LETTERS TO THE EDITOR.

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The Size of the Brain in the Insectivore Centetes.

I DO not think that there is any recent mammal which has so small a brain in proportion to the size of the skull as has *Centetes caudatus*. In an individual of this species, the property of the Hon. Walter Rothschild, which was lately living in the Zoological Society's Gardens, I found the total length of the brain to be 28 mm., of which no less than 8 mm. were taken up by the enormous olfactory bulbs. The dried skull of that individual—measured along the base and not taking into account the projecting occipital region—was 96 mm. The greatest diameter of the brain is 16 mm.; the skull in that region is from 28 to 40 mm. broad. The small size of the brain relatively to the skull has been frequently commented upon and figured in certain of the early genera of Ungulate mammals; and it may be noted that the measurements which characterise *Centetes*, undoubtedly an early type of mammal, are by no means unlike those of such a genus as *Coryphodon*, judging, that is to say, by the published figures of the brain cast and skull of that animal. The resemblance is increased by the small size of the cerebral hemispheres, and by the complete exposure of the corpora quadrigemina in *Centetes*. I hope shortly to give a fuller and illustrated account of the brain of this Insectivore.

FRANK E. BEDDARD.

Thermochemical Relations.

IN a letter in NATURE of February 7 (p. 348), Signor Carlo Del Lungo calls attention to certain thermochemical equalities, and asks for an expression of opinion as to whether these are the result of a casual coincidence or of a definite law. The equalities in question, if they were not accidental, would have an extremely important bearing on thermochemistry generally, but unfortunately, I believe, they are probably mere accidents of coincidence.

The two instances given would seem to indicate that when two elements combine together, the heat evolved is equal to the sum of that which is liberated when these same two elements combine separately with some third. Thus we have (modifying Signor Del Lungo's equations so as to indicate more clearly that the heat of combination refers to that of the elements taken in their ordinary molecular condition):—

and

I have little doubt but that such coincidences might be shown to be accidental by the existence of similar sets of compound, the heats of formation of which show no such relationship. Unfortunately, I have with me no thermochemical works to supply the requisite data. An examination of the above instances, however, will, I think, suffice to prove that the coincidences cannot be the result of any definite law.

When we expand the first of the above equations so as to represent all the stages involved, and the combinations and separations of the atoms in the various molecules, we get:—

which simplifies into

This would mean that any pair of the four groups of atoms in the following diagram would combine together in any way, and yet liberate the same amount of heat—i.e. the thermal result would be the same whether the products were oxygen gas and hydrogen chloride, or water and hypochlorous anhydride.

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This, if established as a generality, would be of fundamental importance. But it is inherently improbable, and the existence of the second instance given by Signor Del Lungo seems to render it untenable. If it were true as regards the gases figuring in the above reaction, it would certainly not be true if one of the products concerned were in the solid condition; if, for instance, we dealt with solid instead of gaseous oxygen, there would be a divergence from equality equal to the heat of condensation and solidification of oxygen. Yet the second instance given by Signor del Lungo contains terms of this very character. When treated as above, assuming for the sake of simplicity that solid carbon molecules contain only two atoms, it gives

Thus the two instances given, inasmuch as they are necessarily dissimilar, must by their coincidence show that this coincidence is the result of accident, and not of any thermochemical law.

Woolacombe, February 12.

SPENCER PICKERING.

The Fraunhofer Lines in the Spectrum of the Corona.

IN some of the preliminary reports of the observations made during the last eclipse, undue importance appears to have been attached to the supposed absence of the Fraunhofer lines from the spectrum of the corona. Mr. Newall, for instance (*Roy. Soc. Proc.*, vol. lxvii. p. 365), says, "it is difficult to reconcile the marked polarisation (of the coronal light) evidenced in this investigation with the absence of Fraunhofer lines in the spectrum of the corona." Mr. Abbot, whose bolometric observations appear to him to indicate that the corona does not reflect much solar light, states that "additional evidence against the theory of reflecting particles is found in the Indian eclipse spectroscopic results of Campbell, who found a continuous spectrum from the inner corona with total absence of dark lines" (*Astrophys. Journ.*, vol. xii. p. 75).

Prof. R. W. Wood (*NATURE*, vol. lxiii. p. 230) has already suggested a partial reconciliation of the apparently conflicting observations, but there are other points which do not seem to have received sufficient attention.

It must be remembered that the Fraunhofer lines were observed in the corona of 1871 by Janssen, and were subsequently photographed by Schuster in 1882, and by Hills in 1893, besides having been seen by various observers on other occasions. Great weight seems to have been attached to the photographs taken in 1898 and 1900, in which no dark lines were recorded, but I venture to suggest that the apparent absence of the lines on these occasions was mainly due to the use of spectroscopes of too great a dispersion to exhibit them. While great dispersion is well adapted to render the bright line spectrum more effective, it is clearly fatal to the dark line spectrum, as in the observation of solar prominences. As a matter of fact, the recent photographs only show continuous spectrum extending to a few minutes from the sun's limb, and if one may take the evidence of the prismatic cameras, this arises mainly from the bright line giving inner corona, which probably does not shine chiefly by reflected light, and would, therefore, not necessarily show the Fraunhofer lines. This view accords well with Schuster's account of the photograph of 1882, in which it is stated that near G there was a sharp decrease in intensity at 0.29 of the solar radius from the limb, a further falling off in intensity at 0.60, with the spectrum fading out of view at 1.47; in the lower regions the spectrum was perfectly continuous, but in the upper regions the solar line G was reversed. Captain Hills also found, in 1893, the Fraunhofer lines only at a considerable distance from the limb. Stone, too, in 1874, observed the dark lines in the outer, but not in the inner corona. In these early observations and photographs we may take it that the dispersion of the spectroscopes employed was much less than in 1898 and 1900, and it would therefore seem desirable to search for the Fraunhofer lines in the next eclipse with instruments of smaller dispersion than those recently in use for the bright line spectrum and special observations.

It may be further remarked that as we do not see a section of the sun, the outer corona must be superposed upon the inner, and the fact that the Fraunhofer lines have not been observed or photographed in the inner corona, even with small dispersion, is sufficiently explained by the superposition of the dark line spectrum on the much stronger continuous spectrum of the inner corona. Such superposition would, of course, be more effective