THURSDAY, OCTOBER 6, 1887.

ALPHITA.

Alphita. A Medico-Botanical Glossary from the Bodleian Manuscript Selden B 35. Edited by J. L. G. Mowat, M.A., Fellow of Pembroke College. [*Anecdota Oxoniensia.* Mediæval and Modern Series. Vol. I. Part 2.] (Oxford: Clarendon Press, 1887.)

THIS interesting vocabulary, which Mr. Mowat has transcribed and edited from a manuscript in the Bodleian Library, is offered by him as a contribution to the study of English plant-names. To explain why it has this and also other claims to attention, we must say a word about the class of literature to which it belongs.

When ancient Greek science was first brought to the knowledge of mediæval Europe it was by means of Latin versions of the Greek writers, made not directly, but at second hand, from Arabic versions written or brought into Europe by the Moors. The earliest known Latin translations of certain works of Galen, Hippocrates, and other medical writers, with probably some of Aristotle, originated in this way. On the basis of these versions, which began in the eleventh century with the writings of Constantine the African, a medical literature grew up, containing many Greek words corrupted by passing through an Arabic channel, as well as Arabic and some Latin terms hardly less strange to the mediæval reader. It is clear that these hard words presented great difficulties, not as a matter of language only, but of practical use, since it was difficult for the reader to identify the diseases spoken of and the drugs recommended for their cure.

To remedy the uncertainties and dangers thus arising, a new class of literature sprang up—that of the writers whom we may perhaps call the synonymists or glossarists—who compiled lists of the hard words occurring in medical works of the Arabian school, with explanations in Latin. The most celebrated though not the earliest of these was Simon of Genoa, whose list of medical synonyms, the "Clavus Sanationis," was largely borrowed from by subsequent writers. Several others might be named, but there are also anonymous collections of the same kind, one of which is the vocabulary or glossary known as "Alphita."

The anonymous character of this production is not merely a matter of accident. In its present form it is clearly not the work of one writer. A vocabulary or glossary originally intended to explain some work of practical medicine (possibly the "Antidotarium Nicolai," as Mr. Mowat suggests) was expanded by matter introduced from many sources, and by the work of many hands, till at length it could only be regarded as a sort of joint-stock compilation to which no one man's name could be attached.

The title under which it goes is not explained (so far as we can see) by Mr. Mowat, and therefore we may say that it is merely the first word of one of the glosses or definitions, "Alphita, farina ordei idem," or "ἄλφιτον—the same thing as barley-meal."

This definition happening to come first in an older form of the glossary was taken as its title. It has been reprinted under this title in the "Collectio Salernitana" of De Renzi, and another Bodleian manuscript (Ashmole, 1398) giving what appears to be an abridged form of that now published is also thus headed.

But what has all this to do with English plant-names? Merely this, that when scholars or scribes in Northern Europe copied or edited these glossaries (most if not all of which were produced in Italy) they often added the French or English vernacular names of plants. Hence these glossaries form a supplement to the earlier lists of names published by Prof. Earle in his valuable "English Plant Names." There is no reason to suppose that these names were contemporary with the original composition; we may rather assign them to the date of the manuscript, which Mr. Mowat refers to the fifteenth century. They thus form a connecting-link between the Anglo-Saxon and Old English names of the earlier lists, and those which we find in Gerarde and the printed "Herbals" which preceded his. Some of these are very interesting, and have been elucidated with much skill in Mr. Mowat's most laborious and valuable notes; the corrupt and barbarous forms of the Greek and Latin words making them often difficult of recognition.

The interest of the work then lies in the preservation of a number of plant-names; and it is worth inquiring first of all in what way the English names have been identified with their classical equivalents. Sometimes the modern name is a mere corruption of the ancient, as in rose, bugloss, tansy, and numbers more. Sometimes the one is a translation of the other; hound's tongue, coltsfoot, cranesbill, are familiar instances. But when a name was thus altered or translated it did not follow that the plant was identified. A curious instance of the confusion which may arise is the following.

Eleutropia, or elitropia, evidently represents the Greek ἡλιοτρόπιον = heliotropium, and the Latin equivalents *Solseqium* and *Sponsa solis* have the same meaning, viz. a flower which turns to the sun; and an Anglo-Saxon glossarist (quoted by Earle) boldly translates the Latin as *Sigel hweorfa* (turning to the sun). It might still, however, remain doubtful what flower was meant; but when we find Calendula used as a synonym of Solseqium, and when we read in "Alphita" (p. 88), "*Kalendula, sponsa solis, incuba* idem, *Anglice* goldwurt vel rodes," we see that the marigold, a common garden flower in the Middle Ages, and known as golde, gold wort, rode-wort, ruddes, marigolds, mary gowles, &c., is meant (though by the bye it had only borrowed from the marsh marigold—*Caltha palustris*—a name which originally belonged to the latter). The Latin name points clearly to the Calendula folding its flowers when the sun goes down.

But the synonym *incuba* in the above line betrays a confusion with intybum, chicory, as shown again in the gloss (p. 53): "Eleutropia, incuba, sponsa solis vel mira solis solseqium cicoria idem, anglice et gallice cicoree;" or in the line from "Sinonoma Bartolomei" which seems meant for a hexameter: "Incuba, solseqium, cicoreaque sponsaque solis." Gerarde also gives sponsa solis as a name of chicory. So that both marigold and chicory are made synonyms of heliotropium. The curious thing is that the glossary gives definitions quoted from Dioscorides of a larger and lesser "Eliotropium," neither of which can be identified with either of the plants above mentioned.

The glossarist can hardly have supposed that marigold and chicory meant the same thing, but he was evidently hazy as to the meaning of incubus, which occurs again in the following gloss (p. 39): "Cicuta, celena, incubus, coniza vel conium, herba benedicta idem. Gallice chanele vel chanelire; angl. hemelok vel hornwistel."

Gerarde has preserved the name Herb Bennet; the other synonyms we must leave Mr. Mowat to explain. He suggests that the strange name hornwistel may be derived from the offensive smell of the plant. Very likely he is right, but, without any pretensions to philological learning, we may suggest that a hemlock stem is easily converted into a *whistle*.

At p. 156 we have the true etymology of the deceptive name meadow-sweet, "Reginela, Regina Prati, medewort," the English name meaning a plant used for flavouring mead, and altered into meadow-sweet possibly, as Dr. Prior suggests, through some confusion with Regina Prati, queen of the meadow, which name, again, is preserved in the French "Reine des Prés."

Several glosses give the old form of primrose, primerole, a diminutive of Italian *prima vera*, the first flower of spring; and show, moreover, that this name was originally assigned to the daisy, called also *Consolida minor*, of which the German "Ortus Sanitatis" gives an unmistakable figure. The reason evidently was that our primrose is a rare flower in Italy, where the daisy is the herald of spring, but the northern botanists found the name better suited to the flower which now bears it, or to the cowslip, *herba Sancti Petri*.

It is still more startling to find Ligustrum (or modern privet) glossed in some lists (though not in this) as primrose or cowslip. But whatever plant may have been originally meant by Ligustrum, the name privet, or primet, was, as shown by Dr. Prior, originally identical in meaning and almost in etymology with primrose, being derived from French Prime-printemps = Primprint, primet, or prim. Why the Latin name was at one time applied to the flower, at another to the shrub now thus called, is not quite clear.

A curious relic of ancient medicine is preserved in the gloss (p. 5): "Allium domesticum, tyriaca rusticorum, gall. angl. garleke." Here *tyriaca* = θηριακή = *theriaca* (treacle), a once celebrated antidote against snakes and venomous animals. A plant supposed to be the garlick was called by Galen a name rendered in Latin *Theriaca rusticorum*, and so became "poor man's treacle," a name which garlick still bears, though the modern transference of the word treacle to molasses makes it appear absurd.

The medical terms in "Alphita" are extremely interesting, but space forbids entering upon the subject. One curious instance may, however, be quoted, which shows that "there is nothing new under the sun." Only last year Prof. Liebreich, of Berlin, introduced to the medical world, under the name of "lanoline," a new fatty substance for ointments, derived from wool, which has proved a most successful novelty. Now, we find in our glossary the following: "Ysopus cerotis vel Ysopum cerotum est succus lane succide per decoctionem extractus. Qualiter efficitur quere in Dyascorides" (p. 198). *I.e.* "the cerate (or ointment) Ysopum is a 'juice' extracted by boiling from uncleaned wool. For the mode of preparation consult Dioscorides." This is, in fact, οἴσυπος, or *æsopus*, mentioned by Dioscorides and Pliny as a fat extracted from the fleeces of sheep, and is practically identical with Liebreich's lanoline.

While thanking Mr. Mowat for this valuable contribution to the history of mediæval science, and the Clarendon Press for their spirited endeavour to make the treasures of the Bodleian common property, we may suggest that there are other scientific relics equally worthy of attention: such, for instance, as some remarkable illustrated manuscripts of anatomy and natural history, or the works of John Arderne, the English surgeon, a relic at least equal in historical value to those already published, and of far greater national significance.

J. F. PAYNE.

OUR BOOK SHELF.

Fresh Woods and Pastures New. By the Author of "An Amateur Angler's Days in Dove Dale." (London: Sampson Low, 1887.)

IN this delightful little volume the amateur angler, who discoursed so pleasantly on the beauties of the streams and fields of Dove Dale a few years ago, recounts his subsequent experiences of country life and amongst country scenes. Angling plays but an inconsiderable part in the present book, but the spirit of the angler is over every chapter —the spirit, namely, which finds placid enjoyment in all the sights and sounds of Nature, and something new and interesting everywhere. His motto is, that the old simplicity of the country "though hid in grey, Doth look more gay Than foppery in plush and scarlet clad." Of this capacity for finding amusement everywhere the chapter on turkeys and peacocks is an example. A battle between two flocks of turkeys is described with much humour; the method in which these birds fight is perhaps new even to persons who think they know a good deal about turkeys; it certainly will be to others. Again, the description of a peacock going to roost is full of quiet fun; few persons, even of those who live in the country, have ever seen a peacock perform the feat of flying into a tree for the night. Yet it is a feat to which great importance is attached by the bird himself; it is only to be done with great circumspection, hesitation, and show of indifference. A score of other topics connected with the country are treated with a like charm. The little book, both in subjects and mode of treatment, is a gem.

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 insure the appearance even of communications containing interesting and novel facts.*]

The British Museum and American Museums.

I VERY much regret to learn that my friend Prof. Flower thinks I have done great injustice to the British Museum of Natural History in my article on "American Museums," which has appeared in the September number of the *Fortnightly Review*. The article was sent to England last February, and I had no opportunity of correcting the proofs, as some very bad misprints will sufficiently indicate. Nothing was farther from my mind than to make any reflections on the management or arrangement of the Museum by

Prof. Flower and the able heads of departments, for all of whom I have the greatest respect; and I am further convinced that much credit is due to them for doing the very utmost that is possible under the circumstances of the case. My strictures on the Museum were intended to apply solely and exclusively to the fundamental *principle* underlying its arrangement, which principle is embodied in the new building as in the old one. I contrasted strongly the principle of moderate-sized rooms as compared with large galleries,—the principle of exhibiting, to the public, on the one hand, strictly limited typical collections; on the other, almost complete series of species,—the principle of making a geographical arrangement the main feature of a museum, as compared with that in which almost no provision at all is made for such an arrangement.

I had always understood that for this fundamental system of arrangement neither the present Director nor the heads of departments of the Museum were in any way responsible, and that in criticising it frankly I should not be considered to reflect on them. So clear was I in my own mind that I was discussing this general system only, that I used some expressions which I now see, with much regret, were capable of being misunderstood. After referring to some of the improvements in the New British Museum, I say, "but the great bulk of the collection still consists of the old specimens exhibited in the old way in an interminable series of overcrowded wall-cases, while all attempt at any effective presentation of the various aspects and problems of natural history as now understood is as far off as ever." To the latter part of this sentence, Prof. Flower objects, as not recognizing the many improvements recently made and still making; but I intended it to apply, as I think the whole context of my article shows, to the *system* and the *building*, which themselves, from the point of view I have taken throughout the article, render any attempt at an "effective" presentation of these aspects and problems impossible. Again, at the end of my article I speak of Prof. Agassiz having said that he intended his museum "to illustrate the history of creation as far as the present state of scientific knowledge reveals that history," and then go on: "It is surely an anomaly that the naturalist who was most opposed to the theory of evolution should be the first to arrange his museum in such a way as best to illustrate that theory, while in the land of Darwin no step has been taken to escape from the monotonous routine of one great systematic series of crowded specimens arranged in lofty halls and palatial galleries, which may excite wonder, but which are calculated to teach no definite lesson." Here I was referring to the fact that the new Museum at South Kensington was constructed and arranged substantially on the same lines as the old one at Bloomsbury, and regretting that the only effective step towards inaugurating a new system of arrangement was not then taken. Prof. Flower, I find, thinks that I imply that no steps are being taken now to render the Museum more instructive and generally interesting. This was very far from my meaning, and I am exceedingly sorry that such an interpretation of my words should have been possible. I visited the Museum several times last summer before leaving for America, and I noted many improvements that were being introduced in all departments; but I could not fail to see that the main principle of the arrangement, both of the building itself and of the collections in it, had not been changed, and it was to this that all my criticisms were directed.

Godalming, September 22. ALFRED R. WALLACE.

The Law of Error.

MR. F. Y. EDGEWORTH has, in NATURE of September 22 (p. 482), replied to Dr. Venn's letter from the mathematical standpoint; perhaps a few words from the meteorological side may not be out of place. The gist of Dr. Venn's remarks lies in his statement that the law of error applies to cases where there are "equal and opposite independent disturbing causes" (September 1, p. 412). Now, the excess and defect of barometrical pressure from the average, depend mainly on anticyclones and cyclones respectively, which though in many respects opposite in character are by no means equal, the latter being much more intense than the former; and there is no reason in the nature of the case why they should be equal, as many of their characteristics are so dissimilar.

As regards the second instance given by Dr. Venn, the chief factor in the variations of temperature at different times of the year is the varying declination of the sun, the rate of change of declination passing through two minima yearly—namely, at the solstices, so named for this very reason. One would naturally expect that about these times the temperature should remain more nearly the same than about the equinoxes; Dr. Venn's curve would consequently give two maxima. The deviations of the temperature of each *day* from the average would not be unlikely to conform to the law of error, but it is evident that a curve formed from the temperatures for the whole year would be of a totally different kind. T. W. BACKHOUSE.

Sunderland, September 26.

Lunar Rainbows.

ON Sunday night, August 28, a lunar rainbow was visible here. As the occurrence seems to be uncommon, some particulars may interest your readers.

We had a very heavy shower before 11 o'clock, with a south-west wind. The rain left off suddenly, as it began, a few minutes past 11; and as the heavy cloud moved away to the north-east it left a gloriously clear sky behind, with the moon, then a little past its first quarter, shining brightly a few degrees above a heavy bank of cloud which lay on the horizon. Looking out of a window on the opposite side of the house, I had the satisfaction of seeing a complete pale white bow in the black cloud to the north-east, which lasted very clear and distinct for about five minutes, when it quickly grew faint as the bank of clouds on the horizon began to rise and obscure the falling moon. The outer edge of the bow was well defined against the intense black of the cloud beyond; the inner edge was much less distinct, and the area within was covered with a slight suffused light, which, however, appeared to diminish as the distance from the bow increased.

The drops of rain were unusually large, and the downpour, while it lasted, was extraordinarily heavy.

A. F. GRIFFITH.

15 Buckingham Place, Brighton, September 22.

A LUNAR rainbow was visible here shortly after 11 o'clock last night. It extended without break through three-quarters of a semicircle, the top of the arch being about 60° high. In colour the bow resembled a moonbeam shining between two clouds, and its brightness was sufficient to cause it to be immediately detected by a casual glance, in spite of the presence of numerous white clouds occupying its centre. The sky just outside the bow appeared darkest, probably by contrast with these clouds. Ten minutes elapsed before the rainbow faded.

Rock Ferry, September 27. S. J. H.

The Perception of Colour.

IS Mr. Stromeyer sure that the observations he made (see NATURE, July 14, p. 246) prove any difference in the rapidity of perception of colour, and that they do not rather show a difference in perception of brightness? It is well known that faint objects are not so quickly perceived as bright ones (see Webb's "Celestial Objects," p. 368 of the 4th edition, under ϵ Pegasi); and as the violet end of the spectrum is much fainter than the rest, the effect described would be produced by the difference in brightness apart from the difference in colour. I have tried Mr. Stromeyer's experiment of rotating the spectrum, and it appears to me that the red as well as the violet end lags behind the middle; though as the red is so much shorter, this is more difficult to see. T. W. BACKHOUSE.

Sunderland, September 15.

Tertiary Outliers on the North Downs.

IN August of last year (NATURE, vol. xxxiv. p. 341), I ventured to draw a distinction between the *unfossiliferous sands* found at certain places on the North Downs and the fossiliferous deposits at Lenham. For reasons assigned, I suggested a certain degree of probability of their being of Bagshot age, and indicating a former extension by overlap of the higher beds of that important Eocene formation. This summer I have had opportunities of examining all the principal outliers referred to; and I must say that I am strongly impressed with the Bagshot character of these unfossiliferous sands, and of the well-rolled flint pebbles associated with them, in some cases (as at Headley) in great quantity. I speak only of those which can be identified with some degree of certainty as Tertiary beds *in situ*. The sands at Netley Heath and at Chipstead have a remarkable *Upper* Bagshot facies. Those at Headley do not present such a strong character in this respect, but I have no hesitation in referring them on lithological grounds to the Bagshot series.

Wellington College, Berks, September 27. A. IRVING.

MODERN VIEWS OF ELECTRICITY.[1]

PART I.

I.

IT is often said that we do not know what electricity is, and there is a considerable amount of truth in the statement. It is not so true, however, as it was some twenty years ago. Some things are beginning to be known about it; and though modern views are tentative, and may well require modification, nevertheless some progress has been made. I shall endeavour in this lecture to set forth as best I may the position of thinkers on electrical subjects at the present time.

It will at once strike you that the whole subject of electricity as at present known is too gigantic for anyone to make an attempt to compass it in a single lecture, even though he assume on the part of his audience a perfect acquaintance with all the ordinary phenomena; and you will admit that it is much better to limit one's self definitely at the beginning to some one branch than by attempting too broad and discursive a survey to risk slurring the whole and becoming totally unintelligible.

I begin by saying that the whole subject of electricity is divisible for purposes of classification into four great branches.

(1) Electricity at rest, or static electricity: wherein are studied all the phenomena belonging to stresses and strains in insulating or dielectric media brought about by the neighbourhood of electric charges or electrified bodies at rest immersed therein; together with the modes of exciting such electric charges and the laws of their interactions.

(2) Electricity in locomotion, or current electricity: wherein are discussed all the phenomena set up in metallic conductors, in chemical compounds, and in dielectric media, by the passage of electricity through them; together with the modes of setting electricity in continuous motion and the laws of its flow.

(3) Electricity in rotation, or magnetism: wherein are discussed the phenomena belonging to electricity in whirling or vortex motion, the modes of exciting such whirls, the stresses and strains produced by them, and the laws of their interaction.

