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A few words in answer to Mr. Distant's "Remarks" on the genus *terias*

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will be a valuable aid to identification. The concluding chapter deals with the best methods of collecting and preserving specimens, and gives within a small compass a considerable amount of useful practical information. A short "Bibliography" is followed by a "Glossary," in which we note several rather serious errors. The "palpocil" is defined as a "collection of stinging cells;" it is really a simple tactile organ. The name "polypide" is referred to the alimentary zooid of the Hydroida, and "polypite" to that of the Polyzoa; the reverse would be true. "Trophosome" is not (as stated) a "Hydroid colony," but the assemblage of nutritive zooids in such a colony. "Operculum" is defined as a "protective covering or lid," which no doubt it is, as any dictionary would show. But the student wants to know its technical use, and should have been told that it is the valve which closes the orifice (oral valve) of the Polyzoa. The definition of "zooid" as "an alimentary or reproductive polyp" is much too limited. The avicularium and vibraculum are equally zooids. It would have been better to follow Huxley—"a term applied to the individuals of compound organisms."

The peculiar significance of the term "sporosac" is not indicated by calling it a "sac-shaped gonophore." It is, in fact, the generative sac—the sac in which the generative elements are developed.

The publication of the present elementary work may be taken as an indication of a somewhat widely diffused taste for the study of marine zoology, and we trust that it may not only gratify that taste in its own measure and degree, but lead many to desire more than it can give them, and to seek a fuller knowledge of the subject at other sources.

MISCELLANEOUS.

A few Words in Answer to Mr. Distant's "Remarks" on the Genus Terias. By ARTHUR G. BUTLER.

MUCH as I dislike unnecessary discussion on points which do not possess any "scientific value," I must call the attention of lepidopterists to the fact that Mr. Distant, whilst apparently answering my statements, has in almost every case avoided the point at issue, and therefore has laid himself open to the very charge of "misrepresentation" which he asks me to own to; this, *in the interest of science*, it is necessary to prove, since it affects the identification not of what Mr. Distant calls varieties, but of what he, in common with myself, would admit to be representatives of different groups.

Mr. Distant's explanation of his *lapsus calami*, for such I am willing to believe it to have been, is ingenious but not admissible: that he did not carefully consider his words when he called a species (not a "*species*") a variety, I can well understand; but that he, in a certain sense, believed that the said species was more than a variety, is evidenced by the constant use of dubious terms throughout his work, such as "new species or variety," "this species is of a varietal

nature," and so on; however, this matter is unimportant and need not be dwelt upon.

The points respecting *Delias ithida* and *Terias sari* may be easily settled; the types of both being in the collection of the British Museum, Mr. Distant, who is often able to visit that institution during the year, should neither have trusted my opinion as to the probable locality of the former nor Mr. Moore's identification of the latter. It is no excuse to say, when an error has been committed, "I did but copy the error of my friend;" it certainly reminds one of the ancient inhabitants of Paradise, but not in their happiest condition.

In answering what I thought a sufficiently clear statement respecting *T. senna*, Mr. Distant seems utterly to have missed the point—that the *T. santana* and *T. senna* of Felder are species allied to *T. drona*, or, if he prefers it, are local races of *T. drona*, and have next to nothing in common with *Terias inanata*. My identification of *T. senna* is the same as it always has been, for Mr. Distant himself proves, by reference to my paper of 1871, that I then regarded it as possibly a variety of *T. santana*; he must therefore be mistaken when he says that he examined specimens of *T. inanata* and *T. senna* in the national collection and failed to see any differences, since the most unskilled lepidopterist would see them at a glance.

Lastly, in my note on *T. æsiopæ* I begin by the distinct statement that "For some years past this species has been entirely misunderstood;" therefore what I or any one else regarded as *T. æsiopæ* in 1879 is entirely beside the mark as an excuse for figuring the "true *T. æsiopæ*" under the name of *T. hecabe*. The typical, and therefore "true," *T. æsiopæ* being a Chinese species and not occurring (so far as is known) in any part of India, the locality "Continental India" is not correct. If my statement on this head was of a misrepresentative character, I regret that it was made; but until I read Mr. Distant's explanation I always supposed that this truly "somewhat wide" one was intended to rank with others separated by the same stop, thus:—"Continental India; N.W. Himalaya; Bombay;" &c. I am not aware that I have anywhere remarked that Mr. Distant has figured as *T. æsiopæ* a variety of *T. hecabe*; what I have stated is that he has figured the male *T. æsiopæ* as *T. hecabe*, var. *a*.

The Nerve-terminations in the Pedicellariæ of Echinida, their Sense-organs and Glands. By Dr. OTTO HAMANN.

Nerve-terminations have not hitherto been described in the pedicellariæ of the Echinida, nor indeed elsewhere in their bodies. The only statement as to a supposed sense-organ has been made by Sladen*, who observed upon the inner surface of the three valves of gemmiform pedicellariæ in *Sphærechinus granularis* cushion-like elevations, which have since been seen also by later observers, such as Köhler†. The latter observer says:—"It is possible that these

* Ann. & Mag. Nat. Hist. ser. 5, vol. vi. p. 107 (1880).

† 'Annales du Musée d'Hist. nat. de Marseille,' 1883.