THURSDAY, DECEMBER 12, 1878

LORD RAYLEIGH'S "THEORY OF SOUND"

The Theory of Sound. By J. W. Strutt, Baron Rayleigh, F.R.S. Vol. II. (London: Macmillan and Co., 1878.)[1]

THE second part of Lord Rayleigh's highly instructive work on acoustics contains the mechanics of oscillatory motions in liquids and gases. Atmospheric air is that medium by which by far the greater number of sound-waves are conveyed to our ear, since it is only exceptional that this happens through solid bodies which are in contact with our teeth or with the bones of the skull. But it is just for this reason that all circumstances are of considerable importance, which influence the transmission of sound-waves in the air, *i.e.*, change either their velocity, their direction, or their intensity. This part of the theory has been worked out very minutely and completely by the author. We find here the compilation and demonstration of a large number of facts which, in other works on acoustics, are hardly mentioned. The author, after having first developed (in Chap. XI.) the general laws of the motion of liquids as expressed in hydrodynamical equations, and then explained the difference between rotational and irrotational motion of fluids, passes on to the simplification of the equations, which is determined by the circumstance that with sound, as a rule, we have to do with oscillations of extremely small amplitude. First, the motion of plane waves is investigated, and it is shown that with waves which move only in one direction half their equivalent of work consists in the *vis viva* of motion, and the other half in the potential energy of the compression and dilatation of the medium. Then follows the explanation of the influence which the change of temperature, taking place with compression or dilatation of gases, exercises upon the velocity of transmission of sound. It is shown, in the manner first employed by Prof. Stokes, that if a perceptible quantity of heat could be exchanged between the compressed and dilated layers of the waves during the lapse of one oscillation, the intensity of the sound-waves would very quickly decrease in their transmision and they would die away.

The subjects treated of up to this point are generally known among physicists; less known are a series of other results of the theory. The author next gives a comparatively very elegant and easily intelligible demonstration of the results at which Poisson and Riemann arrived, when investigating the propagation of sound-waves for which the velocities of oscillation are no longer infinitesimal when compared to the velocity of transmission. It appears that the different layers of the wave transmit their phases with different velocities, viz., with that velocity which represents the sum of the ordinary velocity of transmission of the smallest waves and of the oscillation velocity of the particles of air oscillating in the same direction. The compressed layers of the wave, therefore, are propagated quicker than the dilated ones, thus they must gradually change the shape of the wave, and finally overtake the preceding dilated layers. What would happen in that case, whether perhaps a breaking of the waves of air would take place, is not yet clear, since the hydro-dynamical equations apply only to velocities changing continuously.

These circumstances have not always been considered in experimental researches concerning the velocity of sound. A precise answer to the question regarding the magnitude of this velocity can only be given, if we confine ourselves to oscillations of extreme smallness.

The author has also investigated under what conditions a sound-wave of finite amplitude can move forward without changing its form. It appears that this could happen only under the supposition of a special law for the compressibility of the medium, which does not correspond with the law applying to gases.

The propagation of sound in the atmosphere is subjected to yet other perturbations, which partly arise from the different temperatures and moistures of the superposed strata, and partly from the different force of winds. At the surface of water or extensive masses of solid substance, the sound-waves of the air are totally reflected even under very small angles of incidence; under perpendicular incidence their reflection, although not total in the strict sense of the word, is nearly as complete. For that part of the sound which enters the new medium, the same law of refraction holds good which applies to waves of light. But also from a surface of hydrogen one-third of the sound coming through air at a vertical incidence is reflected, and the angle of incidence for total reflection is not larger than 15¼ degrees.

The problem to determine theoretically how the propagation of sound in the atmosphere is changed by the different temperatures of its strata cannot yet be solved completely. However, it can be ascertained in what direction the most powerful effect must travel. On account of the great dimensions of the strata of the atmosphere, compared to which the wave-length of the large majority of audible tones disappears entirely, the conditions of the propagation of sound are similar to those of light. We may imagine the sound-waves dissolved, as it were, into rays of sound, and then look upon each separate ray as being almost completely independent of the motion of its neighbouring rays. This is no longer admissible if obstacles are in the way of the travelling sound, the dimensions of which exceed the sound wave-lengths only in moderate proportions, as is the case in our houses and rooms, with the transmission of sound through windows and doors. Then, as in the case of light under similar circumstances, diffraction takes place. The great difference in the propagation of sound and light, as it becomes evident in ordinary experience, has its cause in the very different magnitude of wave-lengths. The greater the wave-length the greater the diffraction on the passage through the same aperture. These circumstances, which are forgotten so frequently, the author considers in Chapter XIV. When sound is propagated in the unbounded space of the atmosphere the conditions of the problem are such, that they allow of its decomposition into rays of sound. If a source of sound is near the ground then its sound rays are all bent into an upward direction, as Prof. Osborne Reynolds first pointed out, and those which travel in a direction parallel to the ground are mostly annihilated through friction or other obstacles. The sound proceeding from

[1] For Vol. I. see NATURE, vol. xvii. p. 237.

a source near the ground is, therefore, not heard far off by an observer standing on the ground. It is heard at a much greater distance if the observer or the source of sound be in an elevated position. The state of the atmosphere will have great influence upon these conditions. In dry air and sunshine the deflection of sound upwards will be greater than in moist air which forms clouds above, or during rain.

The decrease of temperature in the upper strata of the atmosphere causes sound to travel at a lesser speed than in the lower ones. Now if a wind is blowing, the velocity of which increases with the height, then, as Prof. Stokes remarks, this causes an increase in the velocity of the sound-waves in the direction of the wind, and a decrease in velocity in the opposite direction; thus, for sound which travels in the former direction, the retarding effect of decrease of temperature is neutralised, and for that travelling in the latter it is augmented. We therefore hear better if the wind blows towards us from the source of sound than if the contrary takes place. Indeed, by an upper windy layer, sound produced in the lower tranquil air may be totally reflected. This influence of wind is remarkable also because it forms an exception to the law demonstrated by myself, viz., the law of reciprocity in the propagation of sound, if the sound-source and the observer change places.

The problems of the reflection of sound by fixed walls, for instance, the phenomena of whispering-galleries, speaking-trumpets, and the echo, are treated in the same manner. Although here the admissibility of the decomposition of sound into sound-rays does not, as a rule, appear quite so unquestionably justified, yet the phenomena observed agree with this hypothesis on the whole.

An essential progress in the application of the theory upon experiments has been made by the author in the calculation of the influence of the open apertures of organ pipes and resonators upon their pitch. In my own demonstration of this part of the theory I had started from those forms of motion which did not render the calculation too difficult, and had then derived the corresponding forms of pipes; finally, I had so determined the optional constants of my hypotheses, that the form of pipe approached the form wished for, the cylindrical one, for instance; yet I remained confined to a few forms if I did not wish to complicate the calculation too much. Lord Rayleigh, on the contrary, supposes a given form of pipe, and has employed the maxims, developed in the first volume of his work, regarding the variation of conditions under which sound-motion takes place, to determine the limits within which the true value of the desired magnitude must lie, and has indeed been able to draw these limits so narrowly for the most important problems, like that of the cylindrical open pipes, that practically the solution is perfectly sufficient. In this way he has been able to treat simultaneously a number of problems, which hitherto had not even received an approximate solution, for instance, the determination of the proper tones of resonators of the shape of bottles with wide body and narrow neck.

Besides the problems mentioned, which are of direct importance to experimental physics, a series of others are worked out, where the mathematical solution can be completely given, such as the propagation of sound in balls, spherical layers and rectangular boxes filled with gas, the reflection of sound from the outer surface of a ball, and the communication of sound to air by oscillating balls and strings. These problems are valuable not only as theoretical exercises, but also with regard to our understanding of physical phenomena. They are examples affording to the mental eye of the physicist a particularly perfect insight into the essence of sound-motion and the changes it undergoes, when the conditions under which it occurs are changed. Thus he obtains quite as good a conception of the typical behaviour of sound as if he had actually seen the phenomena, and this conception will also guide him safely in cases of observation, where the exterior conditions are not as simple as they are in the theoretical example.

At the end of the volume Lord Rayleigh has placed the words: "The End." We hope that this may be only the provisional, not the definite end. There is still an important chapter wanting, viz., that on the theory of reed-pipes, including the human voice. For the former, at least, the principles of their mechanics can already be given, and the methods the author employs seem to me to be particularly well adapted for further progress in these domains.

After reed-pipes we would mention the theory of singing flames, and the blowing of organ-pipes. In the latter case the leaf-shaped current of air, which comes from the wind-case, forms a sort of reed, which oscillates under the influence of the oscillating column of air in the interior of the pipe, and which throws its air now into the interior of the pipe, and now outside.

Altogether, the whole of this important class of motions, where oscillatory movements are kept up through a cause which acts constantly, deserves detailed theoretical consideration. The action of the violin bow, and the sounding of the Æolian harp, also belong to this class.

Lord Rayleigh certainly deserves the thanks of all physicists and students of physics; he has rendered them a great service by what he has done hitherto. But I believe I am speaking in the name of all of them, if I express the hope, that the difficulties of that which yet remains will incite him to crown his work by completing it.

H. HELMHOLTZ

OUR BOOK SHELF

Zoologischer Anzeiger. Herausgegeben von Prof. J. Victor Carus. (Leipzig: Wilh. Engelmann.)

THE idea of a zoological "advertiser" seems to us a most excellent one, and we both welcome and heartily recommend to our readers who are interested in the animal kingdom this little fortnightly journal of Prof. Carus. The editor purposes to publish a number every two weeks, each number to contain from sixteen to eighteen pages, and the yearly subscription to be six shillings. The first number bears the date of July 1, 1878, and already a dozen numbers have appeared. The plan of each number is to commence with a retrospect of the current literature of zoology, a retrospect that we need hardly say will be well done by one so learned and able in the matter of bibliography as the editor of that most necessary work, the "Bibliotheca Zoologica." Then each number contains some short notices on zoological subjects in connection with museums, chiefly such as have a practical bearing. Thus in No. 2 we find an article by G. von Koch, of Darmstadt, on a method by which sections can be made of substances of different degrees of consistency without altering the relative position of the same, which cannot fail to be of interest and value to such as wish to make and mount thin sections of corals or alcyonarians, while in most of the numbers there are under this head to be found notices of collections for sale or specimens to be exchanged, and when this journal becomes, as we have no doubt it will, known to all directors and assistant-directors of zoological museums we anticipate for this section a very extended use. Another portion of the journal is devoted to short notices on general zoological subjects. Thus No. 11 contains a short notice by Prof. Salensky, of Kasan, on the embryology of the ganoids; one by Prof. Goette, of Strasburg, on the development of the bones in the limbs of vertebrates; a note by Dr. A. Gruber, of Freiburg, on the formation of the oviducts in the Copepods; one by Prof. Entz, of Klausenberg (Hungary), on the evolution of gas from the protoplasm of some protozoa, in confirmation of a record of the same fact by Prof. T. W. Engelmann; and one by Prof. E. Martens, on our knowledge of thread-spinning snails.

Another characteristic of this new journal is that, under the heading "Personal-Notizen," will be found a very exact list of all the museums and schools of anatomy and zoology in the world, commencing with those in Germany. This list has now got as far as Belgium. The directors' and assistant-directors' names, with those of the professors and assistant-professors, are given in full, and if, when the list is complete, an index of the names of the various teachers in all the colleges and schools were added, the list would serve many a useful purpose.

We feel persuaded that this most useful little journal will require only to be known in this country to be duly appreciated, and we wish its talented editor every success in his undertaking. E. P. W.

La Vegetacion del Nordeste de la Provincia de Entre-Rios. Informe Cientifico. Del Dr. Don P. G. Lorentz. (Buenos Aires, 1878.)

THIS is a book, or rather a pamphlet, of 179 pages of closely-printed matter, and illustrated by two maps of the country described, the nature of which with regard to its vegetation is very carefully recorded in the first division, which occupies forty-seven pages. The second part consists of a list of species arranged scientifically under each natural order, the paragraph referring to the individual plant comprising such information as to the frequency or scarcity of the species, the colour of the flowers, period of flowering, and any properties for which the plant may be economically valuable. These lists are useful in many ways, for instance they often show the widespread geographical range of many well-known plants, and in the lists before us we find many European introductions. A separate list of thirty-two species of fungi is added, and some notes on the maps given.

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.*]

Was Homer Colour-Blind?

UPON reading Dr. Pole's two papers (NATURE, vol. xviii. pp. 676, 700) my first feeling was to ask: "But how could

'The blind old man of Scio's rocky isle'

know anything at all about colour?" Presuming, however, that the tradition of his blindness might be unwarranted, and further, that it may be a mistake to suppose, as many do, that the "Iliad" is a collection of rhapsodies by different poets, I again asked myself: "Are there in Homer more anomalies in the nomenclature of colours than may be accounted for by the vague use of words? Are there more than we should find in this country among uneducated men of the labouring class?" About two years ago I made extensive inquiry as to the prevalence of colour-blindness among children, and in the village schools of this part of Somersetshire I found that the girls could name the neutral as well as the other tints readily and correctly, but that many of the boys had but about half-a-dozen words to use, and would refer orange to red or to yellow, and purple to brown or to blue, merely for want of terms; for they could match the test papers with other papers, or with the girls' dresses.

If we refer to the old ballads and early romance poetry of our own and other languages, we shall see that the popular poets of the middle ages, like the peasant boys of the present day, misused terms of colour as much as Homer; although the many beautiful paintings that still exist prove that people could see and distinguish colours as well then as now, and that Mr. Gladstone's theory of a development of the sight from one generation to another is a mere delusion. Certain terms are adopted and handed down traditionally as stock epithets in poetry and technical terms in trades. They are known to be wrong, but they are used from habit.

Dr. Pole assumes that the colour-blind see black and white as others do; or, to use his own words (p. 700), that their vision in regard to them is normal. This I doubt. One of the gentlemen who is so affected tells me that he cannot distinguish snow upon the steps of his front door. Now if white is a combination of all the colours of the prism, and we omit red and green, there will be seen a combination of blue and yellow, and these when spun together in a colour top produce stone colour, which I believe to be the white of those who have a dichromal vision. As to black, it is singular that in Anglo-Saxon *blac* means not "black," but as the Flemish *bleek* and Germ. *bleich*, "pale," a case in point to show the instability of language in these matters.

To take Homer's terms *seriatim*:—

Ἐρυθρός.

A poet must not be pronounced colour-blind if he compares wine to blood, and calls it red.

"The king sate in Dumfermline town, drinkin' the *bluid-red wine*."
Sir Patrick Spens, l. 1.

"And aye she dighted her father's wounds, His *blood* ran down *like wine*:"
Douglas Tragedy, st. 8.

Conversely, in slang language to give a man a bloody nose is to "tap his claret." The chair-cushion upon which I am sitting and the curtain of my window are of a dark crimson, but in the language of upholstery would be called "maroon." Now *marron*, from which the word is derived, is a Spanish chestnut, and that is a full brown without any visible red in it.

In our old English ballads and early romances, and in the German, Flemish, Swedish, and Danish, and in some more modern poems gold is constantly called "red" and "ruddy;" as in Dryden:—

"A crown of ruddy gold enclosed her brow."

In a German ballad by Ehrhardt, "Die Nonne," st. 5:—

"Was zog er von seinem Finger? Einen Ring von *Gold so roth*."

In a Flemish ballad of the sixteenth century called "Het Soudaen's Dochterken" in Thijm's "Gedichten," v. i. p. 246:—

"Sijn hayr dat blinckt van verwe schoen,
Als waer het *roode gouden*."

In an ancient Swedish ballad called "Gångarpilten," Arwidss v. ii. p. 156:—

"För jag har intet *rödt guld* att sätta mod er.

And in the corresponding Danish ballad, Dan. Viser iv. p. 122:

"Jeg haver ei *det röde guld*."

Milton, "Paradise Lost," bk. ii. l. 889, calls flame "ruddy flame."

In old herbals, as in that of Lyte, fol. 1578, p. 162, marigolds are called "Ruddes." "They be called in Englishe Marygoldes and *Ruddes*."

Hair, such as is usually called "red hair," is better named in Greek πυῤῥός, fiery; for certainly its colour is widely different from blood. Where in the Old Testament the word "red" is applied to horses and heifers, the Greek of the Septuagint has πυῤῥοί.

What is really red is on the contrary called by Jenner "pink" in the well-known poem upon the signs of wet weather:—

"Closed is the *pink-eyed* pimpernel."

To come to the very age we live in, old croquet-players persist in calling the second ball of the series "pink," although for the last ten years it has always been painted red.

Now if English should become a dead language, what will some future critic suppose *red* to have meant? a term that he finds applied to blood, to gold, to wine, to the marigold, to flame, and to bay horses; and replaced with *pink* in the case of the pimpernel and the second croquet-ball?

Φοίνιξ.

This word meant originally Phœnician, a people from whom the richly-dyed robes that they imported were called; as in an old poem we have a colour designated from Bristol:—

"Her kirtle *Bristol red;*"

and as a deep blue dye is called "indigo" from being first brought from India.

The same word φοίνιξ was applied to horses, probably Syrian ones in the first place, just as from Rouen we call those of a certain colour "roan," agreeably to a common usage in all languages. Thus porcelain is called "China," and a certain leather made of goats' skin "Morocco," although manufactured in Europe.

As to the term φοίνιξ being applied to the lion and the jackal, we may well suppose that Homer never saw the one or the other. It is quite as unlikely that he ever saw a live dragon. If the horses that the Phœnicians introduced were tawny, it would be no misnomer to call the lion and jackal φοίνιξ. We are not to presume that if Phœnician robes were crimson, everything else that was named after them must also have been crimson. A future critic might as reasonably argue that porcelain from China was of an orange colour, because there are "China oranges."

Dates were also called φοίνιξ as being a Phœnician fruit, just as the small grapes imported from Corinth are called "currants."

Ῥοδόεις.

This term referred, no doubt, to a crimson variety of rose, the so-called Damask rose, the one usually cultivated in ancient times. Thence a comparison of its colour to blood implied by a line of Bion:—

Αἷμα ῥόδον τίκτει τὰ δὲ δάκρυα τὰν ἀνεμώναν.

Where this word is applied to oil in the account of the funeral of Patroclus (Il. xxiii. 186), it is to a heavenly oil with which Venus anoints the corpse of Hector to preserve it from putrefaction, and not ordinary olive oil:—

ῥοδόεντι δὲ χρῖεν ἐλαίῳ Ἀμβροσίῳ.

It may have meant either "rose-coloured" or "rose-scented."

Κυάνεος.

This term, which seems in so many passages to mean "dark," would have been very properly applied to the sand of volcanic islands, like those in the Egean Sea. On the coast of the Gulf of Naples near Pompeii it is quite black, and walking over it on a hot sunshiny day I had cause to remember its colour, for my feet were roasted.

Χλωρός.

Grass in the Mediterranean countries soon withers and dries to a pale colour, and remains so the greater part of the summer. It was to this withered grass that Homer seems to have compared a pale complexion, and honey, and olive wood, and the nightingale. Our evergreen meadows are unknown in the south.

Οἶνοψ.

The houses of the ancients were unprovided with glass windows, and were very dark within, so that entertainments must have been given by lamp-light, when wine of a dark colour would have appeared darker still.

Πορφύρεος.

