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II. Wissenschaftliche Mittheilungen.

1. On the Eyes of Molluscs and Arthropods.

By W. Patten, Milwaukee, Wisc.

eingeg. 16. März 1887.

In the last number of the Quarterly Journal of Microscopical Science appears a »review« of my paper on the Eyes of Molluscs and Arthropods.

I have no desire to enter into any controversy with my critic about my laboratory associates in Europe, or to discuss with him matters of a personal nature in the pages of a scientific journal; nor is it my purpose to defend either my observations or my theoretical suggestions, but simply to indicate a few points in which my critic has failed to apprehend or represent clearly my position.

1) The larger portion of my observations, embracing discoveries which served as a guide in my studies upon the Arthropod eye, are

deliberately passed over without a word of comment, while nearly the whole critique is devoted to the theoretical part of my paper.

On p. 543, I warned the reader at considerable length not to regard the statement of my theoretical views as dogmatic assertions, explaining that while conscious of the uncertain ground upon which I trod, I desired to make my statements positive that the reader might clearly apprehend my interpretation of the facts. Moreover on p. 705, I took pains to say that the theoretical remarks of Chapter VI must be regarded as »suggestions« for which I could bring no proof. Had Prof. Lankester read these statements, I can hardly believe that he would have accused me of »laying down the law in a presumptuous manner« or of »making dogmatic statements apparently unconscious of my inability to prove them«.

2) On p. 290, Prof. Lankester quotes the following from my paper: »We must admit that the possibility of regarding the phaosphere, found in Euscorpius Italicus by Lankester, as an aborted nucleus is not so remote as he would have us believe.« Lankester then adds: »Whether the phaosphere can possibly be an aborted nucleus, or not, may be an open question. It is but another instance of Patten's extraordinary inaccuracy when he states that Lankester would have us believe anything on the subject. The matter was not discussed by Lankester at all.« Whether Prof. Lankester would have us believe anything on the subject, or not, may be seen from his' own words. On p. 156 of his paper on the eyes of Scorpions, we find the following statement: »At the same time it is to be observed that the axial rod of the Spider's nerve-end cell must be considered as representing not only the phaosphere, but also the laterally placed rhabdomere of the nerve-end cells of the Scorpion's eye.«

3) In my paper, I referred to the five-fold colorless cells, or retinophorae, of the lateral eyes of Scorpions. Prof. Lankester says of this statement: »Patten quite recklessly attributes to other authorities on the Arthropod eye statements with regard to the presence or absence of pigment in the nerve-end cells which are the reverse of those made by the gentlemen in question.« Graber's name was mentioned to give him credit for having discovered the five-fold nature of the rod-bearing cells. The »nerve-end« cells of the central eyes were described and figured diagrammatically by Lankester as colorless, while he states that in the lateral eyes the pigment is confined to the surface of the »nerve-end« cells. My statement would have been entirely correct had I spoken of the colorless cells of the median eyes as described by Lankester. Prof. Lankester, in the fairminded spirit

characteristic of his whole criticism, has magnified the evident interchange of the word lateral for median into a gigantic mistake.

4) Prof. Lankester, in commenting on my statement that »the amount of energy absorbed by a heliophagous organ would depend upon the most perfect condensation of light upon a given area«, insinuates that I had no time to learn from some text-book the self evident fact that a lens can concentrate only those rays of light which fall upon its surface! Therefore, he adds, there is no necessity for a lens, according to my supposition, for a naked surface equal to that of a lens would absorb more light than would a retina with a lens in front of it. If my observations on the arrangement of nerve fibres are correct, it follows that it is necessary to regulate the direction of the rays of light; the lens is an important factor in bringing this result about. Moreover Prof. Lankester's objection rests upon the supposition that the surface of the retina, is as large or larger than that of the lens. There are some cases, however, in which the surface of the retina is considerably smaller than the surface of the lens.

Prof. Lankester declares that a »naked epidermic surface of an area equal to that of a lens would present a perfect instrument for the absorption of solar energy«. I have mentioned in my paper upon the »Eyes of Molluscs and Arthropods« numerous instances of just such »perfect instruments« as he describes.

