THURSDAY, APRIL 23, 1891.

CATALOGUE OF FOSSIL FISHES.

Catalogue of the Fossil Fishes in the British Museum, Natural History. Part II. By Arthur Smith Woodward, F.G.S., F.Z.S. (London: Printed by order of the Trustees.)

IT is a matter for very warm congratulation to all British zoologists that such excellent work as Mr. Smith Woodward's "Catalogue of Fossil Fishes" should proceed from the great National Museum in Cromwell Road. When one remembers the dingy rooms, the inadequate staff, the poor library of the old Natural History Department at Bloomsbury, it seems scarcely credible that in so few years' time so great a change has been effected. In the new Museum there is ample space for the collections, working rooms for the curators, an increased staff of competent naturalists, and an admirable library with the latest memoirs from all parts of the world for their use. The increased opportunities for genuine scientific work now afforded to the younger members of the staff of the Museum, as well as the improved system of making appointments which, as well as much else which is good in the Natural History Museum, we owe to the Director, are bearing fruit in work like that done by Mr. Smith Woodward. His "Catalogue of Fossil Fishes" is not a mere enumeration of the specimens in the National Collection. Though the series of fossil fishes there deposited is by far the finest and most extensive in any Museum in the world—now that the collections of the late Earl of Enniskillen and of the late Sir Philip Egerton have been incorporated with the older possessions of the Museum—yet of many groups or of important genera the best specimens are as far away as St. Petersburg on the one side and Philadelphia on the other. Mr. Woodward has for some years spent his vacations in visiting (one hopes at the expense of the Trustees of the British Museum) the various Museums of Europe and America in which fossil fish-remains are preserved: he is therefore well acquainted, not only with the literature of his subject, but with all the existing material. Further than this, he shows in his treatment of the remains with which he has to deal a sound knowledge of anatomy, and that care and precision as to systematic arrangement and detail which are the most important qualifications of a museum curator. The "Catalogue of Fossil Fishes" in the British Museum thus becomes, in Mr. Smith Woodward's hands, a valuable treatise on ichthyology, indispensable to every comparative anatomist.

In the matter of the general classification of fishes, Mr. Smith Woodward follows neither the tradition of Huxley nor of Günther, but leans to Cope; at the same time, his two main lines of piscine pedigree, the Hyostylic and the Autostylic, are distinguished by a character first insisted upon by Huxley. He recognizes two sub-classes of Hyostylic fishes—the ELASMOBRANCHII, with three orders, Icthyotomi, Selachii, and Acanthodii; and the TELEOSTOMI, with two orders, the Crossopterygii and the Actinopterygii (these latter including all modern Ganoids and Teleostei). In the other main branch of descent, the Autostylic, Mr. Smith Woodward places also two sub-classes—the HOLOCEPHALI with only one known order, the Chimæroidei; and the DIPNOI, with two orders, viz. (1) Sirenoidei (including Dipterus, Phaneropleuron, and Lepidosiren), and (2) Arthrodira (including Coccosteus and Dinicthys). In addition to these four sub-classes, the author recognizes a fifth—the OSTRACODERMI, with the three orders Heterostraci, Osteostraci, and Antiarcha (why not —i?), typified respectively by the genera Pteraspis, Cephalaspis, and Ptericthys. There is no evidence to show what may have been the nature of the jaw-suspensor in the Ostracodermi—hence the sub-class cannot be referred to either hyostylic or autostylic branch.

There are several noteworthy features in this classification. The Palæicthyes of Günther are rejected, and, more startling still, we miss the familiar "Ganoids" of the palæontologist. Every anatomist must have been struck with the clear indications of a line of descent from primitive fishes through forms like Chimæra to the Dipnoi and so to the Amphibia. But one is not quite prepared for some of Mr. Smith Woodward's pedigree-making. He adopts the view that the Icthyotomi—those delightful extinct sharks with crossopterygial fins—are the most ancient forms of the hyostylic series, and is led to the conclusion that, in both the autostylic and the hyostylic series, the so-called archipterygium (crossopterygium, I should prefer to call it) is really archaic and primitive, and that the "actinopterygium" of Chimæra in the one series, and of modern Elasmobranchs, and, again, of modern Teleostomi in the other series, is a secondary departure, occurring thus at least at three separate and widely remote points in the pedigree. As a result of this mode of viewing the facts, we get Phaneropleuron together with Dipterus associated at the remote end of the autostylic branch with Ceratodus and Protopterus, whilst as far off as possible, at the remote end of the hyostylic branch, we find the "Crossopterygian Ganoids" Holoptychius and Osteolepis—the former of which was so closely associated by Huxley, as previously by the sands of Dura Den, with Phaneropleuron.

It is difficult to persuade oneself that the wide separation of these two old associates now proposed is a natural one. Without proposing another theoretical pedigree, we may point out two facts: (1) that it is somewhat easier to suppose that the change from autostylic to hyostylic suspensorium (or from an amphistylic neutral condition to either) has taken place more than once, than it is to suppose this of the change from crossopterygium to actinopterygium; (2) that the evidence adduced by Balfour and by Thatcher as to the primitive identity of construction of the lateral fins and the median fins of fishes is very strong, whereas the assumption of a primitive fish-ancestor with two pairs of crossopterygia has really not a leg to stand on, because the argument from palæontological succession is faulty, the Icthyotomi being really modern fish (Carboniferous), compared with the primitive fish of Silurian times. There is no sufficient ground for concluding that the actinopterygium has always developed from the crossopterygium rather than that the change has been in the other direction.

However these matters may be settled hereafter, it is probable that the fusion of the Ganoids with the Teleostei as one group (Teleostomi) containing older and newer grades of structural evolution, will be found acceptable by many systematists.

It would be impossible to follow Mr. Smith Woodward in these columns into the details of his work, but there are one or two points to which I may direct attention. The wide separation of Coccosteus from Ptericthys is a bold stroke, and one for which much solid argument is adduced, Coccosteus and its allies appearing to fit in with the Dipnoi, whilst Ptericthys finds allies in Cephalaspis, Pteraspis, and Didymaspis. Dr. Traquair's valuable researches are largely used and acknowledged by Mr. Smith Woodward, and he does not hesitate to accept the former author's association of the large family of the Palæoniscidæ with the Sturgeons, which, as the Chondrostei, form the first sub-order of the Actinopterygian Teleostomi, the point up to which this valuable work has now been brought.

Of my old friends Cephalaspis, Pteraspis, and their allies, Mr. Smith Woodward has a great deal which is extremely interesting to say. It appears that the conclusion (which I resisted in my monograph published by the Palæontographical Society, and elsewhere) is now pretty well demonstrated by new and well-preserved specimens (described by von Alth), viz. that the plates to which I gave the name Scaphaspis are really the ventral plates of the associated species of Pteraspis. Mr. Smith Woodward is able by means of this view to give a more correct interpretation of my specimen of the scales of Pteraspis than I was able to do. It is now in the collection of which this catalogue treats, and remains the only specimen of the body-covering of these fish. The possibility of this relationship of Scaphaspis and Pteraspis was often discussed and considered at the time when I wrote on these fossils, and I was careful to point out the constant association of *Sc. Lloydii* with *Pteraspis rostratus*, of *Sc. rectus* with *Pt. Crouchii*, and of *Sc. truncatus* with *Cyathaspis Banksii*. But no British specimens were forthcoming (although we had hundreds to examine) showing any connection of the two shields as parts of one animal. The remarkable genus to which I gave the name Holaspis (which, being preoccupied by the late Dr. Gray, has had to give place to Mr. Claypole's Palæaspis of ten years later date) seemed to confirm the view that Scaphaspis was a simplified shield of the same character as Pteraspis, Holaspis or Palæaspis being intermediate in complexity. But the specimen described and figured by von Alth leaves no doubt in my mind as to the interpretation of Scaphaspis adopted by him. The absence of the canal system, so well developed in Pteraspis and Palæaspis, is an argument against Scaphaspis being, like those forms, a dorsal cephalic plate.

There are matters with regard to Didymaspis and Eukeraspis (genera of mine which I am glad to see surviving Mr. Smith Woodward's crucible) and Cephalaspis on which new light is thrown in the "Catalogue," which, however, I must not take space to detail. A very interesting thing connected with our Herefordshire cornstones is the discovery, by Dr. Traquair, of a Coccostean, *Phlyctænaspis anglica*, in specimens which I had supposed might be portions of the large *Cephalaspis salweyi*. Quite a series of these specimens are recorded in the "Catalogue" as being in the national collection, whilst in Oxford we have some excellent specimens amongst the other treasures of the Grindrod Collection, which has been purchased for the University Museum.

Sixteen beautifully lithographed plates and numerous woodcuts illustrate this second volume of the "Catalogue of Fossil Fishes."

One last word as to a matter of word-making. I congratulate Mr. Smith Woodward on having the good sense to write "Pteraspidæ" instead of "Pteraspididæ," and "Asterolepidæ" instead of "Asterolepididæ." It would *not* have been more correct to write the longer form, and would have been pedantic.

E. RAY LANKESTER.

THE HONEY BEE.

The Honey Bee: its Natural History, Anatomy, and Physiology. By T. W. Cowan, F.L.S., F.G.S., F.R.M.S., F.S.Sc., &c. Illustrated with Seventy-two Figures of 136 Illustrations. (London: Houlston and Sons, 1890.)

THE interior economy of the hive is known intimately to every bee-keeper; with the anatomy of its makers, rulers, citizens, not one in a hundred is familiar. The mass of facts accumulated during two centuries of discovery lies for the most part embalmed in the Proceedings of Societies, locked up in costly monographs, untranslated from foreign languages: for the first time it is here presented to English readers, in a form at once exhaustive and concise, by the most accomplished of modern apiarians. Careful compilation from former writers, enriched with much matter that is wholly new, conscientious exclusion of theories unverified by experiment, accuracy of illustration secured by direct drawing from photo-micrographs or microscopical preparations, justify, we think, with the deductions inseparable from a first attempt, Mr. Cowan's claim to have produced "the most perfect work of its kind."

From its pages bee-keepers will learn the extent to which the romantic stories current as to their favourites are borne out by scientific research or must be abandoned. They will find the most startling of all, parthenogenesis in the young hive-mother, to be an established fact; the unwedded queen lays eggs profusely, but all of them give birth to drones. No less certain is the derivation of queens and workers respectively from the different kinds of food administered to them in their larval state. The future queen is fed on "chyle-food," elaborated in the chyle-stomach of the nurses, until it assumes the chrysalis change, from which it emerges a perfect female. The future worker is weaned upon the fourth day, and fed henceforth on honey and digested pollen, with the result that its ovaries are rudimentary and sterile, while its further genital structure renders it incapable of mating. The fecundation of the queen takes place within a few days of her quitting the cell, and lasts for life; the millions of spermatozoa injected being imprisoned in a special receptacle, retaining their vitality throughout her lifetime, and escaping one by one to fertilize each egg as it is laid. Similar storage power is present in the digestive system. The food swallowed by the mouth passes, not into the stomach, but into a temporary reservoir, called the honey-sac, from whence it can at will be regurgitated into the cells, or passed into the stomach through a valvular orifice, called the stomach-mouth, a construction which "permits the bee to eat when, where, and how she pleases, without having to trouble her mouth in any way." And this strange power extends even to the respiratory process, one or two rapid breaths so charging the tracheæ, that for the next three minutes no further inhalation is required. One singularly interesting belief is shattered by Mr. Cowan—that the accuracy of comb structure is due to "mathematical instinct." The story of Maraldi's measurements and of Kœnig's miscalculations, of the error in a book of logarithms discovered by the exquisite precision of bee mensuration in constructing the lozenge-shaped plates which form the basis of the cells, has been popularized by many writers. Mr. Cowan's observations show that in no carefully measured piece of comb are all the diameters of the different cells or all the angles of the different plates identical, nor is their variation regular. Cells are sometimes square, sometimes acute-angled, sometimes roughly circular. The mechanical purpose of the bees is confined to the formation of a cylinder, but the mutual interference of the little architects, as in the case of impinging soap-bubbles or bottled peas, compels the hexagonal form.

Though shorn of its mathematical attributes, bee intelligence remains extraordinary. Their prescience, their power of exact mutual communication, the elaborate community of discipline and labour which characterize each colony, throw doubt on the familiar distinction which denies to lower animals the analytic faculty—accords perception to the bee or dog, conception to the human mind alone. Into this question Mr. Cowan does not enter; but we are not surprised to learn from him that the well-defined brain necessary to the possession of intelligence forms in the bee 1/174 part of the volume of the body, sinking in other insects as low as to the 1/4200 part.

The muscular power of the bee is as much in advance of man's as its intellectual power is inferior. A man's power of traction is far below the weight of his body; a bee can draw twenty times its own weight. Its flight exceeds twelve miles an hour, and it will go four miles in search of food. Its wings, braced together in flight by a row of hooklets, bear it forward or backward, with upward, downward, or suddenly arrested course, by a beautiful mechanical adaptation which the author admirably describes. Its voice-organs are three-fold, the vibrating wings, the vibrating rings of the abdomen, and a true vocal apparatus in the breathing aperture or spiracle: the first two produce the buzz; while the hum—surly, cheerful, or colloquially significant—is due to the vocal membrane. Some of its notes have been interpreted. "Huumm" is the cry of contentment; "Wuh-wuh-wuh" glorifies the incessant accouchements of the queen; "Shu-u-u" is the frolic note of young bees at play; "Ssss" means the muster of a swarm; "Brrr" the slaughter or expulsion of the drones; the "Tu-tu-tu" of newly-hatched young queens is answered by the "Qua-qua-qua" of the queens still imprisoned in their cells.

Enough has been said to indicate the interest and value of this compendium. For the description of the tongue, with its rod, spoon, tasting-hairs; of the pulvillus and its adhesive secretions; of the marvellous changes developed in the later metamorphosis of the imperfect insect; of the compound eyes with their mosaic vision, incapable, we suppose, through deficient muscular adjustment (though on this point Mr. Cowan is silent) of delineating to the bee mind a perceptual world resembling ours; of the sting and poison sac, of the wax pockets with their secreted scales, drawn out by the hind leg pincers, made malleable by mastication and saliva, kneaded into foundation walls and cells—the student must betake himself to the book. Full of matter as it is, it leaves much unsettled. The character of the sensory functions, the existence of gustatory and olfactory organs, the meaning of various unintelligible bodies in the antennæ, abdomen, labium, ligula, have not been determined—may perhaps never be understood; for "it is not impossible that insects may possess senses or sensations, of which we can no more form an idea than we should be able to conceive red or green if the human race were blind."

For the second edition of the book, which will doubtless be demanded, certain minor corrections or amendments may be suggested. If bees are *Hymenoptera*, the Greek word for a membrane (p. 2) may fairly be written hymen. In p. 12, ll. 15, 16; in p. 63, ll. 4–9; p. 102, l. 29, slight textual improvements are desirable. Near the bottom of p. 26 is an obvious printer's error. On p. 64, l. 11, the word "those" is not readily referable to the ganglia for which it presumably stands. The engraving on p. 25 is repeated on p. 96. The description of the brain, pp. 68, 69, is indistinct in places, and would gain if the illustrations, 31, 32, were lettered. So, again, the inversion of the male organ, pp. 129, 130, is not clear from Fig. 53. And is it impertinent to hope that the peroration may be re-written or struck out? By all means recognize formally, if it has not indeed been all along assumed, the benevolence and wisdom of a superintending Providence; but the pious platitudes of pp. 190–192 are an anti-climax to the clear reasoning and precise statement which are maintained throughout the scientific portion of the work, and leave on the critical palate an after-taste less sweet than honey to the mouth.

W. Tuckwell.

ELECTRO-METALLURGY.

The Electro-plater's Hand-book. By G. E. Bonney. (London: Whittaker and Co., 1891.)

This book is designated as being a "practical manual for amateurs and young students in electro-metallurgy," and intended "to meet the wants of amateurs and young workmen desiring a practical manual in electroplating at a low price."

It contains a large amount of sound information respecting the details of preparing metals for electroplating; the processes of cleaning, polishing, scratch-brushing, &c.; a number of useful practical tables; numerous recipes for making plating solutions; descriptions of workshop appliances, tools, pieces of apparatus, &c., of immediate practical use to a working electroplater; and it is essentially an ordinary workman's book.

In accordance with the statement made in the preface, that "it supposes the workman to have an elementary acquaintance with electrical science," the book contains scarcely any information respecting the scientific principles of the art, and even Faraday's great law of definite electro-chemical action is not mentioned or explained. It will therefore be more readily purchased by the ordinary than by the superior workman, by those who prefer to work by "rule of thumb" than by those who wish to be guided by the light of science.

With regard to the statement on p. 1, that "the art of electro-metallurgy cannot be said to own an inventor," it is true that the complete art is not due to a single inventor, but it owns a series of inventors, viz. Wollaston (1801), Brugnatelli (1805), De la Rue (1836), Jacobi, Jordan, Spencer (1839), J. Wright (1840), J. Elkington (1865), &c., who were all of them prominent inventors of electro-metallurgical methods.

The rules given on p. 4 for selecting a suitable depositing solution are good, and are well known; and the remark on p. 7, that "the main consideration must be directed to current density," is a widely applicable and very useful one.

The statement on p. 8, that the current deposits the metals from solutions of alloys "in proportions determined rather by their electric equivalents than by the quantity of the metal in solution," is a very doubtful one; for instance, with copper and zinc in solution together, the electro-chemical (not "electric") equivalents of which are about equal, if the copper is in large proportion compared with the zinc, copper is deposited, and no zinc is usually deposited along with it, unless the current is of great density at the cathode.

The idea expressed on p. 12, that "the internal resistance of a battery may be reduced by using longer connecting wires between the cells," is not in accordance with the established conventional understanding that "internal resistance" is limited to the contents of the battery cells. The information given about the arrangement and construction of several kinds of voltaic batteries is full and explicit, and the same may be said about arranging their cells in series and in parallel.

With regard to the chapter on dynamos, it is a very useful one, and the longest in the book; and the construction and action of those machines are as fully described as any other part of the subject; but, whilst several good dynamos for electro-plating are mentioned and illustrated, some obsolete and wasteful ones are figured and described which might have been omitted.

Chapter vi., "Electro-plating with Silver," contains much information, both of general matters and of details useful to the working electro-plater; the statement, however, on p. 125, that the foreign salts which usually exist in a dissolved state in a cyanide of silver plating liquid "offer a resistance to the current," has never yet been demonstrated; in fact, they rather tend to diminish the resistance, and it is only when they give rise to a solid film upon the anode, and that is very rarely, when the liquid is deficient in free cyanide of potassium, that they obstruct the current.

The statement on p. 165, in the chapter on "Nickel-plating," that "very little was effected in a practical way until 1890, when Mr. Adams discovered that nickel could be deposited from a solution of the double sulphate of nickel and ammonia in a reguline condition," requires a little qualification, for both previous to and at that period the double chloride of nickel and ammonium (generally known as "Gore's solution") was rapidly extending in use in America, the early home of nickel-plating.

