

of the ocean were such as is generally supposed. Such physical questions are fully discussed in the Report to the Royal Society of the late exploring expedition in H.M.S. 'Porcupine.'

- A. rosea*, Les. Voyage sur la Bonite, ii. 377, pl. 19. f. 16-20. Med. 310 f.; Æg. 100-250 f.  
*Ladas Keraudreni*, Les. Ph. ii. 205. = *L. planorboïdes*, Forb. Æg. 130 f.

#### PTEROPODA.

- Embolus rostralis*, Souleyet, Voy. Bon. ii. 216, pl. 13. f. 1-10 (*Spirialis*); B. C. v. 114. = ? *Bellerophina minuta*, Forb. Med. 310 f.; Æg. 100-250 f.  
*Spirialis retroversus*, Flem. B. C. v. 115, pl. 98. f. 4. Med. 310 f.; P. 25-173 f.  
*S. bulimoïdes*, Eydoux & Soul. Rev. Zool. (1840) 138; Voy. Bon. ii. 224, pl. 13. f. 35-42. Æg. 100 f.; P. 664 f.  
*S. physoïdes*, Forb. B. A. Rep. (1843), 186 (*Peracle*). Æg. 100 f.  
*Cavolina tridentata*, Forskål. Ph. i. 101, and ii. 70. Med. 50-100 f.; Æg. 130 f.  
*C. inflexa*, Les. Ph. i. 101, pl. 6. f. 18 (*Hyalæa uncinata*), and ii. 71 (*H. vaginella*). Med. 310 f.; Æg. 100-130 f.  
*C. pyramidata*, Browne. B. C. v. 119, pl. 98. f. 6. Med. 40-310 f.; Æg. 130-250 f.; P. 110-1380 f.  
*C. subulata*, Quoy & Gaimard. Ph. ii. 72 (*Cleodora spinifera*). Med. 40-310 f.; Æg. 100-250 f.  
*C. striata*, Rang. Ph. ii. 72 (*Cleodora*). = *Cleodora zonata* (D. Ch.), Ph. Æg. 250 f.

#### VI.—Professor Hæckel and Mr. Kent on the Zoological Affinities of the Sponges. By E. RAY LANKESTER, B.A. Oxon.

It is scarcely right that the criticisms of my friend Mr. Kent should be the only response called forth in this country by the admirable paper of Hæckel, in which he proposes to give the sponges an intelligible position in the classification of organisms. Since others who share Hæckel's views have not pointed out the destructible nature of Mr. Kent's position, and since he, by the excellent work which he has been doing in coral-studies, may be judged to have brought as strong a case against Hæckel as can be got up on that side of the question, I venture to state in what his objections appear to be unfounded. In the first place, Mr. Kent commences by admitting one of the chief points which Hæckel contends for,

viz. the descent of the sponges and corals from a common ancestor, *Protascus*; and he also admits the descent of the Calcispongiæ from the stock-form *Prosyicum* (Häck.), derived, but not very far removed, from the ancestral *Protascus*; then, stating that the corals had reached "the very zenith" of their development before the close of the Silurian epoch (a statement which is very far indeed from being supported by the comparison of Silurian with recent corals), he says, whatever may have been the affinities of sponges and corals in past times, the two groups, as they *now* exist, exhibit affinities of analogy only rather than homology. This is a most extraordinary view; for if the sponges and corals inherited a common plan of structure from a common ancestor, no length of time modifying the functions of the parts of that common plan could destroy the homology\* of the two; and what we should expect to find, and do find, are affinities of homology and but small affinities of analogy. The chief fallacy of Mr. Kent in his criticism upon Hæckel is in mistaking the value of analogical and homological relations, and, indeed, in taking the one for the other. Thus Mr. Kent lays great stress on the food of sponges being taken in by ciliary action, whilst corals use muscular power, and are provided with contractile prolongations of the body-wall to seize food; but this is a mere difference of the function of common parts, and has no homological import. No one thinks of separating certain oligochaet worms, such as *Eolosoma*, which take in their food by ciliary action, from the others which use a powerfully developed suctorial pharynx; nor of detaching the Chaetopods with prehensile jaws in their pharynges from those which have none. Mr. Kent's statements with regard to the *voluntary* action of muscular tissue and the *involuntary* action of cilia, and the consequent psychical distinction between sponges and corals, are worthy of remark, because, in the first place, if the distinction be allowed, we have to admit a totally novel differentiating character. Mr. Kent ventures to say that Hæckel associates sponges with corals by looking to analogical rather than homological affinities,—and then actually proposes to distinguish them by psychical manifestations. Can any thing be further from homological argument than that which he here uses? Moreover the voluntary nature of muscular action and the involuntary nature of ciliary action cannot be admitted on any terms. It is useless to import the term "voluntary" into the discussion; but it is true that where a nervous system