A vague term, but equally vague our *purple;* for while we apply it to the foxglove and many other flowers which present an equal mixture of red and blue, we at the same time apply it to a beech, the foliage of which is of a deep copper colour merging into black without any blue in it at all; and in milliners' language to a deep blue without any red in it.

Ἰοειδής.

What was the flower that the Greeks called ἴον, is very doubtful. That which Pindar describes (Ol. vi. 91) as with ξανθαῖς καὶ παμπόρφυροις ἀκτῖσι, with brilliant yellow and richly purple rays, cannot be our own modest violet. I have always supposed it to mean Centauries of different species, some of them, as the *C. ragusina*, of the brightest gold colour, others, as the *C. cyanus*, of a clear blue, and others of a dark purple. The late J. Hogg in his treatise upon the classical plants of Sicily most unaccountably omits all mention of it. At the present day it is the stock, *Matthiola incana*, which in Italy is called *Violetta*. In the above line quoted from Pindar it must have been a radiate flower that he intended. In this respect uncultivated nations are very inaccurate. The Illyrians at the present day call all wild flowers alike indiscriminately *rosje*, roses; and we may be sure that Jesus Christ in his beautiful apologue—"Consider the lilies"—used the language of the people he was addressing, and did not mean lilies in the strict sense of the word; plants that would not burn if cast into an oven on the morrow of being cut down.

It is very strange that Mr. Gladstone in the essay published in the *Nineteenth Century* of October, 1877, has entirely passed over κροκόπεπλος, saffron-robed, an epithet twice applied to Eos, the dawn of day, in the first lines of "Il.," bk. 8 and bk. 19, a word that proves that Homer saw yellow distinctly; for he never calls Eos yellow-fingered, κροκόδακτυλος, or rosy-robed, ῥοδόπεπλος.

In the above it has been my desire to prove that any inaccuracy in Homer's names of colours was due to the unfixed character of the language, and not to a defective vision on the part of the poet. In illustration of this view let me give a case that occurred to me about two years ago. I took to a flower show at Taunton a dahlia of a rather common variety, and such as most gardeners would call purple; a dark pink with a shade of blue over it, and requested forty-four different people to write me down what they would call its colour. In their replies I got fourteen different names for it. I sent a flower of the same kind to a lady who returned me twelve replies from members of her family and friends, and in the twelve were eight different names. How much more then may we expect diversity and inaccuracy in the nomenclature of their colours among the popular poets of an early period! and how little reason have we for believing in any gradual development of colour vision in successive generations of men!

R. C. A. PRIOR

Colour-Blindness

IN answer to Mr. Podmore's question in NATURE, vol. xix. p. 73, as to the appearance to me of the green of the solar spectrum, I may say that such part of it as inclines to yellow is seen by me as faint yellow, and such part of it as inclines to blue is seen by me as faint blue. The line of division, which I may call neutral green, appears simply colourless or white; there is no dark space, no pigments; neutral green appears to me gray.

When I wrote the paper for the *Phil. Trans.* I applied the descriptions to colours obtained by pigments, because that was the mode that had previously been adopted in treating the subject, and I had not, at that time, the opportunity of making any good observations on direct light. At a later period I went through a series of experiments of the kind with an eminent physicist, but I am not aware that the results have been published. I will endeavour, if possible, to supply the desideratum.

WILLIAM POLE

The Colour Sense

THE note of Mr. Grant Allen in NATURE, vol. xix. p. 32, induces me to state that in the year 1877 I arrived at and developed exactly the same conclusions in several articles of the German journal, *Kosmos* (vol. i. pp. 264-275 and 428-433), namely:—

1. The colour-sense manifestly appears already in insects and many of the lowest vertebrates; its complete absence could therefore hardly be supposed in the very lowest race of men.

2. The anomalies shown in the expressions of colours among the most ancient civilised nations by Gladstone, Geiger, and Magnus, may be perfectly explained, partly by the insufficiency of the primitive store of words for this subject, partly by climatic, physiological, and optical reasons, as stated at length in the above-mentioned articles.

3. The usage of telling terms for the single colours closely followed the progress of the art of dyeing.

ERNST KRAUSE

Berlin, December 2

History of the Speaking Telephone

As the writer of the article on the history of the telephone, to which so eminent an authority as Prof. Watson takes exception in the long and interesting letter he has contributed to your columns, perhaps you will allow me to say a few words. Prof. Watson expresses his "astonishment at the claim now made that he (Mr. Gray) anticipated Mr. Bell in the invention of the speaking telephone," and speaks of the "erroneous statement of facts" contained in the article in question (NATURE, vol. xviii. p. 696). Unfortunately Prof. Watson has not specified the statements which are erroneous, and appears to have overlooked the fact that the article is a review of the works of Mr. Prescott and M. du Moncel on the telephone, and that the "statements of facts" are chiefly quotations from those works. At the same time, using all the materials within my reach, careful inquiry had led me to concur, and in that article I expressed my concurrence in the following opinion, quoted from Count du Moncel's book:—"Si M. Bell a été le premier à construire et à rendre pratique le téléphone parlant, M. Elisha Gray avait le premier conçu le principe de cet instrument."

Gray and Bell were both exhibitors at the Philadelphia Exhibition, and Prof. Watson, writing as one of the judges of the scientific instruments exhibited, shows that whilst Gray merely submitted to the judges an apparatus for the multiple transmission of musical notes, and no speaking telephone, Bell not only exhibited a speaking telephone, but towards the end of June (1876) the judges, Prof. Watson and Sir William Thomson, obtained with Bell's instrument the clearest evidence of the electric transmission of speech;[1] whereupon Mr. Gray was both surprised and incredulous, and even after the publication of Prof. Bell's discovery, he delivered a lecture exhibiting his musical telephone, but making no mention of a speaking telephone.

If the Philadelphia Exhibition were the only means for scientific publication during the year it existed, Prof. Watson's letter would effectually dispose of Gray's claim. An exhibition, however, is not the place for conceptions, but for accomplished facts, and I believe no one denies that to Mr. Bell is due all the credit of having been the first to construct, and that entirely independently of Gray, an articulating electric telephone. Gray's claim, as I take it, rests on his having registered in the American Patent Office, on February 14, 1876, "a means of transmitting and receiving vocal sounds telegraphically," and the drawing he gives of his invention shows a correct appreciation of the true principle of an articulating telephone, to which his previous researches had been gradually leading him.

I should be sorry to appear in any way to depreciate the splendid achievement of Prof. Bell through having referred to other workers in the field of electric-telephony. In fact up to the time the article in NATURE appeared, I fear that, through ignorance, I had done but scant justice to Mr. Gray, having attributed the conception of the principle of an articulating telephone solely to Prof. Graham Bell.

There are two points in the history of the telephone upon which I should be very glad to have authoritative information from Prof. Watson or other of your American readers; the first relates to the claim made by Prof. Dolbear, and the second to the introduction of the ferrotype diaphragm. W. F. BARRETT

Royal College of Science, Dublin, December 9

[1] I am glad to learn the exact date of the trial in question, which was given as August in the article.

The Formation of Mountains

IN the account of M. Favre's experiments in NATURE, vol. xix. p. 103, I find the following passage:—"It is, in fact, very probable that our globe is at the stage when, according to Élie de Beaumont, 'the mean annual cooling of the mass exceeds that of the surface, and exceeds it more and more.' It must follow that the external strata of the globe, tending always to rest on the internal parts, are wrinkled, folded, dislocated, depressed at certain points, and elevated at others."

The whole theory of these dislocations, &c., thus depends on the assumption that the interior of the globe is cooling more rapidly than the crust. This has always seemed to me an impossibility, and even an absurdity, and I shall be very glad if any of your correspondents will explain how it is possible. I have always understood that the surface of the earth does not now derive any appreciable portion of its heat from the interior; but if the interior is cooling rapidly, to what can it part with its heat but to the crust? Volcanoes and hot springs no doubt allow a certain portion of heat to escape, but it must be an infinitesimal part of the heat of the entire mass. If the meaning of the statement is, that the heat received from the sun now keeps the surface at a permanent mean temperature, quite irrespective of central heat or cold, and that therefore the loss of heat by volcanoes, &c., causes the centre to cool while the crust does not—this may be admitted, but it is doubtful whether it can have any bearing on the effects observed. For, on this theory, all the compression would take place in that shallow superficial layer which is kept above its normal temperature by the sun's radiation; and as we go back into past time this superficial layer would be thinner and thinner. But all geological evidence goes to show that folded and contorted rocks were subject to compression at considerable depths; and further, that such contortion was greater in comparatively early than in very late geological times—both facts directly opposed to the theory in question. Will any one of our great physicists enlighten us? ALFRED R. WALLACE

AFTER reading your *résumé* of Prof. Alphonse Favre's interesting experiments on the formation of mountains by lateral thrust, it occurred to me that it would be easy to devise a mode of experimenting which would more nearly correspond with what takes place in nature. In M. Favre's experiments the lateral thrust was simply in one direction. In nature it is in all directions.

If a disk of india-rubber were stretched by means of a steel ribbon bent into a circular spring, on letting the spring slowly recoil there would be a lateral contraction of the india-rubber in all directions. A layer of clay upon that disk would, I think, show not the transverse inequalities of M. Favre's drawings, but a diversified unevenness more nearly resembling the actual surface of the earth. ARTHUR RANSOM

Leicester, December

New Galvanometer for Strong Currents

I OBSERVE in NATURE (vol. xviii. p. 707) an article on a new galvanometer for strong currents by Mr. Eugen Obach. I published a paper on the same form of galvanometer seven years ago, and inclose a copy of my paper which was published in the *American Journal of Arts and Sciences*, vol. ii., August, 1871. JOHN TROWBRIDGE

Harvard College, Cambridge, Mass., U.S.A., November 23

Explanatory

I MUST ask you, in common fairness, to allow me to protest against P. G. T.'s mistaken statement (vol. xix. p. 71) respecting a sentence which he quotes without the explanatory context. The moving force exerted by the earth on the moon as a whole is of course precisely equal to the moving force exerted by the moon on the earth. I had not to learn this from P. G. T., but had said so in so many words. But the moving force exerted by the earth on a given amount of matter in the moon is eighty-one times greater than the moving force exerted by the moon on an equal amount of matter in the earth. P. G. T. will scarcely deny this, and he cannot deny that the whole statement from which he quotes one sentence meant this, and this only. Nor, if he did, would any one who has read the chapter on the moon's motions in my treatise on the moon, believe such a statement.

He quotes a passage from my last book without comment, but, unfortunately, not without serious alteration. Apart from the undue emphasis which he thus gives to certain parts of it, the passage expresses my honest opinion. That I may be mistaken is quite possible. Men are always misunderstanding each other. If I find I have erred, I will acknowledge as much.

Until the word "heat" ceases to be used in common speech in two senses, or I am shown that when used for "temperature" (as when we say blood heat, boiling heat, a heat of 90° F., and so forth), it can be understood to mean "caloric," I intend always so to use it in familiar writing about science. I deliberately struck out the word "temperature" wherever I had used it, and replaced it by the word "heat," in the same way and for the same reason that I often replace the word "velocity" by the word "speed." If in any passage ambiguity has thus been occasioned—or, as I would rather say, if anything I have thus said can be mistaken—I shall be glad to hear of it and set it right.

I must have failed, however, to make my meaning clear to P. G. T. in pp. 194 and 240. If at least he rightly understands me, I must leave him to settle with observed facts in one case and with the recognised authorities in the other.

My account of the earlier experiments of Professors Andrews and Tait was taken, as stated, from a paper by Prof. Heaton. P. G. T. ought to know the facts, and I accept his correction. When my article was written, several years ago, the "now received idea" was not yet received. I did not err in calling that theory "beautiful" and "ingeniously conceived" which is now generally accepted. But if I had, it is a less serious mistake to describe a sound theory as still open to doubt, than to describe a doubtful theory as demonstrated. This the author of the sea-bird theory of comets might remember with advantage.

RICHARD A. PROCTOR

Graphic Granite

I HAVE been spending some time of late in the examination of the rocks of this district, and was pleasantly surprised, a few weeks back, at finding some well-marked specimens of graphic granite among the waste material raised from Huel Agar Mine. It very closely resembles that found at Portsoy, N.B., but the felspar is grey instead of red. As I am not aware that this interesting rock is known to exist in any other locality in England, the observation may be worthy of record.

W. End, Redruth, December 2 FRANK JOHNSON

The Phonograph and Vowel Sounds

IN the interesting paper on "The Phonograph and Vowel Sounds" (vol. xviii. p. 340, *et seq.*), the authors remark that although the general results are the same as I have inferred from my own researches, the special numbers expressing the distribution of total intensity of vowel sounds among the partial tones are very different. Perhaps you will have the kindness to communicate to your readers the following reasons explaining, as I believe, the differences mentioned above.

1. The tables given by the authors, which contain the distribution not of intensity but of amplitudes, must be altered in a manner readily seen in order to be comparable with my tables.

2. The marks impressed by the phonograph contain certain peculiarities which, although without influence on the tones spoken from the instrument, remain effective in modifying the form of the curves obtained by mechanically transferring them.

3. The objective intensity (kinetic energy) determined by the authors is nearly, but not quite, proportional to the subjective intensity (quantity of sensation) which I have measured with the aid of resonators.

4. As I have observed, the differences of English and German pronunciation cause remarkable differences in the distribution of total intensity of vowel sounds among the partial tones.

Taking the above points into consideration it will be seen that the differences mentioned by Messrs. Jenkin and Ewing appear much smaller.

Besides I am pleased to notice that the authors, like myself, consider the flexibility of mouth cavity as important in explaining, where it exists, the characteristic pitch and other properties of vowel sounds. F. AUERBACH

Local Colour-Variation in Lizards

THIS subject has recently been very fully discussed by my friend, Dr. Max Braun, assistant in the zoological laboratory of the University of Würzburg. His paper, which has especial reference to the lizards of Minorca and of some of the smaller islets of the Balearic group which lie round that island, is entitled "*Lacerta Lilfordi* und *Lacerta muralis*," and will be found in Part I. of the fourth volume of Prof. Semper's "Arbeiten aus dem zoologisch-zootomischen Institut in Würzburg," published in May, 1877.

Braun refers constantly in this paper to a memoir by J. von Beidraga, entitled "Die Faraglione-Eidechse und die Entstehung der Farben bei Eidechsen," which was published at Heidelberg in 1876. P. HERBERT CARPENTER

Eton College, December 9

The Range of the Mammoth

ON November 6 Prof. Boyd Dawkins read a paper before the Geological Society on "The Range of the Mammoth in Space and Time." As the professor and several other recent writers have taken it to be proved that *Elephas primigenius* occurs in pre-glacial beds, it will, perhaps, be as well at once to review the evidence.

Geologists often speak of "pre-glacial beds" when they only mean beds beneath some one boulder clay, perhaps No. 6, or even later in the list given below. The succession is roughly as follows:—

6. Hessel Boulder Clay	Upper, Middle, and Lower of the North of England (?)
Hessel Gravel	
5. Purple Boulder Clay	Upper, Middle, and Lower of Lincolnshire, &c. (?)
Bridlington Crag	
4. Chalky Boulder Clay	Upper, Middle, and Lower of East Anglia.
Mid-glacial[1]	
3. Contorted Drift[1]	Lower Boulder Clay of the Norfolk Coast.
Sands[1]	
2. Second Till[1]	
Intermediate Beds[1]	
1. First Till[1]	
Arctic Freshwater Beds.[1]	Pliocene.
Temperate Freshwater Beds.[1] (Land surface.)	
Weybourn Beds, estuarine, including the "Forest Bed."[1]	

As the *lower* boulder clay of Northwich, in Cheshire, appears to be No. 5 or No. 6, and consequently newer than the *upper* boulder clay of East Anglia, the molar of *E. primigenius* found beneath it need not be pre-glacial. The Hertfordshire boulder clay, beneath which Prof. Prestwich found a tooth is, I believe, No. 4.

In East Anglia I have seen two molars of *E. primigenius* from the contorted drift, No 3 in the list, but it has not yet been found lower. All the specimens said to come from the forest bed have been dredged or picked up on the beach, and are of no value as evidence. At Bacton, on the Norfolk coast, I dug out a jaw and three teeth of the mammoth from a post-glacial deposit; if the denudation of the cliffs had proceeded these teeth would have been found on the beach mixed with those of *E. meridionalis*. There appears to be one specimen, and one only, found *in situ* in the Forest Bed which can with any probability be referred to *E. primigenius*; this was found some years ago by Mr. Savin, of Cromer, it has not yet been satisfactorily determined, but from its peculiarity and the difference of opinion about it, it appears certainly not to be the ordinary form. CLEMENT REID

Egton Bridge, Yarm, Yorks

The Bunsen Flame a Sensitive Flame

IT is not generally known, *if* it has ever been noticed before, that the Bunsen lamp gives a flame sensitive to sounds. A lamp should be chosen which has a tendency to "burn below;" this may usually be secured by opening the air passages to the utmost and lessening the supply of gas. The flame should burn quietly. My most sensitive flame is four inches high; the gas at about one inch pressure of water. A smart tap with a penholder on a glass cylinder a yard from the flame causes the characteristic "ducking," which is sometimes so energetic as to extinguish the flame or to cause it to burn below. The acute sound of rattling bottles, of a glass rod against a beaker, and many such familiar sounds of the laboratory, are the most effective. This may explain burning below without obvious cause. A tap on a mortar with the pestle twenty feet distant from a well-adjusted flame causes it, and so, often unintentionally, we may have the same result. W. W. HALDANE GEE

Preston, December 3

OUR ASTRONOMICAL COLUMN

JEAN DOMINIQUE CASSINI.—In the course of his examination of the older archives of the Paris Observatory, which had been placed at his disposal with unrestricted permission to make extracts for use in his lunar re-

[1] These will be described in the Geological Survey Memoir on the Cromer Cliffs

searches, in the first instance by Delaunay and afterwards by Leverrier, Prof. Newcomb discovered that the wide-spread belief that Cassini I. was director of the Observatory, which is even stated to have been the fact in French histories of astronomy, is an error. The establishment appears to have been assigned for the common use of the Academy of Sciences, and no such office as that of director was known or recognised. Prof. Newcomb suggests it may have been the celebrity of Cassini which gave rise to the impression that he was director of the Observatory. Of the astronomical records of that time preserved in the archives a large portion were evidently never intended to be understood or used except by the observers themselves. The note-books have no titles, no indications of the observer or indications of the instruments employed, except in the case of clocks: each observer seems to have had his own instruments, without any reference to or comparison with those of others. In the earlier observations no designations even of occulted stars were attached, so that it was necessary for Prof. Newcomb's investigation to calculate the places of the moon as affected by parallax for the times of observation before the objects could be identified, an operation which, though laborious, was always successful except in the cases of a few small stars. Lalande, in his notice of the work of Cassini I., does not call him director of the Observatory at Paris. Louis XIV., he states, applied to the Pope, Clement IX., for permission for Cassini to pass some years in Paris, where the Academy of Sciences was in course of formation, offering him, through Colbert, 9,000 livres per annum for the period of his residence in France. He arrived at Paris on April 4, 1669, and his reception by the French king was such that he wished to remain permanently in the country. The Pope offered opposition at first, which the king succeeded in overcoming, and Cassini was naturalised, and, as Lalande says, obtained a considerable fortune. He commenced observations at the Paris Observatory in September, 1671.