(4) Electricity in vibration, or radiation: wherein are discussed the propagation of periodic or undulatory disturbances through various kinds of media, the laws regulating wave velocity, wave-length, reflection, interference, dispersion, polarization, and a multitude of phenomena studied for a long time under the heading "Light." Although this is the most abstruse and difficult portion of electrical science, a certain fraction of it has been known to us longer than any other branch, and has been studied under special advantages, because of our happening to possess a special sense-organ for its appreciation.

Now, with some qualms of regret I have decided to refrain from speaking to you about any one of these great and comprehensive groups except the first. It is hopeless to attempt more; and even the small portion of that on which I shall touch will tax the time at our disposal to the utmost, and I must assume acquaintance with the elementary facts in order to proceed to their elucidation.

The great names in connexion with our progress in knowledge as to the real nature of electricity, irrespective of a mere study and extension of its known facts, are

FRANKLIN, CAVENDISH, FARADAY, MAXWELL.

To these, indeed, you may feel impelled to add the tremendous name of THOMSON; but one has some delicacy in attempting to estimate the work of living philosophers, and as Maxwell has been very explicit in acknowledging his indebtedness to his illustrious contemporary, whose work will in the course of nature have to be criticised and appraised by far abler hands than mine and by the philosophers of generations yet unborn, we may well afford to abstain from minute considerations and accept for the present the name of Maxwell as representative of the great English school of mathematical physicists, under whose influence, Cambridge, in the pride of having reared them, is awaking to new and energetic scientific life, and whose splendid achievements will shine out in the future as the glory of this century.

The views concerning electrification which I shall try to explain are in some sense a development of those originally propounded by that most remarkable man, Benjamin Franklin. The accurate and acute experimenting of Cavendish laid the foundation for the modern theory of electricity; but, as he worked for himself rather than for the race, and as moreover he was in this matter far in advance of his time, Faraday had to go over the same ground again, with extensions and additions peculiar to himself and corresponding to the greater field of information at his disposal three-quarters of a century later. Both these men, and especially Faraday, so lived among phenomena that they yielded up their hidden secrets to them in a way unintelligible to ordinary workers; but while they themselves arrived at truth by processes that savour of intuition, they were unable always to express themselves intelligibly to their contemporaries and to make the inner meaning of their facts and speculations understood. Then comes Maxwell, with his keen penetration and great grasp of thought combined with mathematical subtlety and power of expression; he assimilates the facts, sympathizes with the philosophic but untutored modes of expression invented by Faraday, links the theorems of Green and Stokes and Thomson to the facts of Faraday, and from the union there arises the young modern science of electricity, whose infancy at the present time is so vigorous and so promising that we are all looking forward to the near future in eager hope and expectation of some greater and still more magnificent generalization.

You know well that there have been fluid or material theories of electricity for the past century; you know, moreover, that there has been a reaction against them. There was even a tendency a few years back to deny the material nature of electricity and assert its position as a form of energy. This was doubtless due to an analogical and natural, though unjustifiable, feeling that just as sound and heat and light had shown themselves to be forms of energy so in due time would electricity also. If such were the expectation, it has not been justified by the event. Electricity may possibly be a form of matter—it is not a form of energy. It is quite true that electricity *under pressure* or *in motion* represents energy, but the same thing is true of water or air, and we do not therefore deny them to be forms of matter. Understand the sense in which I use the word electricity. *Electrification* is a result of work done, and is most certainly a form of energy; it can be created and destroyed by an act of work. But electricity—none is ever created or destroyed, it is simply moved and strained like matter. No one ever exhibited a trace of positive electricity without there being somewhere in its immediate neighbourhood an equal quantity of negative.

This is the first great law, expressible in a variety of ways: as, for instance, by saying that total algebraic pro-

[1] Expansion of a lecture delivered by Dr. Oliver Lodge, partly at the London Institution on January 1, 1885, and partly at the Midland Institute, Birmingham, November 15, 1886, but not hitherto published.

duction of electricity is always zero; that you cannot produce positive electrification without an equal quantity of negative also; that what one body gains of electricity some other body must lose.

Now, whenever we perceive that a thing is produced in precisely equal and opposite amounts, so that what one body gains another loses, it is convenient and most simple to consider the thing not as generated in the one body and destroyed in the other, but as simply *transferred. Electricity in this respect behaves just like a substance.* This is what Franklin perceived.

The second great law is that electricity always, under all circumstance, flows in a closed circuit, the same quantity crossing every section of that circuit, so that it is not possible to exhaust it from one region of space and condense it in another.

Another way of expressing this fact is to say that no charge resides in the interior of a hollow conductor.

Another is to say that total induced charge is always equal and opposite to inducing charges.

[This is illustrated by the well-known experiment of insulating and charging a parrot-cage with a sensitive electroscope inside connected to its wires; also by the ice-pail experiment.]

When we thus find that it is impossible to charge a body absolutely with electricity, that though you can move it from place to place it always and instantly refills the body from which you take it, so that no portion of space can be more or less filled with it than it already is, it is natural to express the phenomenon by saying that electricity behaves itself like a perfectly *incompressible* substance or fluid, of which all space is completely full. That is to say, it behaves like a perfect and all-permeating *liquid.* Understand, I by no means assert that electricity *is* such a fluid or liquid; I only assert the undoubted fact that it behaves like one, *i.e.* it obeys the same laws.

It may be advisable carefully to guard one's self against becoming too strongly imbued with the notion that because electricity obeys the laws of a liquid therefore it is one. One must always be keenly on the look-out for any discrepancy between the behaviour of the two things, and a single certain discrepancy will be sufficient to overthrow the fancy that they may perhaps be really identical. Till such a discrepancy turns up, however, we are justified in pursuing the analogy—more than justified, we are impelled. And if we resist the help of an analogy like this there are only two courses open to us: either we must become first-rate mathematicians, able to live wholly among symbols and dispensing with pictorial images and such adventitious aid; or we must remain in hazy ignorance of the stages which have been reached, and of the present knowledge of electricity so far as it goes. I need hardly say that by "modern views" I do not mean *ultimate* views; nor do I mean that I can give an account of all the speculations and ideas floating in the minds of some two or three of our most advanced thinkers. All I attempt is to give an account of the stage which has certainly been attained, and to ask you to take for granted that the next quarter of a century will see as great advances made upon these views as they are superior to the doctrines inculcated by the ordinary run of text-books.

Imagine now that we live immersed in an infinite ocean of incompressible and inexpansible all-permeating perfect liquid, like fish live in the sea, and how can we become cognizant of its existence? Not by its weight, for we can remove it from no portion of space in order to try whether it has weight.

We can weigh air, truly, but that is simply because we can compress it and rarefy it. An exhausting or condensing pump of some kind was needed before even air could be weighed or its pressure estimated.

But if air had been incompressible and inexpansible, if it had been a vacuum-less perfect liquid, pumps would have been useless for the purpose, and we should necessarily be completely ignorant of the weight and pressure of the atmosphere.

How then should we become cognizant of its existence? In four ways:—

(1) By being able to pump it out of one elastic bag into another [not out of one bucket into another: if you lived at the bottom of the sea you would never think about filling or emptying buckets—the idea would be absurd; but you could fill or empty elastic bags], and by noticing the strain phenomena exhibited by the bags and their tendency to burst when over full. [Water (or air) was here pumped out of one elastic bag into another, and the analogy with an electrical machine charging two conductors oppositely was pointed out.]

(2) By winds or currents; by watching the effect of moving masses of the fluid as it flows along pipes or through spongy bodies, and by the effects of its inertia and momentum. [A hanging vane in a tube deflected by a stream of water was here likened roughly to a galvanometer; also the effect of suddenly stopping a stream of water, as in a water ram, was mentioned as analogous to self-induction.]

(3) By making vortices and whirls in the fluid, and by observing the mutual action of these vortices, their attractions and repulsions. [Whirlwinds, sand-storms, waterspouts, cyclones, whirlpools.]

(4) By setting up undulations in the medium: *i.e.* by the phenomena which in ordinary media excite in us through our ears the sensation called "*sound.*"

In all these ways we have become acquainted with electricity, and in no others that I am aware of. They correspond to the four great divisions of the subject which I made above. But there are differences, very important differences, between the behaviour of a material liquid ocean such as we have contemplated and the behaviour of electricity. First it is doubtful whether electricity by itself and disconnected from matter has any inertia. It is by no means certain that it has not: the experiments made by Maxwell with a negative result need only prove either that its speed of flow is very small, or that an electric current consists of equal opposite streams of equal momentum. The laws of electric flow in conductors are such as indicate no inertia, and this fact would be conclusive were it not that a recent brilliant paper by Prof. Poynting explains the reason of it completely otherwise, and leaves the question of inertia quite open; on the other hand, the facts of magnetism seem definitely to require inertia, or something corresponding to it. Leaving this therefore as an open question, there can be no doubt but that when in connexion with insulating or dielectric matter *the combination* most certainly possesses inertia.

A more serious and certain difference between the behaviour of electricity and that of an incompressible fluid comes out in the fourth category—that concerned with wave-motion. In an incompressible fluid the velocity and length of waves would both be infinite, and none of the phenomena connected with the gradual propagation of waves through it could exist. Such a medium therefore would be incapable of sound vibrations in any ordinary sense. On the other hand, it is quite certain that the disturbances concerned in light radiation take place at right angles to the direction of propagation—they are tranverse disturbances—and such disturbances as these no body with the entire properties of a fluid can possibly transmit. Remember, however, that the medium which transmits light is the ether and not simply electricity. We have nowhere asserted that electricity and the ether are identical. If they are, we are bound to admit that ether, though fluid in the sense of enabling masses to move freely through it, has a certain amount of rigidity for enormously rapid and minute oscillatory disturbances. If they are not identical, we can more vaguely say that ether contains electricity as a jelly contains water, but that the rigidity concerned in the transverse vibrations belongs not to the water in the jelly but to the mode in which it is entangled in its meshes. However all this is a great and difficult question into which we shall be able to enter with more satisfaction twenty years hence.

Provisionally we will accept as a working hypothesis the idea of the ether consisting of electricity in a state of entanglement similar to that of water in jelly; and we are driven to this view by the exigencies of mode 1, the electrostatic or strain method of examining the properties of electricity, because otherwise the properties of insulators are hard to conceive. If it turn out that space is a conductor, which seems to me highly improbable, then we must fall back upon the other view that it is rigid only for infinitesimal vibrations, and fluid for steady forces.

Return now to the consideration of electrostatics. We are to regard ourselves as living immersed in an infinite all-permeating ocean of perfect incompressible fluid (or liquid), as fish live in the sea; but this is not all, for if that were our actual state we should have no more notion of the existence of the liquid than deep-sea fish have of the medium they swim in. If matter were all perfectly conducting, it would be our state: in a perfectly free ocean there is no insulation—no obstruction to flow of liquid: it is the fact of insulation that renders electrostatics possible. We could obstruct the flow and store up definite quantities of a fluid in which we were totally submerged by the use of closed vessels of course. But how could we pump liquid from one into another so as to charge one positively and another negatively? Only by having the walls elastic: by the use of elastic bags, and elastic partitions across pipes. And so we can represent a continuous insulating medium (like the atmosphere or space) by the analogy of a jelly, through which liquid can only flow by reason of cracks and channels and cavities.

Modify the idea of an infinite ocean of liquid into that of an infinite jelly or elastic substance in which the liquid is entangled, and through which it cannot penetrate without violence and disruption; and you have here a model of the general insulating atmosphere. Our ocean of fluid is not free and mobile like water, it is stiff and entangled like jelly.

Nevertheless bodies can move through it freely. Yes *bodies* can, it is the *liquid* itself only which is entangled. How we are to picture freely and naturally the motion of ordinary matter through the insulating medium of space it is not easy to say. It is a difficulty not fatal but sensible, and due to an imperfection in our analogy.

Insulators being like elastic partitions or impervious but yielding masses, conductors are like cavities, porous or spongy bodies perfectly pervious though with more or less frictional resistance to the flow of liquids through them. Thus, whereas bodies easily penetrable by matter are impervious to electricity, bodies like metals which resist entirely the passage of matter, are quite permeable to electricity. It is this inversion of ordinary ideas of penetrability that constitutes a small difficulty at the beginning of the subject.

However, supposing it overcome, let us think of these insulated spheres and cylinders on the table connected by copper wire as so many cavities and tubes in an otherwise continuous elastic impervious medium which surrounds them and us, and extends throughout space wherever conductors are not. All, however, cavities as well as the rest of the medium, are completely full of the universal fluid. The fluid which is entangled in insulators is free to move in conductors; whence it follows that its pressure or potential is the same in every part of a conductor in which it is not flowing along. For if there were any excess of pressure at any point, a flow would immediately occur until it was equalized. In an insulator this is by no means the case. Differences of pressure are exceedingly common in insulators, and are naturally accompanied by a strain of the medium.

[Here certain electrostatic experiments were shown as evidence of the strain existing at the ends of a long insulated wire connected to a Voss machine.]

There have been, as you know, two ancient fluid theories of electricity—the one-fluid theory of Franklin, and the two-fluid theory of Symmer and others. A great deal is to be said for both of them within a certain range. There are certainly points, many points, on which they are hopelessly wrong and misleading, *but it is their foundation upon ideas of action at a distance that condemns them, it is not the fluidity*. They concentrate attention upon the conductors; whereas Faraday taught us to concentrate attention on the insulating medium surrounding the conductors—the "*dielectric*" as he termed it. This is the seat of all the phenomena: conductors are mere breaks in it—interrupters of its continuity.

To Faraday the space round conductors was full of what he called lines of force; and it is his main achievement in electrostatics to have diverted our attention from the obvious and apparent to the intrinsic and essential phenomena. Let us try and seize his point of view before going further. It is certainly true as far as it goes, and is devoid of hypothesis.

Take the old fundamental electric experiment of rubbing two bodies together, separating them, and exhibiting the attraction and repulsion of a pith ball, say, and how should we now describe it? Something this way.

Take two insulated disks of different material, one metal, say, and one silk, touch them together, the contact effects a transfer of electricity from the metal to the silk; rub slightly to assist the transfer, since silk is a non-conductor, then separate. As you separate the disks the medium between them is thrown into a state of strain, the direction of which is mapped out by drawing a set of lines, called lines of force, from one disk to the other, coincident with the direction of strain at every point. As Faraday remarked, the strain is as if these lines were stretched elastic threads endowed with the property of repelling each other as well as of shortening themselves; in other words, there is a tension along the lines of force and a pressure at right angles to them. When the disks are near, and the lines short, they are mainly straight, Fig. 1,

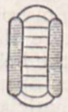

FIG. 1.—Rough diagram of the state of the medium near two oppositely charged disks when close together.

but as the distance increases they become curved, bulging away from the common axis of the two disks, and some even curling round to the back of the disk (Fig. 2), until when the disks are infinitely distant as many lines spring from the back of each as from its face; and we have a charged body to all intents existing in space by itself.

The state of tension existing in the medium between the disks results in a tendency to bring them together again, just as if they were connected by so many elastic threads of no length when unstretched. The ends of the lines are the so-called electrifications or charges, and the lines perpetually try and shorten and shut up, so that their ends may coincide and the strain be relieved. If one of the disks touch another conducting body, some of its lines instantly leave it and go to the body; in other words, the charge is capable of transference, and the new body is urged toward the other disk, just as the disk was from which it received the lines. If this new body *completely surrounds* the disk, it receives the whole of its lines, and the disk can be withdrawn perfectly free and inert. [Faraday's "ice pail" experiment.]

Now take the two charged disks, facing one another, and let, say, a suspended gilt pith ball hang between them. Being a conductor there is no strain inside it, and so it acts partially as a bridge, and several of the lines pass through it—or, rather, they end at one side of it and begin at the other : thus it has opposite charges on its two faces—it is under induction (Fig. 3). Let it now be moved so as to touch one of the disks, the lines between it and the disk on that side have shut up, and it remains with those only which go to the other disk. In other words, it has received some lines from the touched disk. These will pull it over to the far disk and there shut

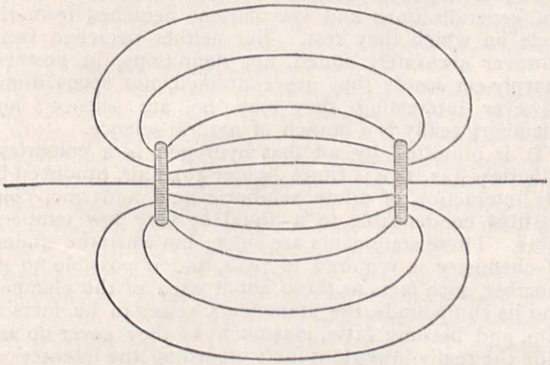

Fig. 2.—Rough diagram of the state of the medium near two oppositely charged disks when separated.

themselves up. From that disk it receives more, and travels with their ends back to the first disk, and so on (Fig. 4), perpetually receiving lines and shutting them up until they are all gone and the disks are discharged.

This mode of stating the facts involves no hypothesis whatever—it is the simple truth. But the "lines of force" have no more and no less existence than have "rays of light." Both are convenient modes of expression.

But so long as we adhere to this mode of expression we cannot form a complete mental picture of the actually

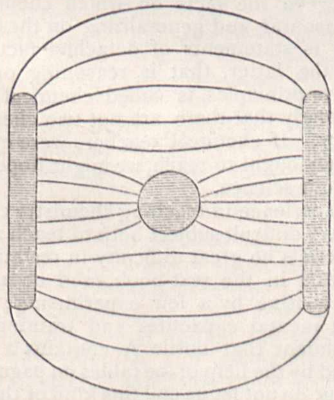

Fig. 3.—Rough diagram of the medium between two disks disturbed by the presence of an uncharged metal sphere. The two halves of the sphere are oppositely charged "by induction."

occurring operations. In optics it is usual to abandon rays at a certain stage and attend to the waves, which we know are of the essence of the phenomenon, though we do not know yet very much about their true nature.

Similarly in electricity, at a certain point we are led to abandon lines of force and potential theories, and to try to conceive the actual stuff undergoing its strains and motions. It is then we get urged towards ideas similar to those which are useful in treating of the behaviour of an incompressible fluid.

In an utterly modified sense, we have still a fluid theory of electricity, and a portion of the ideas of the old theories belong to it also.

Thus Franklin's view that positive charge was excess and negative charge was a deficit in a certain standard quantity of the fluid which all bodies naturally possessed in their neutral state, remains practically true. His view that the fluid was never manufactured, but was taken from one body to give to another, so that one gained what the other lost—no more and no less—remains practically true. Part also—a less part—of the two-fluid theory likewise remains true, in my present opinion ; but this is not a branch of the subject on which I shall enter in the present discourse. It will suffice for the present to fix our attention on one fluid only.

You are to think of an electric machine as a pump which, being attached to two bodies respectively, drives some electricity from the one into the other, conferring upon one a positive and upon the other a precisely equal negative charge. One of the two bodies may be the earth, in which case the charge makes little or no difference to it.

