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Die Spaltpilze. Von Dr. W. Zopf. 3rd Edition. (Breslau, 1885.)

THIS, the third edition, differs in no essential respect from its predecessors. Zopf still adheres to the original proposition of Von Nägeli, that the various forms of schyzomycetes are not permanent species (Cohn), but various stages in the development of the same organism. This proposition is derived from observations of the morphological characters only, and is not based on sufficiently exact methods of *pure cultivation.*

The sections treating of the physiology and chemistry of the bacteria will be found very valuable. A complete and alphabetically-arranged bibliography at the end of the work is the best as yet published.

E. KLEIN

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Evolution of Phanerogams

MUCH as I dislike controversy occasions arise when it must be faced; and Mr. Starkie Gardner's notice of the two new volumes by MM. Marion and Saporta (p. 289) calls for a reply. Personally I am obliged by Mr. Gardner's obvious desire to do justice to my views; but he must excuse me if I say that some of the "main facts" on which he relies are, like similar ones employed by the two French writers, charmingly independent of anything that I can find existing in nature.

Through the kindness of my accomplished friend, the Marquis of Saporta, I received copies of his two volumes as soon as they were published. On perusing his descriptions of the carboniferous direct accidental inj... an animal trespassing upon a plo... his dung upon it), errors in weighing, e... from the ground, and other unavoidable diffic... belong to the carrying out of field experiments, ... these errors are magnified in the case of small plots, a... minimised by the use of large ones. In these directions the criticisms made by Sir Thomas Acland are valuable: but we should like to have seen a greater sympathy with an honest effort, and less anxiety to hold up any results of value as stale, antiquated, and unnecessary.

Any one who has lived as long as Sir Thomas Dyke Acland must know that the proclamation of things old as things new is not confined to agricultural chemists, and he should be more ready to accept as inevitable the *dictum* of the wise man, that "the thing that hath been, it is that which shall be; and that which is done is that which shall be done."

THE NEW EDITION OF "YARRELL'S BRITISH BIRDS"

A History of British Birds. By the late William Yarrell, V.P.L.S., F.Z.S. Fourth Edition, Revised to the End of the Second Volume by Alfred Newton, M.A., F.R.S., continued by Howard Saunders, F.L.S., F.Z.S. Parts xx.–xxx. (London: Van Voorst.)

THE students of British birds have at last received the two final numbers of the new edition of Yarrell's celebrated work on their favourite subject, which was commenced as long ago as 1871. Fourteen years, it must be acknowledged, is a long time to wait, but on the other hand the subscribers to the new "Yarrell" have in compensation of the delay not what would be called in ordinary parlance a new edition, but what is, in fact, a complete and exhaustive summary of the present state of plants I found numerous statements with which I could not agree. Some of these statements refer to questions of facts; others to inferences drawn from real or imaginary facts. Having long enjoyed the valued privilege of a correspondence with my distinguished friend I sent to him a lengthy criticism of parts of his new volume which I thought to be seriously misleading; either because matters of fact were so exhibited as to convey erroneous impressions, and hence, practically, to become not facts—or because they were made to justify conclusions which the facts themselves, rightly stated, would not do. At the same time I gave my correspondent warning that I might have to correct what I regarded as his erroneous or misleading statements.

Mr. Gardner's article leads me to fulfil this announcement sooner than I intended, since he, in turn, has so far countenanced some of what I regard as the errors of the two French palæontologists as to make them his own. Like Mr. Gardner, M. Saporta had previously pointed out to me that the aim and object of his volumes did not necessarily involve interference with matters that have so long been in dispute between M. Rénault, M. Grand'-Eury, and myself. To this I could only reply that in his new work he had repeatedly shown his acceptance of views of these two palæontologists involving both facts and inferences, which I believe to be seriously erroneous. The space which NATURE can afford me will not suffice fully to review all of what I regard as the objectionable parts of the two volumes under consideration, but I may be allowed to make some comments, including some extracts from my letter to M. Saporta, indicating the nature of my objections both to his conclusions and to the comments made upon them by Mr. Gardner.

The latter gentleman makes one statement which I cannot endorse. Because MM. Rénault, Grand'-Eury, and Saporta all adopt the views of M. Brongniart he thinks it hardly possible that they can all be mistaken. This argument cuts both ways—Mr. Gardner applies it to the subject of Calamites *versus* Calamadendron. On this subject I may retort that when such men as Schimper, Weiss, Stur, and perhaps my prolonged investigation of the subject justifies my adding myself, take an opposite view of the matter in debate, it may possibly be equally impossible that we, with our vast array of specimens in our cabinets, should all be mistaken! This *argumentum ad hominem* therefore falls to the ground. I may be allowed to wonder that it should ever have been advanced.

