

Lebensweise, als auch in vielen morphologischen Characteren. *Rh. simus* ist ein Steppenbewohner, und seine Lippenbildung, welche an die des Pferdes erinnert und ohne fingerförmigen Fortsatz der Oberlippe ist, weist auf vorwiegende Grasnahrung hin. Auch *Rh. tichorhinus* war nach meiner Ansicht vorwiegend ein Steppenbewohner oder doch ein Bewohner waldarmer Gebiete<sup>5</sup>, und seine Lippen scheinen dieselbe Bildung gehabt zu haben, wie die des *Rh. simus*. Leopold von Schrenck hat zwar denjenigen fossilen, sibirischen *Rhinoceros*-Kopf, der eine dem *Rh. simus* entsprechende Lippenbildung zeigt, auf *Rh. Merckii* bezogen<sup>6</sup>; aber dieser Kopf gehört sehr wahrscheinlich zu *Rh. tichorhinus*, nicht zu *Rh. Merckii*. Eine Untersuchung der zugehörigen Backenzähne, welche Schrenck nicht vorgenommen hat, würde hierüber leicht entscheiden.

Zum Schluß möchte ich der Hoffnung Ausdruck geben, daß *Rh. simus* im Interesse der Wissenschaft noch recht lange vor der völligen Ausrottung bewahrt bleibe. Man sollte in den betr. Gebieten möglichst strenge Vorschriften zur Schonung dieser interessanten Art durchführen.

## 2. Professor Roule upon the Phoronidea.

A Reply by Dr. A. T. Masterman.

eingeg. 11. März 1901.

In No. 621 of the Anzeiger, Professor Roule has had occasion to refer to my recent work upon the early development of *Phoronis*. I have naturally abstained from making any reply till I had an opportunity of perusing Professor Roule's full paper. Indeed, considering the manner of criticism he has seen fit to adopt I would not have replied at all, but that my silence might be misconstrued. I must protest against the attitude assumed by Professor Roule in adopting the argument that, because he does not find such and such of my results in his larvae, that therefore my work is worthless and that "c'est impossible d'accorder la moindre créance aux études récentes de Masterman". These are strong terms to apply to anyone's work, and are not in the least justified by Professor Roule's results upon another species. In truth, he accepts, in one part of his work, the correct attitude of merely recording the disagreements; but again and again he resorts to this false inference, that I must have exaggerated or used badly preserved material etc., because he has not found the same results. His

<sup>5</sup> Siehe mein Buch »über Tundren und Steppen«, Berlin, 1890. p. 137.

<sup>6</sup> L. v. Schrenck, Der erste Fund einer Leiche von *Rhinoc. Merckii*, St. Petersburg 1880.

inability to do so may be due to one of two causes: it may be due to disability on the part of Professor Roule, or it may be due to disability on the part of the larva. As a matter of fact it is probably a little of both, for I do not think the Professor has used the best methods (see ultra), and there is also no question that the larva of *P. Sabatieri* is an abbreviated larva compared with mine.

However, there are three special points upon which I wish to touch:

- 1) The development of the mesoderm,
- 2) The structure of the adult larva,
- 3) Professor Roule's phylogenetic conclusions.

- 1) The development of the mesoderm.

All the observers before Caldwell employed the method of optical sections and they mostly agreed in the so-called 'mesenchymatous' origin of the mesoderm, i. e. that the first indication was the appearance in the blastocoele of isolated masses of mesoderm. Caldwell applied the method of serial sections, and although he apparently made some error regarding the posterior diverticulum, he saw and figured paired localized cell-proliferations in the entoderm, which grew into the blastocoele to form mesoderm. This I corroborated, and also gave some tentative evidence for a similar origin to a posterior pair, and an anterior unpaired part.

Professor Roule reverts to the old 'mesenchyme' notion, and we might at least have expected some fresh evidence, but none is given. He does not figure a single true section until a stage long past the origin of the mesoderm. Nothing is given except a few optical sections, which as such must be more or less diagrammatic. They merely show a few loose cells, which may have come from anywhere, lying in the blastocoele. He has not seen fit to furnish us with a single microtome-section showing the origin of these cells, though he argues in the text that they originate from the mesoderm. If he has sections illustrating the origin of these cells, why does he waste two or three plates with optical sections which shew nothing more, but rather less, than his predecessors of twenty years ago. And it is for evidence such as this that my researches are not to have 'la moindre créance'! It is almost incredible that Professor Roule should attempt in 1900 to prove the origin of the mesoderm in a much controverted case by a series of optical sections which do not exhibit a single approach to a cell-division. In my paper I drew a close comparison between the method of mesoderm formation in *Tornaria* (according to Morgan) and that of *Actinotrocha*. Neither has a true enterocoelic formation, but a modified

