

to the highest. This is an ingenious compromise between two methods each of which has something to recommend it. The early chapters, that deal with the functions of animals, the modern conception of protoplasm, the elements of structure, reproduction, the evolution of sex, and heredity are particularly good. The chapter on palæontology is, owing to the necessary limitations, far too short for the subject, but a table makes clear the order in which the different classes of animals appeared upon the earth. When we come to the body of the book we notice, as in the opening chapters, the remarkable clearness of the style; and though morphology is in no way neglected, yet some room is always found for the description of the habits of the animals in question. For instance, there are some eight pages devoted to the habits and functions of birds, their modes of flight, their courtship, their nests, moulting, diet, migrations.

One or two minor points may now be mentioned that seem to be open to criticism. Plants and animals, Prof. Thomson says, "represent the divergent branches of a V-shaped tree of life." But plants originated before animals; the nature of their food proves this beyond a doubt. Animals we must look upon as a branch from the primitive vegetable stem. The account of the Hydromedusæ would be much better for an illustration—a figure of a hydroid with the Medusa of the alternating generation or of Tubularia with its Actinula. Such additions would, of course, increase the bulk of the book, but the figure of a frog (p. 560) is superfluous, since everyone knows what a frog is like. Again, the process of natural selection is easily intelligible without Fig. 378.

F. W. H.

Animal Heroes; being the Histories of a Cat, a Dog, a Pigeon, a Lynx, two Wolves, and a Reindeer. By E. T. Seton. Pp. 362; illustrated. (London: Archibald Constable and Co., Ltd., 1906.) Price 6s. net.

MR. SETON has always something fresh and interesting to tell his readers, and in the present beautifully illustrated volume breaks new ground in attempting to reveal some aspects of the strenuous side of the lives of animals, both wild and domesticated. Every one of the stories, we are told—although of course amplified and set out with the picturesque surroundings the author knows so well how to portray—is founded on the actual life of some individual bird or quadruped; the biography of the lynx being based on the author's own backwood experiences. Where all is so good, fresh, and entertaining, it seems almost invidious to select one portion of the book for special commendation. To our thinking, however, the almost pathetic story of "Arnaud," the homing-pigeon, is far ahead of the rest in sustained interest; but some may prefer the history of the tame wolf, while to others, again, the narrative of the wild reindeer may appeal more strongly. Alike to young and old the book may be heartily commended as an excellent example of the best style of animal biography.

R. L.

Some Facts about the Weather. By William Marriott. Pp. 32. (London: Edward Stanford, 1906.) Price 6d.

This pamphlet supplies just the information about meteorological phenomena likely to be useful to the general public. The instruments in use in climatological stations are enumerated, and the determining factors of climate are explained in order. The booklet should be the means of stimulating interest in the scientific study of weather.

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LETTERS TO THE EDITOR.

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Osmotic Pressure.

PROF. KAHLENBERG'S letter published in NATURE on July 5 shows that, as so often happens, the controversy about osmotic pressure is based on a difference in meaning assigned to that term.

We may adopt what now appears to be Prof. Kahlenberg's conception, and regard the osmotic pressure of a solution as a real experimental pressure reached with some actual membrane in certain practical conditions. Such a definition gives us a conception of great interest and importance, especially from a physiological point of view. But unfortunately it has no bearing on the thermodynamic theory of solution—or the allied theories of fusion and evaporation—which apparently Prof. Kahlenberg still wishes to attack by its means, after he has insisted that "the formation of crystals from a solution, or the concentration of a solution by evaporation are not osmotic processes." Of course they are not osmotic processes in Prof. Kahlenberg's sense of the term. But the theory of fusion and evaporation, which, as I pointed out in my letter published on May 31, has been verified experimentally in the case of the depression of the freezing point to an accuracy of nearly one in a thousand, depends on the hypothetical separation of solvent by some ideal and perfect semi-permeable process.

It is such considerations as these that demand the other conception of osmotic pressure, which, suggested no doubt by Pfeffer's experiments on osmosis, has now, in accordance with the usual course of development of the concepts of physical science, come to possess an ideal significance, towards which the actual experimentally measured quantity can but tend as the experimental conditions approach the ideal state postulated in the theoretical definition.

Defining osmotic pressure as the hydrostatic pressure needed to keep a solution in equilibrium with its solvent across an ideally perfect semi-permeable membrane, we obtain a conception, possibly of less chemical and physiological importance, which nevertheless enables us to develop a thermodynamic theory of solution; and this theory has been verified experimentally in cases where we have reason to suppose that the actual conditions approach the ideal.

I have found that this confusion of ideas as to the conception of osmotic pressure has occasioned trouble in other cases. It would be well if a new name could be applied to osmotic pressure when used in one or other of its meanings; but I suppose that each side in the controversy would insist on the rights of possession and customary usage. Hence I would suggest that, at the cost of some complexity of nomenclature, one of the two meanings should be emphasised as "experimental osmotic pressure" and the other as "thermodynamic osmotic pressure."

Prof. Kahlenberg remarks that "in creating the theory of electrolytic dissociation, the actual phenomena of electrolysis have played a minor part," and wishes thus to invalidate my statement that "the theory rests on electrical evidence, and by such evidence it must be tried." I can hardly believe that Prof. Kahlenberg would wish seriously to commit himself to the opinion that the historical train of ideas by which a given hypothesis may have been reached necessarily supplies the only (or even the best) logical basis for its support. We do not always doubt the stability of our houses because it has been necessary or convenient to remove some of the scaffolding used in their construction.

It is true that the abnormally great osmotic pressures and freezing-point depressions of electrolytic solutions originally suggested that the molecules of their solutes were dissociated; but such observations clearly can give no information on the electrical state of the dissociated structures. A valid test for an electric ion must depend on some electrical property, such as motion in an electric field.