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THE FOURTH CRANIAL NERVE

BY

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IN the interesting paper on the fourth cranial nerve by Mr. Parsons in the December, 1921, number of this Journal there are three points that seem to need a little criticism. In a reply to a private communication from myself he has kindly asked me to write a paper on that nerve. I hope to show here :—(1) That the decussation of the fourth nerve is not quite complete ; in short, that it contains a few uncrossed fibres ; (2) that the sixth nerve nucleus gives origin to uncrossed fibres exclusively, all of which pass out in the homolateral sixth nerve ; and (3) that the mesencephalic trigeminal root is not motor, and cannot therefore form the visceral motor component of the fourth cranial nerve. It so happens that a few years ago I wrote two papers⁽²²⁾ which dealt rather fully with all these matters ; so I shall give here merely the salient points of those papers. The title of the earlier paper (1910) was :—“ The mesencephalic fifth root ; the dorsal glosso-pharyngeal nucleus ; and the question of an endo-neural afferent ganglionic system.” The title of the second paper (1913) was :—“ Direct trochlear and crossed oculomotor fibres.” I will begin now with the mesencephalic fifth root.

The mesencephalic trigeminal root is of extreme antiquity ; it is present and has the same relations in a 13 mm. embryo of the very ancient selachian *squalus acanthias* as it has in adult man.

Neal figures it* in that embryo rising in the dorsal wall of the mid-brain and passing out in the trigeminus nerve. The root is composed mainly of large fibres which rise in large globular, vesicular nerve-cells situated in the outer part of the roof of the mid-brain; but a minority of the fibres of the mesencephalic fifth root have been shown by Bregmann, Probst, and van Londen to be ascending centripetal fibres. Up to the year 1909 the root was by some observers held to be motor, partly because in many animals it passes out by the motor fifth root and partly because the majority of its fibres (the large ones) had been proved to be centrifugal. But, as it was known that it contains both centrifugal and centripetal fibres, others regarded it as in part motor and in part afferent. These latter observers failed, however, to recognize the truth that the mere fact that a nerve-fibre is centrifugal and degenerates downwards when cut or destroyed does not in the least prove that it is motor in function. In 1909 Johnston⁽¹⁾ threw quite a new light on the function of the mesencephalic fifth root. He pointed out that the large globular, vesicular cells which form the nucleus of origin of the mesencephalic fifth root are developed in the dorsal zone of the neural tube, and are situated in the roof of the mid-brain. Further, he pointed out that all motor nuclei are developed in the ventral zone of the neural tube (the basal lamina of His, senior). From a consideration of these facts he expressed the opinion that the mesencephalic fifth root is afferent. Briefly stated, his teaching was that the cells of the tectum mesencephali and locus coeruleus, from which the fibres of the mesencephalic fifth bundle originate (he is, of course, referring to the centrifugal fibres of that bundle) belong to the same category as the dorsal giant cells of the spinal cord of fishes and amphibians (the cells of Rohon), and are equivalent to peripheral ganglion cells. He thinks it probable that they have been derived from the neural crest during development, having been enclosed in the neural tube as are the giant ganglion cells of the spinal cord. He points out that in *Amphioxus* a large part of the ganglion cells of the dorsal sensory nerves lie within the central nerve cord. He suggests that the fibres of the mesencephalic fifth bundle are presumably distributed to skin or to muscle-sense organs. A few months later I⁽²⁾ annexed Johnston's hypothesis that the bundle is afferent, and I quoted evidence which led me to conclude that the cells of origin of the centrifugal fibres of the mesencephalic fifth root form what I called an "endo-neural afferent ganglion." These centrifugal fibres degenerate downwards after section, or destruction by disease, and yet they conduct physiological impulses centripetally. In both these respects they exactly resemble the fibres of any

*H. V. Neal, "Neuromeres and Metameres," *Jl of Morphol.*, 1918, Vol. XXXI, p. 293.

peripheral nerve distally of its dorsal-root ganglion. At the time when my paper was published I was not aware that the mesencephalic fifth root contains some ascending (centripetal) fibres. Clearly, therefore, if my view be right, the so-called mesencephalic fifth "nucleus" is really an endo-neural afferent ganglio-nuclear complex. Its large globular cells which give origin to the centrifugal fibres form the ganglionic part, and the remaining cells form an afferent reception-nucleus for the ascending fibres which pass up the mesencephalic root.

A few days after my paper was in print Otto May and Victor Horsley⁽²⁾ published their experimental study of the mesencephalic fifth root. They found that "avulsion of the peripheral branches of the mandibular nerve caused chromatolysis of the characteristic cells of the mesencephalic fifth root, a result suggesting that these axons run in the peripheral branches, though examination by the Marchi method has failed to reveal them." They failed to obtain any chromatolysis after avulsion of the ophthalmic or the maxillary divisions of the trigeminus nerve.* They also failed to evoke any motor response in mandibular muscles by means of electrical stimulation of the exposed mesencephalic fifth root. This was by itself enough to prove that the root is not motor, for motor roots give a motor response with the greatest ease and constancy when electrically stimulated. Yet, in spite of this, May and Horsley hesitated to conclude that the root has exclusively afferent functions, although they knew of the opinion previously expressed by Johnston.

