THURSDAY, FEBRUARY 10, 1881

ALPINE FLOWERS

Alpenblumen ihre Befruchtung durch Insekten und ihre Anpassungen au dieselben. Von Dr. Herman Müller, Oberlehrer an der Realschule I. Ordnung zu Lippstadt. 8vo, 611 pp. (Leipzig: W. Engelmann, 1881.)

THE naturalist who studies animals or plants in a state of nature must often wish that he could test his conclusions experimentally by varying the conditions under which a given set of facts are observed. He wishes that he could change the food of a group of organisms, or the climate to which they were exposed, or that he could diminish the numbers of one sub-group while those of another were increased. As he cannot do this he is obliged to be content with treating the facts within his grasp in the spirit of an experimentalist, by comparing large classes of facts as they occur under different conditions.

The present volume of Dr. Müller's is something more than a descriptive study of the means of fertilisation found among alpine plants, for it is an admirable example of the kind of comparative investigation to which we have alluded, and as such is an extremely valuable contribution to the general science of plant-fertilisation.

It is a difficulty inherent in such inquiries that the observer, not having had a hand in varying the conditions, has to discover exactly in what way the environments differ in his two stations of observation. The most important feature in the environment of a plant considered in relation to fertilisation is the manner in which it is visited by insects. Thus an extensive knowledge of the alpine and lowland insect fauna is a necessary part of Dr. Hermann Müller's inquiry. Nor is it enough to study as an entomologist the relative frequency of bees, flies, butterflies, &c., in the mountains and in the plains; but the observer must discover by long and patient observation the different manner in which these insects visit the flowers in the two regions. The amount of this kind of labour condensed into the volume may be guessed at by looking at the long lists of insect-visits appended to each plant described, or arranged in the statistical tables in the latter part of the book.

The collection of this mass of detail must have tried Dr. Müller's almost unbounded energy and patience to the utmost. Several weeks in each of the last six summers have been devoted to the research; and the record of a single day's work (which we are glad to find was a somewhat exceptional one) will show how well the time at his command has been utilised. On this day, which was spent in the Heuthal on the Bernina, he was surrounded by flowers and insects from 8 a.m. to 4 p.m., during which interval he made notes on the visits of 237 insects, 225 of which were numbered and brought home. Dr. Müller adds that he was continuously spurred to the utmost of his powers by the consciousness that numbers of insects were making unobserved visits behind his back.

Nor has his energy and ingenuity in classifying and tabulating his results been less remarkable, as will be seen by an examination of the twelve tables which are interspersed in the latter part of the book; in these tables the visits of the various orders of insects to various kinds of flowers in different localities are numerically compared.

It is not a little remarkable that the visits of insects, which to the ordinary observer appear so casual and lawless, should be capable of such strict and statistical treatment. But it should be remembered that treatment of this kind is only possible with the large mass of facts which Dr. Müller has collected.

The most striking facts in the book are those connected with the predominance of butterflies in the alpine regions. The changes which occur in the insect fauna as we ascend are briefly that the relative number of Lepidoptera and Diptera (especially the short-trunked flies) increase, while the Hymenoptera and Coleoptera, as well as the other unimportant kinds, diminish in relative number. Thus if we compare the number of different visits made to flowers in the lowlands and the alps,[1] we find the following proportions:—

	Lowlands.	Alps.
Lepidoptera	100	614
Diptera	100	109
Hymenoptera	100	35·5

the number of visits made by each order of insects in the lowlands being taken as 100.

One marked result is that classes of flowers chiefly visited by bees in the lower regions are in the alps much frequented by Lepidoptera. Thus, of a hundred different visits made to Papilionaceæ in the lowlands 73 per cent. are those of Apidæ, 17 per cent. are made by Lepidoptera, so that in the plains they are markedly "bee-flowers." But in the alps only 40 per cent. of the visits made to Papilionaceæ are those of bees, and 56 per cent. are those of Lepidoptera. The same fact was observed in the Labiates and in a number of Composite flowers. The adaptations which alpine flowers exhibit in relation to this preponderance of Lepidoptera form some of the most interesting parts of the book; with some of the facts the readers of NATURE are already familiar, through Dr. Müller's admirable articles on the subject which have appeared in these pages. The principle which underlies these alpine modifications may be illustrated by two sections of the genus Gentian. In the first of these (Cœlanthe) fertilisation is effected by humble-bees creeping inside the corolla. This necessitates so wide a tube that Lepidoptera can steal the nectar without effecting fertilisation. The second section, Cyclostigma, is characteristic of the alpine regions, and the flower has been adapted for fertilisation by Lepidoptera. The passage by which the nectar is reached is so narrow that the proboscis of the butterfly is obliged to touch the anthers, and to effect cross-fertilisation. At the same time the tube in many of the Cyclostigmata is so much lengthened that only such a long proboscis as that of Macroglossus or of Deilephila can reach the nectar. It is probable that the first steps towards the development of closed nectaries were originally serviceable to the plant in protecting the nectar from rain; the flowers being thus rendered more attractive, because the visiting insects had a chance of finding undiluted nectar even after a shower. The lengthening of the corolla tube in the abovementioned section of Gentians which protects the nectar from all but a few long-trunked insects, confers the same kind of advantage on the flower, for it is thus rendered highly attractive to those insects which can alone obtain

[1] Alpine as used by Dr. Müller means above the tree-limits.

the honey; and they will fly from flower to flower, passing over the less attractive kinds.

The genus Rhinanthus has been made especially interesting by Dr. Müller. Rhinanthus is essentially a "bee-flower," but *R. alpinus* has been modified so as to be fertilised by Lepidoptera. The ordinary entrance by which bees visit the flowers of *Rhinanthus cristagalli* is here closed, and a special "butterfly-door," a minute aperture at the tip of the upper lip, has been developed; it is moreover advertised to Lepidoptera by a pair of violet flaps on each side of the entrance. The interesting point about this genus is the existence in it of a species which shows in what manner the flower of *R. alpinus* (fitted for Lepidoptera) may have been developed out of a "bee-flower" such as *R. cristagalli.* This intermediate form (*R. alectorolophus*) possesses a "Lepidoptera-door" like that of *R. alpinus*, but has not closed the bee-door; it is therefore visited by both bees and Lepidoptera and cross-fertilised by both. In spite of our knowledge of this interesting intermediate form, the evolution of *R. alpinus* remains a difficulty. For although it is adapted for the legitimate visits of Lepidoptera only, it is plundered by bees, who break in by the closed bee-door; and these useless or injurious visits are actually more frequent than the advantageous ones of Lepidoptera. It seems impossible to believe that a butterfly-flower could be developed under such circumstances, and the only explanation which Dr. Müller offers requires the assumption of two changes of condition. First, the spread of the bee-fertilised ancestors into regions (such as the Heuthal in the Bernina) where they would be visited exclusively by Lepidoptera, and where the present form of the corolla might be developed. Secondly, we must suppose that the plant has spread into regions where it is visited by bees; or else the plant has remained stationary while bees have invaded its habitat. A similar kind of argument is applied to those flowers, *Polygala alpestris*, various Papilionaceæ, &c., which, though structurally adapted for fertilisation by bees, are, in the alps, chiefly visited by Lepidoptera. They cannot therefore have been developed in their present habitat, but must have spread to the alps from the lowland regions.[1] Dr. Müller compares flowers like *Rhinanthus alpinus*, Gentiana, (Cyclostigma), *Erica carnea*, &c., to air-breathing vertebrates which have been derived from water-breathing ancestors, whose gills have been replaced by special air-breathing structures—the lungs; while *Rhinanthus alectorolophus* corresponds to those intermediate forms which still possess both gills and lungs. We may perhaps, by an inversion of the simile, compare such plants as *Polygala alpestris*, a nearly unmodified bee-flower visited almost exclusively by Lepidoptera, to the Cetacea, which, though actually breathing air, lead almost the life of a fish.

Dr. Müller's treatment of the genera Rhinanthus and Gentiana are instances of the manner in which many other groups are treated. Thus the interesting series of forms exhibited by the Caryophylleæ suggest the possible steps through which bright-coloured flowers adapted for Lepidoptera, such as those of the pinks, have been developed from the pale-coloured scentless flowers with unprotected nectar which are chiefly visited by Diptera.

[1] This view is, for reasons given in the text, put forward merely as a speculation (p. 559).

The present volume gives continual evidence of Dr. Müller's knowledge of the structure and habits of insects. But it does not (and this could not have been expected) contain anything like the valuable study of insects contained in the author's "Befruchtung"—a research of which we take this opportunity of expressing our high admiration, in which we shall be joined by those of our readers who remember the excellent articles by Dr. Müller on insects which appeared in these pages.

In considering the modifications of flowers produced by their relations to insects, we are prepared to find that, for instance, flowers fertilised by bees differ in shape from those visited exclusively by Lepidoptera, but it does not seem *primâ facie* probable that the colours should be characteristic of the two classes. Yet Dr. Müller believes that this is the case, and shows how it may probably be connected with fundamental differences between the lives of bees and butterflies. A bee having to work not only for its own livelihood but also for its nest, is driven to a greater degree of activity than the self-indulgent butterfly. It is therefore important that a bee should work with more method, and thus it happens that bees usually visit one species of flower at a time, instead of passing from species to species and wasting time in the constant change of action. On this account it is obviously an advantage for a bee to be able to distinguish easily a large number of species, thus their unconscious selection has acted in the direction of producing great variety of colouring. It is indeed a remarkable fact that flowers which are visited by short-trunked insects are often characterised by a single colour (usually yellow or white) running through a whole group, whereas closely-related "bee-flowers" are generally varied in colour.

Here then we have a curious chain of cause and effect, beginning with the fact that bees have to provide food for their young, and ending with the varied colours of species of Labiates, Pedicularis, and Trifolium, &c.! If any proof is needed of the correctness of the first link in the argument, it may be found in the curious fact that parasitic or cuckoo bees differ markedly from other bees in their habits,[1] visiting merely those flowers whence they can obtain enough honey for themselves with least trouble and doing it in a dawdling manner which meets with no approval from Dr. Müller.

In the flowers adapted for or chiefly visited by Lepidoptera, red and in a less degree blue are the prevailing tints. There seem to be some grounds for believing that butterflies prefer flowers resembling themselves in tint. Thus in sunshiny weather the orange-yellow flower-heads of Arnica, *Senecio Doronicum*, &c., and the orange-red ones of *Crepis aurea* and *Hieracium aurantiacum* are veritable "Tummelplätze" for yellow-red species of Argynnis and Melitæa. On the other hand the blue Phyteumas which decorate the alpine turf in thousands are especially visited by blue Lepidoptera ("Blues"). It is hard to say whether the butterflies have preferred flowers coloured like themselves, because these tints have been already rendered attractive through sexual selection. Or whether *vice versâ* we may suppose that the colours of their favourite flowers have reappeared as sexual decoration; or lastly, that

[1] P. 522; Dr. Müller adds a caution that the number of observations on this point are perhaps hardly sufficient to warrant a well-grounded conclusion.

some physical quality in their organisation makes certain colours attractive wherever they appear.

To Dr. Hermann Müller belongs the credit of studying not only the means by which cross-fertilisation is effected, but also the means for ensuring cross-fertilisation. He has indeed made this subject peculiarly his own, and has worked it out with valuable and striking results. He has pointed out that flowers which are incapable of self-fertilisation may run great risks of not being fertilised at all. Whereas the flowers in which self-fertilisation is possible are in no danger of becoming sterile, though they may lose the advantage of cross-fertilisation. He has shown that in many plants two forms of flowers exist, one adapted for cross- the other for self-fertilisation. This is the case with *Lysimachia vulgaris* ("Befruchtung," p. 348); when it grows in sunny places where it is freely visited by insects, it has large dark-yellow petals coloured red at the base, conspicuously coloured filaments, and sexual organs arranged so that self-fertilisation can hardly occur; the other form grows in shady ditches, and has a pale yellow corolla and inconspicuous filaments, and the style is so short that self-fertilisation will be sure to take place if no insects visit the flower.

The present volume, though it does not, as far as we are aware, add anything new in principle to the subject of self-fertilisation, contains many illustrations of the correctness of Dr. Müller's views.

We cannot pretend to give, in the short compass of a review article, any fair idea of the richness of Dr. Müller's latest work in new facts and generalisations; we conclude by expressing a hope that it may before long find a translator, or what is a much greater difficulty—a publisher in England.

FRANCIS DARWIN

OUR BOOK SHELF

Lehrbuch der organischen Qualitativen Analyse. Von Dr. Chr. Th. Barfoed. Zweite Lieferung. (Kopenhagen: Andr. Fried. Höst und Sohn, 1881.)

THE first part of this excellent book has already been noticed in these columns. The book is to consist of three parts: the second, which is now published, is characterised by the same completeness and exactness which rendered the earlier part so valuable as a reference book for the laboratory. The present part treats fully of the methods for detecting, in mixtures of varying degrees of complexity, alcohol, ether, chloral, neutral fats, volatile oils, sugar, gum, albumin, &c.

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.]

Mr. Butler's "Unconscious Memory"

MR. BUTLER appears to have somewhat misunderstood the aim and scope of my review. He says, "It is true I have attacked Mr. Darwin, but Mr. Romanes has done nothing to show that I was not warranted in doing so." Why should Mr. Butler have expected any such consideration of his case from me? If I were to assault a man in the street I should not expect the policeman to show that I was not warranted in doing so; it would be for me to show that I was so warranted. Therefore, while acting the part of policeman in this matter, my only object was that which I stated, viz. the punishment of an offender, not the refutation of charges which I advisedly characterised as "preposterous, and indeed ridiculous." Truly it would have been a senseless thing had I for a moment imagined that such charges called for anything like a defence of Mr. Darwin. If ever in the world's history there was a book which appealed to all classes of intelligent readers, that book is the "Origin of Species"; and never in the world's history has a book been more studiously criticised or produced a more tremendous change of thought. Can Mr. Butler therefore seriously believe, that after this book has thundered through the world for more than twenty years, it required him to show in what degree it had been anticipated by some of the most celebrated writers within the last two or three generations? Surely common modesty and common sense, were either present, might alike have dictated caution in attributing to all the world an ignorance such as his own, which could be "thrown off the scent of the earlier evolutionists" by anything that Mr. Darwin could say. The publication of the "Origin of Species" could only have had the effect, whether or not its author desired it, of directing renewed attention to the works of "the earlier evolutionists"; and therefore, to put it on no other grounds, it is difficult to imagine a case in which any intentional concealment of the claims of predecessors could well be more impolitic. But the simple fact is that these predecessors had no claims to be concealed, further than those mentioned in my previous communication; that is to say, while they unquestionably and *notoriously* believed in the fact of evolution, they had nothing which deserves to be called a theory of evolution. Therefore, when Mr. Butler asks of the opening passage in the "Origin of Species," "What could . . . . more distinctly imply that the whole theory of evolution that follows was a growth in Mr. Darwin's own mind?" the answer simply is that this whole theory *was* a growth in Mr. Darwin's own mind. And if Mr. Butler has not judgment enough to distinguish between the scientific value of Mr. Darwin's work and that of "the earlier evolutionists," at least he might pay sufficient deference to the judgment "of all Europe and those most capable of judging" to explain why it is that the work of all the earlier evolutionists proved barren, while the work of Mr. Darwin has produced results unparalleled in the history of thought.

But I am being drawn into a mere waste of time in thus discussing what every one must feel does not admit of discussion. My object in now writing is not to justify Mr. Butler's view that Mr. Darwin requires to be defended from any such nonsensical "attack"; I write in order to withdraw two passages from my review. Mr. Butler says I was wrong in implying that he supposed Mr. Darwin to have entered into a conspiracy with Dr. Krause; he merely supposes Dr. Krause to have acted the part of a "cat's-paw." In this therefore I stand corrected; for while reading "Unconscious Memory" it never occurred to me that Mr. Butler's view was other than I stated. The second passage which I desire to cancel is that which attributes a motive to Mr. Butler in publishing "Evolution, Old and New." He scornfully repudiates the motive which I attributed, and I therefore willingly withdraw the attribution—observing merely that I was induced to advance it because it seemed to present the only rational motive that could have led to the publication of such a book.

Two other allusions to myself may be noticed before I end. Mr. Butler says, "I suppose Mr. Romanes will maintain me to be so unimportant a person that Mr. Darwin has no call to bear in mind the first principles of fair play where I am concerned." To this I answer emphatically, No; but I do maintain that had Mr. Butler been a more important person than he is, he would not have regarded the mere omission of a foot-note of reference to his book, either as an intentional wrong to himself, or as a matter of such grave concern to the public.

Lastly, Mr. Butler says, "I maintain that Mr. Darwin's recent action and that of those who, like Mr. Romanes, defend it, has a lowering effect upon this standard [*i.e.* of good faith and gentlemanly conduct]." I am sure the world of science ought to feel very grateful to Mr. Butler for his kind solicitude on the subject of its morals and gentlemanly feeling. But he has already said in "Unconscious Memory" that he does not look to "ladies and gentlemen of science" for much sympathy, seeing that his case rests on "facts," and that among these "ladies and gentlemen" "familiarity breeds contempt of facts"; and I fear that in this his conclusion will prove better than his argument. For unless some facts and feelings are displayed other than those already exhibited, I cannot think that either the morality or the courtesy of the scientific world is likely to be improved by the renewed exertions on their behalf which are about to be made by Mr. Samuel Butler.

GEORGE J. ROMANES

[This correspondence is now closed.—ED.]

WILL it go any way towards calming Mr. Butler's zeal in the cause of literary honesty to remark that at any rate fifteen years ago, and it may have been further back, Mr. Darwin prefixed to "The Origin of Species" a historical sketch of the progress of opinion on that subject? In view of this it is at least very *misleading* on the part of Mr. Butler to quote the first sentence from the edition of 1859, and then to ask: "What could more completely throw us off the scent of the earliest evolutionists?" as if in those days it would have made a pin's difference to him, or any one else whom he includes in the *us*, whether the scent of the earlier evolutionists lay strong or weak in the track. In these days he should know, if he knows anything of the history of opinion, that these predecessors of Mr. Darwin, with their great though varied merits, had been laughed down, and, for all popular estimation, might be said to have disappeared. To have relied in any way on their authority when Mr. Darwin's book was first published might well have increased the mountain of prejudice against his views without in any way relieving the weight of ridicule that lay upon theirs. When the whole scientific world had been stirred to its foundations and when the whole world almost had been roused into paying attention to science by the awakening genius displayed in the new exposition *de rerum naturâ*, then, when it could best be done, Mr. Darwin turned ridicule into renown, and made all who could even remotely claim to have anticipated or shared his views participators of his fame. Not those who scatter seed at random, but those who cultivate it in chosen ground with indefatigable industry and prevailing skill should, I imagine, be considered the chief benefactors of mankind; and in like manner the fancy that may have fluttered uselessly through many brains becomes at last a fruitful hypothesis or a wide-stretching theory when it falls beneath the cultivation of undaunted genius.

T. R. R. STEBBING

Tunbridge Wells, February 7

"Prehistoric Europe"

WILL you kindly allow me a few words in reply to certain statements made by Prof. Dawkins in his notice of my "Prehistoric Europe." I shall not remark on the perplexing confusion which he gravely puts forward as an outline of my general argument further than to say, in all sincerity, that I fail to recognise in it any trace of what that argument really is. The few observations I have to make shall be confined chiefly to questions of fact.

1. Mr. Dawkins states that I ask geologists to believe that the mammaliferous gravels with Palæolithic implements, which overlie the chalky boulder-clay of East Anglia, were covered by an upper and younger boulder-clay, which latter "has been removed so completely that no trace of it is now to be seen." Now I do not believe that the gravels in question ever were covered by boulder-clay, nor have I written anything which could justify Mr. Dawkins in attributing to me an opinion so absurd.