The chapter on "Electro-plating with Copper," is very brief. Whilst the one on "Dynamos" occupies 38 pages, and might in some respects have been abbreviated with advantage, that on "Copper" fills only 7 pages, and might have been extended.

A rather serious error in the chemistry of the subject occurs on p. 200, where the table, and the column of numbers which are really the atomic weights, are each headed "combining weights"; and the electro-chemical equivalents are headed "electric equivalents." Now, the atomic weights are in many cases very different from the combining weights, and the numbers which are there termed "electric equivalents" should have been called "electro-chemical equivalents."

Instead of employing the simple terms, made, making, stirring, &c., the author, throughout the book, continually uses the phraseology, "made up," "making up," "stirring up," "faking up" the solutions, batteries, apparatus, &c. These, however, are minor matters. The chief defect in the book is that "it supposes the workman to have an elementary acquaintance with electrical science," and, apparently on the basis of that usually fallacious assumption, omits nearly all information respecting the fundamental principles of the subject. Now we know that not only the working man, but the sons of persons in much higher grades of society, are often so incompetent to appreciate the great value to themselves of knowledge of important principles, and so anxious to obtain "quick returns" in the form of money or some easily perceived personal advantage, that they are apt to shirk learning anything which they think will not quickly yield them a profit, and this radical defect should not be encouraged by those who undertake to teach. An omission of all reference to the principles of his subject further indicates that the teacher himself either does not fully understand or adequately appreciate the foundation of the matter he professes to teach. Knowledge of the principles of his occupation is usually the point in which the workman is most deficient and most requires to be instructed.

Notwithstanding this drawback, the book is sound and good in nearly all matters of practical detail; it is also well illustrated, and may be used with advantage as supplementary to a more scientific one containing the principles of electro-metallurgy.

OUR BOOK SHELF.

Primitive Folk: Studies in Comparative Ethnology. By Élie Reclus. (London: Walter Scott, 1891.)

THIS volume belongs to the "Contemporary Science Series," edited by Mr. Havelock Ellis. It contains a popular account of the Eastern and Western Inoits, the Apaches, the Naïrs, the mountaineers of the Neilgherries, and the Kolarians of Bengal. The writer does not group his facts in accordance with any controlling idea, so that the book can hardly be said to have much continuity of interest. He writes, however, in a fresh and lively style, and has brought together many curious facts; and the work may serve as an attractive and useful introduction to the study of some aspects of ethnography. He has, of course, to describe many customs and modes of thought which, if judged from our point of view, would produce a strange impression; but these he puts in their right place as elements which mark definite stages of evolution. Too few references are given in the notes, but the information may be accepted as generally trustworthy, having been for the most part derived from the statements of travellers and missionaries during the first half of the present century.

Tongues in Trees and Sermons in Stones. By the Rev. W. Tuckwell. (London: George Allen, 1891.)

THE greater part of this volume is occupied with papers which bear the general heading "Tongues in Trees." They are interesting essays, written in a clear and pleasant style, and presenting in a fresh light a good many "facts in flower-lore" Among the subjects are tree worship, tree myths and superstitions, plant-names of persons, places, and seasons, plant monsters, gardens, and plant literature. The fact that there may be "sermons in stones" is illustrated by a short paper on sundials, reprinted from the *Gardener's Chronicle.*

Supplement to Euclid Revised. By R. C. J. Nixon, M.A. (Oxford: Clarendon Press, 1891.)

IN this little book the author has placed before the student a few theorems on the more modern geometry of the triangle. The points touched upon relate to the Lemoine and Brocard points, lines, and circles; and a study of these will serve as a good introduction to many of the more advanced theorems. The proofs are concise and complete, and a few exercises are added which are more or less dependent on them.

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 intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Alpine Flora.

FOR some years past we have endeavoured to make as large a collection as possible of the more interesting Alpine plants at Kew. And I am glad to take the opportunity of acknowledging the invaluable assistance which we have received from Mr. G. C. Churchill, who has spared no pains on our behalf in corresponding with collectors in almost every mountainous district of Europe.

The observation of the Kew collection has led me to a few conclusions which have some bearing on the questions raised by Mr. Cockerell and the Rev. George Henslow.

What are called Alpine plants appear to me to fall into two distinct classes: (1) those which have a permanent adaptive habit, and (2) those which have a habit due merely to the local conditions of the growth of the individual plant and not maintained by it if those conditions are altered.

The peculiarities of the former class I am disposed myself to attribute to natural selection. Anyone who supposed that Alpine plants were particularly "hardy," would find himself very rapidly disabused by attempting to cultivate them. I have no definite data on the subject; but I am inclined to think that for the most part they are intolerant of very low temperatures. They certainly are extremely impatient of humidity during the period—a comparatively long one—when they are not in active growth. We are for these reasons, paradoxical as it may seem, obliged at Kew to winter our large collection in frames under glass. The reason is that in Nature, except for a short time, Alpine plants are covered with snow, which keeps them dry and protects them from a very low temperature. Last summer, accompanied by the foreman of our herbaceous department, Mr. Dewar, I crossed the Zwischbergen Pass from the village of Simplon to Saas. This was late in July, and on the south side we crossed a wonderful area of Alpine vegetation from which the snow had only just melted; I suppose that by the end of September it would be covered up again. I presume the ground would remain permanently frozen at no great depth. The plants were therefore reduced to growing in the thin film of soil warmed during exposure by the sun's heat, and this would be, for the greater part of the year, sandwiched between deep overlying snow and the frozen subsoil. No plants which had other than a close dwarf habit would seem to me to have a chance of existing under such circumstances.

On the lower slopes of the valleys of the Alps there is a profusion of interesting plants, which, though not so dwarf as those of higher levels, still derive a good deal of their charm from the more or less compactness of their growth. It is a matter of general experience that, when many of these are transferred to a country such as England, they develop into a coarse weedy habit which deprives them of a great deal of their interest. I am not satisfied that a richer soil in cultivation affords the explanation of this. I am disposed to think that the conditions of solar illumination in the Alps are a much more important factor.

Every practical gardener knows that if you want to maintain a desirable stubby habit in a decorative plant, and to keep the vegetative structures in proper subordination to the flowers, you must expose your plants as freely as possible to sunlight. This is not, however, a question of promoting assimilation, but of preventing elongation and extension of parts already formed. There is probably much the same amount of material in the stubby and the lanky plant.

Now plants which grow at high levels in the Alps are *above* a great screen of aqueous vapour. They therefore get a larger share of the more refrangible rays of sunlight than those that grow nearer sea-level. And it is precisely these rays which it is known inhibit growth or rather extension in length.

But though, as I believe, this restraining and, if you like, dwarfing action has been going on for ages, it is not my experience that the effect is permanently impressed on the species. I wish it were.

I have met with one plant in which I thought that I had got a case of the transmission of an acquired habit. Mr. Churchill procured for us seeds of *Arabis anachoretica*, "a form of *Arabis alpina*, L., with thin tissue-papery leaves, growing in hollows of the rock where neither sun nor rain reach it, just as *Saxifraga arachnoidea*, as also *Heliospermum glutinosum*, and *Zahlbrucknera paradoxa*, all which have very thin tissue-paper leaves." Alas, on cultivation at Kew it reverted forthwith to commonplace *Arabis alpina*.

Mr. Henslow remarks that "the action of the environment on plants is a thing which can be tested," while the accumulation of many useful variations cannot. Hence he appears to infer that Darwinism is an *a priori* theory, while neo-Lamarckism is not. If he rests this conclusion on facts drawn from the study of the Alpine flora, all I can say is, that while horticulturists are obtaining dwarf varieties every day by selection, I am not aware of any one which can be demonstrably shown to be the product of the action of external conditions.

W. T. THISELTON-DYER.

Royal Gardens, Kew.

Neo-Lamarckism and Darwinism.

MR. COCKERELL'S reply to my remarks is interesting, as I cannot find anything in it with which I do not cordially agree. A few words, however, may clear up matters.

(1) I referred only to dwarfs on high Alpine and Arctic regions. *These* are due to cold (see Grisebach, "La Vég. du Globe," i. 49–51; Verlot, "Sur la Production et la Fixation des Variétés," p. 36).

(2) I am quite convinced of the hereditary character of "nanism," as also of "gigantism," as well as of other results of the direct action of the environment on plants; and am glad to find Mr. Cockerell has come to this conclusion from direct observation. As an example of hereditary hypertrophy, the parsnip called "The Student" was raised by Prof. Buckman from seeds of wild plants in 1847, and it is still sold by Messrs. Sutton as their "best variety." On the other hand, Mr. Cockerell is perfectly correct in admitting the, so to say, plasticity of dwarfs. Indeed, my own experiments with aquatic plants lead me to wonder why some species are, as a rule, so fixed as they generally appear to be.

(3) Tall plants are certainly liable to be injured by winds; as I added, "as occurs at lower altitudes"; but are there any such among the dwarfs *above the tree line?* If not, natural

selection has no tall varieties to work upon; or, if natural selection has eliminated them at that altitude, why has it not done so at a lower one? M. Bonnier's experiments go to show that they will not form there (see his figures and descriptions in "Rev. Gén. de Bot.," ii. p. 528). All I ask for is, that Mr. Cockerell's supposition should have some *facts* to rest upon.

(4) I did not question the fact that dwarf Alpine plants may have additional warmth from their close proximity to the soil. De Candolle observes: "Il en résulte que la couche superficielle du sol doit en général être plus chaude sur les montagnes que dans la plaine, la moyenne extérieure étant supposée semblable" ("Géog. Bot.," ii. p. 260). Indeed, I have grounds for suspecting that the prostrate condition, common in Alpine and other exposed plants, is generally, a result of the direct response of thermotropism. In support of this statement, I will give two out of several observations. *Malva sylvestris*, when growing freely, surrounded by a dry hard soil which is readily heated, develops a prostrate form; but it grows erect when in a damp soil and surrounded by other plants. Bluebells often lay their first leaves flat on the ground. The temperature *on* the soil and under such a leaf was 47°·5 at 10 o'clock this morning (April 15). The plant is growing in shade and in damp soil. The temperature of the air 3 inches above it was 44°·5. Lastly, the temperature *under* the flat leaves of a plantain, growing in short grass covered with dew, was 52°·5, while at 3 inches above it, it was 49°·5. What I questioned was the right of building an argument upon a statement, when the opposite condition of great cold arising from radiation at night is ignored. Thus, De Candolle observes:—"Plus on s'élève, moins les rayons solaires sont interceptés par l'atmosphère, plus aussi la déperdition de chaleur par le rayonnement nocturne et par l'évaporation est considérable" (*l.c.* ii. p. 259).

(5) Individual plants may be sheltered; but is not exposure the *prevailing* feature in high regions? Such is, at least, what I have observed.

(6) "Those plants which naturally grow quickly . . . . would survive." Quite true; all I maintain is that the primary cause is not *internal*, as "naturally" seems to imply, but *external* to the plant. That is, the total amount of heat and light received by the plant in a comparatively short time is what induces the rapid development of high Alpine and Arctic plants. But, on the other hand, the responsiveness of plants is not absolutely alike in all cases; therefore we have enough evidence to draw the deduction which I will express in Mr. Cockerell's words: "Given such variations [in the responsiveness to climate], is it not obvious that natural selection would preserve the more rapidly-maturing kinds where the summer was short?" Certainly, as M. Verlot shows (*l.c.* p. 48), what horticulturists are familiar with, that precocious and late varieties are hereditary.

Mr. Cockerell's reply does not combat my sole contention, that it is unscientific to form theories of what "may be" and "might be," when *no, or insufficient, facts* are brought forward on which the supposition is based. There may not be a sufficiency to justify one in unhesitatingly accepting any particular theory; but all I ask for is something *positive*, or well-determined *facts*, upon which a hypothesis may be based. Science is not advanced, but hindered, by "may be's" and "might be's."

George Henslow.

Co-adaptation and Free Intercrossing.

As it appears to me, from his reply, that Prof. Meldola's views on the subject of "co-adaptation" are really the same as my own, I write once more in order to point out the identity.

In the first place, I of course agree that "every [considerable] variation, of whatever kind, must of necessity entail numerous structural and functional modifications"; and therefore "that there is not any 'fortuity' in the supposition that certain variations, A, B, C, D, &c., may arise in the same individual." "But," as Prof. Meldola continues, "the case we have to consider is that in which the variations, A, B, C, D, &c., are not [thus] physically or physiologically correlated; but in which they are supposed to be 'independent variables.'" Now, this being the only case we have to consider, Prof. Meldola says, "The chances against such variations occurring in any one individual are, I concede most fully, 'infinity to one.'" And lower down he as fully concedes the consequence, that if it be supposed necessary for the utility of A that A should occur in association with B, C, D, &c., then the chances must be infinity to one against the perpetuated existence of A. But this is all that Broca's and Spencer's "difficulty" from co-adaptation alleges; and, as I said in my last letter, if it is to be met, it can only be so by denying that any cases do actually occur in nature where the utility of A is thus dependent upon the association of A with the other independent variables, B, C, D, &c. Accordingly, this is the way in which Prof. Meldola's reply does meet the "difficulty." For he concludes—and very properly—"if it be said that A cannot exist by itself, but that A + B is the only form capable of existence, then it remains for those who make this assertion to prove that A and B were coincident in time and space *ab initio*"—*i.e.*, to show cases where the utility both of A and of B is dependent on the association A + B. And he goes on to add, "that the particular case of co-adaptation quoted by Mr. Pascoe [*i.e.* that of the giraffe] may resolve itself into a 'confluence of adaptations.'" But, if so, *cadet quæstio* as regards this particular case. And similarly, he continues, as regards all other possible cases; for the next sentence is,—"In fact, I do not think it would be going too far to put forward the proposition that all cases of co-adaptation may be ultimately explained in the same way, *i.e.* that they arise from the coalescence (by intercrossing) of *n* modifications each *useful* (not useless) in itself, and acquired at successive periods in the phylogeny of the race."

Thus Prof. Meldola meets the "difficulty" in the way which I believe is the only way that it can be met, viz. by denying that, as a matter of fact, any cases of "co-adaptation" (as distinguished from a "confluence of adaptations") ever do occur in nature. This, however, raises a distinct question. The issue between him and those who advance the "difficulty" will now no longer be logical, but biological. And, as stated in my previous letter, I do not intend to go into this distinct question. My only object has been to show that, unless the supposed fact of co-adaptation be denied, any appeal to the analogy of artificial selection is illogical; while, if this supposed fact is denied, any appeal to the analogy—then "real" enough—is superfluous. For, as I said before, it is self-evident that if each of the independent variables is of utility *per se*, natural selection will, sooner or later, blend them all together through free intercrossing.

Thus, with regard to the matter of co-adaptation, it appears that we are in complete agreement. Unless any given case—such as that of the giraffe—is denied to be a real case of co-adaptation, Prof. Meldola "fully concedes" that the chances against its evolution by natural selection alone must be "infinity to one," and, therefore, that the analogy of artificial selection as regards that case would be false. I am sorry to observe, however, that such agreement does not appear to prevail with regard to the distinct and much more important "difficulty of the swamping effects of intercrossing." For in his last paragraph Prof. Meldola ranges himself on the side of the "neo-Darwinians" as regards this difficulty, remarking, on the one hand, that he does not believe it to be "real," and, on the other hand, that it has been "amply treated of by Mr. Wallace and others." The difficulty here alluded to is to conceive how it is so much as logically possible for natural selection *alone* to preserve and accumulate incipient variations in the face of free intercrossing. "Mr. Wallace and others" have attempted to show that this is possible by pointing to the simple and well-known fact that "no being on this earthly ball is like another all in all." They tell us that every specific character—be it of structure, colour, form, size, strength, fleetness, and so forth—is variable round the specific average or mean; so that, if it be desirable either to augment or to diminish any given specific character, there will always be one-half of the number of individuals composing the species which are varying in the required direction at the same time. Now this, of course, is sufficiently obvious; but what does it prove? It only proves what nobody, so far as I am aware, has ever dreamed of doubting—viz. that if it were to become a matter of importance in the struggle for existence that the human form, for example, should be increased as to height, short men and women would be eliminated by natural selection, while tall men and women would transmit their tallness, until in the course of many generations the race would develop into a race of giants. All this is what I would call "pure Darwinism"; and it is not apparent to me how its re-statement by "Mr. Wallace and others" has in any way "broadened" our "ideas" upon the subject. But what I have called "neo-Darwinism" is the Darwinism which fails to distinguish between such a case of the *transmutation* of a specific type in a single line of change, and the *differentiation* of a specific type in two or more lines of change. The race of giants is formed—if I may again quote the pre-Darwinian expressions of the Laureate—by successive generations progressively mounting to higher things on the steps supplied by their own "dead selves." But this ladder-like succession of species in time differs in many respects from a tree-like multiplication of species in space; and the respect in which it differs most has reference to "the difficulty of the swamping effects of intercrossing." The transmutation of species in a single line of change is self-evidently capable of being effected by natural selection alone, because here free intercrossing among the uneliminated "fittest" is the very means whereby the transmutation is effected. But the tree-like multiplication of species (or Darwin's "divergence of character") is no less self-evidently incapable of being effected by natural selection alone; it is not so much as logically possible that any arborescence of species can take place unless natural selection is assisted by some form of isolation at the origin and throughout the development of every branch. The race of giants is evolved by intercrossing being *permitted* between all the uneliminated individuals of each successive generation; but if at the same time a race of pygmies, another race of blacks, another of whites, &c., are to be formed, it is a plain necessity of the case that intercrossing must be *prevented* between all these different races; else natural selection could not so much as begin to differentiate them.

This all-important distinction between what Mr. Gulick has concisely termed "monotypic" and "polytypic" evolution is not observed by "Wallace and others," who are now said to have "amply dealt" with the "difficulty" in question. For they deal only with the case of monotypic evolution, with reference to which no one has been so foolish as to allege any difficulty; and thus their whole treatment of the subject is irrelevant to the only difficulty which has been alleged, viz. the abstract impossibility of natural selection having ever effected *polytypic* evolution, or *divergence* of character, without the co-operation, in some form or another, of segregate breeding by the prevention of free intercrossing. GEORGE J. ROMANES.

Oxford, April 17.

The Flying to Pieces of a Whirling Ring.

DR. LODGE appears to be still in error in making (p. 534), without qualification, the statement that "the dangerous tension will be set up in a straight portion of any endless band running in the direction of its length with the critical speed $\sqrt{\left(\frac{T}{\rho}\right)}$, by the agency of the curved portions which necessarily exist somewhere." For the curved portions of such a band might run *inside* smooth guides which would supply the necessary centripetal pressure and relieve the band from all tension. Such an arrangement was in my mind when I wrote (p. 463) that the band *need not* be stretched to its breaking strain. It is true that Dr. Lodge began his original letter (on p. 439) by specifying a ring "not radially sustained," but he will, I feel sure, allow me to call attention to the fact that without a repetition of this qualification his remark that I have quoted is incorrect: and I think he will see that his supposition that my remarks referred only to a straight bar with free ends, and not to an endless band, is contrary to the expressed terms of my letter. The fact remains that the motion of even an endless band has not necessarily anything to do with the stability of the straight part. Dr. Lodge apparently still wishes us to suppose otherwise.