Six months later Willems⁽³⁾ published a paper (1911) on the masticator and mesencephalic trigeminal nuclei of the rabbit, containing an account of his studies on the anatomy, development, and histology of these nuclei, and of his experimental work by the retrograde chromatolysis method. He points out that the masticator nucleus receives all its connections from the mesencephalic nucleus and gives none to it. "The principal protoplasmic prolongation of the vesicular cells has not the characteristic histological structure of a motor cell." He found that avulsion of the muscular branches of the mandibular nerve gave retrograde chromatolysis in only 51 per cent. of the cells of the mesencephalic nucleus. Avulsion of the branch to the mylo-hyoid and anterior digastric gave no cell changes in the mesencephalic fifth nucleus. Other points I need not here dwell upon. Willems concluded that that nucleus is (1) a sensori-motor apparatus, (2) the homologue of a root-ganglion, and (3) what amounts to an "endo-

*I was privately informed by Miss Tozer in 1914 that she and Professor Sherrington did find chromatolysed cells in the mesencephalic fifth nucleus after section of the ophthalmic and the maxillary trunks. But this work has not been published; and I have reason to believe that it never will be.

neural ganglion" (he writes "resté dans les centres"). He discussed Johnston's work and view, but did not try to find out whether any of the centrifugal fibres of the mesencephalic fifth root go to skin or pass down the ophthalmic and the maxillary trunks. Doubtless, he felt this was foreign to the work he had in hand. But I have said enough here to show that, although the superior oblique muscle of the eyeball is developed in the mandibular head cavity of Elasmobranch embryos, the mesencephalic trigeminal root cannot be the visceral motor component of the nervus trochlearis. We are forced, therefore, to conclude that the fourth cranial nerve has now only two out of the four fundamental nerve-components of a segmental nerve, viz. :—(1) Somatic motor, and (2) somatic afferent (Sherrington,⁽⁴⁾ and Miss Tozer⁽⁵⁾ and Professor Sherrington).

There is embryological, anatomical, pathological and experimental evidence that the fourth cranial nerve contains a few uncrossed fibres. Thus, in 1890 Paul Martin⁽⁶⁾ found that in cat embryos the trochlearis nidulus (embryonic nucleus) sends out its neuroblasts first on the same side, whereas its crossed ones are later in appearance. In 1891 Froriep⁽⁷⁾ observed in young embryos (16 mm.) of *Torpedo Ocellata* that the trochlearis nerve receives fibres from both trochlearis niduli. In 1892 Obersteiner⁽⁸⁾ described some fibres of the trochlearis nerve as passing ventralwards in traversing the posterior longitudinal bundle. He thought that a certain number of these fibres curve back to join the homolateral trochlearis nerve, whilst the majority pass across the raphe. In 1911 Franz⁽⁹⁾ described and figured both crossed and uncrossed trochlearis roots in bony fishes. He found that in some of the fifteen fishes examined the trochlearis has two roots, a dorsal and a ventral, each crossing with its partner. In others only the dorsal trochlearis root is present, in yet others only the ventral. *Lophius* and *Pleuronectes* showed some uncrossed trochlearis bundles. In the latter some bundles cross the raphe twice "so that they amount in effect to uncrossed fibres." In 1913 McKibben⁽¹⁰⁾ studied the eye-muscle nerves of twenty-five adult specimens of *Necturus Maculosus* by the intra vitam methylene blue method. He found great variations in the trochlearis nerve, even occasionally in a single specimen on the two sides of the head. Its root contains from sixteen to twenty-four fibres. Out of these fibres, from four to eight, "seem to enter the nerve uncrossed." But, as these appeared under high magnification to be larger than those which make up the trochlearis nerve, he was inclined to think that they belong to the mesencephalic trigeminal root. There is, then, some uncertainty on this point. In 1897 Siemerling and Boedeker⁽¹¹⁾ found evidence of the existence of degenerated cells in both trochlearis nuclei in a case where only one superior

oblique muscle was paralyzed. During life the right superior oblique was said to be normal, the left was paresed. Microscopical examination showed atrophy of the right fourth nucleus and of its intramedullary root, with slight degeneration of the left fourth nucleus, but none in its intramedullary root. There was also marked degeneration of the left superior oblique muscle, and slight of the right. At the end of their paper they conclude that the trochlearis crossing is total. In some respects the case seems to be unconvincing. If the right superior oblique was really intact, the left fourth nucleus should have been normal, and the right superior oblique should not have shown any degeneration. As syphilis was present, the possibility of multiple diffuse lesions has to be admitted. It is quite admissible, I own, to hold that this case does not really prove the existence of uncrossed trochlearis fibres in man.