But, as has been objected before, if electricity is like an incompressible and inextensible fluid, how is it possible to

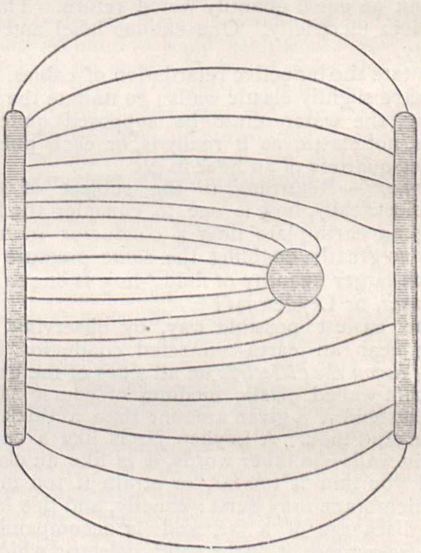

Fig. 4.—Rough diagram of the medium near two oppositely charged disks between which a metal carrier ball is oscillating, having just touched the right-hand disk. (Discharge by "alternate contact.")

withdraw any of it from one body and give it to another? With rigid bodies it is not possible, but with elastic bodies it is easy.

The act of charging this sphere is therefore analogous to pumping water into this elastic bag, or rather into a cavity in the midst of an elastic medium, whose thick walls, extending in all directions and needing a great pressure to strain them, better represent the true state of the case than does the thin boundary of a bag like this.

Draw a couple of such cavities and consider fluid pumped from one into the other, and you will see that the charge (*i.e.* the excess or defect of fluid) resides on the outside. You may also show that when both are similarly charged the medium is so strained that they tend to be forced apart ; whereas when one is distended and the other contracted they tend to approach.

Further you may consider two cavities side by side, pump fluid into (or out of) one only, and watch the effect on the other. You will thus see the phenomena of induction, the near side of the second cavity becoming oppositely charged (*i.e.* the walls encroaching on the cavity), the far side similarly charged (the cavity encroaching on the walls), and the pressure on the fluid in the cavity being increased or diminished in correspondence with the change of pressure in the charged or inducing cavity. In other words, conductors rise in potential when brought near a positively charged body.

The actual changes in volume necessary to the strain of these cavities are a defect in the analogy. To avoid this objection, one will have to accept a dual view of electricity—a sort of two-fluid theory, which many phenomena urge one to accept, but about which I will say nothing to-night. It is sufficient at first to grasp the one-fluid ideas.

Return Circuit.—Sometimes a difficulty is felt about electricity flowing in a closed circuit—as, for instance, in signalling to America and using the earth as a return circuit: the question arises, How does the electricity find its way back?

The difficulty is no more real than if a tube were laid to America with its two ends connected to the sea and already quite full. If now a little more sea-water were pumped in at one end, an equal quantity would leave the other end, and the disturbed level of the ocean would readjust itself. Not the same identical water would return, but an equal quantity would return. That is all one says in electricity. One cannot label and identify electricity.

To imitate the inductive retardation of cables, the tube should have slightly elastic walls; to imitate the speed of signalling, the water must be supposed quite incompressible, not elastic as it really is, or each pulse would take three-quarters of an hour to go.

Condensers.—Returning to the subject of charging bodies electrically, how is one to consider the fact that bringing an earth-plate near a conductor increases its capacity so greatly, enabling the same pressure to force in a much larger quantity of fluid? how is one to think of a condenser, or Leyden jar?

In the easiest possible way, by observing that the bringing near an earth-connected conductor is really *thinning down the dielectric* on all sides of the body.

The thin-walled elastic medium of course takes less force to distend it a given amount than a thick mass of the same stuff took. A Leyden jar is like a cavity with quite thin walls—in other words, it is like an elastic bag.

But if you thin it too far, or strain it too much, the elastic membrane may burst: exactly, and this is the disruptive discharge of a jar, and is accompanied by a spark. Sometimes it is the solid dielectric which breaks down permanently. Ordinarily it is merely the air; and, since a fluid insulator constitutes a self-mending partition, it is instantaneously as good as new again.

There are many things of interest and importance to study about a Leyden jar. There is the fact that if insulated, it will not charge: the potential of both inner and outer coatings rises equally; that, in order to charge it, for every positive spark you give to the interior an equal positive spark must be taken from the exterior. There is the charging and the discharging of it by alternate contacts, as by an oscillating ball; and there are the phenomena of the spark-discharge itself.

But, as you know, *all* charging is really a case of a Leyden jar. The outer coat must always be somewhere—the walls of the room, or the earth, or something—you always have a layer of dielectric between two charges—the so-called induced and the inducing charge. You cannot charge one body alone.

To illustrate the phenomena of charge, I will now call your attention to these diagrams—which less completely but more simply than hydraulic illustrations, serve to make the nature of the phenomena manifest.

(*To be continued.*)

ON THE TEACHING OF CHEMISTRY.[1]

THE question is being often asked, Why does chemistry progress so slowly in this country? Different answers, all more or less true, may be given; one answer that has not, I think, been sufficiently insisted on is: Because chemistry is so little taught.

Classes, nominally in chemistry, are conducted in many schools, and in almost all the colleges, of the country; but I assert that very little of what is taught is really chemistry. For what is it that is taught? On the one hand, catalogues of so-called facts detached from reasoning and from generalizations; on the other hand, definitions and generalizations and speculations detached from the facts on which they rest. But neither detached facts, however accurately stated, nor definitions, in however sharply-cut words they are contained, nor speculations, however interesting they may be, are science; and chemistry really is a branch of natural science.

It is admitted by all that hydrogen is a colourless, odourless gas, 14·435 times lighter than air, produced by the interaction of dilute sulphuric acid and zinc, combustible, condensable to a liquid at very low temperatures. These statements are facts; but when the student of chemistry is required to read, and if possible to remember, such facts as these about each of the elements and its compounds, the statements cease to be facts to him, and become false, inasmuch as they cover up and hide the really important facts regarding the interactions of elements and compounds, and regarding the connexions between changes of composition and changes of properties, which form the subject-matter of chemistry.

I have known students have at their finger-ends the properties of all the elements—as these properties are detailed in the ordinary text-books—and yet be almost wholly ignorant of chemistry.

And I have also known students ready at a moment's notice to repeat the orthodox definitions of atomic and molecular weights, or to draw structural formulæ of complex minerals, or to speak fluently about double bonds and unsaturated units of affinity, and yet be quite innocent of any knowledge of chemistry.

A fatal distinction is too often drawn by chemical teachers between the facts on which chemical science rests, and reasoning and generalizing on these facts: the former, that is statements of detached facts, is called chemistry; the latter, that is reasoning on facts and generalizing to principles, is called chemical philosophy. I believe strongly that there are not two chemistries, but one chemistry. If chemical teachers were quite decided as to what they ought to teach, we might hope for marked advances in our science.

My own experience in teaching chemistry convinces me that it is a very difficult subject both to teach and to learn. Of course there is no great difficulty in restating to a class what is printed in the text-book, and occasionally enlivening the routine by a few experiments; nor does it require high mental capacities and training to tell the laboratory student that bottle A contains a double salt, to be analyzed by the help of the tables on page so-and-so of the book. But do not let us call this kind of thing teaching chemistry. To teach chemistry well requires experience and an educated mind. It is not easy to hit the golden mean. If the teacher despises facts his reasoning becomes absurd, because it is based on nothing; his principles become only speculations; and his laws merely phrases. If he disregards principles, generalizations, and theories, his facts become false, and when facts are false (as they often are) they are deadly, and kill those that trust in them.

Chemistry is a branch of natural science; it deals with one class of natural occurrences, it observes and experi-

[1] A Paper read before Section B of the British Association at Manchester, by M. M. Pattison Muir, M.A., Fellow of Gonville and Caius College, Cambridge.

ments on these occurrences, it classifies and generalizes, and tries to ascend from empirical generalizations to natural laws. The business of the teacher of chemistry I take to be to make his pupil understand the methods of chemistry; to select typical facts and put these before the learner so that he may to some extent see their relative importance in the general scheme of well-built knowledge; to show the student how complex are the phenomena chemistry investigates, and how she simplifies that she may explain; to imbue him with some of that fine enthusiasm without which no great work is possible, by presenting to him glimpses of the greatness of the subject he is studying, and the importance of the prosecution of the study; and thus to build up in the student a scientific spirit, until at last the teacher and the learner are merged in their common investigation of nature.

In teaching chemistry the all-important things appear to me to be chiefly four: (1) to teach so that the student shall acquire real knowledge; (2) to carefully select both the facts and the reasoning to be set before the student; (3) to impress the learner with the importance and value of what he is learning as a part of that orderly and methodized study of nature which we call science; (4) to teach without fear of the examiner.

Real knowledge of chemistry can only be acquired by connecting the experimental work in the laboratory with chemical reasoning and with the principles of the science. This is rarely done in our chemical schools. The student generally hears lectures on chemistry, or at least on the materials from which chemical science is constructed; he sees experiments performed that have some connexion with what the lecturer teaches; then he goes into the laboratory and day after day performs qualitative analyses, for the most part by rule of thumb. The learner, especially the beginner, cannot connect what he is taught in lectures, and told to read in books, with what he does in the laboratory. The introduction of a well-arranged and properly graduated system of practical chemistry, is, in my opinion, one of the things which will do much to hasten the advance of chemistry among us. The work done in the laboratory must be directly connected with the lecture-work and the reading of the student; it must be progressive, beginning with easy experiments and leading the student onwards until he is able to investigate chemical occurrences for himself; and it must be arranged so that as the experimental work becomes more difficult the reasoning on the results becomes more close and accurate.

Such a course of practical chemistry can be arranged without any complicated laboratory appliances. (I may say parenthetically that in my opinion the building of luxuriously fitted laboratories has not been an unmixed gain to chemical science.)

In the course of laboratory work which seems to me to be called for, the student would begin with easy experiments on chemical and physical change, on the distinction between elements and not-elements (this would involve quantitative measurements), and on the classification of not-elements into mixtures and compounds. He would then proceed to classify compounds into acids, bases, and salts, working through well-chosen examples of each class. He would learn by his own experiments what is meant by "the replaceable hydrogen of acids," and by the classification of acids in accordance with their "basicity." He would study classes of elements, and so get a real grasp of the reasons for placing certain elements in one class, and of the principles of chemical classification. After hearing in the lecture-room about the properties of the members of a class of elements—say the iron class—he would at once go into the laboratory and himself prepare typical similar compounds of these elements. He would then turn to the conditions of chemical action; he would practically learn what an ordinary chemical equation teaches, and he would be impressed, by the results of his own experiments, with the importance of determining the conditions under which chemical changes occur, and with the many and varied facts regarding even every-day chemical changes which are not expressed in our chemical notation. The study of the conditions of chemical change would lead on to the study of affinity, of dissociation, and of allied subjects.

Turning again to the study of composition, the student would make determinations of the equivalent weights of a series of similar compounds, and also of several elements; he would determine the molecular weights of a few gaseous bodies, and the atomic weights of one or two elements. He would then proceed to study experimentally some of the methods whereby light is thrown on the constitutions of compounds. For instance, he would determine the specific volumes of a series of carbon compounds, the rates of etherification of a series of alcohols, and the nature of the products of the reaction of such a compound as phosphorus pentachloride with carbon compounds belonging to different classes but showing similarities of composition. Thus the molecular and atomic theory would become a reality to him. Finally, he would be required to repeat an investigation before undertaking himself to advance into the realm of the unknown.

In such a course as this the student would study a series of carefully selected facts; his reading and laboratory work would go hand in hand, each would react upon the other, and so he would be saved from the danger of attempting to draw a distinction between two things which are truly one—chemistry and chemical philosophy.

In selecting the facts and reasoning to be placed before the student of chemistry I think we should now finally abandon the method of treating the elements individually, and rather consider them in groups or classes. If this is done the student soon acquires a fair knowledge of chemical facts; he learns the compositions and properties of groups of similar compounds, he traces some of the connexions between changes of composition and of properties in analogous compounds. By considering the elements in groups the artificial difference between rare and common elements disappears, and this, I think, is a decided gain. The learner also begins to recognize that there are reasons for classing certain elements and compounds together; he sees that order and law pervade the vast domain of chemistry; he connects the constant *atomic weight* with the position of each element in the scheme of classification; and so he gains a basis on which he may rest the superstructure of facts as they are presented to him. This method of treatment inspires the learner with the hope that it is possible to get a firm hold of the subject he is studying. The method is progressive: principles are seen to arise out of the classes of facts considered; each event examined appears as at once the cause and the consequence of other events; generalizing on facts accompanies the acquisition of the facts themselves.

But if the student is expected to learn the properties of each element and its compounds, proceeding from element to element, he generally completely fails to grasp the connexion between similar elements; indeed, he usually and not unnaturally inquires why he should be burdened by these details, which seem to him only unmeaning facts: if he knows and can repeat the properties of half-a-dozen elements, what the better is he for knowing and being able to repeat the properties of a dozen more? The additional facts do not seem to help him to a knowledge of chemistry. And so he either despairs of finding any guiding light in the maze of facts, or he falls into the error of supposing that the maze in which he is wandering without a clue is chemistry. It is the old school method again, which teaches the uselessness of knowledge. When we look back on our school days do we not regret the hours wasted on learning rules and exceptions to rules? Do we not remember how hopeless it all was? how little we advanced? how we spent a year on this subject and a year on that, and failed to gain a grip of any? Did we not sometimes believe that every branch of knowledge was merely a collection of rules? Do not then let us teach science as we learned grammar; else the burden we attempt to bind on the shoulders of our pupils will be more grievous even than that which we bore ourselves in our youth.

The third point on which I would insist is that chemistry should be taught so as to impress the student with the importance of the subject, and with the fact that chemistry is a living and growing part of the scientific study of nature. A teacher of any branch of natural science must thoroughly believe in his subject; and he must have a vivid and active imagination. The subject he has to teach is so immense that, unless his imagination is clear and active, he forms blurred and inaccurate images of the natural occurrences which he wishes to put before his pupils, and so presents them with poor distorted pictures in place of the vivid and vivifying realities of nature.

I see no way of impressing chemical students with the greatness of the subject of their study other than that of constantly keeping before them the many-sidedness of the problems they are trying to solve; of constantly showing how even the smallest, and apparently quite detached, fact is really in living connexion with the whole science; and from time to time of reminding them that the subject of their study is but one part of natural science and is closely connected with many other branches of scientific investigation. But these things can be done only by the teachers and the taught constantly working side by side in the laboratory at some of those apparently simple chemical problems which branch off in many directions, and thus together gaining a real grasp of the many-sidedness of the subject they are studying. The student is thus convinced that, although "there are no boundaries in nature," yet it is necessary for us to draw boundary lines; he also learns the importance of those very facts which, when separated from each other and from the principles that bind them together, retain only a negative value. Science is more than knowledge, and we must make our students realize this. To be in touch with nature is what we aim at; and knowledge alone will not gain this end. We are striving for a real, living, imaginative, acquaintance with the laws of the universe, in order that our lives may thus become "rich and full and satisfying through realities and not through dreams."

Finally, I think that fear of the examiner acts very prejudicially on much of the chemical teaching of this country, more especially on the teaching in schools. For after all—I speak as an examiner—even the youngest examiner is fallible. He is not so very terrible a person as some teachers seem to suppose. Not unfrequently he is a foolish person who knows but little chemistry, and has recourse to text-books for good tips.

If it is really chemistry that is taught, a good examiner will soon find out that the students have learnt the real thing, and a bad examiner will perhaps be incited to leave his rules and definitions, and attempt to gain some real knowledge of the subject in which he examines.

I think that much more care ought to be exercised in selecting those who are to examine the results of the chemical teaching given in schools. Even the seats of the higher learning are scarcely yet impressed with the really tremendous importance of sound scientific teaching; and they still too often wish to get examiners who will set papers for schools in half-a-dozen subjects, instead of selecting men who have made special study of special branches of science, and asking them to examine in their own subjects.

I have not directly insisted much on the importance of research. Of course the aim of all scientific teaching must be to train up men competent to investigate nature for themselves. But unless the men are properly trained, and are taught by the examples as well as by the precepts of their teachers what true scientific research is, they will only add a few more facts to that vast gathering of these "brute beasts of the intellectual domain" which is so often but so falsely called chemistry; and they will persuade themselves that in doing this they are advancing the scientific study of nature.

I would sum up what I have tried to say in a few words. The teaching of chemistry is still too much under the trammels of the old scholastic methods; it stands too much in fear of books, and rules, and definitions. The teacher who is in earnest about his work must break through rules, he must "swallow all formulæ," he must go constantly to nature and take his pupils with him; and his reward will be great.

BOTANY OF THE RIUKIU (LOOCHOO) ISLANDS.

WITHIN the last score of years much light has been thrown upon our knowledge of the flora of Japan. We still, however, know little about the flora of the groups of islands which lie scattered off the coast of her southern boundary. It is true that some botanical collections made during the last few years have shown a certain insight into the flora of some of the archipelago known as the Riukiu or Loochoo Islands, but it is equally true that most of the islands remain as yet absolutely uninvestigated. Since careful studies of the materials, both literature and specimens, scanty as they are, have shown that the flora of the Riukiu Islands form obviously the connecting link between that of Japan, on the one hand, and, on the other, those of South-Eastern China and the Indo-Malayan region through the islands of Hong Kong and Formosa, it seems necessary to take a clear view of the flora of the Riukiu Islands where the boundary lines of those of the two above main regions overlap. Hence it may be worth while to offer a brief summary of our present knowledge of the flora of the Riukiu Islands, taken not only from the materials already presented to the scientific world, but also from those works which have been brought out by the hands of native botanists of Eastern Asia, which have not yet been accessible to most of the Western men of science.

We observe that the earliest records of the plants of the Riukiu Islands are found in that section of the work entitled "Chuzan Denshinroku," or "Records of the Riukiu Islands," which deals with the natural products of that archipelago. These records are detailed in an extended form in another work, the "Riukiu Sambutsushi," or, "The Natural Products of Riukiu." But, as to the works which treat exclusively of the botany of the Riukiu Islands, reference should be made to the "Riukiu Sōmoku Shin Dsu" ("Illustrations of the Plants of Riukiu") and to "Shitsumon Honzō" ("Queries on the Botany of the Riukiu Islands"). The latter, on account of the excellence of its illustrations of plants, and also on account of some interesting facts connected with the preparation of the work, demands here special mention. The author, Go Keishi, a physician of the Island of Okinawa, collected during many years not only the plants of the island in which he lived, but also specimens from the Isles of Takara and Yokō, both of which are situated near 29° N. lat. and 129° E. long. He drew figures, and gave brief descriptions, of these plants, accompanied by dried specimens, forming a collection of about seventy or eighty species of plants at each time, and sent them annually, between 1781–85, to China, in order to acquire further information about those plants which he considered to be doubtful. No fewer than forty-five Chinese representative physicians and herbalists of that time, in various parts of the country, who studied these collections, added information; they also suggested queries, many of which, however, were vague and perplexing. The results thus accumulated formed, in 1789, four volumes, exclusive of a supplementary one. These were divided into sections, "Nai-hen" and "Gwai-hen," referring to those plants used in medicine for internal and external purposes respectively. Each of these two sections is again subdivided into four parts. The work was afterwards published, about 1835, by the order of the then Prince of Satsuma, of the province of Kiusiu, but appears to me to have been printed in Yédo (Tokio). The excellence of the illustrations throughout the work makes it easy for botanists to recognize the characters of plants which are represented in the work. To take an instance, it is interesting to learn in this work, that *Epimedium macranthum*, with its violaceous variety, is found in the Riukiu Islands.[1] There is additional interest in the illustration of a species of *Balanophora*, a genus of the order now known to occur from tropical parts of Asia to nearly 34° N. lat. of Japan, through Hong Kong and the Riukiu Islands; which I have lately treated elsewhere (Journ. Linnean Society, Bot., vol. xxiv.).

