

## CONTRIBUTIONS TO ENCEPHALIC ANATOMY.

PART IO.—NOTE IN REGARD TO THE DIMENSIONS OF NERVE-CELLS AND THEIR NUCLEI.

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THE view has been recently advanced, that the average size of the nuclei in certain nerve-cells having connections with motor nerves, is proportionate to the power developed in the muscles placed under the innervation of the latter. In various papers\* devoted to the announcement of this theory, other inferences are drawn or hinted at, which also have a physiological bearing, and certain objections to such inferences which were made by the present writer are taken up in the last paper of the series quoted. It may not be improper, therefore, to review the argumentative aspect of the questions involved in the light of some well-known facts of neuro-anatomy which seem to have escaped the scrutiny of the writer of the papers referred to.

In regard to the main conclusion of the latter,—that, as a rule, the nerve-cell nucleus of cells related to the innervation of large muscular masses is larger than that of cells related to small muscular masses,—it may be well to say that it has not been questioned by any one. It has probably been an unenunciated idea dwelling in the minds of most neuro-

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\* "Microscopic Studies on the Central Nervous System of Reptiles and Batrachians." By John J. Mason, M.D. JOURNAL OF NERVOUS AND MENTAL DISEASE, Jan. and July, 1880, Jan., 1881.

anatomists, and one so self-evident that it was not considered deserving of special formulation. It is an old observation that nerve-cells, *as a whole*, are larger in the lumbar intumescence of man than in the cervical, and larger in the latter than in the oculo-motor nucleus. What more natural than that the nuclei should vary with the cells as a whole? As connections of the nuclei with the conducting paths of nerve-force have never been demonstrated, while those with the protoplasm (so-called) are clear, those who are accustomed to draw physiological inferences from structural relations could well afford to rest satisfied with the older observation that the cell, as a whole, varied. Even granting the nerve-cell nucleus its greatest possible rôle, there is nothing in its structure, development, or its reaction under pathological circumstances, that justifies one in looking upon it as the *most* important constituent of the nerve-cell.

The observation made that the nerve-cell nucleus is larger, averagely, at the origin of the crural nerves than at that of the brachial nerves of the frog, adds nothing to our knowledge of the relations existing between dimensions and function, beyond a histological confirmation, which, to many, will naturally appear of but a secondary value. The dimensions involved are extremely minute; this by itself constitutes no drawback, but it becomes one when we take into account the fact that the variations in the size of the nucleus have not been shown to be constantly proportionate to the demonstrably important part of the nerve-cell,—its protoplasmic mass. A careful scrutiny of nerve-cells from different parts of the nervous system (and I am now speaking of nerve-cells irrespective of their real or presumed functional rôle) will show that, side by side, cells of the same shape and dimensions have nuclei varying considerably in size; it would not be difficult to demonstrate small nuclei in some large, and large nuclei in some small cells.

Few would, I believe, be willing to follow the author referred to where he leaves his measurements to indulge in speculation. Even he himself will on reflection admit the statement that the nucleus "probably constitutes the true cell" \* to be an altogether gratuitous assumption. That the nucleus is the one permanent ingredient of the nerve-cell, present from the embryonic period throughout life, and serving as a centre for fibril condensation, as the researches of Schmidt on the human, of Hensen on the rabbit's embryo, and of myself on the *Menobranthus* have shown, proves the nucleus to be an important morphological element, but it does not prove it to be anything more nor less than a nucleus for all that. Few things in histology are so well established as that the nerve-cell nucleus is a true nuclear body.

In mentioning the very sound conclusions of Stieda, who seems to have clearly established that the nuclei vary with the cells containing them in the different attitudes of the cord, prior to the undertaking of the measurements which form the basis of the papers referred to, their author claims that Stieda does not fairly state the ordinary view, when he cites his observations as having "great weight against the conclusion that only the large nerve-cells are connected with motor fibres." † Now a perusal not only of the older neuro-anatomical literature, but of many recent essays will show the critic that Stieda has fairly stated not the ordinary view—for that he does not claim,—but a very prevalent one, fostered by the ambiguous statements of standard authors. ‡ In exposing the error of that view he therefore did a substantial service. The writer of the papers under

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\* JOURNAL OF NERVOUS AND MENTAL DISEASE, Jan., 1881, p. 83.

† JOURNAL OF NERVOUS AND MENTAL DISEASE, Jan., 1881, p. 83.