In 1898 van Gehuchten⁽¹²⁾ found that after section of the rabbit's fourth cranial nerve chromatolysis occurred in a few cells of the homolateral fourth nucleus. In a later and fuller research⁽¹²⁾ (1903) he confirmed the existence of a few direct trochlear fibres in the rabbit. In 1909 van der Schueren⁽¹³⁾ found that, after rupture of one trochlear nerve in rabbits, a few cells—a very careful count showed a dozen—were chromatolysed in the homolateral trochlear nucleus, and that a few normal cells were seen in the contralateral trochlear nucleus. The cells which gave origin to the direct (uncrossed) trochlear fibres were distributed, without any apparent order or grouping, throughout the whole extent of the trochlear nucleus. Thus, he confirmed the previous findings of van Gehuchten (1898, 1903) that in the rabbit the trochlear nucleus sends a few direct fibres into the homolateral trochlear nerve. We have proof, then, that a few uncrossed fibres exist in the trochlearis nerve from the ancestors of Elasmobranch fishes up to mammals, and that in cat embryos they appear earlier than the crossed fibres.

In my 1913 paper⁽²²⁾ I quoted a mass of evidence which showed that we have anatomical, experimental and pathological proof of the existence of crossed oculomotor roots and fibres in fishes, amphibians, birds and mammals up to man. And I showed that there is a remarkable agreement among experimentalists that the crossed fibres of the oculomotor nerve come from the dorsal part of the caudal end of the contralateral oculomotor nucleus. Duval⁽¹⁴⁾ (1880) pointed out that the idea of a partial decussation of the oculomotor nerve is a very old one. Thus, this opinion was held by Varolius (1591), Riolan (1649), Vieussens (1685), and Vicq d'Azyr (1805). It was denied by Longet (1842), but was revived by Vulpian and Philippeaux (1853) for mammals, including man.

More than sixty years ago the suggestion was made that lateral conjugate eye-movements are carried out by means of fibres which

the sixth nucleus is alleged to send to the contralateral third nucleus or root for the innervation of the internal rectus muscle in this movement. This theory—which I have called the Duval-Laborde hypothesis—was first brought into prominence by these two writers⁽¹⁵⁾ in 1880, partly owing to their experiments and partly to Duval's anatomical studies. They were also much influenced by certain cases of paralysis of lateral conjugate eye-movements associated with preservation of the power of convergence. In section 3 of my 1913 paper⁽²²⁾ I held that the anatomical work of Duval, and of Duval and Laborde, on this question was not at all convincing. In section 6 of my paper I gave details of six cases that were examined pathologically, viz., that of Féréol (1877),⁽¹⁶⁾ Hughes Bennett and Savill (1889), Gee and Tooth (1898),⁽¹⁷⁾ Spitzer (1899), Bruce⁽¹⁸⁾ (1903), and Siemerling and Boedeker (1897).⁽¹¹⁾ In Féréol's case there was a tuberculous focus near the origin of the right sixth nerve. He regarded the Duval-Laborde hypothesis as possibly correct. The second case was a worthless one. Gee and Tooth's was very carefully examined. It was a case of bilateral loss of lateral conjugate eye-movements, but loss of convergence was also present, and the left sixth nucleus was uninjured. The writers suggested (quite rightly, I think) that "it may be regarded as possible that a lesion involving the division of the posterior longitudinal bundle on both sides would result in paralysis of conjugate deviation on both sides, even though the sixth nuclei were unaffected." In Spitzer's case there was degeneration in the posterior longitudinal bundle. No comment is needed. Bruce's case was one of double paralysis of lateral conjugate eye-movements, which he studied with his usual accuracy and caution. It is clear that he did not blindly accept the Duval-Laborde hypothesis, for he expressly suggests a particular experiment as necessary. I will show presently that van der Schueren performed this experiment in the rabbit a few years later and disproved that hypothesis, for the rabbit at any rate.

Siemerling and Boedeker⁽¹¹⁾ denied the existence of a lateral conjugate eye-movement centre in the sixth nucleus. They recorded a case of bilateral degeneration of the sixth nuclei in which, during life, there had been no paralysis whatever of the internal recti muscles. We cannot, then, hold that in man the sixth nucleus sends any fibres to the opposite third nucleus or root for the conjugate inward movement of the internal rectus muscle in the associated lateral eye-movements. Duval and Laborde⁽¹⁵⁾ (1880) performed stimulation and destruction experiments in the region of the eminentia teres, chiefly in dogs, but also in cats and monkeys. They thus obtained various deviations of the eyes. But their experiments cannot be taken seriously, for

