and presenting a torn appearance in places. Faint nebulous stars are immersed in the wave structure, and here and there the luminous material gives indications of condensation. Surrounding the nebula, and strewn over its surface are numerous stars, which are, however, apparently not physically connected with the general mass. Referring to the structure of the nebula in relation to the methods of stellar evolution, Dr. Roberts says:—"The general appearance of the nebula is that of precipitation of invisible matter—either gaseous or of dust particles—which exists in space as clouds of vast extent. . . . We know of no body whatever existing in space which has no motion of translation; but whether this invisible matter is in motion or at rest, it could be run into by another body that is in motion, with the result that whirlpool motions would be set up that would eventually develop into nebulæ of various forms, such as those which have already been, by photography, shown to exist. If, on the other hand, the clouds themselves are in motion and collide with each other, then vortical motion would be set up over large areas, giving rise by progress of development to such nebulæ as are represented by the photograph. This nebula shows signs of fission, and may pass in its process of development into symmetrical nebulæ and into stars, and again from stars into—what?"

A SUCCESSFUL EXPERIMENT IN LOBSTER-REARING.

Attempts to rear the larvæ of the lobster in this country have never hitherto succeeded. Several years ago Captain Dannevig reared some at Arendal, and published a description of his results, but no other instance of success with the European species has been recorded. In America the rearing of the American species has been successfully accomplished at Wood's Hole by Mr. F. H. Herrick, and a masterly description of the stages of the development, with extremely fine illustrations, was recently published by him as a number of the Fish Commission Bülletin. At the establishment of the Marine Biological Association at Plymouth attempts to rear lobster larvæ have been made, but never with success. In the tanks the larvæ invariably died after a few days, and when the hulk of a superannuated fishing-vessel was fitted up, provided with a well to which the water could have access, and moored in the Sound, she unfortunately sank at her moorings with the thousands of larvæ which she contained. It is therefore a fact not without interest and importance, that the difficult feat has been accomplished with some success during the present season at Falmouth. During the last two or three years, experiments in oyster and lobster culture have been carried on at that place under the auspices of a committee of the Royal Cornwall Polytechnic Society, the cost of the work being defrayed from a fund collected by private subscription, supplemented by grants from the Technical Instruction Committee. Until the commencement of this year the experiments were directed by Mr. Rupert Vallentin, who designed a large floating box 14 feet by 6 feet in area, by 3 feet in depth, provided with windows covered with metal gratings, for the purpose of rearing lobster larvæ. This box is moored in a corner of the docks where the water seemed quite pure. Last year no success was obtained, owing to an injury to the box. During the present season the experiments have been under the direction of Mr. J. T. Cunningham, the Lecturer on Fisheries for the county. Berried female lobsters were first placed in the box on and about June 24, to the number of three. Larvæ were first seen on July 6, and were fed on minced fish, but the number rapidly diminished. Seven more berried females were put in on July 16, and since that time there has always been a considerable number of larvæ alive in the box; some of the females have not yet hatched all their eggs (August 22). Since July 16 the only food supplied has been the crushed and pounded flesh of the edible crab, the females of which could be obtained regularly at small cost. It was found essential to feed the larvæ every day. As usual there was considerable mortality, and the larvæ showed their inveterate tendency to cannibalism; but a few specimens have passed through the various stages of their metamorphosis. Students of Herrick's memoir are aware that the final condition is reached, not considering certain minor features of little importance, at the fourth stage. The first stage is characterised by the entire absence of abdominal appendages, and the presence of the thoracic exopodites. After the second moult four pairs of abdominal pleopods are developed, at the third the uropods on the sixth abdominal somite are added, and at the fourth moult the exopodites are lost, and the antennary flagella appear. A specimen in the third stage was taken from the hatching-box at Falmouth on August 12, and one in the fourth stage on August 22.

The rearing of lobster larvæ may always remain too difficult and too expensive to be of any practical importance, and the survival of a single specimen may appear to be a small success. But there is every probability that other specimens will reach the same or later stages in the course of the experiment, and, considering the large amount of attention that the problem has attracted, the result above described is worthy of record.

THE BRITISH ASSOCIATION.

SECTION K.

BOTANY.

Opening Address by Prof. H. Marshall Ward, Sc.D., F.R.S., F.L.S., President of the Section.

The competent historian of our branch of science will have no lack of materials when he comes to review the progress of botany during the latter half of the Victorian reign. The task of doing justice to the work in phanerogamic botany alone, under the leadership of men like Hooker, Asa Gray, Mueller, Engler, Warming, and the army of systematists so busily shifting the frontiers of the various natural groups of flowering plants, will need able hands for satisfactory treatment. A mere sketch of the influence of Kew, the principal centre of systematic botany, and of the active contingents of Indian and colonial botanists working under its inspiration, will alone require an important chapter, and it will need full knowledge and a wide vision to avoid inadequacy of treatment of its powerful stimulus on all departments of post-Darwinian botany. The "Genera Plantarum," the "British Flora," the "Flora of India," suffice to remind us of the prestige of England in systematic botany, and the influence of the large and growing library of local and colonial floras we owe to the labours of Bentham, Trimen, Clarke, Oliver, Baker, Hemsley, Brandis, King, Gamble, Balfour, and the present Director of Kew, is more than merely imperial.

The progress in Europe and America of the other departments of botany has been no less remarkable, and indeed histology and anatomy, comparative morphology, and the physiology and pathology of plants have perhaps advanced even more rapidly, because the ground was newer. In England the work done at Cambridge, South Kensington and elsewhere, and the publications in the "Annals of Botany" and other journals sufficiently bear witness to this. A consequence has been the specialisation which must soon be openly recognised—as it already is tacitly—in botany as in zoological and other branches of science.

No note has been more clearly sounded than this during the past twenty-five years, as is evident to all who have seen the origin, rise, and progress of our modern laboratories, special journals, and even the gradual subdivisions of this Association. We may deplore this, as some deplore the departure of the days when a naturalist was expected to teach geology, zoology, and botany as a matter of course; but the inevitable must come. Already the establishment of bacteriological laboratories and a huge special literature, of zymo-technical laboratories and courses on the study of yeasts and mould fungi, of agricultural stations, forestry and dairy schools, and so on—all these are signs of the inexorable results of progress.