Now, to examine the Western literature of the flora before us. Though no small number of plants, recorded as indigenous to the Riukiu Islands, are mentioned in botanical literature,[2] the first compact exposition of the flora, drawn up under the careful examination of a number of specialists, is a memoir which appeared in the fourth and fifth volumes of *Engler's Botanische Jahrbücher*, published in 1883 and 1885 with the title "Beiträge zur Flora des südlichen Japan und der Liukiu-Inseln." This memoir, based principally upon the specimens collected by Döderlein and Tashiro in Ohshima, enumerates ninety-five species of plants from that part of the Riukiu Islands. Of these, three species are shown to be new, one of which, *Asplenium Döderleinii*, belongs to Vascular Cryptogams, while the remaining two, *Sceleria Döderleinii* and *Cinnamomum Döderleinii*, represent respectively Mono- and Di-cotyledons. Among these ninety-five species, sixty were previously known to occur in Japan, and also in other parts of Eastern Asia, the remaining thirty-five being unknown in Japan. Of these thirty-five species, sixteen are known to occur in China and the Indian Archipelago, seven in Australia, and the rest in Malacca, Ceylon, Himalaya, and other places. It must, however, be remembered that a Leguminous plant, *Lotus australis*, collected in the Riukiu Islands, though previously known in Australia, has not yet been discovered in any of the transitional regions.

Early in 1886, Maximowicz published, in the *Bullétin* of the Academy of Science of St. Petersburg, the sixth part of his work on the plants of Eastern Asia, which added no less than fifty species of Riukiu plants to those contained in *Engler's Jahrbücher*. The main portion of these plants were collected by Tashiro, who, working among other groups of the islands, made a preliminary contribution to our knowledge of the flora of Miyako-sima, a small isle lying in 45° N. lat. and 125° E. long. The new species here described from Riukiu Islands are eight, viz. *Euonymus Tashiroi*, *Galactia Tashiroi*, *Erythræa japonica*, *Anœctochilus Tashiroi*, *Premna staminea*, *P. glabra*, *Rhododendron Tashiroi*, and *Webera retusa*. The first five species are endemic to the Riukiu Islands, while the last three are found as well in the adjacent isles of Kiusiu, and in the Ogasawara Islands.[3] I may here remark that a new genus of Rubiaceous plant, established by Ahlburg (in *Bot. Zeitg.* 1878, p. 113), under the name *Aucubæphyllum Liukiuense*, which has remained doubtful, will probably be identified with *Psycotria elliptica*.

Lastly, Forbes and Hemsley's "Index Floræ Sinensis," which is intended to include all plants known in China proper, Corea, and their adjacent islands, but excluding Japan, does nevertheless embrace the flora of Riukiu Islands within its scope. As only the first two parts of this important work have made their appearance, it will perhaps be better not to draw any conclusion at present; still it may be of some interest to point out that we are now practically furnished with our first knowledge of the specimens of plants collected in the Riukiu Islands by Charles Wright and a few other botanists, and deposited in those two great botanical store-houses, the Kew Herbarium and the British (Natural History) Museum.

We cannot but feel how imperfect is our knowledge of the flora of the Riukiu Islands. We should attach much importance to its further investigation. For careful examination of the southern group of the Riukiu Islands will probably bring out, to some extent, the relations between the floras of China and Japan, and between those of the latter and the Philippine Islands, the Indian Archipelago, and perhaps even that of Australia. It will be understood that this southern group of islands, known as Yayeyama, which is situated about 24° N. lat. and between 123° and 124° E. long., remains as yet absolutely uninvestigated. The Yayeyama group consists of nine isles—namely, Ishigaki, Iriomoté, Taketomi, Kobama, Hatoma, Kuro-shima, Arakusuku, Hateruma, and Yonakuni; besides which there are three smaller ones—Uchi-Hanaré, Soto-Hanaré, and Kayama-shima, the two latter being the only uninhabited isles in the group. Hateruma, which is situated in 24° 4′ N. lat., is known as the southern extremity of the Japanese Empire. We rejoice to be able to state that our friend, Y. Tashiro, who had previously made some important collections of the plants of the Riukiu Islands, has, by the commission of the Japanese Government, lately extended his researches to this Yayeyama group, where he resided during 1885 and 1886. We hope that the collection resulting from his incessant labours will be intrusted to competent hands, and that an adequate account of it will soon be published.

Cambridge. TOKUTARO ITO.

[1] Up to the time of the publication of my paper on the "Berberidaceæ of Japan" (Journ. Linn. Soc. 1887, vol. xxii.), I found that no one had collected specimens of this plant in the Riukiu Islands.

[2] Hooker and Arnott's "Botany of Beechy's Voyage" contains some account of the plants collected in the Riukiu Islands.

[3] Generally known as the Bonin Islands, which is probably a corruption of *Munin*-jima, *i.e.* destitute of man, another Japanese name for the islands.

NOTES.

AN interesting collection of specimens has just been received at the Natural History Branch of the British Museum, Cromwell Road, from Emin Pasha. They were despatched from Wadelai in November last, *viâ* Zanzibar, through the kind assistance of Mr. Mackay, of the Church Missionary Society in Uganda, and have arrived at their destination in good condition. The collection consists of skins of birds and mammals, butterflies, and some anthropological objects, and, when worked out by the officers of the Museum, will be described in detail at one of the meetings of the Zoological Society during the ensuing session. In a letter received a few days ago by Prof. Flower, dated Wadelai, April 15, Emin Pasha speaks of a further consignment of specimens (chiefly ethnological), as being ready for despatch to the Museum on the first opportunity.

IN 1884 Mr. John Ball, F.R.S., published a paper in the Journal of the Linnean Society (vol. xviii. pp. 203–240) giving the first comprehensive account of the flora of North Patagonia. This was based on a collection obtained from him during his travels in South America from M. Georges Claraz, a Swiss gentleman who had passed several years chiefly at Bahia Blanca. Mr. J. L. Williams-Andrews has now sent to Kew a beautifully-preserved collection made by him in the same region during the years 1881–85. The excellence of the specimens is the more remarkable as the majority of them have travelled more than six hundred miles on horseback. Mr. Williams-Andrews writes to Kew :—"The Indians are certainly a very fast-decreasing race, and at the present day cannot exceed two thousand in number. The combs, or rather brushes, mentioned [by Mr. Ball in his paper, p. 225] are formed of a species of very tough grass, not of roots. The use of vegetable dyes is also rapidly dying out amongst them, though they still make a considerable quantity of textile fabrics, of which I have numerous examples; the same is to be said of their silver ornaments." Vice-Consul Goodhall, of Bahia Blanca, has taken much trouble to obtain information about the plants used by the Indians for dyeing purposes. He has unfortunately failed in obtaining any trustworthy specimens, as "the Indians are most jealous about affording any information on the subject." Mr. Ronald Bridgett, Consul at Buenos Ayres, has sent to Kew some articles dyed by the Indians, in which the greens and yellows are native dyes made from roots and plants. These have been sent to the Chemistry and Dyeing Department of the Bradford Technical College. A number of Indian ornaments, also obtained by Mr. Consul Bridgett, have been forwarded to the Ethnographical Department of the British Museum.

A HIGHLY ingenious modification of Cowper's writing telegraph has been shown at the American Exhibition, by Mr. J. H. Robertson, an American electrician. The movement of a pen at the sending station varies the resistance of two electric circuits along which two currents are flowing. These varying currents act upon two coils at the receiving station so as to impart motion in two directions to a pen filled with ink, so that the resultant motion of this pen exactly reproduces the movement of the writing pen at the sending station. Mr. Robertson has replaced Mr. Cowper's resistance coils by a series of thin carbon disks, which vary their resistance with variation of pressure, as was discovered by Edison and utilized in his carbon telephone transmitter. He has also improved the receiving portion, and has made the apparatus very practical. It is being commercially worked out in the United States, and we shall watch its progress with much interest. It forms a really beautiful system of written messages, and is decidedly simpler than any previous system of facsimile telegraphy. It is very doubtful whether there is a demand for such a system, for the operation is necessarily slow.

THE session of the International Hygienic Congress at Vienna was closed on Sunday, when it was finally decided that the next session should be held in London in 1891. The meetings were remarkably successful, and did much to enlighten the public as to the nature of the questions which are now being discussed by students of hygienic laws. On Wednesday, September 28, interest was centred chiefly in the Third Section, where the circumstances under which cholera is disseminated were considered. Prof. Max Gruber, of the Vienna University, who gave an account of the incidents of cholera in Austria during the years 1885 and 1886, stated that he could find no evidence of water having played any part in disseminating the disease during that period. He believed that cholera was disseminated by human intercourse, and this experience, he said, coincided with that of English observers. On the other hand, Dr. Spattuzzi, of Naples, attributed the absolute immunity from cholera enjoyed by Naples during 1885 and 1886, and the comparatively small extent of the disease during the present year, to the excellent water supply provided in 1884. Prof. Pettenkofer made some interesting statements on the influences which, in his opinion, locality and season have on the spread of cholera. In support of his views he referred to experiences in India, where each province has its own time of year when the disease is more prevalent, but he also freely admitted the effects of pilgrimages and fairs in spreading the disease. In the course of the debate Prof. Pettenkofer again took occasion to pay a high tribute to England for the measures adopted for the prevention of cholera; and M. Proust, of Paris, expressed himself in the same sense. Thursday was devoted to excursions and the visiting of public institutions in Vienna. On Friday Sir Douglas Galton, who presided over the First Section, offered some valuable remarks on the treatment of infectious fevers. He showed that in London much had been done by the system of isolating small-pox and scarlet-fever patients quickly, by taking them to a ship-hospital, or to hospitals remote from dwelling-houses. "But it is most undesirable," he added, "that in these isolated hospitals too many patients should be concentrated in one ward. The principle should be smaller wards, of four to six patients at most, and great simplicity of construction with ample aëration." Sir Douglas expressed the opinion that the bodies of patients who die of infectious fevers should be burned, and in this view he was supported by Sir Spencer Wells, who said that the good done by giving the people pure air and water, wholesome food, and proper dwellings must to a large extent be counteracted by the continual presence of thousands of putrefying bodies in and around centres of population. On Saturday much interest was excited by the proceedings of the Third Section, when the question of preventive inoculation against rabies was discussed. At the final meeting on Sunday morning the usual votes of thanks were passed, and Prof. Ludwig, the President, said that all the objects of the Congress had been attained. Dr. Roth, of London, expressed a hope that the "protectorate" of the next Congress would be undertaken by the Prince of Wales.

SIR SPENCER WELLS was entertained at a grand banquet on Friday evening last by some of the leading surgeons and professors of Austria-Hungary and by a deputation of the younger surgeons and students. Only one toast was given—that of "The Guest of the Evening." It was proposed by Prof. Breisky, who said that he and many others present were indebted to the teaching of Sir Spencer Wells for much of their success. Replying to this toast, Sir Spencer Wells spoke of the changes which have taken place in surgery during the last thirty-five years, and of the great results accomplished by wise sanitary legislation. "If," he said, "we had in England full power, a competent Minister of Public Health, and an efficient staff of health officers and engineers, the present death-rate of London—that is, 19 in a thousand—might certainly be reduced to 14, or probably to 12."

MR. F. A. BATHER AND MR. G. W. GREGORY have been appointed assistants in the Department of Geology of the British Museum (Natural History), to fill the vacancies caused by the resignations of Mr. William Davies and Mr. Robert Etheridge, Junior.

ON Saturday last the foundation-stone of the Walker Engineering Laboratories in connexion with University College, Liverpool, was laid by Sir James Poole, Mayor of the city. Sir A. B. Walker's original gift to the institution was a sum of £15,000, but Sir James Poole was able to announce that Sir Andrew had generously increased the amount to £20,000.

BY his will, dated August 5, 1887, Mr. Richard Quain, F.R.S., who died on September 15, has bequeathed almost the whole of his fortune, amounting to about £75,000, to University College, London, subject to certain annuities to family connexions. The College will at once benefit to the extent of about £60,000. The annual income is to be applied to "the promotion and encouragement in connexion with University College, London, of general education in modern languages (especially the English language and composition in that language) and in natural science." The trustees, Lord Justice Fry, Sir William Jenner, Mr. George Brodrick, the Warden of Merton, and Sir George Young, are authorized to carry out the testator's wishes either by salaries "or other payments to those who teach, as by endowing professorships, or by pecuniary aid to those who are being taught, or by endowing scholarships, or fellowships, or in any other manner which the trustees may in their absolute discretion think proper," and they are requested to place themselves in communication with the Council of University College with a view to preparing a scheme for carrying out the objects of the bequest. The testator desires that in any statement of the foregoing bequest the name of his brother, the late Sir John Richard Quain, shall be associated with his name.

A SEVERE shock of earthquake was felt throughout Greece at 1 o'clock on Tuesday morning, the 4th inst., the strongest disturbance being on the northern and southern shores of the Gulf of Corinth.

MR. D. NUTT will publish shortly a new and thoroughly revised edition of Breniker's "Logarithms, with Supplementary Tables of Natural Functions and Circular Measures of Angles to each Minute of Arc," by Prof. Alfred Lodge, of Cooper's Hill College.

MESSRS. MARCUS WARD AND CO. will issue in a few days "Teneriffe and its Six Satellites," a new work of travel in two volumes by Mrs. Olivia M. Stone, author of "Norway in June." Together with a narrative of wanderings through the seven inhabited islands, Mrs. Stone gives an historical account of the past race of inhabitants, and she draws attention to the value of the archipelago as a health resort. She has also something to say about the flora, and tables of temperature are appended.

THE third scientific voyage of Prince Albert of Monaco, in his schooner, the *Hirondelle* (200 tons), terminated at the end of August, when the vessel came back to Lorient. The Prince was accompanied by Prof. Pouchet, who made a special study of currents; and by M. Guerne, whose work was zoological. Leaving early in June, they went to the Azores, where three weeks were spent in dredging, &c. A newly-captured sperm-whale was examined. The fauna of the lakes at the bottom of craters was studied by M. Guerne. The Gulf Stream was then crossed, and a thousand of the Prince's floats were thrown out. At St. John's, Newfoundland, researches were continued. The vessel was then directed northward along the coast, but bad weather put an end to the project. In returning to Europe the party encountered a violent storm, in which grave damage was averted by the use of oil. The voyage is considered a great success. A noteworthy feature of it is the carrying on of productive and difficult dredging operations entirely without steam.

EXPERIMENTS have been recently made on the Seine, in the stoppage of steamers in motion, by means of a "cable-anchor" invented by M. Pagan. This is a cable having on it a series of canvas cones, which open out by the action of the water, and close again when drawn in the opposite way. The steamer *Corsaire*, running 13 knots, was stopped each time by the apparatus in seven or eight seconds, and in a space of 26 to 30 feet at the most. For comparison, the steamer, running full speed, was stopped in the usual way, by reversal of the engines. This took at least thirty-four seconds, and the space was 350 to 360 feet. It would thus appear that M. Pagan's apparatus effects the result in less than a tenth of the space, and a fourth of the time, of the ordinary method.

IN clinical practice it is often desirable to be able to measure in a simple and direct way the speed with which nerve-impressions are conveyed. An electric chronometer for this purpose by Dr. D'Arsonval, is described in a recent number of *La Lumière Électrique*. An axis with small terminal plate is driven round uniformly by clockwork, making one turn per second. Opposite the plate is another plate connected with the axis of a pointer on a dial. These axes are independent while a current passes through a small electro-magnet holding the second plate; but when this current ceases, a spring brings the latter in contact with the moving plate and the pointer is carried round till the current flows again. The patient to be examined having shut his eyes, the doctor applies to a part of the body a simple instrument which by this application breaks circuit, so that the pointer begins to travel. The patient is required, immediately on feeling the pressure, to press a button, which makes the circuit again, and the pointer stops. The interval can then be determined in hundredths of a second. The velocity of sensations in different parts of the body can thus be compared very rapidly. It is proved that different sensations (pressure, heat, pricking, cold, electricity, &c.) are transmitted with different velocity, and some diseases abolish some while exalting others, &c.

PROF. MUSHKETOFF'S account of his explorations in the Caspian steppes contains some interesting remarks on the work done by marmots (*Spermophylus eversmanii*) in the modification of the surface of the steppe. They made their appearance in the region only a few years ago, but their heaps of earth already cover hundreds of square miles. Like earthworms, they must therefore be regarded as a factor of some importance in modifying the surface of the soil. Their heaps of earth have an average length of $3\frac{1}{2}$ metres, and a width of $2\frac{1}{2}$ metres, with an average height of from 30 to 50 centimetres, and it was found that on each 2 square metres there were no less than five, seven, or even eight heaps, each of which represented at least 2 cubic metres of earth removed. It may be safely asserted that on each square kilometre of surface no less than 30,000 cubic metres of earth have been brought to the surface owing to their activity. Their influence on vegetation is also well worthy of notice.

AT a recent meeting of the Paris Biological Society, M. Mégnin gave an account of a peculiar disease which is very prevalent at present among hares in Alsace. It is a parasitic disease, a sort of pulmonary tuberculosis, caused by the presence, in the lungs, of *Strongylus commutatus* (*Filaria pulmonalis* of Frölich). The same disease was noticed in Thuringia in 1864.

AN introductory lecture at St. Mary's Hospital was delivered on Monday last by Mr. A. Critchett, Ophthalmic Surgeon to and Lecturer at St. Mary's Hospital. Speaking of the studies in which he himself is chiefly interested, Mr. Critchett said he was old enough to remember the introduction of the ophthalmoscope by that great teacher and thinker Helmholtz, who, he rejoiced to say, yet lives to witness the priceless boon which his discovery has conferred upon the human race. It was difficult for those who are now familiar with its use to conceive the wondering eagerness with which the original workers sought, by the aid of their new weapon, to bring to light those numerous hidden diseases of the eye which had till then been only partially and most imperfectly recognized. Numerous modifications of the instrument have since been introduced, and among the most recent and most useful improvements has been the ingenious adaptation of the electric light to the ophthalmoscope by Mr. Juler. After alluding to the labours of his late father, of the venerated Nestor of English ophthalmic surgery, Sir William Bowman, and of the much lamented Von Graefe, the lecturer reminded his hearers of the colossal work which had been achieved by Prof. Donders, who had opened out a new world for thought and investigation, and had elevated the study of practical optics to the dignity of a science.

