THURSDAY, OCTOBER 23, 1890.

BRITISH FARM, FOREST, ORCHARD, AND GARDEN PESTS.

British Farm, Forest, Orchard, and Garden Pests. A Manual of Injurious Insects, with Methods of Prevention and Remedy for their Attacks to Food Crops, Forest Trees, and Fruit, to which is appended a Short Introduction to Entomology. Compiled by Eleanor E. Ormerod, F.R.Met.Soc., &c. Second Edition. (London: Simpkin, Marshall, Hamilton, Kent, & Co., 1890.)

THE first edition of "The Manual of Injurious Insects" was published in 1881, and was then justly considered by all entomologists to be the most important work upon economic entomology since Kirby and Spence wrote their famous "Introduction to Entomology," "combining," as John Curtis said, "truth, instruction, and amusement." It was undoubtedly also by far the most exhaustive account of insects destructive to agricultural and horticultural crops that had been produced since the appearance of the admirable "Farm Insects" of Curtis in 1860. The second edition of this useful "Manual of Injurious Insects" has been recently issued, and contains in addition to the vast stores of information concerning all manner of insects which attack farm and garden crops, the results of the devoted labours, keen research, and scientific observation of Miss Ormerod, during a period of nine years.

In point of volume and matter this last edition is nearly twice as large as the first. As regards interest, practical value, and science, it is likewise of much more importance, because it records the discovery of insects altogether new and undescribed in this country, as well as measures of prevention and remedial methods against these and many other insects, that have been prescribed and adopted within the past decade. It describes, in short, the advance which has been made in economic entomology in this period, in the knowledge of insects, of their life histories, and habits, and of means to protect the crops of cultivators against their ravages. And no one is better qualified to relate this progress than Miss Ormerod, who has herself contributed so greatly towards it.

Most of this new matter has been previously given for the edification of the public and the advantage of farmers in Miss Ormerod's "Annual Reports of Observations of Injurious Insects and Common Farm Pests." It is condensed in the new manual, and arranged under different headings, or parts. These are three, the same as in the first edition. Part I.—Food crops and insects that injure them. Part II.—Forest trees and insects that injure them. Part III.—Fruit crops and insects that injure them.

An Introduction to Entomology is given in this edition as an Appendix, while in the former it precedes the three parts, or divisions. It may be said of this, in passing, that it will be most useful to students of entomology, as it gives in concise terms the main points by which insects of various orders and species may be distinguished in each stage of their life histories. The classification of insects is plainly set forth so that beginners may see almost at a glance the primary division of insects into the two great tribes, *Mandibulata* and *Haustellata*, and the subdivision of the one into eight orders, and of the other into five orders, in accordance with the rational arrangement of Prof. Westwood.

Among the troublesome insects treated of in Part I. are several species of butterflies, moths, and flies which attack cabbages, as the large and small white cabbage butterfly, *Pieris brassicæ* and *Pieris rapæ*, the cabbage moth *Mamestra brassicæ*, the cabbage fly *anthomyia brassicæ*, and others more or less injurious to the brassica tribe. Complete histories are furnished of all these insects, and valuable means of prevention are advised and remedies suggested of a practical nature that can easily be adopted, both on a large scale suitable for farmers and market gardeners, as well as for gardeners and allotment holders.

There is an important monograph of the carrot-fly, *Psila rosæ*, which will be gratefully received by market gardeners and market garden farmers, as the fly has in the last few years been especially destructive, not only in England, but also in Scotland and Ireland. This attack is generally termed "rust," because the leaves of the carrots become yellowish, or rusty coloured, and the roots are covered with rusty patches. To one unacquainted with entomology and not having good eyesight, it is difficult to trace the cause of the disorder to the tiny maggots of this fly in the roots of the plants. Upon very careful examination, however, a diseased carrot will be found to be swarming with legless, slimy, yellowish maggots, not a quarter of an inch in length, many of which are found to be sticking half in and half out of the roots. Miss Ormerod says:—"The grubs may be found in winter as well as summer, and attack all parts of the carrot-root by gnawing galleries on the surface, or into the substance of the root; but whilst the roots are young, the grub appears generally to attack the lowest part." This is not always the case, for in some young carrots examined in July last, which were sent from Ireland, the crowns of the roots were as full of the maggots as the ends.

Under the head of prevention and remedies for this affection, it is stated—

> "The following notes regarding carrot cultivation will be found to bear in various ways suitable to different circumstances of soil and climate on the main points of —1st, such preparation of the ground in autumn, or winter, as will ensure favourable conditions for a healthy, vigorous, and uninterrupted growth from the first sprouting of the seed; 2nd, thinning at such a stage of growth, in such circumstances of damp weather or with such watering or treatment after thinning as may least expose the plants to the attack of the carrot-fly which frequently occurs after this operation. Whether the fly is attracted by the scent of the bruised plants, or what brings it, is not clear, but it is very clear that, as it goes down into the ground to lay its eggs on or by the carrots, all operations which leave the soil unusually loose and open lay at the same time the carrot roots open to attack, and it will be observed that the various methods of treatment in regard to thinning bear upon the means of meeting this difficulty."

This is given as one instance of Miss Ormerod's powers of observation as to the habits of insects, which enable her to recommend suitable and effective remedies and methods against them. The practical conclusion in this case is that carrot-growers should thin carrot plants early, and draw the soil as close as possible, and make the soil very firm around them immediately after thinning has taken place.

Again, "although the summer broods hatch in three or four weeks the maggots may be found in the roots during the winter, and they change to pupæ in the earth adjacent. It is therefore very desirable that all infested carrot-beds should be thoroughly cleared of roots in the autumn, and the ground well dug, or trenched, so that such maggots or pupæ as remain in the bed may be destroyed; some may escape, but the larger number will thus be buried too deeply to come up again or be thrown on the surface to the birds; and a dressing of gas-lime will be serviceable in destroying such of the maggots as are lying near the surface."

Remedies are prescribed for this attack in the shape of dressings with spirits of tar mixed with sand, and of paraffin oil and sand; also waterings with dilute soluble phenyl and paraffin oil, in the proportion of a pint of paraffin to two gallons of water.

Among the many insects that injure corn crops whose histories appear in this "Manual," is a group of flies, among which are the frit fly (*Oscinis frit*)—a minute fly, not the eighth of an inch long, whose attention seems to be confined to oats. The maggot coming from the egg laid by this "fly feeds in the heart of the young oat-plant a little above the ground-level and eats away the centre, so that the shoot above the eaten part is destroyed, and the damage that is going forward then becomes noticeable from the injured shoots turning brown and withering instead of continuing their growth."

The frit fly has been well known in France, Germany, and particularly in Sweden, where it attacks barley, but until 1888, when the attacks of the frit fly were very prevalent in Devon and Cornwall, not much was known of it in this country, although, as Miss Ormerod points out, "the presence of the *Oscinis vastator*, Curtis, which appears to be the same as *Oscinis frit*, was watched and recorded in 1844 by John Curtis in his 'Farm Insects.' In 1881 I was favoured by Mr. R. H. Meade, of Bradford, with the information that the *Oscinis frit* had been observed in the autumn of that year in swarms in an outbuilding, in the lofts of which a lot of newly-threshed barley had been stored, which points to the Swedish form being then present; but it was not until 1887 that I was able to watch this attack throughout its course, up to the development of this fly as a regular field attack."

Farmers now find another fly, the "gout" fly, or ribbon-footed fly, *Chlorops tæniopus*, to be a frequent enemy to wheat, rye, and barley plants. This, as shown in the "Manual," is most prevalent on barley, and is mentioned by Curtis as having done much harm, in 1841, in Surrey and Lancashire. Now it is found in most parts of the country, and is a striking instance of the general spread of insect pests within the last few years among cultivated crops of all kinds.

The action of this insect is thus described by Miss Ormerod:—"Whilst the plant is still young and the forming ear is wrapped in the sheathing leaves, the fly places her eggs either within these leaves or so that the maggot can make its way through them to the ear; there it usually eats away some parts of the lower portions of the ear, and then gnaws or, rather, tears a channel down one side of the stem to the uppermost knot, and beneath the leaves the maggot changes to a reddish chrysalis, from which the gout fly appears about harvest time."

It has been a moot point where this insect passed the winter in this country. In Germany, as Taschenberg states in his Praktische Insekten Kunde, the flies place their eggs on grasses and autumn sown corn, upon which hibernation takes place either in larval or pupal form. As reported by the Consulting Entomologist of the Royal Agricultural Society of England, pupæ of the chlorops were discovered in the main stems of wheat plants just above the ground, in England, in the early part of the spring of 1890 by Mr. Whitehead. The time when these were found and the evident injury caused to the plants proved that the insect had hibernated within their stems.

Another insect belonging to the group of corn flies is the corn saw fly, *Cephus pygmæus*, a very small insect which pierces the stem of wheat and barley plants "just below, or at one of the knots, and inserts there an egg, continuing this process successively to other stems until her egg supply is exhausted. The maggot, which hatches in about ten days, is about half an inch long, yellowish white, fleshy, with a horny, rusty-coloured head, and is peculiar in being footless, although the larva of a saw fly. It feeds on the inner substances, clearing its way sometimes through the knots, even through the topmost, and when nearly full-grown comes down inside the stalk on which it has fed; and about harvest time, or a little before, it comes down to the ground level, where it gnaws a ring so neatly and cleanly round inside the stem that the straw readily falls with its own weight, or from a slight pressure of the wind, the severed stalk showing almost as smooth a fracture as if it had been separated by a knife. When the maggot has thus travelled down the stalk and nearly cut it through (so that nothing may prevent its escape presently as a fly) it goes down into the lowest part and spins itself a silken case in which it passes the winter."

The wheat-bulb fly (*Hylemia coarctata*), though only identified in 1882, has now become one of the pests to be dreaded by wheat growers. Curtis does not speak of it, and it was first distinguished in this country by Miss Ormerod. Taschenberg speaks of this fly as destructive in parts of Germany, and says there are two broods there. As this seems to be a new destroyer here, it is possible that it was brought from Germany with imported straw or produce of some kind.

In the "Manual" it is observed that the attacks of the maggots of the wheat-bulb fly and those of the frit fly are much alike, so far as the method of injury is concerned. But here Miss Ormerod's entomological knowledge and acute perception of the smallest distinctions serve to show how the different flies may be recognized. In the maggot or larva of the wheat-bulb fly "the tail segment projects, and ends in two squarish-ended teeth with flattened edges placed centrally, with one pointed tooth, and sometimes more, on the central square part. . . . The presence of these teeth and the *absence of* a little bunch of stalked spiracles near the head appear to me to be the simplest way of knowing the wheat-bulb from the frit maggots."

The Hessian fly, another member of the group of stem flies, having first appeared in Great Britain in 1886, is graphically described by Miss Ormerod, who has done so much to familiarize agriculturists with the dreaded scourge, and to make them acquainted with preventive measures and remedies against it. This information, published from time to time, is concisely summarized, so that it may be said that, in the few pages devoted to this insect, all that is known about it is plainly set forth.

The least generally known facts connected with the Hessian fly, and those of the most scientific interest, relate to its parasites, which have been carefully studied by the authoress, who had the advantage of long consultations with Prof. Riley in 1887.

The importance of the various parasites of the Hessian fly in tending to keep it down in this country is great. By some it is believed to be desirable to rear them and take them to places that are badly infested, just as, recently, parasites were imported from Australia to destroy the *Icerya purchasi* in the Californian orange groves. It is certain that in this last summer the attack of the Hessian fly was immensely modified by the parasites, which were present in unusual numbers. In several instances where the pupæ of the Hessian fly were transferred to live cages, at least 70 per cent. proved to be parasitized by at least three different kinds of flies. Miss Ormerod and Prof. Riley agree that the parasites of the Hessian fly in Great Britain are of the same species as those found in Russia, and differ from those which infest the Hessian fly in America. Comparative lists of the American and Russian Pteromali are submitted, from which it is seen that they are of the same genus, but not of identical species. "The examination of our parasites," Miss Ormerod concludes, "pointed, therefore, very strongly to the probability of our Hessian fly attack having been imported to us from the east of Europe." And, further, it is suggested that it originated, not in straw imports, as it was first imagined, but in the pupa, or "flax-seed," condition in foul grain imports.

In Part II., devoted to the insects that injure forest-trees, among the principal offenders is shown to be the elm-bark beetle, *Scolytus destructor*, which makes the well known galleries between the bark and the wood, "mainly in the soft inner bark, but so as to leave a slight trace of the working on the surface of the tree." This beetle often causes serious injury to elms both in this country and on the Continent. It generally attacks trees, or the parts of trees that are inclined to disorder, or decay, or that have been previously attacked by beetles. To circumvent the operations of this insect, Miss Ormerod recommends that the rough bark should be scraped off, so that the larvæ are exposed to air, or driven out by the flow of sap from the inner lining of the bark. This was found to answer in France, where upwards of 2,000 trees were thus treated.

The ash-bark beetle, *Hylesinus fraxini*, injures ash trees in the same manner by making galleries beneath the bark, particularly in young trees. It is advised that the bark should be treated with a good coat of soft soap well rubbed into the affected parts of the trees.

Yet another boring beetle is given, known as *Hylurgus piniperda*, or pine beetle, injurious more on account of the harm the beetles cause by boring through the side of the tender shoots of young pine trees and eating their way for an inch or more along the pith, than from the galleries made by the larvæ in pine timber. As they often select dead or diseased trees for boring into for breeding purposes, felled trees should be at once removed and diseased branches or limbs of trees in infected woods should be cut off and carried away. Or traps may be set for the beetle by placing "young Scots pine tops, thinning off all the branches (which makes them convenient to handle) in the plantations or against the lower part of the standing trees." The beetles select these for laying their eggs upon, and they should be taken away and burned in June.

Another pine beetle, *Hylobius abietis*, is even more injurious to many of the coniferæ than the *Hylurgus*. It may be entrapped in the same manner, as it frequents forest clearings, that is, where fir trees, few or many, have recently been felled, and lays its eggs also on logs and stumps.

Against the attacks of many other insects troublesome to trees, such as the pine-bud moth, the pine-shoot moth, the pine saw fly, the spruce gall aphis, the larch aphis, the willow beetle, and the oak-tree roller moth, methods of prevention and remedies are prescribed. This part of the "Manual" cannot fail to be most instructive and useful to those in charge of woods and forests.

In Part III., treating of fruit-crops and insects that injure them, twenty-three different insects are fully described, and in all cases practical suggestions are made for preventing their onslaughts upon the fruit crops, and for diminishing the virulence of their attacks. These suggestions are most timely, as during the last few years the fruit crops of almost all descriptions have suffered much from insects. Not only have new kinds of insects arisen, but long-known foes have increased and multiplied to a terribly dangerous extent, so that whole districts have been cleared of fruit. For example, in the spring of each of the last three years hosts of caterpillars of several species have ruined the apple, plum, and damson crops in many parts of Kent, and in other fruit-producing counties.

Among the fruit pests that have recently sprung up are the white woolly scale, *Pulvinaria ribesii*, found last year upon currant bushes to a considerable extent. A figure of a currant twig covered with white cottony, or woolly matter, forming a covering for the eggs and young scales is appended, which conveys a good idea of the "almost overwhelming nature of the infestation and the serious amount of injury caused by it." This attack has been known in France for some time, and is mentioned by Signoret in his "Essai sur les Cochenilles." Miss Ormerod recommends applying limewash to the infested bushes with a brush, "the same process as whitewashing." Where remedies cannot be brought to bear, or fail, "it would be best to cut off and burn the infested branches, or to destroy and burn the infested bushes if it could be done without serious loss, and thus stamp out this newly-observed pest in time."

A fruit-tree boring beetle new in this country, but well known in Germany and America, from whence it was probably imported, was identified by Miss Ormerod in 1889 as the "Shot-borer," *Xyleborus dispar*. This was found in Lord Sudeley's fruit plantations in Gloucestershire, in the stem of a young plum tree into which it had bored and killed the tree. Several trees were killed in the same manner. The great peculiarity in these insects is the disparity in size between the females and males, from which it is termed *dispar*. The female is about the eighth of an inch long, while the male is only about two-thirds of this length. The injury begins by a small hole like a shot-hole being bored in the side of the stem, from which a tunnel is made into the pith, and a branch tunnel running horizontally about half, or two-thirds, round the stem. Other tunnels are made straight up and down. These borings, and the destruction of the pith, soon serve to kill the branch. The only remedy appears to be to cut off and burn the infested limb, and "coating the trees with some wash or mixture, which will not hurt the bark but will prevent the beetle getting in or getting out. One application advised for trial is a thick coat of whitewash with some Paris green in it."

There is a detailed account of the winter moth, that arch enemy of apple, pear, and plum growers; this is particularly valuable, as it gives the latest experience of practical growers with respect to preventive and remedial measures. The most important of these is the careful banding of the trees in the autumn, before October, with grease and offensive compounds, to prevent the females from climbing up, and the use of arsenites (Paris green and London purple) for washing or syringing infested trees. These washes have been proved to be efficacious in America, where they are universally applied for many insect attacks. In this country, however, cultivators have hesitated to use them on account of their poisonous nature. Miss Ormerod plainly shows that they may be employed without danger and with vast benefit to the fruit grower. For plum trees, the proportion is 1 ounce of Paris green to 10 gallons of water, and for apple trees 1 ounce to 20 gallons. Testimony is given from various growers as to the efficacy of this wash, which from henceforth will, it is presumed, be adopted, as it seems to be the only one which will check the ravages of moths injurious to fruit trees. Full details concerning the use of these American remedies for insect attack are given, which must be most serviceable.

Want of space prevents allusion to many other insects described in this part of the work. It can only be said that they are clearly and minutely defined, and all that is known of their habits and of means to avert or to modify their mischief is set forth.

The "Manual" is replete with capital figures of the insects in all stages. Many of these are from drawings executed by Miss Ormerod, and many are the well-known accurate and inimitable designs of Curtis.

TORNADOES.

The Tornado. By H. A. Hazen, Assistant Professor of the United States Signal Office. (New York: Hodges, 1890.)

THIS is a book that will hardly enhance the reputation of its author. Despite his assurance (which of course will not be questioned) that he has endeavoured throughout to be absolutely unprejudiced, its apparent aim is not so much to set before the reader a concise description of tornado phenomena as to controvert the views put forward by Ferrel and others relative to their mechanical and physical constitution, and to substitute for these certain other speculations (we can scarcely call them a theory) which appear to the author to have the merit of greater probability. Prof. Hazen does not, indeed, restrict his condemnation to Ferrel's theory of tornadoes and thunder-storms. As a root-and-branch reformer, he finds himself in opposition to the majority of those who, during the last quarter of a century, have built up the fabric of modern meteorology, for, while he speaks with deference of "the epoch-making experiments of Mayer [*sic*] and Joule," he appears to regard as inapplicable to the movements of the atmosphere those laws of thermodynamics which are based on the results of Joule's labours. Were it the practice of scientific authors, in imitation of romance-writers, to head their chapters with quotations appropriate to the subject-matter, chapter v. of this treatise, more especially, might be fitly introduced with the well-known lines from "Faust":—

> "Ich bin der Geist, der stets verneint!
> Und das mit Recht! denn alles was entsteht
> Ist werth, dass es zu Grunde geht";

substituting, however, "*entstanden*" for the present tense of the verb.

