

twenty-five years for man's cessation of growth is therefore only an average one, in introducing that number as a factor of the curve, I thought that it would be manifestly an error to take examples of exceptionally long lives, when striking an average for length of life. In the curve as published in *NATURE* (which, although published somewhat before my communication to the *Lancet*, is really a revised curve), I reduced the age of man from eighty to seventy-five years from considerations such as I have just mentioned. Even seventy-five years is somewhat greater than the expectation of life given in Bourne's "Manual," for those who have completed the first half-century of their existence. The age given by Buffon, and quoted by Mr. Bell, is undoubtedly far too long. Similar considerations from some fresh data induced me to reduce the mean lifetime of the domestic mouse from four and a half years to four years, as noticed by Mr. Bell.

W. AINSLIE HOLLIS.

Hove.

Barnes' "Plant Life."

In re centrosomes:

Poor misguided Prof. Zacharias! With absolutely no provocation, he now departs from "the almost universal consensus of opinion among good botanists" by saying of centrosomes (*Bot. Zeit.*, 572: 6, 1899):

"However, on an unprejudiced consideration of the literature involved, one may consider it not impossible that, on renewed search, the centrosomes will finally be again discovered where, for the present ('mark, Jew!'), they have been missed."

And Guignard! What a stupid he is to repeat in greater detail the blunder of figuring and describing those "discredited" centrosomes when all good botanists (who swear by Strasburger and his young American students) know that there are no such things! And to think of his calling them "Les centres cinétiques chez les végétaux" (see *Annales des Sci. Naturelles, Bot.*, viii. 5, 177-220, 1898), as though they were common! How "amazingly behind the times"!

But there must be more reason than assigned for designating "Plant Life" as "amazingly behind the times." Prof. Barnes would really be under obligations to the reviewer if he could find time to indicate by number of page or paragraph (doubtless marked as the book was yawned over) the statements to which he considers this phrase applicable. This request is made in all sincerity, and in the hope that the number of these passages will not be so great as to make it presumptuous in its demands upon the reviewer's time.

C. R. BARNES.

THE reviewer cannot help regretting the evident pain which his remarks (vol. lviii. p. 519) have caused Prof. Barnes, though the latter can scarcely seriously believe that his arguments and assertions meet the original objections to which he has taken exception.

Prof. Barnes appears to be particularly aggrieved at the reference made to the figures and account of centrosomes, but his own explanations merely serve to give force to the reviewer's contention that they ought not to have found a place in an elementary book at all.

If the best final reply he can make is to quote the opinion of Zacharias to the effect that "it is not impossible that on renewed search the centrosomes will finally again be discovered," he should see that his case is parlous indeed. He has, in fact, cast a far greater slur on his own critical judgment than the reviewer would have ventured to do. His further quotation of Guignard's recent work might perhaps be regarded as somewhat *ex parte*, even had that investigator reiterated the old statements on which Prof. Barnes' account was based. As a matter of fact he does not do so, and his silence tells against our author.

The somewhat contemptuous reference to Strasburger (who is nevertheless *facile princeps* amongst botanical cytologists) and to those younger American botanists whose reputations, *pace* Prof. Barnes, are largely founded on the splendid results achieved by them at Bonn, are scarcely calculated to increase one's regard for Prof. Barnes' power of discrimination.

Prof. Barnes appears to be quite unable to realise the degree of mental confusion which would be the inevitable lot of a student endeavouring to deal with the account given by him of the movement of water in plants. In one place (§ 204) root-pressure is spoken of as the force which causes the movement from the root to the evaporating surfaces of the leaves; but in § 207 the author rightly remarks that root-pressure is practically

inoperative at the time when transpiration is most active. But he goes on to add that "recent experiments" indicate that the negative pressure of the gas-bubbles in the tracheids may be "a very important, or even the chief factor in lifting the water." After this one ceases to be surprised that no mention is made of the conclusions reached by Dixon and Joly, or by Askenasy!

But Prof. Barnes asks for further evidence for the reviewer's unfavourable opinion of the book. Only a few instances need be mentioned here, for if "this request is made in all sincerity," the author's own friends will easily supply more.

In a work of this kind, it is astonishing to find no mention of the occurrence of motile antherozoids amongst the lower phanerogams, which is perhaps the most important of all recent botanical discoveries—important for the student as clearly showing the connection between the higher and lower plants.

The account given in § 143 of annual rings is so preposterous as to call for no further comment.

The respiratory quotient of the ordinary plant is still given as unity, when, as a matter of fact, it is nearly always other than 1 in growing plants.

The statement that "true geotropic curvatures are brought about by the acceleration of the growth of the irritable cells" is, as it stands, absurd, for it involves no necessary curvature at all.

A student reading the account given in § 230 would naturally fall into the error of concluding that all the rays of light absorbed by chlorophyll are equally active in promoting assimilation.

In view of the evidence here adduced, at Prof. Barnes' own request, the reviewer considers that his judgment of the book was by no means unduly harsh or severe.

THE REVIEWER.

Optical Experiment.

BEING driven past a row of trees, I noticed that their intermittent shadow on the closed eye-lids gave rise to a vivid chess-board pattern of red and black squares arranged horizontally and vertically. These were perfectly regular, each being equal to about one-twelfth of an inch at ten inches distance. Waving the open fingers in front of the closed eye-lids exposed to the sun gave the pattern fairly well, but better by flashing the sun's rays across the lids by means of a vibrating hand-mirror. I see about seven or eight squares each way, the outer ones not well defined; but a younger man, who was not told what to expect, described them as more numerous.

What structure in the eye gives rise to the phenomenon? It is not caused by the eye-lids, because a piece of tissue-paper can be substituted, the eyes then being open. If the paper is white the squares are white and black. The pattern occupies the centre of the field of each eye.

THOM. D. SMEATON.

Adelaide, South Australia, February 6.

A SEISMOLOGICAL OBSERVATORY AND ITS OBJECTS.

TEN years ago seismologists practically confined their attention to the movements of the ground which could be felt. In Italy and Japan, where these were frequent and sometimes violent, they attracted serious attention; whilst in Britain, where earth tremors were comparatively unknown, any suggestion that this country should establish a seismological observatory might only have cast doubts upon the mental balance of its author. At that time it was popularly supposed that in our islands earthquakes were of such rare occurrence that a special establishment for seismological investigations was unnecessary. Seismology, however, like several other sciences, has in a comparatively short period advanced with strides, and now stands as foster-mother not only to a Romulus and Remus, but also to a number of other children all filled with promise.

Now we know that in England, or in any other non-seismic region on the surface of the globe, at least seventy unfelt earthquakes, each of which have durations varying between twenty minutes and several hours, may be recorded yearly. The probability is that these movements are transmitted from their origins as compressional