

Sir R. Ball's "Cause of an Ice Age."

SOME books appear under such authoritative sanction that, apart altogether from their arguments and their facts, they naturally influence opinion. This must be said of a book recently reviewed in your pages (January 28, p. 289); namely, "The Cause of an Ice Age," by Sir Robert Ball, the first of a series on modern science, edited by Sir John Lubbock.

The position taken up in this work is so much at issue with the views of many prominent geologists, and its general tendency seems so retrograde, that I am a little surprised it has not been adversely criticized.

I do not propose in this letter to enter into the general question as to the astronomical causes of an Ice Age, or whether an Ice Age can be shown to be a consequence of a varying eccentricity, upon which Croll and others have spoken very emphatically. I would rather limit myself to the particular new factor which Sir R. Ball has added to the problem. He claims that he has shown, and I do not contest the matter in any way, that, "of the total amount of heat received from the sun on a hemisphere of the earth in the course of a year, 63 per cent. is received during the summer, and 37 per cent. is received during the winter." This law he claims as "the fundamental truth which is the cardinal feature of his book, . . . the one central feature by which it is to be judged." His chief object, he says, "is to emphasize the relation of these figures to the astronomical theory, which will be entirely misunderstood unless the facts signified by these numbers are borne in mind."

What I wish to point out is that, although I have read the book more than once, I cannot find how this law is in any way connected with the general conclusions of the book.

"The cause of an Ice Age" must surely be something which is not always present and always equally efficient, but which works differently at different times, which, if operating at one time to produce an Ice Age, must either lose its effectiveness or be otherwise modified so as to permit of the existence of a temperate climate at another time.

Sir Robert Ball admits without doubt that the factor he relies upon, instead of being a variable one, is constant. He says: "The datum in our system on which the distribution depends, is the obliquity of the ecliptic"; and he goes on to say that, "amid so much that is changeable in the planetary system, it is fortunate that the obliquity of the ecliptic may for our present purpose be regarded as practically constant" (*op. cit.*, 87). He then goes on to compare the conditions which follow a small and a large eccentricity, and says: "Notwithstanding the wide difference between such a movement and that previously considered" (*i.e.* between movement in a very oblate and one in a more prolate ellipse), "it still remains true that 63 per cent. of the sun's heat is received by each hemisphere in summer, leaving only 37 per cent. for the winter" (*ib.*, 92). He again tells us that the figures 63 and 37 are independent both of the eccentricity of the orbit and of the position of the line of equinoxes; and that while the varying eccentricity created a distinction between a possible winter of 199 days and a summer of 166 days in one hemisphere, and the reversal of these same proportions in the other, that in each case the figures 63 and 37 represent the proportional quantities of heat which that hemisphere receives in summer and winter respectively" (*ib.*, 99). Lastly, speaking of the same figures, he says "they derive their importance from their constancy; they would remain the same however the dimensions of the orbit be altered, however its eccentricity be altered, or in whatever direction the plane of the earth's equator may intersect the plane of the earth's revolution around the sun." "These numbers are both functions of but a single element, which is the obliquity of the ecliptic. As this fluctuates but little, at least within the periods that are required for recent Ice Ages, the numbers we have given are regarded as sensibly constant throughout every phase through which the earth's orbit has passed within Glacial times" (*ib.*, 121).

These statements are explicit enough, and they show that the factor upon which Sir R. Ball relies is a constant factor, and being constant under all circumstances it cannot be the cause of an Ice Age. Whatever potency it has is being exerted now as much as it would be then. If it were an efficient cause of an Ice Age, we ought to be passing through one now. This argument seems to me to be complete and conclusive, and, if so, I cannot see how Sir R. Ball has done anything at all to solve the problem; for, putting this factor aside, we are remitted back to the conditions present to

Croll and others, which have been so completely shown to be inadequate to produce an Ice Age. As I am writing a big book in which I am attacking what I deem to be the extravagant and fantastic views of an influential school of geologists in regard to the so-called Ice Age, I naturally looked forward to Sir R. Ball's book with interest, and have read it with care, but I cannot see how it advances the solution of the problem, or how its position can be maintained.

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House of Commons, February 13.

The University of London.

MR. THISELTON-DYER, in his recent discussion of the London University question (p. 392), makes one statement which seems to me open to criticism:—

The statement is that the Colleges of the English Universities have "intrusted the business of sampling their goods to those who had nothing to do with their manufacture." Of the internal mechanism of the University of Oxford I know nothing; but I do know that in Cambridge the tendency is, and has been for the last ten years, in the direction of the reconstruction of that "teacher-examiner system" which Mr. Thiselton-Dyer believes to have been given up. The higher teaching in Cambridge is falling more and more completely into the hands of three classes of men, namely:—

(1) Professors, appointed by the University, and imposed by the University upon the Colleges, so that in each College there is at least one person who is a member of the body simply by virtue of his University office. In this way at least one subject is represented in every College by a University officer.

(2) University Readers and Lecturers, who give systematic instruction to all members of the University, without distinction of College. As these men are on the one hand appointed by the University, and are on the other hand, as a rule, members of various Colleges, they establish a further bond of union between the Colleges and the University.

(3) College Lecturers, who are now in the habit of throwing open their lectures to members of Colleges other than their own, and who are frequently members of the University Boards of Studies.

In this way the higher teaching is being thrown more and more completely into the hands of men who are under the direct control of the University itself; and a study of the current Calendar shows that the task of examining students is intrusted largely to these very men. Of the examiners for the various Triposes (of whom there are about eighty), at least fifty-six belong to one of the three categories above mentioned. Those examiners who are non-resident, or who are not engaged in teaching, act as a rule in conjunction with colleagues who are actual teachers, so that there is no single Tripos in which a student is not fairly certain to be "sampled" (to use Mr. Thiselton-Dyer's phrase) by a man who has had a great deal to do with his "manufacture."

This is almost precisely the "teacher-examiner system" to which Mr. Thiselton-Dyer refers; and the steady growth of this system in Cambridge is a certain proof that it is not incompatible with the development of the highest type of University in England.

W. F. R. WELDON.

University College, London, February 27.

The Aneroid in Hypsometry.

FROM a review under this title in NATURE of the 11th ult. (p. 339) it appears that Mr. Whympster has done good service to those who use the aneroid in measuring heights, by pointing out a very serious source of error in this instrument. According to the reviewer:—"All who have had any experience in testing aneroids in the usual way, viz. by subjecting them to gradually reduced pressures under the air-pump, and comparing their readings with the concomitant indications of the manometer, are aware that the variations of the two instruments with falling and then with increasing pressures are by no means concordant; but it will be probably new to most that, when the aneroid is allowed to remain for some weeks under the reduced pressure, its indications continue falling, and to such an extent that its final error in certain cases is five or six times as great as when the exhaustion was first completed. On the other hand, aneroids that have been kept for some weeks at a low pressure when restored to the full pressure of the atmosphere take many weeks to regain