Lest it should be thought that these remarks misrepresent or exaggerate the sweeping character of Prof. Hazen's "objections," we extract one or two passages from the chapter in question. On the generally-accepted view that work is performed by an ascending current of air, in pushing aside the atmosphere into which it expands, and that in saturated air the requisite energy is furnished, in part at least, by the condensation of vapour, he observes (p. 52),

> "There is nothing in the science of meteorology, or possibly in any physical science, that has been developed from such a worthless origin as this theory of the liberation of energy on the condensation of moisture";

again (p. 54),

> "All the reasoning regarding the diminution of temperature in dry and moist air, as we ascend in the atmosphere, is founded upon purely theoretical considerations. Every experiment, whether in the laboratory or in Nature, has proved that these theories, in their sum and substance, are false";

and again (p. 56),

> "I am inclined to think that even Espy, with all his disadvantages, was too well informed to adopt such a doubtful and visionary idea as this of effective work performed in the free upper air."

The familiar lecture experiment illustrative of dynamic cooling, in which a cloud is produced in a receiver containing moist air by partially exhausting it with a few strokes of the air-pump, is interpreted in a novel manner consistently with the above opinions (p. 67):—

> "The presence of haze or cloud is no evidence of saturated air, for such cloud has been produced in air having only 2 per cent. of moisture.[1] When air is pumped from the room, it has an enormous number of dust particles in it, and these give the appearance of a fog on sudden expansion."

After these samples of the author's opinions it will be scarcely necessary to notice, in detail, the other numerous

---

[1] Prof. Hazen does not give his authority for this statement, nor does he specify whether the expression is to be understood as 2 per cent. of saturation, or 2 per cent. by weight or volume. If the former, authentication seems desirable; if the latter, the fact is obviously irrelevant.

points on which Prof. Hazen's views are in dissonance with those of most other writers who have treated of tornadoes. Among others, he assures us (p. 57), that "the evidence for [their] gyrations is exceedingly contradictory, and the weight of evidence is overwhelmingly against them"; that (p. 52) it is impossible that warm south wind underruns that which is cooler from the north, "for the denser must always be beneath the lighter"; and (p. 59) that it seems impossible to ascribe the progressive movement of the tornado to the drift of the upper current, "because it moves with a velocity double that of the general storm." Those who are curious to see the further arguments by which these theses are supported must be referred to the work itself. We have yet to notice briefly the alternative views advocated by the author.

The late Dr. Percy used to relate that, in his early days, when the iron would not "come to nature" in the Staffordshire puddling furnaces, the workmen were accustomed to ascribe its perversity to the presence of sulphur in the charge. In still more remote times, the potentate in whose realm that element is supposed to be somewhat abundant, or his agents, would assuredly have been held responsible for what was amiss. But thirty years ago the march of science had brought in other ideas, and the approved explanation of any otherwise unaccountable difficulty of the kind was that electricity had something to do with it. Even at the present day this mysterious agency is the favourite resource of puzzled tyros in physical reasoning, but we should hardly have expected to find it seriously put forward in all its familiar vagueness by an author whose official designation is that quoted above from the title-page of his work. That such, however, is the case stands in evidence in the following extracts, which we give as fully as our space will admit of, lest we should fail to do their author justice:—

> "It is very difficult to believe that electricity has nothing to do with our thunder-storms, and is merely a result and never a cause. . . . Our thunder-storms seem to show an enormous storehouse of electricity at five thousand or six thousand feet above the earth; at least electricity seems to be concentrated there over thousands of square miles during thunder-storm action. We are taught that electricity forms a sort of dual condition, or the electric field is a double one. May not this electric field draw on the sun for its energy? . . . Why may not the sun's electricity, oftentimes observed by its direct effect on our magnetic instruments, and, more often still, indirectly in our auroras, be intercepted by a peculiar condition of the atmosphere or of the earth below, and thus be concentrated in particular localities?"

This may, perhaps, appear somewhat vague as an alternative theory of storm generation; in one particular, however, viz. the accumulation in the storm-cloud of the enormous quantities of water precipitated in cloudbursts, the *modus operandi* is more fully explained. In Prof. Hazen's opinion, it would seem that electricity performs a part in the atmosphere somewhat analogous to that of Clerk Maxwell's hypothetical demons, and which is described as follows:—

> "Is it inconceivable that we have to deal here with a negative electric field, which draws to itself with great velocity particles of moisture from regions perhaps for one hundred miles about, when suddenly, upon a discharge of electricity, the potential upon the particles is diminished, and they unite in great abundance and form rain-drops?"

This remarkable speculation, it is considered, receives support from a novel experiment described as follows:—

> "A Holz machine was run for fifteen minutes in a rather large room; and most careful measurements of the amount of moisture at the machine and at a point twenty feet away, before and after the machine was in action, showed an increase at the machine. When we consider that it was impossible to measure the moisture contents just at the plate of the machine, and also what an extremely slight charge could by any possibility enter the air from the machine, we can but be surprised that any effect at all was observed."

Without imitating King Charles the Second's scepticism in the matter of the fish's weight in water and out of water, but accepting Prof. Hazen's statement of the results as he gives them, we may still inquire whether the operators who worked the Holz machine continuously for fifteen minutes did not exhale a considerable quantity of water vapour in the neighbourhood of the machine. Perhaps they even perspired freely with their exertion. In any case the foundation seems hardly adequate for the superstructure.

Had this book appeared under a less known name, and were it not for the official position of the writer, we should scarcely have deemed it desirable to notice it at such length. A really searching, intelligent criticism of Ferrel's theory, by one who has exceptional advantages for ascertaining the facts of observation, would have been welcome; for, symmetrical as that theory is, it is still mainly deductive, and there are many points in it, and these not the least important, which still lack confirmation. But we cannot attach much weight to the objections raised by Prof. Hazen. They seem to betray a strange misconception of the physical processes which he condemns in such uncompromising terms; and where his arguments turn on the facts of observation, we must decline to accept his sweeping denials, in the face of the positive testimony of numerous, not incompetent, observers. In some cases, indeed, we might adduce our own personal experience of phenomena which are declared by him to be improbable or impossible. Such are, for instance, the superposition of dry northerly above warm and moist southerly currents, and the spiral movement of the air in dust whirls, which, on a miniature scale, represent that of the tornado.

Again, Prof. Hazen's argument that the rise of pressure beneath a thunder-storm is sometimes observed in storms that are rainless, and therefore cannot be due to the cooling of the air by the rain, or to its downward pressure as it falls, is rendered of little weight by the fact that this occurs only when the lowest strata of the atmosphere are very dry. In the recently published "Climates and Weather of India," it is stated that "a complete transition may be traced between [the rainless dust-storms of Upper India] and the north-westers of Bengal, which are accompanied by heavy rain." In the latter province "the dust-storm is. as a general rule, only the first stage of a north-wester." It is attended with a sudden rise of pressure, and "is followed by heavy rain and sometimes hail," and though the dust-storms of the former are occasionally, though perhaps rarely, quite rainless, "the coolness of the wind and that of the atmosphere after the storm is over is hardly to be accounted for otherwise than by supposing that rain is always formed in the cloud overhead, but is re-evaporated before it reaches the earth."

There is nothing inconsistent in the existence of an excessive pressure at the ground surface beneath a thunder-storm and a diminished pressure in the vortex of the storm-cloud, but in ordinary thunder and hail storms this latter is restricted to the cloud-region. As the barograph traces of these storms show, the oscillations of pressure beneath them are very great, and there may and indeed must be still greater differences between the tornado vortex and the neighbouring region of precipitation. Indeed, the great velocity of the air-movement implies as much.

A part of Ferrel's theory which especially stands in need of confirmation is the assumption that, immediately prior to the formation of the vortex, the vertical distribution of temperature is such as to bring the atmosphere into a state of unstable equilibrium, and that a slight casual local disturbance of this equilibrium starts the vortical uprush. This is also his explanation of cyclone generation, and indeed it is that hitherto held by the majority of writers on the subject. On the other hand, it is generally considered that anticyclones are determined by the greater local density of the atmosphere, due to a low mean temperature of the air-column. The last of these assumptions, even in the case of winter anticyclones accompanied by very low temperatures at the ground surface, has now been conclusively disproved by Prof. Hann, of Vienna; and he has also shown very strong reasons for believing that the temperature conditions of extra-tropical cyclones are also incompatible with the prevailing view. It does not, of course, follow that those of tornadoes and hail-storms are equally so, but at least the assumed conditions require verification. This may, perhaps, be some day effected by our mountain observatories.

H. F. B.

OUR BOOK SHELF.

Inorganic Chemistry: the Chemistry oj the Non-Metals. By J. Oakley Beuttler, M.A. (London: Relfe Brothers.)

NOW that there can be obtained a considerable variety of really good text-books of elementary chemistry suitable for all the usual needs of the present day, one is entitled to look for special features in any new manual. We fail to find any reason for the existence of the volume before us: wherein it differs from others that enjoy general recognition, it is incomplete and erroneous. It has neither index nor contents table, but this is quite a trivial matter when compared with the imperfections of the body of the work. On pp. 19 and 20 there are nine attempts at equations, none of which are correct, while many represent impossible or at least unknown reactions; and in the following paragraphs, on graphic notation, bonds, and radicles, there is a collection of statements that read like the imperfect recollections of a student who never understood the subject. A single atom of oxygen is shown with curiously shaped projections as an example of an element with an even number of bonds existing as a single "atom-molecule." It is stated emphatically that "the element having the greatest number of bonds is always printed in thick type," but we search in vain for thick type in any formula in the book. The statements that are intended to convey the facts of chemistry are vague, often misleading, and very rarely of a practical character. For an illustration of the style there is no need to go further than the chapter that treats of the first element, hydrogen. It states that "on throwing a piece of sodium into water the sodium combines with part of the hydrogen of the water to form caustic soda, liberating the other part of the hydrogen." The volume closes, as one would expect, with the questions set by various examining bodies during the last three or four years.

Anatomy, Descriptive and Surgical. By Henry Gray, F.R.S. Twelfth Edition. Edited by T. Pickering Pick. (London: Longmans, Green, and Co., 1890).

OF a solid text-book so well known as the present work it is hardly necessary to say more than that a new edition has appeared. The book has been carefully revised, and the editor has added considerably to its value by introducing sections on topographical anatomy, and amplifying those on surgical anatomy. Both of these classes of sections have been printed in smaller type, so that they may be disregarded by students who wish to confine their attention exclusively to the descriptive part of the subject. There are many new illustrations, some of which are original.

The Story of the Heavens. By Sir Robert Stawell Ball, LL.D. Fifteenth Thousand. (London: Cassell and Co., 1890.)

IT is, for many reasons, satisfactory that there should be a popular demand for a clear, brightly-written work on astronomy. Sir Robert Ball, however, ought hardly to be content with the issue of mere reprints of his book. It may be somewhat misleading to send forth in its original form, in 1890, an astronomical work first published in 1886.

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 Passage of Electricity through Gases.

IN my letter in NATURE of July 24 (p. 295) I objected to Prof. Schuster's statement that the fact that free atoms must turn a gas into a conductor of electricity was fatal to the theory of the electric discharge given by me in the *Philosophical Magazine* in 1883, and I maintained that the presence of free atoms in a gas free from electric strain was in no way essential to the theory given in that paper. I see no reason, after reading Prof. Schuster's letter in this week's NATURE, to change that opinion. Prof. Schuster bases his statement, not on my description of the theory itself, but on the explanation by it of the weakening of the electric strength produced by a diminution in the density of the gas. A reference to this explanation will show, however, that it really rests solely on the well-known fact that dissociation is assisted by diminution of pressure, and that the passage which Prof. Schuster quotes is merely an explanation of this property of dissociation from the point of view of the kinetic theory of gases; if this explanation is held to be inconsistent with the absence of free atoms from gas in a normal state, then any alteration in the explanation which might be made to meet this difficulty, though of primary importance in the kinetic theory of gases, is only of secondary importance for the theory of the electric discharge given in my paper, which I still maintain is not all bound up with the existence or non-existence of free atoms in gases not in the electric field. J. J. THOMSON.

Cambridge, October 18.

Changing the Apparent Direction of Rotation.

IN NATURE of October 16 (p. 585), a curious optical effect is incidentally mentioned. Standing near a windmill, and nearly in the plane of the sails, "it is possible, by a slight mental effort, to change the apparent direction of rotation, and back again."

A similar effect I have often observed, but it seems in no way dependent on the will. Look, for say 30 seconds, steadily at the revolving disks of an anemometer; they will soon reverse their apparent direction, whether you wish it or not. Continue still to gaze, and that reversed direction will be changed back.

All whom I have asked to try this experiment felt the effect to be involuntary. The changes take place not gradually or confusedly, but distinctly and with decision. The fact is plain; the explanation not so simple. HERCULES MACDONNELL.

4 Roby Place, Kingstown.

Earthquake Tremors.

PERMIT me to say that Mr. John Perry, in his criticism (October 2, p. 545) of my "Method of observing the Phenomena of Earthquakes," has assumed that the phenomena observed were due to vertical displacement; whereas they were probably due to a swaying of the building in which the observations were made.

This assumption seems also to have been made in the case of the man mentioned by Mr. Wire in your last issue (p. 593).

Marine Villa, Shanklin, I.W., H. G. DIXON.
October 18.

A Ball of Fire.

AT about 12.5 last night I was going through the street at Milverton, and saw a bright light about south of me. I saw also a bright ball of fire appear through a break in the clouds proceeding with great rapidity, at about the height of 45°, in a direction which I estimate to be from south to north-north-east; it disappeared behind a church, and I saw nothing more. I am told this may be of interest, and therefore forward the account to you. CHARLES RANDOLPH.

Milverton, Somerset, October 17.

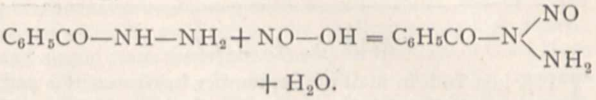
HYDRAZOIC ACID—A NEW GAS.

A NEW gaseous compound of nitrogen and hydrogen has been obtained by Dr. Theodore Curtius, the discoverer of amidogen, and its nature and properties were described by him in the Chemical Section during the recent scientific meetings at Bremen. The composition of the gas is HN_3, and its constitution $H-N\Big\langle\begin{matrix}N\\ \|\\ N\end{matrix}$. It is, in fact, the hydrogen compound corresponding to the well-known diazobenzene imide of Griess, $C_6H_5N\Big\langle\begin{matrix}N\\ \|\\ N\end{matrix}$, the three nitrogen atoms being united in the form of a closed chain. The gas dissolves in water with great avidity, forming a solution which possesses strongly acid properties, and dissolves many metals, such as zinc, copper, and iron, with evolution of hydrogen gas and formation of nitrides, the metal taking the place of the liberated hydrogen. The derivation name of the gas, azoimide, is somewhat unfortunate in view of its strongly acid nature, and Prof. Curtius proposes the name "Stickstoffwasserstoffsäure." Perhaps the nearest English equivalent, open to the least objection, is hydrazoic acid—a name which will serve to recall the many analogies which this acid bears to hydrochloric and the other halogen acids.

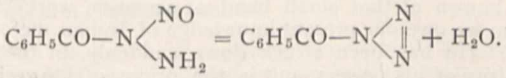
In studying the reactions of his recently-discovered hydrazine (amidogen) hydrate, $N_2H_4 \,.\, H_2O$, Dr. Curtius found that benzoylglycollic acid, $C_6H_5CO-O-CH_2COOH$, was decomposed by two molecules of hydrazine hydrate, with elimination of water and formation of benzoylhydrazine, $C_6H_5CO-NH-NH_2$, and hydrazine acetic acid, $NH_2-NH-CH_2COOH$, in accordance with the equation—

$$C_6H_5CO-O-CH_2COOH + 2N_2H_4 =$$
$$C_6H_5CO-NH-NH_2 + NH_2-NH-CH_2COOH + H_2O.$$

Under the influence of nitrous acid benzoylhydrazine forms a nitroso compound, $C_6H_5CO-N\Big\langle\begin{matrix}NO\\ NH_2\end{matrix}$, which spontaneously changes into benzoyl-azo-imide, $C_6H_5CO-N\Big\langle\begin{matrix}N\\ \|\\ N\end{matrix}$, with elimination of water.

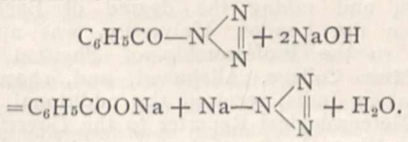


$$C_6H_5CO-NH-NH_2 + NO-OH = C_6H_5CO-N\Big\langle\begin{matrix}NO\\ NH_2\end{matrix} + H_2O.$$

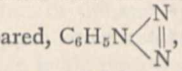


$$C_6H_5CO-N\Big\langle\begin{matrix}NO\\ NH_2\end{matrix} = C_6H_5CO-N\Big\langle\begin{matrix}N\\ \|\\ N\end{matrix} + H_2O.$$

Benzoyl-azo-imide decomposes, upon boiling with alkalies, with formation of benzoate of the alkali and the alkaline salt of the new acid.



$$C_6H_5CO-N\Big\langle\begin{matrix}N\\ \|\\ N\end{matrix} + 2NaOH$$
$$= C_6H_5COONa + Na-N\Big\langle\begin{matrix}N\\ \|\\ N\end{matrix} + H_2O.$$

When this sodium nitride is warmed with sulphuric acid, hydrazoic acid, $H-N\Big\langle\begin{matrix}N\\ \|\\ N\end{matrix}$, is liberated as a gas. The gas is decomposed by hot concentrated oil of vitriol; hence diluted acid requires to be employed, and the gas can thus only be collected in a moist state. HN_3 possesses a fearfully penetrating odour, producing violent catarrh, and dissolves in water with an avidity reminding one of hydrochloric acid. The solution also bears a surprising resemblance to aqueous hydrochloric acid; for, on distillation a concentrated acid first passes over, and afterwards a more dilute acid of constant composition. The aqueous solution possesses the odour of the free gas, and is strongly acid to litmus. With ammonia gas, hydrazoic acid gas forms dense white fumes of the ammonium salt, N_4H_4 or $NH_4-N\Big\langle\begin{matrix}N\\ \|\\ N\end{matrix}$, a compound which is completely volatile below 100°, and which crystallizes, but not in crystals belonging to the cubic system, in this respect indicating its different constitution to ammonium chloride. The aqueous solution rapidly evolves hydrogen in contact with zinc, copper, iron, and many other metals, even when largely diluted. As in the case of hydrochloric acid, the silver and mercurous salts are insoluble in water, the others being generally readily soluble. As the acid possesses feebly reducing properties, solutions of many of its metallic salts, the copper salt for instance, yield precipitates upon boiling of compounds of the lower oxides of the metals. The barium salt, BaN_6, crystallizes from solution in large brilliant anhydrous crystals. With silver nitrate the aqueous solution of the acid or a soluble salt yields a precipitate closely resembling silver chloride in appearance. Silver nitride, $Ag-N\Big\langle\begin{matrix}N\\ \|\\ N\end{matrix}$, does not, however, darken when exposed to light, and is further distinguished from silver chloride by its fearfully explosive properties. During the course of his description at Bremen, Prof. Curtius placed a quantity of this salt less than 0·001 gram in weight upon an iron plate, and then touched it with a heated glass rod. A sharp and loud detonation resulted, and the plate was considerably distorted. The mercurous salt, Hg_2N_6, is likewise very explosive. The metallic salts are readily converted into ethereal salts by reacting upon them with the haloid ethers. The phenyl salt thus prepared,  $C_6H_5N<\begin{matrix}N\\ \|\\ N\end{matrix}$, is in every way identical with the diazobenzene imide, so long ago prepared by Griess.