2. The account I have given of Victoria Cave was written after a careful perusal of all that has been said about it, and my proofs were submitted to Mr. Tiddeman, who reported on the explorations; and therefore I have every reason to believe that my description is correct.

3. The so-called Upper Pliocene deposits at Mont Perrier are described in detail by Dr. Julien, who shows that they are truly interglacial, being younger than the great "pumiceous conglomerate" with its striated stones and blocks, and older than the more recent moraines of the same neighbourhood. Dr. Julien remarks: "La période pliocène supérieure doit disparaître de la science." He correlates the interglacial beds of Mont Perrier with those of Dürnten.

4. The lignites of Leffe and Borlezza, according to Prof. Stopanni, who has carefully studied those closely-adjoining districts, belongs without any doubt whatever to the glacial series; and his observations I have confirmed by a personal examination of the ground. They are generally admitted by Italian and Swiss geologists to be on the same horizon as the lignites of Dürnten.

5. I have not asserted the interglacial age of the so-called Pliocene of Olmo. The newer deposits in the Upper Val d'Arno, which have usually been assigned by palæontologists to the Upper Pliocene, have been shown by Prof. Mayer, after an exhaustive analysis of the evidence (as well stratigraphical as palæontological) to belong to the Pleistocene; and as their mammalian fauna corresponds with the fauna of the lignites of Leffe and Borlezza, I have said that this fact is "significant," meaning thereby that the beds in question may very likely be of the same age as those near Gandino.

6. Mr. Dawkins says that I deal with my subject not with the impartiality of a judge, but as an advocate, and that I have only called those witnesses which count on my side. I am probably as well acquainted with the literature of the subject as my critic, and after many years' careful reading and study must confess that I have not encountered any evidence that contradicts my views. Had it been my fortune to come upon such evidence I feel sure that I should not have been so weak and foolish, or so untruthful as to have ignored it. Doubtless I have met with many forcible statements of opinion by Mr. Dawkins that he does not agree with me; but I may remind him (and not for the first time) that mere expressions of opinion, however emphatic, prove nothing save, as a rule, the sincerity of him who utters them.

7. My critic further ventures the statement that my classification "is based on ice, and ice only." How very far this is from being the case any candid person may see who shall take the trouble merely to run his eye over the "contents" of my book. Geologists rightly refuse to accept classifications which are based upon so narrow a foundation as a single series of phenomena, such, for example, as Mr. Dawkins's attempt to classify the Pleistocene by reference to the mammalia alone—a classification which, while it draws the line that separates Pliocene from Pleistocene at the base of the glacial deposits in England, would carry the same line, in France and Central Europe, through the middle of the glacial series. Or, to put it another way, if we accepted Mr. Dawkins's classification, we should be forced to admit that the Glacial Period attained its climax in France and Central Europe during Pliocene times, but that it did not begin in England until after the Pleistocene had commenced. And this is the classification which, as may be inferred from the tenor of my critic's remarks, I ought to have adopted.

Mr. Dawkins's remarks upon my views in regard to the evidence of climatic changes I am sorry to say I do not understand. All that I am sure of is that he has quite failed to grasp my meaning—that he has attributed to me opinions which I have done my best to refute—in a word, that he has strangely misrepresented me. But I need not attempt to set him right, as those who are sufficiently interested in the matter are not likely, after this repudiation, to accept his travesty for a reliable presentment of my views.

JAMES GEIKIE

Perth, January 7

On Dust, Fogs, and Clouds

A CURIOUS confirmation of Mr. Aitken's theory of fog was brought to my notice a short time ago. A friend of mine residing in Streatham, struck with the perfection of the heating arrangements in American residences, fitted up his house with a similar contrivance. In the basement was a furnace and boiler which warmed pure air that entered from without, and circulated at a regulated temperature throughout the house. A water-pipe that was connected with the boiler became stopped by frost; an explosion ensued, and the house was filled with so-called steam (hot fog, in fact) from top to bottom. Wherever a cold surface (clock faces, metal fixtures, &c.) was found, even in the topmost bed-rooms, the vapour condensed and left behind it black carbon dust. Nowhere else was this dust found.

Again, few persons who have read Mr. Aitken's paper can have noticed the dejected appearance of the late beautiful snow on the first morning of the welcome thaw without thinking of his theory. What on the previous evening was a clean dazzling mass of exquisite white became a sooty speckled heap of dirty snow. As the sparkling crystals liquefied into water which drained away, they left behind the dust and carbon, around which, according to Mr. Aitken, they originally formed, becoming by multiplication molar and visible. In the streets of London the masses of white snow rapidly became, as somebody remarked, like streams of cold *café au lait*. The whiteness rapidly disappeared and left behind mere dirt.

It may interest some of your readers to know that in 1537

Benvenuto Cellini was attracted to Paris from Florence in consequence of the much clearer and more beautiful atmosphere in the capital of France than in Italy! This fact is derived from the artist's autobiography. What a change now! Paris is rapidly becoming as bad as London. W. H. PREECE

February 5

IN NATURE, vol. xxiii. p. 195, I found an interesting abstract of a paper read to the Royal Society of Edinburgh, December 20, by Mr. John Aitken, showing "that dust is the germ of which fogs and clouds are the developed phenomena." It is not in the least the intention of this letter to diminish the value of the above-mentioned paper and experiments, but I wished to say that already, several years past, the same results were obtained by Messrs. Coulier and Mascart (1875) in France (*Naturforscher*, 1875, p. 400; *Journal de Pharmacie et de Chimie*, série 4, xxii. p. 165).

In my "Théorie cosmique de l'Aurore Polaire," p. 36, I have already pointed out the great importance of these results on the relation between auroræ and clouds and the danger of measuring the height of auroral displays by means of superior cloudy apparitions (p. 35). In fact, if the invisible aqueous vapour is able to reach much higher regions than terrestrial dust, and if auroræ are in close connection with cosmical matter in a state of extreme division, like our theory attempts to prove, this cosmical matter is without any doubt enabled to form aqueous clouds in a much higher than the usual level. Moreover we have already, in 1873, in the German journal *Gaea* (Köln und Leipzig, E. H. Mayer), asked: "Welches wohl die weitere Rolle der Eisen- und anderen Dämpfe sei, welche nach der Verbrennung in den oberen Regionen der Atmosphäre schwebend bleiben und offenbar nach vollständiger Abkühlung einen Niederschlag von fein vertheiltem Eisenoxyd und anderen Stoffen bilden. Sollten diese Theilchen . . . keine Veranlassung geben können zu den von deutschen Beobachtern so oft wahrgenommenen 'Polarbändern,' [1] deren Zusammenhang mit dem Nordlicht schon öfters dargethan ward, aber bisher unerklärt blieb. Noch würden wir hinzufügen können, mit Hinweis auf die Beobachtung Secchi's eines angeblichen Nordlichts bei Tage (NATURE, October 17, 1872), dass auch die bis jetzt ganz unerklärte, eigenthümliche Gestalt der Cirri, mit ihren ganz regelmässigen, auf ein gewisses Gesetz hindeutenden transversalen Verzweigungen, von der Anwesenheit feiner Eisenstaubkerne in den Eisnadeln möglicherweise bedingt ist. Bekanntlich schweben diese Cirri in den höchsten Wolkenregionen."

It will further be generally known that microscopic meteorites have been found in the centre of hailstones (*Comptes rendus*, 1872, p. 683). H. J. H. GRONEMAN

Groningen (Netherlands), January, 1881

[1] Or "Polarbanden." My daily observations of these phenomena, beginning with the year 1875, are to be found in the German journal *Wochenschrift*, editor, Dr. Hermann J. Klein in Köln.

New Cases of Dimorphism of Flowers—Errors Corrected

REVIEWING my notes and drawings of some years ago, I find the following new cases of dimorphism of flowers:—

1. *Syringa persica*, L., cultivated in the garden of the Lippstädter Realschule, is gynomonœcious. In the same inflorescence there are found a majority of hermaphrodite flowers of larger size and a minority of female flowers of smaller size. The hermaphrodite flowers are homogamous and short-styled, like *Syringa vulgaris*, L. (H. Müller, "Die Befruchtung der Blumen," p. 340, Fig. 125). The anthers of the female flowers, which are much reduced in size and never contain any pollen, are inserted sometimes above, sometimes beneath, but commonly in the same height with the stigma. In some few of the small-sized flowers the number of the petals is reduced to three.

2. *Stellaria glauca*, L., near Lippstadt, is gynodiœcious, like *St. graminea*, L., as described by F. Ludwig (*Bot. Centralblatt*, No. vi. p. 28), some stems bearing small-sized flowers with very reduced anthers of white colour and greatly-developed stigmas, a vast majority of other stems bearing larger-sized proterandrous flowers with anthers of red colour.

3. *Sherardia arvensis*, L., near Lippstadt, is likewise gynodiœcious, its hermaphrodite flowers being proterandrous and larger-sized, with a corolla of 3½-4 mm. diameter, its female flowers possessing a corolla of only 2½-3 mm. diameter, with extremely reduced anthers.

4. *Asperula tinctoria*, L., produces in Thuringia so frequently flowers with only three petals that in those stems examined by myself by far the greatest part of the flowers were three-petaled.

In my book "Alpenblumen" Dr. Focke of Bremen has detected two errors of naming, which immediately ought to be corrected: the flower described and illustrated on p. 171 is not *Empetrum nigrum*, but *Azalea procumbens*, like that of p. 377; *Cerinthe*, in pp. 264, 265, is not *major*, L., but *glabra*, Mill = *alpina*, Kit. HERMANN MÜLLER

Lippstadt

Geological Climates

I HAVE read with much interest and attention the letters that have appeared in recent numbers of NATURE on the subject of "geological climates," and although it must appear presumptuous on my part to do so, I shall endeavour to show that each of the distinguished writers of these letters may be somewhat in error on at least one point, which—if I am right—must materially affect the correctness of the conclusions they have come to.

I think that Mr. Wallace, whilst very justly giving the Gulf Stream and other currents which *might* exist were certain lands submerged, credit for great influence in ameliorating the rigour of climate, does not take into sufficient consideration the fact that the waters of the Gulf Stream, although warmer, are, in consequence of holding much more salt in solution, heavier than the colder and less saline Arctic current.

Some experiments show, as clearly as anything done on a very small scale can, that two waters brought as nearly as possible to the conditions of the Gulf Stream and the Arctic current do not mingle when simultaneously poured into a long narrow glass trough; the Arctic water invariably taking its place on the surface.

Supposing then that these two currents meet somewhere about latitude 80° or 81° N., the Arctic water flowing south—if my experiments are of any value—will retain its position on the surface and the warm current pass underneath, and thus lose all its heating influence on the air over a Polar area about 1000 geographical miles or more in diameter.

We can have no stronger example of this effect of difference of density of ocean water than is shown by the two currents *in* and *out* of the Mediterranean Sea.

In NATURE, vol. xxiii. p. 242, Prof. Haughton says, "The thickness of this ideal ice-cap at the Pole is unknown, but from what we know of the Palæocrystic ice of Banks Land and Grinnel Land *must be measured by hundreds of feet, and its mean temperature must be at least* 20° F. *below the freezing-point of water.*"

With regard to both the above assumptions—which are in italics—I must beg to disagree entirely with the learned Professor. He appears to consider the so-called Palæocrystic ice as the normal state of the ice at and near the Pole, and as a natural growth by the gradual freezings or increase of a single floe during a series of years; whereas I am of opinion that this mis-called Palæocrystic ice is the result of a number of floes being forced over and under each other by immense pressure caused by gales of wind and currents.

The western and northern shores of Banks and Grinnel Lands are peculiarly well suited for the formation of such ice-heaps, as they are exposed to the full force of the prevailing north and north-west storms, which pile up the ice in a wonderful manner on these shores and others similarly placed, for a distance of miles seaward. The whole of the west shore of Melvile Peninsula is so lined with rough ice of this kind that sledging is impossible.

It will wholly depend upon the form of land—if any—at or near the Pole, whether or not any floebergs are there. If there is no land it is probable there will be few or none, as the ice will meet with no great obstruction, as it is driven by winds and currents.

I have no authorities by me that give the thickness of ice formed in one season at or near the winter quarters of any of the Arctic expeditions, except my own in 1853-4 at Repulse Bay, latitude 66° 32′ north.

The measurements of the ice—taken at some distance out in the bay where there was very little snow—and the mean temperature of the air are given on next page.

1853	Ice thickness	Increase	Monthly Mean Temp. F.
December 20	... 4 feet 7 inches ...	—	... −24°·5 below zero
1854			
January 24	... 5 feet 9 inches ...	14 in 35 days ...	−30°·6 ,, ,,
February 25	... 7 feet 0¾ inch	... 16 in 32 days ...	−34°·9 ,, ,,
April 25[1]	... 8 feet 1½ inch	... 12¾ in 59 days ...	− 8°·5 ,, ,,
May 25	... 8 feet 1¼ inch	... none 30 days ...	+24° above zero

The above table shows that the ice ceased to increase in thickness some time between April 25 and May 25, after which it decreased rapidly; but I was unable to decide what proportion of this decrease was due to thaw and evaporation from the surface, and what amount from the lower part of the floe that was under water: no doubt by far the greater effect was produced by the two first causes.

Eight feet may perhaps be considered a fair or rather a high average of one winter's formation of new ice (not increase of an old floe) over the whole of the Arctic Sea, because Repulse Bay, although in a comparatively low latitude, was particularly favourable for ice-formation, there being no currents of any consequence. Where there are currents, one year's ice does not exceed three or four feet.

The winter's ice of 1875–6 at Discovery Bay, in latitude 81° 40′ N., did not exceed, if I remember correctly, six feet in thickness.

Even were these great compound floes, called Palæocrystic ice, found at or near the Pole, and of only the same thickness as those seen at Grinnel Land—instead of "hundreds of feet"—they would not probably have nearly so low an average temperature all the year round as 20° F. below the freezing-point of water, because only one-sixth of their mass would be exposed to very low temperatures for about six months of the year, the surface being during that time protected by a more or less thick covering of snow, whilst at least five-sixths of their bulk was under water, having a temperature for the whole twelve months at or above the freezing-point of the sea. The question is, how far the very low temperatures of an Arctic winter do penetrate a mass of, say sixty feet of ice, the surface of which is covered with a foot of snow, and fifty feet or five-sixths under water of a temperature at or above the freezing-point of the sea?

From my experience on a much smaller scale, I do not believe that the atmospheric cold would, under the circumstances mentioned, penetrate to the lower surface of ice sixty feet thick; and if it does not do so there would be no increase to its thickness during winter.[2]

An excellent example of formation of Palæocrystic ice, or floe-berg is afforded by the experience of the Austro-Hungarian Expedition under Weyprecht and Payer in the Barentz Sea in 1873–4. Their ship was lifted high out of the water by the pressure of the floes, which were forced over and under each other to a great thickness and extent in a very few days.

The ship and her crew were helplessly drifted about for many months, during which the floes were frozen together into one solid mass, and the inequalities of the surface in a great measure filled up with snow-drift. JOHN RAE

4, Addison Gardens, January 29

On the Spectrum of Carbon

IN addressing to you my former letter regarding Dr. Watts's experiments on the spectrum of carbon, it was not my intention to enter on any discussion concerning matters of opinion. The reference made in that letter to the difficulty of perfectly drying a gas so as to eliminate the ultra-violet spectrum of water had reference to gases at ordinary atmospheric pressure; and the expectation a gas will be dried "to all intents and purposes" by the use of a U-tube of phosphoric anhydride goes far to explain the origin of different experimental results. The cogent experimental evidence which Dr. Watts justly demands may, so far as the relations of carbon and nitrogen are concerned, be found in our complete papers on the spectrum of carbon compounds in the *Proceedings* of the Royal Society.

The supposition, which appears to be a difficulty to Dr. Watts's mind, that traces of nitrogen in hydrocarbons give with the spark the spectrum of nitrocarbons, and that traces of hydrogen in cyanogen give the hydrocarbon spectrum, is not only "reasonable," but appears to me most consistent with the spectrum observations on the whole, and with the chemical regarding the formation and relations of acetylene and hydrocyanic acid.

Cambridge, January 22 G. D. LIVEING

[1] The mean temperature opposite to April is that of March and April combined, and it will be seen that the average increase of ice for each of these months is only 6¼ inches.

[2] That the sea raises the temperature of the ice on its surface even in very cold weather, is evinced by the fact that a snow hut built on the ice is warmer than if built on the land.

Vibration of Telegraph Wires During Frost

MR. T. M. READE asks for an explanation of this phenomenon. In *Science Gossip* for 1874, p. 254, there is a short article of mine on "Frost Phenomena," and one of those referred to is this curious vibration of telegraph wires.

The explanation there suggested, which was only a guess, is probably incorrect; but I think I can give the true one now, and it is, as usual in such cases, extremely simple.

Hoar frost is only deposited in air which is nearly at rest; a strong wind shakes it down as it forms. But there is nearly always a slight air-current in one definite direction, and the ice spicules are built up "in the teeth" of this current, that is on the windward side of the wire or twig.

They always point *towards* the wind. When they have attained a length of, say, half an inch, if the direction of the air-current slightly changes, it may strike the comb-like fringe no longer on the points, but on the side, and, obtaining thus a leverage upon the wire, will twist it round till the pressure is balanced by the torsion. If the pressure were absolutely constant the wire would perhaps remain in this position, but the very slightest variation of pressure would set up a vibratory motion, and this, I think, must be the true cause of the phenomenon.

Birstal Hill, Leicester, February 5 F. T. MOTT

The Star Oeltzen, 17681

THE star Oeltzen, 17681, whose spectrum was announced by me to consist mainly of a yellow and blue band (NATURE, vol. xxii. p. 483), proves to belong to the same class as the three stars in Cygnus discovered by Wolf and Rayet in 1867 (*Comptes rendus*, vol. lxv. p. 292). A curious feature of these spectra is that they resemble each other without being identical, the relative brightness of the lines being very different. A further study of them is much to be desired.

Cambridge, U.S., January 24 EDWARD C. PICKERING

Zeuglodontia

IN consequence of my letter in NATURE, vol. xxiii. p. 54, the sub-editor of the *Graphic* was kind enough to send me the number of that paper containing the engraving of the animal seen from the *City of Baltimore* (not *City of Washington*, as I had misunderstood), and which is that of April 19, 1879. The sketch from which this was taken was sent by Major H. W. J. Senior of the Bengal Staff Corps, with the following description, viz.:—

"On January 28, 1879, at about 10 a.m., I was on the poop deck of the steamship *City of Baltimore*, in lat. 12° 28′ N. long. 43° 52′ E. I observed a long *black* object abeam of the ship's stern on the starboard side, at a distance of about three-quarters of a mile, darting rapidly out of the water and splashing in again with a sound distinctly audible, and advancing nearer and nearer at a rapid pace. In a minute it had advanced to within half a mile, and was distinctly recognisable as the veritable 'sea-serpent.' I shouted out 'Sea-serpent! sea-serpent! call the captain!' Dr. C. Hall, the ship's surgeon, who was reading on deck, jumped up in time to see the monster, as did also Miss Greenfield, one of the passengers on board. By this time it was only about 500 yards off, and a little in the rear, owing to the vessel then steaming at the rate of about ten knots an hour in a westerly direction. On approaching the wake of the ship the serpent turned its course a little away, and was soon lost to view in the blaze of sunlight reflected on the waves of the sea. So rapid were its movements that when it approached the ship's wake I seized a telescope, but could not catch a view, as it darted rapidly out of the field of the glass before I could see it. I was thus prevented from ascertaining whether it had scales or not, but the best view of the monster obtainable when it was about three cables' length, that is about 500 yards distant, *seemed to show that it was without scales*. I cannot, however, speak with certainty. The head and neck, about two feet in diameter, rose out of the water to the height of about twenty or thirty feet, and the monster opened its jaws wide as it rose, and closed them again as it lowered its head and darted forward for a dive, reappearing almost immediately some hundred yards ahead. The body was not visible at all, and must have been some depth under water, as the disturbance on the surface was too slight to attract notice, *although occasionally a splash was seen at some distance behind the head.* The shape of the head was not unlike pictures of the dragon I have often seen, *with a bulldog appearance of the forehead and eyebrow.* When the monster had drawn its head sufficiently out of the water it let itself drop, as it were, like a huge log of wood, prior to darting forward under the water. This motion caused a splash of about fifteen feet in height on either side of the neck, much in the shape of a pair of wings."