The first point to which I would call attention shows that such men as those quoted may blunder and have blundered. I now refer to the subject of the relations of Lepidodendron and Sigillaria to each other and to the rest of the plant world. That I have for many years insisted upon the cryptogamic character of, and the close affinity existing between, both these genera is well known; and equally so, that many of the French palæontologists have followed M. A. Brongniart in regarding the Lepidodendra as Lycopodiaceous plants whose stems contain no exogenous vascular cylinder, whilst all those plants that possessed such a cylinder (a product of a Cambium layer) which they believed to be the case with Sigillariæ must, *de facto*, be Gymnosperms. That this dispute has now been settled in my favour by an important recent discovery does not seem to be known to Mr. Gardner. M. Zeiller has obtained strobili of Sigillaria which have settled the matter even in the opinion of most of the Parisian botanists. Those strobili contain spores, not seeds. This discovery demonstrates the cryptogamic character of Sigillaria, and deals a final blow at the Gymnospermous hypothesis held by the four observers in whose combined infallibility Mr. Gardner expresses such confidence.

My first friendly complaint against the authors of the "Evolution of the Phanerogams" is that they disregard proven facts when such facts inconveniently oppose their theories. *Imprimis*, they became aware of M. Zeiller's important discovery whilst their volumes were passing through the Press. Though this is a sufficient reason for only noticing it in a footnote, it does not justify their very slight recognition of its bearing upon so many pages of their arguments, of which it effectually disposes. It absolutely establishes the fact that *some* Sigillariæ, at least, are *not* Gymnosperms but Cryptogams; which fact, superadded to the many identities of structure in Sigillaria and Lepidodendron, which I have repeatedly shown to exist, renders it increasingly probable that the above statement is applicable to *all* Sigillariæ. At least, it now throws upon the opponents of that statement the onus of proving the contrary to be true, which they have not done.

Several years ago the late Mr. Binney described what he believed to be two plants—the *Lepidodendron vasculare* and the *Sigillaria vascularis*. That the only difference between these two was the possession, by the latter, of an exogenous zone, not seen in the former, was recognised by Mr. Binney. I have shown in a way, which I claim to be unanswerable, that these are one and the same plant which the external and internal characteristics alike demonstrate to be a Lepidodendron. Hence I complain to M. Saporta, "You continue to speak of *Sigillaria* vascularis. I reply that there is no such plant; and to speak of the *Lepidodendron* under that name, after all that I have done in illustration of its organisation, is unfair to me, besides seeming to support M. Rénault's absurd conclusion that an exogenous or centrifugal zone is incompatible with the possibility of a plant possessing such a zone being a Lepidodendron." I then state "further, after enumerating M. Rénault's three supposed types of Lepidodendron, from which he excludes all possibility of the existence of an exogenous zone, you say, 'ce sont les traits *essentiels* des types caulinaires Lepidodendroides.'

"I reply in language as strong as I can possibly use that this is not true. The development of an exogenous zone in the more advanced stages of a Lepidodendron's life is the rule rather than the exception."

After citing numerous proofs of this statement I say in reference to Sigillaria: "It is further a mistake to say that 'ces tiges nous sont principalement connues par les Sigillaria elegans et spinulosa.' We possess the vascular axis of the Sigillaria figured in my Memoir II., Fig. 39. This axis is identical in the minutest details of its organisation with those of the Diploxyloid Lepidodendra, and I have sections of Sigillaria reniformis which are, in structure, equally Lepidodendroid. I ask, therefore, what are the 'diversités appréciables' to what you refer on p. 23, and what ground have you for saying that this double fibro-ligneous region is 'sans analogie avec ce qui existe dans les tiges connues des Lepidodendrées'?"