A. E. TUTTON.

PROF. S. A. HILL.

THE last Indian mail of September brings us the sad news of the death of Prof. S. A. Hill, one of the best-known of that small band of scientific workers, to whom we owe our present knowledge of Indian meteorology. He has been struck down suddenly, in the full maturity of his powers, and in the prime of life, after a few days' illness which gave no reason to anticipate so fatal a result. The son of a clergyman in the north of Ireland, Mr. Hill, after studying in the London School of Mines, and taking the degree of Bachelor of Science in the London University, was appointed, in 1876, to the Professorship of Physical Science in the Muir College, Allahabad, and, shortly after his arrival in India, received the additional appointment of Meteorological Reporter to the Government of the North-West Provinces, in succession to Mr. John Eliot, now the head of the Meteorological Department of the Government of India. In these combined offices, Prof. Hill has laboured for nearly fifteen years. In such spare hours as he could dispose of amid the exacting duties of his educational appointment and the administrative work of his office, in a climate which is but little favourable to mental or physical exertion, he devoted himself assiduously to those original investigations which have made his name familiar to the meteorologists of Europe and America. On subjects dealing with questions of terrestrial physics, he published numerous papers of high value and much originality in the Indian Meteorological Memoirs, the Journal of the Asiatic Society of Bengal, the Austrian *Zeitschrift für Meteorologie*, and the *Meteorologische Zeitschrift;* and an elaborate memoir on some anomalies in the winds of Northern India, in the 178th volume of the Philosophical Transactions. In this memoir he boldly endeavoured to map out the distribution of atmospheric pressure over India, at a height of 10,000 feet above sea-level, and showed how this distribution, differing greatly from that at the earth's surface, explains much that is otherwise anomalous in the winds experienced at the lower level, and especially the dry land-winds which play so conspicuous, and occasionally disastrous, a *rôle* in the meteorology of India. To the pages of this journal he was also a not infrequent contributor.

Having regard to Prof. Hill's high powers and his single-minded devotion to the work, of whatever kind, that lay before him, it is somewhat sad to read the following passage in an obituary notice in the Allahabad *Pioneer*, evidently written by one who knew him well. It need hardly be said that the Government referred to is that of the North-West Provinces and Oudh; not that of India, nor of Bengal, the relations of which to their scientific officers are known to be of a very different character. The writer says:—"Many of our readers who will recall their late friend's clear and accurate mind, his knowledge and his powers of application, will feel with a sense of bitterness that men of his capacity are not meant for the service of a Government, which is not only always ready to pass them over for a joint-magistrate who has been unlucky in his promotion, but will maintain that the latter is the best man. Mr. Hill was, officially speaking, the most unfortunate man of an unfortunate service [the educational service of the North-West Provinces]; but, no doubt because he had a talisman always with him in his devotion to science, he was never embittered by his ill-luck. With none of the eccentricities of a disciple of science, but with all the modesty and virtue of that character, he will pass away from us respected by all, and much more than respected by all those who were privileged to know him with intimacy."

H. F. B.

JOHN HANCOCK.

AT the venerable age of eighty-four years this well-known British naturalist has passed away, and it would be an injustice to his memory not to recall in these pages the effect of his life-work on the zoology of this country. He seems to have inherited his natural history tastes from his father, who was in business in Newcastle in the early part of the century, but was apparently devoted to natural history pursuits; and, in company with other kindred spirits, was intent on working up the natural history of Newcastle and the immediate neighbourhood. Unfortunately the father died at the early age of forty-three, in September 1812, leaving a widow and six children, of whom the eldest was only eight years of age. Mrs. Hancock, however, carefully preserved the collections which her husband had formed, and it was doubtless due to her affectionate interest that three of her children—Albany, John, and Mary—pursued the study of natural history with such success. The subject of this notice, John Hancock, seems to have turned his attention to ornithology in particular, and as early as 1826 he commenced the study of the artistic mounting of animals, which, as Mr. Bowdler Sharpe has said, has made John Hancock's name a password wherever the art of taxidermy is mentioned. Those who remember the celebrated groups of mounted animals which Mr. Hancock sent to the Great Exhibition of 1851, will testify to the revulsion of feeling which his beautiful work created, and every real naturalist felt in his heart that in this way alone could art and nature be combined in a Museum, and the public properly instructed in a due realization of the beauty and symmetry of form which animals possess in nature—beauties which are not reproduced in a Museum gallery once in a hundred times. That Hancock's influence should have been so little felt by the authorities of the British Museum is a reflection upon the officers of this institution, who ought to have utilized the genius of their countryman in making the collection of British animals in the National Museum a model for all nations to envy and copy. Anyone who knew John Hancock, his untiring energy and his unassuming amiability, will vouch for the fact that, if the British Museum had wished to have a collection of native birds naturally mounted, and worthy of this institution, he would have been only too delighted to aid in the achievement of such a task. As it is, the Museum of his native town, which really seems to have appreciated his genius, possesses a collection of birds of which any nation might be proud, and now that he is gone, those Museums (like the one at Leicester, for instance) which have series of birds mounted by this true lover and *connoisseur* of birds in nature, are to be congratulated. Of late years it is true that our National Museum has trodden the path indicated by Hancock, and a vast improvement in its taxidermy has been the result; but it will be a long time before any Museum can show such a beautiful series of birds as that which John Hancock has mounted for the Museum of his native town. An excellent biography of this esteemed naturalist has been published in the *Newcastle Daily Chronicle* of October 13.

NOTES.

EVERYONE was sorry to hear of the death of Sir Richard Burton, the eminent traveller and Orientalist. He died on Monday morning at Trieste, where he had been British Consul from 1872. He was in his sixty-ninth year. Burton was one of the boldest and most successful travellers of his time, and produced a great impression on all who knew him by the wide range of his talents, and by his energy and manliness. His career as a traveller began in 1852, when he undertook the journey to Medina and Mecca, of which he afterwards wrote so fascinating an account. His journey with Speke in 1857, which led to the discovery of Lake Tanganyika, placed Burton in the front rank of explorers. He had previously made a successful expedition into Somaliland; and at a later period he did much brilliant work in various districts of Western Africa and in Brazil.

WE regret to have to record the death of the Rev. J. A. Galbraith. He died at his residence in Dublin on Monday. For more than half a century he was connected with the University of Dublin, where he graduated in 1840. In 1844 his distinction as a mathematician secured for him a Fellowship, and in 1854 he was chosen Erasmus Smith Professor of Experimental Philosophy, along with Dr. Haughton. Prof. Galbraith was the author of various excellent scientific manuals.

DR. ALEXANDER WILLIAMSON, who died at Shanghai on August 28, had for 35 years been a member of various missionary bodies, and in his earlier years had travelled far and wide over North China, at a time when the greater part of that Empire was unexplored. His "Journeys in North China" is still a work of interest and value, for he visited many districts which are even still far outside the ambits of the missionary and the traveller, and his great knowledge of China renders the work very instructive. But his main work in life was the establishment in Shanghai of the Society for the Diffusion of Christian and General Knowledge amongst the Chinese, which is, we believe, maintained by subscriptions from various missionary societies labouring in China. Up to the time of his death, he was the editor and chief manager of the Society. Under his superintendence some hundreds of cheap books and pamphlets on all branches of science and on literary topics, suitable to Chinese intelligence and Chinese pockets, have been issued by the Society. Usually these were compiled by specialists amongst the missionaries, but occasionally a book already published abroad would be altered to meet the circumstances of the new circle of readers, and published in Chinese. It thus comes about that if an intelligent Chinese, knowing no language but his own, desires to make a closer acquaintance with that Western knowledge and civilization of which he has probably heard so much—whether it be anatomy, zoology, botany, mechanics, steam, the history of Napoleon Bonaparte, the story of the American War, the tale of Robinson Crusoe, the telegraph, the principles of hygiene—he goes to Dr. Williamson's series of publications and selects what he wants, usually at the price of a few cents or halfpence. The Society under his care has in fact stood as an interpreter between the East and West, and has striven to give to the former all the best that the latter has to give in the way of intellectual and moral instruction. This is surely as beneficent a task as can engage the energies of any man, and in it Dr. Williamson appears to have been most successful.

THE Agent-General for the Cape of Good Hope invites applications from gentlemen of appropriate scientific training and experience, willing to proceed to the Colony for a term of years, there to fill one or other of the undermentioned posts under the Government, viz. :—(1) That of bacteriologist, to investigate the diseases of domestic animals, supposed to be caused by germs. The salary offered is £500 a year. A free first-class passage by steamer (including railway fare to port of embarkation) will be provided. (2) That of toxicologist, to attend chiefly to forensic cases and to investigate South African native plants having medicinal properties. The salary offered is £400 a year. A free first-class passage by steamer (including railway fare to port of embarkation) will be provided. Applications must be accompanied by testimonials, and by copies of any scientific publications the applicants may have issued; and should reach the Agent-General for the Cape of Good Hope (112 Victoria Street, London, S.W.) by November 15 next. They will then be submitted to the authorities in the Colony, with whom the appointments rest.

AT the meeting of the organizing committee of the Oriental Congress held on the 9th inst., at the British Museum, it was resolved that Prof. Max Müller should be invited to preside over the Congress. He has accepted the invitation. Sir Henry Rawlinson, who was to have taken the chair, has been compelled to retire on account of ill-health.

THE Council of the Institution of Civil Engineers have issued a list of subjects on which they invite original communications. For approved papers they have power to award premiums, arising out of special funds bequeathed for the purpose. The Council will not make any award unless a communication of adequate merit is received, but will give more than one premium if there are several deserving memoirs on the same subject. In the adjudication of the premiums no distinction will be made between essays received from members of the Institution or strangers, whether natives or foreigners, except in the case of the Miller and the Howard bequests, which are limited by the donors.

THE nomination list of proposed members of the Council of the London Mathematical Society, for the session 1890–91, which will be submitted to members at the annual meeting on November 13 next, contains the following changes:—Prof. Greenhill, F.R.S., to be President, *vice* Mr. J. J. Walker, F.R.S.; Dr. J. Larmor, Major MacMahon, R.A., F.R.S., and J. J. Walker, F.R.S., to be Vice-Presidents. The proposed new members are Dr. Hirst, F.R.S., R. Lachlan, and A. E. Hough Love, in place of Prof. W. Burnside, Prof. Cayley, F.R.S., and Sir James Cockle, F.R.S., who retire. At the same meeting the retiring President will read an address on "The Influence of Applied on the Progress of Pure Mathematics," and will present the De Morgan Memorial Medal to Lord Rayleigh, Sec.R.S., in recognition of his writings on physical subjects.

HERR J. DÖRFLER has successfully completed his botanical expedition to Albania, and has returned to Vienna. From Ueskueb he crossed Kalkandele to Waica, and accomplished the ascent of both the Kobilica and the Serdarica-Duran.

THE late Dr. Henry Muirhead, of Bushyhill and Longdales, Lanarkshire, gave directions in his will that his estate—subject to certain life-rent provisions and legacies—was to be used for the establishment and maintenance of an institution to be named the Muirhead College, "for the instruction and education of women in medical and biological science, where women might receive an education to fit them to become medical practitioners, dentists, electricians, chemists, &c." The trustees, having obtained probate, have had several meetings, and it is expected that in the course of a few months they will be in a position to announce the arrangements they have been able to make. As the estate consists chiefly of lands, its money value must in the meantime be more or less a matter of opinion. The trustees, however, are hopeful that £30,000 at least will be available for the College.

We are glad to hear of the continued progress of what is called the University Hall scheme in Edinburgh. Its objects are (1) to make a beginning of social residence among the students of the University, and (2) to associate with this the extension of University influence among the people. The movement was begun in 1887, chiefly by Prof. Geddes. The house in which the experiment has hitherto been carried on having always had its full complement of residents, another house—an old building of considerable historic interest—has been secured; and this was formally opened the other day by the Solicitor-General for Scotland.

At the eighth meeting of the Congress of Americanists, an interesting address on the peopling of America was given by M. de Quatrefages. He expressed a strong belief in the unity of the human race, and in the consequent facts that the original home of mankind must have been confined to a very limited space, and that the world as a whole has been peopled gradually by processes of migration. He holds that America, like Polynesia, was peopled by colonists from the Old World. The peopling of Polynesia, however, was effected, he thinks, during our Middle Ages, whereas the earliest migrations to America date from geological times.

Under the title of "The Partition of Africa," Mr. Stanford will shortly publish a small volume by Mr. J. Scott Keltie, dealing mainly with the events of the past six years, and their results. In an introductory chapter or two, Mr. Keltie will seek to show what has been the footing of Europe in Africa from the earliest times. He will endeavour to estimate the value of the shares of the various European Powers in the scramble, from the point of view of commerce and colonization.

Messrs. Simpkin, Marshall, and Co., have issued the fifth edition of Mr. Rowland Ward's "Sportsman's Handbook to Practical Collecting, Preserving, and Artistic Setting-up of Trophies and Specimens." In the same volume is included a synoptical guide to the hunting-grounds of the world.

The new number of the *Internationales Archiv für Ethnographie* (Band iii. Heft 4) opens with an interesting paper, by Dr. Ed. Seler, on old Mexican throwing-sticks. Prof. Houtsma contributes notes on some pictures which once served as illustrations of a Persian "Fālbook."

The University College of Wales, Aberystwith, has published the Calendar for its nineteenth session, 1890–91.

The Manchester Literary and Philosophical Society has issued the third volume of the fourth series of its Memoirs and Proceedings. Among the memoirs are the following: on the law of cooling, and its bearing on the theory of the motion of heat in bars, by Charles H. Lees; on the combination of hydrogen and chlorine, alone, and in presence of other gases, by Prof. H. B. Dixon, F.R.S., and J. A. Harker; on some applications of caustic soda or potash and carbon in the qualitative and quantitative analysis of minerals, by Dr. C. A. Burghardt; description of a new reflecting telescope and observatory at Bowdon, Cheshire, by Samuel Okell; on the flexure of a flat elastic spring, by Horace Lamb, F.R.S.; and on absorption spectra and a method for their more accurate determination (with eight plates), by Dr. A. Hodgkinson.

The American Association for the Advancement of Science has issued its Proceedings at the meeting held at Toronto in August 1889.

A brilliant meteor was seen in the northern hemisphere from Edinburgh on Saturday last at 3 a.m. Its advent is said to have been announced by a flash of light which illuminated the whole city. A long fiery streak marked its course, and remained visible for more than a minute.

A report from Honolulu states that an eruption of the volcano Kilauea is feared, as a lava stream has formed lately, and part of it rose 15 metres in one day.

A shock of earthquake was felt at Christiansand, on October 8, at 5.15 a.m. The shock was directed from south to north, and lasted 3 or 4 seconds.

A slight shock of earthquake was felt at Lisbon on the evening of October 17.

According to a telegram sent through Reuter's Agency from Catania on October 18, Mount Etna is in eruption. At the time when the telegram was despatched, a thick column of vapours was rising from the central cone. A slight shock of earthquake had been felt on the eastern side of the mountain at Giarre and its vicinity, where a shower of cinders had also fallen.

Probably the deepest mine in the world (according to *La Nature*) is that at Saint-André du Poirier, in France. Of its two shafts, one 3000 feet, the other 3130 feet, the latter is being sunk to 4000 feet. A remarkable feature of this mine is the comparatively low temperature found in it, never exceeding 24° C. In the gold and silver mines on the Pacific coast, with a depth scarcely half that of French mines, there is great difficulty in keeping a temperature low enough for work. In some parts of the Comstock mines the temperature reaches 48° C.

The Harveian Oration was delivered by Dr. Andrew on Saturday last at the Royal College of Physicians. In the course of the oration Dr. Andrew referred to the fact that the relationship between physiology and medicine has in many ways greatly changed during the last 250 years, and that such change is a necessary consequence of the progress made by physiology. "The goal of physiology is truth—*e.g.* perfectly trustworthy knowledge of a certain class of facts and laws; and this independently of any use, good or bad, to which that knowledge may be put. The goal of medicine is power—*e.g.* ability to manipulate certain given forces in such fashion as to produce certain effects. No doubt, theoretically, the two ends coincide, and we may hope in some remote future they will do so in reality and perfectly. For the present we must be content with having in one direction much knowledge which confers little or no power, and, on another side, very imperfect knowledge which yet brings with it very great power, too often ill-directed. Again, their methods are different. Physiology by slow degrees has come to rely more and more on purely scientific modes and instruments of research, and to apply them by preference to matters which can be brought to the test of direct experiment. Medicine, on the other hand, has no choice but to remain, so far as it has a scientific side, a science of observation; for anything like effective investigation of the matters with which it deals by direct experiment is impossible. As physiology slowly reduces to order the apparently hopeless confusion of so-called vital actions, the easiest questions are attacked and answered first, and thus those which have to be faced later in their turn are more and more difficult, more and more refractory to scientific analysis. Now, these more difficult questions are often of vital importance to medicine, and in them lie dormant vast possibilities of increased knowledge of the nature of disease, of increased power over it. And yet, from the great difficulty of subjecting them to experiment, physiology may seem for a time to fail us, and the task of employing physiological results to explain clinical facts, or to form the basis of rational treatment, becomes harder than ever."

The additions to the Zoological Society's Gardens during the past week include a Speke's Antelope (*Tragelaphus spekii* ♀) from Lake Ngami, South Africa, presented by Mr. James A. Nicolls, F.Z.S.; two Reindeer (*Rangifer tarandus* ♂ ♀), European, presented by Colonel W. B. Thomson, F.Z.S.; a Beech Martin (*Mustella foina*) from France, presented by Mr.