THE SECOND COMET OF 1582.—In the list of cometary radiant-points and meteor-showers in the Report for 1877 of the Luminous Meteors' Committee of the British Association, the distance of this comet's orbit at the descending node from the earth's path is stated to be 0·00, and the conjunction with the cometary shower is fixed to November 9 for 1875. This is a mistake, whichever orbit of those calculated may be adopted for the comet. Pingré, in his *Cometographie*, gave two sets of elements, the second being calculated apparently with the view to bring in Santucci's reported observation of a comet on March 10, of which he was the only observer. Tycho observed the comet on May 12, 13, and 17, and it is upon the observations on these three days that the orbits depend. They were reduced with modern elements by Mr. Hind (*Astron. Nach.* No. 880), and from the resulting positions, D'Arrest calculated elements in 1853. In 1865 Mr. Marth, after rectifying one oversight in the reduction, also computed an orbit, and his elements will no doubt be preferable to the other systems, though they do not differ materially from D'Arrest's in *Astron. Nach.*, No. 891. Mr. Marth's elements are as follow:—

Perihelion passage 1582, May 6·4485 M.T. at Uraniburg.

	° ′ ″	
Longitude of perihelion	255 16 43	Equinox
,, ascending node ...	227 13 33	of 1582.
Inclination	61 25 51	
Log. perihelion distance	9·22716	

Motion—retrograde.

Whence the comet's radius-vector at descending-node is 2·87, or the comet is far outside the earth's orbit at that point in its path. The radius-vector at the opposite node is 0·18, so that there is no near approach to our track.

The comet of 1582 was observed by the Chinese for about twenty days from the day of discovery, May 20. Their annals have no reference to Santucci's comet, the existence of which is doubtful, but we follow Pingré in designating Tycho's comet—the second of the year.

GEOGRAPHICAL NOTES

AT the meeting of the Geographical Society on Monday evening, after a few remarks from the Earl of Dufferin, who occupied the presidential chair for the first time, Mr. C. R. Markham, C.B., read three papers on Arctic matters. In the first he treated of the Swedish expedition, of the progress and results of which we have kept our readers informed. The second paper was devoted to the Dutch expedition in the *Willem Barents* during the past season, the results of which are by no means insignificant. Experience of the ice movements between Spitzbergen and Novaya Zemlya was acquired, and a full hourly series of meteorological observations taken, as well as deep-sea soundings with serial temperatures and magnetic observations. Natural history collections were also made, and Mr. W. G. A. Grant, who was in the *Pandora* in 1876, succeeded, in spite of almost constant fogs, in completing an excellent series of photographs. Lastly, Mr. Markham dealt with the best route for future Polar discovery, which he considers to be along the west coast of Franz Josef Land.

WE have to hand a report on the results of the preliminary polar expedition conducted by Capt. Tyson in the *Florence*. The vessel wintered from October 10, 1877, in lat. 66° 13′, at the head of Cumberland Gulf, Amisto Harbour. The published maps locate this place 2° further north. Last winter was very severe, almost an unbroken succession of storms of rain and snow. The *Florence* was not frozen in until the latter part of November, and after the middle of May the ice was unsafe. The coldest temperature was – 52° F. on January 21; the highest, on June 9, was 55°·5. The longest period of cold was from March 5 to 13, when the thermometer averaged about – 40°. The variations of the thermometer were often from 6° to 8° in a single hour, when the wind was veering. The site had never been visited by any naturalist, but the fauna was found by Mr. Kumlein almost identical with that of Baffin's Bay, with only a few notable and apparently unaccountable exceptions. Some North Pacific species and one European were found by Mr. Kumlein, Birds do not occur in any notable number, except perhaps the eider-duck. Some rare eggs were procured, and a good series of skeletons of young and fœtal seals. The flora appears to be extremely meagre. The same species were collected on the Greenland coast, in lat. 70° N., much more luxuriant and abundant. A considerable collection of lichens was made, and a good series of algæ was collected. Only ten species of fishes were met with, some of them, it is true, of interesting forms. The family collected are of Silurian age. Esquimaux skulls and old implements were procured.

A GOOD deal has been said of late as to the practicability of opening trade-routes to the Chinese province of Yünnan from the side of Assam, Burmah, Tonquin, and even Russian Turkestan, and it is, therefore, not uninteresting to note from the Pakhoi Consular Report that the Chinese have a route thither through the south of the empire, of which we believe foreigners have not heard before. Communication between Pakhoi, on the southern sea-board of Kwang-tung, near the Tonquin frontier, and the province of Yünnan is carried on by the west River pass Nanning-fu to Peisi-ting, thence over the mountains, or still by the West River, in very small boats, to Kwangnan-fu, in Yünnan. In addition to the natural difficulties of the route, the border-land of the two provinces is a wild and lonely region, infested by bands of robbers. The valuable trade which will some day spring up with the rich south-eastern part of Yünnan, not devastated during the Mohammedan rebellion, will, no doubt, take the route of Haiphong, as it is shorter than that of Pakhoi, and has practicable water-communication throughout its whole length.

THE latest news from Dr. Gerhard Rohlfs informs us that he is at Tripoli, under the protection of the French consul. He was to proceed to Wadai on December 15, and was daily expecting rich presents from the Emperor of Germany, which he was instructed to offer to the new sultan of this remote kingdom.

DR. CREVAUX, the explorer of French Guiana, has reached the source of the Oyapok, and crossed the range which separates Oyapok from the Amazon basin.

L'Exploration, of December 7, contains several interesting letters from M. Soleillet, who, it will be remembered, started some months ago from the French West African settlements for exploration of the African interior. His last letter is dated from Kouniakary, the capital of Segou, from which he hopes to reach Timbuctoo.

ACCOMPANYING an article on the Bolan Pass in the December number of the *Geographical Magazine* is a map of the Bolan, the Mula, and other Passes, by Mr. Trelawny Saunders, an admirable specimen of accurate and carefully executed cartography.

ARE THE FOSSIL FLORAS OF THE ARCTIC REGIONS EOCENE OR MIOCENE? AND ON THE CAUSES WHICH ENABLED THEM TO EXIST IN HIGH LATITUDES[1]

THE question of the conditions and their origin under which floras, presumably requiring a temperate climate, were enabled to exist in Polar regions has been so frequently discussed both before and since the recent Arctic Expedition, that it is strange to find any aspect of the subject having remained unnoticed.

There are some points, however, which can be by no means regarded as conclusively settled, and upon these I wish to say a few words. In the first place the age of the so-called miocene floras has, I believe, been wrongly interpreted. Again, the temperatures required by them may have been over-estimated. Lastly, there seems to me to be no occasion whatever to invoke astronomical causes or to invent recurring periods of heat and cold, of which we have no geological proof, in order to account for their former presence there.

In the first place, then, if we examine the palæontological evidence, the only kind on which the age of the rocks has been decided, we find that it is very far from conclusive, and instead of pointing to a common miocene age for all the tertiary beds in the Arctic regions, we find that there are many reasons for believing some of them at least to be eocene. The plant evidence is, indeed, in the present state of our knowledge, almost negative; but what similarity there is in the floras to those of the miocene is directly against their being of that age instead of in favour of it, *for no two floras which are much alike and met with in widely separated latitudes can possibly have been contemporaneous, although floras of quite distinct facies may have been so.* The fact that a proportion of the plants have been identified by Heer with those of the miocene of Switzerland is of no weight whatever, as a considerable proportion are equally identified with the undoubted eocene lower lignitic of America, and a number of forms in the latter again with the miocene of Switzerland. The truth is, that at present any formation containing dicotyledons may be, with almost equal plausibility, referred to either eocene or miocene to suit the author's requirements; for besides the similarity in the ovate and lanceolate leaves from both, many species actually range right through them. Were the age of the Alum Bay and Bournemouth beds not thoroughly established, from their fossil leaves, even they would certainly have been referred to miocene. This will not, I am happy to think, always be the case, for there are a great number of plant forms which appear to be decidedly typical of, and confined to, each stage respectively. Already a number of hitherto supposed miocene deposits in Europe have been recognised as eocene, and as soon as those forms which from their range are of negative value, have been set aside, the confusion will cease. I have, indeed, strong hopes that we may be able to recognise each stage in the eocene of temperate latitudes by its plants, more from the incoming of new and distinct types, however, than the continued presence of older forms.

The confused way in which floras of many ages have been mixed together, seems to me to have arisen in a very simple manner. The flora of Oeningen must undoubtedly be accepted as a typical miocene flora, and contains but very few eocene forms—if indeed any. It contains, however, many plants common to other isolated fragments of strata which contain mixed floras, that is, floras with percentages of eocene as well as miocene plants. There being no typical series from undoubted eocene available as standards of comparison, the plants common to the miocene have alone been taken to determine the age of these beds, and the unknown eocene forms have thus been enrolled as miocene, and in their turn used to identify other still more distinctly eocene beds as miocene; much in the same way that the Barton beds were formerly identified, from their possessing a few species in common, as London clay, and the species peculiar to the Barton horizon, subsequently made use of to identify *calcaire-grossier* and Bracklesham beds in their turn with the London clay.

The oldest Arctic flora containing dicotyledons, and which, therefore, has any bearing on the subject, is that of Kome in North Greenland. It is mainly composed of ferns and gymnosperms, and its age may probably have been correctly inferred. The next beds, referred to the upper cretaceous, appear to be approximately of the same age as the Dakota beds, and therefore, in my opinion, decidedly supra-cretaceous relative to that formation in Europe.

We have next, if Heer's nomenclature is right, an immense gap right from the cretaceous to the miocene, to which latter he has referred all the rest of the obviously newer beds. He does not account in any way for the absence of eocene deposits, and relies exclusively on plant evidence, which I think should be, for the present, absolutely set aside. Before quitting the subject of the plants, I wish it to be understood that I in no way intend to disparage Heer's judgment. Considering the nature and condition of the specimens submitted to him, it is a marvel that he could have drawn and described them in so admirable a manner. But Heer has unfortunately never had a large series of definitely eocene plants to describe, and hence almost all his floras are cretaceous or miocene. I profoundly respect his work, and only to strengthen my plea that leaf evidence may be temporarily set aside or reconsidered, I mention that already three floras pronounced by Heer to be miocene, have since proved one cretaceous and two eocene; and still others must follow.

Assuming that I may be allowed for the present to dismiss the plants, I will touch upon the purely physical evidence, which seems to me to be, small as it is, entirely in favour of the eocene age of the beds. In the first place we have the great probability that eocene remains would, if they had ever existed, be found in their proper sequence, considering the number of widely-separated places in the North Polar area from which plant-remains have been obtained. It is certain that they must have existed, since the area continued land throughout eocene times, for there are no marine deposits of that age; and there is

[1] This was written before attention was called to Saporta's address at the anniversary meeting of the Royal Society.

abundant evidence of more than one kind that Europe and America were connected then, and that animals and plants passed between them. Besides that, the floras themselves contain both American and European types, and during the middle eocene a great number of plants were common to both continents.

In the next place the temperature of the eocene period in Europe was much hotter than that of the miocene, and therefore presumably more favourable to the growth of such floras in northern latitudes. To call them miocene we have to admit the former existence of a climate sufficiently uniform to have enabled the same species of plants to grow simultaneously from Italy and the United States to the 70th parallel, a state of things not in accordance with our present experience of plant distribution. But if we assume them to be eocene, the decreasing temperature which prevailed from that time to the miocene would have gradually and naturally driven the forms southward, and thus the very similarity of the miocene floras of America and Switzerland to those of the Arctic regions, renders it most unlikely that they were of the same age, and almost certain that the latter were considerably older.

In comparing the eocene and miocene temperatures we find, as already stated, that the former most readily accounts for the growth of temperate floras in high latitudes. Taking Heer's estimate that the miocene temperature in the latitude of Switzerland at the sea-level was only 9° C. warmer than at the present day, the progressive decrease of heat to the north is not so much in accordance with that of the present time, as it is found to be on the supposition that they belong to the eocene; we have to suppose that the mean temperature diminished in a less degree.

Speaking roughly, the present decrease in the isotherm from latitude 50° of south England, to that of Spitzbergen, is about 10° Fahr. for every 10° of latitude. This is as nearly as possible the ratio of decrease between England and Greenland in eocene times as implied by the floras, supposing them to be of one age. If, as I assume from all the data I can collect, England in middle eocene times possessed a mean annual temperature of 70° F., Greenland would naturally have had one of 50°, which is that assigned to it by Heer (9° C.). The decrease thence to Spitzbergen and Grinnell-land is hardly less rapid, being about 1° F. (½° C.) of cold for each degree of latitude. Heer calculates—principally on the mean temperature required by Platanus—that between Greenland and Spitzbergen, 8° lat., it was 4° C., but very unaccountably fancies that between Spitzbergen and Grinnell-land no further decrease took place, and upon this assumes that trees might have extended to the very Pole itself. The evidence against it, however, seems perfectly clear, for all the planes and limes, and more temperate forms have disappeared, and the genera found there, with a single exception, have representatives which at the present day live within the Arctic Circle. This exception, Taxodium, judging from its present habitat, Mexico and the Southern United States, would necessitate a climate completely different from that required by all the other plants with which it is associated. They form a distinctly Arctic assemblage: the spruce, especially, is never met with fossil to the south, except in inter-glacial beds. The fossil Taxodium must, therefore, only be looked upon as an allied extinct species, whose resemblance to living forms does not imply identity of habit, since all other considerations are against it. A similar instance is found in the willow, which is generally characteristic of the north; yet *Salix humboltiana* is found in the Amazon districts and *S. safsaf* in Egypt; and similarly, although Cassia is eminently characteristic of tropical and subtropical zones, *C. marylandica* flourishes on the banks of the Lake of Geneva. As it is essential to get rid of the evidence of Taxodium, if we are to suppose this former climate followed the present natural laws, I shall refer to some remarks by Lesquereux upon the nearly allied redwoods.

In describing the pliocene plants of California, he concludes that they are related to the present flora of the Atlantic slope, and not to that of California. He accounts for their destruction on the eastern side by the powerful agencies of glacial action, marine submersion, and long-sustained volcanic cataclysms. When these had ceased the sheets of water between the Missouri River and the Rocky Mountains and the mountains themselves prevented the old flora from again occupying the Western area. Some of the pliocene species, however, were preserved through the glacial epoch in California, but modified, for the most part, by the cold conditions they had undergone. "The two species of *Sequoia*—one the more predominant, the other the more remarkable, of the flora of California—are evidently also remnants of the pliocene. *S. gigantea*, which in all probability covered the higher slopes of the mountains of that epoch, has been destroyed everywhere, except in some deep valleys. . . . The other, *S. sempervirens*, left here and there, has again taken the ascendency under more favourable physical circumstances. Its present distribution explains its preservation until the present epoch. According to Prof. Bolander, "the distribution of the redwood depends upon sandstone and oceanic fogs. Where either one of these conditions is wanting there is no redwood. The redwoods begin in the northern part of Monterey County, in isolated groups, in deep, moist cañons. A short distance south of Monterey City, on the Monterey Bay, a white bituminous slate sets in, and extends nearly to Pajaro River. On this no redwood is found, but *Pinus insignis*. At Pajaro River, eight to ten miles from the ocean, they set in again, and extend to nearly twenty-eight miles south of this city (San Francisco), either in deep cañons, or in groves extending over several ridges eastward, as far as the fog may reach. Then they continue in similar localities to latitude 42°, the state boundary."

The existing allied species withstood a glacial period in California; there is no improbability in supposing that older and extinct species may have habitually supported a cold temperature. It appears that they belong to a very old type, now confined to a limited area, and becoming extinct, at whose survival we cease to wonder when we reflect that individual trees have been calculated to be 3,000 years old. To pass through the life of such a species, an enormous period must be required, for only 100 generations might carry us back 300,000 years, with as little modification as an annual plant might undergo in 100 years. The sandstone soil and damp sea fogs required by them in their native habitat, may explain the difficulty in getting them to grow under cultivation except in comparatively warm latitudes—and it is upon plants under cultivation Heer's estimate in Europe is based—but lessens our surprise that they should have existed in Greenland or farther north during the eocene time.

Apart from Taxodium, therefore, there is every evidence, in the disappearance of temperate forms and the preponderance of conifers of boreal type, that, as at the present day, there was a natural and progressive decrease of temperature to the north between Grinnell-land, Spitzbergen, and Greenland.

In the next place I would call attention to the possibility that the respective temperatures thought to be requisite for the growth of such associations of plants as are found fossil in these various lands may be in excess of the minima which would have sufficed. If this were the case, it would of course remove to a slight extent an argument I have just brought forward against the miocene age of the deposits. One of the conditions peculiarly favourable to the growth of trees in northern latitudes is the protracted length of the summer days, and it is an ascertained fact that they require less heat in latitudes above 60°, owing to this rapid lengthening of the days. The chemical action of the sun's rays seems in some way to compensate for feeble warmth, and vegetation receives more impulse from the presence of the sun than from temperature in the shade. As examples of this, De Candolle[1] mentions that *Fagus sylvatica* exists in the north with a less temperature than it can support elsewhere; and that the limits of growth of barley prove the point conclusively.

It appears certain, according to De Candolle, that in very few cases has even intense cold, during natural periods of rest, any injurious influence upon plants, and that their northern limits are not determined by excess of cold but by want of heat. The destructive agents are late spring frosts, or premature heat followed by chills; and so fatal are their effects that one week in May has killed entire stocks of sub-tropical plants which had stood considerable frost in winter. There is no doubt that many plants would grow in much colder latitudes if the temperatures of each month were cyclically regular. *Fraxinus excelsior*, L.,[2] supports great cold, especially when accompanied by fogs, and penetrates as far north as 64°. *Ilex aquifolium*, L., reaches latitude 62° in Norway, and, like Abies, is limited in range, not by excess of cold, but want of heat. *Evonymus europæus*, L., is found just within 60°, and must occasionally suffer intense cold. But perhaps the well-known *Chamærops humilis*, L., affords the most striking familiarly-known instance of capricious distribution. It is indigenous to Nice in latitude 40°, yet it is not found anywhere in Italy, with a trifling exception, until Calabria is reached. Under cultivation it bears a very considerable amount of winter frost, the limit of which I have not ascertained, nor the minimum it encounters at Nice. I merely mention these instances as indicating possible sources of error, for were Cham ærops extinct and found fossil at Nice, we should infer from it, with every appearance of probability, that the temperature of Nice had been the same as that of Sicily or Granada, the more normal homes of the palm.

One of the most remarkable facts connected with Alpine or Arctic plants is the length of time they can endure the absence of light while they are covered with snow, and when thus protected they would be unaffected by even Arctic cold. Evergreens, as we see by the Alpine rhododendron, are equally unaffected, and I have in Switzerland seen laurels, bays, and acubas shrouded in snow for many weeks without injury. I will mention but one other instance of the extent to which trees will sometimes bear cold, quoted by Herschel.[3] "In the valley of the black Irkut, in Siberia, Atkinson found a ravine filled with ice, and with large poplars growing in it, with their trunks imbedded 25′ in snow and ice, while the branches were in full leaf. Around each stem was a hollow of 6″ thawed and full of water." Besides mere heat and cold there are many influences known and unknown which limit the range of plants. The distribution of the vine is a case in point, for it is well kown that in historic times it was extensively cultivated in England, Normandy, and parts of Prussia, in which it will no longer ripen its fruit.

