surface of separation of liquid and solid at as nearly as possible a constant position of 3 centimetres below the top of the tube, until the surface of separation disappears.

The temperature of the tube at the place where the surface of separation was seen immediately before disappearance is the critical temperature.

It may be remarked that the changes of bulk produced by the screw and mercury in Andrews' apparatus are, in the method now described, produced by elevations and depressions of temperature in the lower thermal vessel. By proper arrangements these elevations and depressions of temperature may be made as easily, and in some cases as rapidly, as by the turning of a screw. The dispensing with all mechanism and joints, and the simplicity afforded by using the substance to be experimented upon, and no other substance in contact with it, in a hermetically sealed glass vessel, are advantages in the method now described. It is also interesting to remark that in this method we have continuity through the fluid itself all at one equal pressure exceeding the critical pressure, but at different temperatures in different parts, varying continuously from something above the critical temperature at the top of the tube to a temperature below the critical temperature in the lower part of the tube.

The pressure may actually be measured by a proper appliance on the outside of the lower part of the tube to measure its augmentation of volume under applied pressure. If this is to be done, the lower thermal vessel must be applied, not round the bottom of the tube, but round the middle portion of it, leaving, as already described, 10 or 20 cms. above for observation of the surface of separation between liquid and vapour, and leaving at the bottom of the tube 20 or 30 cms. for the pressure-measuring appliance.

This appliance would be on the same general principle as that adopted by Prof. Tait in his tests of the *Challenger* thermometers under great pressure (*Proceedings*, Royal Soc. Edin., 1880); a principle which I have myself used in a form of depth-gauge for deep-sea soundings; in which the pressure is measured, not by the compression of air, but by the flexure or other strain produced in brass or glass or other elastic solid.

ABNORMAL VARIATIONS OF BAROMETRIC PRESSURE IN THE TROPICS, AND THEIR RELATION TO SUN-SPOTS, RAINFALL, AND FAMINES

IN the first part of his work on the Meteorology of the Bombay Presidency, which was submitted to Government in August, 1875, Mr. Charles Chambers pointed out that the variation of the yearly mean barometric pressure at Bombay shows a periodicity nearly corresponding in duration with the decennial sun-spot period (see "Meteorology of the Bombay Presidency," § 26, p. 12), and in August, 1878, in a letter to NATURE, vol. xviii. p. 567, I drew special attention to this relation, pointing out that the observations of the winter and summer half-years, separately as well as conjointly, show that the pressure is low when the sun-spot area is great, and *vice versâ*, but that the pressure curve *lags behind* the sun-spot curve.

In November of the same year the eminent physicist, the late Mr. John Allan Broun, regarding the relation thus established between the variations of barometric pressure and sun-spots as one of very great importance, in that it gave a probability to the existence of similar laws in the variations of other meteorological elements which he believed was previously wanting, communicated to the same periodical (NATURE, vol. xix. p. 6) an article in which he showed, from the observations recorded at Singapore, Trevandrum, Madras, and Bombay, that the years of greatest and least mean barometric pressure are probably the same for all India, and from this he inferred that the relation to the decennial sun-spot period found for Bombay holds for all India.

In December, 1878, Mr. S. A. Hill supplemented and confirmed Mr. Broun's communication by giving similar data for Calcutta (NATURE, vol. xix. p. 432).

In May, 1879, Mr. E. D. Archibald communicated to NATURE, vol. xx. p. 28, the fact (brought to his notice by Mr. S. A. Hill) that at St. Petersburg the mean annual barometric pressure is high when the sun-spots are numerous, low when they are few, but that the pressure epochs lag behind the sun-spot epochs.

In December of the same year Mr. Blanford presented to the Asiatic Society of Bengal a paper (*Journal* of the Asiatic Society of Bengal, vol. xlix. part ii. 1880, p. 70) in which it was shown that the barometric observations recorded at Batavia from 1866 to 1878, at Akyab, Chittagong, and Darjeeling from 1867 to 1878, at Port Blair from 1868 to 1878, and at Singapore from 1869 to 1878, afford more or less confirmation of the results previously obtained for other stations in India.[1] And in the same paper Mr. Blanford brought forward the observations recorded at the Russian observatories at Ekaterinburg, Slatoust, Bogolowsk, and Barnaul from 1847 to 1877, and showed that at the two former stations during the whole period, and at the two latter during the first half of it, the barometric variations were similar to those previously obtained by Mr. Hill for St. Petersburg.

In a subsequent letter to NATURE, published in March, 1880, Mr. Blanford discussed the same observations in greater detail, dealing with the summer and winter observations separately, as well as conjointly, and showed that the decennial variation of the barometric pressure found for St. Petersburg was exhibited only by the observations of the winter months. He also obtained similar results for Ekaterinburg and Barnaul, but he appears to have overlooked the very important facts that the range of the winter curves rapidly decreases in passing from St. Petersburg, through Ekaterinburg to Barnauland, that the summer curves for the two latter stations are, on the whole, of the *same* character as the summer curves of the Indian stations, as may be seen by comparing the dotted curves for Ekaterinburg and Barnaul, given in NATURE, vol. xxi. p. 48, with the summer curve for Bombay, given in vol. xviii. p. 568 of the same periodical. He also showed that the barometric curves for Batavia, Singapore, and Port Blair were, as at other Indian stations, of the same character both in winter and summer.

In 1873 and 1874 (see British Association Reports for those years) Mr. Meldrum showed that there was strong evidence of a connection between sun-spots and rainfall, and he has recently (see *Monthly Notice* of the Meteorological Society of Mauritius for December 1878) put this question beyond all reasonable doubt by showing that the mean yearly rainfall of Great Britain, the continent of Europe, America, India, and the Southern Hemisphere, varies in the same way as the sun-spots, being on the average great when they are numerous, small when they are few.

In my "Brief Sketch of the Meteorology of the Bombay Presidency"[2] in 1876, I pointed out that the abnormal variations of the monthly mean barometric pressure in that year were mainly variations in the intensity of the usual seasonal movements, although at least some portion of the variations influenced a wider area than the Indian monsoon region, and in the Sketch for 1877 I attributed the uniformly high barometric pressure and the deficient rainfall of that year to a weak development of the equatorial belt of minimum pressure, probably induced by a diminution of the solar heat.

In the Report on the Meteorology of India in 1877 Mr. Eliot showed that the high pressure of that year was a characteristic of the whole Indian area and also of Australia.

In my meteorological sketch for 1878 I showed that the abnormal barometric movements observed at Zi-ka-wei in China and at Manilla in 1878 were similar to those recorded in Western India; that the latter largely influenced the rainfall of the Bombay Presidency; and that in former years of deficient rainfall at Bombay the barometer had been relatively high, not only at Bombay, but also at Mauritius and Batavia.

In the paper (*Journal* of the Asiatic Society of Bengal, vol. xlix. part ii., 1880, p. 70) already quoted, Mr. Blanford has confirmed the fact that the excessive pressure observed in the Indian area in the years 1876 to 1878 extended to China and Australia, and he has also shown that it affected Western Siberia also.