\* In another paper (p. 34) I have discussed the signification of the term homology in evolutionary zoology, and have proposed to replace it by the term "*homogeny*," in the particular sense which is above implied.

influences animal movements, it usually operates on muscular tissue, though there are cases (among the Vermes) where the nervous system does directly influence ciliary movement; and in the case of the pigment-corpules of the frog's skin we have an example of its influencing the amœboid as distinguished from the ciliary movements of simple protoplasm. The close relation of ciliary and amœboid movements and the gradation, therefore, from cilia to muscular fibre, and consequent insignificance of a distinction based on the substitution of one of these forms of motion for another, has been demonstrated by Professor Hæckel's observation of the development of cilia in the embryo of *Siphonophora*, also in Sponges and in certain Monera, on the surface of masses of protoplasm which, as he saw, threw out numerous short pseudopodia, and these pseudopodia gradually assumed the form and movements of cilia. The ciliate Infusoria may certainly be cited as examples of ciliary movements exhibiting a so-called "voluntary" character; and conversely it is not necessary to cite to Mr. Kent the host of examples of involuntary muscular movement, from that of an isolated muscular fibre onwards. So much with regard to a diagnosis which Mr. Kent thinks "may be regarded as of the highest importance." Next, in relation to the differences between the Sponges' canal-system and that of Actinozoa, we find Mr. Kent simply directing his arguments to prove a difference of function between the two, and then turning to us and asking us to admit a great homological distinction between them. The existence of inhalant pores in Sponges and their absence in Actinozoa is the only point of homological distinction of any note which Mr. Kent can find; and this is rendered less broad by the temporary character of the pores in some Calcispongiæ and their actual existence in some Actinozoa. It is simply nothing to the point that there is a current setting one way in Sponges and another way (or no current at all) in Actinozoa, and has no reference to homology, as Mr. Kent must see on reflection. The development of the osculum into an aperture for the inception of masses of food is quite conceivable, especially when we look at such a sponge as *Prosycum*. We must also remember that the currents directed by cilia in the Sponges and the contractile organs round the mouth of Actinozoa are special developments gradually attained by these two diverging stocks which their common parent possessed but in general outline\*.

A second source of error in Mr. Kent's conclusions is that

\* Dr. Richard Greef has this year described, in 'Kölliker u. Siebold's Zeitschrift,' a very simple form of marine hydroid (*Protohydra Leuckarti*) quite devoid of tentacles or tentacular processes.

he persistently compares extreme forms, and demands an agreement in details where a general agreement is sufficient. This is not the way to investigate or to form a judgment of the relationship of two groups: the points of contact must be looked at; and the outlying diverging members of the two groups compared really do not demand attention, tending as they do to mislead the judgment by their extreme contrast. Thus, were we not well acquainted with a great mass of intermediate forms, we should have great difficulty in enclosing *Amphioxus* and Man in one group. And, to take another case, the sponges and corals present no greater chasm between any two forms than is presented by the Chætopod *Aphrodite* and a *Tænia*-proglottid. No one hesitates to associate the Calcispongiæ with the other sponges; and if it can be shown that the Calcispongiæ are more or less closely allied to some of the Cœlenterata, that demonstration is sufficient *so far* to connect the whole of the sponges and the whole of the Cœlenterata.