A REMARKABLE series of experiments upon the synthesis of water by weight is described by Dr. E. H. Keiser in the current number of the *Berichte* of the German Chemical Society, in which not only has a known weight of oxygen in the form of copper oxide been employed, but has also been made to combine with an actually weighed quantity of hydrogen. In the well-known experiments of Dumas it will be remembered that an indefinite quantity of hydrogen was employed, the loss of oxygen by the copper oxide and the weight of water formed furnishing the only data obtainable by the then possible experimental methods. But Dr. Keiser has succeeded in weighing his hydrogen by taking advantage of its peculiar property of being occluded by the metal palladium; it is shown that a piece of palladium 100 grammes in weight will readily take up between 0·6 and 0·7 grammes of hydrogen, when heated in a stream of the gas to about 150°. The palladium-hydrogen compound formed is perfectly stable at ordinary temperatures, and may be preserved unchanged in a vessel filled with hydrogen gas; but on heating this *quasi*-alloy the gas is slowly driven out again, and by weighing before and after the heating, the weight of hydrogen expelled may be accurately determined. This weighed quantity of hydrogen gas was then passed over heated copper oxide, and the weight of water formed determined in the usual manner. But Dr. Keiser has gone further than merely synthesize water by the most direct means possible, he has refined the process so far as to be enabled to employ it as a direct means of determining the atomic weight of oxygen. The minutest precautions were taken against error, and the purification of the hydrogen carried out in a most thorough manner, with the unexpected result that the atomic weight of oxygen is most probably slightly lower than 15·96 and more nearly 15·87. It is interesting to be reminded that Dumas states in his memoir:—"Of all analyses, that of water involves the most uncertainty. It is true that one part of hydrogen combines with eight parts of oxygen to form water, and nothing could be more exact than the analysis of water, if one were able to weigh the hydrogen and the water formed by its combustion. But the experiment is not possible in this form." However, owing to the ingenuity of Dr. Keiser, this happy result has now been achieved.

THE *Times* of September 22 contained an interesting account of the Troglodyte remains in Southern Morocco. The difficulties placed in the way of exploration in Morocco have prevented an examination hitherto of these remains. The Troglodyte caves are situated near Ain Tarsil, a village some three days' ride to the south-west of the city of Morocco, near the borders of the province of Haha. At the village the scenery undergoes a complete change. Instead of the dreary plains one comes across curious hills divided by great ravines, not unlike the cañons of California, in one of which is situated the strange city of the Troglodytes. The gorge is a narrow one, the cliffs on either side rising almost perpendicularly from the bottom of the deep valley; after progressing some little way along this valley the first caves are sighted. They are cut in the solid rock at a great height from the ground, and are in some places single, in others in tiers of two or three, one above the other. The entrances are small, varying from 3½ to 4½ feet in height and about 3 feet in breadth. In places where the rock has fallen away the entrances are faced with masonry of a neat and orderly type, and in one or two cases where the natural formation of the rock necessitated exceptionally large entrances this masonry served also the purpose of dividing the door into two parts, one of which no doubt served as a window. As to the means of access to these caves, the writer discusses three theories, all possible, but one alone of which is probable. The first is that the face of the cliff has since the formation of the caves been worn away by wind and water, and so crags and projecting pieces of rock that once rendered access possible have disappeared; secondly, the Troglodytes may have been so active as to be able to get up perpendicular smooth rock; the third, that they used ropes and ladders. He dismisses the first as unjustified by anything we know of Morocco, and inconsistent with the existence of various tiers of cliffs; and as to the second, although it is stated that these cave-dwellers were swift of foot as a horse, the shape of the cliff renders climbing an impossibility. The walls of rock are perfectly smooth from the bottom to the height of the caves, which is 200 to 300 feet in many cases, and it is only in a few places that there are ledges sufficiently wide for birds of prey. In favour of the ladder theory there is the circumstance that in the doorways of many of the caves there still remains, a few inches above the floor, and crossing from lintel to lintel, a bar of wood some six inches in thickness, which must have acted as a roller for ladders. The freshness of the wood is surprising, but in the Karli Caves in the Western Ghauts there is a roof which is considered by experts to be coeval with the caves themselves. The writer was able to enter the caves in only one or two cases, a landslip having built a pile of earth and stones that rendered access possible. Passing through the low, narrow doorway, the visitors found themselves in an oblong room 15 feet by 7, leading from which at either end were smaller chambers. The wall of the larger room adjacent to the cliff was pierced with three windows; each of the smaller ones also possessed a window. The walls were rough, but the floor and ceiling were well smoothed and cut. The height of each was about 5 feet 2 inches. In no cases were bones discovered, only a little broken pottery and one or two doubtful flint-heads alone being found. No doubt much greater success will await the explorer who manages to overcome Moorish prejudice and bigotry, and by means of ropes and ladders enters the upper tiers of caves which have lain in their present state since the old race died out or took to more rational abodes. The Troglodytes cannot have been such savages as they are usually considered: their excavations are hollowed out skilfully, and their abodes show signs of great labour and some idea of care and comfort.

THE additions to the Zoological Society's Gardens during the past week include two Macaque Monkeys (*Macacus cynomolgus*) from India, presented by Miss Barker; two Arctic Foxes (*Canis lagopus*) from the Faroe Islands, presented by Mr. T. Nordenfelt; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. R. Taylor; a Corn-crake (*Crex pratensis*), British, presented by Mr. Howard Bunn; a Ring Dove (*Columba palumbus*), British, deposited; a Sumatran Wild Dog (*Canis javanicus* ♀) from Sumatra, purchased; a Wedge-tailed Eagle (*Aquila audax*) from Australia, received in exchange; a Mule Deer (*Cariacus macrotis*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

FLAMSTEED'S STARS "OBSERVED BUT NOT EXISTING."—Prof. C. H. F. Peters, in a paper published in vol. iii. of the Memoirs of the National Academy of Sciences of Washington, has discussed Flamsteed's observations of the twenty-two stars which, it will be remembered, Baily, in his "Account of the Rev. John Flamsteed," entered in a list with the above heading, which, as he explains, means stars "of which the observations appear to be accurately recorded, but which still cannot now be found in the heavens." As the disappearance of so many stars in comparatively so short an interval of time appeared to Prof. Peters to be rather improbable, it seemed to him to be desirable that these cases should be scrutinized with the additional means of identification provided by the *Durchmusterung*, which of course was not available at the time of Baily's publication. Acting, then, on the assumption that some otherwise plausible error in Flamsteed's entry of his observation which leads to a modern star-place is to be held much more reasonable than the vague acquiescence in a supposed disappearance of the star, Prof. Peters considers that (making allowance for the probable error of a position in Flamsteed's Catalogue) he has succeeded in finding for every case at least a probable explanation. One of the observations in question—that of the object No. 1647 in Baily's "Flamsteed"—turns out to have been an observation of the planet Uranus, an explanation which was suggested by Argelander but rejected by Baily. The agreement, however, of the position of the object observed by Flamsteed with the place of Uranus computed from Newcomb's Tables is so close as to leave no doubt of its identity.

CORRIGENDA IN VARIOUS STAR-CATALOGUES.—The paper following the above in vol. iii. of the Memoirs of the National Academy of Sciences is likewise by Prof. Peters, and contains a very extensive list of corrigenda to various star catalogues, the great majority of which have hitherto been unknown. Following Argelander's example, Prof. Peters has not been content with simply detecting an error, but has in most cases turned to the original observations, when accessible, to discover the source of error. The catalogues in which the corrections, nearly 700 in all, have been made, are: Oeltzen's Catalogue of Argelander's southern zones, Vol. vi. of the Bonn observations, Weisse's Catalogue of Bessel's zones, between Decl. -15° and $+15^\circ$, and between $+15^\circ$ and $+45^\circ$, Rümker's Catalogue of 12,000 stars (original catalogue and the new series), Schjellerup's Catalogue, Baily's Lalande, Yarnall's Catalogue (second edition), the Glasgow Catalogue of 6415 stars, Moesta's observations at Santiago de Chile, and the Geneva observations, 1842–49. The corrigenda which Prof. Peters has discovered for the Harvard zones have been published in the Annals of the Harvard College Observatory, vol. xiii. pp. 188–208, and are not given here; those for the Washington zones have been communicated to Prof. Holden, and those for Lamont's publications to the astronomers of the Munich Observatory. Prof. Peters certainly deserves the hearty thanks of all astronomers for the very essential service he has rendered in the detection and publication of these errors in their standards.

Besides these lists of errata, Prof. Peters also gives in the same memoir a list of 191 "Anonymous" stars in Yarnall's Catalogue, which he has identified with stars in other catalogues.

THE "SATELLITE" OF VENUS.—One of the standing enigmas of astronomical history has been the number of observations, the great majority of which were made in the years 1761 and 1764, by astronomers of reputation, of some small object close to Venus which was supposed to be a satellite of the planet. The fact, however, that the observations were consistent with no possible orbit, and that no trace of the body has been seen for more than a century, were conclusive proofs that it was not a real satellite, and many theories were started to account for the observations, but all of them were open to one or more fatal objections. The problem, however, seems now to have been fairly solved at last. M. Stroobant, in a paper which he has recently communicated to the Académie Royale de Belgique, gives evidence to show that in a large number of instances the object supposed to be a satellite was actually a star. He was led to this conclusion from the result of an inquiry into the observation of Rœdkiœr and Boserup, August 4, 1761, in which a star, as well as the "satellite," was mentioned as being near the planet. Curious to find out what star this was, he reduced its place and found it to be χ_3 Orionis. It then immediately struck him that the so-called satellite occupied the place of χ_4 Orionis. An attentive examination of the other observations showed that in many cases these also were probably of stars; that of Horrebow's on January 3, 1768, was unquestionably an observation of θ Libræ. Not only did the star occupy the precise place indicated for the "satellite," but the motion of Venus was such as to produce just the apparent motion ascribed to it. Several other observations can be almost as clearly referred to some star or other; the chief objection to such identification—that some of the stars in question are too faint to have been seen near the planet—being overruled by M. Stroobant's own observations, he having found that with a telescope of 6 inches aperture a star of the eighth or even of the ninth magnitude could be seen in the immediate neighbourhood of Venus. In order to present the whole question as fully as possible in one view, M. Stroobant has exhibited all the thirty-three observations in tabular form, has given all the various particulars required referring to Venus, together with an abstract of all the different theories hitherto broached respecting the true nature of the "satellite," and has reprinted the original observations themselves, whilst his own identifications with stars are illustrated by a neat series of little star-maps. Only one series, those of March 1764 (printed by a curious typographical error as Mars 1861), by Rœdkiœr remain without at least a plausible explanation, and it is possible that in this case it may be found that one of the brighter minor planets was sufficiently near to Venus to be seen in the same field with her. At all events, for the great proportion of the observations M. Stroobant has fairly cleared up the mystery which has perplexed astronomers so long.

THE LEANDER MCCORMICK OBSERVATORY.—From Prof. Ormond Stone's Report for the year ending June 1, 1887, recently received, we learn that the 26-inch refractor has been chiefly employed as heretofore in observations of nebulæ. 351 observations of miscellaneous nebulæ have been made during the year, as well as a large number of sketches, and 270 nebulæ have been discovered, which are supposed not to have been hitherto detected. Efforts have also been made to determine with as much accuracy as possible the positions of a select list of nebulæ in order that materials may be accumulated for the determination of their proper motions. Special attention has been paid to the nebula in Orion. Holding that it is not to photography but to photometry that we must look for the earliest possible evidence of change in this object, Prof. Stone has repeatedly examined the brighter portion of the nebula for the purpose of determining the relative brightness of the various condensations of which it is composed. The region "A" preceding the trapezium has been especially observed, and the brightness of its condensations compared with one another and with portions of the "Huyghenian" region. Estimates have also been made of the relative brightness of the stars in the brighter portion of the nebula in order to trace if possible any connexion which may exist between them and the nebula.

THE NICE OBSERVATORY.—M. Perrotin has just published the second volume of the "Annals of the Nice Observatory"; the first, which will contain a description of the Observatory and of its instruments, has not yet appeared, but is in preparation. The present work contains six sections, of which the first is devoted to the determination of the difference of longitude between Paris and Nice, and between Nice and Milan, which M. Perrotin set about making immediately on the foundation of the Observatory and his appointment to the Directorate. In this work he was joined by Commandant Bassot, who observed at the Observatory of the Dépôt de la Guerre, Montsouris, during the first part of the operations, and then exchanged places and instruments with M. Perrotin, the better to eliminate personal errors; M. Celoria, of the Brera Observatory, Milan, co-operating with M. Perrotin in the determination of the Nice-Milan longitude. The observations were made in the autumn of 1881, and the final results agreed very closely with the Milan-Paris longitude which had been determined in the preceding July and August by Colonel Perrier and M. Celoria, as the following figures will show:—

	h. m. s.	h. m. s.
Paris-Nice ...	0 19 51·513 ± 0·01	0 27 25·325
Nice-Milan ...	0 7 33·812 ± 0·01	
Paris-Milan (direct)		0 27 25·315

The Montsouris instrument being 0·288s. to the west of the meridian of Paris, the longitude of the pillar upon which the Nice meridian instrument was mounted is 0h. 19m. 51·225s. east of Paris.

The second section contains the determination of the provisional latitude of the Observatory, which was found to be 43° 43′ 16″·9; the pillar of the small meridian instrument being still, as for the longitude, the place from whence the observations were made. The third section contains a fine series of micrometric measures of double stars, made by M. Perrotin with an equatorial, by Eichens and Gautier, of 0·38 metre aperture and 7 metres focal length; the observations of comets and planets, which were published as made in the *Comptes rendus* of the Paris Academy, and which occupy the fourth section, being made with the same instrument. Some important notes on solar spectroscopy by M. Thollon follow in the fifth section, and include several remarkable observations of solar storms, a correspondence with M. Faye on the interpretation to be attached to the displacements and contortions of the spectral lines, and a study of the B and D groups in the solar spectrum. The concluding section contains notes by MM. Thollon and Puiseaux on the total solar eclipse of May 17, 1882; on the transit of Venus, 1882, by M. Thollon; on the remarkable crepuscular glows and "coronæ" of 1883–84, by MM. Perrotin and Thollon; and elements of Comet 1885 II. (Barnard) and of Minor Planet No. 252 (Clementina), by M. Charlois. The volume is illustrated by seven beautifully finished plates; one of which, viz. that to illustrate M. Thollon's paper on the group, affords an example of the fullness of information and beauty of execution of M. Thollon's drawings of the solar spectrum which are now being engraved for publication in the forthcoming third volume of the "Annales de l'Observatoire de Nice."

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 *OCTOBER* 9–15.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on October 9

Sun rises, 6h. 15m.; souths, 11h. 47m. 20·0s.; sets, 17h. 19m.; decl. on meridian, 6° 15′ S.: Sidereal Time at Sunset, 18h. 31m.

Moon (at Last Quarter October 10, 5h.) rises, 21h. 6m.*; souths, 5h. 6m.; sets, 13h. 9m.; decl. on meridian, 19° 54′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury	8 10	12 58	17 46	14 30 S.
Venus	4 9	10 5	16 1	1 34 S.
Mars	1 32	8 48	16 4	13 49 N.
Jupiter	8 27	13 19	18 11	13 52 S.
Saturn	23 31*	7 20	15 9	19 16 N.

* Indicates that the rising is that of the preceding evening.

Occultations of Stars by the Moon (visible at Greenwich).

Oct.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
11	ζ′ Cancri	4½	3 48	4 16	343 299
12	π′ Cancri	6½	5 57	near approach	323 —
13	Regulus	1½	4 44	5 50	34 225
14	χ Leonis	5	5 15	near approach	305 —

Oct.	h.	
10	13	Venus stationary.
11	15	Saturn in conjunction with and 1° 20′ north of the Moon.
13	8	Mars in conjunction with and 0° 19′ north of the Moon.
14	8	Mercury in conjunction with and 2° 58′ south of Jupiter.
14	14	Venus in conjunction with and 7° 52′ south of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3	81 16 N.	Oct. 13,	4 32	*m*
ζ Geminorum	6 57·4	20 44 N.	,, 9,	21 0	*m*
			,, 14,	21 0	*M*
S Canis Minoris	7 26·6	8 34 N.	,, 13,		*M*
S Herculis	16 46·8	15 8 N.	,, 14,		*M*
U Ophiuchi	17 10·8	1 20 N.	,, 12,	3 11	*m*
			and at intervals of	20 8	
X Sagittarii	17 40·5	27 47 S.	Oct. 13,	0 0	*m*
			,, 15,	21 0	*M*
U Sagittarii	18 25·2	19 12 S.	,, 11,	0 0	*m*
			,, 13,	23 0	*M*
η Aquilæ	19 46·7	0 43 N.	,, 12,	21 0	*m*
S Sagittæ	19 50·9	16 20 N.	,, 11,	22 0	*m*
			,, 14,	22 0	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A. °	Decl. °	
Near γ Persei	44	55 N.	Slow.
,, 41 Arietis	45	26 N.	Swift.
,, τ Geminorum	103	33 N.	Swift; streaks.
	135	80 N.	Swift; streaks.

GEOGRAPHICAL NOTES.

M. SERRANO's recent expedition, and the second expedition which was organized by the Chilian Government in order to determine the watershed between the east and west coast of South America, have settled the most interesting fact that the high chain of the Andes in these regions does not form the watershed between the Atlantic and Pacific Oceans, but that it lies somewhat further east of it, on a plain about 500 metres high. The rivers which rise here and flow towards the Pacific have their source in small lakes, and pass through the Cordilleras in narrow gorges very difficult to penetrate. The land from the eastern slope to the watershed which forms, according to Chilian reckoning, the boundary between Chili and the Argentine Republic, is pampa, and well adapted for cattle-breeding.

CONSUL PLUMACHER, of Maracaibo, in his last report says that the peninsula of Goajira, which forms the extreme north-western part of Venezuela, is chiefly remarkable for its entire abandonment into the hands of the Indians of the same name, who have succeeded up to the present day in preserving their absolute independence, recognizing no authority except that of their own chiefs. They are divided into different clans, or tribes, all, however, being of the same race, with similar language and customs, and the different divisions now existing are developments of individual families of the same general stock. The Venezuelan Government has contented itself with placing a military post on the frontier for the protection of the whites who, attracted by the fine grazing country, have established cattle-farms and small settlements in the neighbourhood. In spite of this precaution, the Indians at times combine in numbers of several hundreds, and make a raid into the civilized territory, retreating to their own domain with the plunder. The Indians know but little of agriculture, but engage largely in the breeding of cattle. Maize and vegetables are cultivated on a small scale, and cotton, which grows wild in some localities, gives exceptional returns when any attention is paid to its culture. The customs of the Goajiras are singular and interesting, and it is noticeable that their laws and usages have remained the same from time immemorial. One of their most striking customs is a complicated system of what is called by them "payment of tears and blood," and this is the principal cause of conflict between the clans. Among all savages revenge is a sacred duty, and as, according to Goajira ethics, an entire tribe is supposed to be responsible in the aggregate and individually for the acts of one of its own members, a trifling affair in the beginning may produce grave consequences ultimately. This is one of the reasons why it is dangerous for white men to enter the Goajira territory, as the Indians make no distinction of nationality, but consider all who are not of themselves as belonging to one great family, all the members of which are responsible for a real or fancied outrage committed by an individual, and any of whom are to be considered to a certain extent as a hostage for the conduct of the rest. By the payment of the compensation of tears and blood, any injury inflicted may be condoned, it being noticed that it is not the aggrieved individual who demands this payment, but his relatives, especially those on his mother's side, who are supposed to be of closer relationship than the family of his father. If an Indian accidentally wounds himself, breaks a limb, or meets with any similar accident, his mother's family immediately demand of him the "payment of blood," on the theory that, as his blood is also their own, he has no right to shed it without compensation. The relatives of the father also claim the payment of their tears, which is of less value. Even the friends who may have witnessed the accident are entitled to compensation for the grief into which they are plunged at seeing their companion suffer. To such an extreme is this system carried out, that should a child die in the absence of one of its parents, the one can demand from the other payment for the tears supposed to be shed over the occurrence.