In my sketch for the year 1879 I have shown that these uniform variations of barometric pressure are accompanied by a nearly uniform variation of the percentage rainfall of all portions

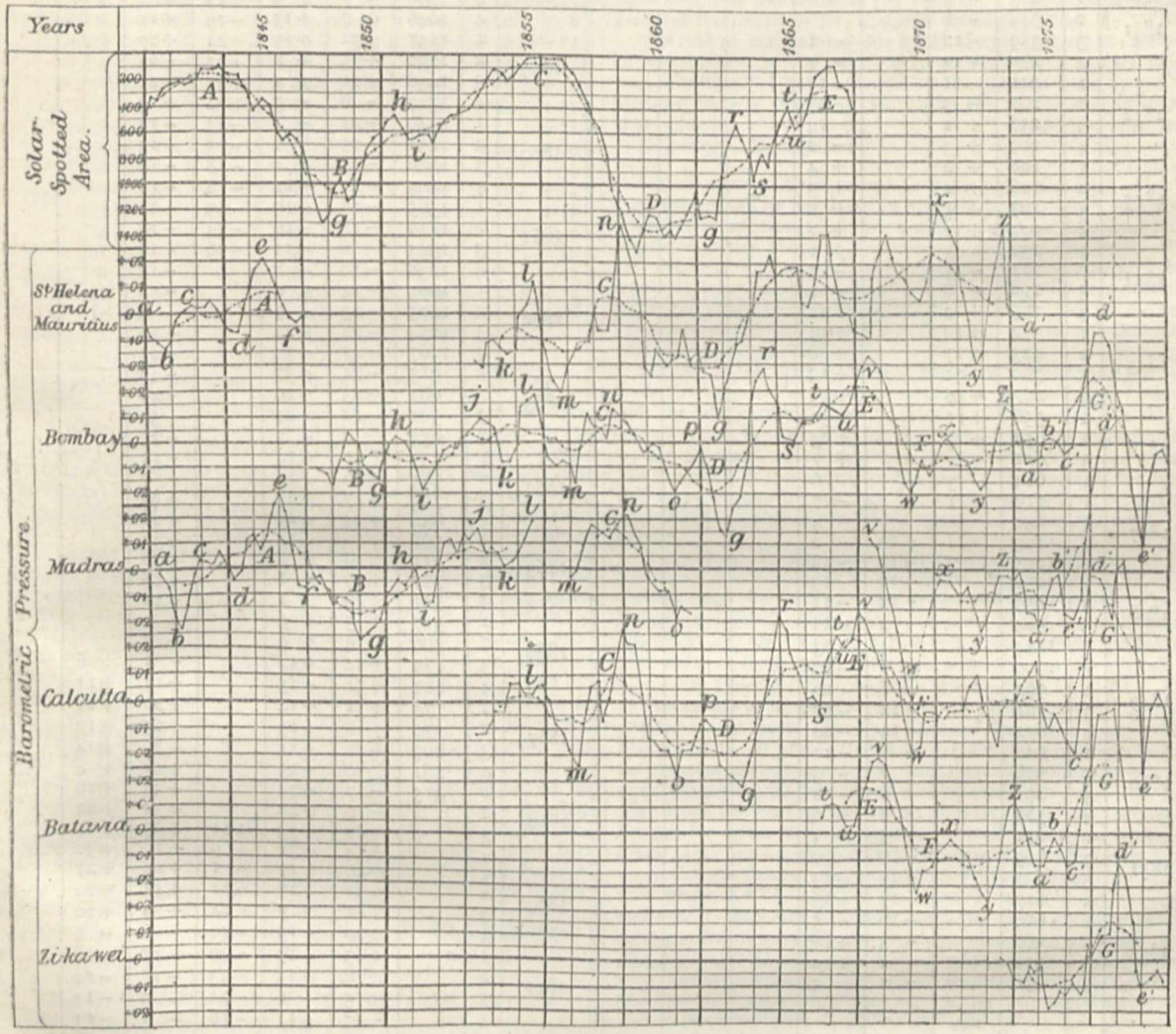
[1] During the first half of these periods the results for Singapore, Akyab, Chittagong, and Darjeeling differ so much from each other and from the remarkably accordant results obtained from the more widely separated stations of Bombay, Calcutta, Port Blair, and Batavia as to suggest that the former are of doubtful validity during the earlier years.

[2] These sketches are submitted annually to Government in August of the year following that to which they refer. See notices in NATURE, vol. xviii. pp. 199 and 619, vol. xxi. p. 384. The sketch for 1879, containing some further important conclusions with reference to the variations of rainfall and barometric pressure, has recently been submitted to Government.

of the Bombay Presidency, and that the proportionate increase or decrease of the abnormal rainfall, corresponding to a fall or rise in the abnormal pressure of a tenth of an inch of mercury, amounts to more than *one hundred per cent.* of the normall fall; but that the variations of the ordinary monsoon gradients produce very different effects on the rainfall of different districts, depending on the geographical peculiarities of the particular locality.

From all these facts it is clear that there is some intimate relation between the variations of sun-spots, barometric pressure, and rainfall; and as famines in general are induced by a deficiency of rain, it is probable that they also may be added to the above list of connected phenomena. What is required in order to gain an insight into the causal relation of these variations is that they should each and all be studied in greater detail than has hitherto been attempted. Accordingly I commenced, more than a year ago, a detailed investigation into the nature of the abnormal variations of barometric pressure, and have been led to the discovery of some new facts which appear to me to be of sufficient importance to render it desirable that they should be published in anticipation of the theoretical conclusions deducible therefrom.

Commencing with the daily abnormal barometric variations observed at several stations in Western India, it was soon found that as the time over which an abnormal barometric fluctuation extended became longer and longer, the range of the fluctuation became more and more uniform at the various stations, thus leading to the conclusion that the abnormal variations of long



duration affect a *very wide area*. To test this inference it became necessary to compare the observations recorded at Bombay with those of some distant tropical station. Batavia was chosen, and on curving the daily observations side by side with those of Bombay, the degree of accordance between them was found to be truly surprising, considering how far the two stations are apart. The next step was to compare the monthly abnormal variations of these two stations, and finding that they presented many similar features, as well as some differences, to smooth the variations by taking three-monthly means. The degree of accordance was now found to be still greater, many of the discordances having been eliminated in the process of smoothing; but as some differences were still observable the process was repeated, giving nine-monthly means of abnormal pressure corresponding to the middle of the months January, April, July, and October of each year. The curves obtained in this way for Bombay and Batavia were then found to be *almost identical in form*, but with this very remarkable difference: *the curve for Batavia was seen to lag very persistently about one month behind the Bombay curve.* Similar results were then worked out from all the available data for the following tropical stations: St. Helena, Mauritius, Madras, Calcutta, and Zi-ka-wei, and for comparison with them the monthly sun-spot areas[1] were treated in exactly the same manner. The results are given in the fol lowing table, and graphically represented by the continuous curves on the annexed plate:—

[1] Taken from the paper by Messrs. De La Rue, Stewart, and Loewy, published in the *Philosophical Transactions* for 1870, p. 122.

TABLE I.

Nine-Monthly Means of Solar Spotted Area and Abnormal Barometric Pressure

Year.		Solar spotted area in millionths of visible hemisphere.	Abnormal barometric pressure in thousandths of an inch. St. Helena.	Bombay.	Madras.	Calcutta.	Batavia.	Zi-ka-wie.
1841	1	473	- 5					
	2	354	- 8					
	3	359	-12		- 1			
	4	285	-14		- 7			
1842	1	241	-11		-18			
	2	207	- 3		-23			
	3	236	+ 1		-14			
	4	209	+ 3		- 2			
1843	1	188	+ 2		+ 4			
	2	116	+ 2		+ 3			
	3	107	+ 5		+ 2			
	4	102	+ 2		+ 8			
1844	1	130	- 5		+ 1			
	2	177	- 7		- 5			
	3	178	- 8		- 1			
	4	265	+ 2		+11			
1845	1	374	+10		+14			
	2	423	+17		+ 8			
	3	360	+21		+13			
	4	407	+16		+25			
1846	1	556	+11		+30			
	2	657	+ 3		+23			
	3	611	- 1		+ 7			
	4	664	- 3		- 6			
1847	1	719	- 1		- 7			
	2	841			- 4			
	3	1110		-10	- 2			
	4	1295		-12	-10			
1848	1	1251		-16	-13			
	2	1053		-12	-11			
	3	969		- 3	-11			
	4	1144		+ 4	-13			
1849	1	1096		0	-26			
	2	962		- 8	-24			
	3	761		-12	-22			
	4	672		-14	-20			
1850	1	618		- 6	-12			
	2	526		0	- 9			
	3	492		+ 2	- 4			
	4	588		0	- 6			
1851	1	671		- 5	- 3			
	2	669		-10	0			
	3	633		-19	-13			
	4	627		-15	-13			
1852	1	659		-10	- 5			
	2	566		- 3	+10			
	3	510		- 1	+12			
	4	456		- 2	+ 4			
			Mauritius.					
1853	1	474		0	+14			
	2	448		+ 3	+11			
	3	412	-19	+ 8	+16	-12		
	4	323	-21	+10	+ 6	-10		
1854	1	213	-12	+ 8	+ 7	- 7		
	2	123	-11	+ 6	+ 7	- 3		
	3	155	-14	- 7	+ 1	- 3		
	4	202	-16	- 7	+ 3	+ 7		
1855	1	175	-15	- 1	+ 6	+ 9		
	2	100	- 2	+13	+12	+ 4		
	3	17	+ 3	+17	+20	+ 3		
	4	16	+12	+19		+ 6		
1856	1	16	- 2	+ 6		+ 7		
	2	30	-15	- 5		+ 1		
	3	26	-27	- 9		- 9		
	4	45	-31	- 7		-11		