A third objection which may be urged against Mr. Kent is the evidence of conservatism (which, indeed, I did not look for in him) and of an undue regard for authority which his paper exhibits, and which does not belong to the spirit of science. The five subkingdoms which have succeeded to Cuvier's four are not ultimate natural facts; they may be doubted, questioned, improved upon, and were two years since almost abandoned by Professor Huxley, who once thought well of them. Change is the method of progress; and if there is a disorderly lumber-room which demands a change more pressingly than any other, it is the Protozoa, embracing as it is sometimes made to do Rhizopoda, Sponges, and Infusoria. It matters very little that Professor Hæckel is "entirely upsetting the clear limits by which a subkingdom is marked out and subdivided with the mutual consent of the most eminent naturalists of the day." The eminence of the naturalists in question has small concern for us if Professor Hæckel can establish his arguments; and it remains to be seen whether the most eminent naturalists will not rather join Professor Hæckel in his work of upsetting.

Fourthly, the clearest evidence of weakness in Mr. Kent's case against Hæckel is his assertion that the only thing to be done with the Sponges in classification is to place them in a subkingdom designated Protozoa, in company, we suppose (for Mr. Kent does not say what are the limits of his subkingdom), with Monera, Amœboidea, Foraminifera, Gregarinæ, Radiolaria, and Infusoria. Mr. Kent says that if we view the composite sponge-organism as an aggregation of Amœbæ (or, as Mr. James-Clark would say, as far as the Calcispongiæ are concerned, of flagellate Monades), "the affinity of the Spon-

giadæ to the Protozoa rather than to the Cœlenterata makes itself eminently conspicuous;" and at the same time their points of agreement with Cœlenterata, which he enumerates, "entitle them," he says, "in a natural and morphological system of classification, to be ranked as the highest representatives of the Protozoa." Why, "viewed in this light," the Cœlenterata themselves exhibit greater agreement with the Protozoa than with themselves as usually viewed! And it is not difficult thus to view all organisms as Protozoa, since the common descent of organic beings from unicellular forms is exhibited in all by a more or less cellular structure, many of the cells in all cases agreeing closely with certain free-living amœboid and flagellate forms. The histological differentiation of the Spongiadæ is not so great as in many Cœlenterata\*; but it is still carried so far that it would be as justifiable to class a *Hydra* as a compound Protozoon as to place a calcareous sponge in that position.

Mr. Kent's relegation of the sponges to the Protozoa will probably not be agreed to by those who may still question as to what is to be done with them, and hesitate about their affinities with Cœlenterata. There was by no means a rigid alternative, as Mr. Kent seems to have thought; he could have left the Sponges alone, neither grouped as Protozoa nor as Cœlenterata. Professor Huxley long since declared the Protozoa to be as unnaturally heterogeneous a series as Cuvier's Radiata; and two years since, at the College of Surgeons, whilst removing the Infusoria to approximate them to the Vermes, he indicated the Sponges as connected with the ancestry of the Cœlenterata, though he did not definitely part them from their old associates in the lumber-room. That the Sponges cannot rightly be left in this position, must be conceded by all who will look into the question. There is a really enormous gap between Sponges and those Protozoa which come nearest to them. The simplest sponges are aggregates of the second order (that is, aggregates of such units as an *Amœba* or a *Monas*), as are also Mollusks, many Cœlenterates, many Worms, and other forms. The compound Radiolaria and Foraminifera offer also examples of secondary aggregation among Protozoa; but on comparing their state of aggregation with that of the sponge, we perceive at once a vast difference. The first is a simple result of growth producing an accumulation of like parts, which remain attached, but might just as

\* What Mr. Kent calls an essentially Protozoic property in sponges (as to the temporary character of certain pores) is the property of sarcodic tissue in all animals; and sponges are largely sarcodic; but this does not make them Protozoic.