H. H. Sharland, F.Z.S.; two Herring Gulls (*Larus argentatus*), British, presented by Mr. Joseph White; a Common Chameleon (*Chamæleon vulgaris*) from North Africa, presented by Mr. V. H. Dudmesh; a White Pelican (*Pelecanus onocrotalus*), South European, deposited; a Bay Colobus (*Colobus ferrugineus* ♀) from West Africa, purchased; a Large Hill-Mynah (*Gracula intermedia*) from India, received in exchange.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on October 23 = 0h. 9m. 4s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 5046	—	—	23 57 36	+15 33
(2) G.C. 5050	—	—	23 58 52	+ 4 35
(3) 30 Piscium	4	Yellowish-red.	23 56 19	− 6 31
(4) δ Andromedæ... ...	3	Yellow.	0 33 24	+30 16
(5) α Andromedæ... ...	1	Bluish-white.	0 2 42	+28 29
(6) W Cygni	Var.	Reddish.	21 31 53	+44 53
(7) T Aquarii...	Var.	Reddish.	20 44 8	− 5 32

Remarks.

(1, 2) Neither of these nebulæ have yet had their spectra recorded. The first is described as "considerably bright; considerably large; irregularly round; very gradually brighter in the middle": the second as "pretty bright; very small; much elongated; very suddenly much brighter in the middle."

(3) A star of Group II., the spectrum being described by Dunér as "very fine." All the bands 2–9 are very wide, dark, and strongly marked. As the star is a comparatively bright one of this class, a detailed study of its spectrum should be made, special attention being given to the brightness of the carbon flutings, and the presence or absence of dark lines.

(4) Secchi thought this star had a spectrum of Group II., but Dunér and Gothard describe it as one of the solar type, the latter observer, however, stating that it approaches Group II. According to Dunér, D and *b* are strong and dark, and several other lines are distinctly visible. At the place of band 2 (the iron fluting) in Group II. stars there is only a narrow and feeble line. It seems probable that the spectrum greatly resembles that of α Tauri, but as the band in the red has disappeared, it is probably a step higher in temperature. A direct comparison with α Tauri, which can now easily be made, might lead to interesting results as to the changes brought about by an increase of temperature in such a star.

(5) A star of Group IV. The usual observations are required.

(6) There will be a maximum of this variable about October 25. The period is short (120–138 days), and the range is from 5·8–6·2 to 6·7–7·3. The spectrum is an exceptionally fine one of Group II., all the bands being very wide and dark. We do not yet know whether any variations of spectrum accompany the slight changes of magnitude of such a variable as this.

(7) The spectrum of this variable has not yet been recorded. It is one of considerable range (6·7–7·8 to 12·4–12·7), and the period is 203 days. As the magnitude at maximum is not small, the observation of the spectrum should not be difficult. There will be a maximum on October 27.

A. FOWLER.

PHOTOGRAPHS OF NEBULÆ.—The current number of *Comptes rendus* (October 13) contains a note by Admiral Mouchez on a photograph of the Ring Nebula in Lyra, obtained at Algiers Observatory by MM. Trépied and Rabourdin. The nebula was given an exposure of six hours, in two evenings of three hours each. The negative obtained is said to be very dense, and a positive copy, enlarged 64 times, has been presented to the Paris Academy. With respect to the photograph, Admiral Mouchez remarked:—"This image of the nebula is certainly the largest that has yet been obtained. It shows, in a very striking manner, the distribution of light in this curious celestial object. We see that a region of maximum light exists at each of the extremities of the minor axis of the elliptical ring. These two maxima are not equal, and in each of the halves of the ring the intensity of the light diminishes gradually up to the extremities of the major axis, where it has the smallest value. These are well-known characteristics of this nebula, and such as may be observed by means of ordinary telescopes. But the photographic observation teaches us other things. In fact, according to the work done at Algiers Observatory, when we photograph this nebula with increasing exposures, the nebulosity does not extend sensibly outside the ring, but spreads more and more towards the centre. On the other hand, when we observe the body in a telescope, we find that the central part of the ring is perfectly separated from the ring itself. The interior of the ring is therefore filled with a material difficult to see, but of which the existence is demonstrated in a certain manner by photography. In fact, the central nebulous star attains an intensity in the present proof nearly equal to that of the feeblest maximum of the ring.

"At the meeting of July 7, 1890, in presenting to the Academy a photograph of the same nebula obtained at Bordeaux Observatory by MM. Rayet and Courty with an exposure of three hours, I pointed out the probable existence of three, and perhaps four, extremely feeble stars which had never been previously indicated, and which formed an almost regular square around the central star in the dark part of the nebula. The existence of at least three of these very feeble stars is now demonstrated with absolute certainty, because of the long exposure, but in the enlarged image they are somewhat confused with the inner edge of the nebula."

At the same meeting of the Academy (October 13), M. B. Baillaud presented a plate of the region about the Ring Nebula obtained at Toulouse Observatory on September 8, 9, 10, and 11, with a total exposure of nine hours. The size of the plate was 9 cm. by 12 cm., and it exhibits about 4800 stars to the naked eye within an area of three square degrees.

STARS HAVING PECULIAR SPECTRA.—In *Astronomische Nachrichten*, No. 2997, Prof. E. C. Pickering notes that photographs of stellar spectra taken by Mr. S. J. Bailey, at Chosica, in Peru, show several stars having peculiar spectra. The following table contains the places of these stars, and a brief description of the spectrum of each:—

Star.	R.A. 1900.	Decl. 1900.	Mag.	Description.
	h. m.	° ′		
Cord. Gen. Cat. 7191	5 59·4	− 6 42	5·8	F line bright.
,, ,, 18859	13 47·7	−46 39	6·6	F line bright.
,, ,, 19737	14 29·2	−41 43	2·5	F line bright.
,, ,, 22855	16 48·4	−44 57	Var.	G and *h* bright.
Cord. Zone Cat. 3612	17 55·1	−32 42	9·0	Bright lines.
S.DM. −19° 4854	18 2·1	−19 25	9·6	Bright lines.
Anonymous	20 9·4	−39 29	Var.	Bright hydrogen lines.
Cord. Gen. Cat. 29232	21 13·6	−45 27	6·0	Type IV.

The spectrum of the two stars with "bright lines" is similar to that of the stars discovered by Wolf and Rayet in Cygnus.

The two variable stars in the above list are new. Their discovery resulted from an examination of photographs of stellar spectra at Harvard College Observatory. A comparison of the intensity of the spectrum of the first-named star, situated in Scorpio, with that of others on the same plate, indicated that it fluctuates between magnitudes 7 and 11·4. A similar comparison of the spectrum of the latter variable, situated in Sagittarius, with the spectra of other stars near it, shows that between May and October 1889 it decreased from 8·5 to 10·7 magnitude. Both the stars have spectra of the same character as Mira Ceti and other known variables of long period.

THE PHOTOGRAPHIC CHART OF THE HEAVENS.—The International Committee of the Photographic Chart of the Heavens, will meet at Paris Observatory on March 31, 1891. The last details as to the execution of the work will then be discussed, and it is hoped that all the participating Observatories will be able to begin operations immediately afterwards.

D'ARREST'S COMET.—In the same journal Prof. Krueger points out that the comet discovered by Mr. Barnard of Lick Observatory on the 6th inst., is identical with that of the periodical comet of D'Arrest, for which Dr. Berberich computed an ephemeris (*Astronomische Nachrichten*, No. 2959). An observation at Strasburg on the 10th inst. confirms the identity.

A NEW ASTEROID.—Dr. J. Palisa, of Vienna Observatory, discovered a new minor planet (299) on the 7th inst. Its magnitude was 14.

THE TEACHING OF BOTANY.[1]

THE discussion was opened at great length by Prof. Marshall Ward, who reviewed the whole subject of teaching botany (1 to very young children and in schools, (2) as an academical study at the Universities, and (3) as a special subject for those who are in training for technical and other pursuits which require a knowledge of that branch of science—*e.g.*, foresters, gardeners, timber merchants, &c. He said :—

As I understand it, we may regard the study of botany as approachable from three points of view. We may speak of three ends to be attained : those of (1) elementary botany as a school subject of general education ; (2) advanced botany, as a subject of University or academic training, with a view to teaching and research ; (3) special botany, for various purposes in after life—*e.g.*, those of foresters, planters, agriculturists, horticulturists, brewers, medical men, timber merchants, &c.

This is, of course, a merely arbitrary division for the argument, and not a philosophical classification of the subject-matter of the science of botany.

The next point is the scope of the teaching in each case. I should advocate that all children pass through the preliminary training embraced under No. 1. Not only so, but I would urge the usefulness and importance of elementary botany in schools quite apart from its possible pursuit afterwards.

It seems to me that the time is gone by when we need discuss the direct applicability of teaching in elementary schools : if school training is read to mean education, in the true sense of the word, then there is no necessity for asking that a boy and girl should learn at school only those subjects of which they will make direct application as they grow older. Of course this does not preclude our keeping in mind the relative utility of the various subjects to be taught, but it does—and emphatically—preclude our falling into the error of imagining that a school-subject is of educational value only in proportion to its direct and foreseen utility in the application afterwards. In other words, educating and teaching may be, and often are, very different things.

Now, as I understand it, the nineteenth century has discovered—possibly re-discovered—the truth, that you may impart a wondrous amount of information to a boy or girl without awakening those powers of observing and comparing that lie dormant in the minds of most healthy human beings, and especially when young ; and that many a brilliant boy grows up without being able to draw correct inferences from the phenomena around him, and therefore less able than he should be to hold his own in the world he awakes in.

The peculiarity of the study of elementary botany, properly understood and pursued, lies especially in the interest it arouses in the child's mind, and the ease with which it may be taught, and I would insist and re-insist on the fact that it stimulates and cultivates just those powers of accurate observation and comparison, and careful conscientious recording of the results, which are so needed by us all ; and which, be it understood moreover, come so naturally to children who are not too much under the baneful influence of the mere instruction—the mere information—system.

What I wish to emphasize is that the educational value of this subject is no more to be measured merely by the number and kind of *facts* which the child remembers, than is the educational value of history to be measured by the dates learnt, and the lists of kings and battles committed to memory. History, reading and writing, arithmetic, and other subjects, have an educational value, if properly taught, quite apart from their value as mere accomplishments, which may be granted ; but children are naturally observers, and why this side of their hungry little natures should be starved at the expense of their usefulness in after life has always been a mystery to me.

To those who allow this, and I am happy to see that their numbers are now many, it should hardly be necessary to point out that the elements of botany afford the cheapest, cleanest, and most easily attained means of cultivating in children the powers of observing and comparing direct from Nature, and of leading them to generalize accurately.

Of course no advocacy is needed for good preliminary education in elementary botany in the case of those who are about to continue the pursuit of the subject as an academic study, or for a special purpose, as noted under the headings (2) and (3) ; but a few words may be devoted to pointing out the shocking waste of time and energy, on the part of all concerned, in the prevailing cases where students come up to a University, or other institution for higher education, insufficiently prepared for progressive study.

It is still true that boys and young men leave school without so much as a notion of the real meaning and aims of science : this applies no less to subjects like physics and chemistry, which are professedly much taught in schools now, than to subjects like natural history and botany, which, though avowedly in the curriculum of some good schools, are usually entirely ignored.

There is considerable discussion about the details, but many practical teachers regard such subjects as unfitted for school, because the boys and girls soon cease to be interested, and get lost in the masses of facts and hard names that beset their path : this, to my mind, simply shows where the whole system is wrong, and wrong because the tyrant empiricism still rules the prevailing methods of teaching in schools.

I shall go so far as to say that the only remedy for this state of things is for the teachers to lose that blind worship of facts, as facts, which dominates our school system. I am aware that this lays me open to very serious misconstructions, but I hope to make that all right in the sequel.

I would say to the teachers, therefore, do not fall into the mistake of measuring a boy's progress by the amount of dogmatic information which he imbibes, and splutters forth on to his examination papers, but look to the quality of his understanding of the relations between relatively few and well chosen facts ; and again, pay less attention to the number of facts which a boy observes and of names he remembers, and more to the way in which he directly makes his observations, and intelligently describes them, even if untechnically.

This is, I firmly believe, the only cure for the malady under consideration—*i.e.* it is the prevention of it.

Children in schools are taught most subjects from printed books, and it is not my province to criticize the necessity of this as regards those subjects ; but let a competent teacher try the experiment of making the children read directly from Nature, and he will soon see that the new exercises have a powerful effect. They will stumble, and they will even make stupid mistakes and mispronunciations ; but do they not do so when they are reading—*i.e.* observing and comparing and interpreting—printed words in a book ? Of course they do, and therefore the teacher must not be discouraged by their stumbling and misapprehending when first they have to look at and compare different leaves and flowers, and give forth the articulate sounds which correspond to the impressions created on their minds.

Every weary teacher knows what a blessing is variety in the studies of the class, and it passes my comprehension why advantage is not taken of the splendid opportunity offered by the study of elementary observational botany.

We now come to the important subject of method. How should botany be taught ?

Here, again, I shall consider the subject from the same three points of view referred to above.

(1) Elementary botany in schools should be confined to lessons in observation and comparison of plants, and the greatest possible care should be taken that books are not allowed to replace the natural objects themselves. Indeed, I would go so far as to advise that books be used only as an aid to the teacher, were it not that a judiciously written text-book might be employed later on by even young children as a sort of reading-book.

The chief aids should be the parts of living plants themselves, however, and, in spite of the outcry that may be expected from pedantic town teachers, I must insist that every school might be easily provided all the year round with materials for study. I even venture to think that these materials might be collected by the children themselves : at any rate there should be no difficulty about this in the country.

I will illustrate these remarks by a few examples. The teaching of elementary botany to children should commence with the observation of external form, and might well be initiated by a comparative study of the shapes of leaves, the peculiarities of insertion, their appendages, and so on.

The point never to be lost sight of is that if you teach a child to discriminate, *with the plants in hand and from observation only*, between such objects as the simple, heart-shaped, opposite, ex-stipulate stalked leaves of a lilac, and the compound, pinnate, alternate, stipulate leaves of a rose, you lay the foundations of a power for obtaining knowledge which is in no way to be measured

---

[1] Discussion at the Leeds meeting of the British Association, in Section D, on September 5.

merely by the amount or kind of information imparted. It does not matter whether the child learns the trivial facts mentioned above, or not, but it is of the highest importance that the child be taught how to obtain knowledge by such direct observation and comparison; and the beauty of it all is that, as is well known, the child will retain most of such information as mere matter of course.

For the main purpose in hand, therefore, it may be contended that any objects would do.

This is no doubt true in one sense, but it should not be forgotten that (1) the mental exercise on the part of the child is best exerted on *natural objects*, to say nothing of the admitted advantages of familiarizing him with Nature, and (2) the parts of plants are so varied, so beautiful, and so common, that he need never lack materials for his simple and pleasant work. Moreover, the parts of plants are clean, light, and easily handled—practical advantages which recommend themselves.

I feel convinced that, if the teachers were not opposed to it, the subject would ere now have been more widely taught; and I shall therefore say a few words in anticipation of difficulties. It has been suggested that materials would be scarce in winter. Not at all. Let the children be familiarized with the observation and comparison of the peculiarities of a sprig of holly as contrasted with one of ivy; or let them be shown how different are the buds and leafless shoots of the beech from those of the oak or the horse-chestnut. Show them how to observe the bud-scales, how to infer the leaf-arrangement from the scars, how to notice the colour, roughness, markings, &c., of the periderm. Or give them introductory notions as to the nature of a hyacinth-bulb as contrasted with a potato-tuber, confining their attention to points which they can make out by observation. Every nut or orange or apple that a child eats might be made interesting if teachers would dare step over the traces of convention, and introduce such ostensibly dangerous articles into class-work—and why not? The doctrine of rewards and punishments is applied more crudely than this in most children's schools!

Be this as it may, there is no lack of material at any season, for children to observe and compare, plant in hand, the peculiarities of shape, colour, insertion, markings, &c., of the leaves, stems, roots, and other parts. The difficulties are supposed to increase when the flower is reached: this is not necessarily the case in the hands of a sympathetic teacher, unless the choice of flowers is very unfortunate and limited.

There is one danger to be avoided here, however. Young children should not be troubled with the difficulties of theoretical morphology: they should be made familiar with the more obvious roots, stems, leaves, tendrils, thorns, flowers, bulbs, tubers, &c., as such, and comparatively, and not forced to concern themselves with such ideas as that the flower is a modified shoot, the bulb a bud, the tendril a leaf or branch, &c., until they have learned simply to observe and compare accurately. Later on, of course, the step must be taken of rousing their minds to the necessity of drawing further conclusions from their comparative observations in addition to recording and classifying them; but if the teacher is really capable of teaching, it will be found that the children begin to suggest these conclusions themselves, and, this stage once reached, the success of the method is insured.

Glimpses of the meanings of adaptations of structure to function soon follow, but they should be obvious and simple at first, and the mistake should not be made of entangling a child in a discussion as to more remote meanings. It should never be forgotten, in fact, that the first steps consist in learning to observe accurately and to record faithfully, comparative exercise being used in addition, both as a check and as a stimulus to the judgment.

The next step is to introduce the methods of the systematic botanist who works in the field, with flower in one hand and lens in the other; and the necessary preliminary and accompaniment of this is to exercise the tyro in describing common plants as a whole. The value of such training in the field can scarcely be over-estimated. As education it is excellent, for it inculcates neatness and accuracy of method, keenness of observation and judgment, and is, moreover, interesting to the young student, as well as healthy in every sense of the word. As preliminary training in all cases where the student will have to pursue the higher branches of botany, or other science, at a University or a technical institution, it is absolutely necessary. There is no need to enlarge on its value to the traveller, the philosopher, and even the *dilettante* who enjoys Nature in his garden, or in the country, or even merely as a reader of books on natural history: just think what enjoyment such a training would add to the lives of thousands who have read Darwin's works imperfectly, and reflect for a moment on what such intelligent appreciation of such writings means to a nation like ours.

(2) The necessities of the higher academic study demand previous acquaintance with the *facies* of a large number of plants—Cryptogams as well as Phanerogams—and it is on this account advisable also that the student has been well trained in field-work: he should, then, be familiar with terms and groups, and be able to observe and compare.

Two chief lines of instruction are open at once to the advanced student, and the first point for discussion is, how far they should be kept separate or together: they are morphology and physiology, for, say what we will, the two are separate studies in their aims and methods.

It is not improbable that the study of pure morphology may be carried too far, as an independent study, and that one-sided views of the nature of plants and their parts may result; but, however true this may be, I take it no botanist will deny that every student should know something of the attainments and aims of modern morphology. If this is admitted, the next point is not likely to be gainsaid—namely, that the study of morphology depends on the study of anatomy and histology, as well as upon that of external form. As we shall see, the same is true, but in a different way, of physiology; but I am concerned at present with morphology only.

It seems to me, in view of these facts, that the advanced teaching must presume an acquaintance with the elements of anatomy and histology; and here, again, I am convinced that if teachers fully recognized how clean, and light, and easily accessible the material is, and how excellent the training of hand and eye on the one side, and of the thinking powers on the other may be made, the difficulties of introducing this elementary laboratory work even into secondary schools would be overcome.

It has been overcome in many cases with regard to chemistry, and there is no reason why it should not be overcome with regard to botany.

