EWART'S INVESTIGATIONS ON ELECTRIC FISHES.¹

THIS is a magnificent memoir containing the very interesting observations of Prof. Ewart concerning some of the most important chapters of comparative anatomy. Everyone who has an idea of the enormous difficulties connected with these investigations will admire the great skill and successful perseverance with which the author has followed up many dark problems and thrown light upon a number of the most obscure questions.

My studies have been in the same direction for nearly twenty years, and I congratulate my companion in work upon his great success.

It might appear a bold attempt for a foreigner to debate the complicated problems treated in the work; but, on the other hand, there is apparently a strong interest attached to the endeavour to enlarge the field of international intercourse, and this will serve as an excuse for any awkwardness of language.

There is no doubt that science is the chapter of knowledge most entitled to international treatment, and Prof. Ewart himself has done his best to acknowledge the merits of foreign authors. Still, I wonder whether he is aware of the fact that in many places his deductions bear a more or less national character. The proof of that fact cannot be compressed in a few notes, but it may suffice to point out the places where the differences of treating these matters between British and continental writers seem to be most apparent.

Everybody will probably agree that the whole of Prof. Ewart's work deserves very high praise. The plates, which have been accurately drawn by the author himself, are beautifully printed, and yield very ample and useful instruction to anyone who wants anatomical and histological information about these interesting, and yet very imperfectly known, organs. They give a clear account of the immense work the author had to accomplish before he could give so exact and complete representations of the electric organs as well as the cranial nerves and the sense organs.

It will remain to Ewart's undisputed credit that he has brought before the public a large amount of information on the anatomy and histology concerned. The explanation of the plates facilitates the understanding of them, and forms the connecting link between the figures and the tenour of the deductions. It is proved by comparing a great number of organs in different species of *Raja*, that there are two distinct kinds of electric organs, viz. "cup-shaped," which occurred in *R. circularis*, *radiata* and *fullonica* of the British seas, in *R. eglanteria* from abroad, and, on the other hand, "disc-shaped," which he found in *R. batis*, *macrorhynchus*, *alba*, *oxyrhynchus*, *maculata*, *clavata*, and *microcellata*.

Everywhere Ewart confirms the statement of former writers, that the electric organs were derived from muscles of the tail which became changed into electric tissue. With great care and skill he has followed the development of these organs in the embryo, and showed how the muscles gave up their firm hold on the sinews, and shrivelled up to discs or cups.

There remains yet one difficulty to be overcome, which the author has not considered; that is to say, he finds the termination of the nerves for each element at the proximal end of each muscular club, and compares them (as other authors do) to the motor end plates of common muscles, which are fixed alongside the striped fibres, and not at the end.

That is, of course, a subordinate question, and I am very glad to repeat here what I have stated in former publications of mine, that the muscular origin of the electric

organ in the skate also appears beyond doubt, and that I thoroughly agree with Prof. Ewart, not only in that principal question, but also in his deductions regarding the phylogenetic development.

One of the general deductions appears rather strange, not only to me, but to most authors on the continent interested in the matter. How is it possible to consider the electric organs of the skate as such of "vestigial character" before any evidence is given in favour of a retrograde development, which takes place at any period of life? Are not all the statements of Ewart, as well as of former authors, clear proofs that the development is progressive, or at least resting at a certain degree of perfection, after having left the starting-point (muscular tissue) only for a comparatively short distance?

The organs might still advance to further perfection (which I presume they do), or they might become rudimentary again; but so long as there is only progressive and no retrograde motion in the development, it is hardly worth while to argue about the probability of their vestigial character.

I differ only so far from Ewart, as he does not convince me that the electric organs of the skate are as equally perfect as the organs of the Torpedo. In the skate, and up to a certain degree also in *Mormyrus*, the striated layer of the organ is histologically and optically (in polarised light) proved to be the rest of the original muscular tissue. If there is so much left of the former state of things, it proves, in my opinion, that the process of transformation going on is not so far perfected as in another case (Torpedo), where nothing of the muscular character is left.