‡ Dr. Richet commits himself to the view of Charcot that "where there are motor centres there are large cells, this is true of the cerebral cortex as well as of the spinal axis."—"Physiology and Pathology of the Cerebral Convolutions," by Charles Richet, translated by E. P. Fowler, M.D. See also Luys' "Recherches, xc," Paris, 1865.

consideration does not himself seem to be altogether free from a very similar error, for he says "it may be true that all large cells connect with motor-nerve filaments," which, unless I am mistaken, he has advanced more positively in a verbal communication to the American Neurological Association, when both Dr. Putnam and myself\* cited observations conflicting with it. These observations and other well-established facts I herewith present in detail, and I shall open with the single one which the writer in question discusses in his last contribution.

On a former occasion† I stated that in the Iguãna "the average dimensions of the cell nuclei of the auditory-nerve nucleus equal those of the motor nuclei of the medulla and cord, and exceed some of them, and that the same statement applies to the cells as a whole." I had also made the same statement regarding the large-celled division of the auditory nucleus in man, on the occasion when one of the papers under notice was read. In evident reply to this statement, but without any disfiguring reference to myself or any one else as the source of the objection, it is stated:‡ "I would suggest, however, to those who may feel disposed to regard these cells as connected with the sense of hearing, that such a view involves giving to this apparatus, in its central portion, a structure almost universally admitted to be motor, like, for example, that concerned in raising the lower jaw; whereas in the central structures for vision and olfaction the cells are all very small."

I am somewhat embarrassed as to the propriety of accepting this suggestion as one directed to my individual address, for the customary reference to the source of the opposing view has been omitted. But as I am not aware of any one

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\*JOURNAL OF NERVOUS AND MENTAL DISEASE, 1880, pp. 476, 477.

† The brain of the Iguãna. JOURNAL OF NERVOUS AND MENTAL DISEASE, 1880, July.

‡ *Loc. cit.*, p. 81.

else having made the same objection in connection with the theories involved in the papers under consideration I shall treat them as directed to myself, leaving the responsibility of an eventual error with the author. In the first place, without insisting on fine verbal distinctions, I would make the counter suggestion, that there is nothing in the structure of any nerve-cell, whether it have demonstrable connections with the motor periphery or not, which the wildest physiological fancy could even remotely construe as a "motor" structure; muscles and cilia have motor structures, not nerve-cells.

It is known, as positively as anything is known, that a nerve nucleus of the human oblongata, which has no possible connections with any other nerve-root than that of the auditory nerve, contains cells rivalling in size the largest known cells of the nervous system, and presenting in their shape some resemblance to what are ordinarily termed motor cells. I therefore consider them as related to the sense of hearing, and their dimensions, so long as no other connections than with a sensory nerve are found to exist, as conflicting with any view which would regard size as necessarily limited to cells having motor connections. The author quoted does not seem to have considered the possibility of these cells being related to motor fields as reflex cells mediating the reflexes from the auditory to the muscular periphery, which would harmonize with the view he follows, and not necessitate the questioning of a universally accepted fact of anatomy for the sake of a theory.\*

The appended clause: "Whereas, in the central structures for vision and olfaction, the cells are all very small," involves the turning-point of the inquiry. It is surprising that such a statement could be made. Leaving out of sight for a moment all central structures, and limiting ourselves to the

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\* This large-ceiled nucleus is identifiable in the iguana.

nervous layer of one of these very peripheries, what do we find? That the retina itself contains nerve-cells of decidedly large dimensions; namely, of twenty to forty micromillimetres and beyond that. Here there is no room for a quibble as to other problematical connections of the cells; they are a part of the immediate recipient area itself! This fact alone disposes of the question raised.

But let us go further. The acoustic ganglia of fish contain large fusiform elements. The ganglion of Gasser and the intervertebral ganglia on the posterior nerve-roots contain cells of the larger size, and with very distinct and large nuclei. The same is true of the cerebellum. Now, whatever function the intervertebral and analogous ganglia exercise, it is safe to exclude any relation to the voluntary muscles! Whatever distant and indirect connection the cerebellar cell of Purkinje has to cerebral "motor" centres, it is certainly not connected with any centrifugal tract! In the light of all we at present know about the cell-forms mentioned, we are bound to consider their proximal connection to be with sensory nerves and with sensory tracts.