Year.		Solar spotted area in millionths of visible hemisphere.	Abnormal barometric pressure in thousandths of an inch. Mauritius.	Bombay.	Madras.	Calcutta.	Batavia.	Zi-ka-wie.
1857	1	86	-21	- 8	- 3	-19		
	2	130	-17	-16	- 1	-24		
	3	234	-12	- 1	+ 9	- 6		
	4	369	-11	+12	+18	+ 5		
1858	1	510	0	+ 7	+16	+ 8		
	2	581	- 7	+ 6	+15	- 7		
	3	798	- 7	+ 2	+13	+ 2		
	4	1046	+ 7	+13	-19	+11		
1859	1	1137	+32	+10	+21	+27		
	2	1222	+35	+ 8	+22	+22		
	3	1436	+20	+ 4	+15	+21		
	4	1519	- 6	- 3	+ 8	+ 3		
1860	1	1340	-21	- 1	- 3	-13		
	2	1231	-25	0	- 5	-15		
	3	1266	-14	- 2	- 8	-18		
	4	1405	-21	-11	- 8	-18		
1861	1	1365	-18	-19	-19	-28		
	2	1416	-18	-16	-14	-18		
	3	1343	- 6	-11	-16	-18		
	4	1238	-18	- 8		-13		
1862	1	1063	-14	- 2		- 8		
	2	1275	-23	- 9		- 6		
	3	1254	-24	-22		-10		
	4	1267	-41	-34		-23		
1863	1	913	-31	-37		-26		
	2	684	-23	-26		-28		
	3	508	-12	-21		-32		
	4	652	0	- 7		-23		
1864	1	757	0	+14		-14		
	2	985	+16	+24		- 7		
	3	783	+17	+29		+ 8		
	4	838	+24	+20		+20		
1865	1	598	+16	+14		+32		
	2	511	+18	+ 2		+28		
	3	377	+19	+ 1		+18		
	4	541	+19	+ 1		+11		
1866	1	517	+ 5	+ 9		+ 1		
	2	406	+ 4	+ 9		+ 2		
	3	174	+12	+12		- 1	+ 6	
	4	101	+31	+15		+14	+11	
1867	1	74	+31	+14		+20	+11	
	2	70	+18	+13		+25	+10	
	3	209	+ 1	+11		+20	+ 4	
	4	261	0	+15		+22	+ 4	
1868	1	415	- 7	+24		+33	+16	
	2		- 8	+29		+31	+22	
	3		-10	+33	+11	+25	+28	
	4		+14	+31	+ 9	+16	+30	
1869	1		+26	+27	- 1	+14	+27	
	2		+31	+19	-11	+14	+20	
	3		+22	+ 5	-24	+ 9	+10	
	4		+16	-15	-44	- 6	- 6	
1870	1		+13	-19	-49	-17	-16	
	2		+ 8	-15	-43	-16	-24	
	3		+ 5	- 6	-24	- 3	-14	
	4		+12	-11	-16	- 3	-13	
1871	1		+27	- 9	- 9	- 4	- 7	
	2		+42	- 1	- 4	- 5	- 5	
	3		+39	+ 2	- 4	- 2	- 2	
	4		+33	0	- 8	- 2	- 4	
1872	1		+27	- 6	-10	- 2	- 7	
	2		+ 9	- 7	- 6	+ 7	- 9	
	3		- 3	-12	-16	+11	-17	
	4		-20	-18	-23	- 1	-24	
1873	1		-15	-15	-19	- 4	-26	
	2		+ 7	- 8	-11	-17	-17	
	3		+18	+ 6	- 2	- 1	- 4	+ 2
	4		+34	+15	- 1	+ 2	+10	0
1874	1		+ 2	+12	- 3	+ 2	+12	- 6
	2		0	+ 5	- 1	+ 8	+ 5	- 8
	3		- 2	- 7	-15	+12	- 4	0
	4			- 6	-16	+16	-11	- 4

TABLE I. (*Continued*)—

Year.		Solar spotted area in millionths of visible hemisphere.	Abnormal barometric pressure in thousandths of an inch.					
			Mauritius.	Bombay.	Madras.	Calcutta.	Batavia.	Zi-ka-wie.
	1			- 5	-22	+ 2	-11	0
1875	2			+ 1	-11	-10	- 7	-18
	3			+ 2	- 4	- 3	- 1	-16
	4			+ 1	- 1	- 9	- 5	-12
	1			- 3	-16	-14	-11	-13
1876	2			- 1	-18	-19	-10	- 8
	3			+ 9	-13	- 3	+ 1	-12
	4			+21	+12	+27	+22	+ 8
	1			+29	+30	+49	+28	+ 4
1877	2			+43	+46	+48	+46	+11
	3			+43	+55	+43	+47	+12
	4			+38		+32	+49	+29
	1			+24		+51	+34	+39
1878	2			+13		+55	+17	+34
	3			-15		+33	- 1	+16
	4			-33		0		- 4
	1			-40		-26		-10
1879	2			-15		- 2		- 7
	3			- 4		+ 4		- 3
	4			- 1		- 2		- 8
1880	1			- 8		-14		

Comparison of Abnormal Barometric Movements at Different Stations.—The general resemblance of all these curves to each other is very remarkable; indeed if the Mauritius curve for the years 1867 and 1868 be excluded, there is scarcely a single prominent feature in any one of the curves which is not reproduced in the others. To show this the corresponding points of the different curves have been marked with the same small letters. It will be seen, however, that there is strong evidence of a want of exact simultaneity in the barometric movements at different stations, and that as a rule the changes take place at the more westerly stations *several months earlier* than at the more easterly ones. This is particularly noticeable in the curves for St. Helena and Madras from 1841 to 1846, when the latter sometimes lagged behind the former by as much as six months; in those for Mauritius and Calcutta from 1855 to 1866, when the latter persistently lagged several months behind the former; in those for Bombay and Calcutta from 1862 to 1866, when the difference in time often amounted to *upwards of six months;* in those for Bombay and Batavia from 1867 to 1878, when (as already remarked) the latter lagged behind the former at an average interval of about one month; and in those for Bombay and Ti-ka-wei from 1876 to 1878, when the latter lagged upwards of six months behind the former. *It appears then that these long atmospheric waves (if such they may be called) travel at a very slow and variable rate round the earth from west to east, like the cyclones of the extra tropical latitudes.*

Bombay FRED. CHAMBERS

(*To be continued.*)

DR. SIEMENS'S NEW CURE FOR SMOKE

FROM among a number of letters which have been sent us on this subject we have selected the following for publication; to these Dr. Siemens has been good enough to append some important remarks.

IN NATURE, vol. xxiii. p. 25, I read with interest an article by Dr. Siemens describing an ingenious gas and coke fire which he suggests as a cure for the smoke nuisance. But although the darkening of the atmosphere or fog will certainly be prevented by its use, I am afraid the *gases from the coke*, especially the carbonic oxide, will make the fogs at least as poisonous and injurious to health as the open coal fires at present in use.

In these circumstances a description of an "Asbestos gas fire" free from this objection, which we have had in use in our smoking room for the last three years, and which, after a few alterations, has proved perfectly satisfactory, may perhaps interest your readers.

A ¼-inch gas-pipe furnished with four Bunsen burners is laid on the hearthstone under the grate and parallel to the ribs, so arranged that the tops of the burners (which are made elliptical to pass through the bars) are flush with the upper surface of the grate, and two inches back from the line of the ribs. The fireplace is loosely filled with a preparation of asbestos in pieces about the size of a hen's egg.

