NOTES ON PROF. JUDSON HERRICK'S PAPER ON THE CRANIAL NERVES OF THE COD FISH.

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I am indebted to the courtesy of Professor Judson Herrick for the opportunity of examining his work on the Cranial Nerves of the Cod Fish before it was sent to the printer, and willingly respond to his considerate invitation for some remarks on those parts of the work that concern myself. Before doing so, however, I must say that, whilst it is very gratifying that he should confirm my work as far as regards the lateral line system, which was the principal object of my paper, it is only doing his work an obvious justice to admit that, by the examination of much older fish by "Markscheidenfärbung," he has made considerable advances as regards two subsidiary but important questions on which I had hoped to have thrown some light. I now beg to add the following notes to his paper:

1. The little twig (Fig. 1, *r. o. p. l.*) is *not* represented in my sections. It must be remembered that the correspondence between the pit-organs of *G. morrhua* and *G. virens* is general, but by no means exact, i. e., applies to *areas* and not to individual sense organs, and the organs innervated by this twig in *G. morrhua* may be absent in *G. virens*. Greater variations than this occur in the lateral line systems of the two species. Note further the absence of the barbule in *G. virens*.

2. Prof. Herrick appears to cast doubt on my statement that the IXth nerve contributes to the *lateralis accessorius*. However inexplicable this statement may be, there can be no doubt as to its foundation in fact. The connection was not always found, but it *was* found in some cases on removing the roots of the IXth and Xth to a dish, and dissecting them care-

fully under a 1 inch dissecting microscope. T. J. Parker ("Zooomy," p. 125, Fig. A) seems also to have found it.

3. *Facial Ganglion.* In his *Menidia* paper Prof. Herrick says (p. 248 of the reprint, p. 404 in the Journal of Comp. Neurology): "The geniculate ganglion [of *Gadus*] is wholly intra-cranial and so closely joined to the Gasserian ganglion that Cole failed to differentiate them and mistook the extra-cranial sympathetic ganglion for the geniculate." Again, in the memoir now under consideration it is stated that, "this description [of the facial ganglion] was most puzzling to me until I discovered that the 'facial' ganglion here referred to is in reality the sympathetic ganglion." Now, if we refer to my actual statements on this question, we find (p. 136): "When I first recognized it in my sections, and saw that its cells were small and corresponded precisely to the cells in the ciliary ganglion, with which I compared it, *I concluded at once that it must be the anterior sympathetic ganglion of the cephalic system described by the older anatomists*" (italics now added). Again on p. 143: "We have seen that the facial ganglion is a ganglion placed on fibers that undoubtedly belong to the seventh or facial cranial nerve. It seems therefore to belong to the trigemino-facial complex, and cannot, in face of this fact, be considered a purely sympathetic ganglion. But we have further seen that it has many sympathetic characters, for in the first place it is connected with the fibers of the fasciculus communis system, which we know to be essentially sympathetic in character; in the second, its cells are small and correspond to the cells found in the ciliary and true sympathetic ganglia; and in the third, it gives origin to the cephalic sympathetic trunk." These quotations prove that I took up the position that I did with my eyes open, and was *not* guilty of the gross error implied to me. That is to say, I was fully alive to the fact that the "facial" ganglion (which observe, together with the "trigeminal" ganglion, is printed in inverted commas on p. 133) had been described as, and might be, a sympathetic ganglion. In fact my statement of the trigemino-facial ganglia on p. 133 is much the same as Prof. Herrick's except: (1) that he confirms my surmise that the

"trigeminal" ganglion was trigeminal *and* facial; (2) he denies that the facial fibers are connected with my "facial" ganglion; and (3) he states that my "facial" ganglion is a true and not a *partial* sympathetic ganglion, as I had supposed.

Now, apart from the fact that whatever the "facial" ganglion is there is a very strong *à priori* probability that it *does* represent a disassociated portion of the trigemino-facial complex, the really cardinal point is, are any facial fibers derived from its cells? In this connection Prof. Herrick's figures 6 and 7 are not quite as much to the point as I could wish. The fact is that the "facial" ganglion is not only closely opposed to the other ganglia, but that many true facial fibers pass right through the center of the ganglion.¹ It is no explanation of this phenomenon to say that "they appear to arise from it merely because of the way the sympathetic ganglion is crowded up into the foramen among these roots." Why should the ganglion be in that position, and has that position no significance? There are no mechanical necessities explaining it that I am aware of, since in the adult trigemino-facial fibers *do not pass through a foramen at all*, but emerge anterior to the pro-otic, without, in front, any bony or other limitations as to space. Further, I thought my sections *did* show a connection between some of the fibers and the cells—a conclusion which should have been fully verified by other methods of research. But it seems to me that even Weigert sections are also insufficient for this purpose, and I am disposed to consider Prof. Herrick's statement that there is absolutely no connection as "not proven." It is probable that Golgi or methylene blue preparations are necessary to settle this point, and I prefer to suspend judgment pending the production of evidence of that nature, which *may* show that, whilst many or most of the fibers simply pass through the ganglion, others are connected with its cells.