Devonport, April 12. A. M. WORTHINGTON.

A Lecture Experiment illustrating the Magnetic Screening of Conducting Media.

A LECHER'S tube (*Wied. Ann.*, xli. p. 850) is put, by means of two cork rings, into another large (4 cm. diam.) glass tube, with a crane on one end. Holding the tube in one hand, and approaching it to a wire in which electrical waves are produced, a continuous lighting in the Lecher's tube is to be seen. This lighting remains even if the large tube, including the Lecher's tube, is filled with water. But if the tube is filled with dilute sulphuric acid the light disappears.

When we open the crane and the acid flows out, we perceive the light again, but only in that part of Lecher's tube which is not surrounded with the acid. The light in the Lecher's tube penetrates but very little below the level of the acid in the surrounding tube. J. J. BORGMAN.

St. Petersburg, University, April 14.

The Intelligence of the Thrush.

IT is, I think, well to record the following observations of the intelligence of the thrush. The first happened on June 28, 1865. I then saw, from the windows that look out on the little lawn north of my house, a thrush steadily "stepping westward" in front of the hedge that parts the lawn from the public road. The bird seemed to be intentionally making for a gravel path that, after passing almost close to the windows, bends to the north-west, toward the small gate of my front garden. It was bearing something in its bill. On coming to the path it attempted to break this on a stone. It did not succeed. It then tried another stone. This time it succeeded. Thereupon it flew away. On the spot I found a remarkably big stone embedded in the path, and round it were scattered bits of snail shell. The bird had eaten the snail.

The second of the observations I would note, and the more striking of the two, happened on June 5, 1890. I then was viewing the gravel path from the westernmost of the four windows. Just beneath me, standing on the path, was a female thrush. She had succeeded in breaking a snail shell. She had the snail in her bill. But, despite of vigorous efforts, she could not swallow it. Up hopped a male thrush. Standing before the female, he opened his bill. She dropped the snail into his bill. He chewed the snail. He dropped it back into the female's ready bill. She swallowed it. The pair blithely trotted off, side by side, toward the small gate. I saw them no more.

JOHN HOSKYNS-ABRAHALL.

Combe Vicarage, near Woodstock, Oxfordshire, April 16.

Cocks and Hens.

WHILST I was living at Highfield House in Nottinghamshire in 1879, a duck-wing game bantam injured her leg after she had been sitting for a fortnight, and could no longer remain on the eggs. The cock bird, however, took her place, and not only hatched the brood, but acted in all respects like a hen, brooding the young, setting his feathers up like a brood hen, and using the same peculiar cluck that a hen has for calling her chickens, also sleeping on the nest instead of his usual perch—being, in fact, a mother for the time. Years before (in 1841) one of my Dorking hens was accidentally killed, leaving a brood of chickens some five weeks old; the Dorking cock took to them and brought them up, but in this instance there was no change except the act of brooding. E. J. LOWE.

Shirenewton Hall, Chepstow, April 12.

Cackling of Hens.

IT is often difficult to recall an actual instance of what may be a matter of very common occurrence. Such is to a certain extent the case with the subject to which Prof. Romanes's query in NATURE of April 2 (p. 516) refers.

In a general way it is my impression that the cackling of jungle fowl is not very commonly heard in India, but I feel certain that I have heard it occasionally, and that I once did hear it upon a somewhat considerable scale is impressed very distinctly upon my memory by certain and special circumstances. My tent for a few days in April 1876 was pitched close to a perfectly impenetrable patch of thorny jungle in Orissa. This cover was full of jungle fowl, and I remember hearing the cackling of the hens, which reminded me of the familiar farmyard sounds of home. It is possible that in this case the safety of their retreat may have had something to do with their not fearing to cackle with unusual vigour. V. BALL.

Science and Art Museum, April 18.

THE PARADOX OF THE SUN-SPOT CYCLE IN METEOROLOGY.

IN 1872 and 1873, three writers, independently of each other and dealing with different branches of meteorological science, revived an old speculation of Sir William Herschel's, that the sun's heat probably varies with the visible changes of its photosphere, and that this variation is sensibly reflected in the meteorology of our planet. Mr. Chas. Meldrum reviewing the statistics of the cyclones of the Indian Ocean, Mr. Norman Lockyer those of the rainfall of Ceylon, Southern India, and Australia, and Dr. Köppen the recorded temperatures of numerous stations in various parts of the world, separately arrived at the conclusion that these three classes of phenomena severally afford evidence of a periodical increase and decrease coinciding with that of the solar spots. The speculation, thus started, was followed up with avidity by a large number of inquirers in different parts of the world. The intensity of the solar radiation, the barometric pressure, the levels of rivers, lakes, and inland seas, and even such more remote effects of meteorological conditions as are manifested in the prices of grain, the recurrence of exceptional vintages, of famines, and in the activity of trade, were all brought under investigation; and, for some years, this and other scientific journals contained frequent articles, bringing to notice some new instance or supposed instance of a recurrent variation conforming more or less accurately to the well-known solar period of eleven years. It must be admitted that some of these supposed coincidences were based on evidence that was far from convincing; and since the major part of the weather phenomena of the globe failed to show any distinct trace of the influence of the solar cycle, interest in the subject gradually declined, and for the last few years the discussion of the question has been comparatively in abeyance.

But anyone who considers carefully the bulk of the evidence brought to light in the course of the inquiry will, I think, be prepared to admit that, amid much that is doubtful and inconclusive, the general result has been to establish so many cases of the variation of certain meteorological elements, coinciding with that of photospheric activity, as to place the general truth of the hypothesis beyond all reasonable doubt; but that the amount of the variation is so small in comparison with the irregular vicissitudes of the weather, that it is for the most part obscured and unrecognizable, except fitfully, or in some few favoured regions where these irregularities are less prominent. This has long been the opinion of the present writer, and a similar view was expressed in no hesitating terms by Prof. Arthur Schuster in 1884, in a paper read before the meeting of the British Association at Montreal, and printed in their Reports. "There can," he says, "be no longer any doubt that during about four sun-spot periods (1810 to 1860) a most remarkable similarity [existed] between the curves representing sun-spot frequency and the curves of nearly every meteorological element which is related to temperature. This is not, in my opinion, a matter open to discussion: it is a fact. But it is equally certain that during the thirty or forty years previously to that time no such relationship existed, and that since 1860 the connection has again in some instances become less distinct."

In so far as regards the temperature of the globe, these conclusions are almost identical with those enunciated by Prof. Köppen, in a paper published in the April–May Heft of the Austrian Meteorological *Zeitschrift* for 1881, and indeed the apparent discrepancy of the temperature and sun-spot curves after 1858 had been indicated by him in his former paper, published eight years earlier. But this discrepancy did not set in simultaneously in different parts of the globe, nor has it been such as to obliterate all trace of the sun-spot cycle in the temperature curves. During about four cycles previous to 1858, the temperature of the tropical zone rose and fell with somewhat striking regularity inversely as the spotted surface increased or diminished, so that the highest temperatures approximately coincided with the sun-spot minima, and the lowest with their maxima. A similar course of variation, but of less amplitude and more complicated with subordinate disturbances, characterized also the temperate zones during a great part of this period; but in the north temperate zone, after 1856, these subordinate disturbances gained in relative importance, and although well-marked minima still approximately coincided with the sun-spot maxima of 1860 and 1870, the intermediate period exhibits a series of oscillations some of which are of not less amplitude than that of the solar cycle. In the south temperate zone, on the other hand, the conformity of the temperature with the [inverted] sun-spot curve continued well marked up to 1870, but the secondary oscillation, which is also shown by the sun-spot curve, is more strongly reflected in the former; while in the tropics, as far as can be inferred from the evidence of the very few stations that furnish continuous registers for these years, the coincidence began to fail in 1858, and from 1870 to 1875 the course of the variation appears to have been entirely in disaccord with that of the solar spots.

Now it is in the tropics, and more especially the central equatorial portion of that zone, that we should expect to find evidence of the coincidence if it really exists. It is not only that the solar action is here most intense, but that the influx of the lower atmospheric currents, which are one of the principal agencies that affect and disturb the local "solar" climate, is here most regular, while the alternations of anticyclones and cyclones—the one bringing clear skies and free radiation, the other a cloud canopy and obstructed radiation—are of minor importance, and indeed in the equatorial zone almost evanescent as disturbing causes.

But, as has been already remarked, the stations in the tropics that furnished comparable and continuous registers before 1875 are very few. Up to 1860 they varied from three to twelve for the entire region, and even up to 1875 they were not greatly more numerous. But, since that date, the temperature of India has been recorded with far greater completeness, and it will be interesting, therefore, to inquire how far the ampler evidence thus furnished for at least one tropical country of great extent confirms or invalidates the conclusions drawn from the very defective data of previous years. If we find, as the result of this examination, that the temperature of India as a whole, since 1875, has followed in its variations from year to year the waxing and waning of the spotted areas of the solar surface, and that with even greater regularity than was shown by the scanty records at Prof. Köppen's disposal for the earlier period, we may then infer with some likelihood that some of the discrepancies shown by his curves really arise from the imperfect elimination of the mere local disturbances which all single registers exhibit, when compared with each other, in a more or less marked degree.

For several years past, the annual reports on the meteorology of India have contained a table showing the deviation of the temperature of the country, as a whole, from its normal or average value, in each successive year since 1875; but this has been computed yearly from the several averages of the stations as known up to date, and as these have been corrected yearly, the standards have varied throughout, and in the earlier years differed from those now accepted by amounts quite appreciable in an investigation of this kind. Moreover, the registers of all the existing stations could not be utilized in the earlier years, their average values being then unknown. In the following table, therefore, the mean annual anomalies have been recomputed from the published registers, taking as a common standard the mean temperature of each station given in the report for 1888, and including the registers of all stations in India, Burma, Ceylon, and the adjacent islands, that have furnished a complete or nearly complete register in each year. The results are given in the third column of the table; the fourth shows the annual increments or decrements, and the two right-hand columns Wolf's relative sun-spot numbers and their progressive variation.

Year.	Stations.	Temperature (° F.).		Sun-spot number.	
		Annual anomaly.	Prog. var.	Annual.	Prog. var.
1875	75	0	—	17·1	−27·5
1876	81	+0·21	+0·21	11·3	− 5·8
1877	90	+0·40	+0·19	12·3	+ 1·0
1878	102	+0·84	+0·44	3·4	− 8·9
1879	107	+0·02	−0·82	6·0	+ 2·6
1880	107	+0·37	+0·35	31·5	+25·5
1881	112	+0·29	−0·08	54·2	+22·7
1882	107	+0·10	−0·19	59·6	+ 5·4
1883	118	−0·30	−0·20	63·7	+ 4·1
1884	114	−0·56	−0·26	63·4	− 0·3
1885	120	−0·26	+0·30	52·2	−11·2
1886	120	+0·10	+0·36	25·7	−26·5
1887	127	−0·20	−0·30	13·1	−12·6
1888	127	+0·36	+0·56	6·7	− 6·4
1889 [1]	29	+0·70	+0·34	5·8	− 0·9

From this table it appears that, except in the two anomalous years 1879 and 1887, the temperature of India, &c., as a whole, has followed with remarkable regularity the course of the sun-spot variation; the maximum temperature occurring in 1878, the year of the sun-spot minimum, and the minimum temperature in 1884, in which the spots were almost as numerous as in the preceding year of their maximum. If the figures of column 3 be smoothed by the same process as was applied by Köppen to those of the years antecedent to 1860, it will be found that when laid down graphically the resulting curve corresponds to the inverted sun-spot curve more closely than in any antecedent period. Thus treated they are as follows:—

1875 ...	—°	1882 ..	+ 0°05
1876 ...	+ 0·21	1883 ...	− 0·27
1877 ...	+ 0·46	1884 ...	− 0·42
1878 ...	+ 0·53	1885 ...	− 0·25
1879 ...	+ 0·31	1886 ...	− 0·06
1880 ...	+ 0·26	1887 ...	+ 0·13
1881 ...	+ 0·26	1888 ...	+ 0·30

The amount of the oscillation during this cycle, appears to have been about 1° F., and is about the same as the average of the four cycles antecedent to 1860 as shown by Prof. Köppen's figures.

So far, then, from there being any reason to conclude that the meteorology of our planet since 1860 has ceased to display that direct influence of the solar cyclical variation that was so marked during the preceding half century, it appears that when ample evidence is forthcoming, and in that region where the sun's effect is most direct and intense, being at the same time least exposed to casual disturbance, that influence is displayed as strongly as ever, and it seems highly probable that the apparent discrepancy in tropical regions adverted to by Profs. Köppen and Schuster is to be in great part attributed to the imperfection of the data at their command.

Of all atmospheric conditions, the temperature is the most direct and immediate effect of the sun's heat, and therefore the most likely to afford indication of any variation in the intensity of that heat. But the element which, in this connection, more than any other, has engaged the attention of meteorologists, is the rainfall; and it is their general failure to obtain any distinct confirmation of the influence of the solar cycle in the annual variation of the rainfall, that perhaps, more than aught else, has tended to bring the whole hypothesis into discredit. But there are very good reasons why such inquiries should fail. No meteorological element is more subject to purely local variation, or is more largely influenced by what, in our ignorance, we must term the casual and irregular movements of the atmosphere; and yet most meteorologists have been content to summarize the results of single stations or of a few stations widely scattered, and to accept them as representative of enormous areas. How erroneous and misleading such assumptions must necessarily be, may be proved by anyone who will take the trouble to discuss the annual distribution of the rainfall of any European country well furnished with rain-gauge stations, or, still better, that of any extensive region, such as the United States or the Indian Empire; and he will find that, in any given year, there is no uniform excess or deficiency throughout, but while some districts, states, or provinces have received more than an average amount, others are in defect of the average.

If, for the sake of argument, we may assume that the influence of the sun-spot cycle has been conclusively established in the case of the temperature of the tropics, it follows necessarily that it must also hold good, though, perhaps, with diminished intensity, in every other kind of meteorological condition and in all parts of the world; but it may well be that the effect is so small, that in the higher latitudes it can only be detected on the average of many cycles, and when the simultaneous condition of the whole temperate zone, for instance, can be brought under review. For the present we may restrict our attention to the tropics.

That the rainfall of the Indian Empire, as a whole, exhibited no regular periodic variation during the 22 years 1864-85, I have shown elsewhere. But to this the Carnatic province, comprising the whole of the great plain in the south-east of the peninsula, affords an apparent somewhat striking exception, to which attention was drawn in NATURE, vol. xxxvi. p. 227. It was the rainfall of this tract, or rather of its chief city, Madras, that furnished one of the instances adduced by Mr. Lockyer, in the paper already referred to at the beginning of this article. From the results of the twenty-two years, comprising, therefore, two complete solar cycles, it appeared that, with a general average of only 35 inches for the whole province, a variation of no less than 14 inches took place between the years of maximum and minimum on the mean of the two cycles, even when the amounts actually recorded had been reduced to the terms of a harmonic series. Whether this remarkable oscillation will be found to hold good in future cycles, I will not venture to speculate; but it must be confessed that it seems extremely disproportionate to the temperature-oscillation, the magnitude of which rests on a far more extensive basis.

The Carnatic is the most southern lowland of the Indian peninsula, and therefore perhaps more favourably situated for displaying the direct effect of any variation of the solar heat than most other parts of the Empire, but it is by no means beyond the influence of the dry winds, the unseasonable incursion of which, I have elsewhere shown to characterize years of drought, as is also the case in Northern India. Ceylon, the southernmost portion of which lies within the equatorial zone of comparatively uninterrupted rainfall, is still more advantageously situated; but undoubtedly the region that holds out the best promise of deciding the question of rainfall periodicity, in the present case, is the Eastern Archipelago, in which the network of rain-gauge stations established by the late Dr. Bergsma, has already furnished data for thirteen years, and should therefore in the course of a

[1] The temperature of this year is taken from a preliminary report, received from India since this article was written. 1889 appears to have been a year of maximum temperature probably, as it was also the year of sun-spot minimum.

few years more afford materials sufficient, at all events, for a preliminary inquiry into the subject.

Meanwhile, it will be asked "Why,—instead of seeking for evidence in these by-ways of atmospheric action and reaction, where, at best, we can but hope to detect some remote and distorted reflection of the varying phases of the primary agent—why do we not appeal at once to the fountain head, and put the question to that very radiant heat which is the prime motor, the law of whose variations we are endeavouring to discover?" The obvious reply is that such a course is at present impracticable. So great and at the same time so variable is the atmospheric absorption, that the attempts that have been made to determine the calorific intensity of the sun's rays on the exterior of our atmosphere—the quantity termed, as if in mockery, the solar constant—give values differing so greatly according to the state of the atmosphere, that it is hopeless to detect in them with any certainty a variation so small as that indicated by the temperature oscillation. The only continuous actinometric register as yet carried out is that of M. Crova at Montpellier since 1883. I have not seen any reduction of his results for the determination of the so-called solar constant in different years, but the variations of the mean measured intensity on clear days from year to year are very irregular, and the mean of 1883 exceeds that of 1887 by not less than 12 per cent.

The more popular instrument, the sun thermometer, is still more unsatisfactory. While it is affected by obscure atmospheric changes equally with the actinometer, it is also influenced by every change in the surrounding objects, and by the wind, and the instrument itself is so uncertain that it is hard to find two that read alike. Many of them also are subject to a gradual deterioration that renders their earlier and later registers no longer comparable. Only in the few rare cases, where one and the same invariable instrument has been observed on the same spot, with the same *entourage*, during many years in succession, can any valid comparison be made between the values of successive years, and owing to the great fragility of these thermometers and the difficulty of adequately protecting them, such records are very rare. One such register was obtained by the late Prof. S. A. Hill at Allahabad during the ten years 1876-85, and the results were very carefully discussed by him, and, as far as possible, corrected for the varying absorption of the atmosphere due to the several elements, dry air, water vapour, and suspended dust. The mean result, in terms of the corrected insolation readings (*i.e.* the excess of the equilibrium temperature of the instrument over the temperature of the surrounding atmosphere), was thus found to be as follows in each year:—

1876	82°·8	1881	81°·8
1877	85·1	1882	79·6
1878	85·2	1883	78·6
1879	83·6	1884	80·4
1880	82·7	1885	83·1

The cyclical variation shown by these figures is identical in character with that of the air temperature of India as a whole, the maximum being in 1878, the minimum in 1883, which were respectively the years of minimum and maximum sun-spots, but, as might be expected, the oscillation is much greater. Even when the range is somewhat reduced by the smoothing process already applied to the temperature records, it amounts to five and a half times as much as that of the air temperature in the same period. From the nature of the observations, however, no great stress can be laid on this fact.