This fire not only evolves a large amount of heat, but has a very cheerful appearance, similar to that of a bright coke fire, and to insure this it is essential that the burners should be placed close to the ribs, as stated above, and not in the centre of the grate. If this is not attended to the asbestos in the centre of the fire will be raised to a high temperature, but will not be sufficient to heat those portions in front, which will then not only be of no use as radiators in themselves, but act as screens to the light and heat generated in the centre. I suspect this was the cause of the failure of Dr. Siemens' pumice gas fire.

The cost of maintaining this fire is simply that of the amount of gas burned, as the asbestos is not consumed, and its prime cost is trifling. I have only further to add that there is not the slightest trace of fumes or smell from the fire two minutes after it is lighted. D. A. STEVENSON

Edinburgh, November 15

DR. SIEMENS has described in your pages the form of coke-gas grate which he has fitted in his own house. As I had fitted a similar arrangement in this house before Dr. Siemens' letter appeared in the *Times* of November 3, and as it is simpler than Dr. Siemens' and succeeds even beyond my expectation, I send you a drawing and description of it. It varies, of course, according to the shape of the grate in which it is fitted; but for the sake of comparison I have copied Dr. Siemens' grate, and drawn my arrangement as fitted into it.

Instead of Dr. Siemens' arrangement for withdrawing the heat from the back of the fire and bringing it to the front, I merely line the whole grate—sides, back, and bottom—with fire-bricks. This obviates the necessity for the close-fitting ash-pan described by Dr. Siemens, which would be rather expensive to fit. I make the fire-brick in the bottom of the grate slope towards the front, and leave a space of one inch between the front of it and the perforated gaspipe down which space the ashes fall on to the hearth.

If my grate is not quite so economical in working as Dr. Siemens', it is very near it, and the first cost of fitting is considerably less. In fact, as most grates are lined with fire-brick at back and sides, nothing has to be done but fit a wedge-shaped fire-brick into the bottom, a half-inch iron gaspipe, perforated with holes in front, and connect it with the gas service, all of which can generally be done for a few shillings.

The saving of kindling-wood and of chimney-sweeping would pay for it in a year. In Dr. Siemens' grate the copper must cost about 1*l*. A grate fitted with this arrangement looks exactly the same as an ordinary grate, and there is nothing to prevent ordinary coal being burnt in it—in fact coal can be burnt in it with much less smoke than in an ordinary grate by turning on the gas for a few minutes when fresh coal is put on, when the dense black smoke emitted by the new coal is completely burnt up in the gas-flame. To people who object that a gas grate must produce a bad smell in the room I can only say, "Come and see." They will find that we have three grates with this arrangement in constant use in these chambers, and that they produce no smell and make a very pleasant fire. Any person who takes an interest in the subject is quite welcome to come in and look at them at any time. COSMO INNES

Adelphi Chambers, 7, John Street, Adelphi

HAVING been experimenting for some years in the direction referred to by Dr. Siemens in NATURE, vol. xxiii. p. 25, I must beg to differ with him most seriously in some of his conclusions. The gas-fire with coke which he describes has, so far as our experience goes, several practical objections which prevent its use in the place of an ordinary gas fire, whilst when compared with a *good* coal fire it fails seriously.

First, with regard to the objections to Dr. Siemens' fire. It requires about half an hour to become anything like warm, as against ten to fifteen minutes with a well-lighted coal fire. Second, it makes as much or more dust and dirt than a *good* coal fire. Third, the grate requires as much cleaning and care as with coal.

I am not surprised at the economy, comparing the coal fire as shown with gas and coke, but if the result had been taken in

comparison with a good Abbotsford grate with solid clay bottom, back and sides, the figures would have appeared seriously the other way.

In a room of exactly half the cubic area of the one referred to by Dr. Siemens we have an Abbotsford grate a little over ⅓rd cubic foot capacity, the actual measurement of the fire space being 5½ inches deep, 8 inches back to front, 14 inches wide. This is lighted at 7 o'clock every morning and at 10 o'clock the grate is filled (not piled high). This fire burns until 10 or 11 o'clock every night untouched, practically smokeless, making the room pleasantly warm all over in the severest weather, and without making a *handful of cinders in a month*. One ordinary boxful of coals lasts two days. We have five, sometimes six, fires going daily at an average cost for coal for the winter season of five shillings weekly, or less than twopence per day per fire. That Dr. Siemens is correct so far as the old style of fire-grate is concerned, I know to my cost, but taking any good grate with clay sides and back and a solid clay bottom, his fire at its best will not compare either for cleanliness, economy, or comfort.

Gas fires are wanted where absolutely no attention and dust can be permitted. Allowing either of these as possible, no substitute I know will approach a well-constructed open fire with a solid clay bottom and fire-box.

With regard to the waste heat, it is no greater than absolutely necessary to take away the products of combustion, as, with our grates, it is utilised for warming the upper rooms. At this moment, with five good fires, there is visible from the tops of our chimneys nothing except a clear transparent current of warm air; any one at a cursory glance would say there were no fires in the house.

It must be borne in mind when I refer to cost that we cook entirely by gas, and the price of good coal here is 14*s.* 2*d.* per ton, coke being about half this price. What is required in a gas fire is a perfectly clean source of radiant heat, without trouble, and quickly available: these conditions are not in any way fulfilled by Dr. Siemens' arrangement. With the exception of two or three minutes expended in lighting, all he has attained can be found in a more perfect form in many of the fire-grates which have been in common use for the last ten years. Amongst our many attempts at gas fires one, although not absolutely the same as Dr. Siemens', was practically so, and was condemned because it required as much trouble as our present fires, and was much slower in lighting. It would be both interesting and instructive if Dr. Siemens would test an Abbotsford grate under the same conditions as his coke-gas fire, and supplement his report with one from the individual who has to do the cleaning up and dusting, a department which it is more than probable he ignores.

Another important matter is that I believe the cost of making and fixing Dr. Siemens' grate would be not less than that of a good modern fire-grate.

THOS. FLETCHER

Warrington

THROUGH your courtesy I am enabled to reply to the objections raised by three correspondents against my proposed gas-coke grate, before they have actually appeared in your columns.

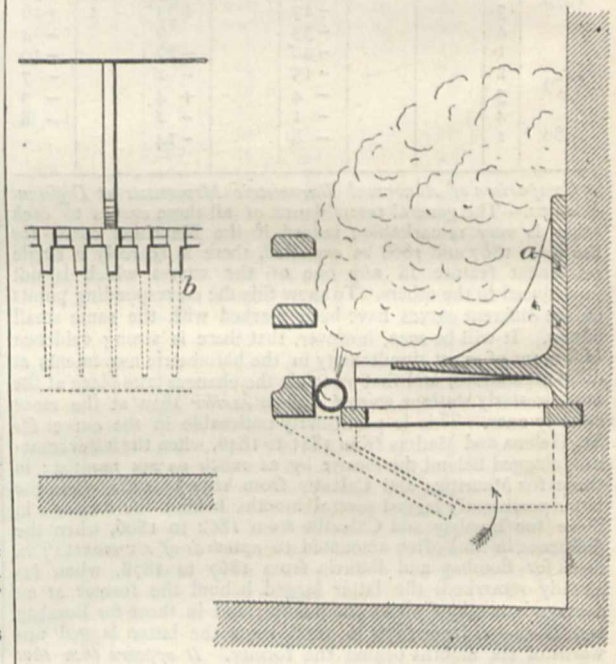
Mr. D. A. Stevenson considers that the use of coke is objectionable on account of the gases evolved in its combustion, and especially the carbonic oxide gas, which would poison the atmosphere. In reply I have to say that in burning coke with a supply of hot air, and in contact in front of the grate with the atmosphere, its entire combustion is insured, resulting in carbonic acid, which is a necessary constituent of our atmosphere. In obtaining the same amount of heat through the perfect combustion of gas, products of combustion at least equally objectionable from a sanitary point of view will be evolved.

The gas-asbestos grate which he describes appears to be judiciously contrived, but its power of heating the room depends entirely upon the combustion of gas unaided by hot air or solid fuel. Now 1000 cubic feet of gas weigh about 34 lbs., and the heat developed in the combustion cannot exceed $34 \times 22{,}000 = 748{,}000$ units of heat.