they actually believed that their stimulation and destruction was limited to the sixth nucleus. We now know, thanks to the labours of many experimenters up to van der Schueren (1913), that ascending vestibulo-ocular fibres pass up in the posterior longitudinal bundle, some of which pass through the sixth nucleus. Clearly, therefore, Duval and Laborde's experimental procedures involved these ascending fibres as well as the sixth nucleus. In 1881 von Gudden⁽¹⁹⁾ found that, after extirpation of the three eye-muscle nerves of newly-born rabbits, there was complete atrophy of the contralateral fourth nucleus. He concluded that all the trochlear fibres are crossed, some of the oculomotor crossed, and all of the abducens nerve direct. In 1909 van der Schueren⁽¹³⁾ established the following facts in rabbits:—(1) After section of the third, fourth and sixth nerves of one side he found, a few days later, that there was chromatolysis of all the cells of the homolateral sixth nucleus, and none whatever in the contralateral sixth nucleus. It is obvious that, if the sixth nucleus sent any fibres to the contralateral third nucleus or root, he would have found chromatolysis of some of the cells of the sixth nucleus on the side opposite to his section of the third nerve. (2) Section of one sixth was followed by chromatolysis of all the cells of the homolateral sixth nucleus. Therefore all the cells of the sixth nucleus give origin to root-fibres of the homolateral sixth nerve. (3) He also established the facts that, in the rabbit: (a) the posterior longitudinal bundle in its course between the level of the sixth nucleus and the fourth and third nerves contains ascending fibres which are connected with the cells of the homolateral fourth and third nuclei; (b) that bundle does not contain any fibres which pass into the peripheral part of the fourth or the third nerve of either side; (c) the sixth nucleus sends no fibres into the posterior longitudinal bundle; and (d) no neurones connect the sixth nucleus with the fourth and third nuclei. In his recent paper (1913) on the posterior longitudinal bundle he again proves the falsehood of the Duval-Laborde hypothesis for the rabbit. We may sum up this question thus:—(1) In all those cases of loss of lateral conjugate eye-movement, accompanied by preservation of the action of the contralateral internal rectus muscle in convergence, which have been carefully examined pathologically, we find evidence of the presence of degenerated fibres in the posterior longitudinal bundle; (2) the case of Siemerling and Boedeker directly negatives the Duval-Laborde hypothesis for man; (3) experiments show that all the cells of the rabbit's sixth nucleus are radicular. It sends no fibres into the posterior longitudinal bundle to the contralateral oculomotor nucleus or root; (4) whatever may be the mechanism of the lateral conjugate eye-movements of man and the rabbit, so far as the brain-stem is concerned, it is not by means of the

Duval-Laborde fibres, for such fibres have no existence. Thirty years ago van Gehuchten⁽¹²⁾ expressed the opinion that the posterior longitudinal bundle seems to be sufficient to carry out the lateral conjugate eye-movements by means of the fibres which pass in that bundle to the various ocular nuclei, and many authors have held the same opinion. In my 1913 paper I stated that there are only two kinds of lesions which can cause effects anatomically and functionally limited to the cells of the sixth nucleus, viz. :— (1) A slow, primary degeneration of its cells; and (2) a retrograde, secondary degeneration of its cells due to a lesion of the homolateral sixth root or nerve. All gross lesions, such as tumour, softening, or haemorrhage, which involve one sixth nucleus, involve also of necessity some of the ascending vestibulo-ocular fibres. A lesion, then, which is strictly limited to the cells of one sixth nucleus, gives exactly the same clinical picture as one which involves the motor fibres of the sixth root or nerve; but a combined lesion of the sixth nucleus and the posterior longitudinal bundle gives rise to loss or diminution of lateral conjugate eye-movement to the side of the involved sixth nucleus, with or without (most commonly with) preservation of the convergence action of the contralateral internal rectus muscle. And, finally, if the Duval-Laborde hypothesis were true we should find in every case of experimental section of one third nerve, and in at any rate some cases of complete unilateral third nerve paralysis in man, that necropsy would show the presence of chromatolysis of some of the cells of the contralateral sixth nucleus; but such changes have, as far as I know, never been seen by any experimentalist or pathologist. I may add that a lesion strictly limited to the cells of one fourth nucleus will give almost complete paralysis of the contralateral superior oblique muscle, together with a very slight paresis of the homolateral superior oblique; and the latter might easily be overlooked. I pointed out in my 1913 paper that ignorance of the fact that the fourth nerve contains some direct fibres can lead to error in diagnosis. A few years ago I heard a very distinguished ophthalmic surgeon relate the case of a man who showed an isolated complete palsy of one superior oblique muscle, which came on suddenly during a paroxysm of coughing in pertussis. The surgeon diagnosed a haemorrhage of the opposite fourth nucleus. But there can be no reasonable doubt that the haemorrhage occurred within the sheath of the homolateral fourth nerve trunk. We can now see how it comes about that a pure sixth nuclear lesion gives unilateral (homolateral) signs alone, whereas a pure trochlear or oculomotor nuclear lesion of necessity gives bilateral palsies or pareses. In my opinion the vast majority of cases of ocular palsies are due to fascicular, radicular, or radiculo-nuclear lesions, and only a small minority to purely nuclear lesions.