While the winter temperatures in these Arctic regions, if accompanied by snow and fogs, may have been of extreme severity, the summer temperature need not have been high, for the present Arctic and Alpine plants, including roses, species of Betula, Salix, Empetrum, Vaccinium, and conifers need but little heat.

Having attempted to show that the amount of heat really required was not so large as has been imagined, I will endeavour to prove that it, even upon Heer's assumption, might easily have been furnished by physical causes which we know did, in all probability, exist in eocene time, and were quite independent of astronomical causes and change in the position of the earth's axis, of which there seems to me no proof whatever, geological or otherwise. At least, to qualify this assertion, if such have existed in the past, there is no need to invoke them in this particular case. Central heat may, of course, be dismissed as having had too little influence in eocene time to be appreciable.

We may roughly estimate, on Heer's basis, that the average temperature between the latitudes and longitudes of England and Iceland was not more than from 15° F. to 20° F. warmer in eocene (or miocene of Heer) times than it is at present, and we may assume also on the evidence we possess that the present climate would permit any of the eocene floras, supposing they still existed, to grow in latitudes not more removed from those in which they are found than 15° to 20° farther south. For example, the English eocene flora could now exist in Madeira, the Iceland eocene flora in the Isle of Wight, that of Spitzbergen in Sweden, and that of Grinnell-land in Northern Norway. We have therefore to seek for some cause adequate to produce a difference in the temperature of Greenland, for instance, equal in degree to that of 20° F. or 20° latitude as a maximum. Following upon a map the isotherms of the 70th parallel, we see that Prince Albert Land has a temperature of but 5° F., whilst Lapland, in the same latitude, has one of 32° F. There is evidently here a cause at work capable of influencing the temperature to the extent of 27° F.; therefore a more powerful cause than is required. The same map shows us obviously that this agent is the sea. Wherever the Arctic waters find egress or penetrate the land, the isothermal lines around the Pole are deflected south. In like manner the line denoting the limits of trees is in many places pushed back more than 10° S. by the ice-laden water flowing from the Arctic Ocean. From Lapland to Siberia it is, except for a short distance, within the Arctic circle, principally within the 70th parallel. Nearing Behring's Straits it is sharply deflected south by the Polar Sea, but away from its influence, it as suddenly rises and again (North America) far overlaps the Arctic circle, until it once more comes under the influence of the cold seas and channels penetrating south into Hudson's Bay, which drive it to below the 60th parallel. Avoiding Greenland, it includes part of Iceland and the whole of the North Cape, owing to the influence of the Gulf Stream. The influence of this warmer water, cold as it is here, is no less remarkable, for, by merely shutting off the Arctic currents from close proximity to the shore, it enables trees to grow on the coast, and at a point on the Arctic circle between Iceland and the Norwegian coast, raises the temperature, according to Herschel, full 20° above that which is normal to the latitude.[1]

We thus see that the limit of trees enters the Arctic circle wherever the land has a great extension south or where the Gulf Stream raises the temperature, but that it especially shuns wherever the Arctic waters penetrate the land, even in the smallest gulfs or bays. The lands between Hudson's Bay and Davis' Straits, cut up by water, and the islands in the Arctic Ocean surrounded by water, are intensely cold and destitute of trees—almost of vegetation. The cause of Greenland's being shrouded in ice is its unknown and exceptional extension towards the Pole and the increased height of land in its northern portion. These appear to be necessary conditions of such complete glaciation as we there see, as shown by the absence of an ice-cap in Grinnell and other equally northern lands. The present condition of Greenland is wholly abnormal, and, presenting such unusual conditions, has heightened the astonishment felt when the former mildness of its climate became known.

If we were able to shut off from the Atlantic the enor-

[1] De Candolle, "Géog. Botanique," vol. i., 1855.
[2] Examples used by De Candolle.
[3] "Physical Geography," p. 312.

[1] "Physical Geography," p. 232.

mous Arctic currents which chill it, we should produce at once a greater increase of heat than is required by the floras. If, further, we impinged the Gulf Stream upon its shores, without cooling it down by floating icebergs upon its back, we might be able to induce at least an even more temperate vegetation to grow there.

Water is thus seen to be the great factor in distributing heat and cold in northern regions, and not land, as has been generally taught. Humboldt believed the rigorous climate of America to be due to high land stretching to the Pole; Lyell taught that with great polar seas and an excess of land at the equator, the hottest conditions possible on the globe would be produced, and that with land at the Poles and a great equatorial sea, the coldest conditions would ensue. A study of the isothermal lines leads to the contrary belief that the presence of land at the Pole, even if ice capped as Greenland is, would be less productive of cold than a polar ocean with free exits, for air has not the distributing power possessed by ocean streams, and when these are ice-laden the effect is still greater.

It only remains to call attention to such proof as we have, that these conditions really did exist in eocene time, and that the Arctic currents were actually shut off from the Atlantic in those days by continuous land which connected the two continents of Europe and North America. In the eocenes of Europe and North America we have evidence of a great, and, comparatively speaking, sudden rise of temperature, and this was followed in due course by a mingling for the first time of the floras and faunas of the two continents. That there was land communication to the north is further evidenced by the occurrence of types of both kinds in the floras now found upon the spots on which they grew. In further support of this theory we have the fact that no trace of sea-deposit of eocene age has ever been found in the polar area, all the vestiges of strata remaining showing that these latitudes were then occupied by dry land.

If we may assume that these conditions really did prevail, and that all the outlets into the Atlantic were closed by the elevation of the present sea-bed between 60° and 70° (where, I believe, the sea is even now shallower) into land of moderate elevation; with or without prolongations south to the 50th latitude; and the north of Greenland submerged, a temperature would ensue more than adequate to support all the plants yet found fossil in eocene or miocene Arctic beds. The result would be that the zone of greatest heat would be far north of the equator; for while the southern hemisphere was still cooled by the Antarctic currents rising to the surface, the North Atlantic would be practically a land-locked sea, cut off from southern cold by the tropics, from northern cold by land, and heated by the sun like the Gulf Stream or Red Sea. There is no need to suppose that the Gulf Stream washed its northern shores, for the temperature would then be raised in excess of what is required, but its aid may be called in to account for the even warmer previous periods evidenced by the older growths of Gleichenia and cycads.

It does not necessarily follow that cold did not then exist towards the Pole. Disko is 20° distant from it, and with an inclosed polar sea we should have a rapid lowering of temperature on the northern shores of the wide belt of land, and might have even a frozen ocean, perhaps as at present, with outlets on the side of Behring's Straits. The assumption that forests stretched to the Poles is not supported by the evidence.

The high temperature in these latitudes would be confined to the Atlantic; and that it was under the same laws as at present seems a reasonable supposition, since the American area even then maintained a relative coolness on account probably of the return and cooler currents being sheared to the west by the rotation of the earth.

To recapitulate. I believe the evidence to be in favour of the eocene age of the Arctic floras in question, and not miocene. I think that the temperature acquired by the plants—especially taking into consideration that their affinities with genera belonging to temperate regions is only inferred upon, in many cases, indistinct fragments—may have been over-estimated. There is no inherent impossibility indeed, that these extinct forms may not be the relics of a flora, like our present Arctic flora, specially adapted to bear a rigorous clime, and colour is lent to this by the abundance of the extinct McClintockia, about whose affinities we know nothing—a flora, perhaps, merely requiring the protecting cover of snow and sea-fog during winter. Finally, I believe that a comparatively slight change in the relative distribution of land and water, such as I have described, would alone account by itself for any fluctuations of temperature, of which we have any record preserved, in, at least, the tertiary rocks.

It does not come within the scope of the present subject, but it is worth consideration, whether wider channels still than those we now possess—some flowing from a more easterly point, so that our land might form the western coast of such a current—would not produce a glacial epoch, intensified by the absence of the Gulf Stream when there was no connecting isthmus (of which there is evidence in recent days) between the two Americas. The present distribution seems, at all events, one productive of more than average cold, as we become aware through the geological record, for the many and wide-existing channels conduct the Arctic waters south, and lower the general temperature of the ocean even to the Tropics.

J. STARKIE GARDNER

ON GAUSSIN'S WARNING REGARDING THE SLUGGISHNESS OF SHIP'S MAGNETISM[1]

Practical Rule and Caution

1. AFTER steering for some time on westerly courses expect—

1. (*a*) Westerly error if you turn to the north;

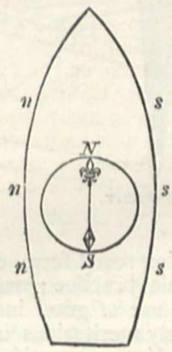
1. (*b*) Or easterly error if you turn to the south.

2. After steering for some time on easterly courses expect—

2. (*a*) Easterly error if you turn to north;

2. (*b*) Or westerly error if you turn to the south.

The diagram representing case 1 (*a*) illustrates the physical explanation, N and S representing the north and



south points of the compass card (or *true south* and *true north* poles of its needles), and the small letters, *s*, *s*, *s*, true southern polarity, and *n*, *n*, *n*, true northern polarity, induced in the port and starboard ends of deck beams and port and starboard sides of ship while steering east, and remaining for some time after she has been turned to north.

In the "Admiralty Compass Manual" Gaussin's warning is given with reference to the direction of swinging, in correcting the compass by magnets according to Airy's

[1] Being an abstract of a Communication by Sir Wm. Thomson, F.R.S., to Section A of the British Association at its last meeting (Dublin).

first method. In the Reports of the Liverpool Compass Committee and in Mr. Towson's "Information for Masters and Mates regarding Ship's Magnetism," instances of perplexing changes in the compass are given, and are referred to the same cause. The "sluggishness" of ship's magnetism, according to which it depends generally in part on the influence experienced some time before the time of observation, and not wholly on the influence at the time, seems to have been first definitely noticed and discussed scientifically by Sir Edward Sabine in his analysis of the results of the magnetic observations in the Antarctic Exploring Expedition of Sir James Ross in the *Erebus* and *Terror*, in the years 1840-41.

The practical rule and caution given above is of great importance in the navigation of iron ships. The amount of the error which may be found cannot be predicted for ships in general, nor for any particular ship except after much experience and careful observation. A small effect of two or three degrees,[1] such as that referred to in the Admiralty Manual as found in M. Gaussin's experience, may be observed in the course of quietly swinging a ship by hawsers or steam-tugs. If the ship under way is steamed round on the different courses the amount of the "Gaussin error" may generally be greater than if she is hauled round by warps; but we must not be sure that it will be so, because the *shake* of the screw which enhances the magnetisation on the east or west courses may shake it out again before the observation is made on the north or south courses.

A good practical rule in correcting the compass is, after having got it quite correct on the north and south courses, correct just half the error which is found after that on the south and north course, in the regular swinging of the ship.

The warning at the head of this article is particularly important for ships of war after firing guns when on easterly or westerly courses; if the course is then changed to north or south, and particularly if, after the firing, the change of course is effected under canvas, without the shaking of the ship's magnetism produced by the engines and screw.

The warning is also very important for ships steaming through the Mediterranean eastwards or westwards, and then turning south through the Suez Canal or north round Cape St. Vincent; and for ships steaming eastwards from America and then turning northwards or southwards into St. George's Channel.

MATHEMATICAL DRAWING INSTRUMENTS[2]

IN his preface the author states that we nowadays expect to find somewhere in print an account of the little mysteries of any particular art, and that partly with the hope of enabling this expectation to be fulfilled, and partly to meet the constant inquiries made respecting certain of the more complicated instruments manufactured by him, he has written his book. The author offers as an apology for any shortcomings in his work, that he is conscious his powers are greater with the lathe and file than in the ways of gentle rhetoric. In our opinion this is unnecessary; we would rather have had the file marks more distinct, and the technical details of construction not so carefully polished out in the work before us as in the well-finished instruments for which the author is so well known. The drawing instruments in most common use, pen, compasses, and dividers, are first described, and the patterns most recommended are illustrated; the type of these instruments now in use seems to meet all requirements, and if of the best quality, appears to require little or no improvement; the needle-point, however, shown on p. 34 (Fig. 1), adapted to compasses or pricker, is an improvement in steadiness on the old form, which was always liable to a little play. The earlier chapters will probably be of use to the beginner in facilitating his choice of the requisite instruments for his work, but he must recollect that dexterity in their use,

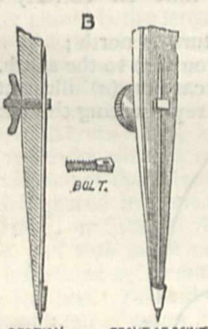


FIG. 1.

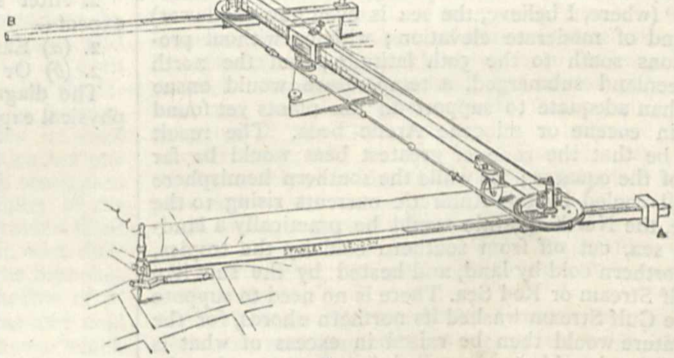


FIG. 2.

even if of the most improved form, can only be acquired with very considerable practice; much time and temper may be saved by the use of good instruments, and there is nothing particularly meritorious in the production of good work with bad instruments if good ones are within his reach.

Of the more complicated instruments next described, some must be regarded rather as mechanical curiosities than of every-day use; others, however, are indispensable where accuracy and the saving of time are of importance; as chief among these we select the eidograph and planimeter. A plan can be reduced or enlarged by dividing it into small squares and filling the details contained in each square into the corresponding squares ruled on the sheet prepared for the copy. This is a tolerably rapid process when the plan is simple in character, and with the help of proportional compasses a good draughtsman may attain considerable accuracy, but for a complicated plan or where great exactness is required, either the pentegraph or eidograph is indispensable. The author justly expresses astonishment at the little use at present made of the eidograph as compared with the pentegraph; the latter as made in this country appears for large work a most clumsy contrivance, offering much unavoidable resistance to motion, and even if made on the most improved Continental pattern is much less handy than the former. The eidograph, as improved by the author, is shown in the cut (Fig 2).

[1] Much greater effects than this are actually found in the cases of gun-practice and of long steaming on easterly or westerly courses referred to below.

[2] "Mathematical Drawing Instruments," by William Ford Stanley. (London: E. and F. N. Spon, 1878.)

The main bar is supported and turns on the pivot carried by the triangular weight; its position with respect to the pivot is adjusted by sliding longitudinally in the box fitted with clamping screw and vernier. On vertical pins at the ends of the main bar turn the two equal pulleys shown; attached to these on their under sides are small boxes also fitted with verniers for the longitudinal adjustment of the two transverse bars. On the similarly situated ends of the transverse bars the tracer and pencil point are carried. When the permanent adjustment of the instrument has been made the transverse arms are parallel, and the pulleys being of equal sizes any rotation given to one communicates an equal rotation to the other by means the flat steel band passing tightly round both; thus the parallelism of the arms is maintained in any position. If now the temporary adjustments are so made that the ratio of the two parts into which the axis of the pivot divides the main beam is equal to ratio of the lengths of the corresponding transverse arms measured from the axes of the pulleys to the pencil and tracer, it is evident that each of those latter is at the apex of a similar triangle, and that the line joining them passes through the axis of the main pivot. Thus the path described by the pencil point is similar to that described by the tracer. The graduations on the bars provides the means of setting the instrument in the required ratio. In the old form the distance between the axes of the pulleys was divided into 200 equal parts, the graduations reading each way from the centre. The transverse arms were made of equal length divided into 200 parts, also reading each way from the centre. For enlargement the setting would be on one side of the centre in each of the three bars and for reducing on the other side. In the improved form shown the tracer and pencil are made interchangeable, and thus the graduation on one half only of each bar is required, while at the same time part of the half arm, B, is dispensed with, making the instrument more handy. In the figure the instrument is set for reducing. The setting is obtained as follows:—Let $\frac{a}{A}$ be the ratio of the scales of the original and reduced plan, and x the reading on the graduations, then for the similar triangles we have $\frac{100 - x}{100 + x} = \frac{a}{A}$, or $x = 100 . \frac{A - a}{A + a}$. The chief improvements introduced by the author in the construction of the eidograph consist in making the pencil and tracer interchangeable, which is a considerable simplification, and the introduction of the small roller under the larger arm of the main beam. The improved instrument is stated to be capable of making a reduction down to one-eighth, while the old form certainly became unmanageable at anything beyond one-third.

There is perhaps no instrument whose true value is so little known in the drawing-office as Amsler's Polar Planimeter. The accurate measurement of an area bounded by curved or irregular lines is daily required; and although this can be effected readily and correctly by the aid of the polar planimeter, it is usually laboriously performed by cutting up the area into triangles whose areas are separately determined, or by the measurement of ordinates.

The instrument may be described with the assistance of the figure (Fig. 3). The weight retaining the pin below it at a fixed point, forms the centre about which the more distant arm revolves; to the other extremity of this arm is pivoted a rod carrying a tracing-point at its free extremity. A small roller is mounted on this rod so that its axis is in a line passing through the tracer and pivot at its ends. The roller is provided with a thin projecting edge and is retained in contact with the paper and free to rotate on its axis during any motion given to the instrument. The rotation of the roller is read off from the graduations on its rim by means of a vernier, the number of whole revolutions being shown on the small dial driven by a worm wheel and screw-pinion on the roller-axle. Any motion on the surface of the paper that is given to the point of contact of the roller is resolved into two components, one at right angles to the axis of the roller which is recorded by the dial and vernier readings, and the other parallel to the axis which is a sliding of the roller longitudinally, and is not recorded. To measure the area inclosed by a boundary line as shown in the illustration, the tracing-point is adjusted to any point of the boundary, the dial and wheel are then read off; the tracing-point is then carried round the boundary-line, carefully following it throughout until the starting-point is again reached. The dial and roller are then read off, and the difference of the readings gives the actual area in square inches, or any other units for which the instrument has been graduated. We may now attempt an explana-

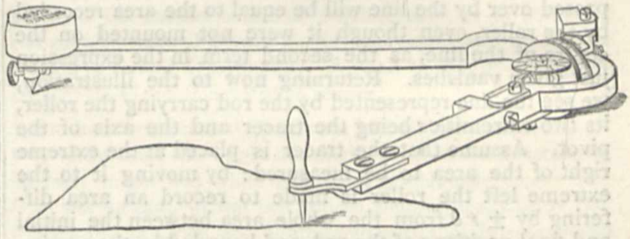


FIG. 3.

tion of the principle of the instrument. Consider first the motion of a straight line parallel to itself. The motion of the centre of the line is compounded of a motion at right angles to it, and one in the direction of its length. The area passed over by the line is equal to its length multiplied by the distance travelled by the centre at right angles to its length.