The heat units produced in burning a pound of coke may be taken at 13,400 (assuming it to contain about 8 per cent. of incombustible admixture, the heat equivalent of pure carbon being 14,500 units), and it requires $\frac{748{,}000}{13{,}400} = 56$ lbs., or just half a hundredweight of this coke, to produce the heating effect of 1000 cubic feet of gas.

Taking gas coke at 18*s.* per ton (which is an excessive price), the 56 lbs. of coke represent a cost of 5·4*d.*, as compared with 3*s.* 6*d.* for the 1000 cubic feet of gas producing the same amount of heat. This great difference of cost at once shows the advantage of making coke do as much of the work as possible. Without it a gas grate will consume 50 to 70 cubic feet of gas per hour, whereas my experiments prove that an average consumption of 8 cubic feet suffices to heat a large room when combined with a moderate consumption of coke, and with the use of the heating arrangement, to which I attach great importance. Another important consideration in favour of the joint use of coke and gas is that the existing gas companies produce both these constituents very much in the proportion in which they would be required, and could therefore provide the means of supplying an enormous number of coke-gas grates, whereas their plant and mains would be quite inadequate to supply a demand upon them for an extended application of purely gas stoves.

Mr. Cosmo Innes describes a gas grate of his construction, having the closed grate and single gas pipe behind the lower front bar which I advocate; he proposes to fill the grate with common coal, using the gas only as a means of kindling the fire. My objections to his proposal are that in using coal he must continue to make smoke, which we are desirous to prevent, and that the hot back to his fire means rapid distillation of the fuel up the chimney in the form of hydrocarbons and carbonic oxide. The gas arrangement as shown by him will be efficacious, no doubt, as a means of kindling a bright and cheerful fire, but he



would do better in that case to use a few logs of wood instead of coals. A bright but short-lived fire may thus be raised quickly at a cheap rate in a dining-room or in a parlour.

Mr. Thomas Fletcher admits that my grate has the advantage of economy over a common coal grate, but thinks the Abbotsford grate the best of all. This grate is according to him practically smokeless, and produces only a handful of cinders in a month, although common coal is used. Now I have no desire to detract from the merits of the Abbotsford grate, but I fail to see why it should be smokeless, considering that raw coal is used; and the extremely small production of ashes or cinder seems to imply that Mr. Fletcher uses an extremely pure and probably a smokeless coal, very different from the fuel we are usually supplied with in London.

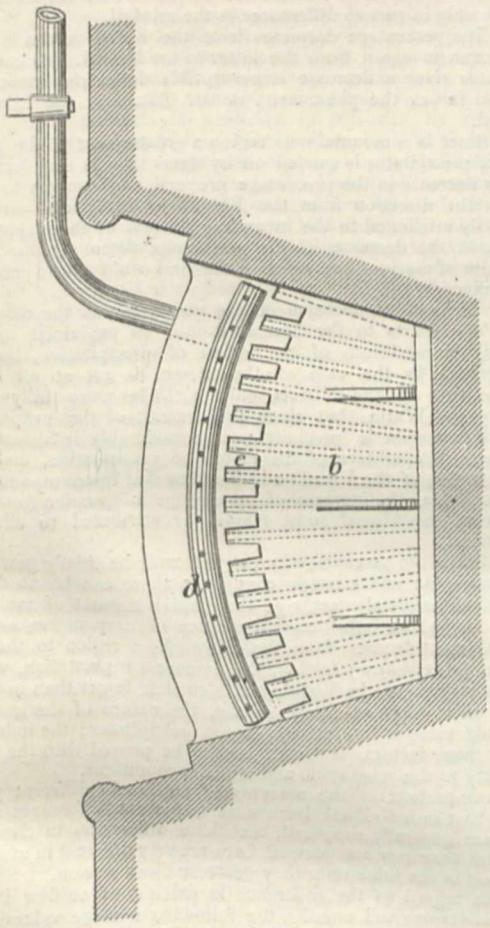
He also objects to the cost of my arrangement, and his opinion in this respect, coming from a practical grate-builder, is entitled to every consideration. In first describing my plan I did not go into the question of cost of application; but having been since asked by grate-builders to advise them regarding the cheapest form of my grate and the easiest mode of applying it to existing fire-places, I have devised a form of application which leaves little to be desired, I think, as regards first cost.

The arrangement is shown by the accompanying sketch, and consists of two parts which are simply added to the existing

grate, viz.:—(1) the gas-pipe (*d*) with holes of about $\frac{1}{16}$ inch diameter, 1·5 inch apart along the upper side inclining inward, and (2) an angular plate (*a*) of either cast or wrought iron, with projecting ribs (*b*) extending from front to back on its under-side, either cast with or riveted to the same, presenting a considerable area, and serving the double purpose of supporting the additional part on the existing grate, and of providing the heating-surface produced by the copper plate and frill-work in my first arrangement. In using iron instead of copper it is necessary however to increase the thickness of these plates and ribs in the inverse ratio of the conductivity of the two metals, or as regards the back plate, from $\frac{1}{4}$ inch to $\frac{5}{8}$ inch.

The arrangement will be rendered more perfect by the use of the bent plate fastened to the lower grate bar, which directs the incoming air upon the heating-surfaces.

The front edge of the horizontal plate has vandyked openings (*c*), so as to form a narrow grating, through which the small quan-



tity of ashes that will be produced by combustion of the coke and anthracite in the front part of the grate discharge themselves down the incline towards the back of the hearth, where an open ash-pan may be placed for their reception.

In adapting the arrangement to new grates, the horizontal grating had better be dispensed with, and the casting with its lower ribs extended downwards, so as to find its fixed support between the back of the fireplace and the inclined deflector plate.

Mr. Fletcher speaks of the large amount of ashes that would be produced, but this amount can surely not be as great as in the case of a coal fire, seeing that the consumption of solid fuel is reduced to less than one-half, of which nearly one-half is anthracite, a fuel remarkably free from ashes. Neither do I participate in Mr. Fletcher's fear regarding opposition on the part of housemaids, except it be from an apprehension on their part that, with Othello's and the chimney-sweeps', their "occupation be gone."

The tendency of grate-builders of the present day, and also of your correspondents, appears to be to look for economy to brick-linings, which no doubt have the effect of producing hot radiating surfaces. I maintain however that such radiation is obtained at too great a cost of fuel, and that superior economical results will, on the contrary, be attained by abstracting the heat from the back of the fire, and concentrating it upon the purely carbonaceous material in front of the same.

To illustrate my reasoning I may here refer to an experiment which can easily be made of throwing a shovelful of bituminous coal into a steel-melting furnace; the result is an instantaneous dispersion of the coal, accompanied with a powerful refrigerative action on the furnace. In constructing gas-producers I take advantage of hot walls to turn solid into gaseous fuel, and a fireplace with hot brick bottom and sides is very much in the condition of a good gas-producer, giving out radiant heat no doubt, but combined with rapid distillation of combustible gases into the chimney. This action is made apparent in placing on the fuel towards the back of such a grate when in full glow a piece of wood, which will be seen to dwindle away rapidly without giving rise to flame, the atmosphere immediately over the glowing fuel being essentially a reducing one.

In my grate the heat, on the contrary, is confined to the coke immediately behind the bars, in contact with the heating gas flames and with the air of the room flowing in towards the chimney, whereas the coke at the back of the grate remains comparatively cool and unconsumed throughout the day. The cold furnace-back also means a cold chimney, and it is rather remarkable to observe that in the case of the application at my office, a thermometer held high up into the chimney showed a temperature of only 130° F., while the front of the grate was in a high state of incandescence. These, I maintain, are conditions most favourable to economy combined with entire absence of smoke or deleterious gases. C. WILLIAM SIEMENS

12, Queen Anne's Gate, S.W., November 24

CURIOUS IMPRESSIONS IN CAMBRIAN SANDSTONE NEAR LOCH MAREE

IN course of the short excursion to Loch Maree and its neighbourhood, Mr. Walter Carruthers, of the *Inverness Courier*, happened, on June 13, to light upon an interesting portion of the Cambrian or Torridon Red Sandstone of the district, forming part of the bed of the burn, near Loch Maree Hotel, on which occur what have been called the Victoria Falls, so named from the fact that the Queen visited them. There an exposed surface of the rock about sixteen feet in length, nearly as much in breadth, and almost perfectly level, is marked by several double grooves quite discernible, and each divided by a very thin raised line. These traverse the whole length of the rock in a perfectly straight line, and on both sides of them are roughnesses which, if we could entertain the idea that the grooving had been caused by some living creatures, might be produced by footprints which have been to a great extent obliterated. The impressions were so striking that they immediately suggested a recollection of the footprints discovered in the sandstones of Morayshire and Tarbatness, though there was no other resemblance than their marked character on the broad, flat rock. Having heard that Mr. William Jolly, H.M. Inspector of Schools, was in the neighbourhood, Mr. Carruthers called his attention to the subject, and indicated where he should find the markings. Mr. Jolly was not slow to examine the spot, and he writes to Mr. Carruthers as follows, as given in the *Inverness Courier* of July 1:—

"I found your curious lines without difficulty, guided by your accurate description of their locality. They are assuredly no 'mare's nest,' but *bonâ fide* ancient impressions of some kind, which should receive the attention of geologists, both on their own account and as existing in the second oldest geological formation in Britain, in which, as yet in Scotland, no evidences whatever of organic life have been discovered.

