of growth of the root, and that the loss of hydrotropism is a consequence of the less vigorous growth in the root; this is practically the same criticism as that which Wiesner has applied to our views on geotropism; namely, that the lessened rate of growth caused by the injury to the *punctum vegetationis* interferes with the power of geotropic curvature.

Wiesner has, however, himself observed the fact that the *less* turgescent roots are those which seem to exhibit hydrotropism best, and as want of turgescence would certainly interfere with normal growth, fully as much as the grease on the tips of the roots, Wiesner's criticism seems to be considerably weakened.

In a notice of the present extent, it would be impossible to notice all Wiesner's experiments and arguments, many of which possess much interest. To do so, would require a whole volume such as Wiesner has devoted to the subject, and to which I must refer those who wish to be better acquainted with his views. Finally, I would ask those who do so, not to forget to refer to "The Power of Movement in Plants," for it is only by studying the two books together, that an adequate opinion on the questions at issue can be formed. FRANCIS DARWIN

OUR BOOK SHELF

Catalogue of the Batrachia, Salientia and Ecaudata in the Collection of the British Museum. By George Albert Boulenger. (London: Printed by Order of the Trustees, 1882.)

THIS volume is a proof of the steady though rapid progress which our great Natural History Collection is making, and is a token as well that under the present keeper of the Zoological Department, the stores of specimens will be made fully available for scientific reference. To the working zoologist there can be no more acceptable gift than such carefully compiled catalogues, and by the publication of such the collection itself not only indirectly but even directly benefits, for an interest is awakened in the objects described, and thereby the stream of donations begins to flow. The first edition of this Catalogue, published in 1858, contained the description of some 283 species, and the collection consisted of some 1691 specimens. The present edition contains the description of 800 species, of which the British Museum possesses 522, represented by some 4692 specimens. The first edition, by Dr. Günther, without doubt gave a great impulse to the study of the tailless Batrachians, and as a result it is now out of date. For the present edition Dr. Günther has been fortunate in securing the services of Mr. Boulenger, the assistant naturalist in the Royal Museum of Belgium, and exceedingly well has the latter accomplished his task. The classification adopted is based on that of Mr. E. D. Cope, somewhat modified, and biologists in general will be pleased to know that this classification seems to harmonise not only with the natural affinities of the genera, but with all that is known of the geographical distribution, development, and physiology of the group. The only serious objection urged against it is the supposed difficulty of ascertaining on the recent specimens the osteological characters, without sacrificing one or more specimens; but, as Mr. Boulenger asserts, it requires only a moderate skill and two or three clean incisions to reveal all the required secrets of the structure of the vertebræ.

One important feature in this catalogue is that we find in it an account of all the known species of the group, so that it to a great extent possesses the merit of being a monograph, and it thus indicates the species which are wanting in the National collection. There is also a very considerable beginning made in the descriptions of the larval forms, and sometimes these are figured, the collecting of these forms we trust may receive a fresh stimulus from the publication of this work.

Mr. Boulenger well merits the confidence with which Dr. Günther writes that "zoologists will thankfully acknowledge the industry and ability with which the author has performed his difficult task."

It only remains to add that this catalogue is illustrated with numerous woodcut illustrations, and with thirty lithographic plates.

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 ensure the appearance even of communications containing interesting and novel facts.*]

On the Conservation of Solar Energy

WITH your permission I should like to offer a few remarks upon the interesting paper of Dr. C. William Siemens on the "Conservation of Solar Energy," published in NATURE (vol. xxv. p. 440). The main hypothesis upon which that paper is based, that of a fan-like action of the sun, is not improbable; nor are the consequences drawn illogical, if we could reasonably imagine space to be occupied by such condensed molecules as he supposes. That space is everywhere occupied by matter, there is no just reason to doubt. The hypothesis of an ether, specifically distinct from matter, is a gratuitous assumption, and one of the last surviving relics of eighteenth century science. Unless it can be proved that highly disintegrated matter is positively incapable of conveying light vibrations, there is no warrant for assigning this duty to a distinct form of substance. But that matter exists in outer space in the same conditions as in the planetary atmospheres is certainly improbable. Its duty as a conveyer of radiant vibrations seems to require a far greater tensity, and its disintegration is probably extreme.

If we assume, then, that matter exists throughout the universe, here as condensed spheres, there as highly rarefied substance, with the atmospheric envelopes of the spheres gradually shading off into the excessively rare matter of mid space; another hypothesis may be deduced, somewhat different to that offered by Dr. Siemens. The views which I desire to present have been already published, but they seem worthy of repetition in connection with his solar theory.

On the Nebular hypothesis, the matter of the sun was once disseminated through space. Gravitative attraction has, therefore, had a double effect. The greater portion of this matter is now drawn together into a contracted mass. The remaining portion yet occupies outer space, in a far more rarefied condition than the original. But an important consequence attends the condensation and rarefaction of gases. This is, that condensed gases become heated, rarefied gases cooled, and this without the aid of heat exchange with outer material. In the one case a portion of the absolute heat of the gas, formerly latent, becomes sensible; in the other a portion of the sensible heat becomes latent. If originally the absolute heat contents and the temperatures were alike equal, condensation and rarefaction would not, of themselves, change the heat contents, but they would change the temperatures. In condensation, the latent heat is reduced, the sensible heat increased, and the temperature rises. In rarefaction the opposite effect is produced, and the temperature falls.

This consideration applies as well to the problem of the condensation of nebulous as of terrestrial gases. The effect of contraction of nebulous gas into a dense sphere, must be a considerable rise in temperature if there be no diminution of absolute heat contents. The effect of rarefaction of the remaining matter of space must be a decrease in temperature. Thus if radiant outflow of heat from the sun had been prevented during its condensation, the eventual result must have been that the sun and the matter of outer space would have continued equal, mass for mass, in absolute heat contents, and yet have become immensely different in temperature.

And from this must have come another interesting result,