There are disadvantages, as the various *Centralblätter* and special journals show; for hurried work and feverish contentions for priority are apt to accompany these subdivisions of labour; and those of us who are most intimately concerned with the teaching of botany will do well to take heed of these signs of our times, and distinguish between the healthy specialisation inevitably due to the sheer weight and magnitude of our subject, and that incident on other movements and arising from other causes. The teaching and training in a university or school need not be narrow because its research-laboratories are famous for special work.

One powerful cause of modern specialisation is utility. The development of industries like brewing, dyeing, forestry, agriculture, with their special demands on botany, shows one phase; the progress of bacteriology, palæontology, pathology, economic and geographical botany, all asking special questions, suggests another. In each case men are encouraged to go more and more deeply into the particular problems raised.

Identification of flowers in Egyptian tombs, of pieces of wood in Roman excavations, the sorting of hay-grasses for analysis, or seeds in the warehouses; the special classifications of seedlings used by foresters, or of trees in winter, and so on, all afford examples. It is carried far, as witness the immense labour it is found worth while for experts to devote to the microscopic analysis of seeds and fruits liable to adulteration, or to the recognition of the markings in imprints of fossil leaves, or of characters like leaf-scars, bud-scales, lenticels, and so on, by which trees may be determined even from bits of twigs.

If we look at the great groups of plants from a broad point of view, it is remarkable that the Fungi and the Phanerogams occupy public attention on quite other grounds than do the Algæ, Mosses, and Ferns. Algæ are especially a physiologists' group, employed in questions on nutrition, reproduction, and cell-division and growth; the Bryophyta and Pteridophyta are, on the other hand, the domain of the morphologist concerned with academical questions such as the Alternation of Generations and the Evolution of the higher plants.

Fungi and Phanerogams, while equally or even more employed by specialists in Morphology and Physiology, appeal widely to general interests, and evidently on the ground of utility. Without saying that this enhances the importance of either group, it certainly does induce scientific attention to them.

I need hardly say that comparisons of the kind I am making, invidious though they may appear, in no way imply detraction from the highest honour deservedly paid to men who, like Thuret, Schmitz, and Thwaites in the past, and Bornet, Wille, and Klebs in the present, have done and are doing so much to advance our academical knowledge of the Algæ; and Klebs' recent masterpiece of sustained physiological work, indeed, promises to be one of the most fruitful contributions to the study of variation that even this century has produced. Nor must we in England forget Farmer's work on *Ascophyllum*, and on the nuclei and cell-divisions of *Hepaticæ;* and while Bower and Campbell have laid bare by their indefatigable labours the histological details of the Mosses and Vascular Cryptogams, and carried the questions of Alternation of Generations and the evolution of these plants so far, that it would almost seem little remains to be done with Hoffmeister's brilliant conception but to ask whither it is leading us; the genetic relationships have become so clear, even to the details, that the recent discovery by Ikeno and Hirase of spermatozoids in the pollen tubes of *Cycas* and *Gingko* almost loses its power of surprising us, because the facts fit in so well with what was already taught us by these and other workers.

It is impossible to over-estimate the importance of these comparative studies, not only of the recent Vascular Cryptogams, but also of the Fossil Pteridophyta, which, in the hands of Williamson, Scott, and Seward, are yielding at every turn new building stones and explanatory charts of the edifice of Evolution on the lines laid down by Darwin.

All these matters, however, serve to prove my present contention, that the groups referred to do not much concern the general public; whereas, on turning to the Fungi and Phanerogams, we find quite a different state of affairs. It is very significant that a group like the Fungi should have attracted so much scientific attention, and aroused popular interest at the same time. In addition to their importance from more academical points of view—for they claim the attention of morphologist and physiologist as much as any group, as the work of Wager, Massee, Trow, Hartog, and Harper, and an army of continental investigators, with Brefeld, Von Tavel, Magnus, &c., at their head, has shown—the Fungi appeal to wider interests on many grounds, but especially on that of utility. The fact that Fungi affects our lives directly has been driven home, and whether as poisons or foods, destructive moulds or fermentation-agents, parasitic mildews or disease germs, they occupy more of public interest than all other Cryptogams together, the flowering plants alone rivalling them in this respect.

A marked feature of the period we live in will be the great advances made in our knowledge of the uses of plants. Of course, this development of Economic Botany has gone hand in hand with the progress of Geographical Botany and the extension of our planting and other interests in the colonies, but the useful applications of Botany to the processes of home industries are increasing also.

The information acquired by travellers exploring new countries, by orchid-collectors, prospectors for new fibres or india-rubber, or resulting from the experiences of planters, foresters, and observant people, living abroad, has a value in money which does not here concern us; but it has also a value to science, for the facts collected, the specimens brought home, the processes observed, the results of analyses, the suggestions gathered—in short, the puzzles propounded by these wanderers—all stimulate research, and so have a value not to be expressed in terms of money.

The two react mutually, and I am convinced that the stimulus of the questions asked by commerce of botanical science has had, and is having, an important effect in promoting its advance. The best proof to be given of the converse—that Botany is really useful to commerce—is afforded by the ever-increasing demands for answers to the questions of the practical man. At the risk of touching the sensibilities of those who maintain that a university should regard only the purely academical aspects of a science, I propose to discuss some cases where the reciprocal influences of applied, or useful, and purely academic or useless botany—useless because no use has yet been made of it, as some one has wittily put it—have resulted in gain to both. In doing this, I wish to clearly state my conviction that no scientific man should be guided or restricted in his investigations by any considerations whatever as to the commercial or money value of his results; to patent a method of cultivating a bacillus, to keep secret the composition of a nutritive medium, to withhold any evidence, is anti-scientific, for by the nature of the case it is calculated to prevent improvement—*i.e.* to impede progress. It is not implied that there is anything intrinsically wrong in protecting a discovery: all I urge is that it is opposed to the scientific spirit.

But the fact that a scientific spirit is found to have a commercial value also—for instance, Wehmer's discovery that the mould fungus, *Citryomyces*, will convert 50 per cent. of the sugar in a saccharine solution to the commercially valuable citric acid: or Matruchot's success in germinating the spores of the mushroom, and in sending pure cultures of that valuable agaric into the market—is no argument against the scientific value of the research. There are in agriculture, forestry, and commerce generally, innumerable and important questions for solution, the investigation of which will need all the powers of careful observation, industrious recording, and thoughtful deduction of which a scientific man is capable. But while I emphatically regard these and similar problems as worthy the attention of botanists, and recognise frankly their commercial importance, I want to carefully and distinctly warn all my hearers against supposing that their solution should be attempted simply because they have a commercial value.