5) On p. 291, Prof. Lankester describes another »melancholy instance« of Dr. Patten's unwarrantable adhesion to theory in the face of opposing facts«, in supposing that the lateral eyes of Scorpion and Limulus were provided with a vitreous body. I had good reasons for making that supposition, and in the case of Scorpions I had the authority of Graber as opposed to that of Lankester. It is not strange that my acceptance of Graber's statements should strike Prof. Lankester as a »melancholy« adhesion to theory. Indeed it must appear very »melancholy« now that Bertkau has also seen fit to doubt the accuracy of Prof. Lankester's observations on this point!

6) My criticism of Prof. Lankester's assertion that mesoblastic pigment cells were present in the ommateum of faceted Arthropod eyes, as was expected, met with scanty approval. I stated my objections to Prof. Lankester's hypothesis; in my opinion, his criticism does not in the least invalidate those objections. Prof. Lankester found a few branching pigment cells in the eyes of Scorpions, and concluded that they were of mesodermic origin, and forthwith divided Arthropod eyes into autochromic and exochromic.

Prof. Lankester now seeks to support his supposition that mesodermic pigment is present in the ommateum of the compound

eye, by appealing to the observations of Kingsley, who, he claims, has actually seen the intrusion of mesodermic tissue into the eye. We find, on consulting Kingsley's short paper of about three pages, that he has described nothing of the kind, but clearly shows that the ommateal pigment is of ectodermic origin. We surely should expect that Prof. Lankester, who in language more forcible than courteous speaks of my »incapacity for accurate observation of books« and of »recklessly attributing to other authorities statements which are the reverse of those made by the gentlemen in question«, would at least avoid those errors which he criticises so severely in others.

7) On p. 289, Prof. Lankester states that one of the most important lines of inquiry into the minute study of Invertebrate eyes is to be found in a determination of the distribution of pigment in the retinal cells, and adds that »Dr. Patten has not given as much attention to this matter as we could wish. It is remarkable that while he indulges in such ‚tall talk' about pigment and heliophagy he professes to have traced the chief optic nerve fibres of Arthropod eyes to the colorless cells«. The reader will be surprised to learn that the question of the presence of pigment in the retinal cells of Molluscs was the theme of my study upon the Molluscan eyes, and the one which furnished me with the most important results of my paper. Prof. Lankester would not have considered it »remarkable« that the »nerve-end« cells of the Arthropod eye were colorless, if he had taken pains to read that part of my paper devoted to Molluscs. I have shown that in the simplest Molluscan eyes most of the retinal cells, if not all, were pigmented, while in the higher forms the true sensitive elements were colorless; therefore the presence of colorless »nerve-end« cells in the Arthropod eye is exactly what we should expect. I, moreover, explained at length, and illustrated by numerous diagrams, the various steps in the phylogenetic development of the colorless cells and the degeneration of the pigmented ones, throughout the Molluscan and Arthropod groups.

8) Prof. Lankester, while commenting upon my observations of intercellular nerves in Molluscs, asserts the well known fact that such nerves have also been observed in Vertebrates. But, he adds, as though it were in conflict with my observations, it is also very generally held that, in organs of special sense, the nerve fibres terminate in the substance of special nerve-end cells. It is a sufficient comment on Prof. Lankester's criticism, that I have repeatedly regarded the presence of such intra-cellular nerve fibres as a criterion in determining what the special sense cells of the eyes were.

9) On p. 286, my assertion that nerves end between the cells in

the Molluscan hypodermis is claimed by Prof. Lankester to be contrary to my statements in reference to the Arthropod eye. It is astonishing that one who deplores the inability of other people to observe correctly the contents of books, should show such an imperfect knowledge of the paper he is criticising as Prof. Lankester does in this last remark. There is nothing whatever contradictory in my description of the nerve endings in Molluscs and Arthropods. In fact, the innervation of the Molluscan hypodermis, according to my description, is exactly the same as that in the Arthropod ommateum, a fact which I have explained in the text and illustrated by more than twenty diagrammatic drawings! Had Prof. Lankester understood the composition of the retinophorae, he would not have regarded the presence of an axial nerve in the crystalline cone cells as in any way contradictory to what I have described in Molluscs.