We now come to what may be termed the paradox of the whole problem. We have seen that both the air temperature and that of insolation seem to testify unmistakably to the fact that the sun's heat is greatest when his surface is least spotted, and *vice versâ*. But the evidence of the spectroscope points in a diametrically opposite direction, and so also do Meldrum's and Poey's statistics of the frequency of tropical cyclones, and, as far as it goes, the more dubious evidence of the rainfall, since, in all cases in which any appearance of a periodical variation has been detected, the rainfall is most abundant about or shortly after the epoch of maximum sun-spots, and least about the years of minimum, implying therefore increased evaporation and an increased movement of the atmosphere at the former epoch. The variation of the barometric pressure which has been detected in the Indo-Malayan region on the one hand, and in Western Siberia and Russia on the other, also seems to show that in years of maximum sun-spots a larger portion of the tropical atmosphere is transferred to high latitudes in the winter hemisphere, which again implies an increased disturbance of atmospheric equilibrium at that epoch between the tropics and the circumpolar zone, and therefore an increased intensity of the disturbing agent.

I must content myself with pointing out this discrepancy without attempting to explain it. It does not necessarily invalidate the evidence of either class of facts, since there may possibly be causes at work, which, when known, will be found to reconcile the apparent inconsistency; but it should assuredly act as a stimulus to our efforts to extend our basis of facts, in full confidence that all inconsistency will eventually disappear.

Before concluding this summary, I must briefly notice a very important investigation of Prof. Hann's, which, at first sight, might seem to negative the whole hypothesis of a cyclical variation of the solar heat; though such a conclusion would be by no means legitimate, and, in point of fact, is not put forward by its author. It was shown by Lamont that, when the diurnal double oscillation of the barometer is analyzed into its two chief constituents, viz. a wave of diurnal and two of semi-diurnal period, while the former varies very greatly in character and magnitude with the geographical conditions of a place, the latter is almost unaffected by these conditions, except that its amplitude decreases the higher the latitude. Assuming that this semi-diurnal tide is a direct effect of the sun's rays absorbed by the higher strata of the atmosphere, Dr. Hann examined the registers of the hourly observations of the barometer at Bombay, Batavia, and Vienna from 1847 to 1862, to see whether the amplitude of this element of the oscillation showed any appreciable increase and decrease corresponding to the phases of the sun-spot cycle. The result was that no such variation was to be detected. The fundamental assumption here made, that the magnitude of this double oscillation should vary with the quantity of heat absorbed by the atmosphere, was verified in a subsequent elaborate memoir, in which it was shown that at the time of perihelion, when our planet receives one-fifteenth more heat than at aphelion, the amplitude of the semi-diurnal tide is, on an average, as much as one-tenth greater—an exaggeration of the effect which Dr. Hann attributes to secondary meteorological actions. In any case, the result satisfies the logical conditions of the inquiry.

Now, accepting Dr. Hann's conclusion that "the heat absorbed by the atmosphere does not vary considerably [*erheblich*] with sun-spot frequency," it remains to inquire whether, taking as our standard the amount of the temperature variation deduced by Köppen, and substantiated by the later Indian registers, the effect to be expected is of such magnitude as would be readily rendered evident in the amplitude of the semi-diurnal wave of pressure. There is no reason to suppose that it would bear a greater ratio to the total pressure effect than does the temperature increment during the sun-spot cycle to the total effect of the sun's rays on the temperature of our atmosphere. What the temperature of our earth would be, in the absence of the sun, we do not indeed know. But we shall probably be well within reasonable limits if we assume that it would be at least 100° below the zero of Fahrenheit's scale, or 180° below the actual mean temperature of the tropics. Of course, under such circumstances, the temperature of the tropics would be no higher than that of the Poles. Making this assumption, then, the oscillation of the temperature during the course of the sun-spot cycle is only 1/180 of the total effect. Now the amplitude of the semi-diurnal element of the barometric oscillation at Batavia (the most equatorial of the three stations) is 1·896 mm., or 0·0746 of an inch, and the 1/180 part of this is 0·0004 inch—a quantity that would be quite inappreciable in an investigation of this kind, masked as it must be by the much larger irregular variations shown by the register. It does not seem, then, that Dr. Hann's negative results really affect the validity of the positive evidence already afforded by other meteorological phenomena, and need not discourage us in our endeavours to obtain an explanation of the paradox indicated above, which, to my mind, is the most interesting feature of the problem. HENRY F. BLANFORD.

THE QUESTION OF THE ASTEROIDS.

IT has already been noted that the editors of the *Berliner Jahrbuch* have decided only to issue ephemerides for certain of the minor planets (NATURE, vol. xliii. p. 111). The Bureau des Longitudes has endeavoured to remedy the inconvenience that arises from this decision by an extension of one of its departments. The general discussion that led the Bureau to adopt this course, and the importance of observations of these comparatively small bodies, are well expounded by M. F. Tisserand in the *Annuaire* for 1891.

Kepler recognized the continuity of the mean distances of the planets from the sun, when he said : "Infra Martem et Jovem novum interposui planetam." The publication of Bode's empirical law in 1772 confirmed Kepler's ideas, and fixed the distance of the hypothetical planet as 2·8 times the mean distance of the earth from our luminary. But the existence of such a planet appeared still more probable when the calculations of Lexell and Laplace had shown that the magnitude of the orbit of the planet Uranus, discovered by Sir William Herschel in 1781, might have been predicted with accuracy from this relation between planetary distances. At a Congress held at Gotha in 1796, it was proposed to search for the unknown body, and twenty-four astronomers each undertook the examination of an hour of the zodiac. The discovery of Ceres by Piazzi on January 1, 1801, almost before the association of observers had got fairly to work, is a matter of common knowledge. Gauss's calculations showed that the mean distance of this planet from the sun is 2·77, which corresponds with that indicated by Bode's law ; hence the gap appeared to be filled, but by a body of very modest size, for the measures made by Herschel only assign it a diameter of about 155 miles.

Olbers's discovery of a second planet, Pallas, moving round the sun at the same mean distance as Ceres, gave the question another aspect. It was proved by Gauss that the two bodies may pass very near to each other at two points situated on the line of intersection of the planes of their orbits. This led Olbers to believe that the new planets were portions of a larger body broken up by some internal disturbance, and he accordingly suggested that other fragments might be found near the points of intersection of their orbits. The hypothesis was supported by Harding's discovery of Juno in 1804, near one extremity of the line of intersection, and the discovery of Vesta by himself in 1807, close to the other extremity.

It was not until 1845 that a fifth planet was discovered by Encke, and this was even smaller than the four that preceded it. After this date the discoveries became more frequent, and now the number has reached 308. But the magnitudes of the newly-discovered bodies are decreasing, for, whilst the first four have magnitudes comprised between 6 and 8, the two discovered by Encke are only of the 9th magnitude, and those now found are rarely brighter than the 13th magnitude.

The hypothesis advanced by Olbers as to the origin of the *asteroids*—a designation due to Herschel—has not found much support. Newcomb, from an investigation of the orbits of the first forty asteroids, found that their planes are far from having a common line of intersection. It may be suggested that this geometrical condition was fulfilled at a certain epoch, and that the perturbations caused by Jupiter and Saturn have caused the present distribution. Calculations show, however, that the required condition never existed ; hence Olbers's hypothesis must be abandoned.

With regard to the width of the zone which contains the orbits of the asteroids, we find that (149) moves round the sun at the shortest mean distance, viz. 2·13, and that (279), the asteroid most removed from our luminary, has a mean distance of 4·26. The periods of revolution of these two bodies are, respectively, 3·11 and 8·81 years. It will therefore be seen that the asteroids revolve in orbits much greater and less than that assigned by Bode's law.

When the eccentricities of the orbits are considered, it is found that (132) may approach to a distance of 1·61 from the sun, whilst (175) may get so far away as 4·73 times the earth's mean distance. The asteroids are, therefore, contained in a wide zone, and the whole of their positions form a kind of ring having a radius a little more than three times the distance of the earth from the sun.

If the asteroids are arranged into groups, of which the eccentricities are comprised between limits differing by 0·05, the following result is obtained :—

23	between 0·00 and 0·05	51	between 0·20 and 0·25
54	,, 0·05 ,, 0·10	12	,, 0·25 ,, 0·30
74	,, 0·10 ,, 0·15	9	,, 0·30 ,, 0·35
62	,, 0·15 ,, 0·20	1	,, 0·35 ,, 0·40

The mean eccentricity, 0·15, is much higher than the corresponding mean of the major planets—viz. 0·86. It appears, therefore, that some notable differences must have existed in the conditions of formation.

But the difference is still more striking in the case of the inclinations of the orbits. The mean inclination is 8°, which is slightly greater than that of Mercury and that of the sun's equator. Of 293 asteroids, 17 have inclinations greater than 20°. These are given in the following table, and also their eccentricities and mean distances :—

	Mean distance.	Inclination.	Eccentric
(290)	2·31	21·55	0·05
(273)	2·40	20·24	0·16
(25)	2·40	21·35	0·25
(132)	2·60	25·0	0·38
(164)	2·63	24·25	0·35
(185)	2·74	23·17	0·13
(247)	2·74	25·7	0·24
(71)	2·76	23·19	0·17
(2)	2·77	34·44	0·24
(148)	2·77	25·21	0·18
(183)	2·80	26·33	0·35
(130)	3·11	22·57	0·21
(276)	3·12	21·58	0·09
(31)	3·15	26·27	0·22
(176)	3·19	22·31	0·16
(154)	3·20	20·59	0·08
(225)	3·40	20·45	0·27

It will be seen that, with two exceptions, the orbits greatly inclined to the ecliptic have also considerable eccentricity. The converse of this is not true, however, for great eccentricity does not appear necessarily to carry with it a high inclination. A consideration of these eccentricities and inclinations naturally leads one to ask whether they were the same at the time of the formation of the bodies whose orbits they represent, or whether the values have been increased under the influence of perturbations. On this point Leverrier made the following remark:—

"There exists a region between Jupiter and the sun, such that, if a small mass could be placed there, in an orbit slightly inclined to that of Jupiter, this little mass would be able to move out of its primitive orbit, and to attain a great inclination to the plane of the orbit of this planet and to that of Saturn. It is remarkable that this position is found at very nearly double the distance of the earth from the sun—that is to say, at the interior limit of the zone where the minor planets are found."

This fact is very interesting in itself, but it is not sufficent to explain the large inclinations that are found at the distances 2·75 and 3·15. M. Tisserand found some years ago that the region of instability is at a distance of only 1·83 from the sun. We must therefore conclude that the perturbations caused by Jupiter and Saturn are insufficient to explain the considerable values of the eccentricities and inclinations of a great number of asteroids; these values are never very small, and consequently the conditions under which Laplace's hypothetical nebulosity existed were not the same at the time of formation of the older planets as at the creation of the asteroids. The question is therefore a very interesting one from a cosmographical point of view, and the accumulation of new discoveries of asteroids is the only thing that will facilitate its solution.

The distribution of asteroids according to their mean distances from the sun, or, what amounts to the same thing, according to their mean diurnal motion, brings out some interesting facts. If the number of asteroids be tabulated having mean movements comprised between 540″ and 550″, 550″ and 560″, and so on, with increments of 10″ up to 1140″, an accumulation is evident about 640″, 780″, and 815″, which movements correspond to the mean distances 3·13, 2·75, and 2·67. Two gaps are seen about 600″ and 900″, or at the distances 3·27 and 2·50. The mean diurnal motion of Jupiter is 299″·12, or nearly 300″. It will therefore be seen that the gaps correspond to two regions where the mean motion of the planet would be exactly double or triple that of Jupiter. There are other gaps less definitely marked, where the relation between the two diurnal motions, instead of being equal to 2 or 3, is represented by the fractions $\frac{5}{3}$, $\frac{7}{3}$, $\frac{5}{2}$, $\frac{7}{2}$, &c. Kirkwood developed this relation in 1866, and generalized it by saying that the parts of the zone of the asteroids in which there exists a simple commensurable relation between the time of revolution of a minor planet and that of Jupiter are represented by gaps similar to the intervals that separate the different rings of Saturn. It should be remarked, however, that the intervals are not so well defined as in the case of Saturn's ring, inasmuch as after a gap the number of asteroids does not increase sharply, but little by little, until it reaches the normal value.

Can these gaps be explained by the theory of perturbations? The illustrious Gauss, writing to Bessel in 1812, remarked: "The mean motions of Jupiter and Pallas are in the relation expressed by $\frac{7}{18}$, a value that ought to be realized more and more exactly under the influence of the attraction of Jupiter, as is also the case in the equality of the motions of revolution and rotation of the moon."

Newcomb in his researches into Saturn's system, found that, in the case of exactly commensurable motions, the perturbations could not increase beyond a certain limit. But this consequence is by no means necessary, for, in all probability, there would only be more or less irregular oscillations, and equilibrium would then be restored. The work of Gyldén and M. Tisserand himself tend to the same conclusion. It is therefore probable that if the gaps had not existed in the beginning, the ulterior perturbations by Jupiter would not have been sufficient to produce them; they must have existed immediately after the formation of the asteroids. This is another reason why the question is of interest from a cosmographical point of view.

The asteroids situated at the outer limit of the ring are not devoid of interest. Some of them have orbits very similar to certain comets of short period; thus the orbit of (175) is very similar to that of Tempel's periodic comet (1867 II.), as is shown by the following comparison:—

	Mean distance.	Eccen-tricity.	Longitude of ascending node.	Inclina-tion.
			°	°
(175)	3·51 ...	0·35 ...	23·6 ...	3·8
Tempel's comet ...	3·49 ...	0·41 ...	72·4 ...	10·8

Other asteroids are important because of their near approach to Jupiter. Of these, (279), discovered by Palisa about two years ago, is of special interest, for in 1912 it will be at the same distance from Jupiter as the earth is from the sun. At this time the attraction of Jupiter on the asteroid will be more than $\frac{1}{50}$ that of the sun; thus the calculations of the perturbations promise to be interesting and difficult, and from them the mass of Jupiter may be determined with considerable precision.

The asteroids which occur at the inner limit of the ring are also useful, especially if their orbits are very eccentric and they approach the earth within 0·7 its mean distance. In such cases their parallax may be very accurately determined by observations made at two stations some distance apart. From the values obtained, the parallax of the sun may be deduced, and this is one of the best methods at our disposal for the determination of an element of fundamental importance in astronomy.

We have said that it is impossible to entertain the idea that all the asteroids were formed by the rupture of a single planet. But groups of two planets may be formed having strikingly analogous elements. The most interesting is formed by the asteroids (37) and (66); their orbits are almost equal ellipses, situated very nearly in the same plane, and differ only in the orientation of their major axes. The analogy is apparent from the following comparison of elements:—

	Mean distance.	Eccentricity.	Longitude of ascending node.	Inclina-tion.
			°	°
(37) ...	2·644 ...	0·176 ...	8·21 ...	3·7
(66) ...	2·645 ...	0·175 ...	8·17 ...	3·6
(106) ...	3·17 ...	0·18 ...	63·2 ...	4·6
(245) ...	3·10 ...	0·20 ...	62·2 ...	5·6
(218) ...	2·67 ...	0·12 ...	170·8 ...	15·2
(246) ...	2·69 ...	0·10 ...	162·6 ..	15·6
(84) ...	2·36 ...	0·24 ...	327·5 ...	9·4
(249) ...	2·38 ...	0·22 ...	334·7 ...	9·7

This quasi-identity of elements cannot be accidental, and it would not require many facts of this nature to throw new light upon the origin and formation of the bodies possessing them.

Enough has been said to prove that the discovery of asteroids has brought out some important facts. For this and other reasons, M. Tisserand thinks that the search for them should be continued. The calculations required to furnish elements and ephemerides are certainly formidable, but they could be divided among several scientific establishments. The Bureau des Longitudes is willing to play an onerous part. We hope that sufficient resources will be accorded to it to allow this extremely useful work to be carried on. RICHARD A. GREGORY.

SOLUTIONS.[1]

THE brilliant presidential address of Prof. Orme Masson at the Chemical Section of the Australasian Association for the Advancement of Science marks a distinct advance in our ideas of solution. The analogy between the behaviour of a liquid and its vapour in presence of each other and of a pair of solvents capable of mutual solution is so striking as to carry conviction. The resemblance of the liquid-vapour curve, with its apex at the critical point, to the solubility curve, with its apex at the critical solution point, appears to me to prove beyond cavil that the two phenomena are essentially of the same nature.

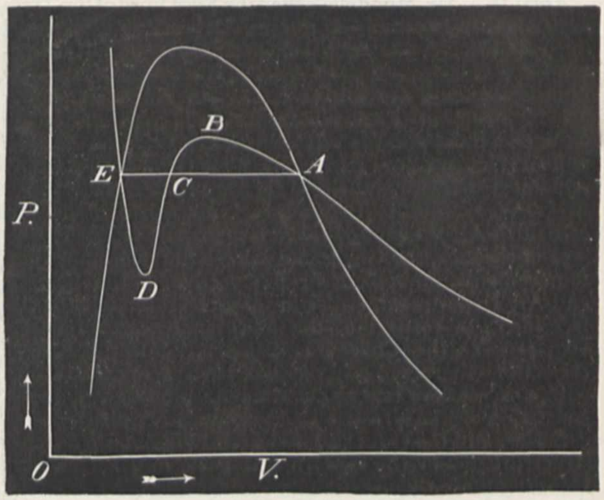
There are two other phenomena, which, it appears to me, are made clear by the ideas of Prof. Masson. The first of these has reference to supersaturated solutions. The curves (published in NATURE, February 12, p. 348) showing the analogy between liquid-gas and solution curves, are isobaric curves, or, more correctly, they represent the terminations of isobaric curves in the region of mixtures, where, on the one hand, a liquid exists in presence of its vapour, and, on the other, one solvent in presence of another (for both solvents play the part of dissolved substances, as well as of solvents). M. Alexéeff's data are not sufficient to permit of the construction of a curve representing a similar region mapped out by the termination of isothermal lines. But it is obvious that it would be possible to determine osmotic pressures of various mixtures by the freezing-point method, and so to construct isothermal curves for such mixtures of solvents. And there can be no reasonable doubt that, as the isobaric curves of liquid-gas and of solvent-solvent display so close an analogy, the isothermal curves would also closely resemble each other.

Granting, then, that this is the case, we may construct an imaginary isothermal curve on the model of the curve for alcohol published in the Phil. Trans. by Dr. Sydney Young and myself. Now, in one series of papers on the liquid-gas relations, we showed that with constant volume pressure is a linear fun ction of temperature ; and we were thus able to calculate approximately the pressures and volumes for any isothermal representing the continuous transition from the gaseous to the liquid state (see *Phil. Mag.*, 1887, vol. xxiii. p. 435). It would be interesting to ascertain whether, if concentration be kept constant, osmotic pressure would also show itself to be a linear function of temperature. But, this apart, it appears in the highest degree probable that there should also exist, in theory at least, a continuous transition from solvent to solvent, the representation of which would be a continuous curve. In such a case, on increasing the concentration of the solution by eliminating one solvent, the other solvent should not separate visibly, but the two should remain mixed until one solvent has been entirely removed. The accompanying diagram will make this clear. The sinuous curve ABCDE may represent either continuous change from gas to liquid along an isothermal on decrease of volume, or it may represent a similar continuous change from saturated solution to dissolved substance on increase of concentration.

