

### The Astronomical Theory of the Ice Age.

MAY I first acknowledge the gentle kindness with which my early teacher and friend, Sir Robert Ball, has pointed out my error in quoting from the old edition of his work. I much regret that I did not make further inquiries, but I was satisfied when the library clerks at Trinity College, Dublin, told me that if there had been any alteration in the text, they would have received a copy of the second edition. It appeared from Sir H. Howorth's letter that the mistake originated with the publishers, who erroneously informed the library agent that the second edition was a mere reprint, and therefore refused to supply a copy.

Both Sir Robert Ball and Dr. Wallace, in their letters in *NATURE* of January 9, have misunderstood the way in which I present my argument. If Dr. Wallace would read my papers again, I think he will see that, so far as I am concerned, the whole of his letter is founded on a complete misapprehension; and Sir Robert Ball will, I hope, also agree that he has somewhat altered the form in which I have stated my conclusions, and that I have fully recognised the difference which he thinks I have ignored. But as the matter really at issue is the present position of the astronomical theory, I may be excused from discussing this misunderstanding further, for even if every word of their criticisms on my conclusions were valid, the astronomical theory, as it issued from the labours of Croll and Ball, would be in no better position than before. Whether I am right or wrong in my belief that the astronomical factor cannot have been the principal one, I venture to think there can be no doubt that the existing exposition of that theory must be given up.

The foundation of the astronomical theory is the fall in temperature directly due to diminished sun-heat. Croll and Ball accordingly give calculations which indicate a large fall. Croll gets  $45.3^{\circ}$  F. for the lowering of mid-winter temperature in Great Britain during the long excentric winter, and Ball's modification of Croll's method gives about  $25^{\circ}$  F. as the lowering of the winter temperature. The first five pages of my article in the *Phil. Mag.* for December 1894 are devoted to showing that

there is no justification for the principle on which this calculation is made, and that the fall must be a mere fraction of that postulated in either exposition of the astronomical theory. The chief flaw in the calculation is, curiously enough, that which Sir Robert and Dr. Wallace erroneously attribute to me, viz. that of considering that changes in terrestrial temperature are directly proportional to the changes in sun-heat, and ignoring the important element of storage and transference by ocean and air currents. How unsafe this is may be judged from the fact that if the method used to calculate the temperature in the Glacial Age from that in the present day were applied to find the summer temperature from the winter temperature, we should find for the British Isles a summer temperature of above  $300^{\circ}$  F. if we take Ball's hypothesis, and some thousands of degrees Fahrenheit if we take Croll's. If we calculate the winter temperature from the summer one, we should get  $-125^{\circ}$  F. for our winter temperature. A method which gives results in such striking contrast to the truth can hardly be accepted as a basis for a scientific theory.

If, therefore, this first portion of my criticism be correct (and hitherto no attempt has been made to refute it) the astronomical hypothesis is in just the position it would occupy if neither Croll's nor Ball's book had been written. So far, *the hypothesis itself* may be true or false; it is only the *reasoning* which has been put forward in its support that has to be abandoned or modified. The theory is, as all will admit, a tempting one, and accordingly I sought for some other means of establishing it. After several fruitless efforts to hit on a fairly satisfactory method of estimating the *direct* effect of an altered distribution of sun-heat on terrestrial temperatures, the method which Prof. Darwin has described occurred to me, and from it, *combined with a discussion on the transference of heat by the Gulf Stream* (see *Phil. Mag.* December 1894, p. 548 and p. 551), I was led to infer that for the British Isles at least the glaciation could not with any degree of probability be attributed to the long winter of great excentricity.

Sir Robert Ball's views, as presented in his letter, seems to involve a return to Croll's point of view, at least to the extent that the purely astronomical reason requires to be supplemented by a discussion of the oceanic and atmospheric currents. This view appears to me a true one; the only hope for the astronomical theory would be to show that the adjustment of terrestrial temperatures by the interaction of ocean and air currents with direct sun-heat is such that a very slight alteration of sun-heat produces a very great alteration of temperature; so that if the sun-heat which falls on Cornwall in winter were to be reduced to that which falls on Yorkshire, with corresponding changes for the temperate latitudes, and somewhat greater ones for the tropical belt, the ultimate result would be an Ice Age. But how can we hope to establish such a theory when we remember what a comparatively small change of temperature is due to the far greater changes of sun-heat from equator to pole as summer gives way to winter? EDWD. P. CULVERWELL.

Trinity College, Dublin, January 14.