If, however, the line be moved, not parallel to itself, but into any other position, it could have been made to reach this position by first moving parallel to itself until its centre reached its new position, and thus, by a rotation of the line about its centre as a fixed point, it could be made to assume the position sought. If a figure representing this be drawn it will be seen that, when the movement is small, the area passed over by the line is approximately equal to its length, multiplied by the per-

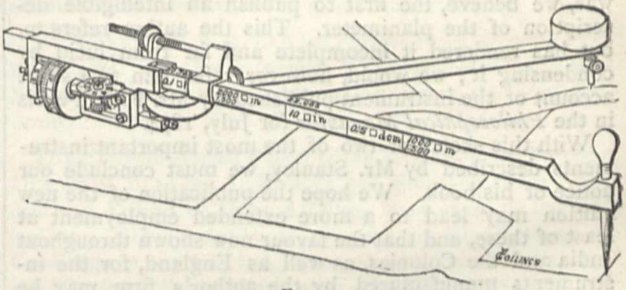


FIG. 4.

pendicular distance traversed by its centre, as before, and that, when the movement is diminished indefinitely, the area described is in the limit equal to the length multiplied by the perpendicular distance traversed. Now let a roller be mounted at the centre of the line, so as to rotate about it as axis, and let it be so graduated on the rim that the length of one division, multiplied by the length of the line, is equal to a unit of square measure. Then, as the line passes from one position to another, parallel to the plane of the paper, the roller will record the sum of the separate infinitesimal movements of the line at right angles to its length, and will thus, by the graduations on its rim, read off, say, at the point of contact with the paper, give the area passed over by the line. Let l = the length of the line, Δx = the perpendicular distance travelled by the roller. Then, in the limit, $l\,dx$ = the element of area described. In passing over a finite area this will equal $\int l\,dx = lx$, where x is the whole distance recorded by the roller. If now the roller be mounted at a distance r from the centre of the line, in traversing the element of area as before, it will first record $l\,dx$ in its motion parallel to itself, and then, as the line turns about its centre, it will either add or subtract from that reading an amount corresponding to the arc of the circle of radius r, through which it turns; the roller will thus show $l\,dx \pm r\,d\theta$, where $d\theta$ is the small angle turned through. In traversing a finite area this will record $lx \pm r\theta$ where θ is the angle between the initial and final positions of the line. It is now obvious that if the initial and final positions of the line are parallel or coincident, the area passed over by the line will be equal to the area recorded by the roller, even though it were not mounted on the centre of the line, as the second term in the expression just given vanishes. Returning now to the illustration, we see the line represented by the rod carrying the roller, its two extremities being the tracer and the axis of the pivot. Assume that the tracer is placed at the extreme right of the area to be measured: by moving it to the extreme left the roller is made to record an area differing by $\pm r\theta$ from the whole area between the initial and final positions of the rod, and bounded by the portion of the circle described by the further extremity of the rod and by that portion of the boundary of the area traversed, r and θ having the meanings previously assigned to them. Let the tracer be now carried round the boundary back to its starting point on the other side of the area; the roller will now revolve the reverse way, and will subtract from its previous reading an area differing by $\mp r\theta$ from the area contained between the two positions of the rod, the arc described by the pivot and the portion of boundary traversed. It is now apparent that the reading of the roller gives the difference of these areas, which is that of the figure required (Fig. 4).[1] A different form of the polar planimeter is shown in the second illustration, and is provided with an adjustment for varying the effective length of the arm carrying the tracer, by which means the dial and the graduations on the roller are made to show the area to different scales. Mr. F. J. Bramwell was, we believe, the first to publish an intelligible description of the planimeter. This the author refers to, but has rendered it incomplete and far from lucid by condensing it; we would, however, refer him to a short account of the instrument published by Mr. F. P. Purvis in the *Philosophical Magazine* for July, 1874.

With this sketch of two of the most important instruments described by Mr. Stanley, we must conclude our notice of his book. We hope the publication of the new edition may lead to a more extended employment at least of these, and that the favour now shown throughout India and the Colonies, as well as England, for the instruments manufactured by the author's firm may be sufficient inducement to keep them up to their present high standard.

[1] It is here assumed for simplicity sake that the points at which the motion of the roller is reversed are at the extreme right and left of the figure.

NOTES

THE announcement that Dr. W. B. Carpenter is about to retire from the post of Registrar of the University of London will be received with general regret. He has filled the office for twenty-three years.

PROF. MAREY has been elected to fill the place of the late Claude Bernard in the Section of Medicine in the Paris Academy of Sciences.

THE tenth annual report of the U.S. Geological and Geographical Survey of the Territories, in charge of Prof. Hayden, will be ready for distribution in a few weeks. The report has been in type nearly a year, but has been delayed on account of the engraving of the plates. These are now completed and the report will be issued at once. It contains 546 closely printed pages octavo, with eighty plates, sections, maps, &c. Fifty of the plates illustrate the remarkable cliff dwellers in Southern Colorado and Northern New Mexico. This is the last annual report pertaining to Colorado and contains a very interesting series of chapters on the geology of that remarkable country. On the whole this report will prove one of great popular interest and ought to have been published in great numbers. Only 4,500 copies have as yet been ordered. About 250 pages of the eleventh annual report of the field work for 1877 are in type at the Public Printing Office. This will contain a detailed description of the geological and geographical features of Southern Wyoming and Idaho. The reports of Sir Joseph D. Hooker and Dr. Asa Gray will give this volume a high character as well as great popular interest. 10,000 copies have been ordered by Congress. There will be very extended geological reports by Messrs. Endlich, White, St. John, and Peale, and geographical reports by Messrs. Gannett and Wilson, and special reports by Leidy, &c., &c.

VARIOUS items on electric lighting are to hand. It is telegraphed from Washington, December 7, that Mr. Edison's application for a patent for his electric light has been favourably passed by the Patent Office, and that the letters patent were to be issued on Tuesday. The *Journal* of the Society of Arts for December 6 contains a useful *résumé* of the practical application of electricity to lighting purposes, by Mr. J. N. Shoolbred. A new system of electric lamp has been invented in Paris and will be experimented on shortly in public. The carbon rods are four in number, as in the Rapieff system, but instead of crossing at an angle they are arranged in two parallel lines. The consumption of carbon for electric lighting is increasing so rapidly that M. Carré, the well-known Paris maker, is extending his works. He is manufacturing now at the rate of 2,000 meters a day. The electric light illumination has been prolonged, by a vote of the Municipal Council of Paris, up to January 19, for the Avenue de l'Opéra and the front of the Legislative Palace. The city engineers have received orders to prepare, during the interval, a report on the several systems which are now in operation or may be proposed.

THE New York papers report that Mr. Edison has stated that he has made an improved receiver for his telephone by means of which persons standing 15 feet from the instrument can hear a whisper uttered miles away.

AFTER the masterly works of Tschudi on "Animal Life in the Alps," and of Heer on the "History of Vegetation in Switzerland," another work likely to be of high value is announced,—H. Christ on the plants of that country—"Das Pflanzenleben in der Schweiz." The interest of the work is all the greater that Switzerland contains on its narrow area nearly all the diversity of plants which grow in middle and northern Europe. Many years' research of the author in the field, his previous works on separate parts of the Alps, as well as his connection with the botanists of Europe, have enabled Dr. Christ to publish a work which may be expected to range with those above-mentioned. It will appear in four fascicules, with many illustrations, and four maps of vegetable zones, one of which, the distribution of grapes and of several plants of the Föhn and lake regions, will appear this month. The whole work will be finished about the spring of 1879, the first half fascicule having just appeared.

A NEW Botanical Society has just been formed at Munich. the president is Prof. Robert Hartig, and the vice-president Dr. Arnold, an eminent lichenologist.

HERR ALBERT KÜPPERS, an eminent sculptor at Bonn, has just finished the model of a statue to be erected in memory of Prof. Jacob Nöggerath, the well-known mineralogist.

THE Chinese are about to commence the erection of a line of telegraph from Tientsin to Taku at the mouth of the Pei-ho, and also to make the necessary surveys for another line between Tientsin, Paoting-fu, and Peking.

ACCORDING to the *Colonies and India*, the surveyor, in making a survey of the new road at Mohikinui, in Buller County, New Zealand, struck a coal-seam five feet thick at a distance of only two miles from the township. The coal is bituminous and excellent in quality; it is, moreover, easily accessible and can be brought to the port at a very small cost.

THE Council of the Society of Arts have addressed a memorial to Lord Salisbury asking him to request her Majesty's Ministers abroad to collect information on the system of technical and industrial education in foreign countries, in continuation of what was published in 1868. Lord Salisbury has promised to give the matter his consideration, though we think this is rather an unfortunate time to address the Foreign Secretary on so peaceful a subject.

IN reference to the statement that Sir William Armstrong has employed electricity, generated by water power, a mile and a half distant, to light his picture gallery, and that he proposes to use the same force to turn machinery about his house, Mr. D. R. Jones sends us a copy of a letter sent by him to the *Australasian*, October 25, 1870, in which, referring to the phenomena of dynamic electricity, he states that it is evident that motion may be transformed into electricity, and *vice versâ*. He suggests that we may have here "the means of utilising the motions of the air and of water, which, for want of means of transmission, have been hitherto allowed to run to waste. Various methods of converting, transmitting, and utilising force will readily suggest themselves as a combination of these well-known facts. While we have such superabundant constant supplies of force it is not right that the stores of Nature should be ransacked."

THE *Times* of Monday contains another letter from Dr. Schliemann, giving an account of his further excavations on the site of Troy. He has succeeded in exploring much of the remains of the ancient structures so that the plan can be very distinctly traced. Dr. Schliemann himself left London on Monday for Paris, and intends to recommence work on March 1. A number of the objects found during the last explorations have been deposited in the South Kensington Museum, where they will be exhibited.

THE *Midland Naturalist*, we are glad to see, has concluded a successful first volume. Its past twelve numbers contain many papers of value, both on local and general scientific matters, and its conduct is creditable to its editors and to the many societies of which it is the organ. The December number contains a carefully compiled index, which must be a great comfort to those in search of any paper or subject in the volume. Several good contributions are promised for the next volume, and we trust that the journal will receive every encouragement from the members of the societies it represents, and that its conductors will strive to make it thoroughly representative of the science of the Midlands.

A CORRESPONDENT "R. C. J.," writes as follows from Driefontein, Heilbronn District, Orange Free State, South Africa, under date October 14:—"Our last winter was dry throughout, and unusually cold, that is, June, July, and August, and on August 3 a piece of country in the Transvaal, about seventy or eighty miles north of this part, and on the road from this to Pretoria, about fifteen or twenty miles wide, and perhaps the same in length, was visited in twenty-four hours with such a sudden change of temperature, from 85° to 42° F., that more than 100 bodies of dead Kafirs, besides oxen, were found as if killed by the sudden abstraction of caloric. There was no wind or rain, but a fall of snow. The land is about as high as this, about 4,600 feet."

EXCAVATIONS are now in progress on the Limburg, in the Bavarian palatinate, which will lead to important results for prehistoric investigation, inasmuch as they are directed to the elucidation of the much contested question regarding the constructors and former inhabitants of the Ringwall near Dürkheim.

La Semaine Française is the title of a new weekly French paper published in London for English readers, and which is meant to appeal "to all those who wish to read good French in the way in which it is most likely to be read with interest and profit." The number before us is well selected as to contents, and contains news of French matters and expressions of French opinion in various departments. A small amount of space, we are pleased to see, is devoted to science.

DR. RAE writes that at about 14m. past midnight of December 5-6, whilst there was bright moonlight, he observed a meteor of intensely bright and white light passing obliquely downwards from west to east. It was first noticed almost directly below the western foot of Orion, and disappeared when slightly to eastward of Sirius, having passed at 3° or 4° of arc below these stars. It was spherical and apparently of 6′ or 8′ diameter, with a fiery red tail four or five times that length.

AN earthquake occurred in Scotland on Tuesday morning last week at Balnacara and other parts of the district of Loch Alsh, on the west coast of the county of Ross, opposite the Isle of Skye. The shock was very marked, the tremulous motion of the earth being distinctly felt, and the houses shaking violently At Balnacara the shock occurred at 5 o'clock, and at Plocton, five miles distant, between 7 and 8.

AN Indian paper states that in the Ferozepore district the rise of the Sutlej has once more broken the head-works of the inundation canals, and over 100 square miles of country are under water. The damage done to property has been great, but on the other hand a quantity of treasure has been uncovered by the floods in the old fort of Momdote, a few miles from Ferozepore.

MR. CORNELIUS WALFORD has reprinted in a separate form his elaborate and valuable paper on "The Famines of the World, Past and Present," read in March last before the Statistical Society.

THE Eleventh Annual Report of the Trustees of the Peabody Museum of American Archæology and Ethnology to the President and Fellows of Harvard College is an unusually interesting one. The description of the new museum buildings at Cambridge is very full, and illustrated with plans and a photographic view; the building seems admirably adapted to the purpose for which it is intended. Besides an account of the additions to the Museum and the work done since the last Report, the present publication contains the following papers:—"Second Report on the Implements found in the Glacial Drift of New Jersey," by Dr. C. C. Abbot; "The Method of Manufacture of several Articles by the former Indians of South Carolina," by Mr. Paul Schumacher; "Cave Dwellings in Utah," by Mr. Edward Palmer; "Notes on a Collection from an Ancient Cemetery in Southern Peru," by Mr. J. H. Blake; "Archæological Explorations in Tennessee," by Mr. F. W. Putnam (this long and amply illustrated paper is separately reprinted); "Observations on the Crania from the Stone Graves in Tennessee," by Mr. Lucien Carr; "On the Tenure of Land Among the Ancient Mexicans," by Mr. A. F. Bandelier. Besides Mr. Putnam's paper most of the others are accompanied by numerous illustrations.

IN a note on "Colonial Grasses as Paper-making Materials," the *Colonies and India* suggests the possibility of utilising some of the coarse grasses which grow with such provoking pertinacity in South Africa, Australia, New Zealand, &c. The *Typha angustifolia*, for example, a large kind of tussock grass (known as *raupo* to the New Zealand natives, who use it for thatching their houses), which grows in enormous quantities in the swampy flats near rivers and lakes, may, like its neighbour, the *Phormium tenax*, prove a rival to Esparto grass; the *wiwi*, a coarse, wiry kind of grass, growing chiefly in the interior of North Island, is also worth an experiment. In New South Wales the grass-cloth plant (*Böhmeria nivea*) has already received some attention, being used for the manufacture of a fine kind of matting. South Africa is probably richest of all in its grasses; in the great Karroo district thousands of square miles are covered with the twaa-grass, the sour-veldt, and the sweet-veldt, the importance of which as fodder may be found equalled by their value as paper-making material. Still more likely to prove valuable is the *Stipa capensis*, a member of the family to which Esparto belongs.

THE *Transactions* of the Cumberland Association for the Advancement of Literature and Science, Part III., 1877-78, edited by Mr. Clifton Ward, is a thickish volume containing papers by members of some of the Associated Societies. The first paper, however, after various reports, is that by Sir George Airy, on the "Probable Condition of the Interior of the Earth," a report of which we gave at the time of its delivery; accompanying it is a diagram of an ideal earth. Mr. Ward has a paper on "Quartz in the Lake District;" Mr. C. Smith one on "Boulder Clay;" Mr. Pickering, on a "Submerged Forest at St. Bees;" and Mr. Fisher Crosthwaite gives an interesting account of Peter Crosthwaite, who, at the end of the last and beginning of the present centuries, did much to promote science in the district.

M. J. POLIAKOFF, who was sent last summer by the St. Petersburg Academy of Sciences to examine the remains of the stone period in the governments of Yaroslaff and Vladimir, gives the following results of his explorations:—Very interesting collections were found in excavating a mound, close by Yaroslaff; numerous skulls of men of the neolithic period were discovered here, together with polished silex hatchets and hammers, and numerous bones of animals of existing species. Far richer collections were found in the valley of the Oka River, in the district of Murom. Here, in the sandy mounds of the valley, as well as in the alluvium of the river, M. Poliakoff has discovered immense quantities of silex implements, polished and rough, of the most varied forms. The implements were always found together with bones of the *Castor fiber*, the *Sus scrofa ferus*, and the *Bos primigenius*, none of which exist now in those regions. Besides, he also discovered vestiges of old wood buildings, very like the lacustrian dwellings of Switzerland. The most important discovery during these explorations was made by M. Poliakoff, in company with Count Uraroff, close by Karacharovo Town, in a very old lake alluvium, being a somewhat washed-up glacier deposit. Here they found rough stone implements of the paleolithic period, together with bones of the mammoth, rhinoceros, and the *Bos priscus*. The character of the deposits proved without doubt the co-existence of man with those extinct mammals in Russia, as well as in other parts of Europe. After having finished his explorations, M. Poliakoff made a journey in Western Europe to study the chief museums, and to compare the implements he has collected during many years in Russia and Siberia, Western and Eastern, with those of England, Sweden, Denmark, France, and Switzerland. We expect that this last journey of M. Poliakoff will accelerate the opening of the projected pre-historic museum at St. Petersburg.

THE additions to the Zoological Society's Gardens during the past week include two Black-faced Spider Monkeys (*Ateles ater*), two Rufous-vented Guans (*Penelope cristata*) from U. S. of Columbia, two Horsfield's Tortoises (*Testudo horsfieldi*) from Turkestan, presented by Mr. A. Gonzalez Carazo; a Green Monkey (*Cercopithecus callitrichus*) from West Africa, presented by Mr. A. G. Lytton Squires; two Black-eared Marmosets (*Hapale penicillata*) from South-East Brazil, presented by the Countess of Cotterham; two Laughing Kingfishers (*Dacelo gigantea*) from Australia, presented by Mr. Edwin A. B. Crockett; a Ceylon Jungle Fowl (*Gallus stanleyi*) from Ceylon, two Japanese Pheasants (*Phasianus versicolor*) from Japan, a Grey Francolin (*Francolinus ponticerianus*) from India, presented by Mr. Geo. Lyon Bennett; a Rhomb-marked Snake (*Psammophylax rhombeatus*), three Rufescent Snakes (*Leptodira rufescens*) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; a Kinkajou (*Cercoleptes caudivolvulus*) from South America, three Snow Buntings (*Plectrophanes nivalis*), European, purchased.

ROYAL SOCIETY—THE PRESIDENT'S ANNIVERSARY ADDRESS[1]

II.

THE modern development of botanical science, being that which occupies my own attention, is naturally that on which I might feel especially inclined to dwell; and I should so far have the excuse that there is, perhaps, no branch of research with the early progress of which this Society is more intimately connected.

One of our earliest secretaries, Robert Hooke, two centuries ago, laboured long and successfully in the improvement of the microscope as an implement of investigation. He was one of the first to reap the harvest of discovery in the new fields of knowledge to which it was the key, and if the results which he attained have rather the aimless air of spoils gathered hither and thither in a treasury, the very fulness of which was embarrassing, we must remember that we date the starting-point of modern histology from the account given by Hooke in his "Micrographia" (1667) of the structure of cork, which had attracted his interest from the singularity of its physical properties. Hooke demonstrated its *cellular structure*, and by an interesting coincidence he was one of the first to investigate, at the request, indeed, of the founder of the Society, Charles II., the movement of the sensitive plant *Mimosa pudica*, one of a class of phenomena which is still occupying the attention of more than one of our Fellows. In attributing the loss of turgescence, which is the cause of the collapse of the petiole and subordinate portions of the compound leaf which it supports, to the escape of a subtle humour, he to some extent foreshadowed the modern view which attributes the collapse of the cells to the escape of water by some mechanism far from clearly understood —whether from the cell-cavities or from the cell-walls into the intercellular spaces.