It is because they are so full of promise as scientific problems, that I think it no valid argument against their importance to theoretical science that they have been suggested in practice. In all these matters it seems to me we should recognise that practical men are doing us a service in setting questions, because they set them definitely. In the attempt to solve these problems we may be sure science will gain, and if commerce gains also, so much the better for commerce, and indirectly for us. But that is not the same thing as directly interesting ourselves in the commercial value of the answer. This is not our function, and our advice and researches are the more valuable to commerce the less we are concerned with it.

It is clear that the magnitude of the subject referred to is far beyond the measure of our purpose to-day, and I shall restrict myself to a short review of some advances in our knowledge of the Fungi made during the last three decades.

Little more than thirty years ago we knew practically nothing of the life-history of a fungus, nothing of parasitism, of infectious diseases, or even of fermentation, and many botanical ideas now familiar to most educated persons were as yet unborn. Our knowledge of the physiology of nutrition was in its infancy, even the significance of starches and sugars in the green-plant being as yet not understood; root-hairs and their importance were hardly spoken of; words like *heterœcism*, *symbiosis*, *mycorhiza*, &c., did not exist, or the complex ideas they now connote were not evolved. When we reflect on these facts, and remember that *bacteria* were as yet merely curious "animalculæ," that rusts and smuts were generally supposed to be emanations of diseased states, and that "spontaneous generation" was a hydra not yet destroyed, we obtain some notion of the condition of this subject about 1860.

As with other groups of plants, so with the Fungi, the first studies were those of collecting, naming and classifying, and prior to 1850 the few botanists who concerned themselves with these cryptogams at all were systematists. So far as the larger fungi are concerned, the classification attained a high degree of perfection from the point of view of an orderly arrangement of natural objects, and the student of to-day may well look back at the keen observation and terse, vivid descriptions of these older naturalists, which stands in sharp contrast to much of the more slovenly and hurried descriptive work which followed.

It may be remembered that even now we rely mainly on the descriptions and system of Fries (1821–1849) for our grouping of the forms alone considered as fungi by most people, and indeed we may regard him as having done for fungi what Linnæus did for flowering plants.

But, as you are aware, a large proportion of the Fungi are microscopic, and in spite of the conscientious and beautiful work of several earlier observers, among whom Corda stands pre-eminent, the classification and descriptions of the thousands of forms were rapidly bringing the subject into chaos.

The dawn of a new era in Mycology was preparing, however. A few isolated observers had already begun the study of the development of Fungi, but their work was neglected, till Persoon and Ehrenberg at the beginning of this century again brought the subject into prominence, and then came a series of discoveries destined to stimulate work in quite other directions.

The Tulasnes may be said to have brought the old period to a close, and prepared the way for the new one; they combined the powers of accurate observation with a marvellous faculty of delineation, and applied the anatomical method to the study of fungi with more success than ever before. Their new departure, however, is more evident in their selection of the parasitic fungi for study, and you all know how indispensable we still find their drawings of the germinating spores of the Smuts and Rusts. It is difficult to say which of their works is the most masterly, but probably the study of the life-history of *Claviceps purpurea* deserves first place, though successive memoirs on the Uredineæ, Ustilagineæ, Peronosporeæ, Tuberaceæ, and then that magnificent work, the "Selecta Fungorum Carpologia," cannot be forgotten.

In England, Berkeley was the man to link the period previous to 1860 with the present epoch. A systematist and observer of high power, and with a rare faculty for appreciating the labours of others, this grand old naturalist did work of unequalled value for the period, and the student who wishes to learn what was the state of mycology about this time will find it nowhere better presented than in Berkeley's works, one of which—his "Introduction to Cryptogamic Botany"—is a classic.

Like all classifications in botany, however, that of the fungi now took two courses: one in the hands of those who collated names and herbarium-specimens, and proposed cut and dried, but necessary and from a certain point of view very complete systems of classification, and those who, generalising from actual cultures and observation of the living plant, proposed outline schemes, the details of which should be filled in by their successors.

No one who knows the history of botany during this century will deny that it is to the genius of De Bary that we owe the foundation of modern mycology, for it was this young Alsatian who, though profoundly influenced by the work of Von Mohl and Schleiden on the one hand, and of Unger and the Tulasnes on the other, refused to follow either the school of the phytotomists—though his laborious "Comparative Anatomy of the Ferns and Phanerogams" shows how well equipped he was to be a leader in that direction—or that of the anatomical mycologists. No doubt the influence of Cohn, Pringsheim, and others of that new army of microscopists who were teaching the necessity of continued observation of living organisms under the microscope, can be traced in impelling De Bary to abandon the older methods; but his own unquestionable originality of thought and method came out very early in his investigations on the Lower Algæ and Fungi. If I may compare a branch of science to an arm of the sea, we may look on De Bary's influence as that of a Triton rising to a surface but little disturbed by currents and eddies. The sudden upheaval of his genius set that sea rolling in huge waves, the play of which is not yet exhausted.

The birth and flow of the new ideas, expressed in far-reaching generalisations and suggestions which are still moving, led to the revolutions in our notions of polymorphism, parasitism, and the real nature of infection and epidemics. His development of the meaning of sexuality in Fungi, his startling discovery of heterœcism, his clear exposition of symbiosis, and even his cautious and almost wondering whisper of chemotaxis were all fruitful, and although the questions of enzyme-action and fermentation were not made peculiarly his own, he saw the significance of these and many other phenomena now grown so important, and here, as elsewhere, thought clearly and boldly, and criticised fearlessly with full knowledge and justice.

I do not propose to occupy our time with even a sketch of the history of these and other ideas of this great botanist; but rather pass to the consideration of a few of the results of some of them in the hands of later workers, in schools now far developed and widely independent of one another, but all deeply indebted to the genial little man whom we so loved and revered.

The most marked feature noticed in the founding of the new schemes of classification of the Fungi was the influence of the results of pure and continuous cultures introduced by De Bary. The effect on those who followed can best be traced by examining the great systems of subsequent workers, led by Brefeld and Van Tieghem, and the writings of our modern systematists. This task is beyond my present scheme, however, and there is only time to remind you of the fungus floras of Saccardo, Constantin, Massee, and others, in this connection.

