

LETTERS TO THE EDITOR.

The Age of the Earth.

I AM surprised to observe, in the article which Prof. Sollas has written on this subject in your issue of the 4th inst., p. 533, that he speaks with approval of Dr. A. R. Wallace's method of calculating the earth's age. About two years ago (I have only this week's number of NATURE at hand) I wrote to you on this subject, and was under the impression that I had proved the complete fallacy of Dr. Wallace's method of calculation.

To put Dr. Wallace's view briefly, he assumes that deposition within a limited area of, if I remember rightly, 3,000,000 square miles, goes on 19 times as fast as denudation over the whole land area, which is 19 times as great, and then argues that the whole maximum thickness of the stratified rocks (and hence the earth's age) could be deposited in 1/19 of the time required to carry away from an equal area of land an equal bulk of material.

The fallacy consists in assuming that a great rapidity of deposit over a limited area can in some way allow of the deposit or formation of sedimentary rocks at a greater rate than that of denudation.

It is obvious that, in a given time, no greater volume of deposits can be formed than the volume of material denuded in the same time. If, therefore, as Prof. Sollas assumes, 1/2400 of a foot of sediment per annum is denuded from the land area, by no arrangement can a land area of equal extent, consisting of sedimentary rocks of the same composition and thickness as those which actually constitute the land area, have been formed as a whole more rapidly than 1 foot thickness over 57,000,000 square miles area in 2400 years. Taking the estimate of Prof. Sollas, viz. 164,000 feet, as the maximum thickness of the sedimentary rocks, and taking the existing land area to be accounted for as 57,000,000 square miles, the time required to form an area of 57,000,000 square miles of rock 164,000 feet thick, at 1/2400 of a foot per annum, is 393,600,000 years, unless the area undergoing denudation was greater or less than it is at present (and it could not be four times as great as at present). No concentration of the deposit over a small area would shorten the time required by a single moment. BERNARD HOBSON.

IF, in the compass of a short article, I did not allude to the controversy which followed the attack made by Dr. Hobson (NATURE, vol. xlvii. p. 175, 226) on Dr. Wallace's method of estimating the age of the stratified series, it was because I thought, as I do still, that the honours of that controversy rested entirely on the side of Dr. Wallace.

There is no fallacy in Dr. Wallace's argument, but a strange misconception on the part of Dr. Hobson, which arises from his consistent disregard of the word *maximum* as prefixed to the estimated total thickness of stratified rocks. It is obvious that stratified systems cannot have a *maximum* thickness everywhere over the whole 57 million square miles of the land surface. As a matter of observation, a system attains its maximum thickness over a very limited area, and over a large part of the 57 millions of square miles of land surface it has no thickness at all, or, in other words, is entirely absent. If "maximum" could be made to mean the same as "average," no doubt Dr. Hobson's contention would hold, but those who have made use of a maximum in estimating the age of the stratified series have observed a strict distinction in the application of the two terms.

Rathgar, April 9.

W. J. SOLLAS.

Polyembryony.

IN connection with the note in the last number of NATURE on the above, I think it should be known that the phenomenon was incidentally observed some two years ago in the red beet (*Beta rubra*) by the late Mr. Romanes and myself. We found that a single seed might produce as many as four distinct plants, and as far as our observations went, polyembryony was quite the normal condition. It seems to be more characteristic of the Gymnosperms than the Angiosperms, and has of course been investigated in the former, and in the latter among the Monocotyledons (Tretjakow) and Dicotyledons (e.g. *Citrus*-Strasburger). The fact of its occurring in such a common type as *B. rubra* should, I think, be taken advantage of by some botanist, as the results could not fail to be both interesting and important. Tretjakow's discovery that the supernumerary embryos in Monocotyledons may be produced by the antipodal cells, certainly suggests his comparison between such embryos and those produced by [parthenogenetic?] apogamy on the prothallia of the lower plants.

FRANK J. COLE.

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IMPROVEMENTS IN PHOTOMETRY.

NEARLY sixty years have passed since it first occurred to the philosophic mind of Sir John Herschel to attempt an arrangement of the relative brilliancy of the stars, upon a method that should be more secure than the eye estimations that had done duty for many centuries. It is not necessary to enter into any description of his method, which may be regarded now as entirely superseded. Doubtless, had he been surrounded by skilled workmen, furnished with better tools, the cumbrous method employed would have been simplified, but the establishment of an observatory remote from the assistance and contrivances of the workshop is not without drawbacks, as he and others since have discovered and regretted. About the same time, Seidel, in Germany, was at work on the same problem, and the fact that two astronomers, independently of each other, undertook the solution of the same problem, is a proof that it was ripe for mature consideration, while the series of astronomers who have laboured in the same path confirms the suspicion that this kind of investigation too long neglected offered a field having a rich prospect of reward.

But a photometer at once convenient and capable of general application to the stars remained to be invented, and this want was effectually supplied by Zöllner, who proposed a form of construction which has certainly obtained the most general use of any of the suggestions that have been from time to time put forward by astronomers, who have recognised its deficiencies and tried to remedy them. The distinguishing characteristics of the Zöllner photometer are the introduction of an artificial star formed from a lamp shining through a small aperture, and the controlling of the light of that star by means of polarisation. This principle is now of such general use that no lengthened description is necessary. But to explain the reason for the introduction of other forms of photometer, it is necessary to point out what are, or what were, considered to be its defects by those who first used the instrument, defects which it is believed care and experience have since done much to diminish, if not entirely to remove. A source of error might be anticipated in the varying brilliancy of the lamp employed to form the artificial star, and in the early days of the instrument this was a fruitful source of annoyance. Next, the light of the lamp had to strike no less than twenty-eight surfaces, and apart from the difficulty of getting so many surfaces true, and ensuring the parallelism of the Nicol prisms by which the diminution of the artificial star is effected, there is also to be considered the inevitable loss of light at so many surfaces. One consequence of this is that the brightest stars of the heavens are apt to be brighter than the artificial star, and since the observation is made by reducing this light to match that of the real star, it is necessary to have recourse to some such expedient as reducing the aperture of the telescope. And then a difficulty is encountered which has not yet met with a complete explanation. The light deducted from the star, as seen with a reduced aperture, does not coincide with that which would be predicted from theory. In some of the recent series of observations the differences between observation and theory are as great as they are perplexing. "There can be no doubt," wrote Mr. C. S. Peirce, of Harvard, twenty years ago, "that the errors introduced by the use of these diaphragms are by far the most serious of those by which my observations are effected." Dr. Wolff met with similar difficulties, and doubtless anomalies such as these have encouraged the production of other photometers which should be free from the suspicion of error. Having regard to the photometric work actually accomplished, we may confine attention to two forms of apparatus known as the Pickering Meridian Photometer and the Pritchard Wedge