However, be it as advanced work at school, or as elementary work at college, the student who proposes to pass on to the higher academic study of botany must face the truth that even an extensive knowledge of the outside forms of plants will not carry him far on the road to be traversed.

Now comes the question hard to answer—Should he study anatomy and histology by selecting the best known and clearest tissues, tissue-elements, &c., from any part of the vegetable kingdom; or should he choose some one plant, and explore the recesses of its structure as thoroughly as possible?

All things considered, I believe the introduction is best effected by the latter method, and for the following reasons. In spite of the drawback that no one plant can be found which shows every tissue or tissue-element at its best, one finds that, by exploring the structure of some one plant as thoroughly as possible, the thoughtful student obtains a better idea of the co-relations of the structural elements than if he seeks for xylem vessels in Maize, sieve-tubes in Cucurbita, collenchyma in one plant, sclerenchyma in another, and so on.

Moreover, the comparative survey can be better carried out, if time permits, by methods such as I advocate.

The next consideration is the selection of the type to be used as a basis. In spite of all its defects, and in anticipation of severe criticism, I maintain that the fern is, on the whole, the most useful and convenient type for the purpose.

No Thallophyte is sufficiently obviously complex in structure to give the student the necessary ideas of co-relations of parts and division of labour; moreover, the lower forms offer peculiar difficulties of observation, cultivation, &c. The moss is too specialized for some purposes, and not sufficiently complex for others. The Phanerogams, on the other hand, although they present the vegetative tissues, members, &c., in the more highly developed and specialized forms familiar to physiology, offer such stumbling-blocks to the tyro in morphology that no one will serve as a suitable type. The pine is the best of those proposed, but even it presents great difficulties to a beginner.

The disadvantages of the fern (taking *Aspidium*) embrace the following: its roots are fine, the stem is short, and the vascular bundles belong to an out-of-the-way type; the spores take a long time germinating, and the prothallus offers difficulties in the way of investigation not easily overcome by a school-boy.

On the other hand, the roots are fairly typical in structure, and introduce the student to the ideas of the root-cap, apical cell, radial bundles, and axial vascular cord. The stem, at least, shows how the vascular bundles have definiteness and continuity of course, in axis and appendages, and these bundles are so large and isolated that an introduction to the notion of their development from embryonic tissue is at least attainable; moreover, the spiral vessels, scalariform tracheides, sieve-tubes, and packing-cells suffice very well—though, of course, in different degrees—to introduce the elements of the xylem and phloem, and I regard it as an advantage to defer the complex idea of cambium.

Elementary notions of other items of complexity appear in the extra fascicular strands of sclerenchyma, while protective hairs, reserve-starch, continuity of leaves and axis, and their origin from the meristem, &c., all serve as foundation stones if properly demonstrated and discussed by the teacher.

But it is the sporophyll on the one hand, and the prothallus on the other, which make the fern so supremely useful as a type. No conceptions in the morphology of plants have been more fruitful than these, and it is of the highest importance that the student really sees and examines these and their accessories for himself.

The beauty of the fern sporophyll as a type for demonstration lies in its being so evidently a leaf, in the sense understood at once by the beginner; then the sorus, sporangium, and spore are evident and easily examined, and even the very useful ideas of the archesporium, tapetum, and the development of the spore can be mastered in the case of the fern with comparative ease.

As for the prothallus, it is admitted to be the most accessible of all, and advantages may be claimed for its independence as a chlorophyll-bearing structure, in spite of its flatttened and somewhat specialized form. The antheridia are curious, no doubt, but the spermatocytes and antherozoids and their development are easily made out so far as general features are concerned: the archegonia are not so typical, perhaps, as those of the moss, but they are sufficiently so to be very useful, and the oosphere, canal-cells, &c., are easily seen by an apt student.

Moreover, I would point out that in the hands of a properly guided student of average intelligence, the teacher can rely upon the fern prothallus for introducing some theoretical notions very difficult to acquire—*e.g.* the gradual separation of the sexual organs, and their withdrawal into the prothallus, and the eventual separation of male and female prothallia, and *their* reduction and withdrawal into the spores, leading to the final specialization of male and female spores, and *their* retention and reduced germination inside the sporophylls, which also become specialized.

I should explain here that I would not propose to carry this explanation of homologies too far at this stage, but my argument is that the foundations for much that is to follow can be laid now with better effect than at any other time. It may be contended that the elementary student cannot possibly understand the Hoffmeisterian morphology until he has mastered the structure of the ovule of the Phanerogam, and that, therefore, it makes no difference in this respect whether he begins at the one end or at the other. I grant this, but my plea is not for the crowning of the student's knowledge of morphology, but for the *foundation* of it, and I lay so much stress on his laying this foundation thoroughly—otherwise it will not bear the weight of the superstructure I should propose to raise on it—that I look for the best type for that purpose; and, bearing in mind that such a type must be convenient, and one wherein the student can find the objects and examine them himself, I believe it has been found in the fern.

It will no doubt be remarked that, in the preceding discussion, I have kept in view more especially the study of morphology as the aim of the young academical botanist, and that it is because the fern is so excellently situated midway in the vegetable kingdom that it forms so good a type for teaching purposes. If it is urged, however, that physiology is the study to be more especially kept in view, then it may be necessary to reconsider the question of a type.

But there are two reasons, to my thinking, for discarding the idea that the study of physiology should be the immediate aim of botanical teaching in schools at present, though I do not despair of its introduction in the near future.

Firstly, the appliances needed, simple as they are in most cases, nevertheless *are* appliances, and will, as matter of fact, bar the way to the study during school life for some time to come; secondly, however much we may insist that the study of the physiology of plants presents its own problems and phenomena apart from those proper to physics and chemistry—and no one can urge this more earnestly than I do myself—nevertheless it cannot be gainsaid that the student of physiology should have a fair acquaintance with elementary physics and chemistry, even at the outset. I am aware that the contrary has been asserted, and that it has been argued that a student may learn to rig up apparatus for demonstrating the respiration of germinating seeds without knowing anything about the properties of oxygen, or what happens when carbon dioxide passes into a solution of barium hydrate, and that he may perform experiments on assimilation knowing no more about starch than that it turns blue with iodine, or on transpiration without understanding anything of the physics of the atmosphere or of water; and I am not prepared to say that such training would be without benefit, but apart from the advantages of the preliminary knowledge of phenomena, every teacher knows how dull is the comprehension of the boy's mind when brought face to face with such experiments devoid of the necessary physical concepts, as they have been termed; and in any case the necessary minimum of physics and chemistry will have to be instilled at the time the experiment is performed.

Secondly, the study of histology—practical acquaintance with the microscope—is a necessary preliminary to physiology, and I am doubtful whether we are at present in a position to demand more than the beginnings of these matters from the schools, though the time will come when it will be disgraceful for a boy to leave school quite ignorant of them.

The study of the fern should be followed by that of the *pine*, and I am not prepared to demand a continued adherence to the type-system beyond this point, except under special and favourable circumstances, such as need not here be discussed. Indeed, I should be quite satisfied if we could depend on school-children learning how to describe plants fairly accurately, and on the boys and girls in secondary schools knowing something more of field botany and how to use a flora, and having a satisfactory acquaintance with the life-history and structure of a fern and a pine. When I speak of field botany as above, it is not intended to exclude an acquaintance with the external appearance of common Algæ, Fungi, lichens, and mosses, &c., though the extent of that acquaintance would necessarily depend upon circumstances.

It must not be overlooked, however, that somewhere between this stage and that of further progress to the higher departments of academic botany, the student will have to do some comparative anatomy and histology, on the one hand, and to master the details of the life-history of certain types of Algæ, Fungi, and Lichens, Muscineæ and Vascular Cryptogams, and look more deeply into that of the Phanerogams.

It depends on circumstances whether the type-system should be followed here or not. If the student is going to specialize in the direction of morphological botany, I am inclined to the opinion that he should steadily pursue the type-system, supplementing his work with comparing special structures selected from allied types as he proceeds. For instance, after working through the life-history of a *Pythium*, he should not need to devote his attention to actually exploring all the details in the life-history of *Mucor* and *Peronospora*, but he should see the sporangia of these, and the haustoria of *P. parasitica*; and again, having worked through the chief stages in the life-history of *Marchantia* and *Funaria*, say, there is no need to insist on the same pursuit of detail in the case of other Muscineæ, but the student might compare with the corresponding structures in his types the sporangia of *Anthroceros* and *Jungermannia*, &c., the leaves of *Sphagnum* and *Polytrichum*, and so on.

If the student is more inclined to the pursuit of physiology, I should prescribe a different course as soon as he has examined a few types of Algæ and Fungi, a moss, and a few Vascular Cryptogams, and I should, moreover, direct his attention at once to the highest plants—the Angiosperms—instead of leading up to them as in the case of morphological studies.

In fact, the system to be pursued for a training in physiology, is to select the best illustrations of the organs, the tissues, and the histological elements of which the functions are to be studied. For the typical root I should go to one plant, but it might be necessary to employ quite another plant for showing root-hairs or root-cap: while selecting the vascular bundles of *Ranunculus repens* or of *Aristolochia* to show certain facts about the bundles as a whole, I might take those of *Cucurbita* for sieve-tubes, those of *Linum* or *Vinca* for bast-fibres, and those of quite other plants for spiral or pitted vessels, &c.

So also with other structures, the training is designed to familiarize the student with the best examples of each structure, and although he must acquire a sufficient insight into the relations of these structures and parts to be able to understand how they work together, and how the functions of some depend on those of others, still his aim is not to follow out their development and relations in space and time, but to deal with their behaviour now and in the mature plant.

Up to a certain point both morphologist and physiologist must work along the same lines: they then diverge, and it is at this period that the more extensive use of books must come in; for the student should now have so *real* a knowledge of the things discussed, that illustrations and information are clear to his understanding. The intending physiologist must put himself in possession of sufficient histology and anatomy to be able to follow the work of the specialists in this domain, and to see what bearings their discoveries have on his branch of investigation: no less must the morphologist follow the special literature, but with his own very different end in view. Both will, of course, have their special literature also.

However, it is obvious that we have now reached a point where no very rigid rules can be laid down, since the advanced academical student is in a position to strike out his own lines, and if he does not display some originality now in his methods, aims, &c., the presumption is that no amount of training on the part of teachers will lead to it. Nay, more than this, it is highly desirable that he should be left alone, for the dormant originality is as likely as not being kept down by the pressure of prescribed studies.

(3) In illustration of what is required in special branches of botanical study, I cannot do better than take the case of the properly-educated forest-student: go where you may, you are not likely to meet with a more representative "practical man" than the trained forest-officer, and consequently his case is peculiarly well adapted for my present purpose.

No one will be so rash as to argue that the botanical training of a forester should err in subordinating a knowledge of trees and wood, the phenomena of germination and nutrition, of growth, &c., to transcendental hypotheses and discussions on the nature of morphological conceptions or on abstruse questions as to the significance of movements of irritability, or the ultimate mechanism of reproduction and the molecular forces concerned in heredity: on the contrary, most people will concur in agreeing with me that the teaching of forest botany should be directed to laying down in the student's mind a good foundation of facts of observation, and showing him how to acquire others, and, further, to training his mind to reason accurately from these facts, so that he may apply his reasoning to the practice which is to be his life's pursuit.

On the other hand, there is a danger which very few people escape when talking on this subject, and that is the danger of supposing that the attention of the forest-student should be confined simply to acquiring and remembering aphoristic statements of facts, and that his accomplishments in this connection measure the fitness of his training. In other words, many so-called "practical men" argue that it is the *quantity of information* which tests the student's progress, and neglect the truth that progress is much more adequately represented by the *quality of the instruction.*

Let us put the case in another way. It is granted that the forest-student must be made acquainted with certain facts of observation, and that he must be informed of important conclusions derived after comparing these facts: it is also granted that his time for training is limited—there is no getting over this, and we need not discuss what the limits are, or why they are so. Now, the problem is, Shall the student devote the whole of this period of training to simply acquiring as many of these facts as possible, the conclusions being limited to those directly applied in the forest; or shall more attention be devoted to the methods of acquiring these facts and of drawing the conclusions from them, and the facts themselves be utilized rather in so far as they are necessary for the training, than as the ultimate aim of that training?

The answer to this question is of the highest importance. If we decide that the chief object of the forest-student's training is to make himself acquainted with the facts themselves, then his whole time will have to be given to such matters as learning the names of plants; the peculiarities of the roots, bark, wood, buds, leaves, &c., of the various trees; the empirical facts as to the relative amount of light, moisture, &c., and the degrees of temperature that each species will bear, and so on; the ascertained growth in height of each species, and the annual increment it exhibits, and so on. It is obvious that, if the student worked continuously for his two years or so of probation, he could make himself or be made acquainted with an enormous mass of such information, but it is equally obvious that he could not nearly exhaust the catalogue of facts. The latter truth becomes still more apparent, however, when we remember that he has to devote his attention to several other branches of study in addition to botany.

But is this the right decision to come to in face of the problem I put before you? I say no! emphatically no! On the contrary, it should be recognized at once that the forest-student cannot acquire more than a small proportion of the facts of his subject while he is in training, and even if he could they would be of no use to him in this shape. The selection being limited, then, it should be the aim of the teacher to direct the student's attention to a selected number of facts (you need have no fear that the list will be a short one) such as throw light upon matters that the student will not be likely to explain for himself, unless he is directed. The facts of the forest will be before him always; why, then, occupy the valuable time of training with an incomplete catalogue of them? There are thousands of other points, however, that he will never know anything about if he does not learn how to observe and infer them while he has the chance with a competent teacher by his side.

Let me give an example. The details of the different modes of germination of the various seeds of trees are numerous, but they can be collated under a few heads. Some seeds, like those of the beech, raise their cotyledons above the surface of the soil, and they become green and expand; others, like those of the oak, remain underground, and devoid of chlorophyll, and do not expand. As sown, however, the beech-mast and acorns are not seeds, but fruits, for each is enclosed in its pericarp. Both agree in having two cotyledons to the embryo; and although the beech seed contains a thin remnant of endosperm, both are usually termed exalbuminous; moreover, the cotyledons have their cells crowded with food-materials consisting chiefly of starch-grains and oil.

The seed of a date-palm, on the other hand, is provided with large stores of food-material in the form of cellulose, as thickening materials to the cell-walls of the endosperm, and it contains a relatively minute embryo, furnished with one knob-like cotyledon only; while the seed of a Scotch pine has a large, fatty endosperm, and a poly-cotyledonous embryo in its axis. The details of germination of the palm and the pine differ, and both in different ways from those of the beech and the oak.

Now it is unquestionable that the forester ought to understand what are called the phenomena of germination; but the inquiry arises, Do we mean by this that he ought to learn the details of the germination of these and a large number of other seeds, or do we mean that he should be made acquainted with what research has shown to be common to all seeds, and then with the chief classes of difference in detail? In other words, is he to be taught generalizations, and shown by a few well-selected examples how they have been and are being arrived at; or is he to be burdened merely with the details themselves, as stated in the words of and on the authority of others? Undoubtedly the former is the true method: the latter is simply empiricism.

Let none fear that the student who is thus taught will learn too few facts—the fetish of the "practical man."

In the first place he cannot proceed without sufficient information to enable him to understand the physiological value of such bodies as starch, cellulose, oils, and proteids; and, without troubling him with the refinements of micro-chemical methods, he will at least have to be made acquainted with the better-known changes which these bodies undergo in the presence of water and oxygen, and with the metamorphoses comprised under metabolism; and here his botanical knowledge comes into intimate relations with his information on elementary chemistry.

But, further than this, how is he to proceed to an understanding of even the outlines of the physiology of germination until he knows the leading phenomena of fermentation on the one hand, and of respiration on the other?

I will not enlarge upon this part of my subject however, but simply assure those unacquainted with the full bearings of these remarks, that there is no paucity of facts in this connection, and that, simply to make himself acquainted with the more salient ones, the student has to devote many hours of careful study in the laboratory.

But he will not understand the process ot germination unless he is acquainted with the structure of the seed. Here, again, it is not the details of structure of the seed-coats, the nucellus, and the embryo, which differ in each seed taken, that are to tax his memory and disgust his mind, but he must be made familiar with the leading features common to all seeds, and illustrated by a few selected examples. The nature of the seed-coats, the structure of the embryo and its relations to the endosperm, &c., are easily taught, if the teacher knows his art, and the pupil is properly led up to his work; otherwise, I fail to see how the latter is to gain any idea of what a seed is on the one hand, or of how a tree arises from the embryo on the other, and if he does not understand what a seed is, he will never comprehend the process of germination, and he thus misses the best chance of elucidation as to the development of the complex structures of the root, stem, and leaf, &c., which follow.

I have said nothing of the phenomena of growth, moreover, and yet the problems of germination will remain obscure and unintelligible until the student knows something about growth; and this presupposes at least some notions as to the phenomena of cell-division in the embryonic tissue, and of cell-growth and development.

Why say more? It is obvious that these studies lead the one to the other, and the real difficulty is to select the best illustrations and use them to the best advantage.

The forest-student's curriculum, therefore, is not to be regarded as a *narrow* one because he needs only a catalogue of facts, but as a *special* one because the exigencies of his professional time demand his attention to certain classes of phenomena. His early training—would that it began at school—should be in the observation and comparison of plants and their organs: he should then proceed to more comprehensive field-work, and exercises in the description of plants and systematic botany. In selecting his examples special attention should be paid to trees and shrubs, which are commonly neglected by students, and the lens should be always at hand.

Studies in the elements of anatomy and histology must follow, otherwise his progress will be hampered when he has to deal with the subjects of germination, nutrition, growth in thickness and formation of wood, cortex, bark, &c.

Refined histology, special anatomy, and speculative morphology will have to be neglected, nor must he aim at becoming a specialist in taxonomy. His laboratory work must be directed to the end that he may understand the general structure and relations of tissues and organs, otherwise he cannot understand what is known of their functions; that he may have clear ideas as to the parts which yield economic products, otherwise he becomes lost in the long catalogue of these; that he may grasp the salient features in the structure of the different kinds of wood, otherwise he cannot attempt to classify and identify them; that he may know something of the biology of fungi, otherwise he cannot hope to understand the diseases of timber which they cause, or the important scavenging and other work which they perform in the forest, and so on.

It would take too much space and time to enlarge on the pity of the fact that young forest-students come up for training almost totally unprepared for such a curriculum, and especially devoid of the elementary knowledge and powers of observation which they should have received at school: the consequence is, much of their valuable probation period is occupied with acquiring the elementary facts and methods without which they cannot possibly make progress in more special work. Now I should like to see all this altered, and the only way to effect the necessary salutary changes is to have some guarantee that such probationers have a suitable training in elementary botany while they are in the receptive condition of school life.

Let me now suppose the case of a young man destined for a career as a brewer. No one will deny that an essential part of his training should consist in a thorough schooling in the methods of cultivating and separating the various forms of yeast, bacteria, and moulds which are met with in every corner of a brewery, and some of which are the agents on the proper action of which he depends directly, while others are his enemies —for I need not remind you that the fermentation industries all depend on various yeasts, and that the diseases of wine and beer, &c., are due to the interfering action of other microscopic organisms of the nature of yeasts, moulds, and bacteria.

