The Planting of Timber Trees.

IN Traill's sketch of the life of Shaftesbury (the first Earl), the following passage occurs in a letter from the Earl to the steward of his estates in Dorsetshire :—

"The best planting of timber trees is with nuts, acorns, seeds, and footsets, and not with young trees removed . . . In setting of chesnuts, acorns, and seeds [it is desirable] to steep them twenty-four hours in milk, which gives them a great advantage. . . . If siccamores [are planted] near my gardens, they will spoil all my fruit with the flies they breed. Therefore pray pluck up all the siccamores that are in the dry meadow behind my kitchen-garden, and in the room of every one of them plant a chesnut, a walnut, or a honey-broke oak."

Can any of your readers inform me whether the soaking of seeds in milk is now, or ever has been, extensively practised, also what is meant by a "honey-broke oak"?

ALFRED W. BENNETT.

Rhynchodemus Terrestris in England.

THE credit of the first discovery of this land-planarian in England lies not with Sir John Lubbock, as Dr. Scharff stated, but with the late Rev. L. Jenyns (Blomefield), who, in his "Observations in Natural History," 1846 (p. 315), makes some interesting remarks on the "Ground Fluke" (*Fasciola terrestris*) and its occurrence in the woods at Bottisham Hall, a locality searched with success by Mr. Harmer.

Rhynchodemus terrestris is widely distributed in England, and I have found it in Derbyshire, North Lancashire, and Westmoreland, under moist conditions and on a limestone substratum.

Any additions to the limited number of land-planarians in Europe are of considerable interest, and mention may therefore be made of Prof. v. Graff's description (*Bull. Soc. Zool. France*, xviii. 1893, pp. 122–3), of *Rhynchodemus pyrenaicus*, n. sp., from St. Jean de Luz, which is not alluded to by Dr. Scharff.

F. W. GAMBLE.

Owens College, Manchester, October 26.

Tan-Spots over Dogs' Eyes.

THE shepherds in some of the east counties of Scotland used to call their black-and-tan collies four-eyed dogs, which agrees so far with Mr. Peal's observations. These collies, twenty years ago, were much in demand. Now they are hardly allowed prizes at shows, and are becoming scarce; black and white, pure white, and, more commonly, brown dogs being greater favourites.

J. SHAW.

A CRITICISM OF THE ASTRONOMICAL THEORY OF THE ICE AGE.

IN a communication to the British Association at Oxford, I gave an outline of a method of obtaining a limit to the direct effect on terrestrial temperature of the diminished winter sun-heat during epochs of great eccentricity, the conclusion being that that effect had been enormously exaggerated, and that the astronomical theory of the Ice Age was really but a vague hypothesis, having no sound physical foundation.

It will be remembered that Dr. Croll's theory is shortly this: In the long northern winters in the time of great eccentricity, far less sun-heat is received than at present; the *direct* effect of this decrease in sun-heat is a *proportionate* decrease in terrestrial temperatures, or, more properly, a proportionate decrease in the excess of terrestrial temperature over the temperature to which the earth would fall in the absence of all sun-heat. So far Croll and Sir Robert Ball, the later expounder of the theory, agree. But now they part company. Croll affirms that the lowering of temperature thus calculated would be quite insufficient, and that it is the *indirect* effect of this fall of temperature (chiefly the effect in disturbing oceanic circulation) which gives rise to the additional lowering of temperature necessary for the production of an Ice Age. Ball, on the other hand, affirms that the direct lowering of temperature due to diminished sun-heat is amply sufficient to cause an Ice Age. I use the word *affirms* advisedly, because neither writer assigns any reason. Apparently Croll's reason was that he thought he could see additional causes, which if they existed must have contributed to the effect, and also that previous writers had said that the direct effect of the change in sun heat would not be sufficient; while Ball seems to have considered that he had strengthened Croll's argument so much that the new form of the theory was as strong without the ocean currents, as Croll's was with ocean currents. It does not seem to have occurred to either writer to ask what change in temperature would be necessary in order to produce an Ice Age, so that they might see if the cause they assigned would be sufficient; yet one would have thought this was the first step towards formulating a theory.

The point in reference to which the two authors employ numerical calculation is in obtaining the fall of terrestrial temperature due to a reduction of sun-heat. The problem is, of course, very complicated, and one would expect that the most approved principles of physics would be employed. Not at all. The physics is founded on an incidental remark of the astronomer Herschel in his "Outlines of Astronomy" (edition of 1869), where he assumes that the radiation of a body in space is *proportional* to its absolute temperature. Yet it has for many years been known to physicists that the radiation increases faster than the temperature, and in 1880 or 1881 what is now known as Stefan's law was published, namely, that the radiation increases as the fourth power of the absolute temperature. This would make an enormous reduction in the calculated fall of temperature due to a diminished supply of heat—*it would reduce it to one-fourth* of the amount obtained on the erroneous assumption employed by Croll and Ball alike. For if temperature be solely due to sun-heat, the heat radiated, say $A\theta^4$, where θ is the absolute temperature, must be equal to that received, say S, or

$$A\theta^4 = S,$$

hence

$$\frac{d\theta}{dS} = \frac{1}{4}\frac{\theta}{S},$$

whereas the law of direct proportionality assumed by Herschel, and adopted by Croll and Ball, gives

$$\frac{d\theta}{dS} = \frac{\theta}{S},$$

a result four times as great as that obtained above—

Turning now to Croll's form of the argument, we find one very remarkable inconsistency, which I think is no bad illustration of the special pleading which characterises that ingenious writer. When, in the first place, he desires to show how great may be the midwinter fall in temperature due to diminished sun heat, he thus employs the argument I have criticised above :—

Let T_p be the present excess of midwinter temperature at the latitude of the British Isles above the temperature of space, *i.e.* above the temperature to which the earth would fall if all sun-heat were to cease, and S_p the quantity of sun-heat at present received on that latitude on Midwinter Day, and let T_g and S_g be the corresponding quantities for the supposed glacial winter. Then, on Herschel's hypothesis, T_g is to T_p as S_g is to S_p. Having in that way got an enormous fall of temperature, Dr. Croll goes on to say that a vast proportion of our midwinter temperature in these isles is due, not to sun-heat received by us, but to heat carried to us by ocean currents. These ocean currents, he argues, will be diverted in the supposed glacial period, and thus there will be a *further* great fall in temperature. The argument for this double diminution of temperature is, of course, utterly invalid. If a great proportion of our winter-heat be not due to sun-heat, then a considerable