The researches of Flechsig have shown that all the great tracts connected with the cerebellum develop *toward* that brain segment, with one exception. Those tracts are centripetal, and therefore sensory. One is the restiform column, another the inner peduncular division, a third a great part of the auditory-nerve root. The *nucleus fastigii*, with which the auditory nerve connects, has cells which cannot be classed among the smaller variety. One tract which connects the cerebellum with the cerebrum and the subthlamic region,—the brachium conjunctivum, developing like other centripetal tracts *toward* the cerebrum, is connected with the beautiful ganglion tegmenti, composed of cells of 45 micromillimetres. Here again are large cells connected with a centripetal, *i. e.*, sensory tract.

The cells of the ganglion geniculatum externum, exclusively connected with the optic tract, are also of a large size; namely, from thirty to fifty micromillimetres in length, and ten to twenty in width.

The following cells of *large* dimensions have demonstrably only sensory or centripetal connections, as far as their relations to the periphery are concerned: 1. The cells of the intervertebral ganglia. 2. Those of the ganglion of Gasser. 3. Those of the acoustic ganglion in fishes. 4. Those of the ganglion geniculatum externum. 5. Those of the nucleus tegmenti. 6. Those of the visual area of the occipital cortex (solitary cells of Meynert).

The following cells of large dimensions are, as far as anatomical and other facts permit us to adopt a conclusion, also connected with sensory peripheries: 1. The large cells in the deep division of the external thalamic zone. 2. The inflated giant-cells of the probably auditory centre in the cortex in the cat,\* recently described by one of my pupils. 3. The flasked-shaped cell of Purkinje. 4. The cells of Clarke's columns.

The following are undoubtedly or very probably connected, at least at one pole, with sensory peripheries: 1. The gigantic cells of the auditory nucleus (100 micromillimetres by 20). 2. The large cells of the deep gray of the optic lobes. Concerning the latter I have already expressed the view, that they mediate reflexes to lower motor altitudes governed by retinal impressions.

Either the statement that all large cells are probably connected with motor filaments, if it requires to be made at all, should be accompanied by so many qualifying clauses as would render it practically void of any meaning, or, better, it should be left unsaid.

It is true that we have large nerve-cells in the giant-

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\*A new cortical centre by Graeme M. Hammond, M.D., *N.Y. Medical Record*, March 19, 1881.

pyramids of the paracentral lobule and in the lumbar enlargement, but to pick these out, to base a theory on them, and to force some conflicting facts under the conception of "doubtful," and to ignore others altogether, is not a logical procedure.

If any generalization is to be attempted as to the relation between the size of a nerve-cell, or that subsidiary element, its nucleus, and the periphery with which the former is connected, a fairer comparison should be made than has been attempted in the papers under criticism.

The cells and their nuclei in the lumbar enlargement should be compared with those of the lumbar intervertebral ganglia, those of the cervical enlargement with those of the cervical intervertebral ganglia, those of the facial with those of the auditory nucleus, those of the trigeminal motor nucleus with those of the Gasserian ganglion, those of the hypoglossal with those of the glosso-pharyngeal nucleus, the giant pyramids of the paracentral lobule with the solitary cells of the occipital lobe—and so on.

If the researches on which the statements criticized appear to have been based had not been limited to reptiles and frogs, the facts would have been recognized: 1st. That the cells of the "sole" auditory origin are not "uniformly small,"\* but that there are three calibres, a small, a large, and a gigantic; the former two having no other even probable peripheral connection than with the auditory nerve. 2. That the cells of the facial nucleus are of the large kind in man. 3. That the large cells scattered near the raphe and in the reticular field have not been confounded with the cranial nerve nuclei by any one; they are the essential ganglionic elements of the general reflex field of the oblongata.†

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\* *Loc. cit.*, pp. 80, 81.

† And it is well to bear in mind that cells corresponding to and exceeding the dimensions of those of every altitude of the cord are here found.



So far as shape and dimensions of nerve-cells are concerned, I can see nothing in the measurements given or the basis of the conclusions criticized, that either adds to our existing knowledge or conflicts with the following, which I stated about a year ago, and which still seems to me to represent our existing state of knowledge on the subject :

“ The central tubular gray masses vary in size with the periphery projected in those masses. A large muscle or group of muscles will have a larger nucleus than a small muscle or group of muscles.