The italics in the foregoing and in the account of Capt. Cox are my own.

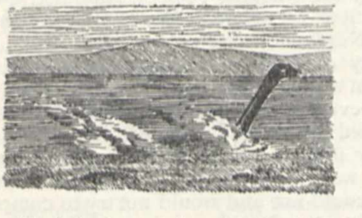


FIG. 1.—The Animal as seen from the *City of Washington.*

The engraving being a large one, of which the foreground is formed by the deck of the steamer, I have copied and send with this that portion of it which shows the animal; and in this it should be observed that besides the splash rising round the neck "like wings," the separate splash at some distance behind the head is also shown, the position of which corresponds to that where the cetacean tail occurs in the figure sent by the captain of the *Kiushiu-maru*, which accompanied my first letter. The foam around the neck, I think, may be due to the splash of the humeroid paddles which a cetacean should possess.

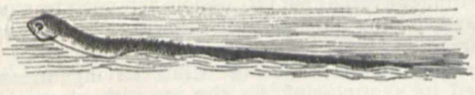


FIG. 2.—The Animal as first seen from H.M.S. *Dædalus.*

The sub-editor of the *Graphic* has also been kind enough to obtain for me tracings from the three figures given in the *Illustrated News* of October 28, 1848, of the animal seen from the *Dædalus.* From two of these I have made the accompanying reductions to one-fourth (linear) of the originals; and the head portrayed in one of these (as seen when the animal passed close under the stern of the *Dædalus*) is evidently not reptilian, but mammalian; and it seems to bear out the "bulldog appearance of the forehead and eyebrow" which Major Senior describes in his case.

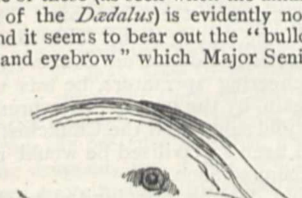


FIG. 3.—Head of the Animal as seen when passing passing under the stern of the *Dædalus.*

From the *Times* of September 24, 1879, I cut the following notice:—

"Capt. J. F. Cox, master of the British ship *Privateer*, which arrived at Delaware breakwater on the 9th inst. from London, says:—'On the 5th ult., 100 miles west of Brest (France), weather fine and clear, at 5 p.m., as I was walking the quarter-deck, looking to windward, I saw something *black* rise out of the water about twenty feet, in shape like an immense snake about three feet in diameter. It was about 300 yards from the ship, coming towards us. It turned its head partly from us, *and went down with a great splash*, after staying up about five seconds, but rose again three times at intervals of ten seconds, until it had turned completely from us and was going from us with great speed, *and making the water boil all round it.* I could see its eyes and shape perfectly. It was like a great eel or snake, *but as black as coal tar*, and appeared to be making great exertions to get away from the ship. I have seen many kinds of fish in five different oceans, but was never favoured with a sight of the great sea-snake before.'"

In this account we have almost a duplicate of that of Major Senior in the dropping of the animal with a great splash into the water prior to its darting forward under it; while the boiling of the water around, which is so inconsistent with the motion of a snake in water (which I have more than once seen) evidently resulted from the strokes of the cetacean tail, and possibly also from those of the paddles, as in the case witnessed by Major Senior. The black colour also is described in both cases.

Capt. Drevar, the statutary declaration of whom and of several of his crew I quoted in my former letter, has written to me, and sent me a printed account (which he says he has circulated) of the conflict which he witnessed, and of the subsequent appearance of the animal rearing its long neck out of the water. This is satisfactory as showing that the declaration I quoted was no hoax, as I feared it might have been; but Capt. Drevar rejects with disdain my suggestion that the animal he saw was not a serpent, though I pointed out to him that nothing having the form of a snake would possess in its submerged portion the buoyancy necessary to enable it to elevate so great a proportion of its length out of water.

Judging from the figures which accompany this and my previous letter, it appears to me that the external form of the animal must resemble the well-known *Pleisosaurus*, if we imagine the hinder (femuroid) paddles of that *Enaliosaurian* to be absent, and a cetacean tail (which is their homologue), to be present in their stead. Since in the direction of the *Porpesse* the cetacean in external form so closely simulates the fish, so it may in another direction simulate this Mesozoic marine saurian, or the gigantic *Elasmosaurus* of the American Cretaceous formations, of which a nearly perfect skeleton is described by Prof. Cope as forty-five feet in length, the neck constituting twenty-two of this length.

Whether, through your circulation, any light on this subject, so far as the character of the skeleton of *Zeuglodon cetoides* is concerned, may be forthcoming from American palæontologists remains to be seen; but there ought, I submit, to remain no longer with naturalists any doubt that a hitherto unknown group of carnivorous cetaceans, with necks of extraordinary length, inhabit the ocean.

It seems to me also most probable that the conflicts which have been so often witnessed (and which Mr. Pascoe in his letter in NATURE, vol. xxiii. p. 35, says he himself twice witnessed), and referred to the Thresher, have been attacks by the animals in question upon whales. SEARLES V. WOOD

Martlesham near Woodbridge

Ice Intrusive in Peat

I HAVE just returned from a walk on the shore at Crosby, where I have been much interested in observing one of the effects of the late severe frost combined with the present thaw.

At the Alt Mouth is a submarine peat-and-forest-bed, and, lying over it, I was much surprised to see innumerable slabs of peat, which an examination showed in most cases contained interlaminations of ice. One slab measured 5 yards by $2\frac{3}{4}$ yards by about 8 inches thick, and right through its mass in a parallel plane with the surface, separating the peat into an upper and lower layer, was a slab of transparent ice, wedge-shaped, being 4 inches thick at one side, diminishing to 1 inch at the other. How the ice got there was the surprising thing, as the peat is very hard and compact, and about 18 inches thick. The holes or places from which the sea had quarried these frozen slabs were plainly to be seen, and I noticed round one of them that the edges or lips of the peat had been forced up by ice inserting itself between the laminæ.

A good deal of water ordinarily oozing through the sandhills flows or trickles over the surface of the peat, and as nearly six inches of rain fell in December last, and the two previous months were wet, the quantity of water would be abnormally increased. In some way or other it must have percolated along the peaty laminæ, and by gradual accretion the frozen water has forced up the layer of peat above it. This has occurred at neaps, and the late high tides, assisted by the thaw and the decreased specific gravity of the mass, has lifted the frozen slabs of peat and inclosed slabs of ice, and torn them from the unfrozen and softer peat below. The slabs may be compared to sandwiches, the ice representing the meat. The ice is evidently fresh-water ice, and possesses a striated prismatic structure at right angles to its surface. In places it protrudes like a tongue from the peat, and is then occasionally perforated with round holes evidently melted through it.

Is it not possible that some of the beds mentioned by geologists in Russia and North America, consisting of alternate layers of ice and earth or gravel, may have been formed similarly by percolation of water, and not be truly bedded, but intrusive?

T. MELLARD READE

Park Corner, Blundellsands, January 30

P.S.—Since writing the above I have again visited the shore to-day, but all the slabs have been rafted out to sea by the high tide. With my geological hammer I broke off some of the frozen peat *in situ*, and find the explanation given to be substantially correct; but I also found that the upper layer of peat was minutely and beautifully interlaminated with ice. It is quite evident that the ice is the frozen water which has percolated from the sand-hills.

January 31

The Squirrel Crossing Water

NEVER having heard of the squirrel taking to the water, I send the following authentic communication. I had heard the story told by another person, and thinking it of sufficient interest I requested her to get it in detail from the lady under whose personal observation it had come. This the latter has most kindly complied with, and I forward it, trusting it may prove of interest to some of the readers of NATURE interested in the habits of animals. Loch Voil, in Perthshire, near Balquhidder, is about four miles in length, with a mean breadth of about one-third of a mile—a considerable extent of water for so small a rodent to face and cross, in search, I suppose, of new nutting grounds.

H. H. GODWIN-AUSTEN

Thalford House, near Guildford, February 5

"*Mountquhanie, Cupar Fife*

"When rowing two ladies down Loch Voil, one afternoon last August, I observed what looked like a little stripe of red brown fur in the middle of the loch. On coming nearer we saw that it was a squirrel swimming across, its tail lying flat on the water. We then heard its claws scratching on the side of the boat, and to our surprise the little bedraggled sprite appeared on the bow of the boat. It was evidently tired, for it sat quite still, staring at us and panting. I rowed on towards the shore, hoping to be able to ferry it across, but after a few minutes it scrambled down to the water again and resumed its journey, probably frightened at the sight of the collie dog who was in the boat. We watched it swimming till it looked like a small speck close to the shore, but lost sight of it before it landed."

SEA-WAVES.—E. B. P., 18, Cromwell Place, S.W., asks: Can any reader of NATURE inform me as to in what books or pamphlets I can obtain the best information relating to the height and length of sea-waves, especially when considered in relation to the navigation of vessels?

BARON NORDENSKJÖLD IN FINLAND[1]

AS is known, Baron Nordenskjöld was born in Finland, and completed his studies at the University of Helsingfors. After his recent visit to St. Petersburg, where the celebrated explorer was made much of, he promised to stop at Helsingfors a few days, for the first time after his successful discovery of the North-East Passage and his circumnavigation of the Eurasian Continent. Having previously paid a short visit to his paternal hall (Frugärd), Nordenskjöld, accompanied by the Baroness his wife, arrived at Helsingfors on the evening of January 13. He was received at the railway station by a deputation consisting of the Rector of the University, Mr. H. Lagus; the President of the Finnish Society of Science, Mr. G. Mittag-Leffler; the Secretary, Mr. L. L. Lindelöf, and others, as well as a select chorus of students, who sang a few patriotic songs. Before the station-house a crowd numbering thousands of people stood cheering and greeting him.

On January 14 the Society of Science had arranged a special meeting, to which friends and followers of science had been invited, and at which were present members of Government, professors of the University, a few of the higher military dignitaries, and a great many fashionables of the town, ladies as well as gentlemen. After an interesting lecture "On the Religions of the Populations of Siberia" by the linguist, Prof. A. Ahlquist, the President of the Society of Science, Mr. Mittag-Leffler, presented to Baron Nordenskjöld a gold medal struck by order of the Society of Science, in memory of their renowned countryman and honorary member, and of the remarkable historical event. The presentation of the medal was accompanied by an address, in which it was stated that the Society of Science, being neither wealthy nor numerous, and well remembering to what a little nation it belonged, could not and would not try to compete with the many eminent scientific societies which had already honoured him with their grants and gifts. Yet the Society of Science hoped Baron Nordenskjöld would kindly accept this tribute of admiration, as having issued from his native country. Nordenskjöld expressed his gratitude in a hearty manner, and then gave a lecture on his "Observations of the Northern Lights at Behring Strait," which greatly excited the interest of his audience. Nordenskjöld was then entertained at dinner by the Scientific Society and the University, at which entertainment toasts were given in honour of Baron Nordenskjöld, the Baroness, and the members of the *Vega* Expedition. At the close of the dinner a torchlight procession, arranged by students, appeared, paying homage to their celebrated countryman by singing and cheering.

The Helsingfors Skating Club having meanwhile adorned its skating-rink on the ice with electric lights and innumerable lamps and torches, then had a visit from the Baron. He was received with singing by a student chorus, followed by the appearance of two polar bears with a chair on skates, who, giving him kind regards from Spitzbergen and Siberia, took him at a tremendous rate up to a pretty little ice temple, where he was greeted by twelve young ladies and gentlemen, all dressed in the picturesque costumes of the Chukchis. These gave him a hearty welcome, and then, with the bears, performed a characteristic dance on skates. Surrounded by thousands of cheering spectators, he was taken back to his carriage again by the bears. In expressing his gratitude Nordenskjöld said that if the Chukchis, and especially the ladies, had been so civilised he would most certainly not have left them so soon.

The following day he was invited to dinner by the Governor-General of Finland, Count Adlerberg, and in the evening the inhabitants of Helsingfors gave a splendid banquet, at which toasts were given in honour of the Emperor Alexander II. and King Oscar II., followed by a speech by Prof. L. L. Lindelöf, relating Baron Nordenskjöld's great deed, and inviting the audience to drink to his health. Other toasts were also given in honour of the Baroness Nordenskjöld, the promoters and members of the *Vega* Expedition, the Fatherland, &c. Nordenskjöld's appearance in Finland excited great rejoicing everywhere, but amid that rejoicing the melancholy thought occurred to one's mind that he had been denied the opportunity of living, and acting, and working in his own country.

On January 16, early in the morning, he left Helsingfors; once more the singing of the students sounded on the platform amid loud cries of "Hurrah" from friends and admirers.

[1] From a Helsingfors Correspondent.

THE JOHN DUNCAN FUND

MR. JOLLY informs us that the subscriptions sent to form a fund to raise this old botanist above the need of parochial relief and provide for his comfort during his remaining years, has already reached a considerable sum, all which has been sent spontaneously from all parts of the country, without the formation of any committee or pressure whatever. More is coming in daily, and the old man's future independence would seem in the end to be pretty well assured. The sympathy shown in the case has been widespread and of the warmest kind. Her Majesty the Queen has graciously sent 10*l.*, and the Duke of Argyll, who sent 10*l.* at first, writes that it is a subscription which ought to be zealously supported by all who are interested in the pursuit of science, and who honour the high moral and intellectual qualities by which John Duncan is distinguished. All this speaks well for the generosity of the country, but more is required. The case is without doubt unusually deserving.

The following is a list of the subscriptions which have been received at this office during the past week:—

	£	s.	d.		£	s.	d.
Amount previously announced	11	15	0	Clay, Sons, & Taylor	1	1	0
Matthew Gray ...	2	0	0	Dr. Sim	1	0	0
F. A. Hamilton ...	2	0	0	Henry Stretch	0	2	0
Received in Registered Letter ...	2	0	0	A. A. Rathbone ...	3	0	0
Jas. Greig	0	10	0	A. G.	0	10	0
George Russell ...	0	10	0	Sidney Billing	0	10	0
Thomas Clarke ...	1	1	0	A. W. Agnew	1	0	0
A Friend	0	10	0	Orry's Dale	0	5	0
L. M.	3	3	0	John Renton Dunlop.	1	0	0
E. V.	0	2	6	Prof. Prestwich, F.R.S.	2	2	0
Miss Wilson	0	5	0	George Knott	1	1	0
Isaholt Fraser ...	0	3	0	H. F. R.	0	10	0
Mrs. Tuckwell ...	0	10	0	Mrs. Henry	1	0	0
Alfred Shipley ...	1	1	0	Miss Weir	0	10	0
E. H. Millar	1	0	0	An Old Woman ...	0	2	6
John Noble	5	0	0	A. E.	0	10	0
W.	0	10	0	J. B. B.	2	2	0
					48	6	0

EXPERIMENTS ON ICE UNDER LOW PRESSURES

CERTAIN theoretical considerations on the relations of the solid, liquid, and gaseous states of matter led me three or four years ago to the speculation that in a perfect vacuum the liquid state would be impossible, and that under this condition it might be possible to raise bodies to temperatures above their ordinary melting-points. These ideas were mentioned to one or two friends at the time, but they naturally considered them as speculations which would not be verified by experiment. From the pressure of other work the subject was for the time dropped, and it was not till the autumn of 1879 that an experimental investigation was commenced. The first substance tried was sulphur, but this was ultimately found to be unsuitable, as under low pressures, though it apparently boiled as low as 130° C., yet at that or a little above that temperature it began to froth. Naphthalene was then tried, but as the pressure at which the boiling-point fell below the melting-point was less than about 7 mm., it was not easy to *maintain* the pressure at a sufficiently low point. Mercuric chloride however, which was the next body tried, yielded better results.

Mercuric chloride melts at 288°, resolidifies at 270°-275°, and boils at 303°. About 40 grammes of the pure compound were placed in the tube A (Fig. 1), and a thermometer arranged with its bulb imbedded in the salt. The drawn-out end of the tube was connected by stout india-rubber tubing with one branch of the three-wayed tube B, whilst the other was attached to the manometer C. B was connected with a Sprengel pump fitted with an arrangement for regulating the pressure. When the pressure had been reduced by means of the pump to below 420 mm., the mercuric chloride was strongly heated by the flame of a Bunsen's burner, with the following results:—Not the slightest fusion occurred, but the salt rapidly sublimed into the cooler parts of the tube, whilst the unvolatilised portion of the salt shrank away from the side of the tube, and clung tenaciously in the form of a solid mass to the bulb of the thermometer, which rose considerably above 300° C., the mercury shooting up to the top of the stem. After slight cooling, the air was let in, and under the increased pressure thus produced the salt attached to the bulb of the thermometer at once melted and began to boil, cracking the tube at the same time.

The experiment was next varied as follows:—About the same quantity of chloride was placed in the tube as before and heated by the full flame of a Bunsen's burner. The lamp was applied during the whole of this experiment, and the size of the flame kept constant throughout. The

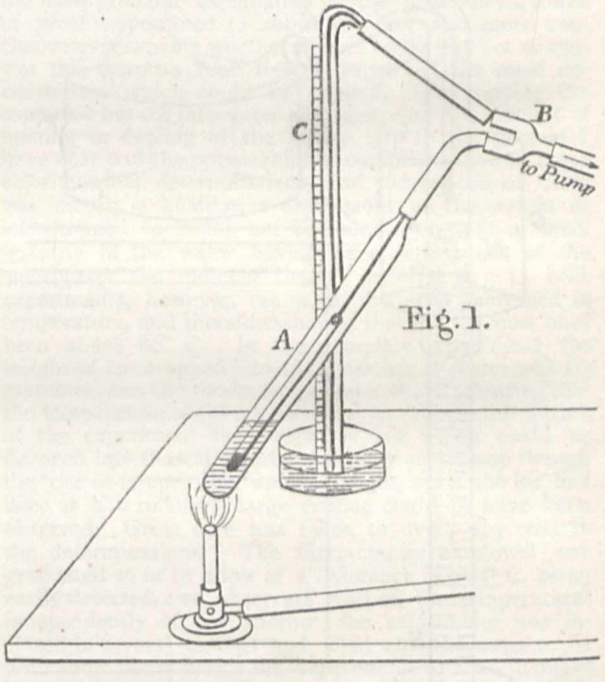


Fig. 1.

mercuric chloride first liquefied and then boiled at 303° under ordinary pressure, and whilst the salt was still boiling the pressure was gradually reduced to 420 mm., when the boiling-point slowly fell to 275°, at which point the mercuric chloride suddenly began to solidify, and at 270° was completely solid, the pressure then being 376 mm. When solidification was complete the pump was stopped working, but the heat still continued to the same extent as before. The salt then rose rapidly to temperatures above that at which a thermometer could be used, but not the least sign of fusion was observed. From the completion of the solidification to the end of the experiment the pressure remained at about 350 mm.

The above experiment, which was repeated three times, shows therefore that when the pressure is gradually reduced from the ordinary pressure of the atmosphere to 420 mm., and the boiling-point simultaneously from 303° to 275°, the salt solidifies while it is still boiling, notwithstanding that it is being strongly heated at the same time, and that, after solidification is complete at 270°, the temperature then rises far above the ordinary boiling-point (303°) of the substance without producing any signs of fusion. Under ordinary circumstances mercuric chloride melts at

288° and re-solidifies at 270°-275°, *i.e.* at a temperature identical with that at which it solidifies under diminished pressure as above described.