The word "fermentation" usually recalls the ordinary processes concerned in the brewing of beer and the making of wines and spirits; but we must not forget that the word connotes all decompositions or alterations in the composition of organic substances induced by the life-activities of Fungi, and that it is a mere accident which brings alcoholic fermentation especially into prominence.

I ventured some time ago to term alcoholic fermentation the oldest form of microscopic gardening practised by man, and this seems justified by what we know of the very various and very ancient processes in this connection.

But the making of beers, wines, and spirits, as we understand them, constitutes but a small part of the province of fermentation, and even when we have added cider and perry, ginger-beer, and the various herb and spruce beers to the list, we have by no means exhausted the tale of fermented drinks. Palm-wines of various kinds, toddy, pulque, arrack, kava, and a number of tropical alcoholic fermented liquors have to be included, and the koumiss and kephir of the Caucasus, the curious Russian kwass, the Japanese saké, and allied rice-preparations must be mentioned, to say nothing of the now almost forgotten birch-beer, mead and metheglin, and various other strange fermented decoctions of our forefathers' time, or confined to out-of-the-way localities.

In all these cases the same principal facts come out—a saccharine liquid is exposed to the destructive action of fungi, which decompose it, and we drink the altered or fermented liquor. As is now well known, the principal agents in these fermentations are certain lower forms of fungi called yeasts, and since Leeuwenhoeck, of Delft, discovered the yeast cells two hundred years ago, and La Tour, Schwann, and Kützing (about 1840) recognised them as budding plants, living on the sugar of the liquid, and which must be classed as Fungi, the way was paved for two totally different inquiries concerning yeast.

One of these was the fruitful one instigated by Pasteur's genius about 1860, and concerned the functions of yeast in fermentation. In the hands of Naegeli, Brefeld, and others abroad, and of A. J. and Horace Brown and Morris and others in England, Pasteur's line of research was rapidly developed, and, as we all know, has had a wide influence in stimulating investigation and in suggesting new ideas; and although the theory of alcoholic fermentation itself has not withstood all the criticism brought against it, and seems destined to receive its severest blow this year by E. Buchner's isolation of the alcoholic enzyme, we must always honour the school which nursed it.

The divergent line of inquiry turned on the origin and morphological nature of yeast. What kind of a fungus is yeast, and how many kinds or species of yeasts are there?

Reess, in 1870, showed the first steps on this long path of inquiry, and gave the name *Saccharomyces* to the fungus, showing that several species or forms existed, some of which develop definite spores.

In 1883, Hansen, of Copenhagen, taking advantage of the strict methods of culture introduced and improved by De Bary, Brefeld, Klebs, and other botanists, had shown that by cultivating yeast on solid media from a single spore it was possible to obtain constant types of pure yeasts, each with its own peculiar properties.

One consequence of Hansen's labours was that it now became possible for every brewer to work with a yeast of uniform type instead of with haphazard mixtures, in which serious disease forms might predominate and injure the beer. Another consequence soon appeared in Hansen's accurate diagnosis of the specific or varietal characters of each form of yeast, and among other things he showed that a true yeast may have a mycelial stage of development. The question of the nucleus of the yeast-cell, on which Mr. Wager will enlighten us, has also occupied much attention, as have also the details of spore formation.

Meanwhile, a question of very general theoretical interest had arisen.

Reess, Zopf, and Brefeld had shown that many higher fungi can assume a yeast-like stage of development if submerged in fluids. Various species of *Mucor*, *Ustilago*, *Exoascus*, and as we now know, numerous Ascomycetes and Basidiomycetes as well, can form budding cells, and it was natural to conclude that probably the yeasts of alcoholic fermentation are merely reduced forms of these higher fungi, which have become habituated to the budding condition—a conclusion apparently supported by Hansen's own discovery that a true *Saccharomyces* can develop a feeble but unmistakable mycelium.

With many ups and downs this question has been debated, but as yet we do not know that the yeasts of alcoholic fermentations can be developed from higher fungi.

During the last two years it appeared as if the question would be settled. Takamine stated that the *Aspergillus* used by the Japanese in brewing saké from rice develops yeast-like cells which ferment the sugar derived from the rice. Jühler and Jorgensen then extended these researches and claimed to have found yeast-cells on other forms of fungi on the surface of fruits, and to have established that they develop endogenous spores—an indispensable character in the modern definition of the genus *Saccharomyces*—and cause alcoholic fermentation.

Klöcker and Schiönning have this last year published the results of their very ingenious and thorough experimental inquiry into this question, and find, partly by pure cultures of the separate forms, and partly by means of excellently devised cultures on ripening fruits still attached to the plant, but imprisoned in sterilised glass vessels, that the yeasts and the moulds are separate forms, not genetically connected, but merely associated in nature, as are so many other forms of yeasts, bacteria and moulds.

It is interesting to notice how here, as elsewhere, the lessons taught by pure cultures are found to bear fruit, and how Hansen's work justifies the specialist's laboratory.

Among the most astonishing results that have come to us from such researches are Hansen's discoveries that several of the yeasts furnish quite distinct races or varieties in different breweries in various parts of the world, and it seems impossible to avoid the conclusion that their race characteristics have been impressed on the cells by the continued action of the conditions of culture to which they have so long been exposed—they are, in fact, domestic races.

Much work is now being done on the action of the environment on yeasts, and several interesting results have been obtained. One of the most striking examples is the fact observed by Sauer, who found that a given variety of yeast, whose activity is normally inhibited when the alcohol attains a certain degree of concentration in the liquid, can be induced to go on fermenting until a considerably higher proportion of alcohol is formed if a certain lactic-acid bacterium is added to the fermenting liquor. The bacterium, in fact, prepares the way for the yeast. Experiments have shown that much damage may be done to beers and wines by foreign or weed germs gaining access with the yeasts, and Hansen has proved that several yeasts are inimical to the action of the required fermentation. But not all pure fermentations give the desired results: partly because the race-varieties of even the approved yeasts differ in their action, and partly, as it appears, on account of causes as yet unknown.

There are facts which lead to the suspicion that the search for the best possible variety of yeast may not yield the desired results, if this particular form is used as a pure culture. The researches of Hansen, Rothenbach, Delbrück, Van Laer, and others, suggest that associated yeasts may ferment better than any single yeast cultivated pure, and cases are cited where such a symbiotic union of two yeasts of high fermenting power has given better results than either alone.

