

worker, they approach one or the other of these favoured specimens. The types are thus in the highest degree arbitrary and accidental, as is also, it must be confessed (though in a less degree), the selection of other specimens to be associated with them.

"It seems to me certain that we are rapidly nearing the time when our ever-increasing collections, revealing as they do the infinite grades of variation presented by living organisms—especially by stock or colony-forming animals, such as corals, in which the varying factors are doubled—will compel us to break loose from the restraint of the Linnean 'species.'"

Finally, the book is well printed, and the thirty colotype plates admirably illustrate the *facies* of the coralla. Mr. Bernard has wisely added three lithographic plates in which are represented carefully drawn details of a typical calicle of most of the species.

OUR BOOK SHELF.

A Compendium of General Botany. By Dr. Max Westermaier. Translated by Dr. Albert Schneider. Pp. x + 299. (New York: John Wiley and Sons. London: Chapman and Hall, Ltd., 1896.)

In this book Dr. Westermaier has attempted to present an account of plants based on the lines indicated some years ago by Schwendener. But so far as English students are concerned, we cannot help thinking that he has rather fallen between two stools. The beginner, on the one hand, will find the book somewhat too advanced for his use; whilst on the other, a student who has already acquired some knowledge of the science, will discover that in the methods of dealing with some parts of his subject, Dr. Westermaier is rather one-sided. Thus, in discussing the factors operative in effecting the ascent of sap, a sketch is given of the views advocated by the author and by Schwendener, almost to the exclusion of those of other investigators; and we certainly cannot accept the conclusions as affording an "authoritative final explanation" of the process.

Notwithstanding, we are ready to admit that the book possesses some good points, and that it is interesting and even suggestive in places. But it scarcely deserves the somewhat ambitious title of "Compendium of General Botany."

The Testimony of Science to the Deluge. By W. B. Galloway, M.A. Pp. viii + 166. (London: Sampson Low, Marston, and Co., Ltd., 1896.)

IT is impossible to treat this book seriously. Such as it were common enough forty or fifty years ago, but we had hoped they had gone the way of the dodo. They are compounded after the following recipe: To the narrowest views in theology, add a general ignorance of the principles of inductive reasoning, collect a number of scraps from scientific books, mainly those written when geology was in its infancy, or if not, carefully separated from their context; stir all together into a hopeless confusion, and serve up with a sauce of pious intention flavoured by some inappropriate quotations from Scripture. Mr. Galloway is one of the stalwarts; he will be content with no local deluges; he will not let us off a square yard of the flood's extent, or a foot of its depth, except perhaps in equatorial regions. This cataclysm produced the rounded and scored rocks, the perched blocks and the boulder-clays with the scratched stones. But he does not explain to us why these products of a universal deluge are restricted to certain parts of the earth, and what were its leavings in districts where our so-called glacial deposits are wanting. A deluge, however, must have a cause. So Mr. Galloway finds this in a sudden shift of the earth's axis of rotation, amounting to about

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18½°; and he unearths some speculations by Dr. Halley, fully two centuries old, in support of his hypothesis. He tells us also much about terrestrial magnetism which does not seem particularly applicable, but we find no explanation of what caused the shift, no proof that the resulting disturbances of water would be powerful enough to transport heavy rock masses in an open country—particularly when it is admitted that the axis may not have "jumped" from one position to the other, but that "several rotations of the earth would probably take place in the progress of the change." Mr. Galloway cannot even cite his authorities accurately. J. Evans (now Sir John) becomes T. Evans, G. F. Browne's Ice-caves becomes Brown's Icy Caves, and so on. But it is waste of time to criticise this book. We present its author at parting with a motto which might have been printed appropriately on his title-page—"Deferar in vicum vendentem thus et odores, Et piper, et quicquid chartis amicitur ineptis." T. G. B.

LETTERS TO THE EDITOR.

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

The Utility of Specific Characters.

ABSENCE from England has prevented my taking part in the discussion on this subject. At this stage I only desire to say that I quite agree with Prof. Ray Lankester, as I stated at the meeting of the Linnean Society, with respect to the contention that the specific characters of the systematist are not necessarily those upon which natural selection has directly acted in bringing about the specific differentiation. These external visible or measurable characters may be, and I believe often are, the outward expression of internal differences of constitution with which the external characters are correlated. In entering the lists at this late period, I am, however, mainly prompted by an omission on the part of Prof. Weldon to strengthen his own case by an argument which appears to me to be quite legitimate. The point at issue is whether the results of his laborious and, in my opinion, most valuable measurements of crabs, are to be interpreted as demonstrating the action of selection, or simply as revealing a law of growth. It might be imagined that if the latter alternative proved to be the correct interpretation, the case for selection falls to the ground. I do not take this view of the work, and, as a member of the Royal Society Committee concerned with the investigation, I am glad of the opportunity, afforded by the discussion in these columns, of giving expression to the idea which I have always entertained on this point, for whatever that idea may be worth. If future observation should show that there is no selection at work upon the young stages, weeding out the individuals whose breadth of carapace falls below a certain standard, but (according to the alternative) that the individuals get broader as they grow older, then it appears to me that the measurements may still be interpreted as indicating the action of selection; only the selection would have done its work in the past history of the species, instead of acting now, as on the original assumption. In other words, breadth of carapace (or some character correlated with it) had a selection value in the phylogeny; now this character appears at a late stage in the ontogeny. It is for Prof. Weldon to decide, by further observation, which of these interpretations is to be accepted. R. MELDOLA.

A Note on the Tesla Spark and X-Ray Photography.

A CROOKES' radiometer was supported by its stem about four inches above the hand, which was placed upon a photographic plate enclosed in two light-tight cases. The terminals of a Tesla coil were placed about half an inch from the bulb on either side of it, inclined to one another at an angle of 120°, the vertex of the angle being in the axis of the radiometer. The Tesla discharge was allowed to bombard the bulb for four minutes. On development a clear picture of the bones in the hand appeared. The experiment shows that the X-ray photo can be produced when an exhausted bulb is used having no terminals.