After the above experiments had been made the investigation had to be unavoidably deferred, and was not resumed till last autumn, when a large number of determinations were made of the boiling-points of several different substances under various pressures, and from these was drawn the general conclusion described in a letter to NATURE (vol. xxii. p. 434), in September last, viz.: "In order that any solid substance may become liquid it is necessary that the pressure be *above* a certain point, called the critical pressure, otherwise it cannot be melted, no matter how great the heat applied." Assuming the truth of this conclusion, I set to work to apply it in the case of ice, as it would undoubtedly have the greatest interest in connection with that substance. On this account my experiments since the end of August have related almost solely to ice.

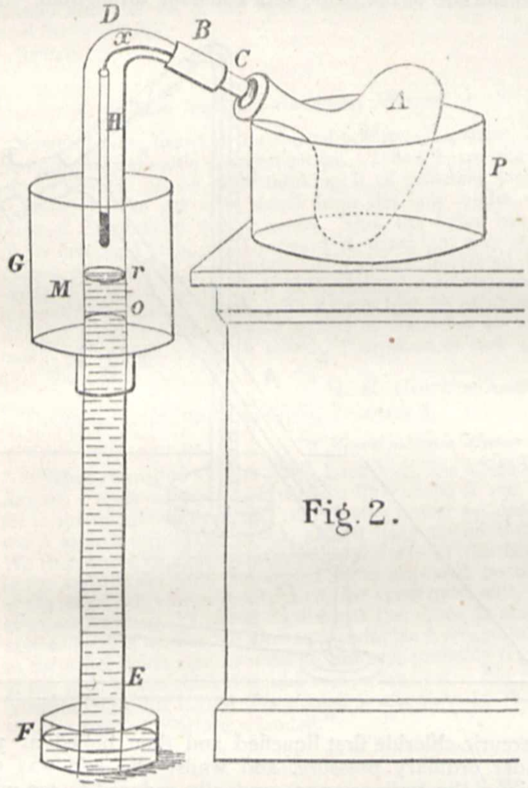


Fig. 2.

The problem to be solved was whether ice could be prevented from melting by maintaining the pressure below its critical pressure, *i.e.* the tension of its vapour at the melting-point, and that whatever the intensity of the heat applied. Now the theory of critical pressure gives us no information as to whether the ice, on non-fusion, would or would not rise above its ordinary melting-point when strongly heated, but as this result had been previously attained in the case of mercuric chloride it appeared not impossible that the ice *might* become hot.

The question as to the rise of temperature of the ice above 0°, though at first but a side issue of the investigation, became from its more especial interest the chief object of inquiry, and the experiments which have been made and those which are at present in hand relate almost solely to this point.

The great difficulty to be overcome was to maintain the pressure in the containing vessel below 4·6 mm., *i.e.* the tension of aqueous vapour at the freezing-point; for it will be easily understood that if the ice be but slightly heated the quantity of vapour given off would soon be sufficient to raise the pressure above that point. After several fruitless attempts, the following plan, involving the principle of the cryophorus, was adopted:—A strong glass bottle, such as is used for freezing water by means of Carre's pump, was fitted with a cork and glass tube C (Fig. 2) and the cork well fastened down by copper wire. A and C were then filled with wet mercury (the water facilitating the removal of the air-bubbles) and C connected with the end of the tube D E by means of the stout india-rubber tubing B, a thermometer having been previously attached by the wire *x* to the lip of the tube at B. The tube D E was about one inch diameter, and about four feet long from the bend to the end E; after connection with C it was completely filled with mercury and the whole inverted over the mercurial trough F, as shown in the figure, when the mercury fell to O, the ordinary height of the barometer. The mercury was run out of A by tilting up the bottle and inclining the tube D E. By this means a Torricellian vacuum was obtained from A to O. D was next brought to the vertical, and the bottle A placed in the trough P. A tin bottle G without a bottom was

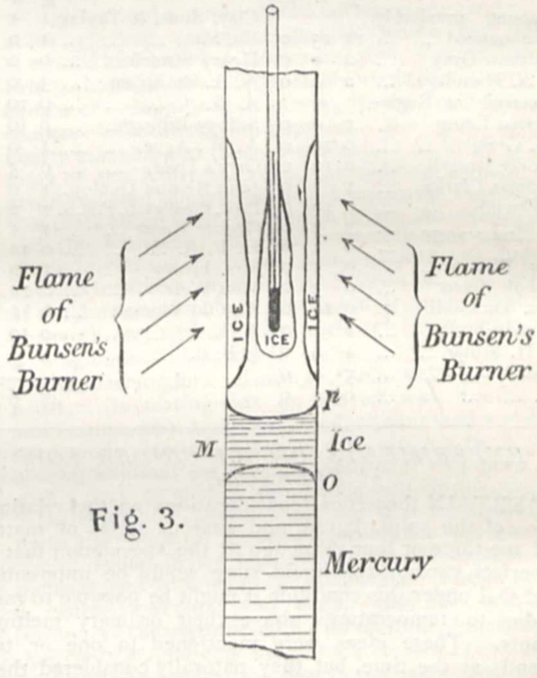


Fig. 3.

fitted with a cork, so that it might slide somewhat stiffly along D E.

To begin with, the tin bottle was placed in the position G and filled with a freezing mixture of salt and ice. Some boiled water was then passed up into the tube D E, sufficient to form a column at M about two inches deep. The thermometer H had been previously arranged so that its bulb might be one or two inches above the surface of the water M. The bottle A was next surrounded by a good quantity of freezing mixture, in order that any vapour given off from the water at M might be condensed in A as fast as it was formed, and thus the internal pressure might never be more than about 1·0 to 1·5 mm. When A had been sufficiently cooled, which required about fifteen minutes, the tin vessel G was slid down the tube D E, and its freezing mixture removed. The water at M had then solidified to a mass of ice, which on heating with the flame of a Bunsen's burner, melted either wholly or partially, and the liquid formed began at once to boil. The fusion commenced first at the bottom of the column of ice, whereas the upper part fused only with difficulty, and required rather a strong heat. The fusion in this case was probably due to the steam evolved from the lower portions of the ice column being imprisoned and unable to escape, and hence producing pressure sufficient to cause fusion.

When the greater part of the ice had been melted, the tube was tightly clasped by the hand, the heat of which was sufficient to produce a somewhat violent ebullition. The liquid in boiling splashed up the side of the tube and on to the bulb of the thermometer, where it froze into a solid mass, as represented in Fig. 3. By this means the ice was obtained in moderately thin layers. The tube at the points indicated by the arrows was then strongly heated by the flame of a Bunsen's burner with the following results:—The ice attached to the sides of the tube at first slightly fused, because the steam evolved from the surface of the ice next the glass, being imprisoned between the latter and the overlying strata of ice, could not escape, and hence produced pressure sufficient to cause fusion, but as soon as a vent-hole had been made fusion ceased, and the whole remained in the solid state, and neither the ice on the sides of the tube nor that on the bulb of the thermometer could be melted, no matter how great the heat applied, the ice merely volatilising without previous melting; thus proving that if the pressure be maintained below the critical pressure the ice cannot be melted. In different experiments the thermometer rose to temperatures considerably above the melting- and even the boiling-point of water, the highest temperature reached being 180° C., when the ice had either wholly volatilised or had become detached from the bulb of the thermometer, but in no case did the ice attached to the thermometer melt when these temperatures had been reached, as erroneously stated in some reports of my experiments. The ice attached to the thermometer did not partially fuse at the commencement of the heating, because, the heat reaching the outer surface of the ice first, evaporation could take place from a free surface and the vapour not become imprisoned, as was the case with the ice attached to the sides of the tube. These experiments were repeated many times with the same result, except in one case in which the heat applied had been very strong indeed, and the ice attached to the sides of the tube fused completely. On removing the lamp however for a few seconds the water froze again, notwithstanding that the portion of the glass in contact with it was so hot that it could not be touched without burning the hand.

The chief conditions necessary for success appear to be (1) that the condenser A (Fig. 2) is sufficiently large to *maintain* a good vacuum. For the size of apparatus given above it ought to be about 1 litre; (2) that the ice is not in too great mass, but arranged in thin layers. Nor must it expose too great a surface for evaporation, otherwise the steam is liable to be evolved more quickly than it can be condensed, and the pressure would therefore rise above the critical pressure. Further, in the case where the heat is applied to the under surface of the layers of ice, the latter must be sufficiently thin to allow of a vent-hole being formed for the escape of the steam coming from below, if not, fusion occurs. When the heat is applied to the free surface of the ice the layers may be much thicker. In order to get the temperature to rise above the ordinary melting-point of ice, it is necessary that a very strong heat be applied, otherwise all the heat is used to convert the ice into steam without raising its temperature; it must in fact be applied more quickly than it can be absorbed for changing the state of aggregation. Prof. McLeod, who has written to me to the effect that he has been unable to obtain any symptoms of hot ice, has failed I believe on account of not having complied with this condition. Dr. Lodge, in an admirable and very clear letter to NATURE (vol. xxiii. p. 264), has endeavoured to explain why "hot ice" is possible, and also points out the absolute necessity for supplying the heat more rapidly than it can be absorbed by the vapour.

Now the question arises, Does the thermometer in the above experiments indicate the real temperature of the ice? It has been said by Prof. Stokes that the ice, though attached to the thermometer, is not at the same temperature as the latter, and that the action is really as follows: The pressure is reduced till the boiling-point falls below the melting-point, and when heat is applied to the ice in contact with the glass tube a film passes into vapour, and thus prevents the ice from touching the glass except at a few isolated points. The great latent heat of evaporation prevents the ice from rising to its ordinary melting-point, and hence no fusion occurs. The ice is only heated —except at the few isolated points of contact—by radiation, and therefore comparatively slowly. A portion of the heat passes through the ice and falls on the thermometer inside, and the latter rises in temperature; this causes the formation of a film of vapour between the ice and the bulb of the thermometer, so that the latter is in contact with the ice at a few points only, and therefore hardly any heat passes by conduction to the ice.

As under the circumstances of the case this appeared the most probable explanation of the phenomena, it was of great importance to show by other and more conclusive experiments whether the ice really was hot or not. For this purpose Prof. Roscoe suggested the most decisive test which could be applied, viz., dropping the supposed hot ice into water and observing the amount of heating or cooling of the latter. Up to the present I have only had the opportunity of completing two of these calorimetrical determinations, and the second of these was merely a qualitative experiment, as the weight of ice dropped in could not be found, owing to a small quantity of the water having been jerked out of the calorimeter the moment the ice entered it. In both experiments, however, the water distinctly increased in temperature, and therefore showed that the ice must have been above 80° C. In the complete experiment the weight of ice dropped into 185 grammes of water was 1·3 grammes, and the rise in temperature 0·2° C., showing that the temperature of the ice was 122° C. From the nature of the experiment the weight of ice which could be dropped into the calorimeter was only small, and though the rise in temperature was but slight, yet if the ice had been at 0° a relatively large cooling ought to have been observed. Great care was taken to avoid any error in the determinations. The thermometer employed was graduated so as to allow of a difference of 0·05° C. being easily detected, two observers read off the temperatures independently of one another, the calorimeter was inclosed in several casings and filled with the water to be used some hours before the experiment, so that it might have the temperature of the room, whilst the time which elapsed between the readings of the thermometer would not be more than about fifteen seconds, and finally the calorimeter was not brought into position to receive the ice till the source of heat had been removed. To place the point beyond doubt, however, several additional and perfectly satisfactory calorimetrical determinations are necessary, and if possible on a larger scale. Such experiments are at present in hand. In the meantime I would make the following remarks in favour of the high temperature of the ice. If the ice is not really hot, notwithstanding that the thermometer indicates say a temperature of 120° C., how is it possible for the ice to hang on to the thermometer? For if it be separated from the bulb by a layer of steam, it cannot hang by steam, it would at once become detached from the thermometer. The thermometer was chosen so that the bulb was of the same, and in most cases of a less, diameter than the stem, so that there was nothing to prevent the ice falling away if so inclined.

In some cases I have had thin plates of ice attached by their edge at right angles to the stem of a paper scale thermometer for a considerable time without being detached or melting, notwithstanding the temperature was so high that the paper scale at that portion of the stem to which the ice clung was charred; this was the case in one of the experiments shown at the Chemical Society. In another instance I have had a thin circular piece of ice attached to the otherwise bare bulb of the thermometer, and though this piece was very thin and no more than about 2 mm. diam., it took fully one minute or more to volatilise, notwithstanding the thermometer indicated a mean temperature of about 70° C., and the surrounding tube was very hot. If the ice were not capable of being heated above its melting-point, a piece so small as that referred to would, I think, under these circumstances have fused or volatilised almost instantaneously. If the ice be really above 80° C. it ought to melt suddenly and at once on discontinuing the heat and increasing the pressure, and this I have in one or two instances found to be the case. Thus in one experiment a beautiful rod of ice nearly six inches long and about half an inch diameter was attached to a glass rod suspended in the apparatus described above and heated very strongly with a large Bunsen's burner for several minutes; the pressure was then let in, when the ice at once fell off the rod into the mercury trough below, melting completely, and as far as could be seen even before it reached the mercury. Careful observations have also been made to see whether any cavity could be detected between the ice and the hot thermometer when the latter was only partially covered with ice, and indicated a high temperature, but such could not be seen either with ice or mercuric chloride. In both cases the substance appeared to rest in actual contact with the bulb of the thermometer, in this respect differing from camphor, which does exhibit such a space. I have however never been able to get camphor above its ordinary melting-point, though by reducing the pressure below 400 mm., it solidifies while boiling, and cannot be re-melted unless the pressure be increased.

One curious point about the ice experiments is the comparative slowness with which the ice appears to evaporate, though the surrounding tube is very strongly heated.

In conclusion, I need hardly say that it is highly desirable that my results should be confirmed by other observers. THOS. CARNELLEY

TELE-PHOTOGRAPHY

WHILE experimenting with the photophone it occurred to me that the fact that the resistance of crystalline selenium varies with the intensity of the light falling upon it might be applied in the construction of an instrument for the electrical transmission of pictures of natural objects in the manner to be described in this paper.

In order to ascertain whether my ideas could be carried out in practice, I undertook a series of experiments, and these were attended with so much success that although the pictures hitherto actually transmitted are of a very rudimentary character, I think there can be little doubt that if it were worth while to go to further expense and trouble in elaborating the apparatus excellent results might be obtained.

The nature of the process may be gathered from the following account of my first experiment. To the negative (zinc) pole of a battery was connected a flat sheet of brass, and to the positive pole a piece of stout platinum wire; a galvanometer was interposed between the battery and the brass, and a set of resistance-coils between the battery and the platinum-wire (see Fig. 1, where B is the battery, R the resistance, P the wire, M the brass plate, and G the galvanometer). A sheet of paper which had been soaked in a solution of potassium iodide was laid upon the brass, and one end of the platinum wire previously ground to a blunt point was drawn over its surface. The path of the point across the paper was marked by a brown line, due, of course, to the liberation of iodine. When the resistance was made small this line was dark and heavy; when the resistance was great the line was faint and fine; and when the circuit was broken the point made no mark at all. If we drew a series of these brown lines parallel to one another, and very close together, it is evident that by regulating their intensity and introducing gaps in the proper places any design or picture might be

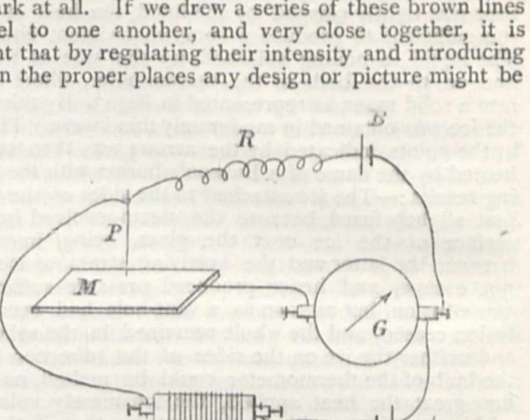


Fig.1.

represented. This is the system adopted in Bakewell's well-known copying telegraph. To ascertain if the intensity of the lines could be varied by the action of light, I used a second battery and one of my selenium cells, made as described in NATURE, vol. xxiii. p. 58. These were arranged as shown in Fig. 1, the *negative* pole of the second battery, B', being connected through the selenium cell S with the platinum wire P, and the positive pole with the galvanometer G. The platinum point being pressed firmly upon

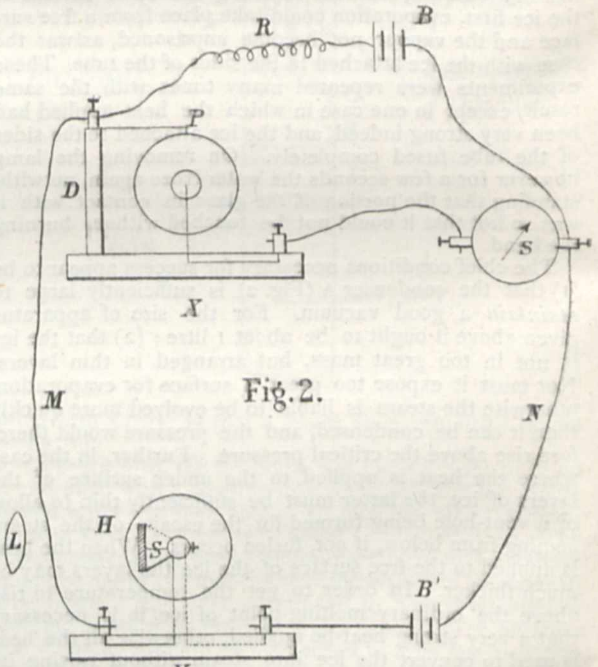


Fig.2.

the sensitised paper and the selenium exposed to a strong light, the resistance R was varied until the galvanometer needle came to rest at zero. If the two batteries were similar this would occur when the resistance of R was made about equal to that of the selenium cell in the light. The point now made no mark when drawn over the paper. The selenium cell was then darkened, and the point immediately traced a strong brown line; a feeble light was next thrown upon the selenium, and the intensity of the line became at once diminished. Lastly, a screen of black paper, having a large pin-hole in the middle, was placed at a short distance before the selenium, and the image of a gas-flame was focussed upon the outer surface of the screen, a small portion of the light passing through the pin-hole and forming a luminous disk upon the selenium. The galvanometer was again brought to zero, and, as before, the platinum point made no mark. When however the gas-flame was shaded a firm and steady line could be drawn; and when the light was interrupted by moving the fingers before the pin-hole a broken line was produced. For this last operation a very sensitive paper was required, and it was found necessary to move the platinum point slowly.

In consequence of the very satisfactory results of these preliminary experiments I made a pair of "tele-photographic" instruments, of which the receiver was slightly modified from Bakewell's form. They are of rude construction, and I shall say nothing more about them except that on January 5 they produced a "tele-photograph" of a gas-flame, which was good enough to induce me to make the more perfect apparatus now to be described.

The transmitting instrument consists of a cylindrical brass box four inches in diameter and two inches deep, mounted axially upon a brass spindle seven inches long, and insulated from it by boxwood rings. The spindle is divided in the middle, its two halves being rigidly connected together by an insulating joint of boxwood. One of the projecting ends of the spindle has a screw cut upon it of sixty-four threads to the inch; the other end is left plain. The spindle revolves, like that of a phonograph, in two brass bearings, the distance between which is equal to twice the length of the cylinder; and one of the bearings has an inside screw corresponding to that upon the spindle. At a point midway between the two ends of the cylinder a hole a quarter of an inch in diameter is drilled, and behind this hole is fixed a selenium cell, the two terminals of which are connected respectively with the two halves of the spindle. The bearings in which the spindle turns are joined by copper wires to two binding screws on the stand of the instrument. The transmitter thus described is represented in diagrammatic section at Y (Fig. 2), where H is the hole in the cylinder and S the selenium cell.