This is all clear, and generally accepted, but I am not so sure that everyone recognizes the fact that the proper study of these fungi and allied organisms is a department of botany; though I am quite sure that many people suppose that it is the province of the chemist to clear up the mysteries of these agents of fermentation and putrefaction.

It requires long practice with the microscope and with botanical methods of investigation to trace the vagaries of even the largest of these ferment-organisms, however; and without implying in the least that some of the methods and results of modern chemistry are not essential in such investigations—for the contrary is really true—I would urge the absolute necessity of a botanical training before the student can grasp the meaning of the problems to be solved.

It is surely childish to reply that the special technical methods of the brewer's microscopist can be acquired without the preliminary training in botany which is here pleaded for. I know they can be acquired, as merely technical processes, and I do not deny that relatively good work has occasionally been done under such conditions by men of genius and industry, who have acquired the botanical knowledge as they proceeded; but the point is that the technologist who has had no training in botany is found groping over problems in a manner he would never have had to do had he a proper view of the nature of plants and plant-life such as a suitable training in the elements of botany would give him.

This training, if commenced at school with exercises on observing and describing plants, and then pursued far enough to give him correct ideas of structure, of the nature and grouping of the histological elements, and of what is best known as to their functions in the physiology of nutrition, growth, and reproduction, would at least save the student from those crude notions as to the so-called physics and chemistry of a yeast-cell or of a fungus-hypha which one so commonly meets with.

I am not in any sense implying that a brewer's technologist should be a botanist, in the accepted meaning of the term: I only urge that he has to confront problems of *physiology* and of *morphology*, over and above his every-day riddles of chemistry and physics; and that even if we concede that physiological actions are nothing more than complex and conditioned physical and chemical actions (and I do not deny this), it is still true that he should be quite clear that this implies much more than it is commonly supposed to imply, and have at least an inkling of what we know as to the complexity of metabolic and other processes.

Now he cannot be clear on this subject unless he knows something of modern plant-physiology; and he cannot follow the teachings of physiology unless he is familiar with what is best known as to the structure of plants, and their general nature. How far he should go in these studies is not for me to limit, but he must at least be able to grasp enough to enable him to understand the progress of the science, and to see how far he is justified in drawing inferences from phenomena observed in other plants and applying his conclusions to the plants he is studying. To attempt to study the behaviour of a yeast-cell, or of a bacterium or mould, without clear ideas as to what is known of the plant-cell generally, seems to me very like obstinately attempting to open a lock in a dark room when you are ignorant of the whereabouts of the lock and have not found the right key.

What I have said with respect to the study of ferment-organisms holds good with regard to the study of what is called bacteriology, and to an even greater extent. For no one is likely to gainsay that such extremely difficult and delicate investigations as those made in the domain of pathology cannot be properly conducted without an intelligent acquaintance with the physiology of parasitic and saprophytic fungi and bacteria, and this being conceded the rest follows as a matter of course.

Yet it is in just this region of special scientific investigation that the grossest sins are committed. It is pitiable to see the wild struggles with facts that have been carried on in the name of bacteriology, and which might have been avoided had the investigators been properly trained in botanical science.

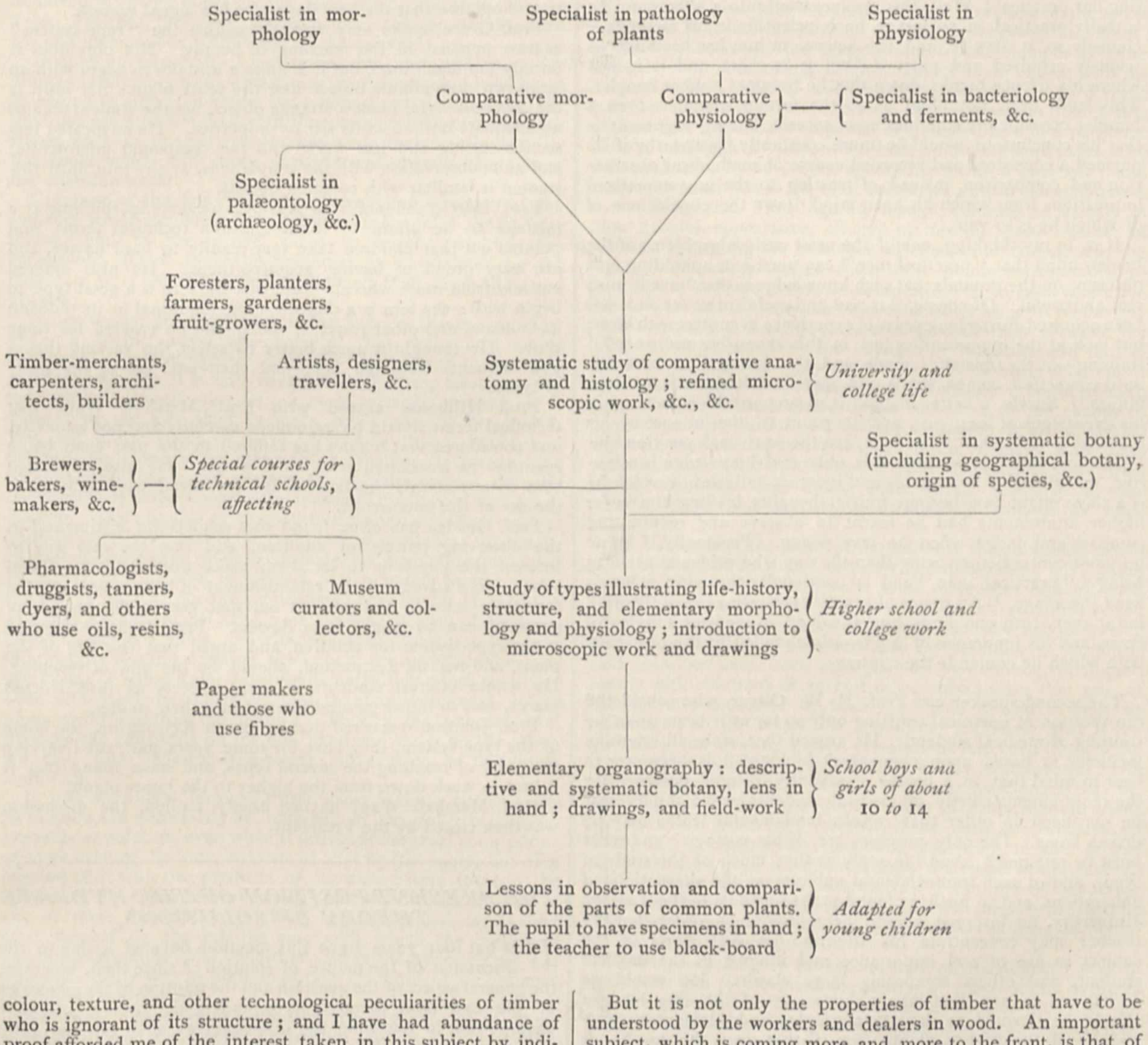
Bacteriology, however, is only one special branch of what is popularly known as the study of germs, and the truth of what has been above stated comes out with yet more startling clearness when we recognize the benefits that have arisen from the study of parasitic fungi and their relations to the diseases of plants. Taking the latter as a special pursuit, it is very difficult to say what should be omitted in a training designed to fit the botanist for investigation. It is only quite lately that pathologists have clearly recognized that the study of the diseases of plants (so important to horticulturists, planters, and foresters) implies by no means a mere acquaintance with the forms of fungi and their systematic relationships, but that it demands, on the one hand, the most patient and refined researches into the life-history of these organisms, and the variations in their biology due to changes in the environment, and, on the other hand, as deep an insight as can be obtained into the normal physiology of the host-plants, and the variations in this due to changes in the environment. In other words, not only must the investigator attack the question of the mutual relations between parasite and host (and he cannot understand these without studying the normal biology of both), but he must also look into the relations of each to a varying physical environment.

As I said before, it would be hard to say what botanical information can be superfluous in such a training.

But there are other technical pursuits which demand a training in elementary botany, and among these that of the timber merchant, and those of the builder, carpenter, and architect may be grouped together.

It is admitted that these people should understand the nature and properties of timber in the wide sense, and especially of certain kinds of wood in particular. My case is made out quite clearly by the efforts one meets with in various articles and books on timber, designed for the information of those engaged in the trades and professions referred to, and by the lamentable failures in conveying clear instructions, owing to the want of acquaintance with the elements of botanical science.

Tabular Résumé of the Various Branches of Botanical Study, as grouped for the preceding argument.



I maintain that no one can properly understand the markings, colour, texture, and other technological peculiarities of timber who is ignorant of its structure; and I have had abundance of proof afforded me of the interest taken in this subject by individuals connected with the numerous callings centred around that of the timber merchant—*e.g.* wood-carvers, turners, cabinet-makers, wheelwrights—as well as by archæologists and geologists, who are brought face to face with problems which require an acquaintance with the structure of timber for their solution.

Now, the structure of timber is a very interesting subject if properly approached, but it is a very complex and hopeless subject for one who is unacquainted with the meaning of the four or five histological elements which compose wood, and of their development from the cambium-cells; and, to comprehend these things, the student should know the elements of botany.

But it is not only the properties of timber that have to be understood by the workers and dealers in wood. An important subject, which is coming more and more to the front, is that of the classification and identification of timbers. It is astonishing how cleverly practical experts can find their way through the difficulties which beset those who have to decide upon the value of timber, and the suitability of different pieces of wood for various purposes; but even more astounding is the vagueness of their replies to the very natural question, How do you decide in difficult cases? One thing is clear—the expert bases his conclusions on keen observations of minute details, and yet these observations are not recorded: the whole system is one of empiricism and blind rule-of-thumb guess-work. It serves the purpose in many cases, just as rough measurements by an experienced eye and hand are often said to serve the purposes of those concerned at the time; but will anyone doubt that scientific accuracy and system would be more reliable? I am aware that "practical men" doubt this, but repeated contact with "practical men" assures one that they pay a heavy penalty in loss of time for their triumphs.

It is repeatedly observable that the "practical man"—the man of experience, in other words—has to spend long periods of time in the acquirement of his unsystematized powers, and the conviction forces itself upon the observer that he could do much more if he were systematically and logically observant, instead of being merely spasmodically so. In other words, he is scientific in so far as his successes go, for in the end it all resolves itself into keenness of observation and comparison; and he would save himself many failures if he were properly trained. How often is it pointed out that such and such a man is unscientific but practical! Well, this resolves itself into a fallacy, for he is really practical in so far as he is scientific in his methods—clumsily so, it may be, and the science in him has been unconsciously acquired and pursued; but it is there, and it is just where his science breaks down that he becomes a mere bungler. This truth need not blind us to the further one that even a bungler occasionally stumbles upon success, but my argument is that his conclusions would be more constantly trustworthy if he pursued a consistent and recorded course of methodical observation and comparison, instead of trusting to the unsystematized impressions from which his keen mind draws the conclusions of of which he is so vain.

It is, to my thinking, one of the most curious problems of the human mind that "practical men" can persist in upholding empiricism, on the grounds that such knowledge as the above is most real and useful. Of course, it is real and useful in so far as it has been acquired during long years of experience in contact with facts; but look at the opportunities lost in this expensive and wasteful training—at the mistakes made and the wrong lines pursued, until correction comes, sharp and merciless because it involves failure. Surely, a better method is to prepare the man to gain his experience at least cost, and to profit to the utmost by his mistakes; and, when all is done, see the equivocal position the "practical man" is put into—his only real knowledge is scientific, and the wild hypotheses and ignorant fallacies to which he is a slave might have become fruitful thoughts, leading him to far higher attainments had he learnt to observe and record, and compare and judge when he was young. Personally, I know no more contradictory being than the one who prides himself on being a "practical man," and is continually throwing at one's head the adage, "An ounce of practice is worth a ton of theory," for at every turn one finds him involved in endless tangles of error, and his ignorance of this is only equalled by the obstinacy with which he contends the contrary.

The second speaker was Prof. F. W. Oliver, who considered the question of botanical teaching only so far as it bears upon the training of medical students. He argued that, since all scientific medicine is based upon elementary biology, it is necessary to bear in mind that, in a course of say fifty lectures, designed for the requirements chiefly of medical students, some things must be sacrificed in order that certain fundamental truths may be driven home. The only questions are, What must go? and what must be retained? And the reply is that much of the study of types, and of such transcendental subjects as the alternations of generations, and so forth, as found in the schedule of the London University, for instance, should be sacrificed in order that the teacher may concentrate his attention on such parts of the subject as are of real importance and interest to the medical student, and others composing large classes. He would go so far as to say that about thirty out of the fifty lectures should be devoted to the organography and elementary physiology of the higher plants; for in that case the teacher is dealing with beings of which everybody knows something, and there is more human interest to the student when the *facies* of the organism is so familiar as is that of common flowering plants. In conclusion, Prof. Oliver pointed out that the responsibility of these matters rests with the examiners and those who draw up such schedules as that of the London University, and laid some stress on the importance of this responsibility.

Prof. F. O. Bower followed, and directed his remarks chiefly to the subject of teaching mixed and elementary classes in a University. He wished especially to deplore the threatened divorce between morphology and physiology, and advocated that such a divorce should be prevented at all hazards. In regard to this, and to some other points, he must differ from Prof. Marshall Ward's conclusions, though he heartily concurred with most of what he had said. He thought that, taking into account the value of the mental exercise, so useful a study as that of morphology should be introduced early, and that the teaching of the main homologies should be insisted upon. With regard to the cut-and-dried schedules now so universal, Prof. Bower was of opinion that, while they protect the weaker teachers, they hamper the strong ones, and he wished very much that more individual freedom should be allowed to lecturers.

Mr. Forsyth was especially interested in Prof. Marshall Ward's remarks on the teaching of botany to children in schools, and described an experiment now being tried in the Leeds Higher Grade School. The children are being taught to bring plants themselves, and to observe them in the field, and the speaker was of opinion that the new departure is a signal success.

Prof. Green spoke very strongly against the "type-system" as now pursued in the teaching of botany. Not only does it occupy too much time, but it is quite a mistake to begin with an unknown and minute object like the yeast plant: not only is the *Saccharomyces* plant a strange object, but the student obtains no adequate notions of its size or properties. He advocated less section-cutting and less work with the compound microscope, and more observation with the simple lens, at any rate until the student is familiar with common objects.

Prof. Hartog differed from previous speakers in thinking it a mistake to be afraid to teach children technical terms, and pointed out that children take very readily to hard names, and are very proud of having acquired them. He also differed entirely from those who advocate that the fern is a good type to begin with: the fern is a difficult type, abnormal in its phloem, its stomata, and other respects, and should be avoided for some time. He thought it much better to select the various tissues and elements from the first, and then pass on to the study of types.

Prof. Hillhouse agreed with Prof. Marshall Ward that technical terms should be introduced carefully and not too early, and considered that botany has suffered in the past from being regarded as associated with hard words. He also advocated that botany affords the best means for introducing students to the use of the microscope.

Prof. Geddes has often found that schools are detrimental to the observing powers of children, and that the real way to interest the pupils is to let them make discoveries for themselves. He advocated the establishment of a botanical garden for every school, and pointed out that very useful notions of geometry can be taught from flowers. Prof. Geddes objected to the type-system for children, and urged that the life of the plant, and not its destruction, should be the aim of teaching. He would interest students in such subjects as insectivorous plants, and so infuse general interest into their studies.

Prof. Johnson remarked that at South Kensington, the home of the type-system, they have for some years past tried varying the order of teaching the several types, and have found that it is best to work down from the higher to the lower plants.

Prof. Marshall Ward having briefly replied, the discussion was then closed by the President.

THE PRESENT POSITION OF THE HYDRATE THEORY OF SOLUTION.[1]

IT is but four years since this Section devoted a day to the discussion of the nature of solution;[2] since then, however, the general aspect of the question and the position of the advocates of the two rival theories have undergone such a complete change, that in renewing the discussion we shall run but little risk of going over the same ground which we then trod. At Birmingham, Dr. Tilden opened the discussion by passing in review all the well-known and long-known facts which might by any possibility throw some light on the nature of solution, and those who followed him in the discussion each gave the interpretation of these facts which harmonized best with his own views, and, as the facts themselves were susceptible of several different interpretations, the not surprising result followed that

---

[1] Paper read before Section B, at the Leeds meeting of the British Association, as an introduction to a discussion on the nature of solutions and the theory of osmotic pressure.

[2] B. A. Report, 1886, p. 444.

each disputant departed holding precisely the same opinions which he had brought with him. Since then, however, each party has obtained, or thinks that he has obtained, positive evidence in favour of his own views; evidence which, if upheld, must be accepted as conclusive, or which must be overthrown before his opponents can claim the victory. The supporters of the hydrate theory claim that the curved figures representing the properties of solutions of various strengths show sudden changes of curvature at certain points, which are the same whatever be the property examined, which correspond to the composition of definite hydrates, and which, therefore, can only be explained by the presence of these hydrates in the solutions; while the supporters of the physical theory, now identified with the supporters of the osmotic pressure theory, claim to have shown that, with weak solutions at any rate, the dissolved substance obeys all the laws which are applicable to gases, and that, therefore, its molecules must be uninfluenced by, and uncombined with, those of the solvent.

In another respect also I may notice that our position to-day differs considerably from what it was four years ago; for instead of having to argue the matter out amongst ourselves, as we did then, we are now favoured with the presence of some of those whose work in this very subject has made their names familiar household words with every physicist and chemist throughout the scientific world.

I propose in the first place to give a brief summary of the evidence which has lately been adduced in favour of the hydrate theory, and in the second place to inquire whether the conclusions drawn from this evidence are invalidated by the important facts elucidated by Raoult, van't Hoff, Arrhenius, and Ostwald.

In one respect the supporters of the hydrate theory start now under a distinct advantage—namely, that their most active opponents do not altogether deny the existence of hydrates in solution, although it is only in the case of strong solutions that they will admit their presence; in such solutions, indeed, it is difficult to see how their presence could possibly be denied. The only means which we have of proving that a liquid is a definite compound is by ascertaining whether its composition remains unaltered by its passage through the gaseous or solid condition—by fractionating it by means of distillation or crystallization. With liquids of comparatively small stability, such as hydrates, crystallization is the only method available; the results of crystallization have led us to conclude that the liquid represented by H_2SO_4 is a definite compound, and precisely similar results must force us to accept the definiteness of the liquids $H_2SO_4SO_3$, $H_2SO_4H_2O$, and $H_2SO_44H_2O$: in the case of each of them the liquid freezes as a whole, and without change of composition; the temperature remains constant throughout the solidification, and any excess of either water or sulphuric anhydride which may have been added may be separated from the pure compound, which alone crystallizes from the mixture. Thus, in the instance taken, between the anhydride on the one hand and water on the other, we have four definite compounds, all existing in the liquid condition.