MUCH attention has been attracted in Australia by the results achieved by Mr. Theodore Bevan in his recent exploring expedition in New Guinea (see NATURE, August 11, p. 351). From a letter addressed to the *Times* by Mr. Thomas Bevan (September 27) we learn that the New South Wales Government have placed at Mr. Theodore Bevan's disposal a suitable steam-launch for further investigation, while the Queensland Government have allowed him the services of a thoroughly competent surveyor, and have offered the use of the steamship *Albatross* to tow the launch over to New Guinea waters. An influential committee has been formed at Sydney for the purpose of promoting Mr. Bevan's work. It was expected that the new Expedition would start in the course of September. Mr. Bevan will carry on his investigations between 200 and 300 miles to the north-west of Port Moresby, and at a still greater distance from the site of the explorations now being made on the Owen Stanley Range by the Victorian branch of the Royal Geographical Society.

ANOTHER advance has been made by Australia towards the fitting out of an Antarctic Expedition. The Agent-General for

Victoria, Sir Graham Berry, has addressed a letter to Sir Erasmus Ommanney, informing him that, in accordance with instructions, he has asked Her Majesty's Government if they would contribute the sum of £5000 towards an Antarctic exploring expedition, provided the Australian colonies contributed a similar sum. Sir Graham has received (September 2) a letter from the Colonial Office, stating that the subject is now under the consideration of Her Majesty's Government. Not only for the sake of promoting science, but also the good feeling and bond of union which should exist between mother-country and colonies; let us hope the answer will be favourable. Here at least is a common work, for the benefit and honour of both. If the reply is favourable, the Agent-General is instructed to communicate with Sir Allen Young, with the view of ascertaining on what terms he would take the command of such an Expedition. If there is any obstacle in the way of a money grant, why should not a suitable vessel be placed at the disposal of Australia?

LIEUTENANT VAN GELE has started for Bangala Station, under instructions from the head-quarters of the Congo Free State at Brussels, for the purpose of solving the problem as to the connexion, if any, which exists between the Wellé and the Mobangi. It is clear that Mr. Stanley does not mean to face this problem, as it was hoped he would do.

METEOROLOGICAL NOTES.

THE new Chief Signal Officer of the United States is making some sweeping changes in the meteorological service. We regret that the series of simultaneous meteorological observations taken at noon, Greenwich time, which began in 1875, at the instigation of the Vienna Meteorological Congress, is to be given up at the close of the present year, from lack of funds. This service has developed from a comparatively limited work to one of great magnitude, covering almost the whole of the northern, and part of the southern, hemisphere. For some time the observations were reduced, and published in the form of daily bulletins and maps, but the continued reduction of the amount at the disposal of the Chief Signal Officer rendered it necessary to give up this great and useful publication, and to limit the work to the issue of a monthly "Summary and Review of International Meteorological Observations," containing the monthly means of all the observations, with explanatory text and maps of the average isobars, isotherms, winds, and tracks of areas of low pressure. This valuable publication will be continued up to December 1887, to complete the data for ten consecutive years in a shape convenient for further research. General Greely states that it is further intended to publish charts of the average monthly pressure and temperature for each month of the year, based on ten years' international observations.

FOR some years Prof. Cleveland Abbe has been engaged, under the superintendence of the Chief Signal Officer of the United States, in the preparation of a general bibliography of meteorology, which has been very largely contributed to by Mr. Symons, by Dr. Hellmann of Berlin, and others; the number of books and pamphlets now catalogued amounts to about 52,000. Prof. Abbe stated, at the recent meeting of the British Association, that the work is now practically complete, and ready for publication. The General Committee of the Association fully recognized the high importance of the work, and expressed a hope that its publication by the Signal Office would speedily render it accessible to all nations.

THE last number of the *Annuaire de la Société Météorologique de France* for April and May contains two interesting papers. (1) On the distance of the arc of the aurora borealis from the ground, deduced from the variation of its angular velocity, or from its breadth, by M. Carlheim-Gyllenskjöld. The author states that the observations made during the Swedish expedition to Cape Thorsden prove that the angular velocity of the movement of the arc increases according to a regular law as the arc rises from the horizon towards the zenith, and that its more or less rapid change depends chiefly on the vertical elevation of the arc above the ground. The formula employed in the calculation is fully explained, and the result arrived at is that the mean height of the aurora borealis is from 30 to 45 miles above the earth, which agrees very closely with the results obtained at Ice Fjord by the Swedish Expedition. (2) A paper by M. G. Guilbert on the prediction of clouds and their succession throughout the day. The author finds that the first arrival of clouds, their movement over us, and their disappearance below the horizon are not left to chance, but on the contrary follow a regular order which renders prediction possible. Several examples are given of the connexion between the succession of the clouds and barometric depressions. The same journal also contains a communication by M. G. Tissandier on an extraordinary decrease of temperature observed in a captive balloon, on January 15 last, near the Champ-de-Mars. The wind was very strong from north-east, and the temperature at the ground was 24°·8 F. at 1h. 30m. p.m., while at about 330 feet it fell to 20°·3. At 1h. 50m. a second ascent of nearly 600 feet was made, where the temperature was 19°·1, showing an unusual diminution in the upper regions, especially as the weather at the time was very cloudy.

THE *Annuaire de l'Observatoire de Montsouris*, near Paris, for the year 1887, has been somewhat late in publication, apparently owing to recent changes in the management of the Observatory. M. Marié-Davy, who had charge of it since 1873, has retired, and from January 1 last the Observatory has ceased to be a Government establishment, and has been taken over by the Municipal Council of Paris. The work of the Observatory is, as before, divided under three heads: (1) Meteorology properly so called, and its application to agriculture and hygiene, together with magnetism and electricity; (2) chemical analysis of the air and of the rain-water collected at Montsouris; (3) microscopic study of the organic dust held in suspension in the air and water, each of these services being intrusted to a separate scientific man under the supervision of a special Commission. The *Annuaire* contains elaborate discussions under each of these heads; the temperature ob ervations date from 1699, and rainfall observations extend from 1689 to 1886; those prior to 1873 were taken at the Paris Observatory. The highest shade temperature last year was 91°·0 on July 21, and the lowest 18°·1 on January 24; the mean for the year was 52°·0. The thermometer screen is an open stand sheltered at top and sides, unlike those used in this country, and the year dates from October or December, being what is called the agricultural or meteorological year; this want of uniformity renders it difficult to compare the observations with others. The greatest monthly rainfall was in June, being 4·57 inches, and the least in February, 0·71 inches. The apparatus used in the different investigations is clearly illustrated.

PROF. HUGO MEYER discusses, in the *Nachrichten der k. Gesellschaft d. Wissenschaften* of Göttingen (No. 9, 1887), the thunder-storms at that place during the years 1857–80. The discussions of thunder-storms have hitherto mostly been for large areas, hence the results of a long series of observations referring to a single place have a special interest. The observations now in question were carefully made by M. Listing, and are preserved in the Physical Institute at Göttingen. They show, with regard to the yearly period, two principal maxima: the first occurring about the beginning of July, being later than at many other places—for instance at Prague and Munich, which have their second maximum about that time; the second maximum at Göttingen being about the middle of August. These observations also show two secondary maxima of thunder-storm frequency, one in the spring (April 1–10) and another in the autumn (September 28 to October 7): the first being a period of unusually rapid increase of temperature; and the second, one of a relatively slight fall of temperature; such a late autumn maximum being of rare occurrence. With regard to the daily period, two maxima occur in all months, one at the warmest part of the day, and one at midnight. In the winter half-year both the maxima occur some hours earlier than in the summer half-year, and the afternoon maximum in winter is divided into two parts. The occurrence of these double maxima, both in the yearly and daily periods, has been previously pointed out by Prof. von Bezold with regard to the thunder-storms in Bavaria. The tables show that thunder-storms at Göttingen only come from between N.W., through N., and round to S.E. in the warm daily and yearly periods, which tends to prove that they are heat thunder-storms. The cyclonic thunder-storms come almost exclusively from a westerly and south-westerly direction. The yearly march of thunder-storm frequency at Göttingen and various other places for the eight principal points of the compass is clearly shown by graphical representations, in the form of wind-roses; the mean direction of motion of all the storms at Göttingen is nearly from S. 68° W.

THE *American Meteorological Journal* for August contains an important article by Prof. W. Ferrel, on the relation of the pressure to the velocity of the wind. He points out that the formula generally used by English and American engineers and meteorologists, and which seems to have come down from a preceding century, is undoubtedly very erroneous. The formula, viz. $p = 0{\cdot}005\ v^2$, is used at all altitudes and for all temperatures, without regard to the varying densities of the air. The true theoretical formula—that is, one that would hold good in case of no viscosity of the air—is given at p. 302 of his "Recent Advances in Meteorology" (NATURE, July 14, p. 255). For an average temperature of, say, 15° C., and air of the standard pressure of 760 millimetres, this formula becomes $p = 0{\cdot}00255\ v^2$, which gives the ratio 1 : 1·96 between the two constants, from which it follows that the velocities usually deduced from pressures should be very considerably increased. The author also objects to the use of the constant 3 which is employed in the reductions of wind-velocities obtained from a Robinson's anemometer of the Kew pattern, and which is about one-fourth too large, except for low velocities, as is shown by recent experiments by Stokes and Whipple in this country, and by others abroad. The same journal also contains interesting articles on the comparison of rain-gauges, by F. Pike, and on tornadoes, by H. Allen. The latter recommends the adoption of the term "low area," or "helicone," instead of cyclone, which he thinks should be applied to West Indian storms only.

THE results of rain and river observations made in New South Wales and part of Queensland during 1886, published by the Government Astronomer for New South Wales, contain a large quantity of valuable statistics on the distribution of rain, the heights of rivers, and evaporation. The number of stations in New South Wales has increased from 641 in 1885 to 772 in 1886, yet there are many parts of the colony still unrepresented. The Report is accompanied by a map, showing very plainly by means of black spots of various sizes the increase in the amount of rainfall as we go northwards into tropical regions, until Innishowen, in Queensland, caps the list with 176 inches. The greatest average rainfall in New South Wales is only 64 inches, at Antony, just under a very high mountain range, and next to this Port Macquarie, 60 inches. The mean rainfall for the whole colony amounted to 26·04 inches in 1886, being 11 per cent. more than the average for the past twelve years.

THE Meteorological Council have issued a new edition of their "Fishery Barometer Manual." The first edition of this work was published by Admiral FitzRoy about thirty years ago, and was freely distributed by the Board of Trade to small ports and fishing-stations supplied with public barometers. This useful practice of supplying barometers to fishing-stations has been continued to the present time, nearly 170 barometers having been erected, in addition to those issued by the Royal National Lifeboat Institution. The present Manual contains much additional elementary information likely to be of use to the fishermen, and refers briefly to the recent advances in the development of weather prediction, especially by means of daily charts. Reference is also made to the telegrams now received daily from America, and to the warnings issued by the *New York Herald* Service. The Manual also contains a table showing the distribution of gales on our coasts during fifteen years, from which it appears that November is generally more stormy than December, and that the maximum storminess in March, which is especially marked in North-East England, entirely disappears in South-West Ireland and South-West England.

WE had occasion recently (NATURE, June 23, p. 184) to refer to the active steps taken by Mr. Clement L. Wragge in promoting the meteorological service in Queensland, and we have now to record a further development by the publication of daily weather charts for Australasia. The charts are drawn for 8 a.m. daily, giving isobars, wind direction and force, and the temperature and humidity of the air. Rainfall is represented by dots of various sizes, while other phenomena, such as dust-storms, fog, hail, &c., are shown by appropriate symbols, and there is also a synopsis of the existing weather. The charts will be of great utility in the study of the weather of the Australian colonies.

WE are pleased to notify the publication of a Monthly Weather Record for the Mauritius, the first issue of which, for January last, has been received. The Record, which is after the style of the United States Weather Review, but without plates, contains the results of observations taken at the Royal Alfred Observatory, ogether with the means and extremes of temperature at four other stations, rainfall observations taken at fifty-five stations, observations taken at Rodrigues and the Seychelles, and observations taken on board ships in the Indian Ocean. The Observatory of Mauritius stands on a plain near Port Louis, three miles from the west coast, 179 feet above the sea-level. From west-south-west through west to north there is an uninterrupted view of the sea, and from north through east to south-east the ground generally slopes to the summit of the Piton, four miles distant, and 917 feet above the sea. Between south-east and south-west there is a chain of mountains, the highest peak of which bears nearly six miles due south, and has an altitude of 2874 feet above the sea. Among the miscellaneous observations it is noted that the tail of a comet (supposed at first to be Barnard's comet) was seen on January 20, and three subsequent evenings from various parts of the island.

THE BRITISH ASSOCIATION.

SECTION A—MATHEMATICAL AND PHYSICAL SCIENCE.

On the Magnetization of Iron in Strong Fields, by Prof. Ewing, F.R.S., and Mr. W. Low. Read by Prof. Ewing.—In the experiments described iron was subjected to very intense magnetization by placing a narrow neck between two massive pole-pieces. In this way values of magnetic induction higher than those previously reached had been attained. Through the kindness of Prof. Tait the large electro-magnet of the Edinburgh University had been transferred to University College, Dundee, and by its means the induction was pushed up to the value of 38,000 C.G.S. units. There seemed, indeed, to be no limit to the value attainable, and so the neck was then turned down to about one-sixth of its previous diameter, and the induction was forced up to 45,000. By turning the neck still further and annealing it, the highest value of 45,350 was reached. An attempt was made to determine the strength of the magnetic field in the immediate neighbourhood of the neck. The quantity $\frac{B - \text{field}}{4\pi}$, where B was the magnetic induction, was found to change from 1680 in an experiment where B was 24,700, to 1420 in the case of the highest value of B attained. This would favour the idea that the intensity of magnetization has a limit. But it is difficult to be quite sure that the field in the immediate neighbourhood of the neck is the same as in the neck itself.

In order to overcome this difficulty the field in the air round the neck was explored by means of three or four coils wound one on top of the other. This will show if the field is varying fast near the iron. If not, it would be natural to assume that the field is much the same as in the iron, because in the median plane there is no surface magnetism.

On Some Points in Electrolysis and Electric Conduction, by Prof. G. Wiedemann.—Before proceeding to the discussion of electrolysis the author wished to congratulate the Association on the appointment of a Committee to investigate this important subject, and further to congratulate the Committee on having Prof. Lodge to direct their labours. He had read with great interest the able report on electrolysis which had been some time ago presented to them by Prof. Armstrong. His own communication would contain much that was old, and something that was new. There was a difficulty in the definition of an electrolyte. Some people say an electrolyte is a salt. Some say it is a binary compound. But what is a binary compound? It is something which can be decomposed into two parts. But water-free hydrochloric acid does not conduct. Nevertheless it can be decomposed into two parts. Whether the water plays a part in decomposition is still an open question, although Kohlrausch thinks he has shown that in very dilute solutions the water does take part. The resistance of an electrolyte is measured by the work done in the wandering of the ions. It had been said that his view was that the viscosity is proportional to the resistance. This is not quite correctly stated. There are to be considered (1) friction of the ions in the liquid, (2) friction of the salt in the liquid, (3) friction of the whole liquid on the walls of the vessel. (3) may be avoided, and therefore we can omit it. The main thing considered has been the friction of the ions in the liquid. Kohlrausch has lately taken very dilute solutions, and can only find the friction of the salts and not that of the ions present here, which agrees with his theory. A difficulty in this connexion is, that in very dilute solutions the impurities of the water conduct better than the salt. There may be double decompositions between those impurities and the salt. Further we know that many salts decompose in water, *e.g.* magnesium salts. Again HCl separates from a solution of ammonium chloride, and here we have acid and base separate in the solution. He did not know how to avoid this difficulty, and must content himself with pointing out the existence of it. Then another question is, What is it that is decomposed in the decomposition of salts? Is the process a simple molecular decomposition? We may ask further how hydrides are decomposed. But it is generally assumed that in the liquid it is only the salt which is decomposed. His son had sent a paper on solutions of chloride of copper. He observed that there is a change of colour in very dilute solutions; and we may be sure that in those solutions the salt has combined with the water. We cannot say whether a salt in solution is alone electrolyzed, or the salt in combination with the water. A relation between conductivity and chemical constitution had recently been obtained by Mr. Hartwig in his laboratory. He found that with rising concentration the conductivity of solutions of acids attains a maximum earlier the more carbon they contain. In regard to the friction of the salt in the liquid there is no doubt that the undecomposed salt in the liquid has a certain influence, and work must be done to produce motion of the salt in the liquid.

Prof. Quincke said that he agreed with the views of Prof. Wiedemann as to the influence of secondary decompositions in the liquid. It was difficult to distinguish between secondary and primary decompositions.

Prof. Fitzgerald, F.R.S., read a paper by Mr. F. Trouton and himself *On the Accuracy of Ohm's Law in Electrolysis.*—To avoid the difficulties due to heating of the liquid by the current the method which Chrystal and Maxwell used for solid conductors was employed, but the alternation had to be more rapid. The amount of accuracy obtained was approximately 1/2000 per cent.; and up to this Ohm's law was verified.

On the general question of electrolysis Prof. Fitzgerald said that the usual reasoning was that if the *atoms* are to be dragged asunder this requires finite E.M.F. On the whole, however, no work is done; and therefore, he contended, whatever theory may be adopted, it cannot require a finite E.M.F. to detach atoms. He believed that by this the whole Williamson-Clausius hypothesis was swept away. There were no separate atoms in the liquid. If in the case of HCl there were separate atoms of hydrogen in the liquid, surely some of them would escape from the surface of the liquid.

Prof. S. P. Thompson read a communication from Prof. von Helmholtz on *Further Researches concerning the Electrolysis of Water.*—Prof. Helmholtz has been working at the question, whether, when you electrolyze water at different pressures, it needs different electromotive forces. He found that in water which was originally free from gases the smallest E.M.F. will send a current through. He likens the difficulty which there is in getting gas to develop in an electrolytic cell originally quite free from gas to the difficulty which is experienced in getting a perfectly clean liquid to boil.

His apparatus consists of a U-tube, bent over at one end, and there blown into two bulbs, which contain the electrolyte. The electrodes are fused into the glass. One limb of the U-tube is open. From the other, which is in connexion with the bulbs containing the electrolyte, there comes off a side tube through which mercury poured into the open limb can escape, and so exhaust the space over the electrolyte to any required extent. An air-bubble is left in the large bulb above the electrolyte.

In another apparatus there was no air, and the mixed gases collected. With 1·79 volts at atmospheric pressure a balance was obtained, and the mixed gases did not increase. He finally fixes upon the superior limit of the E.M.F. with atmospheric pressure at 1·775 volt.

Experiments on the possible Electrolytic Decomposition of Alloys, by Prof. Roberts-Austen, F.R.S.—Experiments were made on gold-lead and silver-lead alloys. The results are absolutely negative. No electrolytic action whatever could be found, although cupellation would certainly have detected a variation of 1/100 per cent. in the composition.

Dr. Gladstone and Prof. Wiedemann were able to confirm the result from experiments performed by other methods in their own laboratories.

Sir W. Thomson said it was a most important discovery.