"The lines or bands in question occur in the chocolate-coloured Torridon sandstone, the Cambrian of Murchison and Geikie, which is so well developed around Loch Maree, and rises into the great dome of the Slioch, or the Spear Head, that guards its waters. The most distinct of the impressions consists of two continuous flat bands side by side, $1\frac{1}{4}$ to $1\frac{1}{2}$ inch broad, and about a quarter of an inch deep, running quite straight across the flat layers of sandstone *in situ*, and perfectly distinct for sixteen feet, disappearing on the west side under the superincumbent rock, and broken only where portions of the sandstone have

been weathered out. In some places a third line runs alongside the two, but this is much less distinct and persistent. The double band resembles nothing more nearly than the hollow impression that would be left by double bars of iron placed closely together and neatly inserted in the rock for clasping some structure on it, if the iron were subsequently removed; or, as you suggest, the marks of a gouge driven by a carpenter across a board. The bands, when looked narrowly into, consist of very fine close hair-like lines, continuous and parallel to their sides, resembling very minute striæ left by glaciation, and look as if caused by some object drawn along the original red sand, before it became the present indurated rock.

"A similar double line runs parallel to this one, about two feet lower down, seven feet long, and a third parallel double line on the other or upper side, three feet long, both of the same breadth as the first. Besides those pointed out by you, which occur on the same flat of sandstone, other lines exist farther down, on the other side of the pool below this rocky flat, on a similar bed of sandstone, part of the same layer—one three feet in length, another six feet, running more or less parallel to those above. Indications of others may also be seen, and, no doubt, several more may be discovered on more careful examination.

"What they are I can scarcely even surmise, having seen nothing of the *same* kind elsewhere. They do suggest the possibility of their being the indentations of the caudal appendage of some huge creature, similar to the hollow tail-lines between the footprints on the sandstone at Tarbatness and along the shores of Morayshire—a suggestion strengthened by the fact of the existence, on both sides of the line, of numerous rounded hollow marks, very like the footprints on these reptiliferous rocks, occurring, as in them, at intervals. But the continuous even breadth and square section of the bands would seem to render this impossible. Then they might be the depressions left on the soft sand by the hinder portions of the shell of some large crustacean—a more likely cause, rendered more probable by the existence of very good ripple-marks on the same sandstone, in the same and neighbouring layers. The striæ-like lines of which the grooves consist would seem to point to some moving agent, organic or physical. They may, however, be the casts or impressions of some great land reed or sea fucoid, the hair-lines being the marks of the fine structure of its stem or the parallel veins of its leaves. It would be desirable to have the superincumbent layer of rock carefully removed where the bands in question disappear under the upper rock, which might shed some light on the nature of the strange marks. I was sorry I could not spend more time on their examination."

The impressions occur about 300 or 400 yards above the Victoria Falls, and immediately beside the last of three lesser waterfalls on the west side of the stream.

THE QUANTITIES OF WATER IN GERMAN RIVERS

AN attempt has recently been made by Herr Graeve (*Der Civil-Ingenieur*, 1879, p. 591) to determine the amount of water in German rivers and its apportionment in different seasons, a question very important for navigation, and also of much scientific interest. His research comprehends the chief rivers of Germany, excluding the Danube, which begins to be navigable only outside of Germany, and including the Vistula and the Memel. He first calculated, from the mean heights of water, the quantities of water flowing out per second, and he adds a table in which the amount of outflow is shown in relation to the extent of the corresponding river territory. When the amount of outflow per 100 sq. km. of the region of precipitation is calculated the following values are obtained:—(1) the Rhine at Coblentz above the Moselle mouth delivers per 100 sq. km. of land 1·070 cub. m. of water in a second; (2) the Weser at Minden, 0·826 cub. m.; (3) the Elbe at Sorgau, 0·579; (4) the Elbe at Barby, 0·554; (5) the Oder at Steinau, 0·460; (6) the Oder below the Warta mouth, 0·413; (7) the Warta near its mouth, 0·344; (8) the Vistula at Montau Spitz, 0·538; (9) the Memel at Tilsit, 0·600.

From these numbers it appears (*a*) that the average outflow of different rivers, from equal portions of their territory, differs much more than is usually thought, for in the Middle Rhine it is about three times, in the Middle Weser two and a half times, and in the Middle Elbe, as also in the Lower Vistula and Memel, more than one and a half times as much as in the Lower Warta. On the whole, it decreases from the Rhine to the Warta, and from the latter increases again to the Memel. (*b*) In one and the same river the quantity from equal portions of land seems as a rule to decrease down stream. (*c*) All calculations of quantity of outflow in streams, based merely on extent of the region of precipitation, must as a rule give incorrect results.

It was important to try and determine the relations of the quantity of outflow to the rainfall of the corresponding regions, and Herr Graeve, doing so by a method which he describes, obtained the following percentage numbers, corresponding to the above series of rivers:—(1) = 38·5 per cent.; (2) = 37 p.c.; (3) = 30 p.c.; (4) = 28·5 p.c.; (5) = 27·2 p.c.; (6) = 21·4 p.c.; (7) = 21 p.c.; (8) = 29 p.c.; (9) = 32·5 p.c.

From this the following conclusions (briefly) are drawn:—

(*a*) The percentage proportion of the amount of outflow to the rainfall differs very considerably in these several rivers, though far less than the amount of outflow from equally large regions of these rivers; hence the differences of the latter can be due only in part to differences in the rainfall.

(*b*) The percentage decreases from the Rhine to the Warta, and increases again from the latter to the Memel. In one and the same river a decrease is perceptible down the stream, at least so far as the phenomena in the Oder and the Elbe are general.

(*c*) Since in a mountainous region a greater part of the atmospheric precipitates is carried off by rivers than in the plain, the steady decrease in the percentage proportion of outflow to rainfall in the direction from the Rhine to the Warta must be primarily attributed to the increasing flatness of the region; so too must the decrease of the percentage down stream. The influence of more or less wood on the land could not be precisely determined.

(*d*) The marked increase of the percentage in the direction from the Warta to the Memel cannot be explained by the orographic conditions of the region of precipitation, because this region in the case of the Memel is not at all hilly, and in that of the Vistula only a little more hilly than that of the Warta, but since the amount of the evaporated part of atmospheric precipitates is considerably influenced by the mean temperature of the region of precipitation, and this in the region of the Vistula and the Memel is lower than in that of the Warta, the increase of percentage in question from the Warta to the Memel must mainly be attributed to climatic conditions.

(*e*) While the percentage in question must be chiefly governed by orographic and climatic conditions, there can be no doubt that other factors also act, *e.g.*, the relative amount of moisture in the air, which influences the degree of evaporation, and in general must decrease from the rainy Rhine region to the dry region of the Warta; further, the amount of plantation, which in the regions of the Vistula and Memel is larger than in those of all other German rivers; lastly, the nature of the ground, allowing more or less passage to the precipitates; the influence of all these factors, however, cannot be proved with the same certainty as the orographic and climatic conditions.

A comparison of the amounts of outflow in different years shows that in individual rivers more important differences occur than are generally supposed, that these differences in rivers of different character and unequal force are very different in amount, and that in the same river they decrease down stream.