[1] "Some Suggestions regarding Solutions." By William Ramsay, Ph.D., F.R.S., Professor of Chemistry in University College, London. Read before the Royal Society on Thursday, March 5.

Mr. Aitken's experiments on the cooling of air containing water-vapour have shown us that it is possible to realize a portion of the curve AB ; the phenomenon of "boiling with bumping" constitutes a practical realization of a portion of the curve DE ; and we may profitably inquire what conditions determine such unstable states with solvent and solvent.

Regarding the portion of the curve AB, I think that no reasonable doubt can be entertained. It precisely corresponds to the condition of supersaturation. In the liquid-gas curve, the volume is decreased at constant temperature without separation of liquid ; in the solvent-solvent curve the concentration is increased without separation of the solvents. Dr. Nicol has shown that it is possible to dissolve dry sodium sulphate in a saturated solution of sodium sulphate to a very considerable extent without inducing crystallization ; and here we have a realization of the unstable portion of the curve AB. In the gas-liquid curve pressure falls with formation of a shower of drops ; in the solvent-solvent curve crystallization ensues, and the solvents separate. The phenomena are, however, not completely analogous ; the complete analogy would be if the temperature were so low that the substance in the liquid-gas couple were to separate in the



solid, not in the liquid, state. This, so far as I am aware, has not been experimentally realized, but one sees no reason why it should not be possible.

I have some hesitation in offering speculations as to the state of matter at the portion of the continuous curve DE. It may be that it corresponds to a syrupy or viscous state. Cane-sugar at a moderate temperature dissolves water ; indeed it is possible to obtain a solution of 1 per cent. of water in molten cane-sugar. And such a solution, if quickly cooled, remains a syrup. But it can be induced to crystallize by the presence of crystals. Thus, in such a mixture of sugar and water, a few grains of crystalline sugar cause the whole mass to crystallize, and water saturated with sugar and sugar separate into two layers. Here, again, a complete analogy fails us, for it is a solid which separates. As we know nothing of the osmotic pressure of a syrup, the analogy is a defective one ; but it is probable that a dilute solution of sugar would pass continuously into a syrup of pure sugar by evaporation of the solvent, and analogy would lead to the supposition that the syrup coincides with the unstable state of the liquid. I would, therefore, offer the analogy between the syrupy and the supercooled states as a tentative one : it lacks foundation in both cases.

One point remains to be mentioned. I have for the past nine months, in conjunction with Mr. Edgar Perman, been determining the adiabatic relations for liquid and gaseous ether : the rise of pressure and temperature when volume is decreased without escape of heat. It is obvious that similar relations are determinable for solutions, and probably with much greater facility. M. Alexéeff has made some measurements which might be utilized for this purpose; but they are far too few in number, and, moreover, the necessary data as regards osmotic pressure are wholly wanting. It would be possible, by a series of differential experiments, to ascertain the evolution of heat on increasing concentration, and so to arrive at a knowledge of the specific heats of the solution at constant osmotic pressure, corresponding to the idea of specific heats at constant pressure; and also of specific heats at constant concentration, corresponding to specific heats at constant volume. I do not know whether such researches would yield as accurate results as those we are at present carrying out, but they are at least well worthy of attention.

THE SCIENCE COLLECTIONS.

We give below the latest Parliamentary proceedings relating to this question:—

April 16.—Mr. Mundella asked the First Lord of the Treasury whether, in proposing to give the large plot of ground opposite the Normal School of Science at South Kensington for a Gallery of British Art, the Government had fully considered the difficulties of placing an art gallery under an independent management between portions of the Science School and the science collections of the Science and Art Department; and whether the Government would appoint a small committee to report on the matter.

Mr. W. H. Smith—In assigning as a site or a Gallery of British Art the plot of ground opposite the Royal College of Science, the Government have not overlooked the requirements, immediate or prospective, of the Science and Art Department, either in respect of its science collections or in respect of the additional accommodation required for the College of Science. For the science collections there will be available, part at once and part within a year or two, a continuous range of galleries, consisting of the present southern, western, and eastern galleries, and the cross gallery which is about to be constructed between the western and eastern galleries, thus affording more than the amount of accommodation which the committee of 1889 considered to be necessary. Over and above this accommodation, Government has at disposal more than three acres of vacant land facing the Imperial Institute, and considerable areas besides to the south of the present southern galleries. A portion of these vacant lands can be utilized for the extension of the College of Science and for future growth of the science collections. Additions to the College of Science must in any case take the form of a separate building, divided from the present building by Exhibition Road; and as access to the lands mentioned above from Exhibition Road will be secured by means of a corridor, the interposition of the Gallery of British Art need have no more serious effect than to increase by some 60 yards (which will be under cover) the distance between the two portions of the Science College. As the Art Gallery will be a distinct and separate building, the fact that it will be under different management need cause no greater difficulty than does the fact that the Natural History Museum is under a different management from that of the adjoining science galleries.

April 20.—Sir H. Roscoe asked the First Lord of the Treasury whether he would have sketch plans prepared and placed in the Library, showing the relative positions of the existing science schools and science museums, the proposed buildings for the same purposes, and the proposed gallery of modern art, on the land at South Kensington.

The Chancellor of the Exchequer—As no definite scheme has yet been framed, it would be impossible to show exactly the appropriation of land now vacant, but I can promise the hon. member a ground plan, showing what land is available for further science buildings.

Sir H. Roscoe asked the right hon. gentleman whether he would give an assurance that the final step would not be taken in the appropriation of the land until after hon. members had had some opportunity of inspecting the plan.

The Chancellor of the Exchequer—The position of the matter is this. The House knows of the very generous offer that has been made of £80,000 to erect a building. A great deal of necessary and unavoidable delay has taken place, and I think it would be unwise to risk the failure of that generous gift by any further delay. Therefore, I am unwilling to pledge myself to any further delay, but I can assure my hon. friend that every security will be given to gentlemen interested in science that their wishes shall be met as far as possible.

There are a few more questions which might be asked, such as the following:—

1. Did the generous donor of the £80,000 know when the site was chosen that its employment for an art gallery interfered with its contemplated and natural use in relation to the buildings adjacent to it?
2. Are there no other sites, including one in Kensington Gardens, which would equally satisfy the donor, and not be liable to the objections so widely raised against the proposed one?
3. Has Mr. Goschen from first to last made any inquiry whatever as to whether there were any objections to the proposed employment of the site?
4. Has the Science and Art Department been asked its opinion, or have the professors of the College been consulted?

It would seem, indeed, that here we have another instance of the way in which Government arrangements in relation to science are made without knowledge.

NOTES.

The Council of the British Association for the Advancement of Science has resolved to nominate as President of the Association for next year Mr. Archibald Geikie, F.R.S., Director-General of the Geological Survey. The meeting will be held in Edinburgh.

On Tuesday next (April 28) Dr. E. E. Klein, F.R.S., will begin at the Royal Institution a course of three lectures on Bacteria: their nature and functions (the Tyndall Lectures); and Mr. H. Graham Harris will on Saturday (May 9) begin a course of three lectures on the artificial production of cold.

The Royal Naval Exhibition will be opened by the Prince of Wales, on behalf of the Queen, on Saturday, May 2.

The following cutting from the *Sydney Morning Herald* —date not stated—has been sent to us for publication:—"The barque *Killarney* had both a stormy and an extraordinary passage, in one portion of which she stood a good chance of being placed on the list of missing vessels. At 9 p.m. on October 15, 1890, when the barque was 50 miles east of Kent's Group, there was a sudden shift of wind during a heavy thunderstorm. In the midst of a heavy clap a bulky mass was heard to fall into the sea about 200 yards from the vessel. The roar of it coming through the air was quite distinct from that of the thunder, and spray was thrown fully 40 feet high on its reaching the water. The falling mass is believed to have been a meteor."

The Wolsingham Observatory Circular No. 31 states that a new variable star was found on March 2 at 4h. 26m. 4s. +65° 53′

('55). The observation has been confirmed at Harvard Observatory.

IN his report on educational work for the year ending February 23, 1891, the Principal of the Mason Science College records that in every department of the College (with the exception of one department which has laboured under special disadvantages through the death of an honoured professor) there is an increase in the number of students, a growing earnestness of purpose, and a creditable record of work accomplished. The analysis of attendances of students shows that the number of students attending in one department only is steadily decreasing, while the numbers attending in two or more departments, and preparing for University examinations, are increasing with equal persistence.

Science of April 3 gives a full account of the somewhat elaborate preparations which have been made for the work of the fourth session at the Marine Biological Laboratory of Wood's Holl, Massachusetts. The laboratory for investigators will be open from June 1 to August 29. It will be fully equipped with aquaria, glass ware, reagents, &c., but microscopes and microtomes will not be provided. In this department there are fourteen private laboratories supplied with aquaria, running water, &c., for the exclusive use of investigators, who are invited to carry on their researches here free of charge. Those who are prepared to begin original work, but require supervision, special suggestions, criticism, or extended instruction in technique, may occupy tables in the general laboratory for investigators, paying for the privilege a fee of fifty dollars. The number of such tables is limited to ten.

M. ROTY, Member of the Institute, has designed for the French Association for the Advancement of Science a new medal, a representation of which is given in *La Nature*. On the obverse are two figures, representing France and Science. France, dejected by the misfortunes of 1870–71, is being encouraged by Science, which points to the symbols of sunrise and recovered prosperity. On the reverse is a fine female figure, seated and reading, described by *La Nature* as "science, poetry, idealized thought."

IN a speech delivered the other day at Kimberley, Mr. Cecil Rhodes announced that he had received "enormous subscriptions" for the establishment of a teaching University in Cape Colony.

THE St. Petersburg correspondent of the *Times* says the Russians seem determined, if possible, to retrieve the failure of Captain Grombtchefsky in trying to get through Afghan Kafiristan. A telegram from Samarkand states that another captain, by name Bortchefsky, is about to start from Southern Bokhara at the head of a scientific expedition to the Pamir, and will endeavour to penetrate Kafiristan and reach the frontier of China.

THE *Kew Bulletin* for April opens with a note on Persian tobacco or tombak, establishing the botanical identity of the tobacco plant cultivated in Persia. Dr. J. Hornsey Casson lately sent to the Kew Gardens, from the Shiraz district, some specimens of fruit, flower, and seed. The material thus obtained "required a good deal of soaking and manipulation before it could be brought into a form in which it could be compared botanically. This, however, having been done, the conclusion was incontestable that the plant of the Shiraz *tunbaku* was nothing more, as had indeed been expected, than ordinary *Nicotiana Tabacum*." The same number contains some letters by the late George Woodruff and Harold Edmund Bartlett, who were sent from Kew in 1889 to take charge of the botanical stations established by the Royal Niger Company in the interior of the Niger Protectorate. The letters were written to former fellow-gardeners at Kew, and, as the *Bulletin* says, are "interesting as showing the type of men that the Royal Gardens turn out; the plucky way in which they face their difficulties, their loyalty to their employers, and the kindly feeling they entertain towards Kew." The gardeners at Kew are specially trained to fit them for such appointments as those accepted by Mr. Woodruff and Mr. Bartlett. "The Royal Gardens have, in fact, always been an advanced technical school. Each gardener is admitted for a two years' course, during which he has the opportunity of seeing every kind of cultivation carried on in the establishment, and, in addition, obtains systematic instruction in scientific subjects connected with his profession. The best men receive appointments as opportunity offers, and they are now to be found in every part of the world." In the April number there are also sections devoted to Aden Barilla, and to "Assam rubber for West Africa."

WRITING to the New York *Nation* from Keneh, Upper Egypt, on March 17, Mr. W. H. Goodyear describes an important and most interesting discovery made by Mr. Petrie at Maydoom. Mr. Petrie has there unearthed "the oldest known Egyptian temple and the only Pyramid temple ever found." Apart from the "Temple of the Sphinx" at Ghizeh, this building is also "the only temple of the Old Empire so far known." It was buried under about forty feet of rubbish. It lies directly at the centre of the eastern base of the Pyramid, on the side facing which it has two round-topped obelisks. "Obelisks and temple chambers so far entered," says Mr. Goodyear, "have the plain undecorated style of the Old Empire, as shown by the Temple of the Sphinx, but hieratic inscriptions in black paint found within fix the name of Seneferoo as builder, and confirm the supposition to this effect hitherto based on the fact that tombs near the Pyramid contain his cartouche. Seneferoo is the king connecting the third and four dynasties, and variously placed in either. According to computations of Mariette and Brugsch, the antiquity will be about 4000 B.C., or earlier." On Tuesday, March 10, Mr. Petrie's workmen reached a platform which appeared to be a causeway terminating with two obelisks at the base of the Pyramid. "In the forenoon of Wednesday," continues Mr. Goodyear, "a workman came to say that an opening had been found under the platform on the side next the Pyramid. This proved to be the top of a doorway choked by detritus, through which Mr. Petrie crawled into an interior of three chambers and discovered the inscriptions mentioned. I had the pleasure of following him. Mr. Petrie thought the apartments had not been previously entered for about three thousand years—that is to say, that the rubbish fallen from the Pyramid had choked the entrance about three thousand years after construction. A friend who was with me noticed on the floor some dried wisps of papyrus, a plant now extinct in Egypt. The chambers thus far found are so filled that one cannot stand erect in them, and a door at the end of the third chamber is blocked by large stones. Over all lies an enormous mass of detritus, whose removal by Arab diggers is now in progress. I had the pleasure next day of carrying the news of Mr. Petrie's find to the gentlemen of the Egypt Exploration Fund at Beni-Hassan, and of witnessing their unaffected delight over it."

PRINCE ALBERT of Monaco has begun the publication of the results of his scientific voyages in his yacht; they are intended to comprise navigation, hydrography, oceanographic physics, and zoology. The zoological portion will be published under the editorship of M. J. Guerne, assisted by numerous specialists. The first part (published by Masson, Paris), on the marine Mollusca of the Azores, by M. Dautzenberg, with four large plates, two of them coloured, has already been issued.

THE "Year-book of Australia for 1891" contains an article upon scientific progress in Australia during 1890. It is compiled from information supplied by the various Australian scientific associations. The compiler cannot be charged with having formed an extravagant estimate of the value of the work done. It may not, he says, have been "so fruitful in important results as have similar labours in older countries; nevertheless, it has not been without interest or utility."

So far as is at present known, the first person who kept a record of the weather was Walter Merle. He did so for the years 1337 to 1344, and his manuscript on the original vellum still exists. Thanks to the courtesy of the officials of the Bodleian Library, Mr. G. J. Symons has had this manuscript photographed, and reproductions of the ten large photographs, with a full translation (the original is in contracted Latin), some particulars as to Merle, and a list of the subscribers, are to be given in a handsomely printed volume. Mr. Symons wishes to call attention to the fact that no one will be able to obtain a copy who does not apply for one before May 1. Except ten copies reserved for subscribers too distant to apply before tha date, not a single copy in excess of those subscribed for will be printed.

In our issue of the 9th instant, p. 548, we published a formula for wind velocities as observed with Robinson's cup anemometer, which is likely to be practically used by engineers as well as meteorologists. The formula, as quoted by the *American Meteorological Journal* for February, contains an erroneous interpolation of v. The correct formula is

$$\log V = 0{\cdot}509 + 0{\cdot}9012 \log v;$$

where V is the wind's velocity, v the velocity of the cups in miles per hour. It should be noted that the constants of the formula apply only to anemometers of the pattern specified, the cups of which are 4 inches in diameter, and the arms, each 6·72 inches, measured from the axis to the centres of the cups. Prof. Marvin says nothing as to the weight of this gear set in motion by the wind, which ought also to be a constant quantity.

The American Hydrographic Office reports, in the Pilot Chart of the North Atlantic Ocean for April, that the weather was very abnormal over the Atlantic during March, the usual conditions of wind and pressure having been entirely reversed for a large part of the month. Unusually low barometric pressure and cyclonic wind circulation have prevailed very generally to the southward of the Azores, and between the Azores and Bermuda, whilst anticyclones have been very frequent and persistent along the 50th parallel. The result has therefore been that the north-east trades have actually been reversed for some time, and the westerly winds that usually prevail along the steamer routes have been often replaced by persistent easterly winds. Fog has been reported in great quantities on the Grand Banks and the coast to the westward as far south as Hatteras.

In the Proceedings of the Royal Prussian Meteorological Institute, vol. i. No. 2 (Berlin, 1890), Dr. Sprung gives an account of an elaborate series of experiments to show the effect of difference of exposure upon the readings of thermometers. The experiments were made at Gross-Lichterfelde, near Berlin, by assistants of the Prussian Meteorological Office, observations being taken six times daily with various screens, and with the "sling" thermometer, from June 1886 to March 1887 inclusive, under very favourable conditions. The readings at each of the hours of observation are tabulated for two representative months, together with the state of the sky and of the wind. The principal results likely to be of interest to English observers are: (1) that exposure outside windows, both with and without screens, if properly protected from radiation, gives readings which agree well together; (2) the readings of the various screens usually employed, which are necessarily exposed to the sun, give results which do not agree so well together; (3) on sunny summer days, the mean temperature in the ordinary screens is about $0^{\circ}{\cdot}7$ higher than that obtained from the window exposure; (4) the range shown by the registering thermometers is considerably greater in the screens than that got from the window exposure; (5) the English (Stevenson) screen generally gives better results than the others, but the humidity observations in the evening are too high, as compared with those obtained by the window exposure and the French screen.

Mr. J. W. Fewkes calls attention in the *American Naturalist* to a peculiar gesture often used by the Zuñi and Navajo Indians. In indicating that a person or thing is far away, or where an event has happened or a person is at the time of speaking, these Indians, instead of turning the head that way or pointing with the finger, raise the head and project the lower jaw in the direction which they wish to indicate. Mr. Fewkes noticed that the same gesture was used several times for exactly the same purpose by a New England Indian with whom he was talking; and he now wishes to find out whether this method of suggesting distance or direction is characteristic of the American aborigines generally. "Those whom I have consulted," he says, "tell me that it is. If it is, we may well wonder why such an insignificant habit should be so tenacious in a tribe so long in contact with the whites, and so much affected by their civilization in much more important particulars, as the Passamaquoddies. It is conceivable that gestures like this, certainly spontaneous and in some respects involuntary, may furnish data of ethnological value."

A new illustrated volume intended for amateur astronomers, and written by Mr. Denning, of Bristol, will shortly be published by Messrs. Taylor and Francis, under the title of "Telescopic Work for Starlight Evenings." The book will include chapters on telescopes and observational matters, in addition to descriptions of the principal celestial objects.

A very useful little book, published by Messrs. Longmans, Green, and Co., and compiled and edited by the Rev. Isaac Warren, M.A., consists of a series of examination-papers in Euclid and trigonometry that have been proposed to students of the University of Dublin. There are also papers in plane trigonometry that have been set in the examinations for the National School teachers by the Board of National Education. In each case the answers are appended, and, in the latter, hints are inserted for their solution. To students of the University who are preparing for these examinations, it is needless to say that they will find the work most useful; while for others, the papers will afford a capital chance for testing their knowledge on the subjects in question.