type. If Professor Roule had studied *Tornaria*, by optical sections only, I have no doubt he (or any other investigator under similar conditions) would have claimed a mesenchymatous origin for the mesoderm, and, who knows, might have been led to deny the existence of body-cavities in *Balanoglossus*. The only real distinction between the two methods of mesodermic origin is that of localized ingrowths and diffused ingrowths. If the evidence of microtome-sections is not to be preferred, in demonstrating cellchanges in an epithelium inside an organism, to the method of optical observation of the entire animal, then I have nothing more to say in the matter.

## 2) The structure of the larva.

Here Professor Roule disagrees with me in many essential points. *Actinotrocha* is to him a trochophore, hence it cannot have the organs described by me. Professor Roule's *Actinotrocha* has, according to him, no organs of the kind; hence mine has them not. Is Professor Roule going to deny the existence of the ascidian notochord because some ascidians do not have a tailed larva? Surely this is one of the most extraordinary lines of criticism to adopt. The *Actinotrocha* with which I have been working has mesenteries separating the parts of the coelom which with a little practice can be recognized with the naked eye. The presence of the lower mesentery has been described by others before me, and the front mesentery was figured in optical sections by Wagener (in 1847), though he took it for a nerve-cord.

In connexion with these mesenteries we may mention one point. The Professor seeks refuge in "bad preservation" to account for my finding a nerve-ganglion, and in shrinkage for producing the Epidermistasche and subneural glands; these are plausible although not true, but I do not think that either of these could in any circumstances account for my mesenteries. It is worth while to record, however, that I have very successfully reduced the mesoderm of some *Actinotrochae* to Professor Roule's condition. The most efficacious method appears to be a sudden change from sea-water either to fresh water or to absolute alcohol. Rapid rotation and contortion of the larva results and as a rule, the sections shew a remarkable approximation to the figures of Professor Roule. I do not wish to imply that he has pursued any so crude method, but I desire to emphasize the fact that mesenteries may disappear under treatment, but that they can hardly be created where none such existed.

The four years that have elapsed since my paper on *Actinotrocha* have led me to modify some of my theoretical conclusions, and an anatomical detail in *Cephalodiscus* which I have corrected in the An-

zeiger, but I have no reason to doubt the accuracy of any of the structural observations. The term Epidermistasche may well be substituted for the neuropore, as it really is a groove between preoral hood and collar. I have never claimed for it that it passes into the interior of the nerve-cord.

The pre-oral pores appear to be subject to considerable variations. Many specimens, although full-grown, do not have them, and some have only one, hence I am quite prepared for the denial of their existence by some superficial worker.

With these reservations I adhere to the account as published in 1896.

Professor Roule objects that I had, at the time of his writing, only examined the fully-developed larva, which is perfectly true, and also that this has led me "à des assertions erronées, à des exagérations, qu'il aurait sûrement évitées, s'il s'était conformé à la bonne methode de l'embryologie" (p. 147), — which, with all due respect to Professor Roule, is untrue, and is not a statement within the legitimate bounds of scientific criticism from one who has not examined the same larva.

I can only take a single instance of the kind of conclusions to which Professor Roule's work leads him. I described in *Actinotrocha* a vascular system of sinuses which were in a very primitive condition but with perfectly definite relationships. In this I merely amplified the statements of Wagener, Schneider, Metschnikoff, Leuckart, and Pagenstecher, Krohn, Wilson and Caldwell. In particular, I described a dorsal vessel with contractile walls, and I have since watched the contractions of the vessel in the living animal. Caldwell especially describes this vessel as a marked structure in the larva (a Mediterranean species, by the way; apparently *P. Kowalevskii*), and Schneider (1862) emphasizes the rhythmical contractions of this vessel. In the face of all this evidence the learned Professor finds in his larva no trace of a dorsal vessel, but in its place a "cordon dorsal" of cells which becomes the intestine of the adult! Should not this important discrepancy (one instance out of a dozen or more) teach the Professor to hesitate before being quite so free with his accusations of exaggerations and erroneous assertions? There is here a fundamental disagreement between the results of the Professor and those of seven or eight workers, the great majority of whom, we may assume, were not, as in my case (according to Professor Roule) biassed by an inordinate "désir de comparer l'actinotroche aux Bryozoaires Ptéro-branches et aux Entéropneustes", and were in many cases working on the Mediterranean species. The discrepancy must again be due to the deficiency of his larva or to his own observations and methods, but I



should not feel justified in giving him a *tu quoque*, and accusing him of erroneous assertions and exaggerations.