When we consider the meaning of direct trochlear and crossed oculomotor fibres we see that the fundamental error has been made of comparing the fourth nerve with the third nerve. The true comparison is between the fourth nerve and the branch of the third nerve which supplies the inferior oblique muscle. We have to remember that, whereas the fourth nerve supplies only one muscle, the third nerve supplies four eye-muscles and a large oculo-palpebral muscle, the levator palpebrae superioris. In 1904 I surmised* that in the laterally placed lateral eyes of vertebrates the two oblique muscles were primitively purely wheel-rotators and the upper and lower recti were purely vertical movers of the eyeball (strictly speaking, of the cornea). At a later stage—which may have come quickly—the obliques and these two recti received new fibres and new functions with the development of inward movements of the eyeballs. In adduction, then, the obliques became vertical movers and the two recti wheel-rotators. Now, primitively the two eyes moved independently of each other, and originally each hemisphere governed the movements of the opposite eye. Thus the upper path was crossed, the lower direct. We must conceive each separate field of fixation in this primitive animal as divided into a right and a left half of the mid-line of vision of each eye. Probably the earliest possessors of lateral eyes were relatively defenceless animals which sought safety in escape from their overtaking enemies. If that be admitted, the earliest eye-movements were backward movements performed by each external rectus, aided for upward and backward movements by the superior rectus and inferior oblique, and for downward and backward movements by the inferior rectus and superior oblique. All this was performed by a crossed upper nerve-path and a direct lower path. It follows that as in the abducted position the upper and lower recti have much greater resistance to overcome in producing upward and downward movements than the obliques have to meet in producing wheel-movements, the number of direct fibres going to the two recti was of necessity far greater than was needed in the case of the two obliques. Hence the obliques received very few direct fibres, and the upper and lower recti a very much larger number than the obliques. But things were quite different when this primitive animal began to need (or at any rate to acquire) adduction movements of each eyeball. These movements were really right-sided movements of the left eye, and *vice versa*. For convenience we may call a muscle a homonymous one when it carries out a movement on its own side, and heteronymous when it performs a movement on the opposite side. The upper nerve-path still remained a crossed one, therefore these

* *Rev. Neurol. and Psychiatry*, 1904, Vol. II, p. 799. "Vertical eye-movement testing in the positions of abduction and adduction."

adduction movements of each eye were governed by the homolateral hemisphere. Clearly, therefore, a crossed lower path had to be evolved in order that these heteronymous muscular actions should be capable of performance. A little reasoning will show that the internal rectus muscle received crossed fibres, the two obliques a very large accession of crossed fibres, and the upper and lower recti a very small accession of crossed fibres. The reason for this was, of course, that in adduction the obliques have to contract against marked muscular resistance, and therefore need a large number of crossed nerve-fibres, whereas the upper and the lower recti have very little resistance to overcome, and therefore need very few crossed nerve-fibres. Of course, we all recognize that it is not easy to understand the exact stages by which the primitive independently acting eyeballs of lower vertebrates arrived at the conjugated actions of higher forms. But one thing is clear:—If the upper path be wholly crossed, the lower path must be double in the case of the two obliques and the upper and lower recti, otherwise the eye-muscles would not be able to perform the various eye-movements of mammals (especially man) as we know them. If, on the other hand, the upper path be double, a single nucleo-muscular path to these four muscles is sufficient. With regard to the internal rectus muscle, we have evidence that it does not receive any crossed fibres from the contralateral sixth nucleus. I feel convinced, however, that all its fibres are crossed, and that all its motor fibres come from the contralateral third nucleus. My reason is that it is entirely a heteronymous muscle, not only in the action of conjugate lateral eye-movements to the opposite side, but also in convergence. In the latter act the left internal rectus performs a right-sided movement, i.e., it acts as a heteronymous muscle. It is believed that in man the cortico-nuclear path is wholly crossed. In the act of convergence each hemisphere governs the action of the homolateral internal rectus muscle by means of the crossed fibres which pass from the contralateral third nucleus to the muscle just named. The same thing happens in the actions of convergence below or above the horizontal plane. In these actions each hemisphere acts on the superior or the inferior oblique of its own side by means of the crossed fibres which pass to these muscles from the fourth and the third nucleus respectively. As to the levator palpebrae muscle, the only reason I know of which suggests that it probably receives some crossed fibres from the contralateral third nucleus is the fact that electrical stimulation of a small area of the cerebral cortex gives slight movements of the homolateral eyelid. (I reject van Biervliet's finding* that all, or nearly all of the fibres received by the levator palpebrae superioris

* J. van Biervliet, *La Cellule*, 1899, Vol XVI, p. 1.

are crossed, as it seems to me to amount to an absurdity.) In the final conclusion of my 1913 paper I summed up the question thus :—" As the upper path from the cerebral cortex to the nuclei of the eye-muscle nerves and to the nuclei of the motor nerves of the sterno-mastoid muscle is believed to be wholly crossed, the lower path has to be a double one to the two obliques and the upper and lower recti muscles of the eyeball, and also to the sterno-mastoid muscle. If it were not so, the eye-movements could not be carried out in the way we know they are performed. If, however, the upper path were double, a single lower path would be sufficient, and there could then be no possible need for any crossed nerve-fibres in either the third or the fourth nerves." To this I may add that if we accepted the doctrine (of von Gudden and others) that all trochlearis fibres are crossed, all abducens fibres direct, and some oculomotor fibres crossed, it would still be true that the eye-movements of man could not be performed by such a lower mechanism if the upper nerve-path is wholly crossed, and it is equally true that by none of the various schemata—not even by Bernheimer's or the more recent one of Brouwer—would the eye-movements be capable of performance.