On this part of the disputed questions I must object to a statement made by Mr. Gardner, in which he says that the structure of Lepidodendron "presents nothing unusual to Cryptogams." Surely a thick *exogenously developed* cylinder of scalariform vessels, arranged in radiating laminæ, separated by true medullary rays, the entire structure being produced by a Cambium zone, is very unusual in Cryptogams. Mr. Gardner then proceeds, as M. Saporta would do, to describe a contrast which has no real existence. "But in Sigillaria, a plant strongly resembling it in nearly every other respect, we find a radiating vascular or woody zone in the cellular stem with unmistakable exogenous growth. It is richly supplied with medullary rays, and, Prof. Williamson allows, presents clear evidence of interruptions to growth succeeded by periods of renewed vital activity." I allow, and never have allowed anything of the kind,[1] if this means my admission that something exists in Sigillaria that does not exist in most Lepidodendra. Mr. Gardner further represents me as believing that "the typical Lepidodendron never produced a ligneous zone." I believe the reverse of this; viz. that a development of such a zone sooner or later was characteristic of most Lepidodendra. True there are some Lepidodendra in which I have not yet discovered such a zone; but I am far from supposing that even in them such a zone will not ultimately be discovered. Anyhow the *typical* Lepidodendron can no longer be regarded as one from which this zone is absent. Mr. Gardner, after the passages quoted above, says: "In Diploxylon there is a further development, the woody zone being made up of an inner or medullary vascular cylinder either interrupted or continuous, composed of large scalariform vessels without definite order, and an outer cylinder of scalariform vessels of smaller size arranged in radiating fasciculi." What does this "further development" mean? This description is simply that of *every* exogenous Lycopodiaceous axis found in the coal measures, whether of Lepidodendron or of Sigillaria. Diploxylon, *as a genus*, has no longer any existence. The term is now useful only as an adjective descriptive of a condition of growth common alike to Lepidodendron and to Sigillaria, as well as to several other genera of Carboniferous plants. Unless I misunderstand Mr.

[1] I may here observe that conspicuous or even visible interruptions to growth are very rare amongst these coal plants They are only very *conspicuous* in my genus Amyelon; but we also find traces of them in Stigmarian roots and in Lygenodendron. Generally these Carboniferous stems suggest the reverse of changing seasons or periodic interruptions of growth.

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Gardner, he here employs words designed to suggest distinctions of organisation between Sigillaria and Lepidodendron, the existence of which I altogether deny.

M. Saporta appears to accept, without demur, statements made by M. Renault respecting Stigmaria ficoides which I emphatically reject. These statements are reactionary in the highest degree. If true they would compel us to cast overboard much of the work done during the last half century by Logan, Binney, Sir William Dawson, and a host of other observers; work, the reality of which, along with the conclusions drawn from it, was unhesitatingly accepted even by Brongniart himself. Such statements, if proven to be true, would involve a rejection of all modern views respecting the origin of coal and a return to the worthless hypotheses that were believed in half a century ago. On this subject I will at present only say that such views are absolutely irreconcilable with well-known facts. Should these views be allowed to pass unrefuted, as Sir William Dawson has properly observed, "some one will be required to rescue from total ruin the results of our labours."[1] I will at present say no more respecting these Stigmarian heresies, since I shall have to deal with them more seriously in a work now in hand for the Palæontographical Society.

Williamson
Coal Plants

Mr. Gardner makes one more statement respecting these Lycopodiaceæ that is unsupported by any evidence which my rich cabinet can supply. He say that "during growth the woody or exogenous zone increased for a certain period, but that this was quickly arrested by the absorption or destruction in some way of the Cambium layer. The *subsequent* increase in diameter took place mainly in the cortical system, and to it the growth and solidity of the stem was principally due. The exogenous element in the oldest known trees is thus seen to have been transitory and subordinate, for had it persisted indefinitely the continued generation of fresh layers or new rings of growth would have produced true Dicotyledonous stems." In the first place we have no evidence whatever of the correctness of Mr. Gardner's statement. That the vascular axis of each of these Lycopodiaceous stems was small in proportion to the diameter of its bark is undoubtedly true, and it was equally probable that the growth in the thickness of that axis was slow; but I know no facts indicating that such growth ever ceased. The diameter of each vascular axis bears about the same proportion to that of the bark, whether the stems are large or small, young or old. Hence we may fairly infer that the cortex and vascular cylinders alike continued to grow *pari passu* so long as each plant continued to live. Anyhow, I know of no facts suggesting a different conclusion.