The receiving instrument, shown at X (Fig. 2) contains another cylinder similar to that of the transmitter, and mounted upon a similar spindle, which however is not divided, nor insulated from the cylinder. An upright pillar D, fixed midway between the two bearings, and slightly higher than the cylinder, carries an elastic brass arm fitted with a platinum point P, which presses normally upon the surface of the cylinder. To the brass arm a binding screw is attached, and a second binding screw in the stand is joined by a wire to one of the brass bearings.

To prepare the instruments for work they are joined up as shown in Fig. 2, two batteries, a set of resistance coils, and a galvanometer being used, in exactly the same manner as in the preliminary experiments. The cylinder of the transmitting instrument Y is brought to its middle position, and a picture not more than two inches square is focussed upon its surface by the lens L. The pictures upon which I have operated have been mostly simple geometrical designs cut out of tinfoil and projected by a magic lantern. It is convenient to cover a portion of the cylinder with white paper to receive the image. The comparatively large opening H is covered with a piece of tin-foil, in which is pricked a hole which should be only just large enough to allow the instrument to work. [I have not been able to reduce it below one-twentieth of an inch, but with a more sensitive selenium cell it might with advantage be smaller.] The hole is then brought, by turning round the cylinder, to the brightest point of the picture, and a scrap of sensitised paper, in the same condition as that to be used, being placed under the point P of the receiver, the resistance R is adjusted so as to bring the galvanometer to zero. When this is accomplished the two cylinders are screwed back as far as they will go, the cylinder of the receiver is covered with sensitised paper, and all is ready to commence operations.

The two cylinders are caused to rotate slowly and synchronously. The pin-hole at H in the course of its spiral path will cover successively every point of the picture focussed upon the cylinder, and the amount of light falling at any moment upon the selenium cell will be proportional to the illumination of that particular spot of the projected picture which for the time being is occupied by the pin-hole. During the greater part of each revolution the point P will trace a uniform brown line; but when H happens to be passing over a bright part of the picture this line is enfeebled or broken. The spiral traced by the point is so close as to produce at a little distance the appearance of a uniformly-coloured surface, and the breaks in the continuity of the line constitute a picture which, if the instrument were perfect, would be a monochromatic counterpart of that projected upon the transmitter.

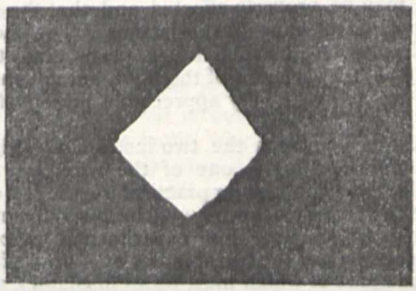


FIG. 3.—Image focussed upon Transmitter.

An example of the performance of my instrument is shown in Fig. 4, which is a very accurate representation of the manner in which a stencil of the form of Fig. 3 is reproduced when projected by a lantern upon the transmitter. I have not been able to send one of its actual productions to the engraver, for the reason that they are exceedingly evanescent. In order to render the paper sufficiently sensitive, it must be prepared with a very strong solution (equal parts of iodide and water), and when this is used the brown marks disappear completely in less than two hours after their formation. There is little doubt that a solution might be discovered which would give permanent results with equal or even greater sensitiveness, and it seems reasonable to suppose that some of the unstable compounds used in photography might be found suitable; but my efforts in this direction have not yet been successful.

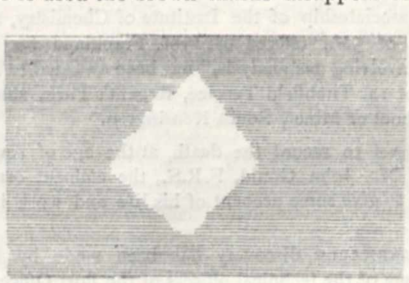


FIG. 4.—Image as reproduced by Receiver.

In case any one should wish to repeat the experiments here described a few practical hints may be useful. In order that as large a portion as possible of the current from the battery B′ (which is varied by the selenium cell) may pass through the sensitised paper, the resistance R must be high; the E M F of the battery B must therefore be great, and several cells should be used.

An electromotive force is produced by the action of the platinum point, and the metal cylinder upon the sensitised paper, and the resulting current is for many reasons very annoying. I have got rid of this by coating the surface of the cylinder with platinum foil.

Stains are apt to appear upon the under-surface of the paper, which sometimes penetrate through and spoil the picture. They may be prevented by washing the surface of the cylinder occasionally with a solution of ammonia.

Slow rotation is essential in order both that the decomposition may be properly effected and that the selenium may have time to change its resistance. The photophone shows that *some* alteration takes place almost instantaneously with a variation of the light, but for the greater part of the change a very appreciable period of time is required.

The distance between the two instruments might be a hundred miles or more, one of the wires, M, N, being replaced by the earth, and for practical use the two cylinders would be driven by clockwork, synchronised by an electromagnetic arrangement. For experimental purposes it is sufficient to connect the two spindles by a kind of Hooke's joint (some part of which must be an insulator), and drive one of them with a winch-handle.

The instrument might be greatly improved by the use of two, four, or six similar selenium cells and a corresponding number of points. If two such cells were used the transmitting cylinder would have two holes, diametrically opposite to each other, with a selenium cell behind each. A second point would press upon the under surface of the receiving cylinder, and be so adjusted that the lines traced by it would come midway between those traced by the upper point. Four or six selenium cells could be similarly used. The adjacent lines of the picture might thus be made absolutely to touch each other, and moreover the screw upon the spindles might be coarser, which for obvious reasons would be advantageous. A self-acting switch or commutator in each instrument would render additional line-wires unnecessary.

SHELFORD BIDWELL

NOTES

THE Murchison medal of the Geological Society has this year been awarded to Prof. Geikie.

THE Associateship of the Institute of Chemistry, along with the prize of 50*l.*, offered by Prof. Frankland for the "best research involving gas analysis," has been awarded to Mr. Frank Hatton, of 14, Titchfield Terrace, Regent's Park, student in the Royal School of Mines, South Kensington.

WE regret to record the death, at the age of seventy-seven years, of Mr. John Gould, F.R.S., the eminent ornithologist. We hope to give some account of his life and work in our next number.

A REMARKABLE discovery has been made by Mr. Alex. Adams, one of the technical officers of the Post Office Telegraph Department. It is the existence of electric tides in telegraph circuits. By long-continued and careful observations he has determined distinct variations of strength in those earth currents, which are invariably present on all telegraphic wires, following the different diurnal positions of the moon with respect to the earth. He will read a paper on the subject at the meeting of the Society of Telegraph Engineers to-night.

MR. JOSEPH THOMSON has, we understand, received the offer of an advantageous post under the Sultan of Zanzibar, which no doubt he is likely to accept. Mr. Thomson's work will be mainly that of geological surveying in the region of the Rovuma River, and the Sultan has offered him every facility for carrying on the work. The Sultan deserves every credit for showing such enterprise, and we have no doubt that Mr. Thomson will be able to do work of great scientific value.

AT the Royal College of Surgeons Prof. W. K. Parker, F.R.S., will give nine lectures on the Structure of the Skeleton in the Sauropsida, on Mondays, Wednesdays, and Fridays, February 11, 16, 18, 21, and 23, at 4 p.m. Prof. W. H. Flower, LL.D., F.R.S., will give nine lectures on the Anatomy, Physiology, and Zoology of the Cetacea, on Mondays, Wednesdays, and Fridays, February 28, March 2, 4, 7, 9, 11, 14, 16, and 18, at the same hour.

WE are glad to learn that the new 23-inch object-glass of Prof. C. A. Young of Princeton, N.J., is completed. Prof. Young has tested it at Cambridge, Mass., and finds it very fine; he hopes by and by to do some good stellar spectroscopic work with it. The mounting is well under way, and it is expected that the instrument will be in place next autumn.

MR. LAMONT YOUNG, the Government geologist of New South Wales, has suddenly and mysteriously disappeared, and foul play is suspected. Mr. Young arrived safely at Bermagui, 180 miles south of Sydney, and at once set out to cross the bay in a boat. No news of him came in, and two days later his boat was found jammed among the rocks of the coast, ten miles north of the point from which he had started. It was at first, and naturally, supposed that Mr. Young and his company had been drowned, and that his boat had drifted on shore. A closer examination proved that the boat had been drawn carefully up on the coast, and that the party had dined after landing. Next some bullet holes were found in the boat, and this suggested the idea that the explorers had been attacked and murdered. But not a single mark of blood or additional trace of any violent assault could be discovered. The party were five in number, and the coast has been examined for traces or tidings of them in vain. An official of the Mines Department has been assisted by detectives and by the boasted "black trackers," natives whose acuteness is seldom at fault in a case of this sort.

PROF. MCK. HUGHES writes on Jannuary 27, suggesting the following scientific uses of the late severe weather:—When this frost breaks up and the frozen snow and ice begin to travel along our rivers to the sea there will be an opportunity of making observations upon several points upon which accurate information will be of use in seeking an explanation of some of the glacial and post-glacial phenomena of the British Isles, *e.g.* (1) Dimensions of the ice floes; (2) whether they consist chiefly of frozen snow or solid ice, *i.e.* an approximate estimate of their specific gravity; (3) amount of material carried by them and dimensions of larger boulders; (4) whether any of these were dropped on to the floe from cliffs of glacial drift so as to give scratched stones and *remanie* drift in modern mud; (5) how far out to sea such floes have been traced with or without earth and stones; (6) salinity of the water where the observations were made; (7) transport of shore shells, &c., by ice; (8) crumpling of mud by impinging ice; (9) grinding of ice along bridge piers, and many similar observations which it will be useful to record.

THE great annual *soirée* at the Observatory of Paris has been a great success. Almost all the Cabinet ministers and M. Gambetta were present. A plan was exhibited in the Astronomical Museum showing the present state of the Observatory, and what it will be when all the works for which credits have been voted shall be completed. A ball took place after a series of lectures and projections given in the *grande galerie*. One of the lecturers, M. Bertus, exhibited magic mirrors, and reminded those present that in 1844 M. Mouchez, then a junior officer in the French naval service, brought home with him one of these mirrors from Japan, which was presented to the Academy of Sciences by Arago. The *Comptes rendus* states that Arago was asked to inquire into the properties of this curious phenomenon, but it does not appear that he made any effort to comply with the request of the Academy.

AFTER a series of experiments which have proved successful, the Administration of French lighthouses has given an order to M. de Meritens to build six magneto-electric machines for the three first lighthouses which are to be illuminated by electricity.

THE Chemical Section of the Russian Physico-Chemical Society has, on the proposal and at the expense of Mr. V. J. Ragosine, established a competition for a prize of 750 metallic roubles (3000 francs) for the invention of a lamp intended to burn the heavy oils of petroleum (naphtha), *i.e.* the parts of the raw petroleum which distil after the kerosene or ordinary petroleum (density from 0·79 to 0·83 at 20° C.); as also astral oil (density 0·83 to 0·85 at 20° C.), but before the oils intended for greasing purposes (density about 0·88), *i.e.* oils whose density is from 0·85 to 0·88 at 20° C. The lamps ought (1) to be as simple as possible in construction, so that they may be easily manufactured and manipulated; (2) only glasses existing already in the retail trade to be used, if they are used at all; (3) to burn, without giving either soot or smell, the heavy oils whose density is at least between 0·865 and 0·875. The lamps must be sent in by January 12, 1882, and three specimens of each should be sent, accompanied by a detailed description in Russian, French, German, or English. There is no restriction as to nationality. Further information may be obtained from the Secretary of the Society, St. Petersburg.

WE would call the attention of our readers to a very valuable and ingenious instrument which has been recently introduced by Messrs. Francis and Co., the Telegraph Engineers, Hatton Garden, London, for the purpose of receiving the "Greenwich Time Signal" at the various telegraph stations and offices of private firms who may be in communication with the Postal Telegraph Service. Hitherto the passage of the time-signal current at 10 a.m. along the wires gives no other indication of its presence than a deflection of the needle of ordinary instruments, and a corresponding movement of the armature of the Morse Ink-Writer and Sounder, so that unless a sharp look-out be kept with the eye constantly directed to the instrument, the actual time of signal may be lost, perhaps also again to be lost on the following day through similar accident. By the new instrument, however, the instant the current is sent the needle on its dial is deflected, and simultaneously a bell rings and continues to ring so long as the current is passing. The index-needle, or in other words the needle of the galvanometer, which is the principal feature of the invention, when deflected, presses against a small spiral spring surrounding the stops or ivory pins on the dial plate, and by this contact the galvanometer forms itself into a "relay" and brings a local battery in circuit with the bell, which is contained in the same instrument, so that when the first part of the time-signal is sent the needle is deflected, and at the same moment the bell rings; thus attention to the time is at once arrested. It should be mentioned that the resistance to the line, although low, is intended to be inserted only during the transmission of the time-signal, as by means of what is generally termed a "switch" the instrument is put on and off the circuit at will, and employed only during the time set apart for the transmission of the "Greenwich Time Signal." However feeble the current may be, the galvanometer is so sensitive that a deflection of its needle is absolutely certain, whilst the bell cannot fail to answer to the power of its local battery. We are informed that not only is Messrs. Francis and Co.'s new instrument capable of doing what we have already stated, but it may be made available for communication from different parts of the building, an advantage which is certain to be recognised and approved by many conducting large business establishments, where the saving of time in conveying messages and giving orders is a matter which is not unfrequently of great importance.

A. P. S. WRITES:—During the late severe frost we had a number of bottles broken in our laboratory by the freezing of their contents, and it is curious to observe what salts tend to prevent such an occurrence. Out of thirty sets of reagents the following were destroyed:—27 ammonium oscalate, 7 calcium sulphate, 8 potassium ferrocyanide, 1 lead acetate. It is remarkable that not one bottle of lime-water was frozen. That calcium sulphate, which only contains $\frac{1}{500}$th of solid, should freeze, is not astonishing; but the ammonium oscalate bore away the palm with ease, although the amount dissolved was considerable. A single bottle of saturated solution of alum was broken, also one of mercuric chloride. A curious thing happened to one bottle, which shows, I think, that ice does not expand suddenly when it freezes. I unstoppered a bottle of Am_2O that was still liquid, when the contents immediately solidified in my hand, without bursting the bottle. The next day I found the ice had protruded $3\frac{1}{2}$ inches from the neck of the bottle, carrying the stopper at its extremity.

THOSE who wish to see women have every fair play in the struggle for existence may be interested to know that at 399, Edgeware Road, Mme. Lina from Geneva is prepared to do good work as a practical watchmaker and jeweller.

A NUMBER of holes of the same description as those which have been observed at Blackheath have been opened in several parts of Paris. These enigmatical holes are several yards wide, long, and deep. Men of science are trying to solve the mystery of their formation.

A VERY satisfactory report was given at the recent annual meeting of the Birmingham Natural History Society, which now has apartments in Mason's College. The number of members is 382.

MR. J. B. JORDAN has issued a little pamphlet giving an account of his glycerine barometer, with plate and tables of correction for temperature. Stanford is the publisher.

UNDER the title of "All about Cardamoms, Botanical Descriptions, Commercial Uses, and Modes of Cultivation," a pamphlet of forty closely-printed pages has recently been issued in Colombo from the office of the *Ceylon Observer*. In this useful little pamphlet nothing new or original is professed to be given, it is simply a compilation of all matter bearing on the subject collected from all available sources, each article being printed in its entirety and its source acknowledged. Thus we find the article on Cardamoms from the latest edition of the *Encyclopædia Britannica*, Flückiger and Hanbury's *Pharmacographia*, Bentley and Trimen's *Medicinal Plants*, and many others. In this arrangement there is of course much repetition of the same matter, but the idea is good as bringing together all that has been published on a given subject which is frequently scattered through many, and often inaccessible publications.

THE works for the Paris Exhibition of Electricity will soon begin. A viaduct will be built for the English electrical railway by Siemens, which will convey visitors from the Place de la Concorde to the Palais de l'Industrie. The internal arrangements will only be made at the end of the Art Exhibition, which will take place from May to July. The French exhibitors of the electric light have come to an agreement in order to combine for the illumination of the nave and other parts. They are trying to obtain from the High Commission an indemnity for their working expenses. It is desirable that the English Government appoint without delay an agent on behalf of the intended English exhibitors, who may be numerous, even in the light department.

ANOTHER slight shock of earthquake was felt at Berne on the night of the 1st inst. Fresh earthquake shocks are reported from Agram, where shocks were observed on January 25 at 1h. 15m. (in the morning) 11h. a.m.; on the night of January 26 at 11h. 28m.; in the morning of January 31 at 3h.; on January 3 at 3h. (in the morning), 1h. 15m., and 4h. 13m. p.m. In the night of January 27-28 shocks were felt at St. Ivan, Zelina (Hungary) at 12h. 52m., 3h. 9m., 4h. 32m. On January 28 two shocks were felt at Gurkfeld (Carinthia) and neighbourhood at 8h. 50m. p.m., direction north-west to south-east. Earthquakes were also noticed on January 25 at Venice, Bologna, and Padua. In the night of January 3-4 shocks were observed in the regions of the Carinthian Alps, in Klagenfurt, at 2h. 22m. 25s., direction east to west, duration 5-6s.; in Trieste at 2h. 24m., direction north-east to south-west, duration about 4s.; at the same time shocks were felt in Laibach, in Gurkfeld, and in Czegled (Hungary).

AN examination has taken place at Brussels of the railway *employés*, in order to test their eyes. More than one-twentieth of them have been found defective, and consequently will be discharged as being unable to fulfil their functions with a sufficient security for travellers.

THE AURORA AND ELECTRIC STORM OF JANUARY 31

WE have received the following further communications on the recent brilliant display of aurora:—

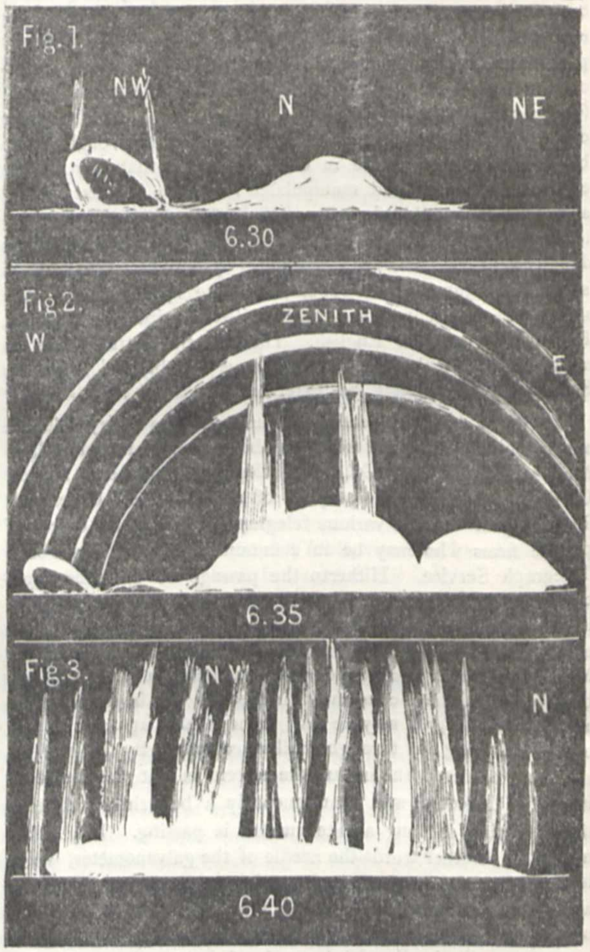
THE beautiful display of aurora on the evening of Thursday was accompanied by the usual earth-current disturbances. They were evident over the whole of the United Kingdom. Telegraphic lines were stopped, railway block-signals were disturbed, and all the usual accompaniments of these curious storms were observed.