A paper upon the chlorobromides of silicon is contributed by M. Besson to the current number of the *Comptes rendus*. There are three theoretically possible compounds of this nature derived from silicon tetrachloride, $SiCl_4$. They are $SiCl_3Br$, $SiCl_2Br_2$, and $SiClBr_3$ respectively. The two first of these were prepared some time ago by M. Friedel, by the action of bromine upon silicon chloroform, $SiHCl_3$, at 100° in sealed tubes. The first compound results from the substitution of bromine for hydrogen according to the equation $SiHCl_3 + 2Br = SiCl_3Br + HBr$. The second compound is then formed by the action of this hydrobromic acid upon the first compound, $SiCl_3Br + HBr = SiCl_2Br_2 + HCl$. M. Besson now describes how all three compounds, including the hitherto unisolated $SiClBr_3$, may be obtained by the action of hydrobromic acid gas upon silicon tetrachloride. Dry hydrobromic acid is without action at the ordinary temperature, but partial substitution of bromine for chlorine occurs at high temperatures, owing to the difference in the relative heats of formation of hydrochloric and hydrobromic acids, and the partial dissociation of the latter. The dry gas is passed, saturated with vapour of silicon tetrachloride, through a porcelain tube heated to redness. The product obtained in the receiver contains considerable quantities of unaltered tetrachloride, but, by repeating the process a few times, a product is obtained consisting largely of the first chlorobromide, $SiCl_3Br$. It is comparatively easy to separate, by fractional distillation, this compound, which boils at 80°, from the remaining tetrachloride, and the $SiCl_3Br$ thus obtained in a fairly pure state serves for the preparation of the second and third chlorobromides by passing its vapour, instead of the tetrachloride, together with hydrobromic acid through the hot porcelain tube. The second chlorobromide, $SiCl_2Br_2$, has been said to boil about 100°. M. Besson finds that his carefully purified specimen boils at 103°–105°. It was found impossible to separate the third compound, $SiClBr_3$, from this second one by fractional distillation, but by taking advantage of the fact that the second chlorobromide cannot be solidified at − 60°, while the third compound solidifies at − 39°, and afterwards distilling the solid obtained, the third compound, $SiClBr_3$, has been isolated as a liquid boiling at 126°–128°. This substance exhibits the property of superfusion in a very high degree; it may be cooled as low as − 50° without solidification ensuing, provided the liquid be maintained perfectly still. On agitation to even the slightest extent, however, it suddenly solidifies, the thermometer rising instantly to − 39°. All three chlorobromides combine directly with gaseous ammonia to form additive compounds, white amorphous solid bodies decomposed by water. In case of the first chlorobromide, $SiCl_3Br$, a similar compound has been obtained under pressure in a Cailletet tube with phosphoretted hydrogen, PH_3. The combination occurs at 0° under a pressure of 25 atmospheres, or at − 22° under 17 atmospheres, all the liquid being then transformed into a white solid, which persists when the pressure is removed, but which is again dissociated upon slightly warming the tube.

THE additions to the Zoological Society's Gardens during the past week include a Lesser Ourang-Outang (*Simia morio* ♂) from Sarawak, Borneo, presented by Commander Ernest Rason, R.N.; two Suricates (*Suricata tetradactyla*) from South Africa, presented by Mr. J. W. Munt; two Azara's Opossums (*Didelphys azaræ* ♂ ♀) from La Plata, presented by Mr. Edward C. Hawe; a Lion (*Felis leo* ♀), bred in Holland; a Nylghaie (*Boselaphus tragocamelus* ♂), bred in France, purchased; a Grey Parrot (*Psittacus erithacus*) from West Africa, deposited.

THE PRESENT METHODS OF TEACHING CHEMISTRY.[1]

IN their second Report, which was presented at the Newcastle-on-Tyne meeting, the Committee gave an account in some detail of the general lines which, in their opinion, an elementary course of instruction in physical science might most profitably follow. During the past year the Committee have been principally engaged in collecting and comparing the regulations, with respect to chemistry, which are issued by the more important of the examining bodies in the kingdom, in order to discover how far their requirements are in harmony with such a course of instruction as that suggested by the Committee. Since the information which has been collected is of general interest, the greater part of it is here printed. It consists of a brief outline of the noteworthy features in the regulations of the various Examination Boards, and, wherever it appeared necessary, of recent examination papers. The examinations about which information is now given are as follows:—

Oxford and Cambridge Schools Examination Board.
University of Cambridge Local Examinations.
University of Edinburgh Local Examinations.
University of Glasgow Local Examinations.
University of London Matriculation.
University of Durham Certificate for Proficiency in General Education.

[1] Third Report of the B.A. Committee, consisting of Prof. H. E. Armstrong, Prof. W. R. Dunstan (Secretary), Dr. J. H. Gladstone, Mr. A. G. Vernon Harcourt, Prof. H. McLeod, Prof. Meldola, Mr. Pattison Muir, Sir Henry E. Roscoe, Dr. W. J. Russell (Chairman), Mr. W. A. Shenstone, Prof. Smithells, and Mr. Stallard, appointed for the purpose of inquiring into and reporting upon the Present Methods of Teaching Chemistry. (Drawn up by Prof. Dunstan.) To which is appended a Paper, by Prof. Armstrong, on "Exercises in Elementary Experimental Science."

Victoria University Preliminary Examination.
College of Preceptors—Professional Preliminary Examination.
Science and Art Department Examination in Chemistry.
Intermediate Education Board for Ireland.
Civil Service of India.
India Forest Service.
Royal Military Academy, Woolwich.
Cadetships, Royal Military College, Sandhurst.
Engineer Students, H.M. Dockyards.

With respect to the regulations which relate to these examinations, the Committee consider it desirable to direct especial attention to the following points.

It is of great importance that natural science should be sufficently represented on the Board which issues the regulations and is responsible for the proper conduct of the examination. It is remarkable that, although chemistry is an important subject in the Oxford and Cambridge Schools Examination, no representative of this science is appointed by either University to act on the Examination Board, whilst Oxford does not appoint a representative of any one branch of natural science.

The Committee note with satisfaction that in these examinations, most of which are held to test proficiency in general education, chemistry is generally included, in addition to one or more branches of experimental physics, and that in many cases the examination is in part a practical one. An important exception to this statement is found in the case of the University of Durham, which, although it grants a certificate of proficiency in general education, does not include among the subjects of this examination either chemistry or any branch of experimental science. Science is represented only by elementary mechanics, and even this is an optional and not a compulsory subject.

As regards the status occupied by chemistry and experimental physics in public examinations, the position of these subjects is still frequently lower than that of the other principal subjects of examination, and much yet remains to be done to secure the adequate recognition of the educational value of natural science. Attention may here be drawn to the position assigned to physical science by the Intermediate Education Board for Ireland, upon whom devolves the examination of most of the Irish public schools. According to the regulations at present enforced by this Board, natural philosophy and chemistry appear as optional subjects, each having a relative value represented by 500 marks, the value of Greek and Latin being assessed at 1200 marks each. It is to be hoped that the Commissioners may, before long, see their way to introduce elementary physical science as a compulsory subject of these examinations, and to increase the marks assigned to it beyond the present number of 500, which is less than one-half of that awarded to Greek or Latin (1200).

Another very anomalous case is that of one of the Civil Service examinations, viz. the examination for engineer students in H.M. Dockyards. In this examination, "very elementary physics and chemistry" are included as a single subject, to which is allotted 100 marks out of a total number of 1950! In the profession for which this is an entrance examination, applicable to boys who are about to leave a public school, not only is the possession of a scientific habit of mind of the highest moment, but a considerable knowledge of physics and chemistry is indispensable.

The Committee are strongly of opinion that some attempt should be made to remedy a conspicuous deficiency in nearly all existing examinational regulations. It is virtually impossible to ascertain, in the course of a single short examination, especially when the number of candidates is large, whether sufficient time has been devoted to the study of the elements of physical science to make it of permanent advantage to the student; neither is it possible to determine whether the character of the instruction has been in every respect satisfactory. Periodical inspection of the teaching by properly qualified inspectors, such as is now practised to some extent by more than one Government department, would seem to constitute the best method of dealing with this defect, the reports of the inspectors, as well as the students' own record of work testified to by the teacher, being taken into account in awarding prizes, certificates, and grants, in addition to the results of an examination.

With respect to the schedules and examination papers, typical specimens of which are here printed, it will be seen that for the most part they do not aim at an educational training of the kind suggested in the Committee's last report. Although nearly all the examinations included are intended to maintain a high standard in general education, yet, as a rule, the schedule of work proposed and the questions set in the papers are more suitable for those who wish to make a special and detailed study of chemistry as a science. Insufficient attention is paid to problems, like those suggested in the Committee's last report, designed to develop the power of accurate observation and correct inference; few of the questions asked are adapted to test the mental power of students, which should have been strengthened and trained by the experimental study of physics and chemistry. The great majority of the questions asked involve an enumeration of the properties and modes of preparation of different chemical substances; but this by itself is a wholly unsatisfactory method of ascertaining whether a student has derived benefit from experimental work. The mere writing out by the student of methods of preparation of individual substances is no proof that he has learned chemistry. The Committee are of opinion that it is not advisable to ask young students to give purely formal definitions of chemical terms. A glance at the examination questions appended will show that definitions of such terms as *atomic weight*, *molecular weight*, *water of crystallization*, *acid*, *base*, *salt*, are often demanded. Such questions encourage many students to learn by rote certain forms of words without attempting to grasp the facts and generalizations which those words summarize. Moreover, as many, if not most, of the terms used in chemistry cannot be defined, the demand for definitions of these terms by examiners leads to a pernicious and unscientific way both of teaching and learning, by which an apparent accuracy in the use of phrases is substituted for a real acquaintance with facts and principles. Again, too much attention is often devoted to calculations which, while they furnish useful exercises, do not necessitate any special scientific knowledge. Another noteworthy feature of these examination schedules and papers is the very general exclusion of any reference to organic substances. There appears to be no reason, even in elementary examinations, why the questions should be exclusively confined to inorganic materials. Moreover, elementary organic chemistry can be made the basis of excellent training in scientific method, especially if the teaching does not follow the formal order or aim at the completeness which are usual in text-books, most of which are written for those who are studying chemistry as a special subject, and not chiefly for the sake of the educational benefit which may be derived from it. In general elementary teaching at any rate it is unnecessary even to make the conventional distinction between inorganic and organic chemistry.

The foregoing remarks apply not only to school examinations, but also to the various Civil Service examinations, where it is of the highest importance that candidates should have received a sound scientific training. Most of those selected will afterwards fill positions in which the scientific method of dealing with the various problems which will constantly be presented for solution cannot fail to be of the highest value.

It may perhaps be thought that a great deal of what has been said in criticism of the present examinational demands in physical science, might more properly have been urged against the teaching. But since the first report of this Committee was issued, in which attention was drawn to the defective character of much of the elementary teaching, it has been repeatedly represented by teachers in schools of every grade that the character of their instruction is necessarily governed by the requirements of examiners, and that if modifications were made by Examining Boards in the present regulations it would be possible at once to make the corresponding changes in the methods of teaching.

The obvious conclusion is that the necessary reforms can only be brought about by the active co-operation of examiners and teachers.

[Here follow, in the Report, a selection of examination papers.]

APPENDIX.

Exercises illustrative of an Elementary Course of Instruction in Experimental Science. By Prof. Armstrong.

The scheme put forward in the report presented last year by the Committee sufficed to indicate the kind of instruction likely to inculcate habits of observing correctly, of reasoning from observation, and of setting new questions and obtaining answers thereto by experiment and observation: habits which it is now generally admitted are of great consequence in the struggle for existence, and which cannot be acquired except through training in the methods of experimental science. Nevertheless, it has been felt that detailed directions how to proceed were necessary for the use of the less experienced teachers, and that even those who fully sympathize with the proposals already made would welcome the more complete display of the system. I have therefore obtained the permission of the Committee to append the following suggestions to their report, in amplification of certain parts of the scheme already published.

It is obviously impossible to sketch more than a small portion of a complete programme of instruction; the portion now offered is that appropriate to the earliest stage in which quantitative studies can be engaged in: its study can be commenced by children of fair intelligence when nine or ten years old. It is an essential feature of the scheme that it has reference to common things, the object being to lead children to engage in the rational study of the objects which are daily brought under their notice.

Time to be devoted to Experimental Studies and Mode of Teaching.—Frequently during the past year the question has been put to me, "How much time is to be devoted to such science teaching?" and complaint has been made of the difficulty of dealing with large classes of children, of keeping them employed, and of providing the requisite space and appliances.

The question as to time will ever continue to be put until the fundamental fallacy which hitherto has retarded the progress of experimental teaching in schools is discarded, viz. that sufficient training in a scientific subject can be imparted in the course of a term or two. This undoubtedly is the view entertained in the majority of schools—girls' schools in particular. It is well known, for example, that of the many hundred students who each year present themselves at the London University Matriculation Examination, the vast majority have had but a *few months' coaching* in chemistry, mechanics, or physics, although they have had lessons in arithmetic and like subjects during the *whole period* of their school career. It was long a superstition that to pass in chemistry all that was necessary was to have read some one of the small text-books, and a very large proportion of matriculants have doubtless had only such preparation. The fact is that our schools hitherto have been all but entirely in the hands of those who have had a purely classical or mathematical training, and who have gained their knowledge by reading; teachers thus trained cannot realize that the useful effect of science teaching is only attained when the instruction is carried out on entirely different lines; they cannot realize that *accurate experimenting* is the essential feature in the system; that knowledge gained by mere reading is and can be of little use, as in acquiring it the mental faculties which it is desired to exercise never become trained. It must be recognized by all who have charge of schools that, in order to secure the due development of those faculties which science teaching alone can affect, the instruction must be imparted *from the very beginning and during the entire period of the school career.*

If this be done, many of the difficulties hitherto encountered may disappear. Probably it will be found advantageous, at least in the earlier stages, rather than disadvantageous, to devote but a short time during any one lesson to actual experimental work. There is no doubt that far too much is usually attempted; that too many facts are brought under the student's notice in the course of the lesson, the result being a blurred mental picture destitute of sharp outlines. After considerable experience I am satisfied that it is difficult to proceed too gradually—it may almost be said too slowly.

The following two sets of instructions are given by way of illustration; it is not pretended that they are complete, nor is it suggested that the exercises should be worked through exactly in the order in which they are stated, or completed by all pupils; the teacher must determine which are suitable for the particular set under instruction.

Studies of Water and Common Liquids.

1. Make every effort to elicit from the pupils by question and answer all that *they* have noticed with regard to water. Induce them to take advantage of any opportunities the neighbourhood affords of observing water and its effects. Let them ascertain the area covered by the school-house roof and the amount of water which falls on it when it rains; institute systematic observations of rainfall, and embody the data in arithmetical exercises. Call attention to the different yearly rainfall of different parts of the country, and point out the influence of hills

and mountains: let outline maps be coloured, so as to indicate the different rainfall of different districts.

2. Call attention to the geographical distribution of water, &c.; also to the work which it does in Nature (cf. Geikie's "Physical Geography," Huxley's "Physiography," &c.), illustrating this part of the subject, especially at an inland school, by lantern photographic slides of ships, sea-coasts, Niagara Falls, &c.

3. Call attention to the disappearance of water, *i.e.* the drying up of rain, the drying of clothes, &c., and lead the pupils to notice that this takes place most quickly in hot weather and in warm places; then let them pour water into a clock glass placed either over a saucepan in which water is boiled by a gas-burner (or petroleum or spirit-lamp, if gas be not available), or in a small gas cooking-stove; they will see that the water evaporates, leaving a certain amount of *residue.* [At this stage experiment on the extent to which water evaporates out of doors and indoors under different conditions and at different times of the year by exposing water in weighed glass (crystallizing) dishes about 4 inches in diameter, and weighing at intervals. Also call attention to the fact that in certain states of the weather things become damp, and that moisture is sometimes deposited on the windows in cold weather; then let the condensation be noted of a liquid indistinguishable from water, which occurs, for instance, when a closed flask filled with water and ice is exposed in a room. Let some seaweed inclosed in a muslin bag be hung up out of doors where it cannot be wetted by rain, and have it weighed daily. At the same time have the temperature, direction of the wind, and character of the weather noted. Later on have the dry and wet bulb thermometer read daily. Have the changes in weight of the seaweed and the dry and wet bulb thermometer readings represented by curves. Lead the pupils to contrast and discuss the results.] The experiment should then be repeated with a known quantity of water and a weighed glass dish, so as to determine the amount of residue; the character of the residue should be noticed. Discuss the origin of the water, and point out whence the residual matter *may* have come. Next, if a well water was taken, let a local river or pond water be examined in a similar way, then rain water, and, if possible, sea water.

4. Let an ordinary 2-ounce narrow-mouth stoppered bottle, having a nick filed down the stopper, be filled with each of the waters and weighed, and let the operation be repeated several times with each water, so that the *experimental error* may be ascertained; it will be found that the different waters, sea-water excepted, have practically the same *density.* At this stage arithmetical exercises relating to the weight of known bulks, and *vice versâ*, of water, the quantities of dissolved solids present in given bulks of various waters, &c., may advantageously be set; these should be solved practically by actual measurement in as many cases as possible.

5. Next ask, "But what becomes of the water when driven off by heat?" If it have not been noticed that water collects (condenses) on some object near at hand, let a cold object be held over boiling water, then let water be boiled in a glass flask connected with a glass condenser. Afterwards have water distilled in larger quantity from a tin (2-gallon) can. The density of the distilled water should then be determined, and its behaviour on evaporation. Data would thus be accumulated, rendering it possible to explain the drying up of water under ordinary conditions, the origin of rain, the differences between waters from various sources, and the method of separating water from the associated foreign matters will have been brought home to the minds of the pupils.

6. As the water is heated to boiling in the flask, if attention be paid to all that occurs, it will probably be noticed that bubbles separate from the water, rising up through it and escaping at the surface; frequently the bubbles adhere for a time to the flask. Let the experiment be repeated in such a way that the something which escapes from the water can be collected and measured. For example, a 2-gallon tin can having been filled with water, insert into the neck a rubber cork through which a bent *delivery tube* is passed; place the can over a burner, introduce the upturned end of the delivery tube into a basin of water, and insert a small jar over it. Heat to boiling. An air-like substance will gradually be driven off, but it will be noticed that after the water has been boiling for some time it ceases to give off gas; let the amount of gas collected be measured, and have the experiment repeated several times. As the gas does not continue to come off on boiling the water, it would seem that it is not a part of the water—there is so little of it, but merely something dissolved in the water; it is like air, and the water had been in contact with air—may it not be air? Let the boiled water be poured out into a galvanized iron pan, and after it has been exposed to the air for several hours let it be again boiled. The water which previously no longer gave off gas, will now yield probably as much as before. It will thus be discovered that water dissolves air as well as the solid matters with which it comes into contact, and the presence of air in water will be recognized. This knowledge will be of value later on when the existence of animals and plants under water comes to be considered.