With regard to the difference in amount of outflow in the various seasons and months, the following average values were obtained. The amount of outflow in winter (from the beginning of November to the end of April) is to that of summer, at the parts of the stream examined, in the Rhine as 1 : 0·922, in the Weser as 1 : 0·434, in the Elbe as 1 : 0·467, in the Oder as 1 : 0·525, and further down stream as 1 : 0·522, in the Vistula as 1 : 0·486, and in the Memel as 1 : 0·389. A better idea of the regularity of the quantities of outflow is given by the relations of these for the dryest and the wettest month of the year; in the case of the Rhine this ratio is 1 : 1·458, in the Weser 1 : 4, in the Elbe 1 : 5·238, in the Oder 1 : 4·5, and further down 1 : 3·68; in the Vistula 1 : 4·19, and in the Memel 1 : 4·51.

The causes of the difference in the ratio of the largest and least monthly amounts of outflow must chiefly be sought in the presence or absence of collecting basins, as also in the orographic and climatic conditions. In the Rhine all those factors combine which affect the regularity of outflow. It possesses in the Swiss lakes large reservoirs; its river-region comprises mountains of various height, and plains, so that the

melting of the snow must occur at very different times of the year. The Memel also possesses reservoirs in its marshes, and its region is perhaps better wooded than that of the other streams of Germany, but the long and hard winters cause an accumulation of large masses of ice and snow which melt suddenly and almost simultaneously in the whole region.

Herr Graeve takes up various other points, which have a practical bearing on navigation, but for these we must refer the reader to his memoir. He remarks in concluding on the desirability of comparing the conditions of outflow of German rivers with corresponding data for other European rivers, though at present the scanty and incomplete character of the data at hand render such inquiry scarcely practicable.

SCIENTIFIC SERIALS

THE *Journal of the Russian Physical and Chemical Society*, vol. xii. fascicules 5 and 6, contain, besides the minutes of meetings of the Society, the following papers:—In fascicule 5: On the dosage of chromium, by M. Th. Willm.—On the composition of the hydrate of peroxide of barium, by M. E. Schöne.—On the distribution of naphtha on the peninsula of Apsheron, by M. S. Goulichambaroff.—On the oxidation of ketones, by M. Goldstein.—On the products of oxidation of erythrite, by M. S. Przibytek.—A necrology of Prof. Nicolas Zinin, by MM. Borodin and Boutleroff.—On the magnetisation of liquids, by M. Ziloff.—On hail, by M. Schwedoff.—Notes by M. Latchinoff on specific heat, on a new dynamometer, and on electrical light.—In fascicule 6: On chlorocamphoric oxide, by M. Latchinoff.—On the action of heat on phosphorites, by M. Beletzky.—On tetrolic acid, by M. Lagermark.—On the solidification and evaporation of drops of liquid, by M. Sloughinoff.—On the dosage of mercury and arsenic in corpses; and an analysis of the artesian wells of Staraya Rousia.

Revue internationale des Sciences biologiques, July, 1880.—J. L. de Lanessan, on the protozoa (a chapter with illustrations from the author's forthcoming "Manuel d'Histoire Naturelle médicale).—A. Hovelacque, on the inferior races of mankind.—M. Debierre, man before and on the threshold of history.—Proceedings of the Academies of Paris, Belgium, and Amsterdam.

August.—J. L. de Lanessan, the coloration and the colouring-matters in plants.—M. Moniez, on the cysticers of Tænia.—M. Debierre, man before and on the threshold of history.—Proceedings of the Academies of Paris, Belgium, and Amsterdam.

September.—M. Vulpian, physiological study of poisons: curare.—M. J. L. de Lanessan, the saccharomycetes and the fermentations caused by them.—Prof. W. H. Flower, on the comparative anatomy of man (translated from NATURE).—M. R. Moniez, on cestoid worms and helminthologists.—Proceedings of the Academy of Paris.

SOCIETIES AND ACADEMIES

LONDON

Chemical Society, November 18.—Prof. H. E. Roscoe, president, in the chair.—It was announced that a ballot for the election of Fellows would take place at the next meeting (December 2).—The following papers were read:—Notes on the oxides of manganese, by Spencer Pickering. Various samples of oxides were procured and heated to various temperatures, until their weight was constant; in some cases they lost weight, in others they gained, whilst in some the weight remained constant.—On aluminium alcohols, by J. H. Gladstone and A. Tribe. When aluminium foil and iodine are heated with alcohol the latter is decomposed, two new organic aluminic compounds being formed, aluminic iodoethylate $(C_2H_5O)_3I_3Al_2$, and aluminic ethylate $Al_2(C_2H_5O)_6$. The authors have applied this reaction to other alcohols, and have thus prepared aluminic methylate, ethylate, propylate (isopropylate could not be obtained), isobutylate, amylate, cetylate, phenylate, cresylate, and thymolate.—Mr. W. H. Perkin then gave an account of the artificial production of indigo by A. Baeyer, and prepared some before the Society. The steps in the process are: toluene C_7H_6O, dichloride of benzyl $C_6H_5CHCl_2$, cinnamic acid $C_9H_8O_2$, orthonitrocinnamic acid $C_9H_7(NO_2)O_2$, orthonitrodibromhydrocinnamic acid $C_9H_7Br_2O_2(NO_2)$; by the action of caustic potash orthonitrophenylpropiolic acid $C_9H_5(No_2)O_2$ is formed, which on reduction in alkaline solution with grape sugar furnishes indigo $C_{16}H_{10}N_2O_2$.—On the synthetical production of new acids of the pyruvic series, by E. Moritz.—On the old alum well at Harrogate, by R. H. Davis. The author gives an analysis of the mineral constituents in the residue.—On the absorption spectrum of ozone, by W. N. Hartley.—On the probable absorption of the solar rays by atmospheric ozone, by W. N. Hartley. The author has photographed and measured the absorption spectrum of ozone; he suggests that the shortening of the solar spectrum at the violet end is due to the presence of ozone in the atmosphere, also that the blue colour of the sky may be ascribed to the same cause.—On peppermint camphor, by M. Moriya of Tokiô. The author has studied carefully the physical characters of this substance; he has also investigated the action of chromic acid, nitric acid, and bromine thereon.

Zoological Society, November 16.—Prof. Huxley, F.R.S., vice-president, in the chair.—Mr. W. K. Parker, F.R.S., read a paper on the development of the skull in the Urodele Batrachians. Mr. Parker described the skull of the adult Gigantic Salamander (*Sieboldia maxima*), the Siren and the Menopoma, and compared their structure with that of the various stages of the skull of the common newt.—Mr. G. E. Dobson, C.M.Z.S., exhibited and made remarks on the head of a partridge (*Perdix cinerea*) with an extraordinary prolongation of the intermaxillary bones.—Mr. W. A. Forbes, F.Z.S., made some remarks on the shedding of the horns of the Prong-buck (*Antilocapra americana*), as recently observed in the specimen living in the Society's Gardens.—Mr. Harting, F.Z.S., exhibited a specimen of Bartram's Sandpiper, recently killed in Lincolnshire.—Mr. Sclater exhibited the skin of the Guinea Fowl, lately described in the Society's *Proceedings* as *Numida Ellioti*. Further investigation had induced him to believe that this bird was the same as *Numida pucherani* of Hartlaub, the inaccurate colouring of the head in Mr. Elliot's figure of that species having prevented its identification.—Mr. G. A. Boulenger read a paper on the Palæarctic and Æthiopian species of *Bufo*, of which he recognised ten species: four in the Palæarctic, five in the Æthiopian region, and one found in both regions.—A communication was read from Dr. Otto Finsch, C.M.Z.S., in which he gave a list of the birds of the Island of Ruk, in the Central Carolines.—A second communication from Dr. Finsch contained the descriptions of some new or little-known species of pigeons from the Caroline Islands.—A communication was read from Mr. Edgar A. Smith, containing an account of the shells of the genus *Myodora* of Gray.—A communication was read from Mr. Martin Jacoby, in which he gave the descriptions of a collection of Phytophagous Coleoptera made by Mr. Buckley at Eastern Ecuador. The collection contained a good many new and interesting species, of which a great part were not alone inhabitants of Ecuador, but had been found either in Peru or the Amazonian region.—A paper by Messrs. F. D. Godman and O. Salvin was read, in which they gave the descriptions of some supposed new species of butterflies collected by Mr. Andrew Goldie in the interior of the district of Port Moresby, New Guinea.

