

discussion of the Origin of Insects. The general question of the Annulosa obviously includes that of Insects, and I therefore desire to correct this statement, and to refer your readers to a paper by me on *Choetogaster* and *Æolosoma*, published in the "Linnean Transactions," vol. xxvi. (read Dec. 1867), in which I have more than alluded to Mr. Spencer's views, and have offered some suggestions on the morphology of the head, and as to the unisegmental Annulose ancestor. Mr. Wallace quotes from this paper in reference to *Choetogaster*, though from the context it would appear that he is quoting from Professor Owen.

Since the researches which have rendered Mr. Wallace's name one of the first among living zoologists have not led him into practical anatomical and embryological studies, I may venture to add one or two strictures upon his statements relating to such matters. In the first place, those who are engaged in the study of insect embryology are not ignorant of Mr. Spencer's or similar views; the wide-spread study of his works in England and America, and of Haeckel's general morphology in Germany, is sufficient guarantee of this. But even if it were as Mr. Wallace supposes, he has not, in the extract given in NATURE, shown at all how Mr. Spencer's views on aggregation are to influence the study of the embryology of insects. Of course, the general theory of somites has immense importance in all studies relating to the Annulosa, but in what way the particular form of it, due to Mr. Spencer, can influence conclusions drawn from the observation of the manner in which insects develop from the egg, Mr. Wallace does not explain. Whether, admitting or denying the truth of Mr. Spencer's or Prof. Haeckel's views, it would be equally conceivable, did the observed facts point in either direction—that the ancestry of insects is to be traced to a simple nauplius-form or to a multi-segmental Annelid-like progenitor, the question of segmentation is not finally settled, though it is largely elucidated by the doctrine of Mr. Spencer. It is no doubt an instructive point of view to take—that segmentation is an arrested production of zooids, but it is equally true that the production of zooids is an exaggerated segmentation. We have no grounds for assuming the one more than the other as the essential process; they are both phases of the same process. The fact appears to be that in certain masses of organised matter, on their reaching a certain limit of growth, "polarities," which were hitherto held in one system, break up into two and so on. The simplest case of this is cell-division, but whether the systems separate entirely, as in simple fission, or remain associated, as in the cleavage of the egg and in the segmentation of the Annulosa, depends on another factor, a cohesive or integrating force proper to the growing mass.

In the present state of knowledge upon the subject, the assumption adopted and held of so much importance by Mr. Wallace—that the Vertebrata do not exhibit a segmentation of the same kind as that of the Annulosa, is by no means justified. Though much of their jointed iterative structure may probably be due to that kind of adaptation which Mr. Spencer so justly distinguishes as "superinduced segmentation," yet that there is a fundamental bud-segmentation, or segmentation of growth identical with that of Annulosa, is in the very highest degree probable. And even as to the Chiton, which Mr. Wallace quotes from Mr. Spencer as quite certainly an example of superinduced segmentation, I think that had he examined the grounds for making such a statement, he would have hesitated. The larva of Chiton is identical with that of an Annelid, and its segmentation makes its appearance in the same way. Why should there not be segmented molluscs? It is necessary most constantly to bear in mind, when considering this matter of segmentation, the possibility of the partial or complete obliteration of segmental characters due to tertiary aggregation, and their modification in most various ways in the evolution either of an individual or of a group.

Further, as to Mr. Wallace's expressions with regard to the segmentation of insects. From what was said above as to the relation of segmentation and zooid production, it follows that the conception of segmentation is erroneous which leads to ascribing to insects peculiar physiological or psychical properties on account of their being composed of "a number of individualities fused into one." This expression should not be allowed to lead to wider conclusions than those it formulates. As a matter of fact, insects are not a number of individualities fused into one, but rather one individuality partially (and as a reminiscence rather than actually) broken up into many, this partial breaking up being due to the mechanical properties of its tissues at a certain period of development.

If, by the "spiracles" of Annelids, Mr. Wallace means the segmental organs, it should be clearly stated that the identity of these with the tracheæ of insects has not yet been in any way proved. The comparison of the mode of development of these two sets of organs is just one of the points upon which embryologists are now at work.

Lastly, the researches of the last fifteen years do not, I venture to submit, lead to the conclusion adopted by Mr. Wallace, that the parthenogenesis of the higher Annulosa is analogous to or identical with gemmation as opposed to sexual reproduction or digenesis, but to the conclusion which is exactly opposed to this, namely, that it is identical with digenesis in all particulars but the absence of the male element.

Naples

E. RAY LANKESTER

Adaptive Coloration, Phosphorescence, &c.

No one who has watched a very young hare stealing from a green covert to brown soil, and observed its cunning movements there when alarmed, can for a moment doubt the value of colour as a protection to the higher animals.

The remarks by Mr. E. S. Morse in NATURE of last week bring to my recollection a good instance (among invertebrates) which occurs on the reddish granite of Cobo Bay, Guernsey. There *Trochus lineatus* especially abounds on the bare parts of the rocks between tide-marks; and every observer must be at once struck by the remarkable fitness of the mollusk for its peculiar site.

Mr. Darwin in truth says,* "It would not, for instance, occur to any one that the perfect transparency of the Medusæ or jelly-fishes, was of the highest service to them as a protection; but when we are reminded by Hæckel that not only the Medusæ, but many floating mollusca, crustaceans, and even small oceanic fishes, partake of this same glass-like structure, we can hardly doubt that they thus escape the notice of pelagic birds and other enemies;" but he makes no mention of the gorgeous colouring of some of these swimming jellies, nor is there any allusion to their remarkable property of phosphorescence. The transparency of the British Salpæ does not prevent their being attacked by sea-birds, which hover in multitudes over them, masses of Medusæ and other Hydrozoa, and a few minute fishes.

If instead of promulgating the visionary idea that the abysses of the ocean depended for their light on phosphorescent animals, the dredger† in the *Porcupine* had applied the notion that the various luminous marine animals used their light to attract each other, so that the most luminous might have a better chance of continuing the race, they would have been able to say more in its favour, without, at least, running counter to established facts.

Murthly, March 26

W. C. MCINTOSH

The Aurora of February 4 ‡

AN aurora of a very unusual splendour for the latitude was seen here on Sunday evening February 4, 1872. The sky, extending in azimuth over 197° from N.E. to nearly W.S.W., was generally illuminated. The brilliance of the glow varied considerably in different directions from time to time during the night. On the south horizon there was a bright bluish segment of light, whose position in azimuth and brilliance varied slightly from time to time. The streamers were well seen, and their convergence towards the point to which the south pole of a magnet is directed could be most distinctly traced. The streamers extended at about nine o'clock to the constellation Orion, and Sirius was well within the auroral glow. With a spectroscope I saw one bright line in the spectrum of the auroral light, but the spectrum was too faint to allow of any successful attempt to determine the refrangibility of the light. Unfortunately our magnetical equipment is such that I can give no information respecting the extent of the magnetical disturbance at the time. The aurora was seen as far north as Bloemfontein, latitude 29° 8' south. A faint aurora was seen here in October 1870, but no such aurora as that of February 4, 1872, appears to have been visible for at least fifty years. The aurora was well seen over a large portion of the colony, and considerably frightened the natives.

E. J. STONE

Royal Observatory, Cape of Good Hope, Feb. 19

* "Descent of Man," vol. i., p. 322.

† Not, however, Mr. Jeffreys

‡ Communicated by the Astronomer Royal.