7. Attention having thus been directed to the solvent action of water, let special experiments be made on its solvent action, using salt, sugar, suet, washing soda, alum, tea and coffee, field or garden soil, clay, chalk or limestone, gypsum, &c.; known quantities of the filtered solutions should be evaporated to dryness, and the residues dried (conveniently in a small gas cooking-oven) and weighed. Opportunity will be afforded to call attention to the separation of some of the substances from solution in definite shapes, *i.e.* crystals; show these under the microscope as well as home-made cardboard models of some of them. Let larger crystals of alum be grown, and call attention to sugar crystals. Natural crystals of calcite, gypsum, pyrites, quartz, fluorspar, &c., would be appropriately shown at this stage. The question may then be put, Does the water which passes through the body dissolve anything? By evaporating urine and determining the amount of dried residue it would be found that a good deal of matter passes away from the body in solution.

8. Having directed attention to the different behaviour of different waters with soap, let determinations be made of the amount of alcoholic soap solution required to produce a lather in distilled and other waters. Directions for performing the soap test are easily obtained from a book on water analysis, and the operation is one of extreme simplicity.

9. Other liquids should now be compared with water, such as methylated spirit, turpentine, petroleum, salad oil, vinegar, and perhaps the common acids—muriatic, nitric, and sulphuric—also. The noticeable differences between these and water—appearance, odour, taste in dilute solution—having been registered, their relative densities should be determined; also their behaviour towards water and towards each other, their behaviour when heated on the water-bath in comparison with that of water, their behaviour when burnt, their behaviour when boiled together with water in a flask attached to a condenser, and their solvent action in comparison with that of water should be ascertained.

10. Having given an account of the origin, &c., of the various liquids examined, and having alluded to the presence of alcohol in beer and wine, demonstrate the separation of alcohol from beer by distillation; then describe the production of alcohol by fermentation and carry out the experiment, first with sugar and yeast, then with malt; explain that yeast is an *organism*, and show it under the microscope and lantern photographs of it. Make several mixtures of alcohol and water, and let the relative density of each be determined; then exhibit a table of relative density of spirit solutions of various strengths. Let a measured amount of beer be distilled, have the distillate made up with distilled water to the bulk of the beer taken, and let its density be determined; reference being then made to the table of relative densities, the strength of the alcoholic distillate would be ascertained, and thus the amount of alcohol in beer would be determined.

11. The behaviour of water when heated may now be further studied: attention having been called to the thermometer as an instrument which enables us to judge how hot or cold it is, water should be heated and the gradual rise of the mercury column noted, and the steady position which it assumes when the water boils. In the same way boiling water should be allowed to cool and the fall of the mercury column noted; further cooling should then be effected by means of ice, so that opportunity might be given for the stationary position to be observed which the column eventually takes up and maintains so long as unmelted ice is present. Having specially directed attention to these "fixed points," describe the construction of the thermometer. Next let a quantity of water be distilled from a flask or can having a thermometer in its neck, and let the steady position of the mercury throughout the distillation be observed. Also let water be frozen by means of a mixture of ice and salt; the "temperature" of the freezing mixture having been ascertained, the thermo-

meter bulb should be inserted into the water which is being frozen (in a test tube), so that the ice may form around its bulb: the temperature should be noted during freezing and also during the subsequent melting of the ice. Do this out of contact with the refrigerating mixture.

12. Let the relative density of ice be determined, *i.e.* after showing that although "lighter" than water ice is "heavier," than turps, let a cylinder partly filled with turpentine be counterpoised, and after the temperature has been lowered by immersing the cylinder in ice water, note the position of the turps, then introduce a few pieces of dried ice, note the rise of the turpentine—thereby determining the volume of the ice—and subsequently weigh in order to ascertain the weight of ice introduced. Have the result thus obtained checked by subsequent observation of the bulk of water which results when the ice melts. The expansion of water on freezing having thus been observed, the bursting of pipes in winter may be explained; and attention may also be directed to the destructive effects on rocks produced by the freezing of water; the extent to which ice floats may be discussed, and arithmetical problems may be set which will lead the pupils to realize the extent to which the volume changes when water changes its state.

13. Let the relative density of water and the other liquids be determined at 0° C. and at a higher temperature—that at 0° by weighing, and that at the higher temperature by observing the expansion of the liquids in bulbs with graduated stems of known capacity; let curves be constructed showing the relation between temperature and volume.

14. Let spirit, turpentine, petroleum, and vinegar be distilled; the temperature during distillation being observed, the gradual rise especially in the case of spirits and petroleum will be noted. Fractionally distil several times some quantity of spirit and of petroleum; let the relative density of each separate fraction be determined, and let the water separated from the spirit be characterized by freezing it and determining the melting-point of the ice and the boiling-point of the liquid which results when the ice melts.

15. Having directed attention to the fact that heat is "used up" in melting ice and boiling water, let determinations be made of the amounts, following Worthington's "Practical Physics," for example.

Studies of Chalk and other Common Solids.

1. Call attention to the use made of lime in building and its production from chalk or limestone; slake a lump of lime; exhibit specimens and pictures of chalk cliffs or quarries and lime-kilns—if not to be seen in the district. Point out on a geological map those parts of the country in which chalk occurs, and those where limestone is met with. Explain how chalk is supposed to have been formed, and show pictures of the forms which are present in it, and, if possible, microscopic slides. Explain that whitening, which is purchasable everywhere, is but lævigated chalk; describe its preparation, and let chalk and sand be separated by lævigation.

2. Let the conversion of chalk into lime be studied quantitatively. For this purpose three to five grams of dried whitening should be weighed out in a small platinum dish and heated to full redness in the *covered* dish during an hour over a Fletcher Argand Bunsen burner: the dish is then removed from the burner, and after about ten minutes, when cold, is weighed; it is then again heated, say for half an hour, &c.; usually there is no further loss. Several experiments should be made in this way, so that it may be noted that practically the same percentage of loss is incurred and the same amount of lime obtained in each case; and similar experiments should be made with chalks from different localities (Note A).

3. At the conclusion of each experiment, the residue should be carefully moistened with distilled water and the effect noticed; usually the lime slakes, becoming hot—some limes, however, slake very slowly, and the heating is imperceptible. The excess of water should then be driven off by heating in a water-oven until the weight no longer diminishes.

4. In comparing the solvent action of the various liquids previously studied, it will probably have been noticed that chalk is dissolved by acids—for example, vinegar or muriatic acid—with effervescence; such an acid may therefore be used, if necessary, in cleaning out the dish at the conclusion of the experiment if any of the solid adhere to it. Then, having made it clear that the effervescence is due to the escape of an air-like substance or gas, which is conveniently termed *chalk-gas*, let the amount of gas which is given off when the chalk is dissolved in acid be determined. For this purpose, the simple apparatus shown in Fig. 1 may conveniently be used. From 1·5 to 2 grams of the chalk is weighed out on a small square of tissue paper, which is then folded up at the sides and dropped into the bottle A, from which the tube B has been removed; a little water is then added (about 5 cubic centimetres), and the chalk is shaken out of the paper; about 5 cubic centimetres of nitric acid is now poured into the tube B, which is then carefully replaced in the bottle A. The cork having been inserted, connection is established by means of the flexible tube C with the bottle D. The side tube E having been so adjusted that the end *e* is on a level with the water in the bottle D, the measuring cylinder H is so placed that any water which runs from *e* may be collected in it, and the bottle A is then carefully tilted so that the acid may gradually run out of the tube B into A; gas is at once given off and expels water from D. As the water sinks in D, the side tube E is lowered so that its orifice remains about on a level with the water in D. The water is then measured. Several experiments should be made, and the results should be compared by calculating the volume of gas which would have been obtained, supposing, say, 100 grams of the chalk had been dissolved.

5. In this way it is ascertained that *chalk-stuff* is *characterized* by (1) yielding between 56 and 57 per cent. of lime, which in-

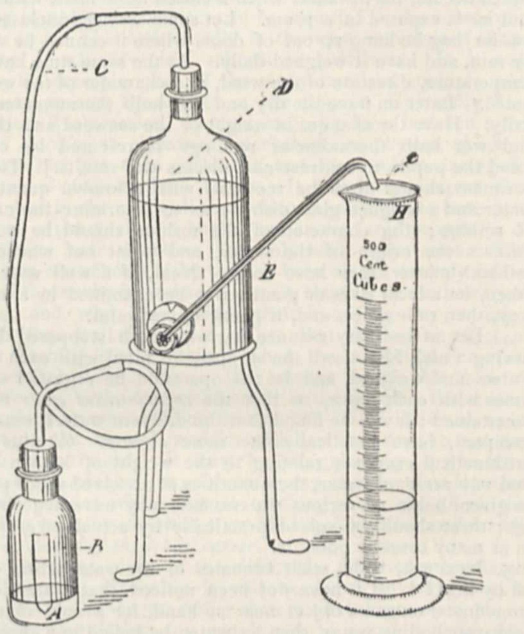


FIG. 1.

creases by about 33 per cent. when slaked; and (2) by yielding about 22,000 cubic centimetres of chalk-gas per 100 grams when dissolved in acid.

6. Comparing lime with chalk, it is found that if the chalk be thoroughly burnt no gas is evolved on dissolving the recently slaked lime in acid; this result serves at least to suggest that the gas which is given off when chalk is dissolved in acid is perhaps expelled during the conversion of chalk into lime. The loss in weight which occurs is therefore determined, and when it is ascertained that it is very nearly the same as when chalk is burnt, no room is left for doubt that the same substance is dispelled by heating and by dissolving the chalk in acid. The experiment is very easily carried out in a small bottle or conical flask provided with a tube to contain acid, and closed by a cork through which pass a narrow tube bent at a right angle and a small drying tube full of cotton-wool. The chalk is weighed out on thin paper and dropped into the flask, a little water is poured on to it, and the acid tube is then introduced, after which the cork is inserted. The bent tube is closed by a small stopper. On tilting the flask, acid escapes and attacks the chalk; the spray is prevented from escaping by the cotton-wool. When the action is at an end, air is sucked in through the narrow bent tube to displace the chalk-gas, and finally the

loss in weight is determined. Such an apparatus gives admirable results.

7. Marble may then be examined in a similar way; as it is found to behave both on heating and when dissolved in acid much as chalk does, it may be presumed to consist of chalk-stuff. Next, limestones should be taken; the result obtained with them may be lower owing to their containing clay, &c.; but this is to a large extent rendered evident by insoluble matter left on treating with acid. Let the percentage of chalk-stuff in the limestones be calculated from the results which they afford, assuming the results obtained with chalk to be practically those afforded by pure chalk-stuff. Lastly, direct attention to the occurrence of crystals (calcite) in limestone rocks, to stalactites, &c.; show specimens, and have them examined: the results will show that they also consist of chalk-stuff.

8. Having pointed out that chalk consists of shells, &c., of sea-animals, coral and shells of various kinds—oyster, cockle, limpet—should be given for examination; all these will be found to give results from which it may be inferred that for the most part they consist of chalk-stuff. Egg-shell and lobster or crab-shell in like manner will be found to yield lime when burnt, and to behave much as chalk does towards acid, but the presence of a certain amount of "animal" matter will be evidenced by the blackening on heating, and the insolubility of a certain proportion in acid.

9. Ordinary bone, gypsum, clay, and rocks other than chalk or limestone rocks are next given for study, in order that it may be discovered that the behaviour of chalk-stuff is peculiar and characteristic, and that there are many varieties of natural solids. Rough estimates of the amount of chalk in soil may be made by determining the amount of chalk-gas evolved on treating the soil with acid.

10. In a hard-water district the residue from the water will probably look more or less like chalk; its behaviour when treated with acid and when strongly heated should therefore be determined, and local boiler or kettle scale should then be studied as chalk was previously.

11. In this manner a large number of data will be accumulated which render it possible to discuss the origin of chalk; to explain the presence of chalk-stuff in water; and its withdrawal from water by animals; &c.

The study of chalk in the manner indicated would make it possible for the student (1) to comprehend the principle of the method followed by chemists in characterizing substances whereby they are led to discover distinct forms or species; (2) to realize not only that there are *compounds*, but also that such substances have a fixed composition; and (3) the entire difference in properties between a compound and its constituents would have been brought out most clearly by comparison of chalk-stuff with its constituents—lime and chalk-gas. The chalk studies, in fact, should serve to incite the student's curiosity, and should lead to further inquiries being undertaken as to the composition of other substances and the characters of their constituents, and as to the nature of other changes; and with regard to the method of undertaking inquiries into the composition of other substances, the important results obtained in the case of chalk by studying the *changes* which it undergoes would serve to illustrate the importance of studying change as a means of determining composition.

It cannot be denied that only well-informed, thoughtfu teachers could give useful instruction in accordance with the foregoing schemes; but this is scarcely an objection. The amount of special training required to carry out the experimental portion would not, however, be great; and there is no reason why such instruction should not be given in schools where there is no special science teacher engaged—although the services of such a teacher would undoubtedly be necessary if instruction in accordance with the more complete scheme embodied in the report presented last year by the Committee were carried out in its entirety.

The suggestion that probably it will be found advantageous, at least in the earlier stages, rather than disadvantageous, to devote but a short time during any one lesson to actual experimental work would be realized in practice if the experimental science lesson were associated with the measurement or practical arithmetic and drawing lessons; and it is difficult to imagine that this is not possible. Suppose a set of twenty-four pupils to be at the disposal of a teacher during an entire morning or afternoon in a room of sufficient size, properly appointed, and that they are set to work to carry out the experiments with chalk, described above. Several—say six—might be told off to weigh out in platinum dishes the necessary quantities of whitening, and having then placed the dishes on Fletcher burners or in a muffle, they would return to their places; at the end of an hour they would remove the dishes, and, after leaving them during ten minutes to cool, would weigh them. To determine whether any change took place on further heating, they would re-heat the dishes during, say, half an hour, at the expiration of which time they would, as soon as the dishes were cool, weigh them again. As soon as the first set of six had weighed out the chalk, a second set of six might be set to work in a precisely similar way if the necessary apparatus were available, or if not at some other exercise involving the use of the balance.

The nature of the experiments which each set were engaged in performing should be made known to the whole class, and all the data should be written up on a blackboard. Each pupil should write out an account of the experiments and of the results; opportunity would thus be given to compare the results of the six or twelve separate experiments. At the next lesson the two remaining sets of the class would carry out the same experiments. Each pupil would thus have the advantage of performing one or other of the experiments, and of knowing what results had been obtained by a number of fellow-students. If necessary, two pupils might be set to perform one experiment, care being taken that they took equal parts in it; and thus the whole class of twenty-four might complete the experiment or experiments in a lesson.

Those of the class who at any time were not actually engaged in carrying out the experiment might be occupied in other ways, *e.g.* in measuring distances, in drawing figures of stated dimensions, &c., in determining areas, in determining relative densities, in working out arithmetical problems, or in writing out notes and answers to questions. It would not be difficult as the class progressed to devise an infinite number of problems and exercises, the data for which were derived from experiments performed by the class.

If only one such lesson were given per week, a single teacher and an assistant might deal with 240 pupils, or with half that number if each class had two lessons per week—a much better course; and, working on a similar plan, much useful work might be done even in the course of two hours.

With regard to the appointments for such work, the school-room should be provided with simple working benches in addition to the ordinary desks and forms. A narrow table might be placed, preferably across one end of the room, on a raised platform, at which the teacher could sit and on which the balances could be placed; the teacher would then be able to supervise the weighing, and secure that due care were taken of the balances. A narrow bench (of deal, into which paraffin had been "ironed," so as to waterproof it) might be fixed against and along the wall at either side of the room. This should be fitted with simple cupboards and drawers for apparatus, and with gas taps if possible; and at a suitable distance from the wall and above the table there should be a bar carried by brackets affixed to the wall, from which various apparatus, small scales, &c., could be suspended. A simple draught arrangement should and might easily be fitted at each working place, so that no unpleasant or noxious fumes need escape into the room. At the other end of the room it would be desirable to have a demonstration table, and behind this, against the wall, a draft closet at one end of a bench at the other end of which was a capacious sink. It would be well also to have a sink within the closet, which could be made use of, for instance, in washing out a sulpuretted hydrogen apparatus. A muffle furnace at the side of the ordinary stove would be a most valuable adjunct.

The cost of carrying out experiments such as have been suggested remains to be considered.

The chief item is undoubtedly the balance. Useful work may be done at a very early stage of the measurement lessons with scales costing five or six shillings, as suggested by Prof. Worthington, but their use for quantitative chemical work, such as is comprehended in the foregoing scheme is entirely to be deprecated. The acquisition of the habit of weighing carefully and exactly is in itself a discipline of the utmost value, to which every boy and girl should be subjected. It is all-important, therefore, that a fairly good balance should be used, and that the utmost care in its use should be enjoined. When not in use the balance should be covered over with a cardboard box. Becker's No. 51 (Fig. 2) and No. 67 balances, to be had from Townson and Mercer, the English agents, are to be strongly recommended, the former being probably the more suitable, as the pans are carried by "bowed" wires, giving more room for manipulation, when, as in determining relative densities by the hydrostatic method, a bridge to carry a glassful of water is placed across the scale-pan. No. 51 costs £1 17*s*. 6*d*.; No. 67, £2 1*s*. A suitable set of weights (No. 31), from 500 grams downwards to centigrams, costs 18*s*. 4*d*. Even if six balances were provided—and such a number would suffice for a large class—the cost would be but £18.

A convenient size of platinum dish to use is one about ¾ inch deep and 2 inches wide, weighing, with a light cover, about 20 grams. At a normal price of platinum, such a dish would cost about 25*s*., so that a considerable number might be provided for an outlay of £10. Such dishes not only last a long time when properly used, but are of value when damaged (Note A).

A water oven for drying would cost about £1; one of Fletcher's small air ovens for drying costs 17*s*. 6*d*.

Fletcher's Argand Bunsen burners, with tripod, are to be recommended as superior to the ordinary burners for school work. The smaller size costs 2*s*.; the larger, 3*s*. Suitable

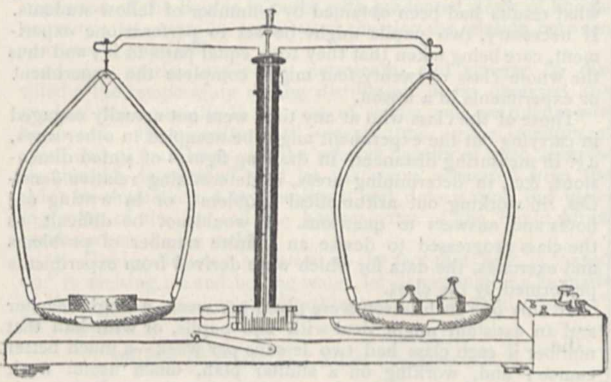


Fig. 2.

black rubber tubing for use with these burners, ⅜ inch in diameter, costs about 9*d*. per foot. A pair of iron crucible tongs costs 1*s*.

The apparatus for measuring the gas evolved on dissolving chalk in acid would cost about 7*s*., including a 500 cubic centimetre measuring cylinder.

Glass basins about 3 inches in diameter cost 4*d*. each; clock glasses, 6 inches in diameter, 5*s*. per dozen.

50 c.c. burettes cost 3*s*. 6*d*. each.

