LETTERS TO THE EDITOR.

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The Cause of an Ice Age.

SEVERAL letters from Sir Henry Howorth, Dr. Hobson, Mr. Culverwell, and Prof. Darwin, having appeared in NATURE relating to my little book on the "Cause of an Ice Age," I shall be glad if you will allow me to make a few remarks on the matter. In his first letter, Sir Henry Howorth thinks I have omitted to give Wiener the credit which was justly his due. Subsequent letters by Dr. Hobson and Sir Henry Howorth may be held to have cleared up this matter; still there is a point which has escaped Sir Henry Howorth's attention, and I therefore refer to it again.

The facts are as follows. When I first began to work at the Ice Age I arrived independently, as any mathematician might easily have done, at a theorem by no means difficult, which seemed to me of importance in connection with the subject of geological climates. I had never seen this theorem before; had I done so I should, of course, have properly acknowledged its prior discovery.

Soon after the publication of my book, Prof. Darwin kindly pointed out to me that the mathematical theorem in question had been already given by Wiener. Thereupon I did all that it seemed possible to do. I called attention to Wiener's priority at once by a letter to NATURE, which appeared on February 18, 1892, and I also mentioned his priority both in the preface and the text of the second edition of the "Cause of an Ice Age," which was published in 1892. Sir Henry Howorth, when he wrote his recent letters in which he thought I had not rendered justice to Wiener, could not, I am sure, have known all the facts as above stated.

Mr. Culverwell thinks that I was wrong in attributing a certain opinion to Croll, and I quite admit that this charge might once have been correct. The fact is, I had been mistaken in the meaning I read into a passage in Croll's "Climate and Time," p. 56. But I think if Mr. Culverwell had known the circumstances, he would hardly have considered it necessary to raise this question again. On the appearance of the first edition of my book, the mistake I had made was kindly pointed out by Mr. Monck, as well as by Mr. Noble, and I think by others; and I accordingly amended the second edition. In the *Geological Magazine* for February 1895, p. 58, Mr. Culverwell appeals to me to correct certain passages relating to this point which he puts into italics from the first edition. My excellent friend had not the slightest notion that these passages had been already corrected in the second edition, published two years before his paper.

I must, however, say that on looking over my book again in connection with this correspondence, I consider that some of the references I have made to this particular point might be further amended. If, however, Sir Henry Howorth still thinks that I have at any time regarded Croll's work otherwise than with due respect, I would like to remind him of the words in both editions, p. 112, in which I said:—

"I was greatly struck by this work ('Climate and Time') when I first read it many years ago. Subsequent acquaintance with this volume, and also with his second work ('Climate and Cosmology'), has only increased my respect for the author's scientific sagacity, and my admiration for the patience and the skill with which he has collected and marshalled the evidence for the theory that he has urged so forcibly."

I have studied with much interest and profit the investigations made by Mr. Culverwell in connection with the astronomical theory of the Ice Age, and I may be permitted to say how glad I am that so excellent a mathematician and physicist should have had his attention drawn to this subject. I may, however, take this opportunity to explain why I have had to remain unconverted by certain of his arguments, notwithstanding that they have carried conviction to Sir Henry Howorth and Prof. Darwin.

In his earlier paper in the *Geological Magazine* for January 1895, p. 9, Mr. Culverwell has demonstrated that the direct sun-heat received on any parallel at the time of greatest eccentricity, is the same as that now received on the parallel not more than three or four degrees north. This seems to me not only a novel, but also a very instructive result, and is in any case a valuable contribution to the theory. Mr. Culverwell, however, goes on to deduce from this that the climatic change in England between the present time and the time of the greatest eccentricity, would be no greater than the present climatic difference between Yorkshire and Cornwall, and hence he concludes that the astronomical theory is incompetent to account for the Ice Age. Prof. Darwin seems to think that this argument is unanswerable; I hope he will forgive me if I say that here my dissent begins. I think the facts cited do not warrant the inference which Mr. Culverwell would draw from them. With due respect to Mr. Culverwell, I would say that he seems at this point to have quite forgotten that the actual temperature in a region depends not merely upon the sun-heat there received, but also upon the transference of heat across the boundaries of that region. He takes the actual temperatures of Yorkshire and Cornwall; but what his argument would really require is a totally different thing. It would be the temperatures of those counties if each of them were perennially surrounded by a wall extending to the top of the atmosphere, and adiabatic to all heat except direct solar radiation.

This point is so important that I must put it in a somewhat different manner. It is certain that the actual climatic gradient from the equator to the pole is very different from what that gradient would have been if each parallel of latitude had marked the course of an adiabatic barricade such that no heat transference *via* earth, air or water could take place from zone to zone. In the latter case I quite admit that the mean temperature due to the sun-heat received on any zone would be actually the mean temperature of that zone, but the same is not true of the actual climatic gradient as we have it in nature. For, on account of heat transference, the mean temperature of a zone is by no means the same thing as the mean temperature due to the sunbeams received by that zone.

May I say that I think the fallacy throughout this part of Mr. Culverwell's argument arises from his overlooking the distinction between the actual gradient and the adiabatic gradient. There may be but little difference between the mean temperatures of a zone through Yorkshire, and a zone through Cornwall; but this does not prove, as Mr. Culverwell's theory requires, that there would be but little difference between a mean temperature due solely to the direct sun-heat falling on the zone through Yorkshire, and a mean temperature due solely to the direct sun-heat falling on the zone through Cornwall. This inference would only be sound if all parallels were adiabatic. This they certainly are not.

I do not question that the difference between present temperatures and the temperatures at the time of highest eccentricity might be fairly represented by the difference between the temperature due to the sun-heat received in the latitude of Yorkshire, and the sun-heat received in the latitude of Cornwall. What I do question are the grounds on which Mr. Culverwell maintains that this latter difference (and therefore the former one) is so insignificant as to discredit the astronomical theory of the Ice Age.

I have thus explained in what respect Mr. Culverwell's investigation involves assumptions which are in my opinion unsound. I am accordingly to this extent unable to accept the conclusions at which he has arrived. ROBERT S. BALL.

Observatory, Cambridge, January 2.

THE letter of Prof. G. H. Darwin in your last issue states very clearly the argument on which Mr. Culverwell and himself rely as affording a demonstration of the inadequacy of the astronomical theory. It now seems opportune, therefore, to lay before your readers the general considerations which lead me to the conclusion that the whole argument they rest upon is unsound; and, further, that Sir Robert Ball's ratio of 63 to 37, representing the ratios of sun-heat received by each hemisphere in summer and winter respectively, is (contrary to Prof. Darwin's view) an important factor in any adequate discussion of the problem.

Accepting Prof. Darwin's estimate that the difference in the amount of sun-heat received in our latitudes during high and low eccentricity, would only give to Yorkshire the amount received by London or *vice versa*, I entirely demur to his statement that this would be also a measure of the amount of change in the climates of these places. To do so is to assume that the climate of a place, as regards the amount and distribution