The subject of the embryology of the fourth nerve and of the other ocular nerves and ocular muscles has been so well expounded by Parsons in his recent paper on the fourth cranial nerve that I have but little to add. That little, however, is of much interest in relation to some of the points presented in my 1904 and 1913 papers. Thus, in 1902 Lamb⁽²⁰⁾ arrived at the same conclusions as to the development of the eye muscles in *Acanthias* as Miss Platt* did in 1891. He confirmed the existence of her large " muscle E," and he agreed with her that it degenerates and disappears in the ontogeny. This he found to occur between the 19 mm. and 26 mm. stage. He found that the inferior oblique is the first of the oculomotor muscles to be differentiated. The oculomotor nerve is differentiated at an 8 mm. stage, the abducens at a 10 mm. stage (Neal, 1898), and the trochlearis nerve not until a 21-22 mm. stage. The trochlearis is the last cranial nerve to be differentiated. Lamb points out that in *Acanthias* the original direction of all the eye muscles, including " muscle E," is longitudinal. This seems to him to represent an originally flexible (jointed) condition of the head. He writes :—" It seems to me improbable that the present musculature of the eye in *Acanthias* is the primitive one for several reasons :—(1) The adult condition is reached only after the constituent muscles have undergone rather extensive alterations in form and transfer of position ; (2) the eye-muscles do not all arise equally early, nor do they reach their

*J. B. Platt, *Jl. of Morphol.* 1891, Vol. V.

definitive condition at the same time; (3) before some of the permanent eye-muscles are formed, one muscle (muscle E), which later disappears, reaches an advanced state of development. This muscle, from its form and position, must either have once been functionally connected with the eye or with some structure now lost, and of which not even an embryonic rudiment is known."

"If, then, the present musculature of the eye is not the primitive one, it becomes an interesting question to inquire if the embryonic development will indicate any stages in the phylogenetic development. Two such stages, it seems to me, are indicated:--(1) A stage, where, if any eye-musculature existed, it was furnished by the anterior somite" (I take it he is referring here to Miss Platt's "cavity A"). "This is indicated, first by the fact that this somite is the only one which from its topographical relations could move the eye; and, second, the longitudinal and serial arrangement of the remaining muscle Anlagen indicate a jointed condition of the head and consequently a functional activity on the part of these muscles which would preclude any connection with the eye. (2) A stage at which four muscles moved the eye. These were the superior oblique, the external rectus, the inferior oblique, and "muscle E." These four muscles were arranged radially. "Muscle E" and the inferior oblique opposed each other, the former pulling the back of the eye dorsally, the latter ventrally. The superior oblique and the external rectus opposed each other, the former pulling the back of the eye forward, the latter backwards. This stage is reached in ontogeny at a length of 21-22 mm. (Figs. 4, 5, 6). The four muscles then have the rectangular radial arrangement described above. They have all reached approximately the same degree of differentiation, which is far in advance of the three remaining eye-muscles."

Neal (1918) in his paper⁽²¹⁾ on "The history of the eye-muscles," published his evidence, from his developmental studies, that the mandibular cavity participates in the formation of the external rectus. This fact was asserted by Dohrn (1904) and by Neal (1909); but, as Neal points out, neither published any figures or other evidence to support his assertion. On the question of Miss Platt's "muscle E" he finds that she and Lamb were both mistaken in describing it as degenerating and disappearing. The former gives no evidence of any histological changes manifested by degenerating muscle cells. Neal shows that "in *Squalus Acanthias* embryos of 18-24 mm. the anlage of the external rectus muscle shows two easily distinguishable elements, one (anterior) derived from the myotome of the mandibular cavity which is recognizable as 'muscle E,' the other (posterior) formed from the myotome of the hyoid cavity. The two elements differ, not only in their staining properties, but the distinction between the

two may be made out, as Miss Platt has stated, through the presence of a limiting membrane bounding the myotome of the third (hyoid) head-cavity." The difficulty of distinguishing the two elements becomes increasingly greater as development goes on. Ultimately, the bounding membrane disappears altogether. Neal shows clearly that " what disappears is not the mandibular element, but the limiting membrane bounding the hyoid element, making it increasingly difficult and finally impossible to distinguish the two. Of the disintegration or degeneration of the muscle cells of ' muscle E ' there is not the slightest evidence. On the contrary, in the stages during which degeneration has been said to occur, the embryonic muscle cells of both elements of the external rectus muscle undergo similar progressive differentiation as elongated spindle-shaped muscle fibres. In both, myofibrillae are visible in *Squalus** embryos of 45 mm., and transverse striae in embryos of 100 mm."