The same remarks apply in general to nearly all the structural features of the larva. The nerve-ganglion, the lower mesenteries, the two pleurochords, had all been noticed to a greater or less extent by observers of such a standing as Metschnikoff, Kowalevsky, and the others named. Whilst my work falls in to line with such authorities, it is a matter of minor importance that it does not agree with that of Professor Roule.

The criticism, that I ought to have followed the early stages, has also been made by others equally ignorant of the facts. The adult *Phoronis* does not occur in St. Andrews Bay, and I question if it would have been justifiable to withhold my paper for some years till an opportunity presented itself of following the young stages. Further, if the mesenchymatous origin of the mesoderm were proved I cannot see that it would make the least difference to the conclusions drawn from the structure of *Actinotrocha*, any more than the different origin of mesoderm in *Amphioxus* and *Tunicata* keeps these apart. In other words, the conclusion drawn from the structural identity of *Actinotrocha* and the *Hemichorda* could not be allowed to depend solely upon the origin of the mesoderm.

### 3) Phylogenetic conclusions.

But though Professor Roule's observations are sufficiently startling, they are far excelled by his inferences. Briefly, he places *Phoronis* beside *Cephalodiscus*, whilst flatly denying the existence of all the fundamental resemblances which caused me to make a similar approximation. Further, he denies that there is any real resemblance between *Phoronis* and *Balanoglossus*, apparently unaware that it was the structural resemblance between these two, as described (Proc. Royal Society Edinb. March 1895), that led me to suggest its alliance with the *Hemichorda*, and to search for a chordeid structure in the larva. But this is not the end. The mouth of *Phoronis* = a blastopore; the anus of *Balanoglossus* = a blastopore; therefore the mouth of *Phoronis* = the anus of *Balanoglossus*!

As the Professor says truly — “La concordance est complète. L'homologie est manifeste!”

Further, *Phoronis* is to be compared to *Balanoglossus* turned upside down. As *Phoronis* is comparable, part for part, with *Pterobranchia*, to get the true relationship between *Cephalodiscus* and *Balanoglossus* we must turn one round and over. The proboscis of *Balanoglossus* has to be sought for in the trunk of *Cephalodiscus*, the

notochord of *Balanoglossus* must be looked for near the anus of *Cephalodiscus*; and so on!

Still the Professor leads us on like a conjuror. The Vertebrata themselves are in the same plight as *Balanoglossus*. We are all Trochophores turned round and turned over — 'L'embryon du Vertébré est un Trochophore renversé'. Can any zoologist venture to contradict such a patent hypothesis? My own impression in reading these conclusions of Professor Roule was that they are of more real service to Science than all the pages preceding them. For if this sort of thing will not serve to put an end to the copious stream of theories of vertebrate origin, involving morphological somersaults and structural contortions, nothing will.

The conclusion of the whole matter appears to be this:

1) That Professor Roule denies entirely the method of mesoderm formation in *Actinotrocha* by localized hypoblastic ingrowths, as described by me. He does this on the strength of his observations upon another species, without having furnished the slightest direct evidence for the origin suggested by himself.

2) That he sweepingly condemns my assertions regarding the structural features of the late larva found at St. Andrews (and with them, those of nearly every other worker who has preceded him, upon Mediterranean larvae), because he has failed to find the same features in the species investigated by him.

3) That, based upon the archaic notion of the homology of the blastopore, he has been led to conclusions regarding the relationship of *Cephalodiscus* to *Balanoglossus*, and of Invertebrates to Vertebrates, which border on the ludicrous.

4) That he has seen fit in his criticism to accuse me of erroneous assertions and exaggerations, and to use other expressions of a very strong nature.

I only ask: — Are the first three 'Science', and is the last in good taste?

Edinburgh, School of Medicine, March 1901.

### 3. On the Composition and Variations of the Pelvic Plexus in *Acanthias vulgaris*.

By R. C. Punnett, B.A.  
(Royal Soc. London, Abstract.)

eingeg. 14. März 1901.

The facts recorded in this paper may be summarised as follows:

1) Considerable variation occurs in *Acanthias vulgaris* with regard to:

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