The electric storm commenced about 3 p.m., it reached a maximum at 6.40 p.m., and disappeared about 9 p.m. It was renewed about 11 p.m., and disappeared again about 1 a.m. on the next morning. The currents attained an intensity that I have never before observed. At Llanfair in Anglesey they measured 41·4 millivebers. At Haverfordwest 30 millivebers; at Bristol 17·32 millivebers; in the Central Station, London, 11 millivebers; at Edinburgh 8 millivebers. Now as working-currents vary from 5 to 10 millivebers, it is clear that these uninvited wanderers must play sad havoc with the working telegraphs. In some instances they were strong enough to ring the bells used on railways. They are eliminated, where this can be done, by joining two wires in *metallic circuit*, and so excluding the earth. They were characterised by the usual reversals, the direction of the current changing slowly. The changes in direction and variation in strength were always observed on the southern lines first. The line of maximum force commenced south-east to north-west, then passed south to north, and ended south-west to north-east.

It is unfortunate that on such occasions the whole energies of the technical staff are taken up in maintaining communication, and that no time or means can be found to obtain accurate measurements. The results however, such as they were, fully confirm my view that these storms are due to a violent disturbance of the distribution of electric potential on the earth's surface arising from violent changes in the electrification of the sun. There was a violent disturbance in the sun's envelope on that day, as I learn from Mr. Norman Lockyer, and I am looking with interest to some particulars of it.

February 5 W. H. PREECE

ON yesterday evening, January 31, a most brilliant display of the aurora was seen here. It was by far the finest I have seen, and others have expressed the same opinion. At 6.25 p.m. I saw a considerable illumination on the northern horizon, and an outlying bright patch on the north-west having somewhat the appearance of the zodiacal light, as shown in the sketch, Fig. 1. This outlying patch was distinctly in motion along the horizon towards the west; streamers from the horizon then shot up, and there appeared several arches of light apparently about the width of an ordinary rainbow, passing from the north-west to north-east points on the horizon; these arches gradually approached the zenith, and the southernmost from the east and west points of the horizon at last passed through it. Some of these arches are shown in Fig. 2, but there were more visible. In a few minutes, about 6.40, the arches faded, and there appeared, rather west of north, a mass of bright green light; then the



streamers from the north lengthened out, as shown in Fig. 3, converging on the Pleiades, as near as I could judge; waves of red light commenced to pass upwards along them, and large sheets of light appeared to pass rapidly over the sky. The streamers gradually died away, leaving flashing lights near the horizon, which in their turn left a slight light over the northern horizon, which gradually faded away. Mr. Percy Smith made the sketches, but owing to the rapid changes their accuracy is only general. Both he and I saw only one line in the spectrum in the usual place. GEORGE M. SEABROKE

Temple Observatory, Rugby, February 1

THE aurora observed during the evening of January 31 was accompanied by a magnetic perturbation, and although it was on a much smaller scale than that registered on August 12 to 14 last, a brief account of it may possess some slight interest to your readers.

The magnets of all three instruments at Kew, the declination, bifilar, and balance magnetomers, began to be disturbed to a somewhat larger extent than usual about noon on the 31st, the general tendency of the movement being an increase of westerly declination and of vertical force, whilst the horizontal force slightly diminished. Quicker movements of the needles commenced at 3.40 p.m. Greenwich mean time, and from that hour until 8.33 p.m. oscillations followed each other at short intervals, although the magnets at no time appeared to be in the state of rapid vibration they were in during the afternoon of August 12.

The principal deviations registered were as follows:—A large westerly deflection of the needle was recorded at 6.2 p.m., but the greatest excursion in that direction took place at 6.47 p.m.; at 7.12 p.m. there was also another considerable westerly movement, followed by an easterly, which reached its maximum at 7.48 p.m. At 8.18 p.m. it deviated again to the west, after which it returned to its approximately normal position at 8.33 p.m. An isolated deflection to the west at about 0.25 a.m. of February 1 wound up the storm.

As regards force, the greatest changes of horizontal force occurred at about 6 p.m., but they were not large in extent. The vertical force curve moved in the extent of an augmented force beyond the limits of registration of the instrument between 4.20 and 5.32 p.m., and again between 6.2 and 6.45 p.m. The greatest movement in the direction of diminished force was at 8.12 p.m.

The self-registering Thomson electrometer was not apparently affected by the aurora; the tension of atmospheric electricity being somewhat high positive at 9 a.m., fell to a low positive tension at 2 p.m., from which it rose gradually, although somewhat irregularly, until 8 p.m.; from that hour until 9 p.m. it was more disturbed; it then became more strongly positive, and remained so until the next day.

This want of accordance between the electrograph and magnetographs was also well marked during the August aurora, and would appear to prove that the electrical disturbances in the upper aërial strata during aurora do not cause changes of tension in the lower at all commensurable with those ordinarily produced by wind, snow, or rain. G. M. WHIPPLE

Kew Observatory, February 2

THE commencement of the aurora consisted in the sudden lighting up of various portions of the sky by patches of white cloud, the northern horizon remaining constantly bright, and sending forth vertical streamers. The general appearance of the heavens was that of a smooth lake ruffled here and there every other second by fitful gusts of wind.

At 6.45 p.m. no ordinary clouds could be seen, but the flashes of white light were incessant, and varied continually in position. The light was strongest towards the north-east horizon, but the whole of the north was well lit up from north-east to west.

At 6.50 the streamers from the horizon increased in length and enveloped Polaris.

At 6.55 the number of the streamers increased, and springing from the whole northern horizon, traversed an imperfect arch of white light which passed between ϵ and ζ Ursæ, and just below β and γ Ursæ Minoris.

At 6.57 streamers 10° west of north passed from the horizon through the zenith, and the display was becoming very brilliant when I was obliged to enter the observatory for a few minutes to observe an eclipse of Jupiter's second satellite.

On returning to the garden at 7h. 5m. nothing remained of the aurora except patches of white light in different parts of the heavens, and a strong glow in the north. Using a hand spectroscope I could see the green auroral line very strongly marked in every part of the sky, but no other line was visible.

There was no change in the phenomenon until 7h. 45m., when a most brilliant cone of light of a reddish hue darted from between α and γ Aquarii, and developed almost immediately into a number of streamers which stretched out towards the Pleiades, this cluster being then some 30° from the zenith towards the west of south. Other streamers also appeared near the horizon from the west point to east of north.

A lull succeeded this display, followed at 8h. 15m. by a grand outburst of red streamers from Aquarius and also from near Orion, both converging towards the Pleiades, those from Aquarius being the brightest. These were visible for at least six seconds along with other rays in the north-west.

Cloud and haze were then collecting fast, and seven-tenths of the sky was already obscured. During this aurora the three self-recording magnets were very much disturbed, their movements being all rapid and extended. During the whole of the morning of the 31st the declination was very irregular, but it was only about noon that the storm began in earnest. From 3.30 until 9 p.m. the declination magnet was oscillating incessantly in long vibrations, several of fully a degree in extent; and between 7h. 53m. and 8 p.m. the western bearing increased by 1° 37′ 24″. Many other movements were nearly equally rapid, but not so extensive. The movements of the horizontal force magnet were irregular from noon till after midnight, and they were very much exaggerated between 3h. 45m. p.m. and 8.15; the most rapid change was the remarkable diminution of 2·1 in. in the ordinate between 6.14 and 6.20 p.m.; this was preceded by a very quick rise, and followed by another nearly equally sharp.

The vertical force magnet was most irregular between 2 p.m. on the 31st and 1 a.m. on the morning of the 1st. The extreme maximum was attained at 4h. 20m., and the two principal minima at 8h. 12m. p.m., and at 12m. after midnight. The movement was most rapid at 8 p.m., and this principal disturbance on all the curves coincided with the grand outbursts of red streamers which converged towards a point some 30° south of the zenith.

Stonyhurst Observatory, February S. J. PERRY

MR. E. DOWLEN has been again good enough to furnish me with notes on the aurora (of Monday last), as seen from Medway, Poynton, Cheshire.

He first saw it at 6.30, it having been previously seen at 6 as a single shaft of white light. At 6.30 it consisted of quickly-darting rays and waving curtains of light, filling almost the whole sky. The horizon from extreme east to west was glowing, and from all this region streamers and waves of light shot upwards, meeting at or near the Pleiades, the rays often passing into Orion. About 7.0 the light in the north-west extending from Venus to some distance north of the moon was rose-coloured; the other parts were white. Now for a few minutes the display almost ceased and clouds began to come up in the north-east; but the glow increased in brilliancy in the north and north-west, forming a concave mass of light, almost an inverted arch; and from this sprang a broad band of streamers filling up all the northern region, and reaching almost to the zenith. This died down, and was succeeded by a similar display having a drifting movement westward, and a rose tint in the upper portion, which extended throughout it as it went westward. This display also died out, and was followed by another similar band more to the west, white in colour.

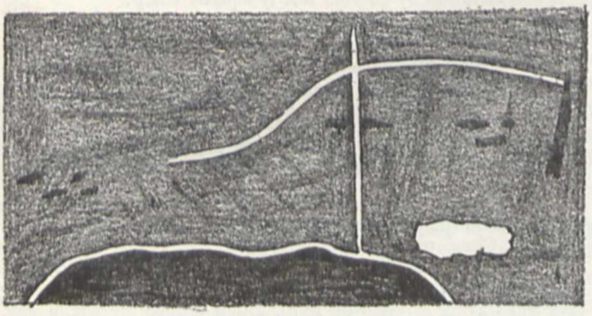
At 7.30 this was gone, and the whole aurora gradually grew smaller, the glow still remaining and giving feeble spurts until 8.30. About 9.30 the clouds were all gone, and Mr. Dowlen saw that a long low arch of inconsiderable width but tolerable brightness had been formed; the crown of the arch being just above Arided in Cygnus. No streamers came from this arch, but there seemed to be a fringe of glow to it. At midnight it faded away. During the whole time there was no wind; and although there was a ground-frost, the temperature up to 9 o'clock, by thermometer without frame suspended at the end of a bough four feet from the ground, was 34°. At 5 p.m. there had been a sharp shower of rain. Mr. Dowlen had no access to a spectroscope at the beginning. Later on he saw only the citron line.

At Guildown the display was not seen, and some fog prevailed. On the Monday morning at 8 a.m. a thermometer read 35° in a Stevenson cage four feet from the ground, while ice one-eighth of an inch thick lay on the garden paths.

The shower of rain is interesting in connection with the suspicion that the aurora is generally formed in a mist or vapour region. I have seen several eye-descriptions of this aurora, but no spectroscopic ones up to the present date.

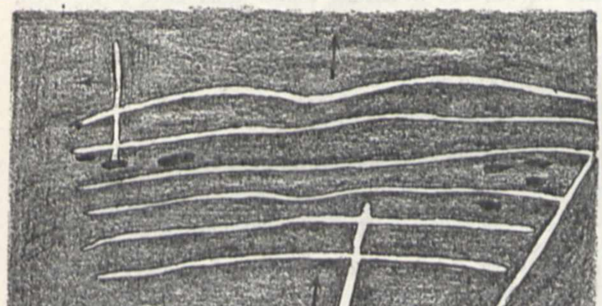
Guildown, February 4 J. RAND CAPRON

THIS was a display of aurora borealis having appearances quite new to me. There was a faint auroral glare at 6 p.m.; at 6h. 15m. a confused but brilliant mass of light was situated west of Ursa Major, which moved quickly horizontally towards the west, there was also another mass of pink light in west, streamers shot up to the altitude of Cassiopeia. 6h. 24m. the first-mentioned mass of light was now mostly to the west of Cassiopeia. There was also another mass of light low down in north by east. Stars shone brightly through the aurora, but without scintillation, and somewhat orange-coloured. 6h. 25m. brilliant streamers in north-east, a wavy arch stretched from west-north-west to east, its east end terminating in a black, almost perpendicular mass. There were also smaller black patches but quite distinct in character to the first-mentioned one. The low dark segment had also a wavy edge, and there was a patch of strong light above the north-east horizon. One very long streamer nearly due north rose from the dark segment and extended beyond the upper arch. At 6h. 27m. there were six well-formed arches, the upper one being that visible at 6h. 25m. They were all bent more or less, pointed towards east and west, and moved in a north current, *i.e.* rising perpendicularly; and increasing rapidly in speed as they neared the zenith, and fading away on reaching a point some 20° S. (of the zenith). Their brilliancy was great, and a strong orange and red glare coloured the walls of the Observatory. A large intense patch of light was situated in north-east, from which streamers rose to the zenith; at 6h. 28m. the whole of the



Seen at 6h. 25m. p.m.

arches (except that of the dark segment) had disappeared, occasional streamers and a strong glare continued, which at 7h. 9m. was unusually brilliant, and red and orange in colour. There were confused patchy lights, but no streamers. At 7h. 10m. these patches, together with their veil-like flames, passed to the south of the zenith, and formed a cupola which did not last a second; 7h. 16m. the phenomenon was now all confusion, and so thin that there appeared to be no brilliancy, yet the time by a watch was readily seen; 7h. 26m. very similar; 7h. 39m. still very bright, but settling down to the horizon; 8h. 34m. the whole northern heavens up to the zenith was a glare of thin sheets of aurora; 9h. 0m. a glare alone remained.



Seen at 6h. 27m. p.m.

The streamers west of north all moved westerly, but not those in north-east, and this also applies to the patches of light, whilst the arches moved in a north current.

The appearance was that of a luminous mist, and from the great speed when near the zenith was evidently much lower than usual. Flashes of thin light were very constant, appearing and disappearing incessantly.

The patches of light were a close copy of the aurora of October 1848, *i.e.* thirty-two years three and a half months ago, and if this be the third return of that appearance the period would be eleven years and thirty-five days. E. J. LOWE

Highfield House, Nottingham

AURORIC lights have been faint and scarce of late. There were some good ones a little before midnight on January 16, while on the 31st there was a most brilliant display, and of the observations made the following is an epitome:—

At 6 o'clock the sky was cloud-masked, with faint traces of orange-red columns between the north-west and north-east; coming up to 6.30 the clouds cleared away, and about 7 there were brilliant white lights to the north-west, making the night as light as day. At 7.40 an oblique band of silver light extended from the west to the upper star of the Plough, and from it shot up horns towards the zenith, while the southern portion of the sky was a fiery red, with columns rising in places. At 7.48 there were remarkable silver lights in two oblique systems, one set rising between the south and west and going northward, and the other between the west and north-east and going southward. The first were steady and fairly constant, the second a series of rapid successive flashes, streaks, and glows. The systems of lights formed beautiful crosses at the zenith. The flush lights passed round to the east, and all disappeared.

At 7.53 fans of pencils of silver lights came up between the north-north west and north-north-east, with at times disks at the north. Red, orange, and purple lights were rising at the same time in the southern portion of the heavens. Subsequently the northward heavens usually were light and bright, with flushes of light at intervals, while the southern portion was dark, with columns of orange and reddish lights; some displays to the northward being very bright, of green, silver, and pink colours.

At 9.30 there appeared an elliptic arch of silver light from the west-north-west by north to the north-east, which continued to 10.50, sometimes being much more distinct than at other times, its length being considerably contracted before it finally disappeared; the arch in general was white, but sometimes a silvery green. The under-sky was different shades of violet, the oversky pale bright yellowish green. At times this arch was very similar to the pictures of auroræ given in books on Arctic travels.

At 10.45, a little before the arch finally disappeared, a brilliant display of silver pencils instantaneously sprang up between west-north-west and north-north-east; those at the north-north-west by north being perpendicular, while those on either side sloped slightly; this lasted for nearly ten minutes; glow and pencils of pink lights were also coming up during the time, especially at the north-west. Afterwards white lights appeared at times in the north heavens, and orange and purple in the south, up to 11.45, when there was another brilliant display of silver lights. It began by pencils shooting up perpendicularly between the west to the north-east, which changed into a fan, the perpendicular lights only rising at the north-north-west; this lasted five minutes, the largest, most constant, and brilliant lights coming up at the north-west. At 12 there was the best display I had seen during the night. It consisted of pencils between the north-west and north north-east, some perpendicular, others fan-shaped; some steady, others in flashes; while at the same time there was a continuous upward stream of waves or lines of vivid white bright lights. The latter were so peculiar that I am at a loss to describe them sufficiently; while this display was in progress at the north, to the south-east, bounded by hard lines at the east and south, there were red lights rising. The white lights in about five minutes disappeared as suddenly as they came, but at 12.10 horizontal wavy white lights shot up in rapid succession between the north-west and north-north-east by north, followed by radiating pencils of white lights, all disappearing at 12.15. No very remarkable lights appeared afterwards up to 1 o'clock, when the last observations were made.

There were severe frosts on the nights of the 30 and 31st, while during the daytime on the 31st and February 1 there were remarkably hot suns. G. HENRY KINAHAN

Ovoca, co. Wicklow

CLEAR starry night; slight frost. Shortly before 12 midnight beautiful auroras, when first observed, consisted of cloud-like white masses extending from about midway between Orion's belt and the Pleiades northward and eastward at about 30° above horizon, a little west of north, where the crown of the arc it formed lay below this arch of cloud-like masses (which at intervals shot up individually to the zenith); there were a number of pencils or rays of white light of varying length and intensity, some bright and sharply defined, and as a rule narrow and extending up to or beyond the cloud-like masses; others short, and some indistinct, all continually altering their position, boundaries, and intensities. While they were being watched, the cloud-like masses kept shooting upwards or horizontally like brush discharges.

Narrow sharply-defined rays were remarked to shoot up on several occasions from the horizon; these gradually widened out, losing their sharp boundaries and becoming less distinct, some times behaving like the cloud-like masses or becoming intensified by a "brush discharge" occurring across them; at others they faded gradually away.

At about 12.30 they had all disappeared, but a bright glow to the north horizon and faint glows at intervals over the sky between north and west.

It was not observed how far the discharges extended eastward.

24, Waterloo Road, Dublin GERARD A. KINAHAN

THE following details of this evening's remarkable aurora may be of interest to your readers. At 6.45 p.m., while the new moon was setting, there was an appearance of a belt of luminous white cloud reaching along the northern and north-western horizon, giving indications of a tendency to divide into two separate parts, of which the western one had its upper surface parallel with the horizon, but that to the north was arched. From both parts rays and vertical bands of white light began to shoot upwards, reaching nearly to the zenith, and becoming more and more distinct, especially in the north (as opposed to the north-west), and one long feathery streamer was very conspicuous, and reached in a slanting direction from a point on the horizon immediately under the Pole Star up to Capella.

In the meantime the cloud-like appearance to the north-west had spread upwards over the heavens and assumed a dark ruddy colour, which gradually became brighter and more rosy, until it exactly resembled the light of dawn or sunset, which is sometimes reflected on the opposite side of the sky to the rising or setting sun. At this time the northern heavens became suffused with white light extending over the space where the bands and rays had been appearing, which throbbed repeatedly and vividly like the electric discharge in a vacuum tube, continuing some minutes. This gradually faded away, and the pink light to the north-west also disappeared by degrees, so that within twenty or twenty-five minutes from the commencement (say at about five or ten minutes past seven) there was little to be seen but a hardly noticeable light along the north horizon. F. HORNER

Mells Park, Somerset, January 31

IT may interest your readers to know that the aurora of January 31 was distinctly seen by me here at about seven o'clock on that evening. Such a sight is so uncommon in this part of London that I had some difficulty in convincing my friends that it was the *aurora*. As I walked down the Wickham Road, Brockley, towards Greenwich, broad bands of light shot up from the northern regions and reached nearly to the zenith. Descending amongst the fog and smoke that overhung the lower parts of New Cross, the light gradually faded, and I saw no more of it.

W. J. SPRATLING

Aske's Hatcham Schools, Hatcham, February 4

THE aurora of January 31 was well seen here. It was at its brightest at 6.40 p.m. It extended from about north-west to nearly east. In the north-west and to the north of the crescent moon there was a large irregularly shaped patch of *greenish* phosphorescent light. Then round from it towards the north rose *crimson* streamers towards the zenith. The streamers continued round to the east nearly, still ascending zenithward, but white rather than crimson, between north-east and east. The streamers changed every instant, but the large greenish patch of light in the north-west was steady for some minutes.