Hooke having shown the way, Nehemiah Grew, who was also secretary of the Royal Society, and Marcello Malpighi, Professor of Medicine in the University of Bologna, were not slow to follow it. Almost simultaneously (1671-3) the researches of these two indefatigable students were presented to the Royal Society, and the publication of two editions of Malpighi's works in London prove how entirely this country was at that time regarded as the head-quarters of this branch of scientific inquiry. We owe to them the generalisation of the cellular structure, which Hooke had ascertained in cork, for all other vegetable tissues. They described also accurately a host of microscopic structures then made known for the first time. Thus, to give one example, Grew figured and described in several different plants the stomata of the epidermis:—"Passports either for the better avolation of superfluous sap, or the admission of air."

With the exception of Leeuwenhoek no observer attempted to make any substantial addition to the labours of Grew and Malpighi for more than a century and a half, and however remarkable is the impulse which he gave to morphological studies, the view of Caspar Wolff in the middle of the eighteenth cen-

[1] Address of Sir Joseph Hooker, C.B., K.C.S.I., the President, delivered at the Anniversary Meeting of the Royal Society, on Saturday, November 30, 1878. Continued from p. 113.

tury (1759), in regarding cells as the result of the action of an organising power upon a matrix, and not as themselves influencing organisation, were adverse to the progress of histology. It is from Schleiden (1838), who described the cell as the true unit of vegetable structure, and Schwann, who extended this view to all organisms whether plants or animals, and gave its modern basis to biology by reasserting the unity of organisation throughout animated nature, that we must date the modern achievements of histological science. Seldom, perhaps, in the history of science has any one man been allowed to see so magnificent a development of his ideas in the space of his own lifetime as has slowly grown up before the eyes of the venerable Schwann, and it was, therefore, with peculiar pleasure that a letter of congratulation was intrusted by the officers to one of the Fellows of this Society on its behalf on the recent occasion of the celebration of the fortieth anniversary of Schwann's entry into the professoriate.

If we call up in our mind's eye some vegetable organism and briefly reflect on its construction, we see that we may fix on three great steps in the analysis of its structure, the organic, the microscopic, and the molecular, and, although not in the same order, each of the three last centuries is identified with one of these. In the seventeenth century Grew achieved the microscopic analysis of plant tissues into their constituent cells; in the 18th, Caspar Wolff effected the organic analysis (independently but long subsequently expounded by the poet Goethe) of plant structures into stem and leaf. It remained for Nägeli in the present century to first lift the veil from the mysterious processes of plant growth, and by his memorable theory of the molecular constitution of the starch-grain and cell-wall, and their growth by intussusception (1858), to bring a large class of vital phenomena within the limits of physical interpretation. Strasburger has lately (1876) followed Sachs in extending Nägeli's views to the constitution of protoplasm itself, and there is now reason to believe that the ultimate structure of plants consists universally of solid molecules (not, however, identical with chemical molecules) surrounded with areas of water which may be extended or diminished. While the molecules of all the inert parts of plants (starch-grains, cell-wall, &c.) are on optical grounds believed to have a definite crystalline character, no such conclusion can be arrived at with respect to the molecules of protoplasm. In these molecules the characteristic properties of the protoplasm reside, and are more marked in the aggregate mass in proportion to its denseness, and this is due to the close approximation of the molecules and the tenuity of their watery envelopes. The more voluminous the envelopes the more the properties of protoplasm merge in those of all other fluids.

It is, however, to the study of the nuclei of cells that attention has been recently paid with the most interesting results. These well-known structures, first observed by Ferdinand Bauer at the beginning of the century (1802), were only accurately described, thirty years later, by Robert Brown (1833). Up to the present time their function has been extremely obscure. The beautiful investigations of Strasburger (1875) have led him to the conclusion that the nucleus is the seat of a central force which has a kind of polarising influence upon the protoplasm molecules, causing them to arrange themselves in lines radiating outwards. Cell-division he regards as primarily caused by the nucleus becoming bipolar, and the so-called caryolitic figures first described by Auerbach exhibit the same arrangement of the protoplasm molecules in connecting curves as in the case of iron-filings about the two poles of a bar-magnet. The two new centres mutually retire, and each influencing its own tract of protoplasm, the cell-division is thereby ultimately effected. This is but a brief account of processes which are greatly complicated in actual detail, and of which it must be remarked that, while the interest and beauty of the researches are beyond question, caution must be exercised in receiving the mechanical speculations by which Strasburger attempts to explain them. He has himself shown that cell-division presents the same phenomena in the animal kingdom, a result which has been confirmed by numerous observers, amongst whom I may content myself with mentioning one of our own number, Mr. F. Balfour. Strasburger further points out that this affords an argument for the community of descent in animal and vegetable cells; he regards free cell-division as derivable from ordinary cell-division by the suppression of certain stages.

Turning now to the discoveries made during the last five years in physiological botany, we find that no one has advanced this subject so greatly as Mr. Darwin. In 1875 was published his work on insectivorous plants, in which he ascertained the fact that a number of species having elaborate structures adapted for the capture of insects, utilised the nitrogenous matter which these contain as food. The most important principle established in the course of these researches was that such plants as *Drosera*, *Dionæa*, *Pinguicula*, &c., secrete a digestive fluid, which has led, through Gorup Bezanez's investigations on the ferment in germinating seeds, to a recognition of the active agency of ferments in the transmission of food-material, which marks a great advance in our knowledge of the general physiology of nutrition.

The extreme sensitiveness of the glands of *Drosera* to mechanical and chemical stimulus (especially to phosphate of ammonia), the directive power of its tentacles, depending upon the accurate transmission of motor impulses, and the "reflex" excitation of secretion in the glands, were all discoveries of the most suggestive nature in connection with the subject of the irritability and movements of plants. The phenomenon of the aggregation of the protoplasmic cell-contents in the tentacles of *Drosera* is a discovery of a highly remarkable nature, though not yet thoroughly understood. Lastly, Mr. Frank Darwin, following his father's footsteps, as it were crowned the edifice by showing to what an extent insectivorous plants do profit by nitrogenous matter supplied to their leaves.

In close relation to these researches are those, also by Mr. Darwin, on the structure and functions of the bladder of *Utricularia*, which he has shown to have the power of absorbing decaying animal matter; and those of Mr. Frank Darwin on contractile filaments of extraordinary tenuity attached to the glands on the inner surface of the cups formed by the connate bases of the leaves of the teasel, and which filaments exhibit motions suggesting a protoplasmic origin. It is to be hoped that their discoverer will pursue his investigations upon these curious bodies, whose origin and real nature in relation to the plant and its functions is involved in obscurity.

The subject of the cross-fertilisation of plants, which, though a long-known phenomenon, first become a fruitful scientific study in Mr. Darwin's now classical work, "On the Various Contrivances by which Orchids are Fertilised," has within the last few years made rapid advance under its author's hand. The extreme importance of avoiding self-fertilisation might indeed be inferred from the prevalence in flowers of elaborate contrivances for preventing it; but it remained to be shown that direct benefit attended cross-fertilisation, and this has now been proved by an elaborate series of experiments, the results of which are not only that both increased fertility or greater vigour of constitution attend cross-fertilisation, but that the opposite effects attend self-fertilisation. In the course of these experiments it became evident that the good effects of the cross do not depend on the mere fact of the parents being different individuals, for when these were grown together and under the same conditions, no advantage was gained by the progeny; but when grown under different conditions a manifest advantage was gained. As instances, if plants of *Ipomœa* and *Mimulus*, which had been self-fertilised for seven previous generations, were kept together and then intercrossed, their offspring did not profit in the least; whereas, when the parent plants were grown under different conditions, a remarkably vigorous offspring was obtained.

Mr. Darwin's last work, "On the Different Forms of Flowers," though professedly a reprint of his paper on dimorphic plants, published by the Linnean Society, contains many additions and new matter of great importance in reference to the behaviour of polygamous plants, and on cleistogamic flowers. Among other points of great interest is the establishment of very close analogies between the phenomena attending the illegitimate union of trimorphic plants and the results of crosses between distinct species: the sterile offspring of the crosses of the same species exhibiting the closest resemblance to the sterile hybrids obtained by crossing distinct species; while a whole series of generalisations, founded on the results of the one series of experiments, are closely paralleled by those founded on the other. The bearing of this analogy on the origin of species is obviously important.

Besides these investigations, Mr. Darwin has produced within the last five years second editions of his volume on the "Fertilisation of Orchids," and on the "Habits and Movements of Climbing Plants," as also of his early works on "Coral Reefs," and "Geological Observations in South America;" all of them abounding in new matter.

Of special interest to myself, as having been conducted in the

Jodrell Laboratory at Kew, are Dr. Burdon Sanderson's investigations on the exceptional property possessed by the leaves and other organs of some plants which exhibit definite movements in response to mechanical, chemical, or electric stimuli. In 1873 this physiologist showed us in our meeting-room that the closing of the laminæ of the leaf of *Dionæa* is preceded by a preliminary state of excitement, and is attended with a change in the electric conditions of the leaf; and this so closely resembled the change which attends the excitation of the excitable tissues of animals that he did not hesitate to identify the two phenomena.

This remarkable discovery immediately directed the attention of two German observers to the electromotive properties of plants, one, Dr. Kunkel, in the laboratory of Prof. Sachs; the other, Prof. Munk, in that of the University of Berlin.

Prof. Munk, whose researches are of much the greater scope and importance, took as his point of departure Dr. Burdon Sanderson's discovery. The leading conclusion to which he arrived was that in *Dionæa* each of the oblong cells of the parenchyma is endowed with electromotive properties which correspond with those of the "muscle-cylinder" of animals; with this exception, that whereas in the muscle-cylinder the ends are negative to the central zone, in the vegetable cell they are positive; and he endeavours to prove that according to this theory all the complicated electromotive phenomena which had been observed could be shown to be attributable to the peculiar arrangement of the leaf-cells.

During the last two summers Dr. Burdon Sanderson has been engaged in endeavouring to discover the true relations which subsist between the electrical disturbance followed by the shutting of the leaf-valves of *Dionæa* and the latent change of protoplasm which precedes this operation. He has found that though the mechanism of the change of form of the excitable parenchyma which causes the contraction is entirely different from that of muscular contraction, yet that the correspondence between the exciting process in the animal tissues and what represents this in plant tissues appears to be more complete the more carefully the comparison is made; and that whether the stimulus be mechanical, thermal, or electrical, its effects correspond in each case. Again, the excitation is propagated from the point of excitation to distant points in the order of their remoteness, and the degree to which the structure is excited depends upon its temperature. Notwithstanding, however, the striking analogies between the electrical properties of the cells of *Dionæa* and of muscle-cylinders, Dr. Burdon Sanderson is wholly unable to admit with Prof. Munk that these structures are in this respect comparable.

In morphological botany attention has been especially directed of late to the complete life-history of the lower order of cryptogams, since this is seen to be more and more an indispensable preliminary to any attempt at their correct classification.

The remarkable theory of Schwendener, now ten years old, astonished botanists by boldly sweeping away the claims to autonomous recognition of a whole group of highly characteristic organisms—the lichens—and by affirming that these consist of ascomycetal fungi united in a commensal existence with algæ. The controversial literature and renewed investigations which this theory has given rise to is now very considerable. But the advocates of the Schwendenerian view have gradually won their ground, and the success which has attended the experiments of Stahl in taking up the challenge of Schwendener's opponents, and manufacturing such lichens as *Endocarpon* and *Thelidium*, by the juxtaposition of the appropriate algæ and fungi, may almost be regarded as deciding the question. Sachs, in the last edition of his "Lehrbuch," has carried out completely the principle of classification of algæ, first suggested by Cohn, and has proposed one for the remaining thallophytes, which disregards their division into fungi and algæ. He looks upon the former as standing in the same relation to the latter as the so-called saprophytes (*e.g. Neottia*) do to ordinary green flowering-plants.

This view has especial interest with regard to the minute organisms known as *Bacteria*, a knowledge of the life-history of which is of the greatest importance, having regard to the changes which they effect in all lifeless and, probably, in all living matter prone to decomposition. This affords a morphological argument (as far as it goes) against the doctrine of spontaneous generation, since it seems extremely probable that just as yeast may be a degraded form of some higher fungus, *Bacteria* may be degraded allies of the *Oscillatoriæ*, which have adopted a purely saprophytal mode of existence.

Your *Proceedings* for the present year contain several important contributions to our knowledge of the lowest forms of life. The Rev. W. H. Dallinger, continuing those researches which his skill in using the highest microscopic powers and his ingenuity in devising experimental methods have rendered so fruitful, has adduced evidence which seems to leave no doubt that the spores or germs of the monad which he has described differ in a remarkable manner from the young or adult monads in their power of resisting heated fluids. The young and adult monads, in fact, were always killed by five minutes' exposure to a temperature of 142° F. (61° C.), while the spores germinated after being subjected to a temperature of 10° above the boiling-point of water (222° F.).

Two years ago, Cohn and Koch observed the development of spores within the rods of *Bacillus subtilis* and *B. anthracis*. These observations have been confirmed, with important additions, in these two species by Mr. Ewart, and have been extended to the *Bacillus* of the infectious pneumo-enteritis of the pig, by Dr. Klein; and to *Spirillum* by Messrs. Geddes and Ewart; and thus a very important step has been made towards the completion of our knowledge of the life-history of these minute but important organisms. Dr. Klein has shown that the infectious pneumo-enteritis, or typhoid fever of the pig, is, like splenic fever, due to a *Bacillus*. Having succeeded in cultivating this *Bacillus* in such a manner as to raise crops free from all other organisms, Dr. Klein inoculated healthy pigs with the fluid containing the *Bacilli*, and found that the disease in due time arose and followed its ordinary course. It is now, therefore, distinctly proved that two diseases of the higher animals, namely, "splenic fever" and "infectious pneumo-enteritis," are generated by a *contagium vivum*.

Finally, Messrs. Downes and Blunt have commenced an inquiry into the influence of light upon *Bacteria* and other *Fungi*, which promises to yield results of great interest, the general tendency of these investigations leaning towards the conclusion that exposure to strong solar light checks and even arrests the development of such organisms.

The practical utility of investigations relating to *Bacillus* organisms as affording to the pathologist a valuable means of associating by community of origin various diseases of apparently different character, is exemplified in the "Loodiana fever," which has been so fatal to horses in the East. The dried blood of horses that had died of this disease in India has been recently sent to the Brown Institution, and there afforded seed from which a crop of *Bacillus anthracis* has been grown, which justified its distant pathological origin by reproducing the disease in other animals. Other equally interesting experiments have been made at the same Institution, showing that the "grains" which are so largely used as food for cattle, afford a soil which is peculiarly favourable for the development and growth of the spore filaments of *Bacillus*; and that by such "grains" when inspected, the anthrax fever can be produced at will, under conditions so simple, that they must often arise accidentally. The bearing of this fact on a recent instance in which anthrax suddenly broke out in a previously uninfected district, destroying a large number of animals, all of which had been fed with grains obtained from a particular brewery, need scarcely be indicated.

In systematic botany, which, in a nation like ours, that is ever extending its dominions and exploring unknown regions of the globe, must always absorb a large share of the energies of its phytologists, I can but allude to two works of great magnitude and importance.

Of these the first is the "Flora Australiensis" of Bentham, completed only a year ago; a work which has well been called unique in botanical literature, whether for the vast area whose vegetation it embraces (the largest hitherto successfully dealt with), or for the masterly manner in which the details of the structure and affinities of upwards of 8,000 species have been elaborated; its value in reference to all future researches regarding the geographical distribution of plants, the southern hemisphere, and the evolution therein of generic and specific types, cannot be over-estimated.

The other great work is the "Flora Braziliensis," commenced by our late foreign Fellow, von Martius, and now ably carried on by Eichler, of Berlin, assisted by coadjutors (amongst whom are most of our leading systematists) under the liberal auspices of His Majesty the Emperor of Brazil. When completed, this gigantic undertaking will have embraced, in a systematic form, the vegetation of the richest botanical region of the globe.

Having now endeavoured to recall to you some of the great advances in science made during the last few years, it remains for me, after the distribution of the medals awarded by your Council, to retire from the Presidency in which I have so long experienced the generous support of your officers and yourselves. This support, for which I tender you my hearty thanks, together with my sense of the trust and dignity of the office, and the interest attached to its duties, has rendered my resignation of it a more difficult step than I had anticipated. My reasons are, however, strong. They are the pressure of official duties at Kew, which annually increase in amount and responsibility, together with the engagements I am under to complete scientific works, undertaken jointly with other botanists, before you raised me to the Presidency, and the indefinite postponement of which works delays the publication of the labours of my coadjutors. I am also influenced by the consideration that, though wholly opposed to the view that the term of the Presidency of the Royal Society should be either short or definitely limited, this term should not be very long; and that, considering the special nature of my own scientific studies, it should, in my case, on this as well as on other grounds, be briefer than might otherwise be desirable. Cogent as these reasons are they might not have been paramount were it not that we have among us one pre-eminently fitted to be your President by scientific attainments, by personal qualifications, and by intimate knowledge of the Society's affairs; and by calling upon whom to fill the proud position which I have occupied, you are also recognising the great services he has rendered to the Society as its treasurer for eight years, and its ofttimes munificent benefactor.

HAECKEL ON THE LIBERTY OF SCIENCE AND OF TEACHING[1]

II.

