

On washing and riddling this clayey matter, well waterworn and smooth gravel was separated from it up to about the size of a bean, and on many of these minute pebbles, with the aid of a microscope of low power, beautifully-developed striæ are to be seen, sometimes on more than one face. This latter fact appears to have been unnoticed before. The conclusion I have come to is that Ramsay had reasonable grounds for his belief in this being a Permian glacial deposit, and think that if he had given more details in his otherwise able paper, geologists would probably have followed him more freely.

T. MELLARD READE.

Park Corner, Blundellsands, December 5.

The Earth's Age.

As Dr. A. R. Wallace's "Island Life" may be regarded as one of the best authorities on its subject, it appears desirable that any errors in it should be pointed out, lest any of its numerous readers should be misled.

In Chapter X. (2nd edition, 1892) is an estimate of the earth's age based on the following data:—Land area of globe, 57,000,000 square miles, coast line, 100,000 miles, width of shore deposits, 30 miles, hence area of shore-deposits, 3,000,000 square miles, hence rate of deposition 19 times as fast as that of mean rate of denudation, which latter is taken to be 1 foot in 3000 years.

Thickness of stratified rocks 177,200 feet, hence time required for deposit 28,000,000 years. This last result is taken to be approximately the earth's age.

It appears to me that Dr. Wallace's data warrant no such conclusion, for, in the 28,000,000 years in question, all that would have been deposited would be a thickness of 177,200 feet of rock, over an area of only 3,000,000 square miles, whereas, what has to be accounted for is an area of 57,000,000 square miles (neglecting igneous rocks and sedimentary deposits beneath existing seas) of the same thickness. Therefore, so far from Dr. Wallace's data leading to 28,000,000 years as the earth's age, they actually lead to a result 19 times as great, viz. 532,000,000 years.

Sir A. Geikie's estimate is (*NATURE*, vol. xlvi., p. 322), 100 to 680 million years. Personally, I think, the method of taking maximum thicknesses of deposits unsatisfactory, for it assumes that every formation was deposited, with its maximum thickness, over the whole land area of the globe. The absurdity of this supposition is obvious. The only defence of it is that it is held to make an ample allowance (of unknown amount) for repeated denudation. It would, perhaps, be better to ascertain the actual thickness of a great series of successive formations, say in the Colorado Cañon and other regions, and from such data to estimate the total *average* thickness. This estimate, of course, would allow nothing for repeated denudation, but would enable one to form an idea of the earth's *minimum* age.

BERNARD HOBSON.

Owens College, Manchester, December 5.

I AM glad that Mr. Hobson has formulated his difficulty as to the measurement of geological time by the comparative rates of denudation and deposition, because it shows that I cannot have explained my views as clearly as I thought I had done; yet on again reading over pp. 217-223 of "Island Life," I can hardly understand how he has missed the essential point of the argument. Fortunately, there is no dispute as to the data, only as to the conclusions to be logically drawn from them.

Mr. Hobson says that I account for a deposit of 177,200 feet (the supposed thickness of all the stratified rocks) over an area of 3,000,000 square miles (the estimated area over which at any one epoch stratified rocks are being deposited) in 28,000,000 years (the deduced estimate of known geological time); and then adds: "Whereas, what has to be accounted for is an area of 57,000,000 square miles of the same thickness" (my italics). This seems to me a most amazing misconception; for it means that every single formation and every stratum or member of each formation, was deposited to the same average thickness over the whole land surface of the globe (area 57,000,000 square miles)! And this implies that at every successive period, from the Laurentian to the Pliocene, the conditions of denudation and deposition were totally different from what they are now, since at the present time it is demonstrable that the area of deposition of continental debris is only a fraction of the whole continental area. It implies further, that

during each geological period the whole of the existing land area must have been, either at once or in rapid succession, sunk beneath the sea in order to allow of its being all covered with each successive formation—an amount of repeated upheaval and depression which hardly the most extreme convulsionist of the old school would have postulated. I cannot make the matter clearer, and trust that on further consideration Mr. Hobson will admit that his objection is invalid. ALFRED R. WALLACE.

The Colours of the Alkali Metals.

IN *NATURE* (vol. xlvii., p. 55) is a communication by Mr. G. S. Newth, entitled "Note on the Colours of the Alkali Metals."

I write to call attention to my article on "The Colour and Absorption-Spectra of Thin Metallic Films, and of Incandescent Vapours of the Metals; with some observations on Electrical Vitality," published in the *American Chemical Journal* (vol. xiv. p. 185) and reprinted in the *Chemical News* (vol. lxvi. p. 163), which gives the method employed by Mr. Newth, as well as other methods for obtaining metals in thin films.

It in attention is also called to the fact that the colour of the film of the metal and the colour of the vapour are widely different.

Mr. Newth, however, succeeded in getting a film from sodium on glass, while I did not, and his success was probably due to the use of a higher vacuum than I employed. He also obtained a rubidium film.

In my paper I called attention to the similarity in colour of the film by transmitted light and that of the *incandescent* vapour which is very striking in many cases. In this respect the film of rubidium as obtained by Mr. Newth follows the rule fairly well; but the film which he got from sodium is exceptional, as according to the analogy furnished by other metals it should be yellow. The presence of potassium, however, might cause the green colour which he observed, by the combination of yellow and purple.

WM. L. DUDLEY.

Vanderbilt University School of Chemistry,
Nashville, Tenn., December 2.

Osmotic Pressure.

IN an article on osmotic pressure, in *NATURE* (*ante*, p. 103), Mr. Rodger very truly remarks that "at present the attitude of the prominent upholders of the new theory [of solutions] is one of indifference as to the exact mechanism of osmotic pressure. The numerical agreement between the measurements on solutions and those on gases is regarded as ample justification for considering dissolved substances to be in a pseudo-gaseous condition." Such an indifference is surely to be regretted from any truly scientific point of view, especially as those explanations which have been given of the mechanism of osmotic pressure have been based on the supposition that the dissolved substance is in a *veritable* and not merely *pseudo-gaseous* condition. There are, however, many reasons for supposing that while the dissolved substance may for many purposes be regarded as analogous to a gas, it must in reality be in a very different condition, and that osmotic pressure is not due to the bombardment of the free molecules of the dissolved substance against a diaphragm through which they cannot pass. The impenetrability of the diaphragm to certain molecules can scarcely be attributed to any other cause than that the molecules are too large to pass through the interstices of the former, and it is scarcely conceivable that the molecules of water which do pass through can be much smaller than the molecules of simple salts, which do not pass through; still less that they can be smaller than the single atoms into which these salts are said to be dissociated.

A very simple experiment, which I mentioned some time ago in the *Ber. d. deutsch. Chem. Gesell.* (24, 3639), appears to settle definitely against the view that osmotic pressure is due to impenetrability to the dissolved substance. A solution of propyl alcohol and water was put into a porous pot and immersed in a vessel of water; the water passed through the pot to the solution, and this, according to the usual explanation, would show that the pot was impermeable to the propyl alcohol. The same solution was then immersed in a vessel containing propyl alcohol, when the alcohol was found to pass through to the solution, from which we should have to draw the diametrically opposite conclusion that the pot is impermeable to the water. The true conclusion obviously is that the pot is impermeable, neither to the alcohol alone, nor to the water alone, but to the solution of these in each other, and that the molecules composing this solu-