Respecting the relation of Calamite to Calamodendron, Mr. Gardner says my evidence as to their identity is negative rather than positive. If he will honour me with a visit I think I can soon convince him that this is a mistake, and would only add that there is little possibility and no probability of Mr. Gardner's suggestions being true, viz., that I have "*not come across an undoubted Calamite*," and that such may be common in France though absent from our British deposits. We have them by thousands. What I insist upon is that they differ in no respect from the so-called Calamodendra, the supposed differences being merely due to conditions of preservation. That as soon as we get Calamites with any portion of their internal organisation preserved, they all prove themselves to be Calamodendra. And that even when their internal organisation is not preserved the marking on the surface of their thin carbonaceous covering itself demonstrates that identity. The volumes of MM. Marion and Saporta contain other statements to which, as I have informed my friend, I cannot give my assent; but what I now put on record suffices to show the general nature of the points on which we disagree. M. Zeiller's discovery has settled the questions of the existence of exogenous Cryptogams in the minds of most men—even of several of those who hitherto believed in the accuracy of Brongniart's hypothesis. Patient and persevering investigation will, in time, demonstrate which of us is right in reference to other debated questions. Meanwhile the continuance of co-operation and mutual kindly feeling, notwithstanding our differences of opinion, must be important factors in the attainment of certainty.

Manchester, July 31 W. C. WILLIAMSON

[1] Address to the American Association for the Advancement of Science, p. 22, 1883.

Grisebach's "Vegetation of the Earth"

IN No. 823 of your valued paper is an article by Mr. W. Botting Hemsley on the new edition of Grisebach's "Vegetation der Erde," closing with a reproof to editor and publisher for offering the public an old book as new. For my part I have to say that it was my strong desire to have a really new edition of Grisebach's classical work, which was no longer to be had in the booksellers, by one of our geographical botanists of the first rank. This, however, proved unattainable. Seeing I was bound by contract to the family of Grisebach, and the son of the deceased, Dr. Edward Grisebach, German Consul in Milan, insisted on bringing out the "new" edition himself, all entreaties, representations, and explanations were of no avail. He declared he would never trust the work of his father to other hands and that he felt himself called upon to prepare a new and improved edition. I had therefore but the alternative of seeing the work completely disappear or committing the task of a new edition to the hands of Dr. E. Grisebach, and I think no one will reproach me for choosing the first. At the worst I could only look forward to the new edition being a nearly unchanged copy of the old work (what in point of fact it is), and this seemed to me a far less evil than the complete disappearance of the work, an opinion which friendly and competent judges shared with me. W. ENGELMANN

Leipzig, August 10

A Singular Case of Mimicry

HAVING often read in the pages of NATURE of several cases of protection by simulation (or mimicry), I beg to mention one which has recently come under my own observation, and which, I think, ought to be registered.

I refer to a small insect which I found in a state of larva, and of a white colour, whose back (only) was covered with a layer of moss, and whose movements in this condition were so natural and rapid, that one could immediately perceive that it was the natural *modus vivendi* of the insect. The layer of moss was firmly attached to the body, and completely covered it. I made the experiment several times of placing it on its back, feet uppermost, on a sheet of paper placed on a table. After a few movements the insect, without disturbing the moss, returned to its normal position by making certain movements which resembled those of an acrobat, who, lying on his back, makes use of his hands, and, by a backward somersault, returns to his feet. The little creature is so completely disguised by this layer of moss that, on placing it on the trunk of a tree covered by the same moss, its movements are with difficulty perceived, as the moss in movement may easily be confounded with the moss of the tree. An insect or larva under these conditions could, only with great difficulty, be recognised by its natural enemies (those animals which prey on it).

I send you the specimen to which I refer, the only one I have met with, and which may, during the voyage (of thirty days more or less), die on the way, or pass through some transformation. At all events, you will be able to see the protecting cape, and determine the species, larva or insect, which it protects.

Porto-Alegre, Brazil GRACIANO A. DE AZAMBUJA

[The larva has apparently passed into the pupa stage during the voyage, and has closed the lower side of its protective covering with a silken web. If the perfect insect should emerge, we will endeavour to ascertain its name.—ED.]

Solid Electrolytes

HAVING been for some months occupied with the electrical behaviour of the compounds of copper, silver, and lead with tellurium, selenium, and sulphur, I can confirm the observation communicated to your pages by Mr. Bidwell as to the behaviour of sulphide of copper. He has constructed a primary cell with solid sulphides for the electrolytes. The smallness of the electromotive force which he has obtained is entirely due to the close proximity of copper and silver in the thermochemical series in respect to their heats of combination with sulphur. The theoretical electromotive force should be only ·05 volt.

Let me add to Mr. Bidwell's observation one of my own. If a piece of sulphide of copper is placed between platinum electrodes, a current of electricity from a battery can be passed freely through it, as it is a good conductor. But if after a time the battery is removed and the platinum electrodes are connected with a galvanometer, a current is observed. The solid sulphide between two platinum plates constitutes, therefore, a secondary cell or accumulator capable of being charged and discharged.

SILVANUS P. THOMPSON

Finsbury Technical College, August 17