well be separated as far as their well-being is concerned. In the Sponge, on the contrary, a new principle is detected at work : the aggregation is arranged so that the parts may work for the life of the whole ; there is *differentiation* and *individualization* ; and this process is carried to a very high degree ; so that we have endoderm and ectoderm, fibrous tissue, sarcodine, spicular layers, antimera, ciliated tracts, a canal-system with pores and osculum, and such complete highly individualized forms as *Euplectella* and *Grantia*. There is not the slightest trace of an approach to this kind of thing among any Protozoa, certainly not among Mr. James-Clark's interesting forms of Flagellata, which he assimilates to ciliate sponges. It is the particular *kind* of individuality seen in sponges which removes them so far from their quondam associates, and demands consideration as to whether it is a plan of individualization peculiar to the group, and entitling them to a solitary position, or whether other organisms present the same features of structure. At the same time, be it remarked, we may look for forms connecting Sponges to some of the Protista ; since the course of development has not improbably left some examples belonging to a stage of such an intermediate character. Supposing such forms to be recognized, they must not influence our judgment as to the significance of the high development of the more elaborately organized Sponges.

The probable result of Professor Hæckel's arguments with most naturalists will be, in the first place, to cause them to admit the separation of Sponges from what are generally known as Protozoa. He will have greater difficulty in getting the association with Cœlenterata, and with Anthozoa in particular, admitted.

In preceding paragraphs it has been pointed out that the function of the mouth, cavity, and canal-system does not affect their homology or homogeny ; and in the face of such forms as *Prosyceum*, and the clearly demonstrable ectoderm and endoderm and antimera of some forms and the general community of plan which Sponges and Corals exhibit (as even Mr. Kent allows in assenting to *Protascus*), it is not possible to maintain that the canal-systems of sponges and Cœlenterata are two unrelated structures having no common genetic origin. The weight of evidence goes very much in favour of a common origin for the two sets of canals when impartially examined. The possibility of the two systems being nothing more than homoplastic has to be weighed in considering this matter. A greater mass of evidence will certainly be welcome in the way of clearing up doubt as to the possible independence of these two systems ; but Professor Hæckel's facts go a very long

way. The degree of intimacy in which Sponges are to be associated with Actinozoa, Hydrozoa, and Ctenophora is a question which Hæckel does not attempt to decide as yet; and here Mr. Kent has unintentionally done the brilliant Professor of Jena an injustice. He does not definitely propose, as Mr. Kent represents, to group Sponges with Corals as *Thamnoda*, at the same time placing Hydromedusæ and Ctenophora in an equivalent group *Medusæ*; this he merely offers as a suggestion of the direction which affairs may have to take on account of the closer affinities of Sponges with Corals than with Hydroids\*. He is more inclined at present to the separation of Cœlenterata into these two classes, Spongiæ and Acalephæ (or Cnidæ or Nematophora of Huxley), the latter including the Actinozoa, Hydrozoa, and Ctenophora—an excellent arrangement, which Hæckel does not finally adopt because urticating cells are to be found in some worms and mollusks. The urticating cells of worms and mollusks, however, are by no means such remarkable and characteristic organs as those of Nematophora. Whilst it is desirable to associate sponges with the Cœlenterata, the fact that they *diverge* from a common stock must not be forgotten if we wish to exhibit their relations in a true genetic classification; and as yet, I venture to think, the two groups cannot be placed much nearer than within the limits of a large division: the higher Nematophora are not more closely “related in blood” to the Sponges than is *Aphrodite* to the Cestoids, or the Mammalia to *Amphioxus*, allowance being made in the comparison for the increased complexity of structure of the Worms and Vertebrates.

Professor P. J. Van Beneden of Louvain long since expressed very much the same opinion as to the nature of Sponges as that now advocated by Hæckel, and previously to him by Leuckart—that is, so far as affinities with the Cœlenterata generally are concerned. Professor Van Beneden, in his work ‘*Zoologie médicale*’ (Paris, 1859, t. ii. p. 394), written in conjunction with Professor Gervais, said of the Sponge, “C’est l’animal du type polype réduit à sa plus simple expression.”

\* Through such a form as *Protohydra* it is more easy, if we only look to recent forms, to imagine a connexion between Sponges and Cœlenterates; but we must allow that in past time there have existed very simple Anthozoa also, which are not known to us now.