Experiments on the Speeds of Ions, by Prof. Lodge, F.R.S.—These experiments are still going on. The object is to determine directly the speeds of the ions in a liquid. The current is sent through a tube of liquid which contains some detecting substance.

At first something to give a precipitate was used, the advance of which could be timed. But this has the disadvantage of removing the substance from the tube, because the current does not affect it when it becomes solid. Now he uses fluid detectors—such as some of the aniline bodies—to detect the advance of acidity or alkalinity. Thus, for example, he may have the tube filled with solution of sodium chloride, with a trace of caustic soda, and a body which is coloured in alkaline solution, but which loses its colour when the alkalinity disappears. If now, in the course of the electrolysis, ions from the substance being electrolyzed which will unite with the Na of the caustic soda travel along the tube, they will cause the alkalinity to disappear, and the rate at which this change travels can be measured.

The composition of the liquid, however, does not remain constant, and therefore we get a broken slope of potential in the tube, because the bad-conducting alkali is turned into the good-conducting acid. This difficulty is got over by making the principal ingredient in the measuring tube the same as the product of the action for any given case. A small addition to its amount is therefore of no consequence.

The theory of Kohlrausch with regard to the speed of ions was shown to be in accordance with the results.

On Chemical Action in a Magnetic Field, by Prof. H. A. Rowland.—It had been observed by his colleague, Prof. Remsen, that if a thin plate of iron be placed between the poles of an electro-magnet and then acted on by $CuSO_4$, the copper was deposited in lines very similar to the equipotential lines. Around each pole was a clear space where the iron was not acted on at all. This part of the field is of course the part where the rate of variation of the square of the magnetic field is greatest; and it occurred to Prof. Rowland that the want of action of the sulphate of copper in this position was due to the attraction of the magnet on the iron. With the help of Mr. L. Bell he had carried out experiments on the point.

Between the poles of a powerful electro-magnet was placed a glass beaker containing the liquid whose action upon iron it was desired to test. Nitric acid generally acted very well; so did sulphate of copper, and almost any salt which would deposit metal on iron. In the liquid were immersed two pieces of iron, one of which was pointed. The greater part of each piece was covered with wax, and what was exposed to the liquid was a point in the one case and a plane surface in the other. They were connected through a galvanometer, and a current was obtained which was not reversed on reversing the direction of the current of the electro-magnet. This indicated that the point was protected from the action of the liquid.

On the Electro-deposition of Alloys, by Prof. S. P. Thompson.—In a mixture of metals which is electrolyzed, the most negative metal comes down first. Prof. Thompson made a series of experiments on solutions of zinc and copper in cyanide of potash solution of different strengths. The electromotive force was measured for each strength of the cyanide of potash solution. The curves representing the E.M.F. for copper and zinc were found to cut at a certain strength of the KCN solution. Beyond this strength copper became positive to zinc. In the ordinary brassing solution he found that it depended on the temperature whether zinc was positive to copper or copper to zinc.

On the Action of the Solvent in Electrolytic Conduction, by T. C. Fitzpatrick. This paper was communicated by Mr. W. N. Shaw.—Mr. Fitzpatrick found that although methyl alcohol has greater conductivity than water, yet a solution of calcium chloride in the former liquid is a worse conductor than an aqueous solution. He found similar results for calcium nitrate, lithium chloride, and lithium nitrate solutions. Solutions in ethylic alcohol were also used. He was much impressed with the idea that electrolysis is the electrolysis of molecular aggregates.

The next paper was by Prof. S. P. Thompson, on the *Industrial Electro-deposition of Platinum*. He exhibited specimens illustrating a new process.

The Princeton Eclipse Expedition, by Prof. C. A. Young.—The expedition had its origin in his desire to repeat observations made by him seventeen years ago, as objection had been taken, not to the accuracy of the observations themselves, but to the conclusions he had drawn from them. Prof. Libby was to do the photography. Photographs of the corona were to be taken; and they were anxious to determine whether there are true dark lines in the corona or not. The place selected for the observations lay 150 or 160 miles to the north-west of Moscow. It is needless to say that on the morning of the eclipse it rained, and hardly anything could be done. They made an attempt to determine the end of totality by the amount of light. The diminution of the light was gradual, but after totality there was a sudden burst.

Prof. L. Weber, of Breslau, described photo-metric measurements made during the eclipse at Breslau; and then read a paper on *Observations of Atmospheric Electricity*. Prof. Weber said that the increase of potential seemed to be a linear function of the height; but the presence of dust in the air disturbed this relation. The earth represents a surface of equipotential, and the other surfaces of equipotential are parallel, but come closer together above the mountain-tops.

Prof. Schuster said that, granting that the earth has a given potential at any moment, the convection-currents in the air would tend to reduce this, or to equalize the potential within the earth itself.

Prof. Everett remarked that wherever electricity is carried down by raindrops, an inequality of potential will be caused; and evaporation would also cause inequalities.

Prof. Rowland said that observations had been made during the last four years at his laboratory by the U.S. Signal Service. He did not see how the raindrops could disturb the distribution of potential much. If the earth is electrified most of the electricity would be on the outside of the atmosphere. He therefore looks for some other theory, and has given one in the *Phil. Mag.*, viz. that the earth would naturally be uniformly electrified if it were not for currents of air in the upper atmosphere, which will carry the electricity of the atmosphere towards the poles, making auroras there. At the equator, therefore, a space must be left which has to be filled up with electricity, and this takes place by thunderstorms. Accordingly there is a circulation of electricity. In this connexion it is to be remembered that thunderstorms are most common about the equator.

The Hygrometry of Ben Nevis, by Mr. H. N. Dickson.—This paper gives an account of observations which were undertaken for the purpose of testing the applicability at high-level stations of existing tables and formulæ for calculating the dew-point and humidity from the readings of wet- and dry-bulb thermometers. The construction of the direct hygrometer used, that of Prof. Chrystal, is described, and the action of the wet and dry bulbs under different meteorological conditions is examined in considerable detail; the results showing that for investigations of this kind a great range of humidity is necessary, the indications of the wet and dry bulbs being very uncertain when the difference between them is small.

The reduction of the observations is performed, in the first place, by a graphic method, from which the following expression is reduced: $f' - f'' = (t - t')k$, f' being the vapour-pressure at the temperature t' of the wet bulb, f'' that at the temperature of the dew-point, and t the air temperature. The truth of the above equation being assumed, the values of the quantity k are next found by direct calculation from the observations. A sudden large change takes place in its value at the freezing-point, and a similar, though much smaller, discontinuity is shown to occur when the wet bulb stands between 39° and 40°.

The Different Varieties of Thunderstorms, and a Scheme for their Systematic Observation in Great Britain, by the Hon. R. Abercromby.—The writer said that there were three well-defined types of thunderstorms in this country: (1) squall thunderstorms, *i.e.* simply a squall associated with thunder and lightning; (2) a very common form which occurs in secondary cyclones: the nature of this class needed investigation; (3) far the most curious class was that which might be called line-thunderstorms, because their shape was a long narrow belt sometimes 200 or 300 miles long and only 4 or 5 broad. They move broadside on, and are usually preceded by a squall of extreme violence. He explained a scheme for the future systematic study of thunderstorms, and invited the co-operation of volunteer observers.

Sir W. Thomson said that the natural history of thunderstorms was less known than any other part of meteorology, and that Mr. Abercromby's scheme would be likely to give much information on the subject.

On the Magnetization of Hadfield's Manganese Steel in Strong Fields, by Prof. J. A. Ewing, F.R.S., and William Low.—Messrs. Hadfield, of Sheffield, manufacture a steel containing about 12 per cent. of manganese and 0·8 per cent. of carbon, which possesses many remarkable qualities. Prominent amongst these, as the experiments of Hopkinson, Bottomley, and Barrett have shown, is a singular absence of magnetic susceptibility. Hopkinson, by applying a magnetic force, $\mathfrak{H}$, of 244 C.G.S. units to a specimen of this metal produced a magnetic induction, $\mathfrak{B}$, of only 310 C.G.S. units; in other words, the permeability μ was 1·27, and the intensity of magnetization $\mathfrak{I}$ was a little over 5 units.

The experiments made it clear that even under magnetic forces extending to 10,000 C.G.S. units the resistance which this manganese steel offers to being magnetized suffers no break-down in any way comparable to that which occurs in wrought iron, cast iron, or ordinary steel at a very early stage in the magnetizing process. On the contrary, the permeability is approximately constant under large and small forces.

The conclusion has some practical interest. It has been suggested that this steel should be used for the bed-plates of dynamos and in other situations where a metal is wanted that will not divert the lines of induction from neighbouring spaces. In such cases the magnetic forces to which manganese steel would be subjected would certainly lie below the limit to which the force has been raised in these experiments. We may therefore conclude that in these uses of the material it may be counted upon to exhibit a magnetic permeability only fractionally greater than that of copper, or brass, or air.

On the Influence of a Plane of Transverse Section on the Magnetic Permeability of an Iron Bar, by Prof. J. A. Ewing, F.R.S., and William Low.—It has been remarked by Prof. J. J. Thomson and Mr. H. F. Newall that when an iron bar is cut across and the cut ends are brought into contact, the magnetic permeability is notably reduced (Cambridge Phil. Soc. Proc., February 1887). The attention of the authors was directed to the matter by finding the same phenomenon present itself in experiments on the magnetization by the "isthmus" method; and they proceeded to examine the effect by an application of the method Hopkinson has used to measure magnetic permeability ("Magnetization of Iron," Phil. Trans., Part 2, 1885). A round bar, nearly half a square centimetre in section, and 13 centimetres long, had its ends united by a massive wrought-iron yoke to reduce it to a condition approximating to endlessness, and its magnetization by various magnetic forces was examined, both when free from stress and when compressed by a load of 226 kilos. per square centimetre. It was then cut in the lathe and the halves placed in contact, and the magnetization again examined with and without load. It was next cut into four parts, and finally into eight parts, and magnetized in each case. Every new plane of section caused a notable loss of permeability. The following are the maximum values of the permeability in each case:—

Solid bar	1220
Bar cut in two ...	980
Bar cut in four ...	640
Bar cut in eight ...	400

Next another bar was tested, first when solid, next with one cut finished in the lathe, and finally with the cut surfaces faced true by scraping and comparing them with a Whitworth plane. So long as the bar was not compressed its magnetic permeability was nearly the same, whether the ends were left roughly finished or were faced true. But when load was applied the effect of facing the ends was remarkable: the faced bar then behaved as a solid bar would, while the bar with rough cut ends still showed a decided defect of permeability as compared with a solid bar.

This made it seem highly probable that the whole effect was due to a film of air between the cut faces. Applying Hopkinson's method to calculate the thickness this film would need to have in order to account for the observed increase of magnetic resistance, the authors find its thickness is only about 1/35 of a millimetre when the magnetic force is 10 C.G.S. units, and diminishes to about 1/70 of a millimetre when the force is 50 C.G.S. In the case of the bar cut into four or eight parts, each cut has an effect equivalent to the introduction of a film of this thickness. The authors conclude that in all probability the whole phenomenon is due to the surfaces being separated by these short distances.

On the Magnetic Properties of Gases, by Prof. Quincke, Ph.D. —A few years ago he invented what he called a magnetic manometer. It consists of a bent tube, of which one limb is much wider than the other. In the wide limb is the gas to be experimented on. The narrow limb and the connecting horizontal piece contain liquid, and the difference of level of the liquid in the narrow limb produced by the magnetic field is what is measured.

The magnetic pressure per unit of area is given by the formula—

$$p = \frac{R}{8\pi} H_1^2.$$

If h be the difference of level of the liquid in the two limbs, *i.e.* the hydrostatic pressure, we have—

$$h\sigma = \frac{R - R_1}{8\pi} H_1^2.$$

The smallest diamagnetic constant for the gases experimented on was found to be that of hydrogen. Oxygen had the highest. He compares his results with Faraday's, and finds that they agree substantially, the differences being probably due to impurities.

Final Value of the B.A. Unit of Electrical Resistance as determined by the American Committee, by Prof. H. A. Rowland.—His determination in 1876 gave 1 B.A. unit = ·9878 ohm. For his present determination the apparatus was on a very large scale. He employed both the Kirchoff and the Lorenz method. By the former method he got a final value of ·98646 ± 40, by the latter a value of ·9864 ± 18; so that the latter method has a probable error of less than a half that of the former. His value for the resistance of 100 cubic centimetres of mercury came out ·95349 B.A. units.

Lord Rayleigh said that the results showed that the absolute determination of the B.A. unit by various experiments agreed much better than the comparison with the mercury standard. This was exactly the opposite of what he would have expected. Prof. Rowland had suggested that one cause of the difference between their determinations of the mercury standard might be that in the American experiments the tubes had been mechanically wiped, so that there was no chance of dust remaining in them. He hardly thought, however, that this was likely. The want of uniformity in the diameter of the tube might possibly have an effect.

On Induction between Wires and Wires, by W. H. Preece, F.R.S.—A continuation of a subject brought before the Association last year, when it was shown that electro-magnetic disturbances extended to distances much greater than was imagined, and that effects were observed across many miles of country. Experiments were made on the banks of the Severn and Mersey, on the Portcawl Sands of South Wales, in the fields in the neighbourhood of Cardiff, on the roads and railways of Oxfordshire, Worcestershire, and Shropshire, in the air and under water, in the corridor of the General Post Office in London; and the law was formulated that the distance depended directly on the strength of the currents inducing the disturbance and on the length of the wires opposed to each other, and inversely on the square of the distance separating them, and on the electrical resistance of the disturbed wire.

The influence of 1 mile of wire carrying 1 ampere of current can apparently extend to a distance of 1·9 mile. The law is given by the following formula:—

$$C_2 = M\frac{C_1 l}{d^2 r_2},$$

where C_1 is the primary current, C_2 the secondary, l the length of the wires opposed to each other, d the distance separating them, r_2 the resistance of the secondary circuit. When these quantities are represented in C.G.S. units, M equals ·005. The current induced by 1 mile, of 1 ampere at 1 mile distant is $1·3 \times 10^{-13}$ ampere. A current is still perceptible at 1·9 mile distant; hence we can calculate that a Bell telephone requires six ten-thousand-millionths of a milliampere, or, in figures, ·0000000006 milliampere, to be audible.

One curious result of these inquiries is that the disturbances are transmitted equally well through water and the earth as through air, and hence our cables are disturbed as well as our land wires. Communication with coal-pits is possible, though nothing but the earth intervenes.

On the Effect of Continental Lands in altering the Level of the adjoining Oceans, by Prof. Edward Hull, F.R.S., Director of the Geological Survey of Ireland.—The effect of the attraction of continental lands upon the oceanic waters adjoining seems to have been very much overlooked by British physical geographers. That some slight effect arises in the direction of elevating the surface of the ocean in proximity to the coast is generally admitted, but the amount of rise is considered to be small, perhaps insignificant. The prevalence of these views was attributed by the author to the widespread influence of Lyell's hypothesis of the uniformity of the ocean-surface all over the globe.

The author proceeded to discuss the effect of continental lands, showing that this was in the first instance divisible under two principal heads: The effect (1) of the unsubmerged, and (2) of the submerged masses. In the former case, where the mass rose above the surface, one component of the attraction acted in a more or less vertical direction; in the second case, all in a lateral direction; but both had the effect of elevating the surface of the ocean. The horizontal distance to which the vertical effect extended owing to the curvature of the earth's surface was then considered: and it was shown that, where continental lands rise from a deep ocean, the effect of the lateral attraction far exceeds that of the vertical attraction of the unsubmerged mass. Prof. Stokes has furnished the author with a hypothetical case, in which the elevation of the ocean was estimated to reach 400 feet above the mean geodetic surface of the earth.

For the purposes of illustration three cases were selected. viz.:—

(1) The table-land of Mexico, between lats. 18° and 26° N.
(2) The table-land of Bolivia, ,, 19° and 26° S.
(3) The Andes of Chili, ,, 26° and 35° S.

The mean elevations, distances from the ocean, and extent having been determined, and the mean density of the crust being taken at 2·6 for emergent, and 1·6 for unsubmerged land, the results of the attraction of the mountain masses in each case were as follows:—

(1) Mexico, 780 feet; (2) Bolivia, 2160 feet; (3) Chili, 1580 feet.

The total calculated rise of the ocean-waters at a distance of 900 miles from the coast in lat. 10° S. would amount to 2568 feet.

The above results, which are probably rather under than over estimates, fall considerably short of those to be drawn from Suess and Fischer's formula, but are probably much in excess of the views held by British physical geographers generally; and the conclusion was drawn that if the same processes of reasoning and calculation were applied to all parts of the world, it would be found that the ocean waters were piled up to a greater or less extent all along our continental coasts, producing very important alterations in the terrestrial configuration as compared with an imaginary ellipsoidal, or geodetic, surface, to which all these changes of level must necessarily be referred.

On a Standard Lamp, by Prof. A. A. Vernon Harcourt, F.R.S.—At one of the meetings of this Section last year a lamp devised by the author for producing a constant amount of light was shown and described by Mr. W. S. Rawson. The lamp now exhibited serves the same purpose, but is simpler in principle, more easily adjusted, and less affected by draughts. It consists of a glass reservoir with tubulure and stopper of the form and size of a large spirit-lamp, mounted on a metal stand provided with levelling screws. The wick can be turned up and down in the normal manner within a long tube attached to the body of the lamp. Round this tube is a wider tube 100 × 25 mm., and the two being joined together above and below by flat plates constitute the burner of the lamp. When the burner becomes warm by conduction of heat from the flame of the lamp, the pentane in the wick volatilizes and burns at a considerable distance above the point to which the wick is turned down. Thus the size, or texture, or quality of the wick does not affect the flame. Around the burner and the lower part of the flame is another cylinder open at both ends and contracted above the burner to a tube 21 mm. in diameter. A similar tube forms the lower part of an upper chimney, which is enlarged above to a diameter of 25 mm. The upper part of the flame is concealed by this chimney, excepting where a narrow slit, 10 × 3 mm., on each side shows the tip of the flame and enables its height to be regulated. Through the interval between the two chimneys the flame shines, and the light which it gives is the same whenever the tip of the flame is visible through the slit, whether towards the lower or the upper end. The two chimneys are attached together by two curved metal bands sufficiently removed from the flame on either side not to affect it. The attachment of the bands to the lower chimney are adjustable, so that the opening through which the central parts of the flame are seen may be made larger or smaller. By means of small cylindrical blocks, whose thickness is accurately gauged, the width of the opening may be set either to that at which the light emitted is one candle, or, if a greater or smaller light is desired, a candle and a half or half a candle. The liquid with which the lamp is fed is pentane, obtained in a manner already described from American petroleum.

Mr. W. N. Shaw read a paper by Mr. J. T. Bottomley on *Expansion by Heat of Wires under Pulling Stress.* The wires were two fine copper wires. One of them carried about half its breaking weight and the other about a tenth of its breaking weight. The wires were suspended in a tube, a scale being attached to one, and a pointer moving over the scale to the other. Thermometers were inserted into the tube at various points, and the wires were heated by passing steam into the tube. It was found that the more heavily weighted wire extended much more than the lightly-weighted one. An amount of permanent elongation remained, but more in the heavily-strained wire. Each time the heating was done there was more and more permanent elongation, and ultimately one of the wires was broken under less than its breaking load in the normal state. Further experiments were made with wires which had been hardened, and the final result is that the coefficient of expansibility for heat of copper wire strained by a certain weight is greater than that of similar wire less heavily weighted.