If these statements are confirmed, they enhance the theoretical importance of some investigations I had made several years previously. English ginger-beer contains a curious symbiotic association of two organisms—a true yeast and a true bacterium—so closely united that the yeast-cells imprisoned in the gelatinous meshes of the bacterium remind one of the gonidia of a lichen entangled in the hyphæ of the fungus, except that there is no chlorophyll. Now it is a singular fact that this symbiotic union of yeast and bacterium ferments the saccharine liquid far more energetically than does either yeast or bacterium alone, and results in a different product, large quantities of lactic and carbonic acids being formed, and little or no alcohol.

In the kephir used in Europe for fermenting milk, we find another symbiotic association of a yeast and a bacterium; indeed, Freudenreich declares that four distinct organisms are here symbiotically active and necessary, a result not confirmed by my as yet incomplete investigation. I know of at least one other case which may turn out to be different from either of the above. Moreover, examples of these symbiotic fermentations are increasing in other directions.

Kosai, Yabe, and others have lately shown that in the fermentations of rice to produce saké, the rice is first acted on by an *Aspergillus*, which converts the starch into sugars, and an associated yeast—hitherto regarded as a yeast-form of the *Aspergillus*, but, as already said, now shown to be a distinct fungus symbiotically associated with it—then ferments the sugar, and other similar cases are on record.

Starting from the demonstrated fact that the constitution of the medium profoundly affects the physiological action of the fungus, there can be nothing surprising in the discovery that the fungus is more active in a medium which has been favourably altered by an associated organism, whether the latter aids the fungus by directly altering the medium, or by ridding it of products of excretion, or by adding some gas or other body. This granted, it is not difficult to see that natural selection will aid in the perpetuation of the symbiosis, and in cases like that of the ginger-beer plant it is extremely difficult to get the two organisms apart, reminding us of the similar difficulty in the case of the soredia of Lichens. Moreover, experiments show that the question of relative abundance of each constituent affects the matter.

I must now return for a moment to Buchner's discovery that by means of extremely great pressures a something can be expressed from yeast which at once decomposes sugar into alcohol and carbon-dioxide, and concerning which Dr. Green will inform us more fully. This something is regarded by Buchner as a sort of incomplete protoplasm—a body composed of proteid, and in a structural condition somewhere between that of true soluble enzymes like invertin and complete living protoplasm.

If this is true, and Buchner's *zymase* turns out to be a really soluble enzyme, the present theory of alcoholic fermentation will have to be modified, and a reversion made towards Traube's views of 1858, a reversion for which we are in a measure prepared by Miquel's proof in 1890 that *Urase*, a similar body extracted from the urea-bacteria, is the agent in the fermentation of urea. At present, however, we are not sufficiently assured that the body extracted by Buchner is really soluble, and I am told that very serious difficulties still face us as to what solution is. The enormous pressures required, and the fact that the "solution" coagulates as a whole, might suggest that he was dealing with expressed protoplasm, still alive, but devoid of its cell-wall; against this, however, must be urged the facts that the "solution" can be forced through porcelain and still act, and this even in the presence of chloroform.

We may fairly expect that the further investigation of Buchner's "zymase," Miquel's "urase," and the similar body obtained by E. Fischer and Lindner from *Monilia candida* will help in deciding the question as to the emulsion theory of protoplasm itself.

In any case, soluble or not, these enzymes are probably to be regarded as bits off the protoplasm, as it were, and so the essentials of the theory of fermentation remain, the immediate machinery being not that of protoplasm itself, but of something made by or broken off from it. Enzymes, or similar bodies, are now known to be very common in plants, and the suspicion that fungi do much of their work with their aid is abundantly confirmed.

Payen and Persoz discovered diastase in malt extract in 1833, and in 1836 Schwann discovered peptase in the juices of the animal stomach. Since that time several other enzymes have been found in both plants and animals, and the methods for extracting them and for estimating their actions have been much improved, a province in which Horace Brown, Green, and Vines have contributed results.

It seems not improbable that there exists a whole series of these enzymes which have the power of carrying over oxygen to other bodies, and so bringing about oxidations of a peculiar character. These curious bodies were first observed owing to studies on the changes which wine and plant juices undergo when exposed to the action of the oxygen of the air.

In the case of the wine certain changes in the colour and taste were traced to conditions which involved the assumption that some body, not a living organism, acts as an oxygen-carrier, and the activity of which could be destroyed by heating and antiseptics. It was found that similar changes in colour and taste could be artificially produced by the action of ozone, or by passing an electric current through the new wine; indeed, it is alleged that the ageing of wine can be successfully imitated by these devices, and is actually a commercial process.

The browning of cut or broken apples is now shown to be due to the action of a similar oxydase—*i.e.* an oxygen-carrying ferment, and the same is claimed for the deep-colouring of certain lacks, or lackers, obtained from the juice of plants such as the *Anacardiaceæ*, which are pale and transparent when fresh drawn, but gradually darken in colour on exposure to air. Bertrand found in these juices an oxydase, which he terms *laccase*, and which affects the oxygen-carrying, and converts the pale fluid juice to a hard dark brown varnish.

Other oxydases have been isolated from beets, *dahlia*, potato-tubers, and several other plants.

These discoveries led Bourquelot and Bertrand in 1895 to the explanation of a phenomenon long known to botanists, and partly explained by Schönbein as far back as 1868. If certain Fungi (e.g. *Boletus luridus*) are broken or bruised, the yellow or white flesh at once turns blue: the action is now traced to the presence in the cell-sap of an oxydase, the existence of which had been suspected but not proved, and the observers named assert that many fungi (59 out of 107 species examined) contain such oxydases.

It will be interesting to see how far future investigations support or refute the suggestion that many of the colour-changes in diseased tissues of plants attacked by fungi are due to the action of such oxydases.

Wortmann, in 1882, showed that bacteria, which are capable of secreting diastase, can be made to desist from secreting this enzyme if a sufficient supply of sugar be given them, and since then several instances have been discovered where fungi and bacteria show changes in their enzyme actions according to the nature of their food supply. Nor is this confined to fungi. Brown and Morris, in 1892, gave evidence for the same in the seedlings of grasses: as the sugar increased, the production of diastase diminished.