4. With regard to the identity of the branches I described on p. 135, I am unable to be of any assistance with

¹ I take it that Prof. Herrick fully confirms this statement. A *true* sympathetic ganglion occupying such a position may have been described before, but I cannot call an instance to mind.

regard to branches 1 and 2, since I only mentioned them because they passed through the "facial" ganglion. I did not trace their peripheral course carefully. Branch 3 is correctly identified by Herrick. The nerve I referred to as the "pre-spiracular or chorda tympani division of the facial" was dissected carefully on many specimens as regards its proximal course, and was traced on to the lower jaw peripherally. It is lettered H³ in my Fig. 2. Prof. Herrick has shown that it is not the pre-spiracular nerve, and hence cannot be the chorda tympani. Were it not for his statement that the post-spiracular communis component is lacking in *Gadus* (which surprised me after reading his *Menidia* paper), I should have said it was the r. mandibularis internus VII, as he uses this term. I think now it must be the communis fibers in the mandibularis V, although the course of the latter is different from that as observed by me.

5. *Ramus lateralis accessorius*. With regard to this question, it must at the outset be understood that the first *real* demonstration of the communis nature of this system was by Prof. Herrick himself in his *Menidia* paper—a work published after my own. Working on the basis of Strong's paper, I think I was justified in concluding (p. 141, foot-note) that the communis system was a visceral system of nerves. Prof. Herrick still believes that to be the case, and holds that the somatic distribution of the communis fibers has been secondarily acquired. It now seems to me, however, that it may be precisely the opposite that has occurred. If we believe with Dohrn in the gill slit origin of the mouth (a view which has distinctly gained ground lately), and if I am right in saying that the communis fibers are distributed mostly to the mucous membrane of the stomodæal involution and the outer skin, it seems at least as probable that it was originally a cutaneous system, which has, like the early teeth, invaded the mouth. The above statement *re* the communis nature of the accessorius explains our differences as to Baudelot. The essential difference between the two kinds of fibers was not established at the time I was writing, and hence the confusion. Since writing the above, I have concluded that the communis nature of the lateralis accessorius

may have been deduced from Stannius, as well as from the other authors mentioned in the note in my *Gadus* paper. I was, however, so influenced by Strong's paper that I was loth to believe it.

Regarding my statement that the accessorius system is composed of somatic sensory fibers, Prof. Herrick says: "My examination of the literature has led me to no such conclusion, nor can I accept Cole's results as decisive in the matter." To this I only have to say that in spite of Prof. Herrick's demonstration of the communis nature of the accessorius, which I now fully accept, my statement is perfectly correct. If there is to be any uniformity in the use of a term, the term "somatic sensory" must be applied to all sensory nerves distributed to the skin. If Prof. Herrick thinks that it is unwise to apply one term to two systems of fibers of a different character, then his course is to reject the term altogether. He cannot, however, object to *my* legitimate use of it. But if we reject the term, what will this lead to? I do not consider the lateral line system of nerves to fall within the definition of somatic sensory nerves, for the same reason which prevents me from describing the optic nerve as such. They are mostly distributed to structures which, though they are of course derived from the skin, have since acquired other connections and are typically no longer topographical dermal structures. Their nerve fibers moreover are perfectly circumscribed, and do not occur outside of the Ichthyopsida. On the other hand, with regard to the terminal buds, they are truly dermal structures and only represent a section of a multitudinous array of dermal sense organs. For example in mammals we have distinct sense organs for heat, cold, pressure and so on. It is, I believe, now proved that each such class of sense organ has its distinctive nerve fiber. But would Prof. Herrick classify such fibers into a multitude of systems? Such ultimately may be done, but in the mean time it seems a little premature to dogmatize on the subject of nerve components.

One more point. In his *Menidia* paper, p. 42 (of the reprint, p. 198 in the Journal), Prof. Herrick says: "Is the sim-

plicity of the lateral line system in *Menidia* as compared with many other fishes, especially the lower fishes, to be regarded as primitive simplicity or as the result of degeneration? Cole would say the former, for he argues that the naked condition of the sense organs is always the primitive, and that in the decline of the system these organs are lost before the canals. But how about the Amphibia in which the system is fluctuating on the verge of the extinction and yet no canals are present, only naked organs? On the whole, I incline to regard the condition in *Menidia* as reduced, rather than primitive." But can Prof. Herrick produce any evidence to show that the lateral line system *ever was below the surface in Amphibia*? It seems to me that the lateral line organs are characteristically superficial in recent Amphibia, and the fossil forms apparently show the same thing. Moreover, when I state that the primitive lateral line system must have been a superficial one, I do *not* imply the converse, that any fish having superficial lateral line organs has a primitive lateral line system. Further, it is inconceivable that there can be any degradation of a tubular lateral line system which is not set in motion by the degeneration of the sense organs themselves. There are several cases in the literature where it would seem that the sense organs have almost disappeared and left the canals behind. The latter then seem to have varied considerably. On the other hand, *Menidia* may be a teleostean, and therefore a specialized fish, and still have a developing lateral line system, although this would not be expected; or its sense organs may have been reduced both in number and complexity and involved the *consequent* movement of the canals toward the exterior. Prof. Herrick's statement, therefore, that the canals of *Menidia* are degenerate may by no means be improbable.