10) Prof. Lankester, in criticising my »treatment« of Grenacher, states: »On p. 728, this young American, after citing an opinion published by Prof. Grenacher, says: ‚This he knows is absurd and cannot be true.'« The facts are these: Prof. Grenacher, in order to illustrate an opinion, compared a row of retinal cells to a line of soldiers. In my opinion this comparison led to an absurdity, i. e. to the supposition that each retinal cell had a rod, a pigmented and a colorless portion. This, I said, »he knows is absurd and cannot be true,« because, as everyone will admit, it is contrary to fact; some cells bear rods and some do not, some are deeply pigmented and others colorless. One, however, is led to infer from Prof. Lankester's criticism, that I said in so many words, that Prof. Grenacher knew his own opinion was absurd and could not be true, whereas my statement referred, not to Prof. Grenacher's opinion, but simply to an inference which I myself drew from his comparison.

11) Prof. Lankester declares that it is Dr. Patten's avowed intention to follow his own course, picking out such facts as suit his theories, and denying the existence of those which do not.« Prof. Lankester goes even farther and asserts that Dr. Patten »openly professes that he has made it his habit in constructing his views upon the structure of eyes« to pick out those facts which »support a favorite theory, or amplify a startling generalization, and ignoring or flatly denying, without troubling to bring them to the only test recognized by loyal students of nature, those which cannot be thus used!«

I did say, in reference to a single case in which the observations of two authors were diametrically opposed: »Since the doctors disagree, it is necessary for us to choose our own

course, picking out those facts which seem to point in the right direction.« Moreover, in that instance, I gave good reasons for finally choosing the course I did. This is the only foundation Prof. Lankester has for bringing against me one of the most serious charges that could be made against a scientific worker.

I have not thought it necessary to consider here all the points noticed in this »review«, but enough has perhaps been said to give the reader a fair idea of its animus.

My reviewer closes his remarks with the assertion that I have shown in my paper »as great a lack of social as of scientific education«, and it is evident that Prof. Lankester, as a »master« and »teacher«, has written for my benefit what he regards as a model criticism.

In conclusion, I beg permission to express my thanks to Prof. Lankester for devoting so many pages of his valuable journal to the discussion of the theoretical suggestions contained in my »ill regulated production«, and also to assure him that, in spite of my »lack of social and scientific education«, I am still able to appreciate these qualities in others. I am, moreover, of the opinion that these very qualities are no less admirable, and no less to be expected, in an experienced and able investigator, than in a »novice«.

Milwaukee, Feb. 28.

2. Zur Abwehr.

Von J. Nusbaum, Warschau.

eingeg. 24. März 1887.

Wiewohl ich bis jetzt niemals in irgend eine Polemik mich eingelassen habe und einen Widerwillen gegen unnütze Streitigkeiten hege, so finde ich mich doch veranlaßt, die gegen mich von Herrn Samuel Grosglik aus Warschau in No. 245 des Zool. Anzeigers (»Enterocoel oder Schizocoel«) in Betreff der Embryologie der Arthropoden angeführten Angriffe mit folgenden Zeilen zurückzuweisen:

Da Herr Grosglik sich selbständig nie weder mit der Anatomie noch mit der Embryologie der Arthropoden beschäftigt hat, und sein Aufsatz in einem nicht wissenschaftlichen Tone gehalten, von Persönlichkeit geleitet ist, und obendrein Herr Grosglik sich Mangel an Gewissenhaftigkeit[1] zu Schulden kommen läßt, so betrachte ich es für überflüssig, mich mit diesem Herrn in eine wissenschaftliche Discussion einzulassen.

[1] Die Art der Zusammenstellung meiner polnischen und russischen Arbeit seitens dieses Herrn ist irreleitend; denn in beiden, die denselben Gegenstand be-