It is the diastatic activity of *Aspergillus* which is utilised in the making of saké from rice in Japan, and in the preparation of soy from the soja-bean in the same country, and a patented process for obtaining diastase by this means exists; and Katz has recently tested the diastatic activity of this fungus, of *Penicillium*, and of *Bacterium magatherium* in the presence of large and small quantities of sugar. All three organisms are able to produce not only diastase, but also other enzymes, and the author named has shown that as the sugar accumulates the diastase formed diminishes, whereas the accumulation of other carbo-hydrates produces no such effect.

Hartig's beautiful work on the destruction of timber by fungi obtains new interest from Bourquelot's discovery of an emulsion-like enzyme in many such wood-destroying forms. This enzyme splits the Glucosides, Amygdalin, Salicin, Coniferin, &c., into sugars and other bodies, and the hyphæ feed on the carbo-hydrates. I purpose to recur to this subject in a communication to this Section. The fact that *Aspergillus* can form invertins of the sucrase, maltase, and trehalase types, as well as emulsin, inulase, diastase, or trypsin, according to circumstances of nutrition, will explain why this fungus can grow on almost any organic substratum it alights on, and other examples of the same kind are now coming to hand.

The secretion of special enzymes by fungi has a peculiar interest just now, for recent investigations promise to bring us much nearer to an understanding of the phenomena of parasitism than we could hope to attain a few years ago.

De Bary long ago pointed out that when the infecting germinal tube of a fungus enters a plant-cell, two phenomena must be taken into account, the penetration of the cell-walls and tissues, and the attraction which causes the tips of the growing hypha to face and penetrate these obstacles, instead of gliding over them in the lines of apparent least resistance.

The further development of these two themes has been steady and unobtrusive, and from various quite unexpected directions more light has been obtained, so that we are now in a position to see pretty clearly what are the principal factors involved in the successful attack of a parasitic plant on its victim or "host." That fungi can excrete cellulose-dissolving enzymes is now well known, and that they can produce enzymes which destroy lignin must be inferred from the solution of wood-cells and other lignified elements by tree-destroying fungi. Zopf has collected several examples of fungi which consume fats, and further cases are cited by Schmidt, by Ritthausen, and Baumann. In these cases also there can be no doubt that an enzyme or similar body is concerned.

There is one connection in which recent observations on enzymes in the plant-cell promise to be of importance in explaining the remarkable destructive action of certain rays of the solar-light on bacteria. As you are aware, the English observers Downes and Blunt showed long ago that if bacteria in a nutrient liquid are exposed to sunlight, they are rapidly killed. Further researches, in which I have had some part, gradually brought out the facts that it is really the light rays and not high temperatures which exert this bactericidal action, and by means of a powerful spectrum and apparatus furnished by the kindness of Prof. Oliver Lodge I was able to obtain conclusive proof that it is especially the blue-violet and ultra-violet rays which are most effective. This proof depended on the production of actual photographs in bacteria of the spectrum itself. Apart from this, I had also demonstrated that just such spores as those of anthrax, at the same time pathogenic and highly resistent to heat, succumb readily to the action of these cold light-rays, and that under conditions which preclude their being poisoned by a liquid bathing them.

The work of Brown and Morris on the daily variations of diastatic enzyme in living leaves, and especially Green's recent work on the destructive action of light on this enzyme, point to the probability that it is the destruction of the enzymes with which the bacterial cells abound which brings about the death of the cell.

That these matters are of importance in limiting the life of bacteria in our streets and rivers, and that the sun is our most powerful scavenger, has been shown by others as well as myself. In this connection may also be mentioned Martinand's observations, that the yeasts necessary for wine-making are deficient in numbers and power on grapes exposed to intense light, and he explains the better results in Central France as contrasted with those in the South as largely due to this fact. Whether, or how far, the curious effects of too intense illumination in high latitudes and altitudes on plants which might be expected to grow normally there, can be explained by a destructive light action on the enzyme of the leaves, has not, so far as I know, been tested; but Green's experiments certainly seem to me to point to the possibility of this, as do the previous experiments with screens of Pick, Johow, myself, and others.

It is interesting to note that Wittlin and others have confirmed the conclusion my own few trials with Röntgen rays led to; they show no action whatever.

That branch of mycology which is now looked upon by so many as a separate department of science, usually termed bacteriology, only took shape in the years 1875-79, when its founder, the veteran botanist Cohn, who recognised that the protoplasm of plants corresponded to the animal sarcode, and who has been recently honoured by our Royal Society, published his exact studies of these minute organisms, and prepared the way for the specialists who followed.

It is quite true that isolated studies and observations on bacteria had been made from time to time by earlier workers than Cohn, though it is usually overlooked that Cohn's first paper on Bacteria was published in 1853. Ehrenberg in particular had paid special attention to some forms; but neither he nor his successors can be regarded as having founded a school as Cohn did, and this botanist may fitly be looked upon as the father of bacteriology, the branch of mycology which has since obtained so much diversity.

It should not be overlooked that the first proof that a specific disease of the higher animals is due to a bacillus, contained in Koch's paper on Anthrax, was published under Cohn's auspices and in his "Beiträge zur Biologie der Pflanzen" in 1876, four years after Schroeter's work from the same laboratory on pigmented bácteria, and that the plate illustrating Koch's paper was in part drawn by Cohn.

It is of primary importance to recognise this detail of Koch's training under Cohn, because, as I have shown at length elsewhere, popular misapprehensions as to what bacteriology really consists in have been due to the gradual specialisation into three or four different schools or camps of a study which is primarily a branch of botany; and, again, it is of importance to observe that the whole of this particular branch of mycology, to which special laboratories and an enormous literature are now devoted, has arisen during the last quarter of a century, and subsequent to the foundation of scientific mycology by De Bary. When we reflect that the nature of parasitic fungi, the actual demonstration of infection by a fungus spore, the transmission of germs by water and air, the meaning and significance of polymorphism, heterœcism, symbiosis, had already been rendered clear in the case of fungi, and that it was by these and studies in fermentation and in the life-history of the fungus *Saccharomyces* that the way was prepared for the ætiology of bacterial diseases in animals, there should be no doubt as to the mutual bearings of these matters.

Curiously enough, it was an accident which deflected bacteriology along lines which have proved so significant for the study of this particular group of minute organisms, that an uninitiated visitor to a modern bacteriological laboratory (which in England, at any rate, is usually attached to the pathological department of a medical school) hardly perceives that he is in a place where the culture of microscopic plants is the chief object—for the primary occupation of a bacteriologist is really, after all, the cultivation of minute organisms by the method of "microscopic gardening," invented by De Bary, Klebs, and Brefeld, whether the medium of culture is a nutritive solution, or solid organic substrata like potato, agar, or gelatine, or the tissues of an animal.