CHAPTER V. treats of the methods of teaching, and contrasts the *genetic* method, as advocated by Haeckel, with the *dogmatic* one recommended by Virchow. The sensation which Virchow's address caused in wider circles was only partly the result of his opposition to the descent theory; its principal cause was his surprising conclusions with regard to the liberty of teaching. Virchow demands that in the school—from the elementary school up to the university—*nothing should be taught which is not absolutely certain*; only *objective* but no *subjective* knowledge is to be communicated to the pupils by the teacher; only facts, no hypotheses. Haeckel remarks that rarely has an eminent representative of science made such an attack upon the liberty of science as did Virchow at Munich. "Where," he asks, "are we to find the limits between subjective and objective knowledge?" According to his conviction no such limit exists, and all human knowledge as such is subjective. "An objective science consisting only of facts, without subjective theories, cannot be imagined." He then proceeds to review various sciences in turn, and to point out how much objective knowledge and "facts," and how much subjective knowledge and "hypotheses" they contain. He begins with *Mathematics* as the science which is eminently the most *certain* one of all: "What about the simplest and deepest maxims upon the firm basis of which the whole proud building of mathematics rests? Can they be proved for certain? Certainly not! The most fundamental maxims are indeed 'maxims,' and incapable of 'proof.' Only in order to show by an example how even the first mathematical maxims may be attacked by sceptics and shaken by philosophical speculation we recall the recent discussions regarding the three dimensions of space and the possibility of a fourth dimension, discussions which are still continued by a number of the most illustrious mathematicians, physicists, and philosophers. So much is certain that mathematics is absolutely objective as little as any other science, but has a subjective basis in man's own nature. . . . But even if we own that mathematics is an absolutely certain and objective science, how about all other sciences? No doubt those are 'most certain' amongst the 'exact' sciences, the maxims of which are founded on pure mathematics, in the first line therefore a great part of *physics*. We say a great part, because another great part—upon close examination by far the greater—is incapable of an exact mathematical foundation. Or what we do know with certainty about the essence of *matter* or the essence of *force?* What do we know for certain about gravitation, about mass-attraction, about action at a distance, &c.? We look upon Newton's gravitation theory, the basis of mechanics, as the most important and most certain theory of physics, and yet gravitation itself is only a hypothesis. And then the other branches of physics—electricity and magnetism, for instance. The whole knowledge of these important branches is based upon the hypothesis of 'electric fluids' or of imponderable substances, the existence of which is certainly not proved. Or optics? No doubt optics belongs to the most important and most complete branches of physics, yet the vibration theory, which to-day we consider to be its indispensable basis, rests upon a hypothesis which cannot be proved, viz., upon the 'subjective' supposition of the light-ether, the existence of which nobody can objectively prove. Nay, even more; before Young established the vibration theory of light, the emanation theory taught by Newton reigned supreme in physics for centuries; this theory has to-day been abandoned as untenable. According to our view the mighty Newton acquired the greatest merit with regard to the development of optics, as he made the first attempt to connect and explain the mass of objective optical facts by a subjective leading hypothesis. But according to Virchow's view Newton sinned most heavily by teaching this false hypothesis; because in 'exact' physics only *single* and *certain facts* are to be taught and to be ascertained by 'experiment as the highest means of proof;' but physics as a *whole*, resting as it does upon a number of unproved hypotheses, may be the object of research, but must not be taught!" Turning to *Chemistry*, Haeckel shows that its objectiveness stands upon still weaker feet than that of physics. Here the whole of the science is built upon the hypothesis of the existence of atoms, a hypothesis as unproved and as incapable of proof as any. No chemist has ever seen an atom, and yet he thinks the mechanics of atoms the highest problem of his science, and describes and constructs the positions and groupings of atoms, as if they were before him on his dissecting table. According to Virchow, we therefore ought *to banish chemistry from the school* and teach only the properties of bodies and their reactions, which can be shown to the pupils as "certain facts." This matter becomes still more ludicrous when we turn to the other sciences, which are all more or less *historical*, and therefore do not possess that "half-exact" basis upon which chemistry and physics rest. Geology, for instance, would, according to Virchow, have to confine itself to the description of certain facts, *i.e.*, the structure of rocks, the forms of fossils, the shape of crystals, &c., but would in the school have completely to abandon all speculation regarding the development of the earth's crust, *i.e.*, nothing but unproved hypotheses from beginning to end. We might not even teach that fossils are the actual remains of organisms which existed in former periods, because even this is an "unproved" hypothesis. Even down to the eighteenth century many eminent naturalists believed fossils to be "freaks of nature," an enigmatic "*lusus naturæ*." In a later part of his address Virchow admits fossils as "objective material proofs;" but even here we may go no further than our actual experience allows, and we may not draw subjective deductions from the objective facts. Virchow's remark about quaternary man being an "accepted fact" affords Haeckel an opportunity for pointing out his inconsistency, and the uncertainty and vagueness of most hypotheses concerning the age and the first geological occurrence of man; indeed, the distinction of a tertiary and a quartery age in itself is nothing but a *geological hypothesis*. "Virchow tells us that never has a fossil ape skull been found which really belonged to a human proprietor, and that we cannot consider it as a revelation of science, we cannot teach, that man descends from the ape or from any other animal. If that be true, then nothing remains but the descent from a god or from a clod of earth."

Zoology, botany, and other biological doctrines do not fare better, if we consider them in the light in which Virchow would have them taught. Haeckel shows the utter untenability of Virchow's demands, since no science, not even history, and certainly not philosophy, could be tolerated in our schools; indeed, the only one which could remain would be theology. "Incredible as it seems, Virchow, the sceptical antagonist of dogmas, the combatant for the liberty of science, now finds the only certain basis of instruction in the dogma of Church religion. After all that has happened the following phrase leaves no doubt on this point:—'All attempts to transform our problems into doctrines, to introduce our theories as the basis of a plan of education, par-

[1] Freie Wissenschaft und freie Lehre. Eine Entgegnung auf Rudolf Virchow's Münchener Rede über "die Freiheit der Wissenschaft im modernen Staat." Von Ernst Haeckel. Continued from p. 115.

ticularly the attempt simply to depose the Church, and to replace its dogma by a religion of descent without further trouble—these attempts, I say, must fail, and their failure would at the same time bring the greatest dangers upon the position of science generally.' After this every one will easily understand the joyful outbursts of the whole clerical press after Virchow's Munich address. It is known that there is ten times more joy in heaven over one repentant sinner than oven ten just men. If Rudolf Virchow, the 'renowned materialist,' the 'radical progressist,' the principal representative of the 'atheism of science,' is suddenly so completely converted, if openly and loudly he proclaims the 'dogmas of the Church' as the only certain 'basis of instruction,' then, indeed, the combatant Church may sing 'Hosanna in excelsis!' There is only one point to be regretted, and that is that Virchow has not stated which of the many different Church religions is the only true one, and which of the numberless and contradicting dogmas are to become the certain basis of education. We all know that each Church thinks itself the only one leading to eternal bliss, and its dogma the only true one. Now whether it is Protestantism or Catholicism, Reformed or Lutheran confession, Anglican or Presbyterian dogma, Roman or Greek doctrine, Mosaic or Islamitic tenets, Buddhaism or Bramahism, or one of the fetish creeds of the Indians or coloured tribes which is to become the lasting and certain 'basis of instruction,' this, no doubt, Virchow will not hesitate to state at the next meeting of the German Association of Naturalists and Physicians."

"At all events, the instruction of the future," according to Virchow, "will be very much simplified. Because the dogma of the Trinity as the basis of mathematics, the dogma of the resurrection of the flesh as the basis of medicine, the dogma of the infallibility as the basis of psychology, the dogma of the Immaculate Conception as the basis of the science of generation, the dogma of the stoppage of the sun as the basis of astronomy, the dogma of the creation of the earth, animals, and plants, as the basis of geology and phylogeny, these or some other dogmas from other creeds, will make all further doctrines rather superfluous. Virchow, this 'critical nature,' of course knows as well as I do, that these dogmas are *not true*, and yet, according to his view, they are not to be replaced as 'bases of instruction' by the theories and hypotheses of modern natural science, of which Virchow says himself that they *may* be true, probably *are* true to a great extent, but have not been 'proved for certain' as yet."

Finally, Haeckel points out that it seems to be bitter irony if Virchow, at the opening address, recalls the memory of Oken, whom he celebrates as the martyr of free science, and at the end of the same address demands that this "liberty of science" shall apply only to *research*, but not to *instruction*, and that no problems, no theories, no hypotheses are to be taught.

In Chapter VI. the application of the theory of descent to socialism is discussed. The author entirely endorses Prof. Oscar's Schmidt's view on the subject, and shows that the theory of descent and the socialistic theory are "like fire and water to one another." The theory of descent is, on the contrary, aristocratic in the highest sense of the word. Of course, from any theory, be it ever so true and sound, the most absurd deductions may be drawn if it is misapplied, and the author warns particularly against the misapplication of scientific theories to political or social questions. Theory and practice rarely correspond in human life. Haeckel points to the history of Christendom to illustrate his argument. "It is certain that the Christian religion, as well as the Buddhistic doctrine, if freed from all dogmatic fables, contain an excellent humane kernel; now it is just this humane and truly social-democratic part of the Christian creed, which proclaims the equality of all men before God, and preaches the 'Love thy neighbour as thyself,' in fact 'love' in its noblest sense, compassion with the poor and unfortunate, &c., —we say these truly humane sides of the Christian faith are so natural, so pure and noble, that we comprise them with pleasure amongst the moral laws of our monistic natural religion. . . . But what, we must ask, have the chosen representatives, the 'God-taught' (Gottgelehrte) priests, made of this 'religion of love'? It is written with letters of blood upon the pages of the history of mankind for the last 1,800 years! All that different Church religions have done for the forcible propagation of their creeds and for annihilation of 'heretics,' all that Jews have done against heathens, Roman Emperors against Christians, Mohammedans against Christians and Jews, all that is surpassed by the hecatombs of human victims which Christianity has slaughtered for the propagation of its doctrine. And indeed Christians against Christians. Rightly-believing Christians against wrongly-believing Christians. We need only think of the middle ages, of the Inquisition, of the unheard-of and inhuman cruelties which the '*most Christian kings*' of Spain and their worthy colleagues in France, Italy, &c., have committed. Hundreds of thousands then died the most cruel death of fire, simply because they did not bend their reason beneath the yoke of the most flagrant superstition, and because their dutiful conviction forbade them to deny what they had recognised to be natural *truths*. There exists no detestable, abominable, or inhuman action which was not then and up to the present day committed in the name of and on account of 'true Christianity.' And what about the *morals of priests*, who designate themselves as servants of God's word, and whose duty first of all should be to regulate their own lives according to the teachings of Christianity? The long, uninterrupted, and horrible chain of crimes of all kinds which forms the history of the Roman pontiffs, gives the best reply to this question. And as these 'representatives of God upon earth' have done, so did their helpmates and subordinates, so did the 'rightly-believing' priests of other confessions, not failing to establish as glaring as possible a contrast between the practices of their own lives and the noble teachings of Christian love which they always talk about. What we have just said of Christianity applies to all other religious and moral doctrines, indeed to all doctrines which in the wide domain of practical philosophy, in the education of the young, and the civilisation of the masses, are to show their power. The theoretical kernel of these doctrines can always and everywhere form the greatest contrast with its practical application, according to the contradictory nature of man. But what does all this matter to the scientific investigator? His sole and only task is to find out *truth*, and to *teach* that what he has *found to be true*, unheeding what consequences may be drawn from his teachings by the various parties in the State or Church."

In the last chapter Haeckel compares the "Ignorabimus" speech of Dubois-Reymond (delivered at the Leipzig meeting in 1872) with Virchow's "Restringamur" address of Munich, and refutes some of the views expressed by Dubois-Reymond, particularly the view that there are two *insurmountable* obstacles in the way of our completely understanding nature and the world, viz., the essence of matter and force and the human consciousness. Haeckel points out that even if the problems in question are not solved at present, no one has a right to declare them unsolvable. He then proceeds to explain the reasons why the opposition to the theory of descent has mainly originated amongst the Berlin biologists, and adduces examples from the history of science to show that a similar opposition to what have now become established truths, has repeatedly sprung from the same quarter. "It seems, indeed, to be the fate of the most interesting of all sciences, of the *history of evolution*, that its most important steps of progress and its greatest discoveries meet with the most powerful and lasting resistance. Just as Wolf's fundamental epigenesis theory, which was founded in 1759, but was acknowledged only in 1812, so Lamarck's theory of descent, founded in 1809, had to wait during full fifty years before Darwin, in 1859, transformed it into the most important acquisition of modern science. And how was this most comprehensive of all biological theories fought against during this time, in spite of all progress of the empirical sciences? Let us only remember how, in 1830, the celebrated George Cuvier silenced the most eloquent advocate of this theory, Geoffroy Saint Hilaire, in the midst of the Paris Academy, and how almost at the same time, in 1829, its founder, the great Lamarck, ended his laborious life, blind and in misery and poverty, while his antagonist, Cuvier, enjoyed the highest honour and the greatest splendour. And yet to-day we know that the despised and derided doctrines of Lamarck and of Geoffroy Saint Hilaire then contained the most important truths, while Cuvier's much-admired and generally-adopted creation doctrine has been now abandoned generally as an absurd and empty error. Now if neither Haller against Wolff, nor Cuvier against Lamarck, could permanently impede the progress of free research, then still less will Virchow succeed in crushing Darwin's admirable theory, even if he be assisted in an unenviable manner by the noisy Capuchin sermons of his friend Bastian. Much as we regret Virchow's hostile position in this great 'combat for truth,' we do not underrate the effect of his well-founded authority upon wider circles. No doubt the hostile attitude which the greatest part of the Berlin press has assumed with regard to the doctrine of evolution must be attributed to this authority. But much as we must regret the reactionary current in this and other intelligent Berlin circles, yet we must point out that by this evil we are guarded from a far greater one. This greater, indeed the greatest, evil which could befall German science would be a Berlin 'monopoly of knowledge,' the *centralisation of science*. What highly disastrous fruit this centralisation has borne in France, for instance; how the Paris 'monopoly of knowledge' causes a constant degradation of French science, and has led it downwards from the greatest heights for the last half-century, is well known. Probably the wide-spread differentiation and the many-sided individuality of the German national spirit, the often-decried German particularism, will save us from a centralisation of science of this kind, which particularly in our capital, Berlin, would be doubly dangerous. Little as our 'small states' could be politically of any duration or could lead to a useful state-form, they have certainly been most beneficial and fertile for German science. Because this owes its principal advantages over others to the numerous little centres of education, which the capitals of the German small states formed, and to the many little universities which were always in healthy competition with one another. Let us hope that this beneficial decentralisation of science in our politically united Fatherland will continue permanently. Next to the centrifugal striving of our German national spirit, nothing will further this object so much as an energetic resistance to the free progress of science, just as now again it begins to show itself in the capital of the empire. Because at the same rate as this will remain behind in the mighty current of free and unimpeded mental progress, other numerous centres of education in Germany which follow this current enthusiastically, or at least willingly, will outrun it."

"If Emil Dubois Reymond wanted to make his 'Ignorabimus' the watchword of science, and Rudolf Virchow his still further-reaching 'Restringamur,' then from Jena and from a hundred other educational centres they are met with the call—

"*Impavidi progrediamur!*"

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

STUDENTS of Natural Science who would much rather know French and German than Greek will be glad to learn that a very strong memorial against the retention of Greek as a subject for all honour candidates has been presented to Cambridge University. It is signed by ten heads of public schools, including Drs. Hornby (Eton), Butler (Harrow), and Abbott (City of London), Messrs. Matthew Arnold, Carlyle, W. E. Forster, the Bishops of Exeter and Manchester, Dean Cowie, Dean Stanley, and Dr. Vaughan, Prof. Jebb, and Mr. Roby, to say nothing of such bulwarks of science as Mr. Darwin, Prof. Huxley, Sir Joseph Hooker, Mr. Spottiswoode, and Prof. Tyndall.

THE Board of Musical Studies at Cambridge have applied for the appointment of a University Reader in Acoustics.

THE sum in the hands of the Sedgwick Memorial Committee for the erection of a new building for the geological collection is 12,000*l.*, not 1,200*l.* as we stated last week.

King's College (London) *Magazine*, No. 5, vol. ii. of which has been sent us, contains some pleasant reading, but no one would infer from its contents that the College was an important centre of scientific instruction and research.

DR. J. COSSAR EWART has been appointed by the Crown to the Chair of Natural History in the University of Aberdeen.

MR. F. GUTHRIE, formerly of Graaf-Reinet College, has been appointed to the Chair of Mathematics at the South African College, Capetown.

THE *Journal de St. Pétersbourg* gives the following particulars concerning the public provision for education in Russia:—The sum assigned in the Budget of this year for education is 15,971,289 roubles (about 2,395,000*l.*). There are eight Universities (not reckoning that of Helsingfors for Finland), with 5,629 students. Of these 85 are divinity students, 583 belong to the philological faculty, 1,629 to the faculty of law, 30 to that of Eastern languages, 622 to the mathematical faculty, 550 to that of natural science, and 2,130 to the medical faculty. There are 53 ecclesiastical seminaries, with 12,227 pupils; 195 gymnasia and pro-gymnasia, with 50,701 pupils; 56 middle-class schools, with 10,888 scholars. There are 19 military gymnasia, but the number of pupils is not given. For females there are 223 gymnasia and pro-gymnasia, having 34,878 pupils; and this does not include the many institutions which are subject to the control of the Fourth Division of the Imperial Chancellery. There are 68 normal schools and training colleges for teachers, having 4,968 students. There are 10 other such institutions supported by non-public funds. The number of elementary schools in operation this year is 25,491, with 1,074,559 pupils.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, December 5.—"On the Illumination of Lines of Molecular Pressure, and the Trajectory of Molecules," by William Crookes, F.R.S., V.P.C.S.

Induction Spark through Rarefied Gases. Dark Space round the Negative Pole

The author has examined the dark space which appears round the negative pole of an ordinary vacuum tube when the spark from an induction coil is passed through it. He describes many experiments with different kinds of poles, a varying intensity of spark, and different gases, and arrives at the following propositions:—

Illumination of Lines of Molecular Pressure

a. Setting up an intense molecular vibration in a disk of metal by electrical means excites a molecular disturbance which affects the surface of the disk and the surrounding gas. With a dense gas the disturbance extends a short distance only from the metal; but as rarefaction continues, the layer of molecular disturbance increases in thickness. In air at a pressure of ·078 mm. this molecular disturbance extends for at least 8 mm. from the surface of the disk, forming an oblate spheroid around it.

b. The diameter of this dark space varies with the exhaustion; with the kind of gas in which it is produced; with the temperature of the negative pole; and, in a slight degree, with the intensity of the spark. For equal degrees of exhaustion it is greatest in hydrogen and least in carbonic acid, as compared with air.

c. The shape and size of this dark space do not vary with the distance separating the poles; nor, only very slightly, with alteration of battery power; nor with intensity of spark. When the power is great the brilliancy of the unoccupied parts of the tube overpowers the dark space, rendering it difficult of observation; but, on careful scrutiny, it may still be seen unchanged in size, nor does it alter even when, with a very faint spark, it is scarcely visible. On still further reduction of the power, it fades entirely away, but without change of form.

The author describes numerous experiments, devised to ascertain if this visible layer of molecular disturbance is identical with the invisible layer of molecular pressure or stress, the investigation of which has occupied him for some years.

The Electrical Radiometer

One of these experiments is as follows:—An ordinary radiometer is made, with aluminium disks for vanes, each disk coated with a film of mica. The fly is supported by a hard steel cup instead of a glass cup, and the needle point on which it works is connected by means of a wire with a platinum terminal sealed into the glass. At the top of the radiometer bulb a second terminal is sealed in. The radiometer can therefore be connected with an induction coil, the movable fly being made the negative pole.

Passing over the phenomena observed at low exhaustions, the author finds that, when connected with the coil, a halo of a velvety violet light forms on the metallic side of the vanes, the mica side remaining dark throughout these experiments. As the pressure diminishes a dark space is seen to separate the violet halo from the metal. At a pressure of half a millimetre this dark space extends to the glass, and positive rotation commences.

On continuing the exhaustion the dark space further widens out and appears to flatten itself against the glass, and the rotation becomes very rapid.

When aluminium cups are used for the vanes, instead of disks backed with mica, similar appearances are seen. The velvety violet halo forms over each side of the cup. On increasing the exhaustion the dark space widens out, retaining almost exactly the shape of the cup. The bright margin of the dark space becomes concentrated at the concave side of the cup to a luminous focus, and widens out at the convex side. On further exhaustion the dark space on the convex side touches the glass, when positive rotation commences, becoming very rapid as the dark space further increases in size, and ultimately flattening against the glass.

Convergence of Molecular Rays to a Focus

The subject next investigated is the convergence of the lines of force to a focus, as observed with the aluminium cup. As this could not be accomplished during rapid rotation an instrument was made having the cup-shaped negative pole fixed, instead of movable. On exhaustion, the convergence of the lines of force to a focus at the concave side was well observed. When the dark space is very much larger than the cup it forms an irregular ellipsoid drawn in towards the focal point. Inside the luminous boundary a focus of dark violet light can be seen converging, and, as the rays diverge on the other side of the focus, spreading beyond the margin of the dark space, the whole appearance being strikingly similar to the rays of the sun reflected from a concave mirror through a foggy atmosphere.