It is unnecessary to refer to the cost of the few remaining articles required for the suggested experiments, as they are well known. An expenditure of £50 would certainly cover the cost of apparatus required by a class of, say, twenty-four, and which would suffice for the use of several such classes.

Note A.—The unfortunate rise in the price of platinum, which makes the purchase of any number of platinum vessels for school use out of the question, has led me to make a number of experiments in the hope of substituting silver; but, as was to be expected, this has proved to be impossible. I find, however, that porcelain may be used, provided that the heating be effected in a muffle furnace. Small thin hemispherical porcelain capsules may be obtained from the dealers, about the size of the platinum dishes specified, which are more suitable than porcelain crucibles for the experiment. Such dishes may also be used in studying the effect of heat on organic substances, the char being burnt in the muffle furnace.

SCIENTIFIC SERIALS.

American Journal of Science, April.—On allotropic silver, by M. Carey Lea. This paper is in continuation of one contained in the March number of the *Journal*, in which the gold-coloured forms of allotropic silver were examined. The subject now considered is the relation existing between the allotropic forms of silver taken generally and silver as it exists in its compounds, and more especially in the silver haloids. The investigation leads to the conclusion that silver may exist in three forms:—(1) Allotropic silver, which is protean in its nature, may be soluble or insoluble in water, and have almost any colour; but in all its insoluble varieties always exhibits plasticity. It is chemically active. (2) The intermediate form, which may be yellow or green, but is never plastic, and is almost as indifferent chemically as white silver. (3) Ordinary silver. Allotropic silver is affected by all forms of energy, and the effects are strikingly analogous to those produced on silver haloids by the same agencies. It is, therefore, concluded that in the silver haloids silver may exist in the allotropic form.—The phenomena of rifting in granite, by Ralph S. Tarr.—The red-rock sandstone of Marion County, Iowa, by Charles R. Keyes.—The volumetric composition of water, by Edward W. Morley (continued from the March number of the *Journal*). The hydrogen used in the investigation was obtained by the electrolysis of dilute sulphuric acid. By this means it has been found possible to get hydrogen containing less than one-hundreth of a cubic centimetre of nitrogen in two litres of hydrogen, and containing no other impurity in amount large enough to be detected. An apparatus for the measurement of gases has been constructed, in which the mean error of measurement of the volume of hydrogen and oxygen used in the experiments has been less than one part in fifty thousand. With this, twenty experiments have been made, which give a maximum value for the composition of water 2·00047, a minimum value of 2·00005, and a mean value 2·00023. The composition of water may, therefore, be taken as 2·0002 volumes of hydrogen to one volume of oxygen.—On certain points in the estimation of barium as the sulphate, by F. W. Mar. Some experiments made by the author indicate that hydrochloric acid may be introduced freely and without detriment to quantitative exactness, in the precipitation of barium in the form of sulphate from pure solutions. Up to a determined point, the amount of hydrochloric acid employed accelerates the precipitation. The quantity of alkaline salts present is shown to have no very marked influence on the time of formation of the precipitate.—On halotrichite, or feather alum, from Pitkin County, Colorado, by E. H. S. Bailey. An analysis of the mineral shows that it is essentially a sulphate of alumina and ferrous oxide, with a part of the former replaced by ferric oxide, and a part of the ferrous oxide replaced by magnesia.—On a new serpent from Iowa, by R. Ellsworth Call.—On crystallized azurite from Arizona, by O. C. Farrington.—On the occurrence of xenotime as an accessory element in rocks, by Orville A. Derby.—On the magnetite ore districts of Jacupiranga and Ipanema, Sao Paulo, Brazil, by Orville A. Derby.—On pink grossularite from Mexico, by C. F. de Landero.—Restoration of *Triceratops*, by O. C. Marsh.—Development of the Brachiopods, Part 1, introduction, by Dr. Charles E. Beecher.

SOCIETIES AND ACADEMIES.

London.

Geological Society, April 8.—Dr. W. T. Blanford, F.R.S., Vice-President, in the chair.—The following communications were read:—The Cross Fell inlier, by Prof. H. A. Nicholson and J. E. Marr. The tract of Lower Palæozoic rocks lying between the Carboniferous rocks of the Cross Fell range and the new red sandstone of the Eden Valley is about sixteen miles in length, and little more than a mile in average breadth; the inlier extends in a general north-north-west and south-south-east direction, and the normal strike of the rocks is about north-west and south-east. The tract is divided along its entire length by a fault, which separates the Skiddaw slates (with the Ellergill beds of one of the authors, and the Millburn series of Mr. Goodchild) from higher beds on the west. A detailed classification of the Skiddaw slates is not attempted, but the authors describe the succession of the rocks in the faulted blocks of the western portion. Their classification is as follows:—

Coniston grits = Ludlow.
Coniston flags (lower portion) = Wenlock.
Stockdale shales = Llandovery-Tarannon.
Ashgill shales
Staurocephalus limestone
Dufton shales and Keisley limestone
Corona beds
Rhyolitic group
} = Bala.

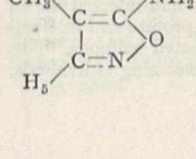
A brief comparison of these rocks with those of other regions is made by the authors. Two appendices are added : one, by Mr. Alfred Harker, contains petrographical notices of certain sedimentary and volcanic rocks in the Skiddaw slates, of the volcanic rocks of the Eycott and Rhyolitic groups, and of the principal varieties of intrusive rocks; the second, by Mr. A. H. Foord, contains a description of some Cephalopods from the rocks of the inlier. The reading of this paper was followed by a discussion, in which Prof. Boyd Dawkins, Dr. Hicks, Mr. Rutley, Mr. Hudleston, the Chairman, and Mr. Marr took part.—On the igneous rocks of the south of the Isle of Man, by Bernard Hobson. Omitting the Foxdale granite, the oldest igneous rocks of the island appear to be the diabase dykes of Langness, &c., intrusive in Lower Silurian slates. The Crosby microgranite dyke is also intrusive in these beds, and though its age is difficult to fix, it is probably newer than the Foxdale granite, which appears to be of post-Lower Silurian and pre-Carboniferous age. Next come the volcanic rocks of Lower Carboniferous age—an augite-porphyrite series consisting of tuff, breccia, agglomerate, bedded lava, and intrusive masses exposed in a narrow strip extending from Poolvash to Scarlet Point. A vent seems to have been opened during or after the deposition of the Poolvash limestone, from which fine volcanic ashes were ejected to form marine tuff. At intervals between the eruptions the Poolvash marble was deposited, and became interstratified with the tuff. The vent then probably became plugged up, and, a violent explosion following, supplied material for the agglomerate overlying the tuff. Lava then welled forth, and finally the volcano became extinct, and the intrusive mass of the Stack, regarded by the author as a volcanic neck, was exposed by denudation. It was probably at the close of volcanic activity that a melaphyre dyke was formed, resembling the porphyritic olivine-basalt of the Lion's Haunch, Edinburgh. At Poortown an intrusive mass occurs, provisionally termed augite-picrite-porphyrite, and considered by Mr. J. G. Cumming to be of post-Carboniferous age. Numerous dykes of ophitic olivine-dolerite occur between Bay-ny-Carrickey and Castletown Bay, at Langness, &c. They are post-Lower Carboniferous, and possibly of early Tertiary age. Full details with regard to the development and the macroscopic and microscopic characters of the various igneous rocks are supplied by the author, who acknowledges his indebtedness to Prof. Boyd Dawkins for the use of his geological map and notes. Prof. Boyd Dawkins said that the paper was the first instalment of the results of the geological mapping on the six-inch scale of the Isle of Man, which he had been carrying on for several years, and in which he had been assisted by the author. The igneous rocks of the island presented points of considerable difficulty. The author had, in his opinion, made a valuable addition to our knowledge. Replying to a question put by Mr. Rutley, Mr. Hobson explained that the igneous rocks at Scarlet Point belong to two distinct periods, the augite-porphyrites being of Lower Carboniferous age, while the olivine-dolerite dykes are certainly post-Lower Carboniferous, and perhaps of early Tertiary age.

Royal Meteorological Society, April 15.—Mr. A. Brewin, Vice-President, in the chair.—The following papers were read:—Some remarkable features in the winter of 1890–91, by Mr. F. J. Brodie. The author points out the peculiarities or special features of interest in the weather which prevailed over the British Isles during the past winter. In addition to the prolonged frost, which lasted from the close of November to about January 22, he finds that the barometric pressure for the whole winter was about a quarter of an inch above the average; and that, when the wind was not absolutely calm, there was an undue prevalence of breezes from some cold quarter. The percentage of winds from the southward did not amount to one-half of the average. The number of foggy days in London was no less than twice the average. The rainfall over the greater part of the British Isles was less than half the average. The author says that almost every element in the weather has been influenced to an abnormal degree by the remarkable prevalence of high barometrical pressure; and if we were called upon to define the season 1890–91, we should have little hesitation in giving it the name of the "anticyclonic" winter.—The rainfall of February 1891, by Mr. H. S. Wallis. This was one of the driest months upon record, the mean rainfall over England, excluding the Lake District, being only 0·066 inch, or about one-fortieth of the average.—On the variations of the rainfall at Cherra Poonjee, in the Khasi Hills, Assam, by Mr. H. F. Blanford, F.R.S. Cherra Poonjee has long been notorious as having a heavier rainfall than any other known place on the globe, the mean annual fall being frequently given as about 600 inches. Mr. Blanford has made a critical examination of the various records of rainfall kept at this place, and has come to the conclusion that the above amount is too high, and that the average annual rainfall is probably only a little over 500 inches.

Mineralogical Society, April 14.—Mr. R. H. Scott, President, in the chair.—The following papers were read:—On the occurrence of an aluminous serpentine (pseud-ophyte), with flint-like appearance, near Kynance Cove, by Howard Fox.—Note on the occurrence of melanterite in the Upper Eocene strata of the Thames basin, by the Rev. A. Irving.—Notes on various American minerals, including Bastnäsite, oligoclase, quartz, and sapphire, by G. F. Kunz.—On the devitrification of cracked and brecciated obsidian, by Prof. Grenville A. J. Cole.—Notes on crystallites, by F. Rutley.—On a method of observing the absorption spectra of minerals with the aid of the microscope, by Allan Dick.

PARIS.

Academy of Sciences, April 13.—M. Duchartre in the chair.—On the algebraical integration of differential equations, by M. H. Poincaré.—Description of an open manometer 300 metres high established at the Eiffel Tower, by M. L. Cailletet. The construction of the Eiffel Tower offered exceptional advantages for the installation of a large open manometer, and M. Cailletet appears to have carried out his design with great success. The length of the manometer is 300 metres, hence the pressure which it is possible to attain with the tube filled with mercury is 400 atmospheres. A glass tube would not sustain such an enormous pressure, so a steel tube 4·5 mm. in diameter has been employed. Specially devised stop-cocks are fitted on the tube at intervals of 3 metres. Each of these is connected with a vertical tube of glass about 3 metres high. When any pressure is required the stop-cock at the proper height is opened, and the hydraulic pump set working until the mercury appears in the glass tube that has been put in communication with the steel one. A manometer of this character is of prime importance, as it furnishes a means of accurately testing others, and must be invaluable to M. Cailletet in his researches on vapour tensions and the compressibility of gases.—Report on a memoir by M. de Sparre, entitled "On Foucault's Pendulum."—On the measurement of a new base for French triangulation, by General Derrécagaix. The base chosen is between Villejuif and Juvisy. Its length is 7226·792 metres at 19°·26 C., with a probable error not exceeding a centimetre.—Observations of the Barnard-Denning comet and some new asteroids, discovered by Borrelly and Palisa, made at Algiers Observatory with the telescope of 0·50 metre aperture, by MM. Rambaud and Sy. The comet's position is recorded from April 4–7, Borrelly's asteroid on April 4 and 6, and Palisa's on April 7.—On linear differential equations, by M. E. Vessiot.—On a class of complex numbers, by M. André Markoff.—Relation between the electro-magnetic and electro-static units, by M. H. Pellat. A series of twenty experiments gave the relation as $3·0093 \times 10^{10}$, and another series of thirty-three experiments gave $3·0091 \times 10^{10}$. It is pointed out that the number $3·009 \times 10^{10}$ only differs by $\frac{1}{600}$ from Cornu's value for the velocity of light ($3·004 \times 10^{10}$).—On the variation of fusion points with pressure, by M. B. C. Damien.—Transformation of cupreine into quinine, by MM. E. Grimaux and A. Arnaud. Heating to 100° in sealed tubes, for twelve hours, a mixture of molecular proportions of cupreine (the base of *Quina cuprea*) and methyl chloride with an atomic proportion of sodium (the whole dissolved in methyl alcohol), yields quinine. The sulphate possesses all the characteristics of the sulphate of natural quinine. The authors deduce from the experimental evidence given that quinine is the methoxide of cupreine.—On the action of hydrobromic acid upon silicon chloride, by M. A. Besson.—The calorimetric study of platinic chloride and its combinations, by M. L. Pigeon.—The estimation of rhodium by electrolyis, by MM. A. Joly and E. Leidié. The conditions under which this estimation can be accurately carried out are given in detail; the method cannot be employed in presence of nitric or oxalic acids.—On an amidoisoxazol, by M. Hanriot. The preparation and properties of methylethylamidoisoxazol are described. The formula



is allotted to the new compound. It

is an isomeride of the monoxim of propionylpropionitrile, $C_2H_5 . C(NOH) . CH(CH_3) . CN$, from which it is prepared.—The use of phenylhydrazine for the determination of sugars, by M. Maquenne.—New combinations obtained with certain metallic sulphites and aniline, by M. G. Denigès.—On a violet colouring matter derived from morphine, by M. P. Cazeneuve.—Note concerning aspergillin, a vegetable hæmatin, by M. Georges Linossier. The author controverts the statement of M. Phipson, that aspergillin is identical with palmellin. A comparison of the properties of the two bodies shows many analogies of aspergillin with hæmatin, but none with palmellin.—Note on the influence exerted by neutral mineral salts of potassium upon the solubility of potassium bitartrate, by M. Ch. Blarez.—On the characterization of fig-wine, by M. P. Carles.—A method of recognizing margarine in butter, by M. R. Lézé.—On the process of purification of an alcoholic liquor (from molasses) during rectification, by M. Ed. Mohler.—Artificial reproduction of daubreelite (schreibersite), by M. Stanislas Meunier. Daubreelite has been obtained by treating at a red heat with sulphuretted hydrogen (1) a mixture in the proper proportions of ferrous chloride and chromic chloride; (2) very finely powdered natural chrome iron ore; (3) an alloy of iron and chromium. The last method yields the best result.—On *Clusia* of the *Anandrogyne* section, by M. J. Vesque.—On the existence of the medullary liber in the root, by M. J. Hérail.

STOCKHOLM.

Royal Academy of Sciences, April 8.—The Neuroptera of Scandinavia; Part 2, Neuroptera Trichoptera (*Phryganea*, L.), by the Rev. H. Wallengren.—Further remarks on the history of the vegetation of Greenland, by Prof. A. G. Nathorst.—Studies on enstatite, and the products of its metamorphoses, by K. Johansson.—Research on a group of long-periodical elementary links in the reduction of time, by Dr. K. G. Olsson.—*Tetrao bonasiotetrix*, Bogdanow, a hybrid between the black-cock and *Tetrao bonasia*, by Hr. G. Kolthoff.—Contributions to the knowledge of the Salices in the mountains of the south-west of Jemtland, by Hr. B. Floderns.—Contributions to the knowledge of *Bolocera*, a genus of the Actiniæ, by Hr. O. Carlgren.—On the rate of mortality within a determined age, by Dr. G. Eneström.—On a new sort of hygrometer, by Hr. C. Sondén.—On a new chronometer, which shows the thousandth of a second, exhibited by the inventor, Hr. W. Schmidt.

AMSTERDAM.

Royal Academy of Sciences, March 28.—Prof. van de Sande Bakhuyzen in the chair.—Mr. van der Waals dealt with the pressure of co-existent phases of a mixture, and particularly of solutions of salts and acids. He gives for this pressure a formula, into which enter two parameters, viz. the parameter c of electrolytic dissociation, and the parameter a of physical action; and points out that the solutions of salts and acids may be classified into two groups. For solutions belonging to the first group, for which $(a-2)\,c > 1$, the relative diminution of pressure per molecule always exceeds 2, and possesses a maximum value. The second group, $(a-2)\,c < 1$, shows for this diminution a minimum value and a maximum value. To the first group belongs the solution of KOH, to the second group the solution of SO_4H_2 in water.—Mr. Beyerinck spoke of the accumulation of atmospheric nitrogen in cultures of *Bacillus radicicola*. Whilst on a former occasion he had to state experiments with *Bacillus radicicola*, from which he deduced that, under the conditions thereby observed, an absorption of atmospheric nitrogen did not take place, he could now fix circumstances wherein such an accumulation may be obtained. To a decoction of stems and leaves of beans, 2 per cent. cane-sugar was added, and 100 c.cm. of this solution were filled into several Kjeldahl nitrogen balloons, and sterilized under cotton-enclosure. Some of these balloons were infected with very active material of *Bacillus radicicola* var. *Fabæ*, and exposed during eight weeks to a temperature of 5°–15° C. in a semi-obscure part of the laboratory. The matter for the infection was isolated in 1889, from tubercles of Windsor beans, and lately cultivated on a decoction of lucerne-stems with 2 per cent. cane-sugar and 8 per cent. gelatine, this being an extremely good soil for our *Bacillus*. Next to every infected balloon there was put a non-infected one for controlling the nitrogen absorption. A very profuse and interesting vegetation was soon perceived in the infected balloons. This vegetation contained rods, rapid-moving swarming individuals, bacteroids, and "stars." These stars are bacteroids, with many ramifications instead of three. The nitrogen was dosed after Kjeldahl's method, with iodometric determination of the ammoniac. The numbers would certainly have been found greater if the cultures had not been interrupted whilst the bacteria were still growing and swarming with great vigour, as was the case after eight weeks. In the following table the increase of nitrogen, albuminous matter, and bacteria corresponding with this increase—the latter being calculated as containing 75 per cent. water—from twelve experiments are inserted. The increase of nitrogen, 0·0011718 gr. per 100 c.cm., in the second experiment was found as the difference between 0·0061194 gr. in the infected, and 0·0049476 gr. in the non-infected balloon, &c.

	Increase of nitrogen per litre.	Increase of albuminous matter per litre (= N × 6·25).	Increase of bacteria therewith corresponding (= N × 6·25 × 4).
1st experiment ..	0·009114	0·0569625	0·227850
2nd ,, ...	0·011718	0·0731375	0·292550
3rd ,, ...	0·018228	0·1129140	0·451656
4th ,, ...	0·015624	0·0976500	0·390600
5th ,, ...	0·010416	0·0651000	0·260400
6th ,, ...	0·013020	0·0813750	0·325500

It is yet to be determined whether this increase is due to the fixation of free atmospheric nitrogen by the bacteria, or to the complete exhaustion of the environment of all nitrogen compounds by these organisms, and a thence caused "affluing" of the ammoniac or the other nitrogen compounds of the air into the culture-liquid.

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