Physical Society, November 13.—Prof. W. G. Adams in the chair.—Mr. Bosanquet, of St. John's College Physical Laboratory, Cambridge, read a paper on the nature of the sounds which occur in the beats of consonance. From mistimed octaves and twelfths he found that when the beats of the harmonics are cleared away each beat consists entirely of variations in the intensity of the lower notes. He gave the mathematical theory of these beats, and likewise of the curves given by the harmonograph. He also described an ear-tube for using in connection with a resonator. It is difficult to get definite results with a resonator unless the passage from the latter to the ear is closed to sound. The ear tube consists of a copper pipe bent into a sickle shape to gird the face, so that the ends may enter the ears, into which they are screwed, plugging them close. The sound is led from the resonator to the middle of the bent pipe by a flexible india-rubber tube, and thence to the ears.—Mr. Brown read a paper on action at a distance. He drew attention to the fact that though Newton disbelieved in action at a distance, he did not pronounce whether the medium was material or immaterial. Mr. Brown showed that the hypothesis of a material medium was encumbered with difficulties, since, among other reasons, direct contact could not explain gravity, projection of small particles from one body to another could not explain attraction, and Lesage's theory of corpuscles (as modified by Mr. Tolver Preston) required an enormous degree of porosity in masses of matter. The nature of magnetism and vibrations was also discussed by the author.—Mr. J. Macfarlane Gray read a paper on the mechanical nature of the forces called attraction, and gave grounds for attributing them to the pressures of a universal material ether of a gaseous nature. The paper was long, and had to be in part left unread. The hypothesis held by Mr. Gray

is remarkably confirmed by numerical results obtained by him.—Prof. Cottrell threw some doubts on Mr. Gray's results on the score that numerical coincidences were not always safe ground for basing theoretical deductions on. Mr. Gray stated that in the parts of the paper which had to be skipped Prof. Cottrell's objections were answered. He also pointed out that Mr. Brown in his criticism of the gasiform ether had not taken into account the important condition that the particles of ether have volume.—Professors Perry and Ayrton read a note on the contact-theory of Herr Exner recently brought before the Academy of Sciences of Vienna. They showed that Exner's experimental results disagreed with the concordant results of several independent experimenters, namely, Kohlrausch, Hankner, and Ayrton and Perry. They concluded that Herr Exner's experiments were inaccurate. They further argued that Exner's second and later paper, so far from being a disproof of the contact theory of electromotive force as now received, is in reality a proof of it. Dr. Wright stated that he will read a paper on this subject soon; and Prof. Reinhold said that Herr Exner had since corrected some of the results of his early papers on contact electricity.—Prof. Minchin of Cooper's Hill Engineering College exhibited a new photo-electric cell. This consists of a vessel of water containing a little acid, carbonate of calcium, and two tinfoil plates. When a beam of lime light was allowed to fall on one of the plates, a powerful current was set up in the cell, as seen by the deflection of a galvanometer connected in circuit with the plates. When a red glass screen intercepted the beam, the effect was very slight. Prof. Minchin had begun his experiments with fluorescence, but found "hard" water containing this salt of lime do equally well. The cell possesses this advantage: that the current it gives soon decreases in the light. When first the light falls on it, the exposed plate is positive, but it soon changes to negative. Prof. Minchin had tried the cell in place of a selenium one in the photophone, but with unsatisfactory results.

Anthropological Institute, November 9.—Edward B. Tylor, D.C.L., F.R.S., president, in the chair.—A paper was read on anthropological colour phenomena in Belgium and elsewhere, by J. Beddoe, M.D., F.R.S. Within the last few years the numerical method had been extensively applied to the determination of ethnological colour-types, the Anthropometric Committee of the British Association having set the example. The Continental nations were, however, now far ahead of us. In Germany Prof. Virchow had procured the tabulation as to the colour of the eyes and hair of all the school population, with the exception of Hamburg. In Switzerland Dr. Guillaume, of Neuchâtel, had obtained school statistics. For Belgium an elaborate monograph had been written on the subject by Prof. Vander Kindere, who, by the aid of the National Geographical Society, had induced the Minister of Public Instruction to include questions on the colour of the children's eyes and hair in the educational census. The results obtained have been of considerable importance, and bring out a remarkable contrast between the Flemish and Walloon provinces of Belgium.—Mr. J. F. Rowbotham read a paper on different stages in the development of the art of music in prehistoric times. Musical instruments, though their varieties may be counted by hundreds, are yet readily reducible under three distinct types: 1. The drum type. 2. The pipe type. 3. The lyre type. And these three types are representative of three distinct stages of development through which prehistoric music has passed. Moreover, the stages occur in the order named. That is to say, the first stage in the development of instrumental music was the drum stage, in which drums, and drums alone, were used by man. The second stage was the pipe stage, in which pipes as well as drums were used. The third stage was the lyre stage, in which stringed instruments were added to the stock. The three stages answer respectively to rhythm, melody, and harmony. And as in the geological history of the globe the chalk is never found below the oolite, nor the oolite below the coal, so in the musical history of mankind is the lyre stage never found to precede the pipe stage, nor the pipe stage to precede the drum stage.—A paper was read on neolithic implements in Russia, by Prince Paul Poutiatine. From the evidence of certain finds on his estate the author came to the conclusion: 1. That the Sclave-Scythians existed there in the stone period. 2. That they possessed instruments resembling those of the Celt-Scythians, and burned their dead. 3. That the old iron period of that neighbourhood was a continuation of the stone period. 4. That they supported themselves partly by hunting. 5. That they understood corn-growing.

Meteorological Society, November 17.—Mr. G. J. Symons, F.R.S., president, in the chair.—The following gentlemen were elected Fellows: G. Corden, E. T. Dowson, F. Hepburn, B.A., C. M. Hepworth, J. Mulvany, M.D., R.N., F. H. G. Newton, Capt. M. Parry, E. P. Phillips, and H. L. Roth.—The papers read were: Table of relative humidity, by Edward E. Dymond, F.M.S.—Rainfall in South Africa, by John G. Gamble, M.A., M. Inst. C.E., F.M.S. The author gives the monthly totals of rainfall from 103 stations for the thirteen months, December 1878 to December 1879, and also the monthly means from all stations in South Africa from which a record of five years or upwards could be obtained. It is shown that the Cape Peninsula, the South-West and the West Coast, have winter rains with a dry summer, characteristics of what is called the sub-tropical region, the rains coming with the north-west wind or anti-trade; while Natal, Aliwal north, and in a less degree Queenstown, have the tropical features of a wet summer and dry winter. On the South Coast the rainfall appears to be more equally distributed throughout the year, though there seems to be an October maximum at Port Elizabeth and Uitenhage. In the Central and Northern Karroo the maximum of the very scanty rainfall occurs in February and March. These rains generally fall in thunderstorms; each storm seems to come from a westerly direction, but it is a more or less well-ascertained fact that these rains do not fall up country until the south-easters have set in on the South and South-West Coasts. In the south-east of the colony the transition towards tropical features may be noticed, both Grahamstown and King Williamstown showing a winter minimum in June.—On the meteorology of Mackay, Queensland, by Henry L. Roth.—Thermometrical observations on board ship, by Capt. W. F. Caborne, F.M.S.

VIENNA

Imperial Academy of Sciences, November 18.—Contributions to general nerve and muscle physiology, by Dr. Biedermann.—On rhythmic contractions of striped muscles, produced by chemical stimulation, by the same.—On some platino-cyanide compounds, by Herr Scholz.—On resorcin colouring matters, by Drs. Wesselskyund and Benedikt.—On the formation of carboxylnatron acid from Brenz, catechin, and the constitutional formula of benzol, by Prof. v. Barth.—Note on mononitro-pyrogallol, by the same.—The distribution of rainfall over Austria in the period August 11-15, 1880, and its relation to distribution of air-pressure, by Herr Hann.

CONTENTS