This accident—I use the word in no disrespectful sense—was Koch's ingenious modification of the use of gelatine as a medium in which to grow bacteria: he hit upon the method of pouring melted gelatine containing distributed germs on to plates, and thus isolating the colonies.

Pasteur and Cohn had already coped with the difficulty of isolating mixed forms by growing them in special fluids. When a given fluid favoured one form particularly, a small quantity containing this predominant species was put into another flask of the fluid, then a drop from this flask transferred to a third flask, and so on, until the last flasks contained only the successful species, the others having been suppressed: these "fractional cultures" were brought to a high state of perfection by the botanist Klebs in 1873.

Then Brefeld (1872) introduced the method of dilution—*i.e.* he diluted the liquid containing his spores until each single drop taken contained on the average one spore or none, whence each flask of sterile nutritive solution receiving one drop contained either none or *one* spore. Brefeld was working with fungi, but Lister—now Lord Lister, and our late President—applied this "dilution method" to his studies of the lactic fermentation in 1878, and Naegeli, Miquel, and Duclaux carried it further, the two latter especially having been its chief defenders, and Miquel having employed it up to quite recently.

Solid media appear to have been first generally used by Schroeter in 1870, when he employed potatoes, cooked and raw, egg-albumen, starch-paste, flesh, &c. Gelatine, which seems to have been first employed by Vittadini in 1852, was certainly used by Brefeld as early as 1874, and even to-day his admirable lecture on "Methoden zur Untersuchung der Pilze" of that date is well worth reading, if only to see how cleverly he obtains a single spore isolated in gelatine under the microscope. Klebs used gelatine methods in 1873.

We thus see that when Koch proposed his method of preparing gelatine plate-cultures in 1881 he instituted, not a new culture-medium, for cultures on solid media, including gelatine, had been in use by botanists for eight or ten years; nor did he introduce methods for the isolation of spores, for this had been done long before. What he really did was to ensure the isolation of the spores and colonies wholesale, and so facilitate the preparation of pure cultures on a large scale, and with great saving of time.

It was a brilliant idea, and, as has been said, "the Columbus egg of Bacteriology"; but we must not lose sight of the fact that it turned the current of investigation of bacteria from the solid and trustworthy ground established by Cohn, Brefeld, and De Bary, into a totally new channel, as yet untried.

We must remember that De Bary and Brefeld had aimed at obtaining a single spore, isolated under the microscope, and tracing its behaviour from germination, continuously to the production of spores again; and when we learn how serious were the errors into which the earlier investigators of the mould-fungi and yeasts fell, owing to their failure to trace the development continuously from spore to spore, and the triumphs obtained afterwards by the methods of pure cultures, it is not difficult to see how inconclusive and dangerous all inferences as to the morphology of such minute organisms as bacteria must be unless the plant has been so observed.

As matter of fact, the introduction and gradual specialisation of Koch's methods of rapid isolation of colonies encouraged the very dangers they were primarily intended to avoid. It was soon discovered that pure cultures could be obtained so readily that the characteristic differences of the colonies in the mass could presumably be made use of for diagnostic purposes, and a school of bacteriologists arose who no longer thought it necessary to patiently follow the behaviour of the single spore or bacillus under the microscope, but regarded it as sufficient to describe the form, colour, markings, and physiological changes of the bacterial colonies themselves on and in different media, and were content to remove specimens occasionally, dry and stain them, and describe their forms and sizes as they appeared under these conditions.

To the botanist, and from the points of view of scientific morphology, this mode of procedure may be compared to what would happen if we were to frame our notions of species of oak or beech according to their behaviour in pure forests, or of a grass or clover according to the appearance of the fields and prairies composed more or less entirely of it, or—and this is a more apt comparison, because we can obtain colonies as pure as those of the bacteriologist—of a mould-fungus according to the shape, size, and colour, &c., of the patches which grow on bread, jam, gelatine, and so forth.

Now it is obvious that this is abandoning the methods of morphology, and the consequence has been that two schools of descriptive bacteriologists are working along different lines, and the "species" of the one—the test-tube school—cannot be compared with those of the other, the advocates of continuous culture from the spore.

The difficulty of isolating a bacterium and tracing its whole life-history under the microscope is so great, that the happy pioneers into the fascinating region opened up by the test-tube methods may certainly claim considerable sympathy in their cry that they cannot wait. Of course they cannot wait; no amount of argument will prevent the continual description of new test-tube "species," and all we can do is to go on building up the edifice already founded by the botanists Cohn, Brefeld, De Bary, Van Tieghem, Zopf, Prazmowski, Beyerinck, Fischer, and others who have made special studies of bacteria.

The objection that such work is slow and difficult has no more weight here than in any other department of science, and in any case the test-tube school is already in the plight of being frequently unable to recognise its own "species," as I have convinced myself by a long-continued series of cultures with the object of naming common bacteria.

I wish to guard myself against misconstruction in one particular here. It is not insinuated that the test-tube methods and results are of no value. Far from it; a vast amount of preliminary information is obtained by it; but I would insist upon the discouragement of all attempts to make "species" without microscopic culture; and continuous observation of the development as far as it can be traced.

The close connection between bacteriology and medicine has been mainly responsible for the present condition of affairs; but it is high time we recognised that bacteriology only touches animal pathology at a few points, and that the public learn that, so far from bacteria being synonymous with disease germs, the majority of these organisms appear to be beneficial rather than inimical to man. There is not time to attempt even a brief description of all the "useful fermentations" due to bacteria, but the following cases will point the conviction that a school of bacteriology, which has nothing to do with medical questions, but investigates problems raised by the forester, agriculturist, and gardener, the dairyman, brewer, dyer, and tanner, &c., will yet be established in England in connection with one or other of our great botanical centres.

(To be continued.)

PHYSICS AT THE BRITISH ASSOCIATION.

THE meeting of the American Association at Detroit and the central position of Toronto have contributed greatly in bringing together a large number of Canadian and American mathematicians and physicists to meet their co-workers on this side of the Atlantic The opportunity thus afforded of conference and exchange of ideas has been one of the chief pleasures of the meeting.