It would be interesting to know whether observers in America noticed any unusual solar activity at the same absolute time as the aurora was occurring here, and also whether the magnetic elements in both hemispheres (north and south) showed disturbances in sympathy. D. TRAILL

Raleigh Lodge, Exmouth, February 4

GAS AND ELECTRICITY AS HEATING AGENTS[1]

II.

GAS engineers have been under the impression until now that a supply of cold air was favourable to the production of a brilliant flame. This is a misconception, which was very general also as regards the combustion of solid fuel in furnaces, until it was disproved by Stirling, by Neilson, and by the introduction of the Regenerative Gas Furnace. The "duplex burner" owes its brilliancy to the heating effect of the one burner upon the other; and my brother, Mr. Frederick Siemens, has more recently constructed a burner in which the flame of the gas is reversed in its action in order to heat in its descent the ascending current of flame-supporting air.

By the application of the principle of conduction before described, I obtain the hot-air current in a most simple manner without interfering with the free action of the flame. The construction of my burner will be seen from the diagram. A is an ordinary Argand burner, taking its supply of gas through the enlarged vertical copper tube B. This copper pipe terminates in a rod C of highly conductive copper, which passes upward through the burner, and carries at its top a ball of porcelain or other refractory material. The rod is coated with platinum or nickel to prevent oxidation when heated (almost to redness) by the heat of the flame. The tube B is armed with radial plates of copper presenting a considerable aggregate surface, and abutting externally against a covering of asbestos or other non-conductive material.

The waste heat of the flame, or that portion of the heat produced in combustion which is not utilised in luminous rays, serves to heat the ball of refractory material D and the conductive rod C. The heat is thus transferred by conduction to the tube B, with its laminar radii, between the extensive surfaces of which currents of air are free to ascend toward the Argand burner. The air is thus heated to from 700° to 800° F. before meeting the gas, and the ultimate temperature of the flame is increased to at least the same amount, causing a larger proportion of the heat developed in combustion to reach the point of luminous radiation.

But not only the quantity of light but its quality is improved by the higher temperature obtained.

It may appear surprising, but it is a fact susceptible of accurate proof, that the light obtained in consumption of a given amount of gas may thus be increased by some 40 per cent., and that in this large proportion the deleterious influences connected with gas lighting may be diminished. Gas will thus be better able to hold its position against its more brilliant rival the electric light, except for such large applications as the lighting of public halls and places, of harbours, railway stations, warehouses, &c, for which it is pre-eminently suited. Add to these improved applications of gas the ever-increasing ones for heating purposes, and I have only to express regret that I am not a gas shareholder.

If gas is to be largely employed, however, for heating purposes, it will have to come down in price; and considering that heating gas need not be highly purified, or possessed of high illuminating power, the time will come, I believe, when we shall have two services, one for illuminating, and the other for heating gas.

In many towns two systems of gas mains already exist; and it would only be necessary to appropriate the one for illuminating and the other for heating gas. The ordinary retorts could be used for the production of both descriptions of gas, it being well known that even ordinary coal will give up gases of high illuminating power during a certain portion of the time occupied in their entire distillation. The gases emitted from the retort when first charged are to a great extent occluded gases of low illuminating power such as fire-damp or marsh-gas, and these should be turned into the heating mains. In the course of half-an-hour these occluded gases, together with the aqueous and other vapours, will have left the coal, which is then in the best condition to evolve olefiant gas and other gases rich in carbon, and therefore of high illuminating power. The period during which such illuminating gases are emitted extends over probably two hours, after which the retorts should again be connected with the heating gas mains, until the end of the process. The result of this *modus operandi* would be that the illuminating gas supplied, say in London, from Newcastle coal would probably exceed 20 candle power, instead of 16 as at present, whereby the objectionable results of gas lighting would be greatly diminished, and there would be, say, an equal volume of heating gas available, consisting for the most part of marsh-gas, which, although greatly inferior to olefiant gas in illuminating effect, would be actually more suitable for heating purposes, because less liable to produce soot in its combustion.

The total cost of production would not be increased by this separation of the gases, and the price might with advantage

[1] A lecture by C. William Siemens, D.C.L., LL.D., F.R.S., on January 27, in St. Andrew's Hall, Glasgow, under the auspices of the Glasgow Science Lecture Association. Continued from p. 329.

both to the supplier and to the consumer be so adjusted that the latter, while paying for his illuminating gas an increased price proportionate to the increase of illuminating power, would be furnished with a heating gas at greatly reduced cost; for the heating gas could be reduced in price in a much larger proportion than the illuminating gas would have to be raised, because it would not require the same purification from sulphur which renders illuminating gas comparatively costly. The enormous increase of consumption would moreover enable the gas companies to reduce prices all round very considerably without interfering with their comfortable revenues.

For large applications of heating gas to the working of furnaces and boilers, simpler means than the retort can be found for its production. I constructed a gas producer many years ago in connection with my Regenerative Gas Furnace; this I need not now describe in detail. In it all the carbonaceous matter of the coal is converted into combustible gas, the solid carbon yielding a supply of carbonic oxide; the resultant mixture of combustible gas contains a very large proportion averaging 61·5 per cent. of nitrogen, which swells its volume without in any way contributing to its heating power.

It has been my endeavour for some time to construct a gas producer which, without losing the simplicity of the first, should be capable of yielding a heating gas of superior calorific power. This producer consists of a wrought-iron cylindrical chamber, truncated downwards, and lined with brickwork. The fuel to be converted into gas is introduced through a hopper, and the cinder and ashes work out through the open orifice at the bottom.

Instead of a grating for the introduction of atmospheric air a current of heated air is brought in, either through the hopper or through the orifice at the bottom, and is discharged into the centre of the mass of fuel; the effect is the generation of a very intense heat at that point. The fuel, after its descent through the hopper, arrives gradually at this region of intense heat, and when subjected to it, parts with its gaseous constituents. At the point of maximum heat coke is consumed, producing carbonic anhydride, which, in passing through the considerable thickness of fuel surrounding this portion, takes up a second equivalent of carbon, and becomes changed into carbonic oxide. Here also the earthy constituents are for the most part separated in a fused or semi-fused condition, and in descending gradually reach the orifice at the bottom, whence they are removed from time to time. Air enters through the bottom orifice to some extent, causing the entire consumption of the carbonaceous matter, which may have got past the zone of greatest heat; water is also here introduced in a hollow tray, and after evaporation by the heat of the hot clinkers, passes upwards through the incandescent mass, and is converted by decomposition into carbonic oxide and hydrogen gas. The exit orifices for the gases are placed all round, near the circumference of the chamber, ascending upwards into an annular space, whence they are taken through pipes to the furnace or other destination.

The advantages connected with this *modus operandi* consist in the intensity of the heat produced within the centre of the mass, whereby the whole of the fuel is converted into combustible gases, with the least amount of nitrogen. The hydrocarbons formed in the upper portion of the apparatus have to descend through the hotter fuel below, and in so doing the tar and other vapours mixed up with them are decomposed, and furnish combustible gases of a permanent character.

The orifice at the bottom of the apparatus may be enlarged, and so arranged that, instead of ashes only being produced, coke may be withdrawn, and in this way a continuous coke oven may be constructed, which is at the same time a gas producer, or in other words an apparatus in which both the solid and gaseous constituents of the coal are fully utilised.

The intense heat in the very centre of a large mass of fuel has for its result a very rapid distillation, and thus one gas producer does the work of two or three gas producers of the type hitherto employed; this more concentrated action will moreover allow of the introduction of gaseous fuel, where want of space and considerations of economy have militated hitherto against it, and in favour of the ordinary coal furnace.

It has been already proved that steam boilers can be worked economically on land with gaseous fuel, and there is no reason that I know of why the same mode of working should not also be applied to marine boilers. The marine engine has, within the last fifteen years, been improved to an extent which is truly surprising: the consumption of coal, which at the commencement of that period was never less than 8 lbs. per HP., has been reduced by expansive working in compound cylinders to 2 lbs., or even less, per actual HP. The mode of firing marine boilers has, however, remained the same as it was in the days of Watt and Fulton. In crossing the Atlantic one may see a considerable number of men incessantly employed in the close stoke-hole of the vessel opening the fire-doors and throwing in fuel. Each charge gives rise to the development of great clouds of black smoke issuing from the chimney, to the great annoyance and discomfort of the passengers on deck. If, instead of this, the fuel could be discharged mechanically into one or more gas producers, the gaseous fuel produced would maintain the boilers at a very uniform heat, without necessitating the almost superhuman toil of the fireman; no smoke or dust would be emited from the chimney, and a large saving of fuel would be effected.

This change would be specially appreciated by the numerous tourists visiting the Western Highlands. Speaking from my own experience on one occasion, I may say that the pleasure of a trip on the beautiful Loch Lomond was very seriously marred in consequence of the fumigation which my fellow passengers and myself had to endure.

The change from the use of solid to gaseous fuel would be the prelude probably to another, and still more important change, namely the entire suppression of the steam boiler. We are already in possession of gas-engines working at moderate expense as compared with small steam-engines, even when supplied with the comparatively expensive gas from our town gas-mains, and all that will be required is an extension of the principle of operation already established. The realisation of such a plan would of course involve many important considerations, and may be looked upon as one of those subjects the accomplishment of which may be left for the energy and inventive power of the rising generation of engineers.

Before leaving this branch of the subject I wish to call attention to a favourite suggestion which I had occasion to make some years ago. It consists in placing gas-producers at the bottom of the coal mines themselves, so that instead of having to raise the coal by mechanical power, the combustible gases ascending from the depth of the mine to the surface would acquire by virtue of their low specific gravity such an onward pressure that they could be conducted in tubes to distances of many miles, thus saving the cost of raising and transporting the solid fuel.

Glasgow with its adjoining coal-fields appears to me a particularly favourable locality for putting such a plan to a practical trial, and the well-known enterprise of its inhabitants makes me sanguine of its accomplishment. When thus applied with gaseous fuel, the town would not only be able to boast of a clear atmosphere, but the streets would be relieved of the most objectionable portion of the daily traffic.

I now approach another and the last portion of my address, the attainment of very intense degrees of heat either for effecting fusion or chemical decomposition. Although by means of the combustion of either solid or gaseous fuel heats are produced which suffice for all ordinary purposes, there is a limit imposed upon the degree of temperature attainable by any furnace depending upon combustion. It has been shown by Bunsen and by St. Claire-Deville, that at certain temperatures the chemical affinity between oxygen on the one hand and carbon and hydrogen on the other absolutely ceases, and that if the products of combustion CO_2 and H_2O be exposed to such a degree of temperature they would fall to pieces into their constituent elements. This point of dissociation, as it is called, is influenced by pressure, but has been found for CO_2 under atmospheric pressure to be 2600° C. (or 4700° Fahr.). But long before this extreme point has been arrived at, combustion is greatly retarded, and the limit is reached when the losses of heat by radiation from the furnace balance its production by combustion.

To electricity we must look for the attainment of a temperature above that of dissociation, and we have evidence of the early application of the electric arc to such a purpose. In 1807 Sir Humphry Davy succeeded in decomposing potash by means of an electric current from a Wollaston battery of 400 elements, and in 1810 he surprised the members of the Royal Institution by the brilliant electric arc produced between carbon points through the same agency.

Magneto-electric and dynamo-electric currents allow of the production of the electric arc much more readily and economically than by the use of Sir Humphry Davy's gigantic battery, and Messrs. Huggins, Lockyer, Liveing, and other physicists have taken advantage of the comparatively new method to advance astronomical and chemical research with the aid of spectrum analysis.

My object is now to show that the heat of the electric arc is not only available within a focus or extremely contracted space, but that it is capable of producing such larger effects as will render it useful in the arts for fusing platinum, iridium, steel, or iron, or for effecting such reactions or decompositions as require for their accomplishment an intense degree of heat, coupled with freedom from such disturbing influences as are inseparable from a furnace worked by the combustion of carbonaceous material.

The apparatus which I employ to effect the electro-fusion of such material as iron or platinum is represented in the drawing. It consists of an ordinary crucible of plumbago or other highly refractory material, placed in a metallic jacket or outer casing, the intervening space being filled up with pounded charcoal or other bad conductor of heat. A hole is pierced through the bottom of the crucible for the admission of a rod of iron, platinum, or dense carbon, such as is used in electric illumination. The cover of the crucible is also pierced for the reception of the negative electrode, by preference a cylinder of compressed carbon of comparatively large dimensions. At one end of a beam, supported at its centre, is suspended the negative electrode by means of a strip of copper, or other good conductor of electricity, the other end of the beam being attached to a hollow cylinder of soft iron free to move vertically within a solenoid coil of wire, presenting a total resistance of about 50 units or ohms. By means of a sliding weight the preponderance of the beam in the direction of the solenoid can be varied so as to balance the magnetic force with which the hollow iron cylinder is drawn into the coil. One end of the solenoid coil is connected with the positive and the other with the negative pole of the electric arc, and, being a coil of high resistance, its attractive force on the iron cylinder is proportional to the electromotive force between the two electrodes, or, in other words, to the electrical resistance of the arc itself.

The resistance of the arc was determined and fixed, at well within the limits of the source of power, by sliding the weight upon the beam. If the resistance of the arc should increase from any cause the current passing through the solenoid would gain in strength, and the magnetic force overcoming the counteracting weight would cause the negative electrode to descend deeper into the crucible; whereas, if the resistance of the arc should fall below the desired limit, the weight would drive back the iron cylinder within the coils, and the length of the arc would increase, until the balance between the forces engaged had been re-established.

Experiments with long solenoid coils have shown that the attractive force exerted upon the iron cylinder is subject only to slight variation within a range of several inches, which circumstance allows of a working range to that extent of nearly uniform action on the electric arc.

This automatic adjustment of the arc is of great importance to the attainment of advantageous results in the process of electric fusion; without it the resistance of the arc would rapidly diminish with increase of temperature of the heated atmosphere within the crucible, and heat would be developed in the dynamo-electric machine to the prejudice of the electric furnace. The sudden sinking or change in electrical resistance of the material undergoing fusion would, on the other hand, cause sudden increase in the resistance of the arc, with a likelihood of its extinction, if such self-adjusting action did not take place.

Another important element of success in electric fusion consists in constituting the material to be fused the positive pole of the electric arc. It is well known that it is at the positive pole that the heat is principally developed, and fusion of the material constituting the positive pole takes place even before the crucible itself is heated up to the same degree. This principle of action is of course applicable only to the melting of metals and other electrical conductors, such as metallic oxides, which constitute the materials generally operated upon in metallurgical processes. In operating upon non-conductive earth or upon streams of gases it becomes necessary to provide a non-destructible positive pole, such as is supplied by the use of a pole of fused platinum, or iridium, or by a plumbago crucible. In working the electric furnace some time is taken up in the first instance in raising the temperature of the crucible to a considerable degree, but it is surprising how rapidly an accumulation of heat takes place. In using a pair of dynomo-machines capable of producing 70 webers of current with an expenditure of 7-horse power, and which, when used for purposes of illumination, produce a light of 12,000 candles, a crucible of about 8 inches in depth, immersed in a non-conductive material, has its temperature raised to a white heat in fifteen minutes, and 4 lbs. of steel are fused within another fifteen minutes, successive fusions being effected in somewhat diminishing intervals of time. The process can be carried on on a still larger scale by increasing the power of the dynamo-machines and the size of the crucibles.

The purely chemical reaction intended to be carried into effect within the crucible might be interfered with through the detachment of particles from the dense carbon used for the negative pole, although its consumption within a neutral atmosphere is exceedingly slow. To prevent this I have used, both in this connection and also in the construction of electric lamps, a water pole, or tube of copper, through which a current of water circulates, so that it yields no substance to the arc. It consists simply of a stout copper cylinder closed at the lower end, having an inner tube penetrating to near the bottom for the passage of a current of water into the cylinder, which water enters and is discharged by means of flexible india-rubber tubing. This tubing being of non-conductive material, and its sectional area small, the escape of current from the pole to the reservoir is so slight that it may be neglected. On the other hand some loss of heat is incurred, through conduction, with the use of the water pole, but this loss diminishes with the increasing heat of the furnace, inasmuch as the arc becomes longer, and the pole is retired more and more into the crucible cover.

In the experiments which I shall now place before you the current which has supplied the one electric lamp in the centre of the hall will be diverted by means of a commutator through the electric furnace. After it has been active for five minutes to warm the crucible, I shall charge it with 8 lbs. of broken steel files, which I shall endeavour to melt and pour out into an ingot mould before your eyes.

By some obvious modifications of this electric furnace it can be made available for a variety of other purposes where intense heat is required combined with immunity from disturbing chemical actions. By piercing a number of radial holes through the sides of the chamber, and introducing the ends of wires through the same, an excellent means is provided of heating those wire ends very rapidly, without burning them, for the purpose of welding them together. The electrical furnace will also be found useful, I believe, in the hands of the chemist to effect those high temperature reactions between gaseous bodies which require the employment of temperatures far exceeding the hitherto available limits, and will thus increase the area of available reactions at his disposal for the attainment of either scientific or practical ends.

I have endeavoured to compress within the limited space of a single lecture, subject matter that might occupy the close attention of the student for weeks or months, and I may therefore be pardoned if I have failed to convey to you more than a very rough outline of what may be accomplished by the judicious use of gaseous fuel, and of the electric current, as heating agents. The one purpose that has been foremost in my mind in preparing this lecture, has been to make war upon the smoky chimney, which, so far from being a necessity under any circumstances whatever, should be regarded only as a remnant of that stage of our industrial and social progress which satisfied with the attainment of certain ends, could afford to neglect the economical and sanitary conditions under which those ends were accomplished.

The Exhibition which has lately been held in this city of appliances for heating and illuminating by means of gas and electricity, in which your President, my esteemed friend, Sir William Thomson, took so prominent a part, as he does in everything tending towards the advancement of human knowledge and well-being—proves how deep is the interest felt amongst you in those very questions with which I have had to deal this evening.

And so I thought you might not be disinclined to give attention once more to a particular view of the question, which happens to be the result of the independent labour of one who may claim at any rate to have given a life-long attention to the subject.

PHOTOPHONE EXPERIMENTS

MR. ANDREW JAMIESON, C.E., Principal of the Glasgow Mechanics Institution, sends us an account of the following experiments on the photophone, shown by him at a lecture delivered by him on January 19, before the Glasgow Philosophical Society, on the history of selenium.

The effects of light and heat on the conductivity of selenium were shown by means of a simple and inexpensive form of "cell" joined up in a Wheatstone's Bridge with a reflecting galvanometer. The cell is constructed in the following way:—A piece of plate-glass or of a glass tube of about an inch diameter and about three inches long is chosen, and upon its exterior are tightly wound two separate parallel wires of No. 25 B.W.G., the wires themselves being of copper covered with silk or cotton. A red-hot iron or poker is then applied to the middle region of the coil of wire so as to burn off the insulating covering of silk

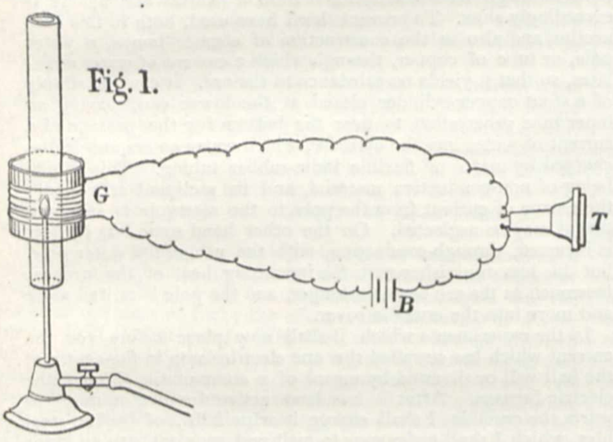


Fig. 1.

or cotton. The bare wires are cleaned, and the blank cell being raised to the proper temperature, vitreous selenium is rubbed on the wires so as to fill the narrow interspaces left by the removal of the silk covering. The selenium is afterwards annealed in the the usual fashion to render it more highly conductive. One of the cells thus used had resistances of 5740 and 3440 ohms respectively in the dark and in the light; but others have less resistances, one being as small as 500 ohms in the dark. The first-named cell (a flat one) was twenty-one days old, and had increased several thousand ohms in that time.