“ There is a tendency in higher animals to a differentiation of the motor cell-groups into sub-nuclei related to separate muscles or groups of muscles.

“ Hypertrophied segments of the body, such as the extremities, are accompanied by lateral extensions of the cornua, in which flexor and extensor muscles probably occupy the same relative position as the one stated for the general flexor and extensor masses.

“ In this direction a gross error has been committed, and is repeated every day, one for whose origin the French anatomists, particularly Luys, are largely responsible ; while to Charcot and his followers we owe its dissemination. They have stated the *large* nerve-cells to be motor, and *per contra*, the small cells to be sensory. Now, I can show that very small cells are found in unquestionably motor nuclei (origin of third pair), and very large ones in patently sensory centres, such as the *ganglion geniculatum externum*. So that any differentiation of nerve-cells as to functions, based on dimensions solely, is fallacious. It has been also predicated as characteristic of the motor cell, that it is richly multipolar ; but there are, on the one hand, richly multipolar cells in the sensory nuclei, such as the auditory ; and, on the other, we find that undoubted motor cells in very low vertebrates have few processes. So that this line of demarcation must be overturned. So far there is but one character which I should be willing to predicate for the so-called motor cell, namely, that the transition from the body to the processes is so gradual that it is difficult to say where the body ends and the process begins, while in unquestionably sensory cells the transition is always abrupt. Viewing the question in the abstract, there is no *a priori* reason why sensory elements should differ from motor ones. Comparing a large num-

ber of sensory with motor cells, we may say that the character above given seems to be the only one on which an anatomical differentiation can be based; exceptions\* there seem to be, but not in the case of any cells whose physiological rôle is clearly established.

"Quite a notable feature in many of the sensory nuclei is the presence of fusiform elements, whose bodies are inflated, and which have two processes—one at each end—and few or no processes otherwise. There is a greater resemblance between the trophic and these sensory nerve-cells than between the trophic and the motor ones.

"The cells in the anterior spinal cornu of the frog are very rich in processes; those of the salamander, and still more so those of the siren, are therein poor; the spinal co-ordination of the frog is correspondingly higher than that of the urodela. The nerve-cell of the cerebral cortex is a free nucleus in the monobranchus, bipolar in the amphiuma (Schmidt), has but few more processes in the scaly reptiles, fewer in the rabbit than in the dog, in the dog than in the ape, and in the ape than in man. (Herbert Major states in his paper on the cortex of a cynocephalus baboon, that he could discover no other difference between the nerve-pyramids of the human and simian cortex than the lesser richness in processes of the latter. I can confirm this observation for macacus and cebus; in the chimpanzee I could discover no difference, taking into account that the staining was imperfect.) The proteus, amphiuma, reptile, rabbit, dog, ape, and man, occupy, with regard to the respective number of processes appended to the cortical cell, the *same* order which they occupy in the intellectual series!

"Here we perceive that the nerve-cell, following the law which we have announced for the *entire nervous system*, gains in functional dignity with the increase of its associations.

"The lumbar enlargement is more marked in animals possessing powerful posterior extremities (man, kangaroo) than in those possessing weak or rudimentary ones (bat, porpoise). The cer-

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\*When writing this clause I omitted considering the fact that *all* the nerve-cells of certain insects are inflated, have few processes, and resemble the cells of the intervertebral ganglia of vertebrates. I believe that a careful study of this branch of the subject will overturn all demarcations, even the tentative one set forth by myself. In fact, when we take into account the possibility, nay, great probability, of one and the same cell having different connections, and that specialization of connections is a feature of higher development, we will be led to expect that in the lowest animals presenting nerve-cells, these will be alike, and in the highest ones more unlike. So we actually find it, but the dissimilarity is not, as the writer criticized would have it, one of dimensions at all.

vical enlargement is proportionately larger in the bat, with its anterior extremities over-developed, and in the mole (for a similar reason), than in the dog and rabbit. The oculo-motor and trochlearis nuclei are almost absent in the *pipistrella* bat, and entirely so in the mole, since the eyes of the former are poorly developed, and those of the latter rudimentary. The lower facial nucleus of the elephant follows the hypertrophy of the facial muscles (trunk); the hypoglossal nucleus in the seal is reduced, just as the tongue is limited in motion. The anterior tubercles of the corpora quadrigemina are atrophic in the bat and mole, for the same reason assigned in the case of the oculomotor nuclei; and in the land turtles the extreme atrophy of the parietal muscles in the dorsal region is accompanied by a greater diminution in the area of, and number of cells in, the dorsal gray matter, than in any other animal. *Per contra*, in the axolotl and other urodela, as well as in the apodal lacertians (*pseudopus*) and snakes (anaconda, boa, rattlesnake), the cervical and lumbar enlargements are either scarcely, or not at all perceptible, just as the limbs are absent or insignificant."\*