Green Phosphorescent Light of Molecular Impact

At very high exhaustions the dark space becomes so large that it fills the tube. Careful scrutiny still shows the presence of the dark violet focus, and the part of the glass on which fall the rays diverging from this focus shows a sharply defined spot of greenish-yellow light. On still further exhaustion, and especially if the cup is made positive, the whole bulb becomes beautifully illuminated with greenish-yellow phosphorescent light.

This greenish-yellow phosphorescence, characteristic of high exhaustions, is frequently spoken of in the paper. It must be remembered, however, that the particular colour is due to the special kind of soft German glass used. Other kinds of glass phosphoresce in a different colour. The phosphorescence takes place only under the influence of the negative pole. At an exhaustion of 4 M[1] no light other than this is seen in the apparatus. At ·9 M the phosphorescence is about at its maximum. When the exhaustion reaches ·15 M the spark has a difficulty in passing, and the green light appears occasionally in flashes only. At ·06 M the vacuum is almost non-conductive, and a spark can be forced through only by increasing the intensity of the coil, and well insulating the tube and wires leading to it. Beyond that exhaustion nothing has been observed.

Focus of Molecular Force

In an apparatus specially constructed for observing the position of the focus, the author found that the focal point of the green phosphorescent light was at the centre of curvature, showing that the molecules by which it is produced are projected in a direction normal to the surface of the pole. Before reaching the best exhaustion for the green light, another focus of blue-violet light is observed; this varies in position, getting further from the pole as the exhaustion increases. In the apparatus described, at an exhaustion of 19·3 M, these two foci are seen simultaneously, the green being at the centre of curvature, while the blue focus is at nearly twice the distance.

Nature of the Green Phosphorescent Light

The author adduces the following characteristics of the green phosphorescent light as distinguishing it from the ordinary light observed in vacuum-tubes at lower exhaustions:—

a. The green focus cannot be seen in the space of the tube, but where the projected beam strikes the glass only.

b. The position of the positive pole in the tube makes scarcely any difference to the direction and intensity of the lines of force which produce the green light. The positive pole may be placed in the tube either at the extremity opposite the negative pole, or below it, or by its side.

c. The spectrum of the green light is a continuous one, most of the red and the higher blue rays being absent; while the spectrum of the light observed in the tube at lower exhaustions is characteristic of the residual gas. No difference can be detected by spectrum examination in the green light, whether the residual gas be nitrogen, hydrogen, or carbonic acid.

d. The green phosphorescence commences at a different exhaustion in different gases.

e. The viscosity of a gas is almost as persistent a characteristic of its individuality as its spectrum. The author refers to a preliminary note and a diagram[2] of the variation of viscosity of air, hydrogen, and other gases at exhaustions between 240 M and ·1 M. From these and other unpublished results, the author finds that the viscosity of a gas undergoes very little diminution between atmospheric pressure and an exhaustion at which the green phosphorescence can be detected. When, however, the spectral and other characteristics of the gas begin to disappear, the viscosity also commences to decline, and at an exhaustion at which the green phosphorescence is most brilliant the viscosity has rapidly sunk to an insignificant amount.

f. The rays exciting green phosphorescence will not turn a corner in the slightest degree, but radiate from the negative pole in straight lines, casting strong and sharply-defined shadows from objects which happen to be in their path. On the other hand, the ordinary luminescence of vacuum tubes will travel hither and thither along any number of curves and angles.

Projection of Molecular Shadows

The author next examines the phenomena of shadows cast by the green light. The best and sharpest shadows are cast by flat disks and not by narrow pointed poles; no green light whatever is seen in the shadow itself, no matter how thin or whatever may be the substance from which it is thrown.

From these and other experiments, fully described in the paper, he ventures to advance the theory that the induction-spark actually illuminates the lines of molecular pressure caused by the electrical excitement of the negative pole. The thickness of the dark space is the measure of the mean length of the path between successive collisions of the molecules. The extra velocity with which the molecules rebound from the excited negative pole keep back the more slowly-moving molecules which are advancing towards that pole. The conflict occurs at the boundary of the dark space, where the luminous margin bears witness to the energy of the collisions.

When the exhaustion is sufficiently high for the mean length of path between successive collisions to be greater than the distance between the fly and the glass, the swiftly-moving, rebounding molecules spend their force, in part or in whole, on the sides of the vessel, and the production of light is the consequence of this sudden arrest of velocity. The light actually proceeds from the glass, and is caused by fluorescence or phosphorescence on its surface. No light is produced by a mica or quartz screen, and the more fluorescent the material the better the luminosity. Here the consideration arises that the greenish yellow light is an effect of the direct impact of the molecules, in the same electrical state, on the surface of the glass. The shadows are not optical, but are molecular shadows, revealed only by an ordinary illuminating effect; this is proved by the sharpness of the shadow when projected from a wide pole.

Phosphorescence of Thin Films

An experiment is next described in which a film of uranium glass, sufficiently thin to show colours of thin plates, is placed in front of a thick plate of the same glass, the whole being inclosed in a tube with terminals, and exhausted to a few millionths of an atmosphere. Of this the following observations are recorded:—

a. The uranium film, being next to the negative pole, casts a strong shadow on the plate.

b. On making contact with the coil, the thin film flashes out suddenly all over its surface with a yellowish phosphorescence, which, however, instantly disappears. The uncovered part of the plate does not become phosphorescent quite suddenly, but the phosphorescence is permanent as long as the coil is kept at work.

c. With an exceedingly faint spark the film remains more luminous than the plate, but on intensifying the spark the luminosity of the film sinks and that of the uncovered part of the plate increases.

d. If a single intense spark be suddenly sent through the tube, the film becomes very luminous, while the plate remains dark.

These experiments are conclusive against the phosphorescence being an effect of the radiation of phosphorogenic ultra-violet light from a thin layer of arrested molecules at the surface of the glass, for were this the case, the film could under no circumstances be superior to the plate.

The momentary phosphorescence and rapid fading of the film prove more than this. The molecular bombardment is too much for the thin film. It responds thereto at first, but immediately gets heated by the impacts, and then ceases to be luminous.

[1] M signifies the millionth of an atmosphere.
[2] *Proc.* Roy. Soc., Nov. 16, 1876, vol. xxv. p. 305.

The plate, however, being thick, bears the hammering without growing hot enough to lose its power of phosphorescing.

Mechanical Action of Projected Molecules

When the coil was first turned on, the thin film was driven back at the moment of becoming phosphorescent, showing that an actual material blow had been given by the molecules. Experiments are next described in which this mechanical action is rendered more evident. A small rotating fly, capable of being moved about in any part of an exhausted bulb, is used as an indicator, and by appropriate means the molecular shadow of an aluminium plate is projected along the bulb. Whether entirely in, or entirely out of the shadow, the indicator scarcely moves, but when immersed so that one-half is exposed to molecular impact, the fly rotates with extreme velocity.

Magnetic Deflection of Lines of Molecular Force

With this apparatus another phenomenon was investigated. It is found that the stream of molecules, whose impact on the glass occasions evolution of light, is very sensitive to magnetic influence, and by bringing one pole of an electro-magnet—or even of a small permanent magnet—near, the shadow can be twisted to the right or to the left.

When the little indicator was placed entirely within the molecular shadow, no movement was produced. As soon, however, as an adjacent electro-magnet was excited, the shadow was twisted half off the indicator, which immediately rotated with great speed.

The Trajectory of Molecules

The amount of deflection of the stream of molecules forming a shadow is in proportion to the magnetic power employed.

The trajectory of the molecules forming the shadow is curved; when under magnetic influence the action of the magnet is to twist the trajectory of the molecules round in a direction at an angle to their free path, and to a greater extent, as they are nearer the magnet: the direction of twist being that of the electric current passing round the electro-magnet.

Laws of Magnetic Deflection

An apparatus was constructed so that the deflection of a spot of light was used instead of that of a shadow, a horse-shoe magnet being placed underneath the negative pole to deflect the trajectory. The action of the north pole being to give the line of molecules a spiral twist one way, and that of the south pole being to twist it the other way, the two poles side by side compel the line to move in a straight line up or down along a plane at right angles to the plane of the magnet and a line joining its poles.

The ray of molecules does not appear to obey Ampère's law, as it would were it a perfectly flexible conductor, joining the negative and the positive pole. The molecules are projected from the negative, but the position of the positive pole, whether in front, at the side, or even behind the negative pole, has no influence on their subsequent behaviour, either in producing phosphorescence, or mechanical effects, or in their magnetic deflection. The magnet gives their line of path a spiral twist greater or less according to its power, but diminishing as the molecules get further off.

Numerous experiments were tried in this apparatus with different gases, and with the magnet in and out of position.

Working with exhausted air it was found that the spot of green phosphorescence on the screen is visible at an exhaustion of 102·6 M, when the mean free path of the molecules, measured by the thickness of the dark space round the negative pole, is only 12 mm. Hence, it follows that a number of molecules sufficient to excite green phosphorescence on the screen are projected the whole distance from the pole to the screen, or 102 mm., without being stopped by collisions.

Alteration of Molecular Velocity

If we suppose the magnet to be permanently in position, and thus to exert a uniform downward pull on the molecules, we perceive that their trajectory is much curved at low exhaustions, and gets flatter as the exhaustion increases. A flatter trajectory corresponds to a higher velocity. This may arise from one of two conditions; either the initial impulse given by the negative pole is stronger, or the resisting medium is rarer. The latter is probably the true one. The molecules which produce the green phosphorescence must be looked upon as in a state differing from those arrested by frequent collisions. The latter impede the velocity of the free molecules and allow longer time for magnetism to act on them; for, although the deflecting force of magnetism might be expected to increase with the velocity of the molecules, Prof. Stokes has pointed out that it would have to increase as the square of the velocity, in order that the deflection should be as great at high as at low velocities.

Comparing the free molecules to cannon-balls, the magnetic pull to the earth's gravitation, and the electrical excitation of the negative pole to the explosion of the powder in the gun, the trajectory will be flat when no gravitation acts, and curved when under the influence of gravitation. It is also much curved when the ball passes through a dense resisting medium, it is less curved when the resisting medium gets rarer; and, as already shown, intensifying the induction spark, equivalent to increasing the charge of powder, gives greater initial velocity, and, therefore, flattens the trajectory. The parallelism is still closer if we compare the evolution of light seen when the shot strikes the target, with the phosphorescence on the glass screen from molecular impacts.

Focus of Heat of Molecular Impact

The author finally describes an apparatus in which he shows that great heat is evolved when the concentrated focus of rays from a nearly hemispherical aluminium cup is deflected sideways to the walls of the glass tube by a magnet. By using a somewhat larger hemisphere and allowing the negative focus to fall on a strip of platinum foil, the heat rises to the melting point of platinum.

An Ultra-gaseous State of Matter

The paper concludes with some theoretical speculations on the state in which the matter exists in these highly exhausted vessels. The modern idea of the gaseous state is based upon the supposition that a given space contains millions of millions of molecules in rapid movement in all directions, each having millions of encounters in a second. In such a case the length of the mean free path of the molecules is exceedingly small as compared with the dimensions of the vessel, and the properties which constitute the ordinary gaseous state of matter, which depend upon constant collisions, are observed. But by great rarefaction the free path is made so long that the hits in a given time may be disregarded in comparison to the misses, in which case the average molecule is allowed to obey its own motions or laws without interference; and if the mean free path is comparable to the dimensions of the vessel, the properties which constitute gaseity are reduced to a minimum, and the matter becomes exalted to an ultra-gaseous state, in which the very decided but hitherto masked properties now under investigation come into play.

Rays of Molecular Light

In speaking of a ray of molecular light, the author has been guided more by a desire for conciseness of expression than by a wish to advance a novel theory. But he believes that the comparison, under these special circumstances, is strictly correct, and that he is as well entitled to speak of a ray of molecular or emissive light when its presence is detected only by the light evolved when it falls on a suitable screen, as he is to speak of a sunbeam in a darkened room as a ray of vibratory or ordinary light when its presence is to be seen only by interposing an opaque body in its path. In each case the invisible line of force is spoken of as a ray of light, and if custom has sanctioned this as applied to the undulatory theory, it cannot be wrong to apply the expression to emissive light. The term emissive light must, however, be restricted to the rays between the negative pole and the luminous screen: the light by which the eye then sees the screen is, of course, undulatory.

The phenomena in these exhausted tubes reveal to physical science a new world—a world where matter exists in a fourth state, where the corpuscular theory of light holds good, and where light does not always move in a straight line; but where we can never enter, and in which we must be content to observe and experiment from the outside.

Chemical Society, December 5.—Dr. Gladstone, president, in the chair.—Prof. Tidy read a lengthy and important paper on the processes for determining the organic purity of potable waters. The conclusions at which the author arrives, after many experiments and a careful examination of the comparative analyses of over 1,600 waters, may be briefly summed up as follows: *The ammonia process* furnishes results which are marked by singular inconstancy, and are not delicate enough to allow the recognition and classification of the finer grades of purity or impurity. The errors incidental to the process form an array of difficulties which become infinitely serious, seeing that the range (as regards albumenoid ammonia) between pure and dirty waters is comparatively small. *The combustion process* has all the evils of evaporation to encounter, but the organic carbon estimation is trustworthy; the organic nitrogen determination, however, scarcely yields absolutely trustworthy evidence on which to found an opinion as to the probable source of the organic matter. The process, nevertheless, is of great value. *The oxygen* (permanganate) *process* avoids the errors incidental to evaporation; its results (when properly used) are constant and extremely delicate; it draws a sharp line between the putrescent or probably pernicious and the non-putrescent or probably harmless organic matter; by it a bad water can never be passed as good. As far as the three processes are concerned, the oxygen and combustion processes give closely concordant results, whilst those yielded by the ammonia process are often at direct variance with both.

Photographic Society, November 12.—James Glashier, F.R.S., in the chair.—After the medals awarded for the best pictures in the exhibition had taken place, a paper by Leon Warnerke was read—photographic notes from a travel in Russia with exhibition of various works, apparatus, and materials. John Thomson, F.R.G.S., described his photographic experiences in Cyprus. Mr. Warnerke described the artistic and scientific progress of photography in Russia as having arrived at a high state of perfection, as also the very important position which photo-lithography occupies in the Government establishments of the country where large maps of the Russian frontiers are produced.

Boston, U.S.A.

American Academy of Arts and Sciences, November 13.—Hon. Charles Francis Adams in the chair.—Prof. Mitchell, of the U.S. Coast Survey, read a monograph upon the tides of the Gulf of Maine. The results of the late surveys prove that a lift of the tide far out from shore requires an almost inappreciable time to be felt along the coast. The variations in the tides were shown to come from the north rather than from the south. In the discussion of Prof. Mitchell's paper Prof. Benjamin Peirce maintained that the results corroborated his own theory of vibrations and nodal points.—Prof. A. E. Dolbear, of Tuft's College, read a paper upon his claims of the invention of the speaking-telephone, and exhibited his early models and also a great variety of new speaking-telephones. Among these were a Morse sounder transformed into a telephone; a miniature voltaic cell, one plate of which was spoken against, and speech thus transmitted; an electrophone depending upon the variable resistance of the extra spark, which can transmit speech over a distance of more than two hundred miles; various forms of transmitters and modifications of Reiz's telephone, which Prof. Dolbear said are innumerable.—Mr. N. D. C. Hodges exhibited a new instrument for determining the magnetic dip. Two soft iron bars joined at right angles move on a vertical graduated circle. A small magnet with a mirror is suspended at the point of crossing of the pieces of soft iron. When the latter make equal angles with the line of dip, the magnet remains at zero. By four simple reversals and measurements the residual magnetism of the iron is eliminated. The instrument gives very constant results and requires less time to make an observation than the ordinary dipping needle.

Paris

Academy of Sciences, December 2.—M. Fizeau in the chair.—The following papers were read:—On the torsion of prisms of mixtilinear base, and on a peculiarity which may be presented by certain uses of the logarithmic co-ordinate of the isothermal cylindrical system of Lamé, by M. De Saint Venant.—M. Marey was elected Member for the section of Medicine and Surgery in the room of the late M. Cl. Bernard, the other candidates being MM. Bert, Charcot, and Gubler.—Experimental researches on meteoritic nickelised irons; mode of formation of concretionated syssidera, by M. Meunier. The mixture of chlorides of iron and nickel gives, by reduction in hydrogen, alloys perfectly definite, and sometimes beautifully crystallised. Association of the alloys together can be effected in two ways (specified). Next, grains of peridot or fragments of dunite can easily be covered with a continuous coat of various alloys of iron and nickel, the metallic concretion sometimes penetrating into the fine fissures of the stone. By placing the pieces of rock repeatedly in the incrusting medium fed with diverse mixtures of the two chlorides, superposed deposits of various alloys are obtained, and a complete facsimile of the cosmic rocks in question.—On a new phenomenon of static electricity, by M. Govi. A reference to his former experiments on the phenomenon studied by M. Duter. Different liquids having given different amounts of contraction, and mercury no contraction, he inferred condensation of the liquid against the walls of the jar. He apparently doubts M. Duter's conclusion.—On the electromotive force of induction arising from rotation of the sun; determination of its amount and direction, whatever the distance of the induced body, by M. Quet. He gives a mathematical estimate of the force, with formulæ.—Note on the effects of vapours of sulphide of carbon, by M. Poincaré. He experimented on animals, with reference to the symptoms presented by workmen in vulcanisation of caoutchouc. Guinea pigs and frogs resist the action much less than man; and the period of excitation is wanting in them, the manifestations being mainly paralytic. The auricles are distended with dark blood, livid spots appear in the lungs, the brain is often reduced to a diffluent pulp, and drops apparently of sulphide of carbon form in the cerebral vessels, blocking the passage of blood corpuscles and sometimes causing rupture. The author considers the use of vulcanised caoutchouc should be restricted to really useful objects, and that the manufacture of small balloons and toys of it should be interdicted.—On the mode of formation of some phylloxeric nodosities, by M. d'Arbaumont.—Latitude of Algiers, and fundamental azimuth of the Algerian triangulation, by M. Perrier. The author describes his method.—Nebulæ discovered and observed at the Observatory of Marseilles, by M. Stephan.—Double stars; certain groups of perspective, by M. Flammarion.—Evaluation of a definite integral, by M. Appell.—On the repulsion resulting from radiation, by Mr. Crookes.—Note on cholalic acid, by M. Destrem.—Researches on vaso-motor nerves, by MM. Dastre and Morat. The principal branch of termination of the sciatic nerve plays, with regard to the region of the finger, the *rôle* of a vaso-constrictor nerve, and there is no ground for supposing, in this nerve-trunk, vaso-dilator nerves either more or less than in the cervical cord of the sympathetic. Thus the controverted point as to whether the sciatic is a vaso-dilator nerve, is answered in the negative. The authors generalise the results, applying them to the nervous trunks which go to the skin.—On the cardiac and respiratory effects of irritations of certain sensitive nerves of the heart, and on the cardiac effects produced by irritation of sensitive nerves of the respiratory apparatus, by M. François-Franck. Different effects are produced according as an irritating injection (say hydrate of chloral) is made into the right or the left heart; in the former case there is diastolic stoppage of the heart, in the latter systolic. (The mechanism of these effects is studied.)—On the change of form of fixed cells of loose connective tissue in artificial œdema, by M. Renaut.

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