It does not follow, however, that every hydrate which exists in solution can necessarily be obtained in the solid condition; probably no solution, even when it possesses the exact composition of some existing hydrate, consists of that hydrate only, but of a mixture of it with the products of its dissociation (though the amount of these may be very small); and whether the hydrate or one of these dissociation products crystallizes out on cooling must depend on the relative ease with which the bodies in question assume the solid condition; when the hydrate does not crystallize easily we can hope to obtain evidence of its presence by indirect means only.

Mendeleeff's conclusions respecting the densities of solutions of sulphuric acid and alcohol,[1] mistaken though I believe they were, led to the discovery of the means whereby such evidence might be obtained.

He stated that on plotting out the rate of change of the densities with the percentage composition of the solution (the first differential coefficient) he got a series of straight lines, forming figures with well-marked breaks at points corresponding to definite molecular proportions; but on plotting out the experimental points which he said formed these figures, it is impossible to see any justification for this statement; in the case of sulphuric acid the points and Mendeleeff's drawing of them have been given side by side in the Trans. Chem. Soc., 1890, p. 81, and in the case of alcohol they will be found in the *Zeit. f. Phys. Chem.* VI. i. 10. Crompton then showed[1] from an examination of Kohlrausch's values for the electric conductivity of sulphuric acid solutions that a second differentiation might in some cases be necessary before rectilineal figures with breaks in them were obtained. In my own work on various properties of solutions of the acid I have made free use of this process of differentiation, but I have combined it with, and now nearly entirely rely on, an examination of the original curves with the help of a bent ruler.

In the *Phil. Mag.*, 1890, vol. i. p. 430, will be found rough sketches of the figures representing the densities, contraction on formation, electric conductivity, expansion by heat, heat of dissolution, and heat capacity of the solutions, and in the Trans. Chem. Soc., 1890, p. 338, that representing the freezing-points. In some cases, such as the freezing-points of solutions near 58 and 100 per cent. strength, a mere inpection of the figure enables us to locate the position of abrupt changes of curvature; in general, however, the recognition of such changes is more difficult. On attempting to draw any of these figures with the help of a bent ruler it was found that the whole figure could only be drawn in several sections, and it was also found that each section thus drawn consisted of a single curve of a parabolic nature, although a ruler, when bent by the pressure exerted by the two hands, by no means necessarily forms a parabola; and moreover—and this is the most important part of the evidence—it was found that these figures, though differing so greatly in their general appearance, all split up into the *same* number of sections, indicating the existence of changes of curvature at the *same* points; and, further still, these points corresponded to solutions of definite molecular composition in all cases where the ratio of the acid to the water was sufficiently large to render any such comparison possible; the average difference between the composition indicated by the changes of curvature and that of definite hydrates was only 0·057H_2O. With weak solutions it is, of course, impossible to assert that the changes occur at definite molecular proportions, owing to the smallness of the change in percentage composition which would be caused by an additional molecule of water to each H_2SO_4; but the changes with these weak solutions are of precisely the same character as those with strong solutions, and, unless some strong evidence to the contrary be forthcoming, we must attribute them to the same cause.

To discuss fully the value of the evidence thus obtained would take me more hours than I can now afford minutes; but I think that I may say that these results stand at present unquestioned and uncontroverted, and that unless they can be controverted we must accept the presence of hydrates in solution as having been proved. I may also add that my results with sulphuric acid solutions have been strengthened by obtaining analogous results with solutions of several other substances: that one of the hydrates indicated by them has been proved to exist by isolating it in the crystalline condition: and lastly, that a law governing the freezing-points of solutions has been formulated, according to which we can calculate within experimental error the freezing-point of any solution, whatever its strength may be, provided we acknowledge the existence of every hydrate which my work has indicated; whereas, if we deny the existence of these, the freezing-points calculated according to this or any other law show such divergences from the found values that all semblance of agreement disappears. I am indeed labouring under no small disadvantage in attempting to support the hydrate theory when the greater part of the evidence existing in favour of it is as yet unpublished.

Before proceeding to the second part of my subject I wish to draw attention to the great complexity of some of the hydrates which my work has indicated, as well as to the fact that the indications of sudden changes are nowhere more marked than they are with these very weak solutions. The changes, which are observed in the heat of dissolution curve from 5 per cent. downwards,[2] afford a good illustration of this latter fact; or, again, the freezing-points of weak solutions may be instanced,[3] where the rate of fall from 0 to 0·07 per cent. is a quarter as great again as it is from 0·07 to 1·0 per cent. The complexity of the hydrates indicated is so great that in the extreme cases they must be represented as containing several thousand H_2O molecules, and the suggestion of such complexity will no doubt prejudice many against my conclusions in general; though on what grounds I know not, for we are entirely in ignorance at

---

[1] *Zeit. f. Phys. Chem.*, i. p. 275; Chem. Soc. Trans., 1887, p. 778.

[1] Chem. Soc. Trans., 1888, p. 116.

[2] *Ibid.*, 1890, p. 107.

[3] *Ibid.*, p. 343.

present as to the possible complexity of liquid molecules. It is interesting to note that a similar complexity of molecular grouping must be admitted if we accept Raoult's original statement that one molecule of any substance dissolved in 100 molecules of a solvent lowers the freezing-point of this latter by about 0°·63; for, if this be so, we must assign to the molecules of the various substances entered in the second column of Table I. the magnitude there indicated when they are dissolved in the solvent named in the first column, for it requires that proportion of these bodies to lower the freezing-point of 100 molecules of the solvent by 0°·63; and, amongst these few instances which I have collected from my own determinations, we find molecular aggregates containing as many as 200 of the fundamental molecules, and even this number, I may mention, probably understates the complexity to a very considerable extent; for the depression in this and some of the other cases had to be estimated from that observed with solutions containing as much as 10 gram molecular proportions to 100 of the solvent, and the molecular depression increased rapidly with the strength of the solution: $1000H_2O$ would probably be a low estimate of the complexity of the molecules of water when dissolved in a large excess of the hexhydrate of calcium chloride, a complexity comparable with that of the hydrates, which my other work has indicated, and that too in the case of that very substance which these hydrates contain—water.

TABLE I.—*Molecular Weights of Substances in Various Solvents.*[1]

Solvent.	Dissolved substance producing 0°·63 depression.[2]
$100H_2SO_4H_2O$	$32H_2O$
,,	$63H_2SO_4$
$100H_2SO_44H_2O$	$8H_2O$
,,	$15H_2SO_4$
$100(CaNO_3)_24H_2O$	$90H_2O$
,,	$42Ca(NO_3)_2$
$100CaCl_26H_2O$	$210H_2O$
,,	$63CaCl_2$

Now as to the question of how far the theory of osmotic pressure, and the results on which it is based, are antagonistic to the hydrate theory: and let me first define clearly the position which I take in this matter. I do not for one moment call in question any of Raoult's classical work, which is now so familiar to us, nor do I question that these results reveal the existence of a depression of the freezing-point which is approximately and generally constant; and I consequently admit that we can generally obtain an approximately correct value for the molecular weight of the substance by observing the depression which it causes; nor, again, do I wish to question the correctness of the mathematical relationship which van't Hoff and Arrhenius have shown to exist between osmotic pressure, the lowering of the freezing-point, and other properties, provided we accept the fundamental assumptions on which their calculations are based—the truly gaseous nature of dissolved matter, and the dissociation of salts into their ions. But what I do question is that the facts of the case warrant such assumptions, and that the constancy and regularity of the results are so rigorous as to justify the conclusion that the solvent has no action on the dissolved substance, and that there are no irregularities such as would be caused by the presence of hydrates.

According to the osmotic pressure theory, the dissolved matter, so long, at any rate, as it is not present in greater quantity than it would be in the same volume of its gas, if it were gasified under normal conditions, is really in the gaseous condition, and obeys all those laws which apply to gases. According to the hydrate theory this will be but partially true. That the dissolved substance is in a condition comparable with that of a gas, in so far as the separation of its own particles from each other is concerned, must be admitted—indeed, I arrived independently at this same conclusion from a study of thermochemical data; but inasmuch as there is present the solvent, which we believe is *not* an inactive medium, its molecules cannot have the same freedom as if they were truly gaseous, and will, therefore, obey the laws of gases imperfectly only.

It will be well to confine our attention to but one of those properties connected with osmotic pressure, and to select for that purpose the one which has been most fully investigated—the lowering of the freezing-point of a solvent: and the tests which may be applied to assertain whether in producing this lowering the dissolved substance behaves as a perfect gas or not, may be grouped under three principal headings:—

1. Is the molecular depression (*i.e.* that produced as calculated for one molecule dissolved in 100 molecules) constant, independent of the nature of the solvent?

2. Is it independent of the strength of the solution, so long as this strength does not exceed the limits ("gas" strength) above mentioned? (Boyle's law.)

3. Is it independent of the nature of the dissolved substance? (Avogadro's law.)

In the *Phil. Mag.*, 1890, vol. i. p. 495, will be found instances of the variation in the molecular depression which may be noticed by altering the solvent (see also Table I. above). With water in six different solvents it varied between 1°·072 and 0°·003; with sulphuric acid in four different solvents, between 2°·15 and 0°·01; with calcium chloride in two different solvents, from 2°·773 to 0°·01; and with calcium nitrate in two solvents, from 2°·5 to 0°·015; while many instances may be collected from Raoult's data showing that the same substance which acts normally in one solvent may act abnormally (give only half the usual depression) in another. Such variations are so great—from 100 to 35,600 per cent.—that there can be no doubt but that the solvent is *not* that inert medium which the supporters of the physical theory would have it to be, but that it has a very great influence on the results obtained. It must be noted, however, that this objection, though applying to Raoult's original views, does not, or, at any rate, may not, apply to van't Hoff's theory, for according to this theory the nature of the solvent *has* an influence in determining the lowering of the freezing-point, W, in van't Hoff's equation, $\delta t = \frac{0{\cdot}02T^2}{W}$, representing the heat of fusion of the solvent. But the lowering is according to this equation independent of the nature or the amount of the dissolved substance, so that the two following objections will apply to van't Hoff's theory as well as to Raoult's statement.

Secondly, as to the influence of the strength of the solution. It is remarkable that, although the osmotic pressure theory depends on the behaviour of solutions below a certain strength, no attempt whatever has been made by its supporters to obtain any data respecting such solutions. The data on which their views were founded referred to solutions considerably stronger than the requisite "gas" strength, and though, no doubt, it was convenient to work with data which afforded a ready excuse for any awkard irregularities which might be met with, such data must lack the conclusiveness which is so eminently desirable. The few data which I have accumulated as to solutions of an "ideal" strength can leave no doubt that, even in their case, the depression is not a constant independent of the strength.

A solution of sulphuric acid containing $0{\cdot}08H_2SO_4$, $100H_2O$ would be of a strength comparable with the gas from the acid if it could be gasified at normal pressure and temperature, and the molecular depression should be constant for all solutions below this strength: it should be represented by a horizontal line such as AB in Fig. 1, whereas the observed deviations from constancy are very great, being represented by the lines marked H_2SO_4; and, moreover, these deviations are by no means regular, and cannot therefore be attributed to imperfect gasification; they possess none of the characteristics of the deviations of gases from Boyle's law. The determinations on which these results are based are very numerous; there are about sixty experimental points on the portion here shown, and the mean error of each point as determined in two different ways was only 0°·0005, a quantity represented by one-tenth of one of the divisions of the paper; the deviations from regularity amount to thirteen times this quantity, and to as much as 16 per cent. of the total depression measured.

The other lines in Fig. 1 represent the deviations from regularity in the case of calcium chloride, calcium nitrate, and alcohol respectively, and these, though they are smaller than in the case of sulphuric acid, are far too great to be attributed to experimental error; and the fact that they occur sometimes in one direction, sometimes in the other, precludes the possibility of attributing them to any constant source of error in the instruments used or in the method adopted.

Remembering that these are the only data which we have at present respecting very weak solutions, we must conclude that the hypothesis that such solutions exhibit perfect regularity is

---

[1] Other instances of high molecular weights are mentioned by Brown and Morris (Chem. Soc. Trans., 1888), and Gladstone and Hibbert (*Phil. Mag.*, 1889, vol. ii. p. 38).

[2] Determined from the freezing-points of very weak solutions.

wholly untenable; and it must be specially noticed that one of the substances showing these irregularities—alcohol—is a non-electrolyte, in which case the theory of dissociation into ions cannot be brought forward as an explanation of their existence.

It is important to observe that when we pass on to stronger solutions, where the actual magnitude of the deviations becomes so great that they would be revealed by the roughest experiments—deviations of even 70°—and where, I believe, even the supporters of the osmotic pressure theory would not hesitate to attribute them to the disturbing influence of hydrates; these deviations occur in precisely the same irregular manner as they do in the case of weak solutions, and must evidently be attributed to the same cause. The results with alcohol given in Fig. 2 illustrate these irregularities in a very striking manner. It must also be pointed out that, apart from the irregularity of these deviations, their very direction shows that they cannot be attributed to the dissolved particles being brought within the sphere of each other's attraction, as in the case of the deviation of gases from Boyle's law, for the result of this would be that their attraction on the particles of the solvent would be diminished, and the freezing-point of this latter would consequently be lowered to an abnormally small extent, whereas precisely the reverse is the case in nearly every instance at present investigated: the freezing-points of strong solutions are abnormally *low*. Various instances of this will be found in the *Phil. Mag.*, 1890, vol. i. p. 500, that of sulphuric acid, which is illustrated here in Fig. 2, being by no means the most prominent; while the case of alcohol, now for the first time displayed (Fig. 2), is the only exception which has, so far, been met with, and that is an exception only in the case of excessively strong solutions.

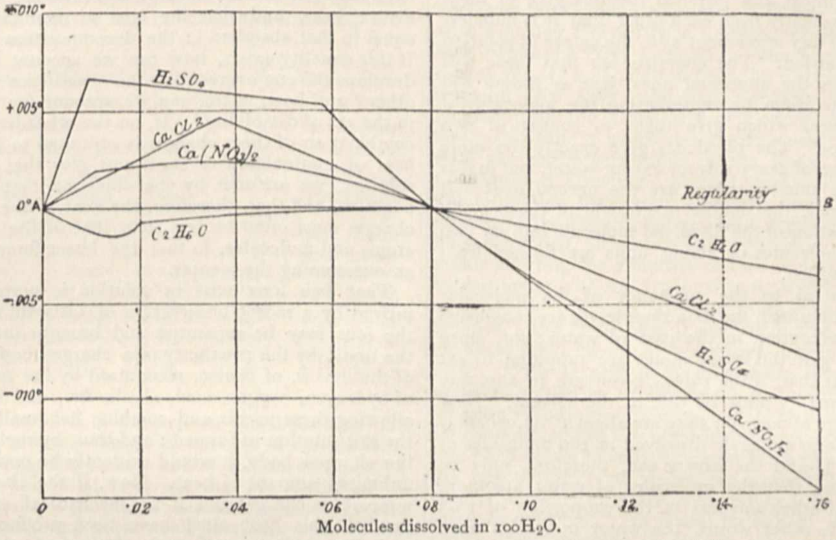


FIG. 1.—Deviation from regularity of the freezing-points of very weak solutions.

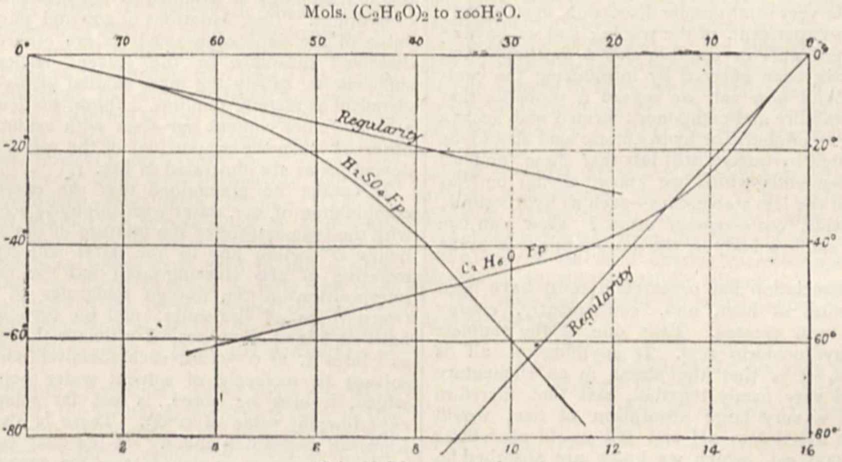


FIG. 2.—Freezing-points of sulphuric acid and alcohol solutions.

From the instances above mentioned some answer may be obtained to the third question—whether the molecular depression is independent of the nature of the dissolved substance. The values obtained with these four substances, taking solutions of a strength corresponding to that of their gases, are:—

Calcium chloride	2°·850
Calcium nitrate	2°·744
Sulphuric acid	2°·313
Alcohol	2°·180

a variation of 30 per cent., which must give an emphatic denial to the idea of absolute constancy; and if we take instances from other substances, where the data available refer to solutions of somewhat greater strength, we find that the very substances on which the idea of constancy was originally founded show variations reaching 60 per cent. (*Phil. Mag.*, 1890, vol. i. p. 492), while in other cases, which I have quoted elsewhere (*loc. cit.*, p. 493),[1] the variation attains the still larger dimensions of 260 per cent.

---

[1] The depression produced by H_2O in $100H_2SO_4$ is 1°·07 instead of 0°·07 as there given.

To every one, therefore, of the three test questions as to constancy and regularity, the experimental results give an unhesitating negative.

In the instances quoted above the depression actually found for alcohol has been doubled in order to simplify the comparison of it with the other substances. Alcohol belongs to that class of bodies which give just half the value in water that the majority do, and of which there are some instances in the case of every solvent yet examined. The explanations which the supporters of the chemical and physical theories give of these half values differ so radically from each other that it is hopeless to attempt to arrive at any agreement as to the nature of solution till this difference is settled. The chemists say that these half values are in all cases the abnormal ones, just as Raoult did originally, and explain them by representing the molecules of the dissolved substances which give them to consist of two fundamental molecules. The physicists give exactly the same explanation in the case of every solvent except water, but in this case they say that the smaller values are the normal ones and the larger the abnormal, the double magnitude of these being caused by the dissociation of the dissolved molecule into its two ions, whereby two molecules or acting units are formed from every one originally added.

If Raoult's views as to the constancy of the molecular depression can be maintained, the data themselves are conclusive against making this exception in the case of water; for, since the substances which give the lower values are supposed to act normally, it is evident that, if the values given are in any way abnormal, this abnormality must be due to the solvent. Now the values certainly *are* abnormal; they are about 1°·03, whereas the normal value for one molecule dissolved in 100 molecules of other solvents is 0°·63, and the excess can, therefore, only be explained by assuming that the molecules of water are more complex than those of other solvents in the proportion of 1·03 to 0·63, or $1\frac{1}{2}$ to 1; in other words, the water molecules must be $1\frac{1}{2}H_2O$. This view cannot be reconciled with the atomic theory.