The industrious observer whose views are here contradicted, will, with the excellent preparations and the leisure at his disposal, find that the most sluggish of the *urodela*, the *menopoma*, the *menobranclus*, and the *amphiuma*, have far larger nuclei in their nerve-cells than the active *anolis*, and *alligator* or serpents. It is the protoplasm of their cells and the processes that are poorly developed in the *urodela*, a fact which is in favor of the current view, and against the doctrine announced by him.

I would further call attention, not in a hypercritical spirit, but with all fairness, that such statements as the following †: "In the *chelydra serpentina* (snapping turtle, weighing 24½ pounds) all the motor nuclei were much larger than those of the smaller specimens. The same rule holds true in frogs and alligators. The smaller the animal, the smaller the cell-nuclei. I have not seen any mention of this fact

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\*Architecture and Mechanism of the Brain. JOURNAL OF NERVOUS AND MENTAL DISEASE (pp. 4, 17, 45, 76 of reprint), 1879-1880.

† *Loc. cit.*, p. 84.

in any works on anatomy,"—run some risk of being considered as entering the domain of the trivial. No work on anatomy has probably made this special statement, for it is well known that the permanent organs of the body grow, and that their cells grow with the general growth of the body. It would be just as original, and precisely as valuable, for an observer to measure the length of the tail, the dimensions of the scales and tubercles on the skin, the area of the carapax scales, and the diameter of the eyeball, in a young and old snapper, and to deduce the fact that they grow with age. It is well known that the nervous system, and that naturally includes the component elements, grows with the rest of the body, though at a gradually decreasing rate from the date of birth.

It does not seem to have been considered that if there is a constant connection between the size of nerve-cell nuclei and of the muscular masses in supposed relation with them, that there should be some nuclei of the smallest size in the crural enlargement, for there are exceedingly small muscular masses in the foot, as small as any found in the body, and smaller even than the *musculus choanoides*, which is under the oculomotor innervation.

A very remarkable fact, one which seems to conflict with the establishment of any absolute laws in this field, is the relation to each other of the different nuclei of the muscles which move the eyeball in different animals. In man the cells of the abducens origin are far larger than those of the third pair, but in the iguäna the relations are reversed. The cells of the third- and fourth-pair origins are, in the latter, among the larger cells of its isthmus; those of the abducens cells among the most minute. It is to be also borne in mind that the rectus externus of man receives a larger supply of nerve-fibres than any other of the oculomotor muscles, though it is not proportionately larger.

The greater size of the lumbar enlargement in birds is not necessarily accompanied by an increase in the actual ganglionic matter. The researches of a French investigator have shown that much of the enlargement of the region of the sinus rhomboideus is due to a non-nervous development. *A priori*, one should infer that the cervical enlargement should preponderate in its nerve-cells in those birds which have feeble legs and powerful wings, while the reverse would hold good, particularly in the struthionidæ.

The development of a peculiar non-nervous structure in the lumbar enlargement of birds, especially well marked at an embryonic period, is, I think, of some bearing on the recently agitated question of a so-called lumbo-sacral brain in the extinct *sauranodon*, based on the great calibre of the spinal canal at that point. In all embryos there is a temporary enlargement, and even an indication of a rhomboid sinus at this region, and it is not necessary to go beyond this fact and the established development of a non-nervous structure in other sauropsida at the same point, in attempting to account for the dilatation of the spinal canal there found. This matter is not germane to the present subject, but as it has recently been attempted to bring both into correlation, randomly as this was done, I take the liberty of referring to it here.

#### PART XI.—THE "ASSOCIATION" CELL.

About twelve years ago Meynert\* described, as the typical structural element of the fifth or deepest stratum of the frontal, and the eighth of the occipital cortex, certain fusiform nerve-cells. These elements are at the apices of the gyri, parallel in direction with the pyramidal cells of other layers, but at the sides of the gyri and the bottom of sulci they occupy a different position, and are

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\* Der Bau der Grosshirnrinde und ihre örtlichen Verschiedenheiten. *Vierteljahrschrift für Psychiatrie*, 1868.

parallel instead of vertical to the surface, with their long axes.