It was universally felt that the presidential address of Prof. Forsyth (pp. 374–378) formed a clear and eloquent exposition of the claims of pure mathematics, and at its close Lord Kelvin, in moving a vote of thanks, declared that any one in science who could possibly choose would elect to belong to the mathematical rather than the non-mathematical class. President London, of the Toronto University, in seconding the vote, said that the address was specially needed in Toronto, because the public there had accused the university of attaching too much importance to mathematics.

Mr. J. A. Paterson, in a paper on the unification of time, described the efforts made by several scientific societies in Canada to secure uniformity in the specification of time by astronomers, navigators and the public; the suggestion being that the day should commence and end at midnight, and the hours be counted from 0 to 24. The proposal gave rise to some discussion. Prof. Newcomb pointed out that navigators, making observations usually at noon, found that time most convenient as the commencement of the day, while astronomers for similar reasons would choose midnight. Prof. Rücker gave an account of the inquiries made by the British Royal Society, Foreign Office and Admiralty, from which it appears that any international agreement is at present hopeless; so that the Nautical Almanac for 1901 will be compiled in the same manner as its predecessors.

Prof. Rücker exhibited photographic records of objective combination tones, both summational and difference tones having been obtained. In this research he was assisted by Messrs. Forsyth and Sowter. The method and apparatus used were the same as in the investigation of Prof. Rücker and Mr. Edser—namely, the observation of interference bands produced by the light reflected from a mirror carried by a resonant tuning-fork; the shift of the bands by the motion of the fork was, however, photographed on a moving sensitive surface, instead of being observed by eye.

An account of the work of the Committee on Seismological Observations was given by its indefatigable secretary, Prof. Milne. An examination of earthquake records seems to show that sub-oceanic earthquakes and landslips are more frequent than those on land, and that the Tuscarora deep is the origin of many of them. The most important portion of the report is, however, that relating to the rate of propagation of seismic waves from their origin to various points on the earth's surface. The records show that the velocity of propagation increases with the distance travelled, so that most probably the wave goes through the earth and not round its superficial crust, the speed of transmission being greater in the interior than in the crust. This, as Lord Kelvin pointed out, indicates that the moduli of elasticity of the material of the earth's interior are greater than those of the crust, possibly because of the higher pressure at great depths.

At the meeting of the section on Friday, Dr. N. E. Dorsey described some careful experiments to determine the surface tension of water by the method of ripples, the results of which agree with those of M. Sentis, obtained by an entirely different method. In the case of dilute aqueous solutions, the surface tension obtained by this method is a linear function of the concentration.

Prof. Callendar and Mr. Barnes gave an account of their new method of measuring the specific heat of liquid, by passing an electric current through a fine tube through which a current of the liquid flows. The experiment is continued until the temperature-difference between the ends of the tube becomes steady; this temperature-difference and the rate of flow of the liquid are then measured. Loss of heat by radiation is almost eliminated by surrounding the tube with a vacuous chamber, and small losses are allowed for. Another important communication on calorimetry was that of Profs. Ewing and Dunkerley on the specific heat of superheated steam. Their method consists in passing saturated steam through a porous plug, thus superheating it; the results show that for 10° superheating at atmospheric pressure the specific heat is about 0·44, while the ordinarily accepted value, 0·48, is only correct if the superheating exceeds 25°, as in Regnault's experiments.

A crowded audience assembled to hear Lord Kelvin's paper on the fuel-supply and air-supply of the world. He argued that, as the earth was in all probability originally hot and liquid, no primeval vegetable fuel existed; further, no free oxygen existed at that period, since it is not found in gases evolved from minerals or in the spectra of stars. Probably, therefore, the oxygen of the air has resulted from the action of sunlight on plants, and as this oxygen would be furnished by 340 million million tons of fuel, we have an upper limit to the amount of fuel in the world. On the other hand, the British Coal Supply Commission of 1831 estimated the amount of available fuel in England and Scotland to be 146,000 million tons, which is greater than the average for the whole earth. It follows, then, that the oxygen of the atmosphere resting over Britain is insufficient to burn up the fuel of the country, and the cessation of life may possibly occur by asphyxiation rather than want of fuel. In the discussion on this paper Prof. Fitzgerald stated that, according to his calculations, the sun's energy will support five persons to every square metre, so that there is no fear of life becoming extinct by failure of the sun's energy, as some people have supposed.

In spectroscopy, Prof. Runge stated that Prof. F. Paschen and himself had succeeded in separating the spectrum of oxygen into six series, two principals each having two subordinates, the lines of one principal series and its subordinates being triple. The importance of this paper lies in the fact that the oxygen spectrum is shown to be analogous to that of helium; and as oxygen does not, so far as we know, contain a mixture of elements, the idea that helium is a mixture has now been abandoned. Profs. Runge and Paschen find also that the spectra of sulphur and selenium each give a principal series of lines and two subordinate series, but in each case one line occurs which does not fit into any of the series, and which may be the fundamental line of another series. Using the large grating of the Johns Hopkins University, Mr. W. J. Humphreys has succeeded in causing the lines in the arc spectra of metals to shift appreciably by increasing the pressure of the atmosphere surrounding the arc; in all cases increased pressure causes the wave-length of the lines to increase, the lines move towards the red end of the spectrum. The shift is of the same order of magnitude as the Doppler effect, but could be distinguished from the Doppler effect in a celestial spectrum by the fact that lines belonging to principal and subordinate series are differently shifted by pressure, whereas they are all displaced equally in the spectrum of a receding body. Dr. J. Larmor has discussed the subject mathematically, and finds that the displacement is of the same order as would be produced by change of specific inductive capacity of the air by pressure. Prof. Schuster has photographed a metallic spark-spectrum on a film moving rapidly at right angles to the slit of the spectroscope; the result shows that the air-lines flash out for an exceedingly short time: the metallic particles, however, remain luminescent for a much longer period with gradually diminishing intensity. He was able to trace the motion of the metallic particles from the electrodes to the middle of the spark, and to measure their velocity, which ranged from 400 to 2000 metres per second.

Prof. S. P. Thompson distinguished four varieties of kathode rays, differing in their power of exciting fluorescence, exciting X-rays, and deflexion by a magnet. The first kind is the ordinary kathode ray; the second kind is produced when kathode rays have fallen on a surface and produced X-rays (they have then lost their power of exciting more X-rays). The third variety arises when kathode rays are passed through a negatively charged metallic spiral or gauze-sieve; they cannot be deflected