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.

HENRY H. HOWORTH.

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

their condition of equilibrium. The greater part of the recovery takes place in the first week, and a considerable part in the course of the first day."

Now I have little doubt that both the want of accordance in the readings of the instrument with decreasing and then with increasing pressure, and the "after-working" mentioned above, are mainly, if not entirely, due to the imperfect elasticity of the corrugated disk that forms the cover of the exhausted chamber. No metal is perfectly elastic except with very minute stresses, and, as a consequence, when a metal is made to go through a complete stress cycle, there is always more or less lagging of strain behind stress. Again, there is with all metals more or less of *time-lag*, so that any alteration of stress does not produce its full effect all at once. Provided the temperature be kept constant, and the metal be not in any way disturbed, the time-lag is of such a nature that for equal successive intervals of time the corresponding changes of strain form a descending geometrical progression. With some metals, such as tempered steel, and with moderate stresses, the effects of imperfect elasticity are not of any material consequence. With others, however, such as aluminium and zinc, and the alloys of the latter metal—namely, brass, *German-silver*, &c.—we meet with very appreciable deviations from the laws of perfect elasticity, even when the stresses used do not produce any permanent deformation. I understand that the corrugated cover is frequently made of an alloy something like German-silver, only softer. If this be so, I can well believe, from my experience of this alloy, that grave errors might arise, and probably have arisen, in the determinations of heights by the aneroid. If such a thing be feasible, I would suggest that the cover should be made of tempered steel.

HERBERT TOMLINSON.

King's College, Strand, February 19.

Sparrows and Crocuses.

THE time of year has arrived when we shall once more be hearing of the ravages of sparrows on crocus blooms, and the theories advanced in order to account for this propensity for destruction on the part of the sparrow in suburban gardens and elsewhere. One pet theory is that the sparrow has a fondness for *yellow*, and shows it by destroying crocuses of that colour. Most unfortunately for the holders of such an opinion, the sparrow does not confine its attentions to yellow crocuses only, but attacks also the purple, white, &c., as any grower of crocuses can prove. Undoubtedly the yellow suffer most, probably because they are the first to appear, and meet the birds' most pressing requirements. Moreover, the sparrows sometimes attack the flowers while still in the sheath, and before it is certain what colour they will be.

The object of the sparrow in destroying the flowers is simply to obtain *succulent food* at a time of year when such in the form of larvæ, &c., is scarce. I have repeatedly watched the operation from my study window at a distance of very few feet. The stalk of the flower is bitten off by the bird some little distance below the flower itself. The succulent stalk is then nibbled away until the flower falls to pieces. The reproductive parts, and especially the anthers are not attacked, as some writers have asserted; but in consequence of the structure of the flower, they, like the petals and sepals, often fall away owing to the close nibbling of the bird.

Primroses also suffer. Early primroses are usually the common *yellow form*, *ergo*, according to theory-makers, the same cause is at work. So it is, but not in the direction they would have us believe. Here, again, I have distinctly seen the birds eating the flower-stalk.

I had written you a letter to the same effect as this about the same time last year, but from some cause or other it was not forwarded. I take this opportunity of possibly anticipating other letters on the same subject, and of inducing theorists to carefully watch the *modus operandi* as I have done before rushing into print.

R. MCLACHLAN.

Lewisham, February 26.

A Possible Misunderstanding.

I HAVE seen a report that, in a recent number of the *Atti della Regia Accademia delle Scienze di Torino*, Prof. Galileo Ferraris is credited with a statement which might mean that one of the formulæ which appear in a paper read by me before the Physical Society of London, in May 1888, was derived from a

paper by him. If that be Signor Ferraris's meaning, he is entirely mistaken. My formulæ were obtained quite independently of Signor Ferraris or of anyone else.

THOMAS H. BLAKESLEY.

Royal Naval College, February 29.

HERMANN KOPP.

HERMANN FRANZ MORITZ KOPP, a distinguished German chemist, and one of that band of literary and scientific workers which, five-and-twenty years ago, made Heidelberg celebrated as a centre of intellectual activity, passed away from the scene of his labours on February 20, in the seventy-fifth year of his age. He had been in failing health for some time past, and although his recuperative power at times seemed wonderful, his friends were not wholly unprepared for his decease.

Born October 30, 1817, at Hanau, where his father, Johann Heinrich Kopp, practised as a physician, Hermann Kopp received his school training at the Gymnasium of his native town, and thence passed to the Universities of Heidelberg and Marburg with the object of studying the natural sciences, and more particularly chemistry. The special bent of his mind towards chemistry would seem to have been given by his father. The elder Kopp occasionally busied himself with experimental chemistry, and Leonhard's *Taschenbuch* and Gehlen's *Journal* contain papers by him on mineral analyses and on investigations relating to physiological chemical products.

In 1839, Hermann Kopp joined Liebig at Giessen, drawn thither by the extraordinary influence which has made the little laboratory on the banks of the Lahn for ever famous in the history of chemical science. For nearly a quarter of a century Kopp found in Giessen full scope for his scientific and literary activity. In 1841 he became a *privat-docent* in the University, two years later he was made an extraordinary professor, and in 1853 he became ordinary professor. In 1864 he was called to Heidelberg, where he remained until his death, occupying himself latterly with lectures on the history of chemistry, and on chemical crystallography.

At the very outset of his career as an investigator, Kopp seems to have devoted himself to that field of inquiry in which his chief distinction as an original worker was won, viz. physical chemistry. One of his earliest papers—"Ueber die Vorausbestimmung des specifischen Gewichts einiger Klassen chemischer Verbindungen," published in *Poggendorff's Annalen* in the year he went to Giessen—deals with the conception of *specific volume*, which he here introduces for the first time. During the ensuing five-and-twenty years, so far as laboratory work was concerned, he was almost entirely occupied in attempting to trace experimentally the connection between the physical properties of substances and their chemical nature. We owe to Kopp, in fact, all our broad fundamental generalizations concerning the connection between the molecular weights, relative densities, boiling-points, and specific heats of substances, and on the relations of crystalline form and chemical constitution to specific volume. For work of this kind Kopp was eminently well fitted. To remarkable manipulative dexterity and great ingenuity—much of which, as in the case of Wollaston, was spent in satisfying a certain fastidiousness for simplicity of apparatus and experimental method—was joined the most scrupulous sense of accuracy and illimitable patience. As proof of his accuracy, it may be stated that, although many observers have had occasion, from time to time, to review his work on the thermal expansion of liquids—and on a far more ambitious scale, and with more refined apparatus, than was possible half a century ago—his determinations have been practically unchallenged, and retain their place among the best ascertained constants of their kind.