According to Neal, " the history of the eye-muscles is the history of the transformation of the first three myotomes of an *Amphioxus*-like ancestor into the definitive six eye-muscles of man. This history may be very briefly summarized:—Primarily, as in *Amphioxus*, the three anterior myotomes were members of an unbroken series of segmented muscles extending throughout the entire length of the body. When lateral line organs and enlarged cranial ganglia associated with them made their appearance, the anterior myotomes became split lengthwise into dorsal and ventral moieties. Further separation and displacement followed the enlargement of the optic and otic vesicles. In this way eventually two sets of muscles, one dorsal and one ventral, were brought in close proximity to the enlarging optic vesicles with which they became functionally associated. How these two sets of muscles became gradually transformed into the eye-muscles is revealed by their ontogenesis in Elasmobranchs. Each of the divisions (dorsal and ventral) of the first myotome divides again, thus forming the four muscles innervated by the oculomotor nerve. The second myotome undergoes no further subdivision. Its dorsal moiety, however, becomes the superior oblique muscle and is innervated by the trochlearis nerve. Its ventral portion, however, unites with the third myotome to form the external rectus muscle and becomes innervated by the abducens nerve. The dorsal moiety of the third myotome does not differentiate muscle fibres. Like the myotomes of somites 4, 5, and 6, it has disappeared phylogenetically, leaving the eye-muscles as an isolated group unconnected with the post-otic myotomes." At this point I may mention that Neal, in his

*The name *Squalus*, or *Squalus Acanthias*, was, I believe, the name originally given by Linnæus to the interesting Selachian which most modern embryologists describe under its later name *Acanthias Vulgaris*.

paper on "The morphology of the eye-muscle nerves" (1914), points out that in the Torpedinidae the so-called ganglia of the trochlearis "are irregular fragments of the neural crest lying in the region through which the trochlearis grows" (Dohrn). But in the Squalidae (Neal) the trochlear anlage has no such relations to irregular fragments of the neural crest. But in *Squalus Acanthias* the trochlearis has relations with cell masses that are really sympathetic ganglia (these are figured by Neal).

In conclusion, something must be said about the possible interpretation of the dorsal chiasma of the trochlearis nerve. On this I have nothing original to offer, but I will refer to two explanations that have been offered in addition to those mentioned by Parsons in his December, 1921, paper in this Journal. In 1905 Johnston wrote* thus (p. 210):—"The course of the root fibres dorsally through the brain wall may be due to the course of the fibre-tracts through which they run. The position of the nucleus of the nerve relative to the tracts which form the ansulate commissure in typical fishes suggests strongly that the axons from the cells of the trochlearis nucleus may have followed some of these bundles as the path of least resistance. The tracts between the tectum opticum and base of the oblongata, the tracts between the inferior lobes and the cerebellum, and others, all running more or less dorso-ventrally in the side wall of the brain and decussating ventrally at the level of the trochlearis nucleus—these bundles, which lie ectal to the nucleus of the trochlearis, may have constituted an effective barrier to the axons of the trochlearis in their attempt to reach the ventro-lateral surface of the brain. The axons may then have turned upwards along the ental surface of these bundles until they reached the dorsal surface of the brain. If the fibres were thus directed in their course they would be carried to the mid-dorsal line before gaining an exit from the brain, and if they then grew straight on they would pass to the opposite side."

Neal⁽²¹⁾ (1914) offers the following suggestion in explanation of the dorsal chiasma of the trochlearis:—"Figure 82 of this paper suggests two possible phylogenetic stages in the development of the trochlear chiasma. An earlier stage is represented on the left of the diagram, and a later stage, corresponding essentially to that seen in some Elasmobranch embryos, is shown on the right. It is assumed that originally the trochlear, as a somatic motor nerve, was distributed to the myotome of the second somite of its own side after the fashion of typical somatic motor nerves, and

*Owing to the haste with which I wrote this paper I have not had time to verify this reference given by Neal. Johnston wrote two papers in 1905, both of which I know. The first was on the morphology of the vertebrate head, etc., etc., in the *Jl. of Comparative Neurology*, 1905, Vol. XV; the second was on the cranial nerve-components of *Petromyzon* in the *Morphol. Jahrbuch*, 1905, Vol. XXXIV. Probably the reference given by Neal as "Johnston (1905), p. 210" refers to the first paper.

that this myotome, when fully developed, extended dorsally to form a union or interdigitation with the antimeric myotome. As Fürbringer has correctly stated, no skeletal structures would prevent the extension of fibres across the median plane. It may be imagined that this extension of muscle fibres across the median plane was correlated with the muscular development of the prostomial region. *Petromyzon* still shows (Fig. 77) the extension of myotomes into this region. Under such conditions slight variations in the length of the nerve fibres which grow to connect with these muscles might bring about a peripheral chiasma. The possibility that such muscles had connections with the epiphysis is not excluded, but such a supposition does not seem necessary. Changes in the extension and direction of growth of muscle and nerve fibres in this region may have been correlated with the development of the cephalic flexure which would seem to require some adjustment of the muscles since the fore-brain and mid-brain regions were flexed into a more ventral position. The final result of the flexure, however, appears to have been a shifting of those portions of the musculature which persisted in this region into a more ventral position and a separation of the muscles which had been opposed in the median plane above the brain wall."