I may be permitted to quote here a suggestion I made in a former number of this journal (January 19, 1893) regarding the probable way of phylogenetic development in these organs. It seems to me possible that a kind of physiological alteration changes certain muscles so gradually into electric tissue that a comparatively still imperfect element under favourable circumstances might give an electric shock, which proves useful to the skate for maiming small animals upon which it preys; and so the fish might continue to improve the organ by using it.

Ewart (*R. circularis*, p. 546) argues exactly in the same way as I did, but hesitates to assume that weak electric shocks might be of any use to the skate. It should be kept in mind that small aquatic animals are often extremely sensitive to electricity, and that an unexpected weak electric shock startles even a human individual. At another place, where the author treats the same theory, and grants the possibility of all the other presumptions, he holds back from the universally accepted principle, that constant use makes an organ increase. (Skate, p. 411).

Of course, up to a certain degree, the phylogenetic development of the electric organs contains still a good deal of mystery, and will, I fear, always lack the scientific proof so eagerly looked for; but I must repeat my conviction, that it is easier to imagine the transformation of striped muscle in electric tissue, than to explain by natural selection the development of any distinct, lively-coloured pattern on the wing of an insect.

The cautiousness of the author is, however, to be praised, and it is very interesting to follow his arguments about the *pro et contra* in these complicated matters.

I cannot admire as much another chapter of his paper, where Prof. Ewart does not seem to be quite up to the international mark; it is that in which he compares the number of electric elements in the different electric fishes. (Skate, p. 397). The total of electric elements for each organ in *Torpedo marmorata* he gives as 250,000, and in spite of the great difference of this number with the sum found by other writers before Ewart, he does not say one

¹ "Electrical and Lateral Sense Organs, and on the Cranial Nerves of Elasmobranchs." By Prof. J. C. Ewart. (Edinburgh, 1893.)

word about that divergence. I spent much time in counting the elements in the organ of Torpedo, and confirmed my results by counting also the ganglionic cells belonging to the plates. My total comes very near to that of Valentin, but amounts only to 179,625 in each organ.

Prof. Ewart cannot expect me to give up my number in favour of his, published much later, before he proves that I made a wrong estimate. He speaks also of the large Torpedo of America, and calls it *Torpedo gigantea*. It seems he is not aware of the fact that the name of *Torpedo gigantea* is given to the petrified species from the Monte Bolca, whilst the American species got the name of *T. occidentalis* (Storer). He ignores, or neglects, at the same time, the fact that a near related species, which has about the same number of electric elements, generally named *T. nobiliana* (Bonap.), occurs also in the British seas.

If it is difficult to explain such want of harmony with other authors; it amounts to an impossibility for a foreigner to give a clear account of the following papers concerning the cranial nerves of Elasmobranch fishes. Not that I mean to blame my learned colleague for this; on the contrary, I admire the papers very much, and recommend them with all my heart to everyone who wants instruction about the finer details of these nerves; but with regard to the nomenclature employed, I am afraid very few continental authors will agree with the homologies stated by Ewart. When he in a certain place says against Sappey, "that nerve is all but universally acknowledged to be a part of the facial," Sappey will most decidedly state "that nerve is all but universally acknowledged to be a part of the trigeminal."

Considering it of little use to discuss the confusion of names in the papers quoted by Ewart, I wish it to be borne in mind that the leading principle of the author to prove his homologies is the equality of distribution of the nerves in the peripheric organs. If that holds good, as I am convinced it does, how can he at the same time give the name of a true motor nerve (*N. facialis*) to a cephalic nerve of a true sensitive character? Perhaps he will answer, "All, or at least most, of the other authors do, why shouldn't I do the same?" Putting aside the protest many continental authors (myself included) make against such nomenclature, at any rate his principle of innervation is given up, and I am firmly convinced that the comedy of errors in the nomination of cranial nerves in comparative anatomy will not cease until quite new names or, perhaps still better, only numbers are applied to them according to the place of origin in the substance of the central nervous system. Motor and sensitive nerves must be kept separated by all means, segmental and not segmental nerves may be designated at the same time in any proper way.