The student examining cortical sections from man, will be unable to find a sufficient number of these cells in many regions to justify the designation of their aggregate as a special layer. He will, however, find one statement of Meynert's confirmed, that they accurately follow in direction the arched fibre-bundle which, under the name of a *fasciculus proprius*,\* appears to unite the apices of neighboring gyri. It is evident that these cells are forced into parallelism with the fibres of that bundle. Where the processes of the cells are seen connected with fibres, this is usually at the extremities of the long axis, and the fibres are then a part of the arched fasciculus; rarely can a connection of lateral processes (which are generally absent), with fibres penetrating to other cortical layers, be discovered. From their position and their relations to what are evidently functional associating tracts, Meynert was led to look upon the cells as connection points in the functional association of distinct innervations and impressions. Everything so far known justifies this view.

Now it might be anticipated, in agreement with the well-known principle that the development of a given mechanism is greater where the functional rôle is more important, that in the human brain, the seat of the most numerous and intricate associations, these cells should be also more abundant and well developed than in any other animal. Whether this anticipation would be a just one as it stands, I shall now consider.

In a section from the cortex of an ungulate,† I find the largest, most numerous and, in every respect, best differ-

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\* *Fibræ propriae*. Arnold.

† I, unfortunately, had the cortical segments from an ox, a calf, and a sheep in the same jar, and am unable to state from which of the three it was obtained. The general type of all is, however, the same.

entiated fusiform cells; they are closely crowded, and the very distinct layer they constitute is in places half as thick as the layer of pyramidal cells (excluding the barren endymal stratum). Their structure is the same as that of the cells described by Meynert. In no other animal have I found them so well-marked; those of the human brain will not bear comparison with them.

This fact might, on first sight, be considered as a fatal blow to the theory of Meynert. And, indeed, if Meynert's theory were to be taken up strictly as announced by that author, without duly considering a complementary theory or rather principle announced in this JOURNAL two years ago,\* it would be difficult to ward it off.

It was announced on the occasion referred to, that in higher development the nerve-tracts show a tendency to emancipate themselves from the interruptions offered by intercalated ganglionic matter. That the tendency is to the development of uninterrupted tracts, interrupted tracts being maintained to a certain extent, in obedience to organic needs that do not vary much in the animal range. That in obedience to this law, the long tracts of the cord replace the fibrillary and interrupted network of and near the gray substance, and that the internal capsule and the optic radiations encroach on the interrupted fibre-systems running through the great ganglia.

If this is true of the projection-system, the same must be true of the association-system. No special associating tracts can be identified in the reptilian brain; functional association is mediated by the hypothetical union of cell with cell, and the few fibrils of the white substance, which are seen to run apparently from one cortical area to another, are probably interrupted detachments of the projection-

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\* Architecture and Mechanism of the Brain. Preliminary considerations. JOURNAL OF NERVOUS AND MENTAL DISEASE, October, 1879. Also, Contributions to Encephalic Anatomy. *Ibidem*, July, 1878.

system. Next, we find associating tracts developed and richly provided with a special form of cell; and in highest development the association-tract loses its interrupting stations, for every ganglionic element to be traversed delays the transmission of the nerve-current. The uninterrupted associating tract is a more perfect mechanism than the interrupted one. If it is asked why such interruptions are ever developed, the answer is that they constitute *etappes* in phyllogenetic development; that no fibre was ever developed in the central nervous system, for which a nerve-cell interruption must not be surmised to have existed ancestrally, and that the interrupting association-cell is nothing but a specialization of the same cell-group, which, in the main, remains a projection-field.

I have observed another fact in this connection. The associating fasciculi are better marked in large animals than in small animals of the same zoölogical order. It seems as if with the diminished distance of cortical area from cortical area, that the intracortical fibrillæ suffice for the performance of those functions which necessitate distinct tracts with greater cortical distances.

#### PART XII.—THE CONTESTED ORIGIN OF THE TRIGEMINUS.

While the origin of the lesser motor root of the trigeminus from the motor trigeminal nucleus and the raphe is well established, and that of the sensory root from the ascending radicle and the gelatinous nucleus in the level of exit is now universally adopted, considerable doubt enshrouds the question as to which of the two roots receives the descending radicle, which is known to be derived from the mesencephalis nucleus of the fifth pair.