"The growth and great enlargement of the lateral eyes also brought about changes in the (van Wijhe's) second myotome which became split into dorsal and ventral moieties (my. 2 v.l., my. 2 m., my. 2 d.l.) in precisely the same way as occurs ontogenetically in the post-otic muscles of *Petromyzon* as a result of the growth of the otic vesicle (Figs. 78, 79). It may be assumed that, as in the latter case, the median portion of the myotome degenerated, together with its branch of the somatic motor root (trochlear nerve), while the lateral moiety became innervated by a branch of the abducens (Fig. 82, abd.). The dorsal moiety, however, retained its connection with the trochlear nerve, and possibly also with fibres from both sides of the brain, by means of a dorsal peripheral chiasma. Then, when later this dorsal moiety degenerated with the exception of that portion which became attached to the eyeball to form the superior oblique muscle the result would be essentially the conditions that obtain to-day in those forms of Selachian embryos in which a peripheral chiasma persists (Fig. B., p. 25), and are shown on the right hand side of the diagram" (i.e., Neal's Fig. 82). "It is necessary, however, to assume that phylogenetically the chiasma came to be more and more central, and that gradually the fibres of the trochlear nerve became exclusively crossed fibres. What factors determine the survival of the crossed fibres and the elimination of the direct is no less mysterious than those which have produced the ventral chiasma of the eye and the pons. Possibly purely mechanical conditions of the sort

suggested by the writer ('98) and by Johnston ('05) are responsible. All who have discussed the chiasma agree in one essential point—namely, that the chiasma of the trochlear is secondary and that it constitutes a coenogenetic modification of a somatic motor nerve. Therefore its existence does not affect our views of its morphology." If the view of the fourth cranial nerve presented here by me be correct, or even in part correct, I think we must conclude that the peculiarities which have been looked on as marking it out from all other nerves are, for the most part at any rate, more apparent than real. Of all the suggestions as to the meaning of the dorsal chiasma of the trochlearis I like Neal's the best, although he does write of the elimination of its direct fibres. However, he and I agree that they were the primary ones.

REFERENCES.

1. Johnston, J. B.—*Jl. of Comp. Neurol.*, 1909, Vol. XIX p. 593.
2. May, O., and Horsley, V.—*Brain*, 1910, Vol. XXXIII, p. 175.
3. Willems, E.—*Le Névrase*, 1911, Vol. XII, p. 5.
4. Sherrington, C. S.—*Proc. Roy. Soc. Lond.*, (a) 1897, Vol. LXI, p. 247; (b) 1898, Vol. LXIV, p. 120.
5. Tozer, F., and Sherrington, C. S.—*Ibidem*, 1910, Vol. LXXXII, p. 450.
6. Martin, P.—*Anat. Anzeiger*, 1890, Vol. V, p. 530.
7. Froriep, A.—*Verhand. Anat. Gesell.*, Vol. V. (München) in *Anat. Anzeiger*, 1891, Vol. VI, p. 55.
8. Obersteiner, H.—*Anleitung beim Studium der Nervöse Centralorgane*, 1892, p. 395.
9. Franz, V.—*Anat. Anzeiger*, 1911, Vol. XXXVIII, p. 592.
10. McKibben, P. S.—*Jl. of Comp. Neurol.*, 1913, Vol. XXIII, p. 153.
11. Siemerling, E., and Boedeker, J.—*Arch. für Psychiatrie*, 1897, Vol. XXIX, pp. 420, 716.
12. van Gehuchten, A.—*Jl. de Neurol.*, 1898, Vol. III, "Travaux du Labor. de Neurol. de Louvain;" *Le Névrase*, 1903, Vol. V, p. 265.
13. van der Schueren, A.—*Le Névrase*, 1909, Vol. X, p. 117; 1913, Vol. XIII, p. 187.
14. Duval, M.—*Jl. de l'Anat. et de la Physiol.*, 1879, Vol. XV, p. 492; 1880, Vol. XVI, p. 285.
15. Duval, M., and Laborde, J. V.—*Jl. de l'Anat. et de la Physiol.*, 1880, Vol. XVI, p. 56.
16. Féréol.—*Gaz. des Hôpitaux*, 1877, p. 738.
17. Gee, S., and Tooth, H. H.—*Brain*, 1898, Vol. XXI, p. 1.
18. Bruce, A.—*Rev. Neurol. and Psychiatry*, 1903, Vol. I, p. 329.
19. von Gudden.—*Tagebl. d. Deutscher Naturforsch. in Salzburg.*, 1881-1885.
20. Lamb, A. B.—*Amer. Jl. of Anat.*, 1902, Vol. I, p. 185.
21. Neal, H. V.—*Jl. of Morph.*, 1914, Vol. XXV, p. 1; 1918, Vol. XXX, p. 433.
22. Kidd, L. J.—*Rev. Neurol. and Psychiatry*, 1904, Vol. II, p. 799; 1910, Vol. VIII, p. 594, 673, 749; 1913, Vol. XI, 507.