

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 conclusion 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."<sup>1</sup> 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 Palaeontographical Society.

Mr. Gardner makes one more statement respecting these Lycopodiaceæ that is unsupported by any evidence which my rich cabinet can supply. He says 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

#### 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

<sup>1</sup> Address to the American Association for the Advancement of Science, p. 22, 1883.

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 0.5 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