Meynert\* traces the external detachment of the descending radicle into the sensory root. I have never seen any-

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\* Vom Gehirne der Sauge thiere, p. 775.



thing in hundreds of sections taken through every level concerned in this question, and from a number of different animals, that could conflict with this view. It was with considerable surprise, therefore, that I read Forel's statement\* that this detachment reaches the motor root, and forms a part of it, undergoing complete admixture with its fibres. There is such an affectation of accuracy and detail in the treatise of the latter author, and I was able to confirm so many of his other observations, that in my larger treatise I adopted his view. In this, as in some other respects, I fear that, like others of Gudden's pupils, Forel has needlessly complicated a very simple question. Such a contradiction as he made should have been based upon only the clearest appearances, especially as experimental confirmation of Meynert's views had been furnished by Merkel.†

But aside from the question of personal equation which has entered into the consideration of this matter, there has lately entered another which presents some amusing features.

In a very full *compilation* of the recent results obtained in brain anatomy, Schwalbe‡ quotes Henle as one of those entertaining the same view as Forel, opposing Meynert, in regard to this matter. In his first edition Henle makes no such statement; the second edition is not at my disposal, but I feel certain that whatever the text may contain, the very excellent and truthful figure 155 has not been expunged. The figure in question represents a powerful bundle of the sensory root derived from above and arching over the motor nucleus. Any one familiar with the subject, could give the figure but one interpretation, namely, that of the strongest confirmation of Meynert's views. If Schwalbe saw this figure, he must have supposed the

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\* *Archiv fuer Psychiatrie*, vii.

† Untersuchungen aus dem Anatomischen Institut, zu Rostock, 1874.

‡ Hoffmann-Schwalbe, ii.

motor nucleus to lie *behind* the sensory root, in failing to correct the evident misinterpretation of the figure. Henle does commit one actual error; he denies, in his first edition, the participation of the ascending radicle in the building up of the sensory root. This was due to the fact that his longitudinal sections are at the same time directed forward and inward. Such sections may be better calculated to reveal the relations of the descending radicle than those I am about to describe, but it is evident from the fact that the ascending radicle runs cephalad\* and laterad, that sections running caudad and laterad must fail to show its continuity. In a series of sections made parallel to the direction of the ascending radicle, I can demonstrate the correctness of the generally accepted view, that the ascending radicle is a true trigeminal fasciculus; in fact, I have transverse sections that were conclusive to my mind on this head before I prepared the longitudinal series referred to.

But I was also able to demonstrate, in the latter series, that not only the descending radicle sends at least a great mass of its fibres to the sensory root, and this so clearly that it is remarkable Forel could question this relation, but that, in addition, the processes of cells appertaining to the mesencephalic nucleus of the fifth enter that bundle in the same section. Although I cannot trace a single process all the distance to the sensory root, yet I can trace such beyond the level of the motor root, and the course of the fasciculus, as a whole, is perfectly clear. While I am not able to exclude a participation of the descending radicle in the formation of the motor root, I would insist that there is every ground for stating that that division which is derived from the mesencephalic nucleus passes altogether into the sensory root.

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\* *Cephalad* equals forward; *caudad*, backward; *dorsad*, upward; *ventrad*, downward; *laterad*, outward. These terms are gaining ground in comparative anatomy, which science has generally been in advance of human anatomy in respect to terminology.

I would, therefore, correct the contrary statement which in excessive deference to authority I was induced to incorporate in the larger essay referred to. The following facts concerning the cells of the mesencephalic nucleus seem to me well established :

1. The cells of this nucleus are equally well developed in all the mammalia so far examined,\* and of the same shape and relations in all of them.
2. They are also present (more dorsally though) in reptiles.
3. Their efferent processes accumulate in the outer part of the descending radicle of the trigeminus and leave the brain in the sensory root of that nerve.
4. Other processes of the same cells seem to be connected with the radiatory fibres of the optic lobes.

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\* Forel says "well developed" in the mole ; this fact may conflict with my theory that the innervation of the lachrymal gland resides in these cells. I do not know in the first place whether the atrophy of the eye in the mole is accompanied by atrophy of the lachrymal gland. The statements of Gudden and his pupils about the optic lobes in the mole have been contradicted by Tartuferi, as that this observation requires confirmation and future study.