and its subsequent issue by "the Hydrographer to the King by order of the Lords of the Treasury" there can be no doubt whatever.

The first Government Geological Survey undertaken in the British Islands was that of Dr. John Macculloch, and the work that he accomplished single-handed was a very remarkable one. Several geological maps of Scotland, differing very widely from that of Dr. John Macculloch, have been issued and withdrawn during the last fifty years; but any one who will compare the first geological map of Scotland with the latest, also "published with Government authority," will be interested to see how far the work of the early pioneer in Scottish geology has been found to be correct in most of its essential features by those who have come after him.

John Macculloch's title to be the author of the first geological map of Scotland is as indisputable as are the similar claims of William Smith and Richard Griffiths with respect to England and Ireland respectively.

JOHN W. JUDD.

16, Cumberland Road, Kew, December 6.

Glaciers of Val d'Herens.

THE two glaciers of Arolla are interesting, inasmuch as one, the Arolla, is retreating, while the other, Glacier de Zigiorenove, is advancing. This has been going on for twelve years, according to the report which appeared in *NATURE* (vol. xlv. p. 386), by Dr. Forel, dealing with Alpine glaciers and their changes.

Having visited these glaciers last summer with the object of observing the effect of their respective movements upon the morainic accumulations in front, I think a brief account may be worth recording.

The Arolla glacier occupies the head of the valley, and is fed by the snow from Mont Colon and the fields of névé extending towards Mont Brülé. The Zigiorenove glacier is one mile further down the valley, but does not descend so low as the Arolla glacier by about 300 feet. It receives its main supply from the Pigne d'Arolla, a mountain which rises immediately above the glacier, and is conspicuous by its massive snow-cap. This glacier is not only nearer to its supply, but descends at a steeper gradient than the Arolla glacier.

This may in some measure account for the former advancing while the latter is retreating, or, more correctly speaking, melting backwards. I was informed by local guides that the Arolla glacier has been swelling behind for some years; if this be correct, and the seasons remain normal, then, in a short time, this glacier must advance also; in other words, being longer or further away from its feeding ground, the extra supply has not yet had time to reach its extremity. Appearances at the end of the glaciers are in themselves quite sufficient to indicate their respective movements; the snout of the Arolla glacier is buried in its own debris, composed of rocks and loam borne upon or concentrated to the surface as the ice melts. This debris is being constantly shot down grooves or water-courses furrowed in the sloping end or side of the glacier. At the bottom of these spout-like grooves conical-shaped mounds are formed, one behind another, as the ice melts backwards, resulting in a moundy, zigzag arrangement constantly seen lower down the valley, many miles away from existing glaciers, and can be seen also in many of the higher valleys of North Wales.

The advancing Zigiorenove glacier, instead of being buried in debris, turns up in shell-like flanges, exposing its under surface in its endeavour to climb over, rather than push forward, the loose rocks in front; a part of this loose material only is pushed forward, forming a small bank, which in no place exceeded 5 feet in height, up which the ice mounts with the shell-like flanges projecting beyond; the under part of these projections being fluted into a perfectly symmetrical pattern.

In some way the projecting rocks forming the glacier bed produced these convolutions, but whether by cutting, melting, or by the ice crystals flowing round each side, I was unable to determine, but I may state there was no appearance of cutting or grinding which would necessarily leave behind the shavings or ice particles cut out or ground off, and little or no water was present as if from melting. We had much snow for three days, with a total absence of sunshine; the atmosphere at mid-day, when making these observations, registered 38° F. Appearances rather suggested the idea that the ice crystals were displaced in a similar manner to water in a rapid river when it meets with obstructing boulders in its bed.

Climbing up one of the old lateral moraines, for there are several, the glacier is seen to be encroaching laterally as well as longitudinally; it soon extends to the base of the inner moraine, and then climbs upwards in a similar manner as it advances in front by turning up its edges, carrying upwards a few loose rocks in front, but without materially disturbing the moraine itself. Higher up it reaches the top of this moraine, and then rises above it like a wall, having caused the loose rocks it had borne up to roll down into the valley between the inner and outer moraine, with masses of ice broken off the edge of the glacier.

As far as I am able to ascertain, by creeping under the flanges the glacier slowly spreads out the bank which it had pushed up in front as it advances into a more even bed to travel over, but my observations were restricted to a few yards owing to the contraction of the sub-glacial space between the ice roof and the floor. I was much impressed by the fact that the glacier did little in exerting that force which might be expected from such a power behind, in the way of ploughing through or removing such comparatively trifling obstructions as loose rocks or moraine banks.

It is evident from the various ages of moraines that the lower parts of those glaciers have fluctuated considerably during recent times, but the line of demarcation between these minor fluctuations and the period after these lateral valley glaciers had been confluent with the main Valais glacier, and were retreating backwards, is very marked. Beyond two miles down the valley from the Zigiorenove glacier no striæ from the trunk glacier could be found, but at this distance it suddenly appears with fresh glaciated forms; precisely the same phenomenon was noticed in the neighbouring Val d'Anniviers, as if this were the limit of the more recent fluctuations.

At the Col de Bertol, 4500 feet above Arolla, or about 11,000 feet above sea level, there are no indications of glaciation, ancient or modern, above the surface of the snow; so it would appear, however much the ice level has varied below, at this dividing ridge it has always remained the same, or, in other words, it has been dispersed by wind or ice movements to lower levels at a rate equal to deposition, when the snow line was low enough to allow the ice to travel down the Val d'Herens, the higher ice streams would simply run down upon its surface, instead of descending as low level glaciers or mountain rivers, as at present.

WILLIAM SHERWOOD.

Eastbourne House, Sutton Coldfield.

Ancient Ice Ages.

MESSRS. BLANFORD, in their letter (*NATURE*, p. 101) called forth by Dr. Wallace's notice of a palæozoic glacial conglomerate in Victoria, Australia, say:—"It has become an accepted article of faith amongst most European geologists that no ice-age occurred before the last glacial epoch." There is no doubt that the tendency of opinion has been in that direction, notwithstanding the evidences to the contrary brought forward by Dr. Blanford and others. The late Sir Andrew Ramsay was the first, so far back as 1855, to suggest that the Permian conglomerate of Abberley and the Clent hills was an ancient glacial deposit. Although this reading was accepted by so cautious a philosopher and critic as Sir Chas. Lyell ("Principles," 10th ed., vol. i. p. 223), the idea has languished from disfavour. Having devoted a considerable portion of the last twenty years to the study of glacial phenomena, I early this year paid my first visit to the district. The best section I saw is an excavation at Abberley in what may be called boulder-gravels, and, except for its prevailing red colour, the deposit, if dropped down on the coast of Wales, would, in general appearance and arrangement of the materials, be undistinguishable from many of the glacial deposits to be found there. Many of the stones have flattened faces, and in general shape resemble glaciated boulders, but the striæ, so far as I could observe in the limited time at my disposal, were not very pronounced. Since then I have found a deposit of glaciated gravels on the top of the Screes, Cumberland, 1600 feet above the sea, which on comparison with the specimens brought home from Abberley enables me to understand the latter better. The volcanic rocks of the lake district of which these gravels are composed break up into very similar shapes, and are planed and striated in a very similar way to those of Abberley. I also brought home from Abberley two specimens of the "paste" which fills up the interstices of the finer gravel

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 striae 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. xlvii, 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 ga-es 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-