It is in this respect that the want of an international understanding is most severely felt, and we must hope that the future may provide an advancement of science also in the matter; for before a firm and clear base for these homologies of nerves is given, we might just as well talk Chinese toge her.

Professor Ewart's investigations about the cranial nerves had for their chief purpose a clearer insight into the innervation of certain organs of sense, treated in another paper annexed to the same volume. I am very glad to state that the impossibility of accepting his homologies does not interfere with his results as regards the innervation of these sense organs.

The anatomical skill of the author is best shown in the treatment of the structure of these organs. So far as my own experience in these matters goes, I am led to ask, Are his statements and figures of the sensory canals and the nerves belonging to them very correct and complete? He overreaches the previous writers treating the same objects by the admirable finish of his papers

which, as far as I see, ought to be followed by another concerning the ampullary canals.

In this chapter I have also to object to his way of treating the literature and of stating homologies in spite of his own principles.

The sense organs I discovered on the skin of Raja, and called "Spaltpapillen," named "pit-organs" by Ewart, were not found by any other author before. It is not true that Merkel saw them on the back of *Mustelus* and at the mouth of *Spatina*; he described only "freie Nervenbügel" (free nervous collines) in these places. The name itself proves that the sense organs described by Merkel belong to another group altogether, and so does their position.

How Ewart, who places such importance in the distribution of the nerves, can find that the "Spaltpapillen" probably correspond to organs of *Squatina* placed at the mouth of a very different make and different innervation, he may know himself, but the reader finds it impossible to follow such argument.

The figure he gives of the pit-organ (sensory canals, pl. 3, Fig. 10) does not show such an organ fully developed. Otherwise the split would be narrow and straight, the cells by which it is lined flattened and columnar, not rounded, the papilla itself much higher raised above the surface of the epithelium, and pigment cells frequently found between the epithelial cells. (Comp. my paper: "Über Bau und Bedeutung der Canalsysteme unter der Haut der Selachier." *Sitz. Ber. d. Berlin Akad. d. Wissensch.*, 1888, s. 291.)

The papilla is, in the adult, a good deal raised above the level of the skin, so that even the sense organ at the bottom of the split in the papilla has still a somewhat elevated position. This position prevents me from admiring the name of "pit-organs," as a pit ought to mark a depression below the main level. Continental writers will also shake their heads in reading that a differentiated group of epithelial cells forming a sense organ, and resting between them, is called a "follicle," which by all means wants a kind of stronger envelope enclosing the cells. But I quite agree that in England Latin words might be admitted, which would not do on the continent.

Before Prof. Ewart proceeds to describe the ampullary canals, I recommend him once more to study the paper of Savi concerning these canals. He places the French author amongst those who take also the ampullary canals for sense organs, which is a great mistake, as Savi affirms in most decided terms the excretory function of the ampullary canals. (Mateucci et Savi, "Études Anatomiques sur le Système Nerveux et sur l'Organe Electrique de la Torpille," p. 331.)

But such objections very slightly detract from the great merit of the author. They only prove the strong interest which the perusal of Prof. Ewart's papers has aroused in me, as it will do in all other readers. I cannot conclude my remarks without acknowledging once more, with all my heart, the magnificent results obtained by him, and I trust that he may succeed further in the same direction.

GUSTAV FRITSCH.

NAVIGATION BY SEMI-AZIMUTHS.¹

THE year 1893 should be an interesting one to nautical men. A new Daniel has come to judgment in the person of Mr. Ernest Wentworth Buller, M.R.A.I., M.R.U.S.I., M.I.E.E., the inventor of the semi-azimuth system of navigation, who is equally earnest in denouncing the shortcomings of the existing systems of

¹ "Semi-Azimuths. New Method of Navigation, being a combination of Spherical Trigonometry and Mercator's Sailing." (London: Norie and Wilson, 1893.)