The musical note of a "singing flame" was reproduced in the telephone by means of one of the annular cells thus formed upon a glass tube in the following manner, suggested by Prof. Blyth (Fig. 1):—The cell, C, joined in circuit with a battery, B, and telephone, T, was placed outside entirely surrounding the glass tube in which a small gas-jet was "singing." Speech was afterwards reproduced by the arrangement shown in Fig. 2. At the

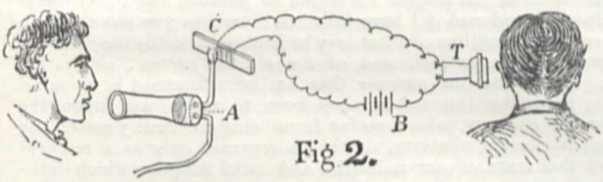


Fig 2.

back conical mouthpiece which receives the voice is fixed a membrane of goldbeater's skin which forms the front of a chamber, A, into which gas is led, and from which a short tube leads to a small gas-jet, in the manner devised by König. Opposite the gas-flame was placed the selenium cell in circuit with a battery of twenty cells and a distant telephone. There were thus eleven changes going on simultaneously:—

1. Muscular movement of speaker's vocal organs.
2. Vibration of air opposite speaker's mouth.
3. Corresponding vibrations of the thin membrane.
4. Variations of pressure controlling the supply of gas to jet.
5. Hence increase and decrease of gas-flame.
6. Increase and decrease of resistance of the selenium cell.
7. Rise and fall of battery current.
8. Increase and decrease of magnetism in magnet of telephone.
9. To-and-fro movement of telephone disk.
10. Vibration of air opposite the same.
11. Vibration of drum of ear of listener at the telephone and a sound heard.

Not only the pitch but the tone of the voice was distinctly heard.

THE COFFEE-LEAF DISEASE

TWO interesting papers on this subject were read at the last meeting (3rd inst.) of the Linnean Society, the one treating of its ravages in India, the other its nature and spread in South America.

In the first Mr. Wm. Bidie, in a letter to Mr. J. Cameron of Bangalore, refers to the Coorg country, situated in the Western Ghats, where European enterprise in coffee has been wholly developed within the last twenty-five years, and no disease was observed till four or five years ago. The author mentions that the disease appears to have been imported from Ceylon by way of Chickmoorloor, a district of Mysore, sixty miles distant from Coorg. It seems worst in impoverished, exposed fields, and least where there is shade and rich soil. A small red insect has been noticed feeding over leaves covered with the pest, but what the insect's relation is to the disease as yet remains undetermined. Plants grown from Ceylon seed suffer most, while those trees of Coorg origin and growth are least affected. A system of "renovation-pitting" has been successfully tried, a pit being dug at short intervals wherein, after judicious pruning, all the affected leaves are buried, and this precaution seems to check the spread of the disease, particularly among the Coorg coffee-trees.

In the second communication Dr. M. C. Cooke describes and summarises all the data extant up to the present time of the progress of coffee disease in South America. Plantations in Venezuela, Costa Rica, Bogota, Carácas, and Jamaica have been affected. He discourses on the nature of the blight, and is of opinion that the disease is a complicated one, being himself as yet unprepared to affirm that either the *Septoria*, the *Sphærella*, or the *Stilbum*, three so-called different kinds of fungi, or altogether, is the true cause of the disease. At the same time he thinks it possible that none of these forms of fungus are autonomous, and that all may be related to each other as forms or conditions of the same fungus, of which *Sphærella* is the highest and most perfect manifestation. He observes that the discoloured spots may be without any visible fungus upon them, and exhibit no trace of mycelium in the tissues, or they may nourish a *Septoria*, as seen by the Rev. M. J. Berkeley, or a *Sphærella* as found by himself, or finally a species of *Stilbum* as seen by Prof. Saenx and himself. Further, the *Stilbum* may occur on the same spot as the perithecia of the *Sphærella*, or both perithecia and *Stilbum*; the one without the other may be found occupying different spots. Mr. Cooke admits that altogether it is difficult satisfactorily to answer the question, What is the cause of this form of coffee disease?

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—Sir William Harcourt announced on Monday that the evidence taken before the Oxford University Commissioners would be laid before Parliament without delay.

CAMBRIDGE.—The first Smith prize has been adjudged to A. R. Forsyth, of Trinity College, Senior Wrangler. R. S. Heath, of Trinity College, Second Wrangler, and A. E. Steinthal, also of Trinity College, Third Wrangler, were equal in the competition for the second prize.

Mr. W. J. Lewis, M.A., of Trinity College, Cambridge, and Fellow of Oriel College, Oxford, has been elected to the Chair of Mineralogy, in the place of the late Prof. Miller, F.R.S. The University is to be congratulated on having secured as Professor of Mineralogy one so competent to take Prof. Miller's place.

Mr. A. Scott is giving demonstrations in Elementary Organic Chemistry at the University Laboratory. Mr. J. F. Walker is lecturing on the same subject at Sidney Sussex College.

Lord Rayleigh is giving a short course on the Unit of Electrical Resistance, and on February 21 will commence an advanced course of lectures on Sound. Mr. Glazebrook is giving demonstrations on Advanced Electricity and Magnetism, and

Mr. Shaw on Heat. All these courses are given in the Cavendish Laboratory.

Prof. Stuart is lecturing on the Differential Calculus and its Application to Mechanics; the Demonstrator has a course on Elementary Applied Mechanics.

Dr. Michael Foster continues his course of Elementary Physiology. The advanced lectures announced this term are by Mr. Lea (who has been appointed Lecturer in Physiology at Gonville and Caius College), on Physiological Chemistry; Mr. Langley, on the Histology and Physiology of the Digestive System, and Mr. Hill (Downing College), on the Central Nervous System.

The Report of the Syndicate on the Higher Education of Women is to be discussed to-morrow (February 11).

The Board of Natural Science Studies recommends that the agreement between the University and Dr. Dohrn, of the Zoological Station at Naples, by which 75*l.* per annum is paid from the Worts Travelling Bachelors' Fund towards the expenses of the station, be renewed for five years. The Board calls attention to the services which those members of the University who have studied at Naples have rendered to science and the University, and to the fact that three of them have obtained professorships elsewhere, namely Professors A. M. Marshall (Owens), T. W. Bridge (Mason's College, Birmingham), and A. C. Haddon (Dublin).

At Newnham College Miss Harland is lecturing on Euclid and Algebra, and Miss Scott on Analytical Conics, Mr. Garnett lectures on Statics and on Experimental Physics, Mr. Hudson on Arithmetic and on the Differential Calculus, Mr. Hillhouse on Botany, while Miss Cross superintends practical and paper work in Chemistry and Geology.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, December 9, 1880.—"The Electrostatic Capacity of Glass," by J. Hopkinson, M.A., D.Sc., F.R.S.

In 1877 I had the honour of presenting to the Royal Society (*Phil. Trans.*, 1878, Part i.) the results of some determinations of specific inductive capacity of glasses, the results being obtained with comparatively low electromotive forces, and with periods of charge and discharge of sensible duration. In 1878 Mr. Gordon (*Phil. Trans.*, 1879, Part i.) presented to the Royal Society results of experiments, some of them upon precisely similar glasses, by a quite different method with much greater electromotive forces, and with very short times of charge and discharge. Mr. Gordon's results and mine differ to an extent which mere errors of observation cannot account for. Thus for double extra dense flint glass I gave 10·1, Mr. Gordon 3·1, and subsequently 3·89 (Report of British As ociation for 1879). These results indicate one of three things, either my method is radically bad, Mr. Gordon's method is bad, or there are some physical facts not yet investigated which would account for the difference. Two suggestions oocur:—1. Possibly for glass K is not a constant, but is a function of the electromotive force. 2. When a glass condenser is discharged for any finite time, a part of the residual discharge will be included with the instantaneous discharge, and the greater the time the greater the error so caused. To test the first I measured the capacity of thick glass plates with differences of potential ranging from 10 to 500 volts, and also of thin glass flasks between similar limits; the result is that I cannot say that the capacity is either greater or less where the electromotive force is 5000 volts per millimetre than where it is $\frac{1}{5}$ volt per millimetre. The easiest way to test the second hypothesis is to ascertain how nearly a glass flask can be discharged in an exceedingly short time. A flask of light flint glass was tested; it was charged for some seconds, discharged for a time not greater than $\frac{1}{17000}$ second, and the residual charge observed so soon as the electrometer needle came to rest; the result was that the residual charge under these circumstances did not exceed 3 per cent. of the original charge, also that it mattered not whether the discharge lasted $\frac{1}{17000}$ second or $\frac{1}{20}$ second. These experiments suffice to show that neither of the above suppositions accounts for the facts.

I have repeated my own experiments with the guard-ring condenser, but with a more powerful battery, and with a new key which differs from the old one, inasmuch as immediately after the condensers are connected to the electrometer they are separated from it. In no case do I obtain results differing much from those I had previously published.

Lastly, a rough model of the five plate induction balance used by Mr. Gordon was constructed, but arranged so that the distances of the plates could be varied within wide limits. So far as instrumental means at hand admitted Mr. Gordon's method was used. A plate of double extra dense flint and a plate of brass were tried. In the first, by varying the distances of the five plates, values of K were obtained ranging from $1\frac{1}{4}$ to $8\frac{1}{4}$, with the latter results from $\frac{1}{10}$ to 3. It is clear that the five plate induction balance thus arranged cannot give reliable results.

The explanation of the anomaly, then, is that the deviation from uniformity of field in Mr. Gordon's apparatus causes errors greater than any one would suspect without actual trial. It is probable that the supposed change of electrostatic capacity with time may be accounted for in the same way.

January 27.—"Dielectric Capacity of Liquids." By J. Hopkinson, F.R.S.

These experiments have for object the determination of the refractive indices and the specific inductive capacity of certain liquids, and a comparison of the square of the refractive index for long waves and the specific inductive capacity.

In the following table are given the results obtained for refractive index for long waves deduced by the formula $\mu = \mu_\infty + \frac{b}{\lambda^2}$, the square of μ_∞, and the observed values (K) of the specific inductive capacity.

	μ_∞^2	K
Petroleum spirit (Field's)	1·922	1·92
Petroleum oil (Field's)	2·075	2·07
,, (Common)...	2·078	2·10
Ozokerit lubricating oil (Field's) ...	2·086	2·13
Turpentine (Commercial)	2·128	2·23
Castor oil	2·153	4·78
Sperm oil	2·135	3·02
Olive oil	2·131	3·16
Neatsfoot oil	2·125	3·07

It will be seen that whilst for hydrocarbons $\mu_\infty^2 = K$, for animal and vegetable oils it is not so.

Zoological Society, February 1.—Prof. W. H. Flower, LL.D., F.R.S., president, in the chair.—Mr. F. M. Balfour, F.R.S., read a paper on the evolution of the placenta and made some observations on the possibility of employing the characters of this organ in the classification of the mammals.—Mr. Sclater read notes on some birds collected by Mr. E. F. im Thurn in British Guiana, amongst which was an example of a new species of *Agelæus*, proposed to be called *A. im-Thurni*, after its discoverer.—Mr. W. T. Blanford, F.R.S., read an account of a collection of reptiles and frogs made at Singapore by Dr. W. B. Dennys. In this collection were two new species of Ophidians, which were named respectively *Cylindrophis lineatus* and *Simotes Dennysi*, and two new frogs, which the author proposed to call *Rana laticeps* and *Rhacophorus Dennysi*.—Mr. A. D. Bartlett read an account of a peculiar habit of the Darter (*Plotus anhinga*) in casting up parts of the epithelial lining of its stomach, as observed by him in the specimen now living in the Society's collection.—A communication was read from Mr. A. Heneage Cocks, F.Z.S., containing notes on the breeding of otters, as observed by him in specimens living in his possession.—The Secretary read a paper by the late Mr. Arthur O'Shaughnessy, containing an account of a large collection of lizards made by Mr. C. Buckley in Ecuador. The collection was stated to be of great interest, both on account of the number of new species it contained and the fresh material it afforded for the study of species already known. Mr. O'Shaughnessy had given last year a partial notice of this collection, confined however to a preliminary list of the species of *Anolis* identified. The present paper gave the results of a study of the whole collection, and was not restricted to a description of the new forms, but enumerated all the species, for the purpose of recording additional remarks and revisions which appeared necessary. In it twenty-seven species were mentioned, ten of which were new.—Mr. G. A. Boulenger read an account of a new species of *Enyalius* in the Brussels Museum, from Ecuador, which he proposed to name *Enyalius O'Shaughnessyi*.—Lieut.-Col. H. H. Godwin-Austen, F.R.S., read the first part of a memoir on the land-shells collected on the island of Socotra by Prof. I. B. Balfour. The present communication comprised an account of the species of *Cyclostomaceæ* found on the island.

Photographic Society, January 11.—J. Glaisher, F.R.S., president, in the chair.—Papers were read by E. Viles on the lime-light. The principal matter insisted upon was that the oxygen and hydrogen gases should unite in one stream, just before issuing from the nozzle of the burner, and the tubes kept entirely free from wire gauze or any impediment whatever; also that the lime cylinder should be in two pieces, when if the upper part splits the lower part (already heated) could be screwed into position at once.—Also by T. Bolas, F.C.S., on the detective camera. This apparatus consists of two cameras working simultaneously together: in one the image can be seen, whilst in the other a sensitive dry plate is ready for instant exposure by pneumatic power. The whole is inclosed in an unsuspicious wooden box, which can even be placed upon the ground, and scenes and persons photographed unawares.

Victoria (Philosophical) Institute, February 7.—The Earl of Shaftesbury, K.G., in the chair.—A paper was read by Dr. Samuel Kinns, F.R.A.S., on "The Truths of Revelation confirmed by the Advances of Science."

Institution of Civil Engineers, February 1.—Mr. Abernethy, F.R.S.E., president, in the chair.—The paper read was on the Portsmouth Dockyard Extension Works, by Mr. Charles Colson, Assoc, M. Inst. C.E.

PARIS

Academy of Sciences, January 31.—M. Wurtz in the chair.—The following papers were read:—On the long duration of the life of germs of *charbon*, and on their preservation in cultivated earth, by M. Pasteur, with MM. Chamberland and Roux. This relates to an inquiry made by a committee elected by the Paris Society of Veterinary Medicine. Sheep caught the disease from being a few hours daily on ground where animals that died of *charbon* had been buried twelve years before. There was no grass to eat; the germs must have entered the sheep by reason of their habit of smelling about the ground. The farmer had a scar of malignant pustula.—Observations on the birds of the Antarctic Region, by M. Alph. Milne-Edwards. This relates to the first part of a work on the fauna of Austral regions. Birds serve more than any other animals to mark the profound differences between faunas of the southern and those of the northern hemisphere. The geographical distribution of penguins and spheniscans present interesting features in this respect.—On a mode of representation of functions, by M. Gyldén.—On a fall of sleet at Geneva, on January 19, by M. Colladon. The grains were compact and pretty round, and they showed curious dancing movements (sometimes after being motionless two or three seconds), like those of pith balls under electricity.—M. Clos was elected Correspondent in Botany, in room of the late M. Godron.—On the circulatory apparatus of edriophthalmate crustaceans (continued), by M. Delage. This relates to Amphipoda and Læmodipoda.—Action of sulphocarbonate of potassium on phylloxerised vines, by M. Mouillefert. The effects of three to six, two, and one year's treatment are severally considered.—On the figure of planets, by M. Hennessy. For the earth and near planets supposed like it the compression deduced from the theory of fluidity agrees better with observation than that deduced from the theory of superficial erosion.—On the series of Fourier, by M. Jordan.—On an extension of the rule of signs of Descartes, by M. Laguerre.—On a particular cyclic system, by M. Ribaucour.—On the quadrature on which depends the solution of an extensive class of linear differential equations with rational coefficients, by M. Dillner.—On the distinction of integrals of linear differential equations into sub-groups, by M. Casorati.—On the invariant of the eighteenth order of binary forms of the fifth degree, by M. Le Paige.—Action of hydrochloric acid on metallic chlorides (continued), by M. Ditte. In the case of chlorides very soluble in water (less so in acid liquor), and deposited in it as crystallised hydrates, hydrochloric acid diminishes the weight of chloride dissolved, and in the acid liquors one still finds hydrated salts, though much less rich than the crystals that form in this liquid. Another (and last) group contains chlorides that crystallise anhydrous in water or hydrochloric acid, but the solubility of which in concentrated acid is reduced almost to zero.—Determination of the colours which correspond to fundamental sensations, by means of rotatory disks, by M. Rosenstiehl. The line which represents the proportion of extreme sensations in the intermediate colours is a straight one (they are thus, to sight, rigorously equidistant). The line which represents the sensation of yellow reaches its culminating point in the ordinate corresponding to yellow. The sensation of red rises in a straight line to the red, and beyond that to the orange, where it culminates; then it falls to the yellow, where it is zero.—On the determination of carbonic acid in air, by MM. Muntz and Aubin. They have studied the variations of which the proportion of CO_2 in special parts of the atmosphere is susceptible, and here first describe their method (absorption by pumice-stone impregnated with potash solution, then liberation, and measurement of volume), and verify its value.—Observations on a note by M. Eisenberg, on separation of trimethyalmine from substances accompanying it in commercial chlorhydrate of trimethylamine, by MM. Duvillier and Buisine.—On a process of total destruction of organic matters, for investigation of poisonous mineral substances, by M. Pouchet. The principle is that it is possible to heat between 300° and 400°, in presence of carbon or organic compounds, mineral elements contained in a mixture of sulphuric acid and acid sulphate of potash. The sulphate of potash retains substances the most volatile and decomposable (*e.g.* salts of mercury), while the organic matters are quickly destroyed.—On the invasion of pulmonary tissue by a champignon, in peripneumonia, by M. Poincaré.—The third edition of M. Domeyko's Treatise on Mineralogy (in Spanish) was presented. It contains original researches on various South American minerals.

VIENNA

Imperial Academy of Sciences, February 4.—V. Burg in the chair.—F. Osnaghi and V. Lorenz, fifth report of the Adria Commission on the Physical Exploration of Adria.—Prof. L. Pelz, on scientific treatment of axionometry.—Prof. A. Wassmuth, on the possibility of magnetising iron at high temperatures.—Prof. T. Tlann, on the daily course of some meteorological elements in Vienna (city).—G. Bruder, on the knowledge of the tura-formation of Sternberg near Zeidler (Bohemia).—Prof. Sigmund Exner, on the knowledge of the minute structure of the cortex of the brain.

GÖTTINGEN

Royal Society of Sciences, August 7, 1880.—On fluospar in granite of Drammen, by O. Lang.—Some experiments on induction in conducting bodies, by F. Himstedt.

November 5.—On an increase of the meteorite-collection of the University, by C. Klein.—Communication regarding the publication of a text-book of analysis, by R. Lipschitz.—Electrical shadows, by W. Holtz.

December 6.—Electrical shadows (continued), by W. Holtz.—On the connection between the general and the particular integrals of differential equations, by L. Königsberger.—Observations in the magnetic observatory, by K. Schering.—On congenital growth in the thallus of Pollexfenieæ, by P. Falkenberg.—Communications on the University Library from 1876-79, by W. Willmanns.

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