

is little more than a fixed fleshy stalk, upon which grows the body and its shell"—"a palpable case of what we call degeneration." If all degeneration were on the lines of the barnacle's life-history, it would be difficult to distinguish it from progress. We wish there had not been these and other blemishes in this sprightly and interesting little book, for it is sure to be popular.

J. A. T.

Le Chauffage des Habitations par Calorifères. By M. Raymond Périssé. Pp. 173. (Paris: Gauthier-Villars et Fils, n.d.) Price 2½ francs.

THIS little work is of a very practical nature; and although it appeals more particularly to the engineer and architect it may be read with advantage by the general reader, for it sets out, in a manner which is clear and easily intelligible to all, the advantages which accrue from the systems of heating dwellings by the various warming apparatus which are installed, not in the living rooms, but on the ground floor or in the basement. The advantages in the use of steam, hot-water, or hot-air apparatus, instead of fireplaces or stoves, are certainly real ones; for the house is more uniformly and better heated, at a less expense, trouble, and risk, and the apartments are not encumbered with the large stoves so generally seen on the Continent.

The advantages and disadvantages of the different systems are explained, and various applications of these systems are illustrated. The reader is also told how each may be best applied and regulated to meet the variable requirements as to heat, the different circumstances of the dwelling, the conditions of occupancy, and the like.

Auslese aus meiner Unterrichts- und Vorlesungs-praxis. By Dr. Hermann Schubert. Erster Band. Pp. 240. (Leipzig: G. J. Göschen, 1905.) Price 4 marks.

TEN chapters dealing with a variety of subjects—logarithms, cyclotomy, the theory of physical dimensions, systems of circles and spheres being the most important. The principal novelty is the treatment of logarithms (pp. 1-68), fair approximations being obtained by combining inequalities such as

$$-\log m+2 \log (m+1)-\log (m+2)>0.$$

The method is quite elementary and very ingenious, but it has no practical value, and strikes one as being artificial. The chapter on dimensions (reprinted from the *Naturw. Wochenschr.*, 1895) is interesting, but not convincing; its essential feature is

$$[\text{mass}] = [\text{length}]^2 [\text{time}]^{-2}.$$

Pangéométrie; ou Précis de Géométrie fondée sur une Théorie générale et rigoureuse des Parallèles. By N. J. Lobatschewsky. Réimpression facsimilé conforme à l'édition originale. Pp. 279-340, and list of errata. (Paris: A. Hermann, 1905.) Price 5 francs.

LOBATSCHEWSKY shares with the Bolyais the credit of founding the theory of non-Euclidean geometry, in which Euclid's axiom of parallels is not assumed to be true. His "Pangéométrie" was communicated to the mathematical faculty of the University of Kazan in 1855 on the occasion of his jubilee; this fact might well have been indicated in the present reprint. It is the author's last and most complete exposition of his theory, and mathematicians will be glad to have it in this accessible form, though, like other similar reprints, it is rather trying to the eye-sight.

LETTERS TO THE EDITOR.

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The Bates-Müller Hypothesis of Mimicry: a Question of Historical Accuracy.

A PAPER dealing with the above subject, by the late Dr. A. S. Packard, has just been published in the *Proceedings of the American Philosophical Society* (vol. xliii., No. 178, p. 393), in which this well known entomologist endeavours to show that the markings of organisms ("pœcilogensis") are "due to the physical rather than to the biological environment." I must leave it to others to consider how far the late author has established his case as against Bates, Fritz Müller, and those who have accepted the theories of mimicry associated with these names. My object in asking you to give space to this letter is to point out a distinct error which, if allowed to pass unchallenged, is likely to be accepted as a true statement of Darwin's views in the sense conveyed by the American writer.

Happening to know the actual history of the Müllerian theory of mimicry through the courtesy of Mr. Darwin himself, I can assure those who read the paper that the passage which is quoted does not refer to that theory at all. In the letter to Fritz Müller referred to (August 28, 1870) Mr. Darwin says:—"I should not be at all surprised if your suggestion about sexual selection were to prove true; but it seems rather *too speculative to be introduced in my book*," &c. ("More Letters," vol. ii., p. 91). Now Dr. Packard quotes only the words which I have italicised as "Darwin's own estimate of Müller's little paper," but this is a misstatement of the facts. Darwin, it will be observed, is referring to a suggestion about sexual selection, and I am in a position to state what that suggestion was. At the date of the correspondence quoted (1870), Fritz Müller had observed the striking resemblances, or "mimicry," between butterflies belonging to "protected" groups, as, in fact, Bates had done before him. In searching for an explanation of this apparent violation of the Batesian theory, he suggested that it almost appeared as though the females of one protected species had been influenced in their choice by seeing the predominant pattern of other protected species always about them. Mr. Darwin was good enough to allow me to read Müller's letter to him, and in forwarding it to me in 1872 he added:—"You will also see in this letter a strange speculation, which I should not dare to publish, about the appreciation of certain colours being developed in those species which frequently behold other forms similarly ornamented" ("Charles Darwin," by E. B. Poulton, p. 202). This is the "suggestion about sexual selection" to which Darwin refers in his letter to Müller, and, so far as my memory serves me, I do not think this speculation was ever formally published to the scientific world.

The Müllerian theory which the late Dr. Packard considered that he had demolished was not published until 1879, the "little paper" in question having been contained in a number of *Kosmos* which Mr. Darwin forwarded to me in that year. On reading the said note I was at once convinced that Müller had found the true explanation of mimicry between protected groups, and I accordingly directed Mr. Darwin's attention to the matter and published a translation of the paper (*Proc. Ent. Soc.*, 1879, p. 20) in order to bring it under the notice of English entomologists. Writing to me in 1879 about this paper, Mr. Darwin said:—"F. Müller's view of the mutual protection was quite new to me" (Poulton, *loc. cit.*, p. 213). It is thus evident that Dr. Packard confused a tentative speculation of Müller's, which was contained only in a letter to Darwin, and probably never intended for publication, with the now well known Müllerian theory which was published formally some nine years later.

R. MELDOLA.