Indeed the theory of dissociation into ions is altogether unintelligible to the majority of chemists. It seems to be quite irreconcilable with our ideas of the relative stability of various bodies, and with the principle of the conservation of energy. Of course we know that each ion when dissociated is not supposed to be permanently dissociated, but to be continually combining with its neighbours and separating again from them as in every other case of dissociation; but at any particular moment a very large proportion of them is supposed to be free; a proportion which, according to the very results under discussion, must be very nearly, if not quite, 100 per cent. of the whole; and we have to settle whether it is probable or possible that a decomposition such as this could have been effected by introducing the compound into water. And how can we regard it probable that compounds of such stability and compounds formed with such a development of heat as sulphuric or hydrochloric acid should be thus entirely dissociated by water; still less that these, and all the most stable compounds which we know, should be thus demolished, while all the less stable ones—such as hydrocyanic, sulphurous, boric acids, &c.—remain intact? How can we admit that the more stable a body is, the more prone it is to be dissociated?

And if such a dissociation has occurred it must have been without any absorption of heat, and, consequently, energy must actually have been created. Take one of the simplest instances, that of hydrochloric acid. If anything at all is certain about atoms, it is that the atoms in an elementary molecule are united very firmly together, and that therefore in separating them a very large absorption of heat would occur. To separate 2HCl into 2H and 2Cl would absorb far more than the 44,000 cal. which we know are absorbed in separating 2HCl into H_2 and Cl_2. Yet the supporters of the dissociation theory would have us believe that this separation has actually taken place, not only without any absorption of heat, but actually with a development of 34,630 cal.; that is, that $44,000 + 34,630 + x$ cal. have been *created*, and that too through the intervention of the water, which has *ex hypothesi* no action whatever.

This difficulty is realized by the supporters of the physical theory, but the way in which they meet it does not appear to me in any way to overcome it. To explain the non-absorption of heat in the dissociation of the salt, they suppose that a charge of electricity combines with the liberated atoms, and, in doing so, evolves an amount of heat exactly equivalent to that absorbed in the separation of the atoms from each other; and a later development of the theory is, I believe, that the atoms, though separated, are still held together by means of these charges, so that the net result is the supplanting of the chemical bond by an electrical bond of a precisely similar value. It appears to me that nothing substantial is gained by such a substitution, and that its occurrence is not merely hypothetical but impossible. Whence come these electric charges, and by what agency are they brought into play? On what grounds can it be maintained that a charge can combine with matter so as to evolve heat, and that the heat so liberated is always exactly equal to that absorbed in the decomposition of the compound? If this equality exists, how can we account for the force which develops the one overcoming the *equal* force which develops the other? and how, again, can we account for the heat developed in the act of dissolving? If, on the other hand, the heat of the combination of these charges is supposed to be equivalent to the heat of combination of the atoms *plus* that of the heat of dissolution, we are met by the objection that the latter is often negative, and that, therefore, the heat of the combination of the charges must often be less than that of the combination of the atoms and molecules, so that the lesser force must be regarded as overcoming the greater.

That free ions exist in solution is supposed to have been proved by a recent observation of Ostwald's, to the effect that the ions may be separated and brought into different parts of the liquid by the proximity of a charged body. The separation of the ions is, of course, recognized by the subsequent liberation of hydrogen, oxygen, acid, alkali, &c., and it is certain that on allowing these to mix and combine heat will be developed, and the salt solution re-formed; and thus, by replacing and removing the charged body, it would evidently be possible to produce an unlimited amount of heat. Now, if the charged body has lost none of its charge, and if no mechanical energy has been expended, this heat must have been produced out of nothing, and the whole ground-work of physical science must be false; whereas, if energy in some form has been expended on the solution, the experiment proves nothing, for there is nothing to show that this energy has not been utilized in bringing about the very dissociation the previous existence of which was in question.

I have already shown that the experimental data prove the absence of that constancy and regularity which ought to exist according to the physical theory, and to place the hydrate theory on unassailable grounds it is only necessary to show that deviations from constancy and regularity are of a magnitude such as might reasonably be assigned to deviations due to the presence of hydrates. That variations of 260 and 36,000 per cent. in the value of the depression—such as are observed by altering the dissolved substance or the solvent respectively—are amply sufficient to satisfy the most exalted views of the influence of chemical attraction, requires, I think, no demonstration, and we may therefore content ourselves with examining the deviations observed when the proportions of the solvent are altered—such deviations as are illustrated in Fig. 1.

It cannot be maintained that the energy of the chemical combination of, say, water with sulphuric acid, is the only reason why the temperature of the mixture of the two must be cooled below 0° before any of the latter will crystallize out; some lowering of the freezing-point will be caused by the mere interposition of the foreign molecules of sulphuric acid between those of the water, and on certain grounds, which I have explained elsewhere,[2] I estimate this mechanical lowering, as I term it, at 0°·56 for each dissolved molecule to 100 of the solvent (a molecule of solvent water being $3H_2O$), a value which, it may be noted, is not far removed from Raoult's experimental value of 0°·63. There is also another source of lowering depending mainly on the heat capacities of the substances concerned, which I term for convenience the physical lowering; but its value, in the case of weak solutions, is very small, and I need, therefore, say no more about it here. Both these lowering causes would exist whether there were hydrates present or not; but if these were present we should get a further depression due to their existence. Any given hydrate would have to be decomposed into the next lower one before it could give up any water for crystallization, and a certain amount of resistance would thus be offered to this crystallization, to overcome which the solution would have to be further cooled.

---

[1] On the view that hydrates exist in solution, there is n difficulty, as I have shown elsewhere, in explaining the absorption of heat during dissolution, without violating the principle of the conservation of energy.

[2] Proc. Chem. Soc., 1889, p. 149.

The necessary cooling may be estimated in the following way: Supposing the solution to be a mixture, and to be cooled below its normal freezing-point, then, on solidification, the temperature would rise to this point, but if this solidification involved a chemical decomposition which absorbed x cal., the rise of temperature would be thereby reduced, the reduction thus caused amounting to $x \div$ the heat capacity of the solution. As the heat absorbed in the decomposition of the various hydrates of sulphuric acid is known, we can calculate the lowering produced by their presence.

TABLE II.—*Freezing-Points of Solutions of Sulphuric Acid.*

I. Per cent. H_2SO_4.	Calculated.				IV. Found F. p.	Next hydrate.	
	II. Mech.	III. Phys.	IV. Chem.	V. Total.		VII. Calc.	VIII. Found.
						Per cent.	Per cent.
0·068	0·0209	0·0	0·0110	0·0347[1]	0·0354	0·37	0·36
0·362	0·1114	0·0004	0·0248	0·1508[1]	0·1582	1·43	1·05
1·06	0·3275	0·0044	0·0589	0·4314[1]	0·4272	3·54	4·02
4·02	1·285	0·071	0·077	1·582[1]	1·59	8·40	8·59
8·59	2·879	0·388	0·189	3·815[1]	3·80	18·17	18·49
18·49	6·96	3·23	1·59	11·78	11·83	29·7	29·5
29·53	12 85	18·82	3·50	34·17	34·00	37 5	37·7

In Cols. II., III., and IV., I have given the depression due to the three above-mentioned causes in the case of certain solutions, Col. V. containing their sum; and it will be seen what a small proportion of this total lowering can be attributed to purely chemical causes. With most solutions it does not exceed 10 per cent. of the total, and with weak solutions, such as are generally used in freezing-point determinations—say 5 per cent.—it amounts to considerably less than 0°·1; this, too, in the case of sulphuric acid, where the heat of formation of the higher hydrates is greater than with any other known substance.

The reason, therefore, why the deviations from constancy are so small as to have escaped detection hitherto, and the reason why solutions behave almost as if their chemical nature was non-existent, becomes apparent; but this near approach to constancy and regularity, instead of proving the correctness of the physical theory and giving a death-blow to the chemical theory, is really one of the strongest arguments which can be adduced in favour of the latter. If the hydrate theory is right, the influence of hydrates must often be nearly inappreciable.

But it is not only a general concordance between the found and calculated magnitude of the irregularities which the hydrate theory is capable of affording, but a concordance so exact that the precise value of the deviation at any point may be calculated. In Col. VI. of Table II. are given the observed freezing-points of the solutions, and these show an average difference of but 0°·004 for the three weaker solutions, and 0°·06 for the four stronger solutions, from those calculated (Col. V.). The last two columns exhibit this concordance in a different manner; from the observed freezing-point we can calculate the composition of the hydrates which must exist in the solution (Col. VII.), and these are found to agree so fully with those indicated by the examination of the curved figures representing various properties of the solution (Col. VIII.) that the maximum difference between the two is only 0·48 in the percentage of acid present.

When we can by simple calculations, based on one series of determinations, prove that the hydrates in solution must be the same as those which totally independent experiments have led us to suppose, we have, I think, arrived at proof as nearly absolute as it is possible to conceive; and, if I have succeeded in showing that this proof may be accepted without in any way rejecting the facts on which the advocates of the osmotic pressure theory rely—approximate constancy, approximate regularity, and approximate similarity between dissolved and gaseous matter—I shall feel that I have done far better work than the mere establishment of the hydrate theory, by pointing out a possible *modus vivendi* for both theories almost in their entirety, and by helping to break down that wall of separation between physicists and chemists which is fast crumbling into dust.

SPENCER UMFREVILLE PICKERING.

---

[1] The actual total has been increased by 10·4 per cent. of its value to give the figures quoted in these five cases, for reasons which will be given elsewhere. Some of the numbers in this table may be subject to slight corrections, as they have been quoted in the absence of the original calculations.

A TEACHING UNIVERSITY FOR LONDON.

THE following letter has been addressed to the Lord President of the Privy Council:—

MY LORD,—We, the undersigned, the President of University College, London, and the Principal of King's College, London, beg leave to address your Lordship in reference to the joint petition from the Councils of our two Colleges for the incorporation of a Teaching University in London, which has for some time been before the Privy Council. Your Lordship had the goodness to receive a deputation from the Councils of our two Colleges in July 1889; and your Lordship then intimated your judgment that the University of London should be allowed a reasonable time in which to propose a new charter in accordance with the recommendations of the Royal Commission on the question of a Teaching University in London. In obedience to this intimation from your Lordship, our Councils have, at the request of the Senate, entered into negotiation with them, and have consented, subject to the satisfactory settlement of some points affecting the Faculties of Law and Medicine, to a scheme for our union with the University, embodying a separate system of graduation for our students in the Faculties of Arts and Science. We desire that power should be reserved in certain events to make similar arrangements in regard to the Faculty of Law. With respect to medicine, the Senate have stipulated that they should be at liberty to make different arrangements, separately from our Colleges; and in the absence of opportunities for conference with the other institutions specially interested in this Faculty, we have not thought fit on this ground to break off the negotiations; but we reserve power to reconsider our position, if arrangements are contemplated by which it would be seriously affected. We claim, further, as essential to the efficiency of our teaching in science, that our medical students, for the purpose of their examination in pure science, known as the "Preliminary Scientific Examination," shall be considered as belonging to the Faculty of Science on the teaching side of the University, and not to a separate Faculty of Medicine.

Having been informed that urgent protests are raised by University Colleges in the country, particularly at Birmingham, against influence being given to London Colleges in the Senate while they are excluded, we beg to remind your Lordship that the amalgamation of the proposed Teaching University for London with the existing University was not our proposal, but has been, thus far, accepted by us in deference to the principal Report of the Royal Commissioners. We consider that, if this amalgamation is effected, we are entitled to a representation on the governing body of the reconstituted University proportionate to our concern in University teaching for London, considered as one of its two spheres of work; and that the nature of the case does not admit of a similar effective representation of institutions elsewhere. If this reconstitution of the existing University should be found, by reason of such opposition, or for any other reason, impracticable, we desire to be replaced in our original position, as petitioners for the establishment in London of a Teaching University upon the lines of our petition presented in 1887, and of the draft charter thereto appended, to which, in that case, we still respectfully adhere.

We have the honour to remain, your Lordship's obedient humble servants,

JOHN ERIC ERICHSEN,
President of University College, London.
HENRY WACE,
Principal of King's College, London.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The election of a Professor of Mechanism and Applied Mechanics, in succession to Prof. James Stuart, will take place on November 12. The names and testimonials of candidates are to be sent to the Vice-Chancellor by Saturday, November 8. The electors are the Vice-Chancellor, Mr. W. Airy, Dr. Besant, Sir F. J. Bramwell, Dr. Cayley, Mr. H. Darwin, Mr. Martin, Dr. Phear, and Lord Rayleigh. The stipend is £700. The Senate has approved a new scheme for the management of the department, under which the Professor is directly responsible for the carrying on of the workshops.

Mr. Chaplin, the President of the Board of Trade, has proposed to the Chancellor that the University should undertake the systematic education of students of agriculture. The question of funds stands in the way, but a syndicate is to be appointed to consider the question, and it is hoped that by a subvention from the County Councils, or by private benefaction, means may be found for the formation of an agricultural department.

Mr. Wynter Blyth and Dr. Ransome have been appointed additional examiners in Sanitary Science. Between fifty and sixty candidates presented themselves for examination, of whom about forty satisfied the examiners, and have received the University diploma in Public Health.

Mr. J. G. Adami, of Christ's College, has been elected to the John Lucas Walker Studentship in Pathology, in succession to Dr. William Hunter, of St. John's College.

Mr. E. Lloyd Jones has been appointed Demonstrator of Pathology in succession to Mr. Adami, resigned.

Mr. L. R. Wilberforce, of Trinity College, has been appointed Demonstrator of Physics, in succession to Mr. F. Newall, resigned.

The honorary degree of M.A. has been conferred on Dr. Joseph Griffiths, Assistant to the Professor of Surgery, and Pathologist to Addenbrooke's Hospital.

Dr. Donald MacAlister, of St. John's College, has been appointed Assessor to the Regius Professor of Physic.

The following have been nominated as Examiners in Natural Science:—Physics: Prof. Carey Foster, F.R.S., and R. T. Glazebrook, F.R.S. Elementary Physics: Prof. J. J. Thomson, F.R.S., and L. R. Wilberforce. Chemistry: Prof. Liveing, F.R.S., and Prof. Emerson Reynolds, F.R.S. Elementary Chemistry: M. M. Pattison Muir and Dr. Ruhemann. Geology: Prof. A. H. Green, F.R.S., and J. E. Marr. Botany: Prof. D. H. Scott and Prof. J. R. Green. Zoology: Prof. Ray Lankester, F.R.S., and A. E. Shipley. Elementary Biology: Prof. Marshall Ward, F.R.S., and A. Sedgwick, F.R.S. Anatomy: Prof. Macalister, F.R.S., and Prof. Windle. Physiology: L. E. Shore and C. S. Sherrington. Pharmaceutical Chemistry: H. Robinson and E. H. Acton.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, October 13.—M. Hermite in the chair.—M. Tisserand presented the second volume of his "Traité de Mécanique Céleste," and noted that it deals principally with two subjects—viz. the figure of celestial bodies, and their movement of rotation.—Presentation of the fifth volume of the "Bulletin du Comité international de la Carte du Ciel"; state of progress of preliminary works, by Admiral Mouchez.—On a photograph of the Ring Nebula in Lyra, obtained at Algiers Observatory, by the same author.—On a photograph obtained with a nine hours' exposure at Toulouse Observatory, by M. B. Baillaud. (For the three above communications, see Our Astronomical Column.)—Observation of D'Arrest's comet (rediscovered by Mr. Barnard on October 6, 1890) made at Paris Observatory with the West Tower equatorial, by M. G. Bigourdan. The observation for position was made on October 10.—On the linear equations from partial derivatives, by M. A. Petot.—Vibrations of a platinum wire rendered incandescent by an electric current, under the influence of successive interruptions of this current, by M. T. Argyropoulos. The author has stretched horizontally a platinum wire, 0·70 metre long and less than a millimetre in diameter, and has raised it almost to white heat by means of an electric current. By inserting a commutator in the circuit, the wire immediately vibrated, and became subdivided into a series of waves having well-marked ventral segments and nodes. The number of segments was augmented by very slowly decreasing the tension of the wire. On increasing the tension the number was diminished until the incandescent wire vibrated transversely with a single ventral segment at the middle.—Combinations of cyanide of mercury with lithium salts, by M. Raoul Varet. The following compounds have been prepared: (1) an iodocyanide of mercury and lithium, having the composition $HgCy_2, 2LiCy, HgI_2, 7H_2O$; (2) a bromocyanide of the same metals, for which the formula $2HgCy_2, 2LiBr, 7H_2O$ is given; (3) a chlorocyanide of mercury and lithium, of doubtful composition.—Researches as to the best conditions for the preparation of mono-isobutylamine in quantity, by M. H. Malbot.—On a general process for the synthesis of β-ketonic ethers and nitriles, by M. L. Bouveault. The author gives the most general method for the formation of β-ketonic nitriles, and shows that these bodies may readily be transformed into the corresponding ethers. The method is given in sufficient detail, and several examples of its application shown.—Upon the presence and the disposition of trehalose in mushrooms, by M. Em. Bourquelot.—On the lateral nerve of Cyclopteridæ, by M. Frédéric Guitel.—Physiological researches on floral envelopes, by M. Georges Curtel. It is concluded that (1) the flower possesses energetic respiratory and transpiratory functions, superior in general to those of the leaf of the same plant; (2) the assimilation is generally feeble, and cloaked or much diminished by the very intense respiration; (3) the volumetric proportion of carbon dioxide emitted to oxygen absorbed is always small, and less than unity.—On the porphyritic eruptions of Jersey, by M. A. de Lapparent.

SYDNEY.

Royal Society of New South Wales, August 6.—Dr. Leibius, President, in the chair.—Seven new members were elected.—A letter was read from the Committee appointed by the Victorian branch of the Royal Geographical Society of Australasia and the Royal Society of Victoria conjointly, inviting the co-operation of the Royal Society of New South Wales in carrying out the proposed Swedish-Australian expedition to the Antarctic Regions, and stating that Barons Nordenskiöld and Oscar Dickson had promised to defray half the cost of the expedition, providing an equal amount (£5000) was raised in the colonies.—The following papers were read:—On the theory of repetition measures of angles with theodolites, by G. H. Knibbs.—Record of hitherto undescribed plants from Arnheim's Land (part ii.), by Baron Ferd. von Mueller, K.C.M.G., F.R.S.—On the Australian aborigines, varieties of food and methods of obtaining it, by W. T. Wyndham.—On some photographs of the Milky Way, recently taken at the Sydney Observatory, by H. C. Russell, F.R.S.

September 3.—Dr. Leibius, President, in the chair.—The following papers were read:—Record of hitherto undescribed plants from Arnheim's Land (part iii.), by Baron Ferd. von Mueller.—On the application of the results of testing Australian timbers to the design and construction of timber structures, by Prof. Warren.—Exhibits: Enlargement of photograph of a negative of Fresnel's interference bands, for lecture purposes, by Prof. Threlfall; Edison's latest perfected phonograph, by C. L. Garland.

CONTENTS.