Experiments on Electrolysis and Electrolytic Polarization, by W. W. Haldane Gee, Henry Holden, and Charles H. Lees.—This is a preliminary notice of experiments that are in progress in the Owens College Physical Laboratory. The experiments fall under four heads: (*A*) electrolysis under pressure; (*B*) time-rate of fall of polarization in closed circuit; (*C*) irreciprocal conduction; (*D*) the production of an oily fluid in electrolysis with palladium electrodes.

A. Numerous experiments have been made in order to determine the variation of the resistance of polarization of a sealed voltameter in which dilute sulphuric acid was electrolyzed between platinum wire electrodes, it being thus subjected to the pressure (up to 200 atmospheres) of the evolved gases. It was found that the resistance markedly decreased, and the polarization decreased slightly. These changes may, however, it is thought, be due to change of temperature, the influence of which would appear from later experiments not to have been fully eliminated. In two cases no change whatever was perceived: (1) when two platinum *plates* were used as electrodes, and (2) when two voltameters were connected together forming a sealed vessel, one voltameter being used to increase the pressure, while observations were made on the other. As it has not been possible to obtain glass tubes sufficiently strong for the high pressures desired, an apparatus of gun-metal has been constructed. This apparatus, which is fitted with a Bourdon's gauge recording to six tons on the square inch, may also be arranged for pressure experiments in general by attaching to it, by means of a strong metal tube, a suitable receiver. In two of the experiments, when the pressure had reached between 200 and 300 atmospheres, the evolved oxygen and hydrogen gases combined with explosion, although precautions had been used to prevent the gases from coming into contact with the platinum, except in the liquid.

B. The object of this research was to try to learn the parts played by the various portions of the evolved gases: (1) that occluded by the electrodes; (2) that deposited on them; and (3) that contained in the liquid in influencing polarization. The method employed was to vary the conditions under control, *e.g.* time of changing, density of current, &c., and to observe the time-rate of the fall of the polarization thus produced in closed circuit. It was found to be very difficult to apply this method, because though the conditions under control were kept as constant as possible, yet the time-rates of fall in two successive observations were often different. This was thought to be due to the insufficient cleaning of the electrodes between each experiment, and various methods were tried to remedy it, with the general result that the more perfect the cleaning became the more regular did the curves giving the time-rate of fall of the polarization become, but still the inconsistencies were not wholly removed. Heating the electrodes by the electrical current seemed preferable to the other methods of heating.

C. Whilst electrolyzing strong sulphuric acid between platinum electrodes, it was noticed that when the current density at the anode had exceeded a certain value decomposition apparently ceased. The value of the anode current-density necessary to produce this phenomenon is increased by diminishing the concentration or increasing the temperature of the acid (thus diminishing the viscosity), and is diminished by cleaning the electrodes. It was found that this great diminution of the current was not caused by the formation of an opposing E.M.F., but by a sudden increase of from 500 to 50,000 ohms in the resistance of the circuit. That the insulating condition occurs at the anode is shown by successively replacing the kathode and the anode by clean plates; in the first case the stoppage of the current persists, in the second case the current is readily conducted. The cause seems to be a sheath of oxygen bubbles which firmly adhere to the anode when the insulating condition is formed. The film is removed by momentarily breaking the circuit, or short-circuiting the voltameter, or reversing the current, or by replacing the anode by a clean plate.

D. During the electrolysis of various liquids between palladium electrodes it has been observed that a dense-looking liquid streams from *one* of the electrodes (the anode in dilute sulphuric acid, the kathode in caustic soda) *after a reversal of the current.* The liquid seems to be a compound of oxygen and hydrogen, presumably hydroxyl.

On the Vortex-Theory of the Luminiferous Ether, by Prof. Sir W. Thomson, F.R.S.—"In endeavouring to investigate turbulent motion of water for my communication on that subject to this Section, I have found a solution (many times tried for within the last twenty years) of the problem—to construct, by giving vortex-motion to an incompressible viscid fluid, a medium which shall transmit waves of laminar motion as the luminiferous ether transmits waves of light. Let *xav*, *xzav*, *xyzav* denote space-averages, linear, surface, and solid, through infinitely great spaces." After defining and illustrating this method of averages by examples, and remarking in passing that a general property of it is that

$$xav \frac{dQ}{dx} = 0,$$

where Q is any quantity which is finite for infinitely great values of x, he proceeded thus:—

Suppose now the motion to be homogeneously distributed through all space. This implies that the centres of inertia of all great volumes of the fluid have equal parallel motions, if any motions at all. Conveniently, therefore, we take our reference-lines, OX, OY, OZ, as fixed relatively to the centres of inertia of three (and therefore of all) centres of inertia of large volumes; in other words, we assume no translatory motion of the fluid as a whole. This makes zero of every large average of u, and of v, and of w; u, v, and w being the velocity-components; and we may write as the general expression for nullity of translational movements in large volumes—

$$0 = \text{ave. } u = \text{ave. } v = \text{ave. } w,$$

where ave. denotes the average through any great length of straight or curved line, or area of plane or curved surface, or through any great volume of space. In terms of this generalized notation of averages, homogeneousness implies—

$$\text{ave. } u^2 = U^2, \text{ ave. } v^2 = V^2, \text{ ave. } w^2 = W^2,$$
$$\text{ave. } vw = A^2, \text{ ave. } wu = B^2, \text{ ave. } uv = C^2,$$

where U, V, W, A, B, C are six velocities independent of the positions of the spaces in which the averages are taken. These equations are, however, infinitely short of implying, though implied by, homogeneousness.

Suppose now the distribution of motion to be isotropic. This implies, but is infinitely more than is implied by, the following equations in terms of the above notation, with further notation, R, to denote what we shall call *the average velocity* of the turbulent motion—

$$U^2 = V^2 = W^2 \tfrac{1}{3}R^2,$$
$$0 = A = B = C.$$

Large questions now present themselves as to transformations which a distribution of turbulent motion would experience in an infinite liquid left to itself with any distribution given to it initially. If the initial distribution be homogeneous through all large volumes of space, except a certain large finite space, S, through which there is initially either no motion or turbulent motion, homogeneous or not, but not homogeneous with the motion through the surrounding space, will the fluid which at any time is within S acquire more and more nearly as time advances the same homogeneous distribution of motion as that of the surrounding space, till ultimately the motion is homogeneous throughout? Probably, I think I may say certainly, yes—at all events for a large class of cases.

But can it be that this equalization comes to pass through smaller and smaller spaces as time advances? In other words, will any given distribution, homogeneous on a large enough scale, become more and more *fine-grained* as time advances? Probably *yes* for some initial distributions; probably *no* for others. Probably *yes*, for vortex-motion given continuously through all of one large portion of the fluid while all the rest is irrotational. Probably *no* for the initial motion given in the shape of equal and similar Helmholtz rings, of proportions suitable for stability, and each of overall diameter considerably smaller than the average distance from nearest neighbour. Probably also *no*, though the rings be of very different volumes and vorticities. But probably *yes* if the diameters of the rings or of many of them, be not small in comparison with distances from neighbours, or if the individual rings, each an endless slender filament, be entangled or nearly entangled among one another.

Again a question: If the initial distribution be *homogeneous and æolotropic*, will it become more and more isotropic as time advances, and *ultimately quite isotropic?* Probably *yes* for any random initial distribution, whether of continuous rotationally-moving fluid or of separate finite vortex-rings. Possibly *no* for some symmetrical initial distribution of vortex-rings, conceivably stable; though it does not seem probable that there is any such stability.

If the initial distribution be homogeneous and isotropic (and therefore utterly *random* in respect to direction) will it remain so? Certainly *yes*.

We shall now suppose the initial motion to consist of a laminar motion $[f(y), 0, 0]$ superimposed on a homogeneous and isotropic distribution $(\mathfrak{u}_0, v_0, w_0)$; so that we have—

$$\text{when } t = 0,\ u = f(y) + \mathfrak{u}_0,\ v = v_0,\ w = w_0;$$

and we shall endeavour to find such a function, $f(y, t)$, that at any time, t, the velocity-components shall be—

$$f(y, t) + \mathfrak{u},\ v,\ w,$$

where $\mathfrak{u}$, v, w are quantities of each of which every large enough average is zero.

With this assumption the equations of motion yield the following—

$$\frac{df(y, t)}{dt} = -\ xzav\ \frac{d(\mathfrak{u}v)}{dy}.$$

It is to be remarked that this result involves no isotropy, no homogeneousness in respect to y; and only homogeneousness of *régime* with respect to y and z, with no translational motion.

The translational component of the motion is wholly represented by $f(y, t)$, and, so far as our establishment of the above equation is concerned, may be of any magnitude, great or small relatively to velocity-components of the turbulent motion. It is a fundamental formula in the theory of the turbulent motion of water between two planes; and I had found it in endeavouring to treat mathematically my brother Prof. James Thomson's theory of the "Flow of Water in Uniform *Régime* in Rivers and other Open Channels" (Proceedings of the Royal Society, August 15, 1878). In endeavouring to advance a step towards the law of distribution of the laminar motion at different depths, I was surprised to discover the law of propagation as of distortional waves in an elastic solid, which constitutes the conclusion of my present communication—

$$\frac{d}{dt}\ xzav\ (\mathfrak{u}v = -\ \tfrac{2}{9}R^2\frac{df(y, t)}{dy}.$$

Eliminating the first member from this equation, by the former, we find—

$$\frac{d^2f}{dt^2} = \tfrac{2}{9}R^2\frac{d^2f}{dt^2}.$$

Thus we have the very remarkable result that laminar disturbance is propagated according to the well-known mode of waves of distortion in a homogeneous elastic solid; and that the velocity of propagation is $\frac{\sqrt{2}}{3}$ R, or about ·47 of the average velocity of the turbulent motion of the fluid. This might seem to go far towards giving probability to the vortex-theory of the luminiferous ether.

But a difficulty remains unsolved: a possible rearrangement of vortices within each wave, giving rise to dissipation of the wave-energy.

The mathematical investigation appears in full in the October number of the *Philosopical Magazine*, with some slight farther considerations regarding this virtual viscosity, and the question of what, if any, distribution of vortices can either have no tendency to the vitiating rearrangement, or can, with the requisite fine-grainedness, be slow enough in the vitiating rearrangement to allow the propagation of waves of light to go on through a hundred million million miles of space, or a million times the earth's distance from the sun.

The Committee of the Section reported that at a meeting of the Committee it had been resolved, on the motion of Prof. Gustav Wiedemann, of Leipzig, seconded by Sir William Thomson:—"That this Committee of the Mathematical and Physical Science Section of the British Association hereby convey to Dr. Joule their sense of the great loss sustained by the Section in consequence of his inability to take part in this meeting of the British Association in his native city, and express their sincere regret at the cause of this loss, and their hearty sympathy with him in his illness. The Committee take this opportunity of recording their appreciation of the splendid work of this most painstaking and conscientious seeker after truth, who, with his discoveries, has led the way in the greatest advance in knowledge made in this age, and, by his life, has conferred on mankind a precious example for their admiration and imitation."

SCIENTIFIC SERIALS.

American Journal of Science, August.—History of the changes in the Mount Loa craters (continued), by James D. Dana. In this paper the history of Kilauea is continued from January 1840 to the end of 1886, during which period sufficient facts were accumulated for a widened and apparently final explanation of the method of filling the pit. The eruptions of 1849, 1855, 1868, and 1886 are fully described, and the whole subject is illustrated with maps of the burning mountain at various dates during the period under consideration.—On some phenomena of binocular vision (continued), by Joseph Le Conte. In this paper, the twelfth of the series, the author deals with certain peculiarities of the phantom images formed by binocular combination of regular figures. The phenomena here described, none of which have hitherto been satisfactorily accounted for, are all explained by the law of corresponding points, justly regarded as the most fundamental law of binocular vision.—Chemical integration, by T. Sterry Hunt. In this paper the author deals more fully with several points connected with chemical metamorphosis, which were more briefly noticed in his recently published work, entitled "A New Basis for Chemistry."—Studies in the mica group, by F. W. Clarke. In this paper the author deals with specimens of muscovite from Alexander County, North Carolina; of lepidomelane from Baltimore and Litchfield, Maine; of iron biotite from Auburn, Maine; and of iron mica from near Pike's Peak.

SOCIETIES AND ACADEMIES.

London.

Institution of Mechanical Engineers, September 30.—Mr. E. H. Carbutt, President, in the chair.—A supplementary paper by Major Thomas English, R.E., on the initial condensation in a steam cylinder, was read and discussed in connexion with the paper by the same author on the distribution of heat in a stationary steam-engine, read at the spring meeting on May 17, an abstract of which has already appeared in Nature (vol. xxxvi. p. 115). The supplementary experiments were carried out in a portable engine of ordinary type, the cylinder of which was jacketed on the cylindrical portion but not at the ends. The steam was admitted directly from the boiler into the steam chest, and the quantity required for each experiment being small compared with the capacity of the boiler, no question of priming or condensation before admission can arise. The connecting-rod was disconnected, and the piston was rigidly blocked at the end of the stroke furthest from the crank, the interior of the cylinder surrounding the piston-rod being entirely filled up with wood and iron packing. The steam passage between the valve seat and the end of the cylinder next the crank was also solidly filled up; and the port itself was closed by a brass plate scraped down to the level of the valve seat. The port admitting steam to the end of the cylinder furthest from the crank was left open; and the crank shaft, eccentric, and valve were driven by another engine. The steam pressure in the boiler was maintained at a uniform amount, and the regulator was kept open during a trial. The steam was measured by connecting the exhaust port with a surface condenser and collecting the resulting water. The results of the experiments appeared to indicate that the net initial condensation, or excess of condensation, over re-evaporation by the clearance surface varies directly as the initial density, and inversely as the square root of the number of revolutions per unit of time. The paper was discussed, and was followed by one on irrigating machinery on the Pacific coast, by Mr. John Richards, which dealt very fully with the forms of pumps required for the various services to be performed. The discussion of this paper was adjourned.

PARIS.

Academy of Sciences, September 26.—M. Hervé Mangon in the chair.—On the recent waterspout in Lake Geneva, by M. H. Faye. In reply to M. Ch. Dufour's letter stating that several persons had noticed an *ascending* gyratory movement in the waterspout that swept over Lake Geneva on August 19, the author points out that, although the movement is really descending, as he holds against most meteorologists, there is nothing remarkable in this apparent contradiction, which is due to a purely optical illusion on the part of the observers. In the same way the spirals of a vice or screw, placed vertically to a horizontal base, when turned in the reverse direction, seem to the spectator to ascend along the line of the main axis, presenting the appearance of continually retiring from the base upwards, and burying itself in the handle or top cross-piece. The cause of the illusion is simple enough. Each anterior semi-spiral is successively replaced, as the screw revolves, by the posterior half, which, being at a higher level, the visible half-spirals, taken separately and together, seem to ascend. So with waterspouts, which, as already repeatedly explained, never ascend, but always descend, being the result of forces having their existence in the upper atmospheric regions.—On the measurement of the forces brought into play in the flight of a bird, by M. Marey. Anatomy shows that nearly all the muscles acting on the wing serve to lower it, while the kinematic data drawn from photo-chronography show that during this lowering of the wing the mass of the bird is upheld against gravity and propelled forward against the resistance of the air, the result being flight. The author here studies these two elements of the motor power separately, whence may ultimately be deduced the sum total of the motor power.—Remarks accompanying the presentation of vol. xiii. of the "Mémorial du Dépôt de la Guerre," by General Perrier. This volume is occupied exclusively with the operations connected with the extension of the geodetic and astronomic lines from Spain to Algeria.—Observations of Brooks's comet (August 24), made at the Observatory of Algiers with the 0·50m. telescope, by MM. Trépied, Rambaud, and Sy. The observations extend over the period from September 10 to 16, and give the positions of six comparison stars of the eighth and ninth magnitudes.—Observations of the same comet at the Observatory of Lyons with the 0·18m. Brunner equatorial, by M. Le Cadet. The observations cover the period from September 13 to September 21.—Positions of Barnard's comet (of May 12, 1887) and of Palisa's new asteroid (September 21, 1887), measured at the Observatory of Besançon, by M. Gruey. The observations of the comet run from June 13 to July 23. Those of the asteroid were taken with the 8-inch equatorial on September 23.—On the relative distances of the planets in relation to the sun, and on the distances of the periodical comets, by M. Delauney. The planetary distances being represented by the formula $D = 86^{1 \cdot 0669^n}$, where n receives the successive values 1, 2, 3, 4, ... , the unity of distance is the semi-diameter of the sun, and if this unity be changed and the distance be taken, for instance, of the earth from the sun, the formula becomes $D = 0{\cdot}0032680 \times 86^{1 \cdot 0669^n}$. The calculation shows that with this same unity the mean distances of the six known periodical comets from the centre of the sun may be one presented by the analogous formula $D = 1{\cdot}8940 \times 1{\cdot}1511^{2^n}$. Further considerations show that there exists a gap in the series corresponding to $n = 1$, and that seven comets may be regarded as forming a single group analogous to the minor planets of the solar system. The distances increase so rapidly with n that for $n = 6$ we get 15,455, corresponding to a periodicity of nearly 2,000,000 years. Other considerations lead to the inference that the periodical comets appear to be produced by the cosmic matter of the zodiacal light.—Researches on the spheroidal state, by M. E. Gossart. The author here seeks to determine by calculation and experiment the meridional semi-section of any liquid drop whatsoever in a state of calefaction on a horizontal plaque. It is shown that there exists a characteristic form of the spheroidal state, which may easily be represented graphically according to a given scale. The measurements of the various elements of these curves may furnish useful information on the capillary constant.—On the distillation of citric acid with glycerine, MM. Ph. de Clermont and P. Chautard. The product of the process here described presents absolutely the same properties as the pyruvine obtained by distilling a mixture of tartaric acid with glycerine, although it seems difficult to explain how the same substance should result from the distillation, in the presence of glycerine, of an acid such as citric acid, which differs so greatly from tartaric acid.—On the development and structure of young Orobanches, by M. Maurice Hovelacque. Since M. Caspary's observations on the germination of the Orobanches (*O. cruenta*, *O. ramosa*, *O. minor*, *O. Hederæ*), dating from 1854, nothing was published on the subject till its study was resumed by Koch in 1883, the results being published in a comprehensive memoir recently issued by him. In the present communication M. Hovelacque indicates several important points where his own observations differ considerably from the conclusions of the learned German botanist.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Exercises in Quantitative Chemical Analysis, including Gas Analysis: W. Dittmar (Hodge).—Weather Charts and Storm Warnings, 3rd edition: R. H. Scott (Longmans).—Proceedings and Transactions of the Royal Society of Canada for 1886, vol. iv. (Dawson, Montreal).—Report of the Voyage of H.M.S. *Challenger*, vol. xxi. 2 Parts, Zoology.—An Elementary Treatise on Kinematics and Dynamics: J. G. Macgregor (Macmillan).—Key to Todhunter's Conic Sections: Edited by C. W. Bourne (Macmillan).—Handbuch der Palæontologie, 1 Abth. Palæozoologie, 3 Band, 1 Liefg. (Williams and Norgate).

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